The Effect of Exposure to Corruption Information on Specific and Diffuse Political Trust

Evidence from a Survey Experiment

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This thesis is submitted for the degree of
Master of Philosophy in Politics (Comparative Government)

Candidate N^{o}1030710 May 4, 2020

Acknowledgements

I express my deepest gratitude to all the academic advisors and peers that have supported me in this project. I am grateful to my thesis supervisor, Dr. Spyros Kosmidis, for providing me with the necessary guidance and encouragement to bring this project to a good end. His insight has been an incredibly precious resource for the production of this thesis. I would also like to thank all my peers from the Department of Politics and International Relations for making this project so much more interesting and enjoyable.
All errors are my own.

Abstract

How does the disclosure of factual information about corruption affect different dimensions of political trust? Existing literature suggests that exposure to corruption will have a strong negative effect on people's reported levels of political trust. However, little empirical work has been produced to identify whether that effect is driven by a decrease in the individual's trust in particular political agents and institutions or by a drop in the confidence they express with regard to the principles of the political regime as a whole. This thesis is the first to address this research question from an experimental angle. I employ a vignette-based survey experiment, based on original data collected from an online survey conducted in March 2020, through which I seek to measure the extent to which revealing information about corruption deteriorates various dimensions of political trust. I further analyze how political trust judgments change when individuals are confronted to different types of corruption, namely personalized or institutionalized. I find that when corruption revelations do not provide sufficient information to effectively attribute blame to particular political objects, specific political trust and trust in the principles of democracy deteriorate the most strongly, while trust in the overarching institutions of the state remains intact. This effect is amplified when the vignettes identify bribery as the most common form of corruption, which I theorize is the result of blame being primarily attributed to political agents. When the vignettes identify lobbying as the most common form of corruption, however, trust in the overarching institutions of the state suffers a meaningful decrease as well, which I argue occurs because blame attribution is no longer exclusively personal but indicates the existence of a systemic or procedural flaw. Given the lack of inter-temporal outcome measurement, I suggest that these results should be interpreted with caution and perhaps complemented with further longitudinal analysis.

Keywords: corruption, political trust, socialization, evaluation

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Chapter 1

Introduction

1.1 Research Question

This thesis investigates the relationship between exposure to information about corruption and various dimensions of political trust in advanced democracies. In particular, it seeks to measure how exposure to corruption information affects trust in specific and diffuse categories of political objects, depending on whether the blame is primarily attributable to individual behavior or institutional processes. I refer to bribery as the prime example of corruption where the blame is principally carried by political agents, rather than institutional structures or procedures, and conversely, I use lobbying as the main example of an institutionally (rather than individually) blameworthy instance of corruption.

1.2 Puzzle and Relevance

Although many studies focus on the relationship between corruption and political trust, few of them have attempted to disaggregate the outcome variable into its component parts – and none, to my knowledge, have done so in an experimental setting. Interestingly, the variable of trust has received extensive experimental attention but rarely in relation to corruption. Trust has generally been studied in laboratory settings in the context of investment games through which researchers have tried to understand the game-theoretic underpinning of trust decisions (Berg et al., 1995; Johnson and Mislin, 2011). Corruption has also been at the center of its fair share of experiments, however, these have generally focused on explaining outcomes related to the way in which corruption is detected and punished in social environments (Guerra and Zhuravleva, 2019), as well as treating corruption as the variable to be explained (Armantier and Boly, 2011). Alternatively, experiments have been carried

out to estimate the impact of corruption on social, rather than political, trust (Rothstein and Eek, 2009), or other political variables, such as turnout (Chong et al., 2015). Therefore, it has so far been difficult to tell whether the fluctuations in reported levels of political trust that scholars have observed in response to increases (or decreases) in corruption have been driven by variations in the diffuse or specific dimensions of political trust. Answering this question could have far-reaching implications for society and democracy as a whole. If exposure to information about corruption has a moderately negative effect on specific political trust, but only a weak one on diffuse trust, then it will not undermine democracy and could in fact reinforce it. Scholars have previously argued that a certain dose of specific political distrust can be helpful for democracy, as it stimulates political engagement and keeps politicians accountable and responsive (Hibbing and Theiss-Morse, 2002; Lenard, 2008). In addition, the causes of specific political distrust are relatively easy to tackle, for instance, corrupt politicians can be removed from office through elections. However, scholars have also concluded that democracy necessitates a high dose of diffuse political trust to maintain its legitimacy and smoothen the decision-making process (Braithwaite and Levi, 1998; Gamson, 1968; Hetherington, 1998). Unlike specific distrust, diffuse political distrust also represents a more insidious threat since institutions are generally resistant to change and the core principles of democratic government can hardly be altered or re-negotiated other than through revolutionary means. Therefore, if exposure to information about corruption has a meaningful deteriorating effect on diffuse political trust, it could pose a major obstacle to the consolidation of the democratic order. Overall, I believe that gaining a better understanding of how exposure to corruption-related facts affects diffuse and specific types of political trust will not only allow us to better assess the non-material consequences of corruption, but will also have implications for the way in which the fight against corruption is carried out and publicized.

Beyond the broader societal and intellectual relevance of this research project, this thesis will also seek to fill theoretical and methodological gaps in the literature on corruption and trust. First, I develop an original theoretical framework that seeks to provide a more nuanced account of political trust formation compared to existing models. In brief, I argue that both diffuse and specific types of political trust contain an evaluative and socialized component, which are, however, weighted differently. While diffuse political trust results primarily from one's socialization in the years of youth and is less reliant on objective evaluations, specific political trust is only weakly socialized and therefore predominantly evaluative. I also argue that the evaluative aspects of each type of political trust are sensitive to different kinds of information, as specific trust is primarily concerned with the conduct of individuals, while diffuse trust is mainly concerned with the structural or procedural features of a political

system. My second contribution to the literature on corruption and trust is a methodological one. I approach my research question using a fully experimental design based on an online survey that I fielded in March 2020. This represents an important step forward since the relationship between corruption and political trust has received little experimental attention. In fact, studies that have tackled this question have typically relied on purely observational analyses based on descriptive survey data (Levi and Stoker, 2000). Survey experiments represent a promising framework for the study of political attitudes formation, as they strike a balance between the internal validity of experimental designs and the external validity of observational approaches.

1.3 Summary

In this thesis, I attempt to reconcile the two primary strands of the literature on trust formation, which conceptualize trust as either the product of socialization or the result of objective evaluation. The socialized aspect of political trust is developed early in life and becomes deeply embedded in an individual's personality traits. This dimension of trust will therefore not vary as individuals progress through life, meaning that individuals will always have a certain degree of blind trust in the different elements of the political system. However, political trust also contains an evaluative aspect, which implies that individuals update a part of their beliefs about the trustworthiness of political entities depending on their direct assessments of their performance. I argue that political trust is a function of both an individual's early political socialization, as well as their objective evaluation of a political object's performance, which can be measured along ethical or practical lines. However, I posit that the weight of these two dimensions differs for specific and diffuse types of political trust. I theorize that in advanced democracies, individuals are primarily socialized to develop confidence in the more diffuse aspects of the political system, such as its foundational principles or overarching institutions, rather than the more specific ones, such as elected officials and their particular offices. I therefore expect diffuse political trust judgments to depend to a lesser extent on objective performance evaluations compared to judgments that target specific political objects. I further argue that the evaluative components of diffuse and specific trust judgments are sensitive to different kinds of political information: informational stimuli that concern the behaviors and decisions of individuals within the political system should primarily affect specific value judgments, while exposure to information that relates to institutional processes should primarily impact diffuse value judgments.

I tackle this paper's research question using an experimental design. Given the strong correlation between political and social trust, I also incorporate measures of generalized

and particularized social trust in my study. I use an online survey vignette experiment to first measure the differential effect of general corruption information on various dimensions political trust, going from the most specific to the most diffuse ones. I then employ the same approach to determine the effect of corruption vignettes in which the corruption is associated with either bribery or lobbying on trust measures. I perform a series of heterogeneous effects tests, predicting that the strength of the treatment effect will vary depending on the respondents' cultural and sociopsychological profile, partisan attachments and ideological orientation, as well as their level of political sophistication. Lastly, I test the validity of this paper's causal mechanism by analyzing whether the treatment effects are moderated by how early respondents first learned about more or less specific political objects, as well as by how much they value these objects for instrumental or non-instrumental reasons. I argue that both of these variables reflect the influence of the respondents' early political socialization process, which I expect will partly determine the extent to which their reported levels of political trust will vary when confronted to corruption information.

In terms of results, I find partial support for my main hypotheses. First, corruption vignettes in which the blame is either undefined or attributable to individual political agents do have a disproportionately larger effect on indicators of specific political trust and no significant effect on trust in the overarching institutions of the state. Second, corruption vignettes in which the blame is rather institutional have a strong effect on trust in politicians but also have a substantively meaningful and statistically significant negative impact on institutional trust. Lastly, contrary to my theoretical expectations, trust in the diffuse principles of democracy is consistently the most heavily affected dimension of political trust, including when the attribution of blame is undefined. These results are largely robust to a series of alternative modelling techniques, restricted samples analyses, as well as the inclusion of control variables. Heterogeneous effects analyses indicate that the treatment effect might be moderated by the respondents' partisan and ideological preferences, level of political sophistication, and psychological and cultural attitudes. However, the interaction terms I estimate generally fail to achieve statistical significance, which I believe is partly due to insufficient variation in covariates caused by small sample sizes. Lastly, analyses of the causal mechanism contradict the logic of this paper's theory, as having learned about certain political objects early in life and associating them with more non-instrumental values does not increase the stability of political trust when confronted with corruption revelations. However, these interactions also fail to achieve statistical significance, which is why they should be interpreted with caution.

1.4 Roadmap

This thesis is divided into six chapters. In Chapter 2, I outline the definitional scope of the main explanatory and dependent variables, followed by a review of the existing theories of trust formation and an evaluation of the main contributions in the literature on corruption and political trust. Chapter 3 outlines the theoretical framework that I develop to explain how the effect of exposure to information about corruption will vary depending on the dimension of political trust that is being measured, as well as the mode of corruption that is being described. Chapter 4 describes the empirical strategy that will be employed to test these relationships, including a discussion of the survey structure, variable operationalization, and methodological limitations. Chapter 5 discusses the primary and secondary results of the online survey experiment, including analyses of heterogeneous effects and tests of the causal mechanism that underlies the aforementioned association. Finally, Chapter 6 presents concluding remarks, including a summary of core findings, as well as a discussion of this paper's limitations, potential avenues for future research, and policy implications.

Chapter 2

Literature Review

2.1 Definitional Scope

2.1.1 Defining Political and Social Trust

The notion of trust belongs to a broader set of attributes that scholars refer to as social capital. Traditionally, social capital is defined as the sum of the norms, values, and beliefs that enable social groups to function effectively. These attributes are not natural or innate characteristics, but correspond to forms of learned behavior (Ridley, 1997). The concept of social capital was initially formulated at the beginning of the 20th century (Hanifan, 1916) but it only started to garner serious academic interest between the 1960s and 1980s (Bourdieu, 1984; Coleman, 1988; Salisbury, 1969), and became a mainstream object of study in the fields of political science, economics, and sociology in the 1990s, after being popularized by Putnam (Helliwell and Putnam, 1995; Putnam et al., 2000). Although definitions vary across time, Putnam's analysis of social capital remains the dominant one in the literature. According to him, social capital refers to "connections among individuals – social networks and the norms of reciprocity and trustworthiness that arise from them" (2001, pg. 19). Trust therefore appears as one of the main constituents of social capital. Even though the notion of trust is difficult to define with precision, given its multi-faceted and abstract nature, it has generally been described as a strategic relationship between people and other individual or institutional entities. In short, trust represents *A's belief that it is in B's interest to not do them harm* (Levi and Stoker, 2000; Oskarsson, 2010; Zmerli and Newton, 2007).

Trust can be disaggregated into two empirically related but conceptually distinct parts, namely horizontal trust (also referred to as interpersonal or social), which concerns the individual's propensity to trust other people, or vertical trust (also known as political), which refers to the relationship of trust between citizens and political regimes, institutions, and

actors. In other words, horizontal trust covers trust relations between family members, friends, neighbors, work colleagues, or fellow citizens in general, while vertical trust refers to the confidence people place in political entities, such as parliaments, courts, or political leaders. Both of these types of trust possess a thick and thin dimension. First, interpersonal trust can be either particularized or generalized. The former is based on "personal, first-hand knowledge of individuals" (e.g. trust in particular individuals), while the latter depends on "general information about social groups and situations" (Zmerli and Newton, 2007, pg. 3). To rephrase, particularized social trust concerns relationships between people that interact with each other directly, while generalized social trust refers to one's willingness to trust people belonging to a certain community in general. Similarly, political trust can also be separated into thick and thin variants, otherwise known as specific and diffuse. Specific political trust refers to the trust that people place in politicians or particular political offices, while diffuse political trust is directed towards to broader institutional structures, procedures, and more abstract political values. This thesis will focus primarily on political trust but will also incorporate an auxiliary analysis of the treatments' effect on social trust.

The process of separating the concept of political trust into its component parts is deeply tied to the ways in which this variable has been measured historically. The first reliable measure of political trust was created by Stokes and Miller (1962) through the National Election Studies trust-in-government survey questions. These questions sought to evaluate the sentiment of the American population towards the government by requesting respondents to make a series of judgments regarding state institutions and elected officials based on criteria such as ethics and efficiency. At first, however, government officials and institutions were being treated as one and the same thing, which meant that it was impossible to know whether the observed variations in trust responses were driven by an increase of distrust in the political system, particular institutional processes, or the incumbent authorities. In order to fill this conceptual gap, Easton (1965) proposed to disaggregate the notion of political trust into its component parts, introducing the distinction between diffuse support for the system or regime and specific support for the incumbent authorities. Following this theoretical development, the popularity of political trust as a dependent variable grew rapidly within the academic community. Extending Easton's binary distinction between diffuse and specific trust, Norris (1999) developed a more fine-grained classification, identifying five different levels of political trust, ranging from the most concrete, to the most abstract, namely trust in political actors, regime institutions, regime performance, regime principles, and the political community. In this thesis, I tweak Norris' conceptualization of political trust and identify four levels at which trust can be expressed, namely trust in active politicians, the public offices that they occupy, the overarching formal institutions of the state, and lastly, the core principles

of the democratic regime. Given the absence of an inter-temporal outcome measurement component in my experimental framework, I will only analyze variations in short-term trust judgments.

2.1.2 Defining Political Corruption

Political corruption is traditionally defined as the use of public power to benefit a private interest. Senior (2006) describes political corruption as an action to secretly provide a good or a service to a third party so that he or she can influence certain actions which benefit the corrupt agent, a third party, or both, and in which the corrupt agent has authority. Corruption can occur on different scales: petty corruption consists of small favors that have a more or less localized effect, while grand corruption occurs at the highest levels of government and has the potential to generate meaningful political, social, and economic costs. Beside magnitude, corruption can also be categorized on the basis of its modality. Thompson (2000) proposes a distinction between institutional and individual corruption. Institutional corruption, he argues, arises primarily due to the weaknesses of an organization or process, while individual corruption refers to the illegitimate actions undertaken by agents within a system. He posits that corruption is more institutional than individual insofar as (1) the gain an official receives involves goods that serve a valid institutional function, such as a campaign contribution, (2) the advantage the official provides takes the form of access more than action, and (3) the connection between the gain and the advantage manifests a tendency to subvert the legitimate procedures of the institutions. Scholars and political commentators have often used the terms legal and illegal corruption to refer to the aforementioned conceptual distinction. Bribery, embezzlement, or nepotism are the prime examples of illegal corruption, while lobbying and campaign contributions are the most common forms of legal corruption. While these two sides of corruption are highly relevant to the theoretical argument I develop, this thesis will also be concerned with the phenomenon of corruption at its most general level, meaning some respondents will be exposed to information about corruption that does not specify the nature or modality of the act but simply measures the extent to which a country is corrupt as a whole.

2.2 The Politically Exogenous and Endogenous Theories of Trust Formation

Before reviewing the literature on the relationship between corruption and political trust, I first explore the two main ways in which the nature of trust (in either its social or political form) has been conceptualized. This is an important preliminary step since existing theories of trust formation make different claims regarding whether or not trust should vary as people progress through life – and by extension, whether factors such as corruption revelations should have any effect at all on trust decisions. Fundamentally, the willingness to bank on the trustworthiness of others involves taking a risk. Therefore, scholars have tried to understand why or under what circumstances individuals decide to take the risk of trusting other people or institutions. Two main theoretical models have been formulated to answer this question. The first major theory of trust, also known as the politically exogenous approach, is composed of two branches, namely the psychological and socio-cultural models, which correspond to micro- and macro-levels of analysis. The second major theory of trust is known as the politically endogenous approach and can also be further divided into two branches, namely the civil society and institutional models, which similarly refer to micro- and macro-levels of analysis. I review both of these theories below. A summary of the names and main variables associated to the theories mentioned in this section are presented in Table 2.1.

According to the politically exogenous approach, trust is determined by an individual's socializing experiences. It is an attitude that becomes embedded within one's core set of psychological features as a function of one's socioeconomic and cultural environment. In this case, trust appears as the product of an individual's lifelong process of socialization, which begins in infancy and may continue into late adulthood. From this point of view, trust-related survey questions merely reveal the individual's general disposition towards the world, rather than their response to external social, economic, and political events. Also, this approach implies that political trust is simply an extension of social trust; in other words, the extent to which an individual is willing to trust political entities is a projection of their level of social trust onto the political realm (Helliwell and Putnam, 1995; Verba and Almond, 1963). Conversely, the politically endogenous approach to trust conceptualizes trust as the product of rational calculations that individuals make in order to assess the trustworthiness of others. According to this view, trust is a malleable resource that is constantly being modified or updated in response to external social, political, or economic events. In this case, political trust appears as an endogenous phenomenon, meaning that it is in large part determined by features of the political system and the way in which individuals interact with them, rather than people's politically exogenous predispositions. This view implies that instead of

capturing entrenched individual inclinations, trust-related survey questions actually inform us of how respondents assess the trustworthiness of society. Also, this model acknowledges that social and political trust are likely to covary but suggests that social trust may partly originate from political trust and not the other way around. Unlike the politically exogenous model, this theory emphasizes the evaluative dimension of trust.

Although both of the aforementioned theories offer compelling insights into the origins of trust, their predictions with regard to how trust measures should behave differ greatly. While those that emphasize the importance of socialization in the formation of trusting behaviours expect people's willingness to trust to remain almost entirely constant throughout their lives, those who see trust as the product of evaluation expect trust to oscillate in response to external events. It is therefore important to note that the large variations in attitudinal measurements of interpersonal and political trust that longitudinal surveys have been capturing through the years cast doubt over whether the politically exogenous theory can by itself explain how trust judgments are made. In this thesis, I attempt to reconcile these two perspectives and incorporate them into a unified theoretical framework by arguing that specific and diffuse dimensions of political trust both contain a socialized and evaluative component. In the following sections, I summarize the insights of the psychological, socio-cultural, civil society, and institutional models of trust formation, after which I explore how these have been applied to the study of corruption and political trust in particular.

2.2.1 Psychological and Socio-Cultural Models

The psychological component of the politically exogenous theory represents the micro-level branch. This model argues that an individual's willingness to trust is a trait or disposition learned through primary and secondary socialization in early childhood, which then becomes part of the individual's core personality. This disposition then tends to persist throughout the individual's life, although it can still be altered by traumatic experiences (Uslaner, 2000). This model sees trust as a component of a broader set of personality characteristics, which include optimism, agreeableness, or cooperativeness, that ultimately determine how an individual interprets and interacts with the world. For instance, an optimist might be more inclined to trust and cooperate with strangers because of a more developed sense of altruism, which will also translate into a higher propensity to trust the authorities. Pessimists, on the other hand, will tend to have more misanthropic personalities and therefore close themselves off more easily to possibilities of social cooperation, as well as be more intransigent with political actors and institutions (Allport, 1961; Cattell, 1965; Erikson, 1950; Rosenberg, 1956; Uslaner, 2000).

The socio-cultural component of the politically exogenous theory represents the macro-level branch. This variant of the theory defines trust as the product of an individual's social, rather than psychological, characteristics. In other words, trust is not merely the result of primary socialization but rather a sentiment that can be updated throughout one's life in accordance to one's objective socioeconomic attributes, such as their income, education, or employment status (Marková and Gillespie, 2007). In this case, the evolution of trust can also be shaped by changing cultural norms and traditions, which tend to have a homogenizing effect on a society's trust levels (Verweij, 2005). Although this model acknowledges that trusting attitudes might vary depending on the evolution of one's socioeconomic profile, it still predicts a relatively high degree of stability in attitudinal trust measures since intra-generational mobility tends to be incremental.

2.2.2 Civil Society and Institutional Models

The civil society model represents the micro-level branch of the politically endogenous theory. This model claims that both social and political kinds of trust are a product of voluntary associations and other subjective experiences of micro-level interactions between individuals and the institutions that structure life in society. In other words, we learn to trust by communicating with other actors or groups of individuals and participating in collective organizations, which facilitate repeated social interactions (Dinesen, 2012; Glanville and Paxton, 2007; Helliwell and Putnam, 1995). This view is inspired by 19th century political thinkers, such as de Tocqueville or Mill, who argued that sustained, face-to-face involvement in community governance would help people understand the value of cooperation, reciprocity, and empathy, and therefore generate trust, in the system and in each other. According to this view, what matters most are direct personal experiences; in the case of political trust, this implies that individuals update their trust beliefs once they directly experience the actual consequences of institutional or political action on a micro-level, such as suffering from corruption by having to bribe a street-level bureaucrat in order to obtain assistance.

The institutional model of trust emphasizes the importance of structures and systems, and their aggregate performance, in encouraging trusting behavior. These structures include formal institutions, such as courts, the police, the legislative and executive branches of government, or the bureaucracy. This view is built upon a simple intuition, which is that trust is a necessary heuristic for life in society: due to practical and cognitive limitations, people are unable to acquire and process all the information that might be required in order to decide whether something or someone deserves to be trusted. Therefore, in order to make this decision, people take cues from the objective aggregate performance of the entities whose trustworthiness they are evaluating (Coleman, 1993; Dasgupta and Gambetta, 1988;

Hetherington, 1998). Unlike the psychological and socio-cultural accounts of trust formation, the civil society and institutional models are complementary.

Table 2.1 Theories of (Social and Political) Trust Formation and Related Variables

Theories	**Variables**	**Level of Analysis**
	Politically exogenous approach (socialisation)	
Psychological model	Primary socialisation, traumatic experiences	*Micro*
Socio-cultural model	Socioeconomic characteristics, cultural norms	*Macro*
	Politically endogenous approach (evaluation)	
Civil society model	Voluntary associations, direct interactions and experiences	*Micro*
Institutional model	Aggregate institutional performance	*Macro*

2.3 Literature on Corruption and Political Trust

In the previous sections, I outlined the main ways in which the nature and origin of trust have traditionally been conceptualized. In this section, I examine the state of the literature on the relationship between a particular type of politically endogenous factor, namely corruption, and political trust, which is this paper's core dependent variable. Corruption has been identified as one of the key explanatory variables accounting for variations in patterns of political trust by proponents of both the civil society and institutional models of trust formation. According to these strands of the literature, trust outcomes can either result from the citizens' direct experiences with corrupt processes and behaviors, or from their attempts to evaluate the overall performance of their country's political system along different lines, such as bureaucratic efficiency, ethical conduct, or economic growth – all of which are affected by corruption. The arguments formulated in the literature on corruption and political trust can be grouped into two schools of thought, which predict opposite effects: one of them claiming that corruption revelations have a positive effect on political trust, while the other affirming that corruption information has a corrosive impact on confidence in institutions and politicians.

Scholars pertaining to the first school of thought have argued that when individuals are confronted to corruption-related information, their level of political trust either remains

constant or increases. These outcomes could be the product of several different mechanisms. First, individuals might interpret the newfound visibility of corruption scandals in the media as a signal that the system is capable and willing to filter its bad seeds out. According to this view, people are aware to a greater or lesser extent of the degree of corruption that exists in their country, which means that exposing them to new information about corruption does not challenge or update their pre-existing worldview, but rather indicates to them that something is being done to curb corruption. A different mechanism that predicts a similar outcome posits that, in certain contexts, corruption greases the wheels of bureaucracy (Shleifer and Vishny, 1993) and therefore has a positive effect on economic growth (Saha and Ali, 2017), which in turn is expected to increase the citizens' level of trust in the political system. According to this perspective, the possibility of engaging in quid pro quo exchanges with public officials allows private economic agents to navigate the bureaucracy more smoothly, therefore increasing the efficiency of many administrative processes that would otherwise be costlier in terms of both time and money (Bertrand et al., 2007). This argument applies mostly to institutionally underdeveloped contexts, as well as places where corruption remains relatively constrained. If corruption manifests itself on a national rather than local scale, then its costs are likely to outweigh the benefits and its effects on political trust are expected to reverse (Saha and Ali, 2017). Lastly, if the corruption information that is being disclosed to individuals paints a better-than-expected picture of the level of corruption in a given country, citizens might interpret it as a signal that circumstances are in fact improving and therefore express more confidence in political actors and institutions. This argument is largely based on the literature that studies the relationship between the disclosure of information about corruption and electoral accountability, which emphasizes the importance of prior beliefs in determining political value judgments (Arias et al., 2018; Ferraz and Finan, 2008).

Scholars pertaining to the second school of thought argue that corruption has a profound corrosive effect on political trust. This outcome can yet again result from multiple distinct mechanisms. First, corruption may have a deteriorating effect on political trust because it is perceived as a breach of morality by individuals (Wang, 2016). According to this argument, individuals expect public officials, as well as institutional processes, to function in a morally exemplary way and act fairly (Whiteley et al., 2016). Any violation of ethical standards will generate a strong sense of resentment and anger that will ultimately lead into an increase of political distrust. Second, corruption could also decrease political trust because of its material consequences on the welfare of individuals. According to this view, distrusting reactions might be triggered by either the direct costs of corrupt behavior, such as the money one might lose by having to bribe a street-level bureaucrat, or by the indirect negative effects that corruption might have on the overall performance of the system, such as increasing

political instability, reducing the level of human capital, or discouraging private investment (Mo, 2001). Lastly, tapping once again into the literature on prior beliefs and electoral accountability, scholars have argued that if the corruption information that is being disclosed paints a worse-than-expected image of a given country's corruption level, then the citizens' willingness to trust political entities will go down (Arias et al., 2018; Ferraz and Finan, 2008).

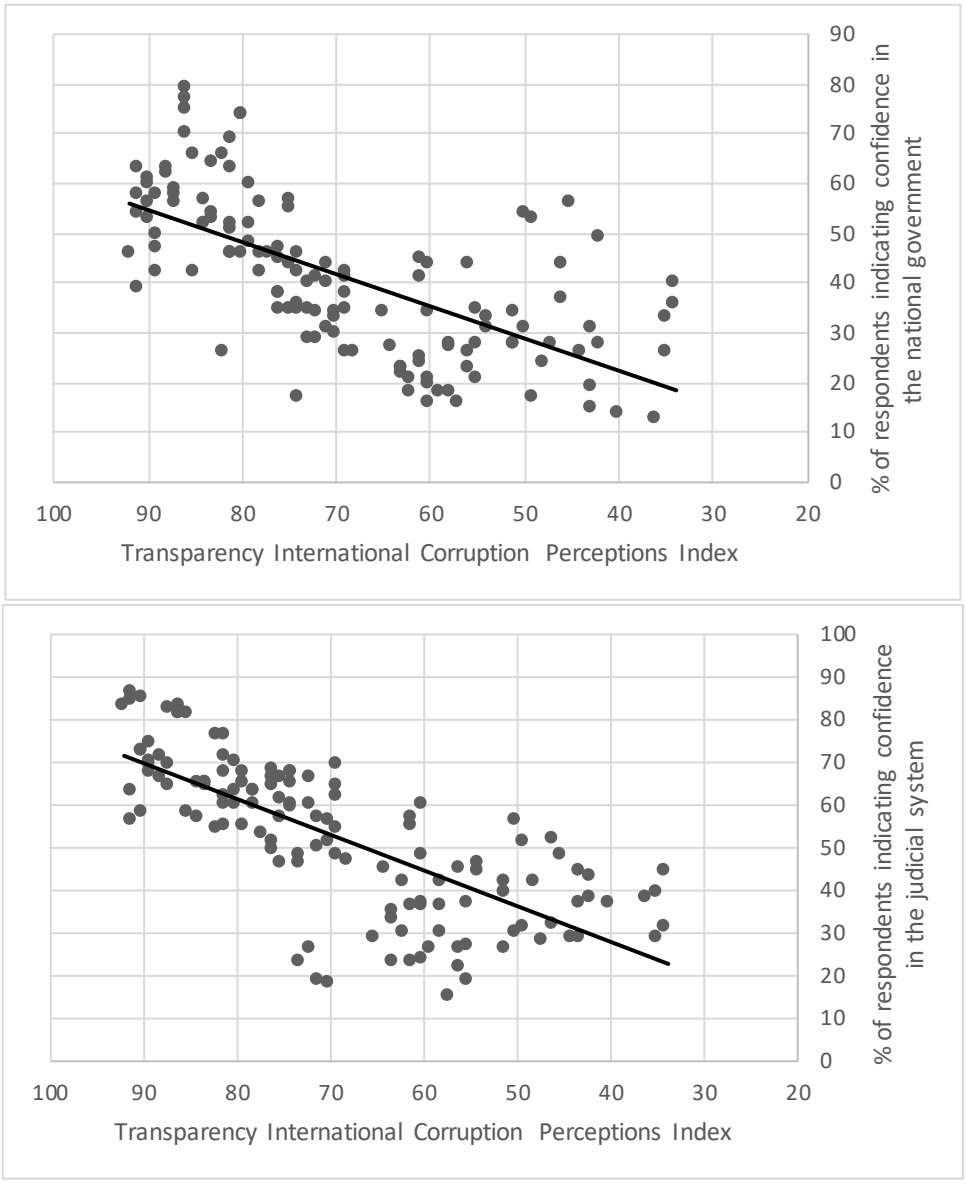
2.4 Empirical Findings on Corruption and Political Trust

In the previous section, I listed several theoretical channels through which corruption information disclosure could impact political trust. In this section, I review the level of empirical support that each of these channels has received in order to justify a key assumption that I formulate in this thesis, namely that exposing individuals to corruption information will negatively affect their level of political trust.

The broad consensus in the literature seems to be that exposure to corruption information has a negative effect on political trust (Clausen et al., 2011). The argument according to which citizens interpret the increased visibility of corruption as a sign that the system is successful at filtering out its bad seeds finds no empirical support in the literature: greater levels of corruption are universally associated with lower levels of confidence in institutions and politicians. For instance, Chong et al. (2015) demonstrate experimentally that corruption information is more likely to quash the voters' hopes, rather than inspire mobilization or signal an improvement in transparency; they find that Mexican voters respond to incumbent corruption information by withdrawing from the political process, which results in decreasing voter turnout and the erosion of partisan attachments. On a more general level, Anderson and Tverdova (2003) use surveys conducted in 16 mature and newly established democracies around to globe to examine the effect of corruption on people's attitudes toward government, including their level of trust in civil servants. Their analysis demonstrates that citizens in countries with higher levels of corruption exhibit much lower levels of trust in civil servants. Similarly, Van der Meer (2010) performs multi-level analysis on stapled data from the European Social Survey (2002 – 2006) in order to identify the factors that explain cross-national differences of trust in Parliament. The author finds that corruption plays an important role in accounting for this variation. Hakhverdian and Mayne (2012) also employ a multi-level model approach to explain cross-national variations of the effect of education on institutional trust as a function of the pervasiveness of public-sector corruption. The authors find that corruption has a corrosive effect on institutional trust that worsens as education improves. The direction of the effect of corruption on political trust is also verified observationally. Figure 2.1 plots the percentage of Gallup World Poll respondents indicating

confidence in the national government and the judiciary system over measures of perceptions of corruption based on Transparency International's Corruption Perceptions Index (the higher the score, the less corrupt and more transparent the country) in OECD countries between 2006 and 2015. The scatterplot reveals that the less transparent and more corrupt a country is, the less trusting its citizens are of important state institutions.

Fig. 2.1 Perceptions of Corruption and Institutional Trust in OECD Countries, 2006 – 2015



While this evidence is damning for the mechanisms that predict a positive effect of corruption on political trust, some of the variables upon which they focus on could still function as attenuating factors. For instance, the negative effect of exposure to corruption on political trust might be less pronounced in countries where corrupt state bureaucracies are more efficient. A similar outcome could be expected in places where citizens are confronted to corruption revelations that are not as severe as they expected. Regarding the efficiency grease hypothesis, several studies reject the idea that bribe paying or clientelism increase institutional trust. For instance, Lavallée et al. (2008) analyze Afrobarometer survey data from 18 sub-Saharan African countries to show that corruption never produces trust-enhancing effects regardless of the evaluation of public service quality. Additionally, Shleifer and Vishny (1993) call into question the very notion that corruption could ever improve efficiency: they conclude that although corruption can occasionally improve the allocation of certain goods to those who value them the most, it is generally responsible for expensive distortions in the markets, as well as suboptimal social outcomes. However, the argument claiming that the effect of corruption on political trust depends on the individual's pre-existing knowledge of corruption receives stronger empirical backing. For instance, leveraging a field experiment that informed Mexican voters about malfeasant mayoral spending before municipal elections, Arias et al. (2018) demonstrate that when information about incumbent malfeasance is better than what voters expect, incumbents are rewarded at the polls (in this case, increase in incumbent support acts as a proxy for heightened political trust).

Continuing the exploration of the empirical literature on corruption and political trust, the set of arguments that predict a negative effect of corruption on political trust have received broad empirical support. First, it is clear that individuals have a certain number of ethical expectations from political actors and institutions, which corrupt behaviors evidently violate. For instance, Wang (2016) analyzes data form three East Asian countries and finds that citizens expect their government to be both competent and ethical (i.e. not corrupt), but that they put more weight on ethics than competence when evaluating their trustworthiness. Similarly, Rothstein and Eek (2009) perform an experiment in which they ask undergraduate students to respond to different scenarios where hypothetical citizens are required to pay a bribe in order to receive immediate assistance from a public official; they find that when it is the official who takes the initiative to offer assistance in exchange for a bribe (rather than the person asking for help), trust is more strongly negatively affected, even though the outcomes would be the same. Although ethics matter, individuals also seem to be deeply concerned by the material consequences of corruption on their personal finances and the economic wellbeing of society as a whole. For instance, Schumacher (2013) finds a positive association between the size and number of bribes a politician takes and citizens' political distrust.

On a more general level, Wang (2016) finds that evaluation of government performance interacts with perception of corruption to negatively influence political trust. Unsurprisingly, the argument according to which the direction of the effect of corruption on political trust depends on the individual's pre-existing expectations with regard to corruption levels also works in reverse. To cite a famous example, Ferraz and Finan (2008) find that releasing the audit outcomes of corrupt incumbents in Brazilian municipalities damages their electoral performance (yet again, a proxy for political trust) but only when they are discovered to have surpassed the median level of corruption that voters expected to witness. The magnitude of this effect increases as the discrepancy between actual and expected corruption levels widens. This finding suggests that although voters are far from perfectly informed, their pre-existing level of political knowledge determines to a large extent how they will react to corruption revelations. Table 2.2 summarizes the predictions, mechanisms, and level of empirical support that each of the arguments discussed in this section have received

Table 2.2 Summary of Mechanisms Linking Corruption to Political Trust

Direction of Effect	**Mechanism**	**Empirical Support**
Corruption increases (or does not affect) political trust	Increase in visibility of corruption means system is successful at identifying and punishing malfeasance	*No*
	Corruption greases the wheels of bureaucracy and promotes growth	*No*
	Citizens are positively surprised to find out that corruption is lower than (or equal to) their prior expectations	*Yes*
Corruption decreases political trust	Corruption is perceived as breach of morality	*Yes*
	Corruption imposes either a direct or indirect material cost on citizens	*Yes*
	Citizens are negatively surprised to find out that corruption is higher than their prior expectations	*Yes*

Chapter 3

Theory

This chapter incorporates the lessons of prior scholarship and presents an original theoretical framework to describe how individuals process information about corruption in order to update their levels of specific and diffuse political trust. My theory consists of two adjacent arguments. The first argument concerns the relationship between general information about corruption, meaning exposure to facts that relate to the aggregate level of corruption in a country, and political trust. The second argument examines how the effect of corruption information disclosure on political trust varies depending on the modality of the act of corruption, meaning whether blame can be primarily attributed to institutional processes or individuals. Based on the findings described in the previous chapter, I build upon the assumption that revealing information about corruption has a negative effect on political trust. I then draw upon the politically exogenous and endogenous theories of trust and more specifically, their psychological and institutional models, to make the argument that diffuse and specific types of political trust judgments are the product of both socialized beliefs and objective performance evaluations, although not to the same extent. I posit that diffuse political trust is more strongly socialized and therefore less evaluative than specific trust, which leads me to predict that it will be less severely affected by exposure to general information about corruption. However, I also expect the strength of this effect to vary depending on the type of corruption that is being described: the evaluative component of diffuse political trust will be more sensitive to signals of institutionally entrenched forms of corruption, while specific trust will more strongly decrease when citizens are informed about individual misconduct. It is also important to note that although the theoretical framework presented in this paper can accommodate both the civil society and institutional models of political trust, the experiment I design refers to corruption as a feature of the political system rather than an experience that individuals may have in their daily lives.

3.1 General Corruption and Political Trust

Political corruption can be described in multiple ways: one can make reference to its degree of isolation, the number and types of actors involved, or the institutional processes through which it is carried out. However, at its most general level, corruption can also be described by making reference to its economic cost – an indicator of the degree to which it affects a set of institutions or group of individuals. In fact, much of the literature on corruption and political trust has adopted this broad view of corruption as a general indicator of a system's aggregate performance. I refer to this type of corruption revelation as being "blame-agnostic", since it does not provide sufficient information for people to effectively assign responsibility for its existence to any political entity. The first part of this paper's theoretical framework therefore studies how blame-agnostic presentations of corruption phenomena affect diffuse and specific political trust attitudes. The core argument that I develop below predicts that although disclosing general information about corruption to individuals will always deteriorate their reported levels of specific and diffuse political trust, it will not do so equally; in fact, I expect corruption to have a strong negative effect on specific political trust and a relatively weaker one on diffuse trust.

I theorize that both specific and diffuse types of political trust contain an evaluative and socialized component. However, I argue that these two components are weighed differently when producing specific and diffuse trust judgments. Diffuse political trust, which refers to the confidence one places in the principles and overarching institutions of a particular system, depends predominantly – though not exclusively – on one's socialization in the early years of life and to a lesser extent on the independent evaluation of the respective system's merits. Conversely, specific political trust, which refers to the confidence one places in particular entities within the larger system, such as political leaders or offices, is only weakly socialized and therefore predominantly evaluative. When confronted to general corruption facts, or in other words, corruption-related information based on which the attribution of blame is unclear, I expect individual levels of specific political trust to more rapidly decrease compared to diffuse trust. I argue that this happens because when the information that is being provided is insufficient to properly assign responsibility, individuals will associate the blame with the conceptual category whose performance they are most likely to be interested in scrutinizing. In other words, since specific political trust judgments are predominantly evaluative, specific political objects are logically the primary suspects when blame attribution can only be inferred. The rest of this section is therefore dedicated to explaining why diffuse and specific types of political trust are unequally reliant on socialization and by extension, evaluation. The logic outlined in this section is based on three premises; (1) diffuse and specific political concepts have an instrumental and non-instrumental value, (2) their non-instrumental value

relies on normative propositions, which are socialized, while their instrumental value is based on outcomes, which are evaluated, and (3) political socialization in advanced democracies prioritizes the internalization of the non-instrumental value of diffuse concepts rather than specific ones. Each of these premises is further explored below.

Traditionally, the promotion of democracy has been justified along two different dimensions, namely an instrumental and non-instrumental one. On the one hand, instrumentalism evaluates democracy consequentially, by making reference to the outcomes it produces relative to other methods of political decision-making. On the other hand, non-instrumentalism argues that democracy is an intrinsically superior system due to its inherent moral qualities. For instance, at its most general level, the instrumental value of democracy is related to its procedural mechanisms of accountability, which produce more effective laws and policies, promote social stability, stimulate economic growth, and according to some, improve the character of its participants (Mill et al., 1861; Sen et al., 1999). Conversely, the non-instrumental value of the democratic institutional framework is related to its attachment to individual rights and freedoms and commitment to equality and transparency (Christiano, 2004; Gould, 1988; Rostbøll, 2012; Singer, 1975). A similar pattern can be applied to the more specific components of the democratic system; instrumentally, elected officials are expected to be capable managers and effective administrators of public resources, while non-instrumentally, they are responsible for incarnating the general will and providing moral example to society.

The instrumental and non-instrumental dimensions of democracy are processed in a fundamentally different manner by individuals. The instrumental value is gauged through the practical consequences of democracy on the livelihoods of individuals. In other words, if one's support for democratic government (or for an incumbent politician) depends on the degree to which their level of welfare has improved since adopting said system (or voting in said politician) then their support becomes a function of the system's (or politician's) performance. This means that instrumentalism is the product of more or less objective evaluation. Assessing a system's non-instrumental value, however, depends to a far greater extent on subjective preferences for certain abstract values, such as liberty, equality, or individualism. Although one might consider these principles as universally desirable, they are not perceived as such across all political systems. Some political cultures, for example, might prioritize collectivistic rather than individualistic dynamics, even if they come at the expense of some freedoms. These types of values are not evaluated for their objective or practical implications but rather seen as inherently desirable and morally superior to the alternatives. Therefore, it appears that non-instrumentalism relies in most part on the early (political) socialization of individuals, as people's attachments to fundamental values tend to become entrenched from early on in their lives. It is important to keep in mind that

socialization does not refer merely to something that is learned in childhood but rather to the process by which a set of norms and values become internalized, or embedded, into an individual's personality. Therefore, a core implication of the distinction between socialized non-instrumental beliefs and evaluative instrumental beliefs is that only the latter will vary throughout an individual's life. It is also crucial to keep in mind that the evaluation of democracy's instrumental success is based on political knowledge that might be acquired at different moments in an individual's life, including childhood; however, it does not rely on any kind of strongly socialized conviction. For instance, while an individual's belief that democracy produces positive economic outcomes might vary depending on the country's performance, their conviction that democracy is the fairest type of regime is unlikely to be challenged. To summarize, one's belief in – and appreciation for – the moral or abstract value of democratic political objects is deeply engrained and rarely questioned beyond the years of youth; however, one's assessment of the outcomes produced by these political objects will constantly be updated in response to changing circumstances. This view also appears to be echoed in the literature on democratic socialization. To illustrate using a few examples, Conway et al. (1996) use longitudinal survey data from the US to analyse the effect of both formal and informal education on students' democratic beliefs; they conclude that participation in high school activities significantly predicts holding of democratic beliefs among young adults. In a more recent study, Heyne (2018) examines support for democracy in 26 European countries and finds evidence that socialization meaningfully affects the citizens' democratic expectations. Overall, the consensus in the literature seems to be that early life experiences shape political preferences and attitudes in a durable way, in particular with regard to attachments that people have to the non-instrumental value of various political objects.

So far, I have established that both specific and diffuse dimensions of democratic government are valued for instrumental, or consequential, and non-instrumental, or intrinsic, reasons. I have also argued that trust in the non-instrumental value of political objects is primarily the result of one's early political socialization and will therefore not vary as individuals progress through life, while trust in instrumental values is evaluative, which means that it relates to calculations that may produce different results depending on one's circumstances. However, if both specific and diffuse types of trust were subjected to equally profound socialization pressures, then we should not expect specific and diffuse political trust judgments to differ – which is what this thesis predicts. To cross this theoretical hurdle, I argue that in advanced democracies, the process of early socialization, which occurs through either schooling or family upbringing, prioritizes the diffuse aspects of democracy over its specific ones, which therefore implies that diffuse political trust will depend more strongly

on socialized, non-instrumental beliefs, while specific trust will primarily be a function of instrumentally-minded assessments. In other words, I expect specific political trust to vary relatively more strongly overall and be more sensitive to stimuli such as the disclosure of general information about corruption.

The emphasis that advanced democracies place on diffuse political socialization is justified in several ways. First, having a deep attachment to the broad principles upon which a country's political system is built is a highly effective form of political identity for the purpose of developing active citizenship and stimulating political engagement. Social harmony and stability can hardly be achieved if the citizens of a national community do not agree upon a set of core principles of government, such as the rule of law or the importance individual freedoms. However, disagreement about the proper conduct of specific political agents offers far greater room for debate and rarely threatens the integrity of the community's social fabric. Second, diffuse concepts generally appeal more directly to intuition and are easier to grasp from a young age. Although abstract concepts, such as fairness or freedom, are not innately present in the human brain, they tend to resonate with individuals from early ages. For example, several behavioural psychology studies demonstrate that children develop the capacity to understand concepts such as fairness or restorative justice as early as the age of three (Mendes et al., 2018; Riedl et al., 2015). Additionally, the cultural focus that advanced democratic societies place on the more diffuse dimensions of the political system is strongly reflected in the civic education curricula that countries such as France, the United Kingdom, or the United States have put in place. Although scholars have expressed uncertainty with regard to the influence of civic education on students' political attitudes (Langton and Jennings, 1968); the content of civic education curricula reflects the broad societal consensus regarding what forms of knowledge and values should be prioritized. For example, French school pupils engage with civic and moral education classes from the first grade. According to the Official Bulletin of National Education, the primary aim of the national education system is to "acquaint pupils with the values of the Republic", as "school is responsible for shaping students as future citizens" and "teaching [them] the fundamental values and principles inscribed in the country's Constitution" (article L 111-1 of the Code of Education). Similarly, citizenship has also been introduced as a statutory subject in the English National Curriculum and is taught as part of the school curriculum to all pupils aged between 11 and 16 years in maintained schools in England. The citizenship curriculum is based on key diffuse attributes of the democratic system, such as the principles of justice, rights and responsibilities, and diversity – although comparatively speaking, British citizenship classes go further in depth into the technicalities of the political system compared to the French model. A similar approach is also observable in the United States, where the

subject of civics has been integrated into the Curriculum and Content Standards to enhance the comprehension of democratic values in the education system.

To sum up, the first part of this paper's theory predicts that exposure to general information about corruption, meaning corruption information that does not directly assign blame to particular categories of political objects, will have a strong negative effect on specific political trust but only a weak one on diffuse political trust. I justify this by arguing that the specific and diffuse variants of political trust both result from partly socialized and evaluative dynamics, but that the diffuse component is more strongly socialized and therefore less evaluative than the specific one. In other words, individuals have a certain amount of blind trust in democracy at both its specific and diffuse levels, but the diffuse dimension benefits from it to a greater extent than the specific one. This ultimately implies that specific political trust will be more sensitive to fluctuations in the political system's aggregate performance – in this case, embodied by corruption – compared to its counterpart. Another corollary of this argument is that in the absence of treatment, people will naturally have a higher degree of confidence in diffuse political entities than specific ones. This argument was developed on the premises that, in democratic societies, early political socialization efforts through which individuals internalize the non-instrumental value of various elements of the political system are primarily directed at diffuse political objects. This means that diffuse trust judgments will be strongly influenced by beliefs formed through early political socialization, which should not vary as individuals progress through life, and to a lesser extent by objective evaluations of the system's performance; conversely, specific trust judgments will rely primarily on objective evaluations of the system's outcomes.

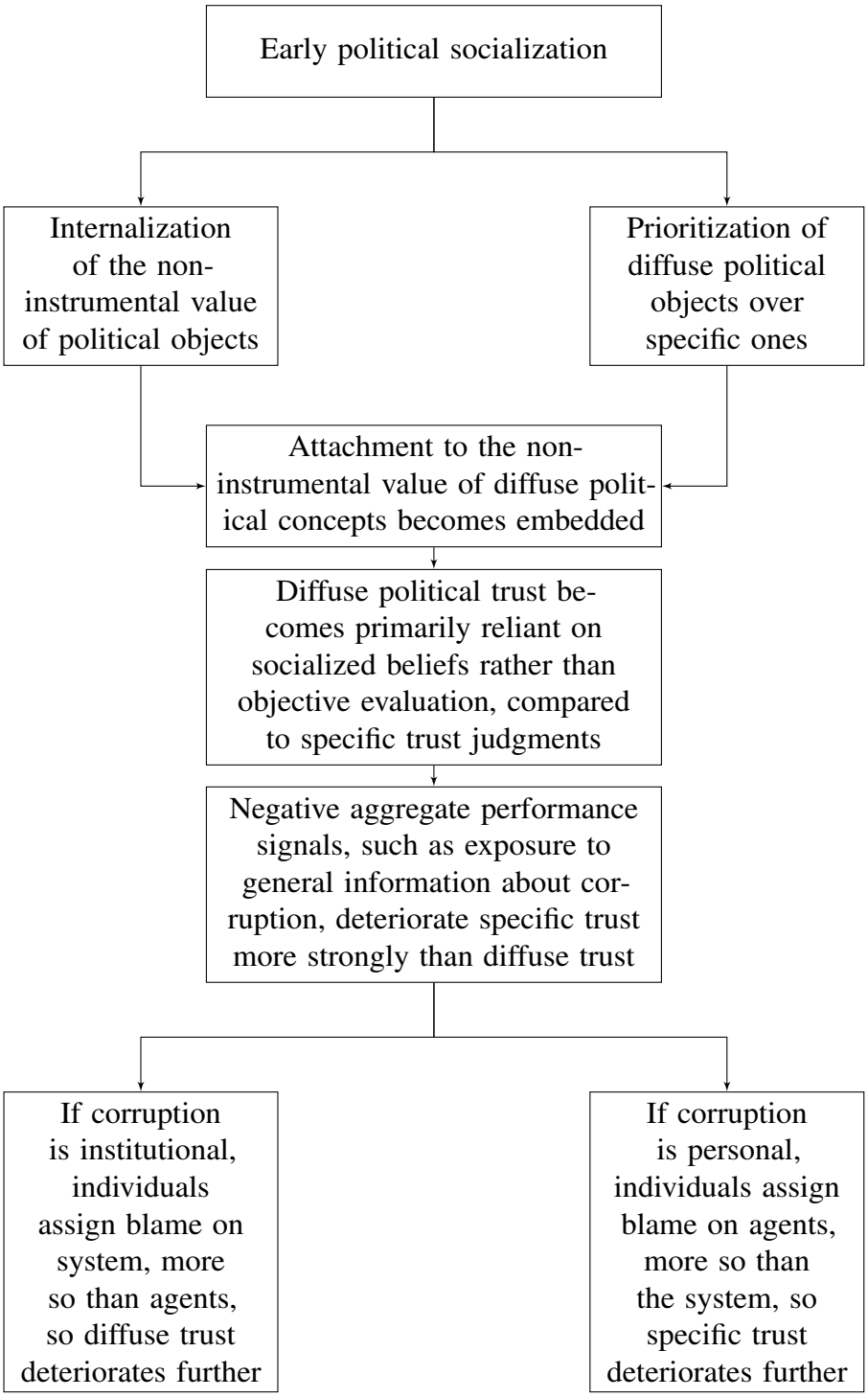
3.2 Bribery, Lobbying, and Political Trust

In this section I present the second part of this paper's theory, which addresses the differences in the ways in which the evaluative dimensions of specific and diffuse political trust operate. I argue that the evaluative dynamics that individuals rely on when producing specific and diffuse types of political trust judgments are unequally sensitive to different kinds of information, and more specifically, different manifestations of corruption. Although corruption can be referred to in a general manner, one can also make reference to its multiple modalities, which can be categorized depending on which political object is most blameworthy. Personal corruption refers to corrupt acts that occur at an individual scale and that involve an unlawful *quid pro quo* exchange on behalf of a public official seeking to use their power for private gain. In these cases, the blame is directly attributable to the particular individuals who chose to engage in the corrupt exchanges and therefore undermine the procedural order of

the system or institution within which they operate. Conversely, institutional corruption is impersonal in the sense that the particular agents involved in the act do not have the corrupt motives that characterize agents who participate in *quid pro quo* exchanges. This form of corruption also stems from legitimate institutional practices that provide benefits to otherwise uncorrupted institutions – it is therefore an integral part of the system and does not require agents to engage in morally reprimandable actions. To sum up, corruption can either be the property of institutions or individuals. The most common form of personal corruption is bribery, while lobbying or campaign contributions represent the most common embodiments of institutional corruption.

This paper argues that although both specific and diffuse types of political trust contain an evaluative dimension, they do not weigh all political performance signals equally. I predict that the most specific degrees of political trust will be particularly sensitive to exposure to corruption acts that can be categorized as personal, while trust judgments directed to more diffuse aspects of the political system will be more strongly influenced by institutional corruption. The reason for this is that personal corruption involves a morally reprimandable action where the blame can clearly be assigned to particular agents within the system but cannot necessarily be interpreted as a sign of broader or systemic dysfunction. Conversely, institutional corruption undermines diffuse trust because it embodies a weakness in the organization or process that governs a given system, rather than a failure of its agents to comply with the rules. Although no empirical work has been carried out to test this particular relationship, multiple studies have demonstrated that individuals are capable of separating personal and institutional responsibility and assigning blame accordingly. For instance, Alcañiz and Hellwig (2011) present evidence from 17 Latin American countries to show that blame for policy outcomes shifts towards international actors instead of national politicians, when countries become more integrated into global markets. Figure 3.1 summarizes the theoretical mechanisms outlined above

Fig. 3.1 Theoretical Mechanism and Related Predictions



3.3 The Case of Non-Democratic Regimes

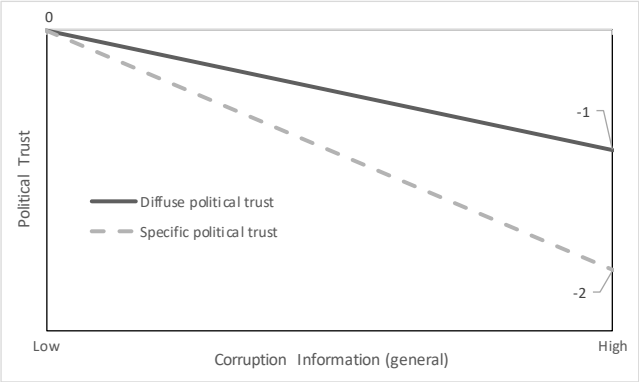
An important corollary of this paper's theoretical argument is that it should apply to any regime type, including non-democratic ones, as long there is a strong emphasis on the non-instrumental value of diffuse political concepts during the early socialization process. However, although the aforementioned emphasis is pervasive among democratic nations, it is plausible to assume that hybrid or authoritarian regimes might prefer to justify their existence by making reference to particular political leaders and therefore focus less on cultivating confidence in the system's more diffuse aspects. Another alternative would be that non-democratic or hybrid regimes could choose to encourage their citizens to develop purely instrumental attachments to the political system and thus avoid having to invest in their political socialization at all.

3.4 Hypotheses

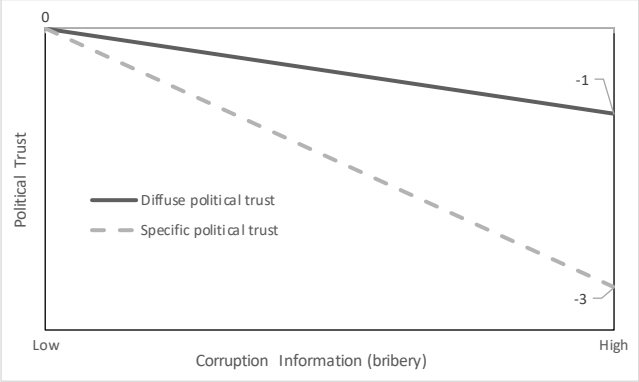
The arguments developed in the previous sections lead me to formulate the following hypotheses. Figure 3.2 presents stylized approximations of the net effects of exposure to different kinds of corruption information on the two main dimensions of political trust.

- H_1: *Exposure to general information about corruption will have a relatively stronger negative effect on specific political trust compared to diffuse political trust.*
- H_2: *If additional information is provided which associates the corruption with bribery, specific political trust will be even more strongly affected, while diffuse political trust will not be further affected relative to* H_1.
- H_3: *If additional information is provided which associates the corruption with lobbying, diffuse political trust will be even more strongly affected, while specific political trust will not be further affected relative to* H_1.
- H_4: *If additional information is provided which associates the corruption with both bribery and lobbying, specific and diffuse political trust will both be more strongly affected relative to* H_1.

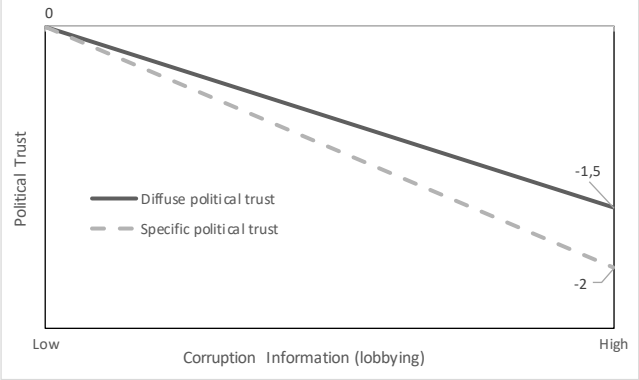
Fig. 3.2 Stylized Illustration of Empirical Expectations



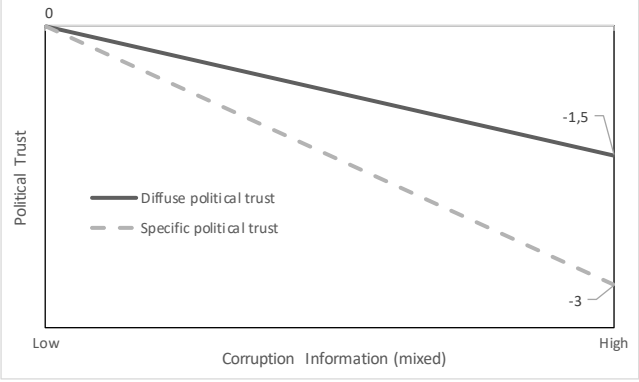
(a) H_1



(b) H_2



(c) H_3



(d) H_4

Chapter 4

Empirical Strategy

Prior studies suggest that exposure to information about corruption has a significant negative effect on reported levels of political trust but few have provided experimental evidence to evaluate this claim and even fewer have attempted to gauge the differential effect of corruption on specific and diffuse dimensions of political trust. The following chapter outlines the paper's experimental design and its empirical scope, details the case selection and data collection process, examines the operationalization of the main variables, and concludes with a discussion of key methodological challenges and where applicable, the strategies used to mitigate them.

4.1 Outline of Research Design

This thesis employs an experimental approach based on original data collected from an online vignette-based survey experiment that was fielded in March of 2020 in the United States. Approaching this paper's research question experimentally allows me to bypass a series of issues that scholars of political trust have previously been confronted to, including omitted variables bias, reverse causality, or simultaneity. For instance, prior studies have found that less trusting individuals are consistently more censorious of politicians' misbehavior and more likely to perceive the presence of corruption than are their more trusting peers (Morris and Klesner, 2010; Wroe et al., 2013). By randomly assigning respondents to treatment and control groups, I am thus able to isolate, with a high degree of confidence, the effect of the treatment on the outcomes of interest. Running my own survey also allows me to better tailor the questions and treatments that respondents are confronted to. In particular, I am able to properly disaggregate political trust along multiple dimensions, going from its most diffuse dimension to the most specific one.

The survey is structured as follows: I begin by asking respondents questions concerning several demographics, including their socioeconomic status (e.g. employment situation, income level, sector of employment), partisan attachments and ideological preferences, as well as cultural and psychological attitudes. Respondents are then randomly assigned to either a control group, which directs them straight to the outcome measurement questions, or one of the experimental treatment arms. The treatment arms differ in their content but present the information using the same format. In the main treatment group, respondents are introduced to Transparency International, a well-reputed civil society research organization, and then exposed to general information about their country's level of corruption, which consists simply of a figure identifying the total cost of corruption in the year 2019, as estimated by Transparency International. Two further treatment arms are used to disentangle the effects of personal and institutional types of corruption. In the personal corruption treatment arm, two paragraphs are added to the vignette, which serve to (1) identify bribery as the most common act of corruption, (2) describe the kind of individual-level dynamic that bribery entails, and (3) remind respondents that the law strictly prohibits offerings of value to public officials and that several institutional mechanisms have been established to enforce this norm. Conversely, in the institutional corruption treatment arm, the vignette indicates that (1) lobbying is the most common form of corruption, while also (2) detailing the legitimate institutional function it serves and (3) reminding respondents that lobbying represents an entirely legal practice. These vignettes therefore seek to clearly highlight the fact that personalized corruption is not enabled by institutional processes and that institutionalized corruption is indeed systemic and not merely a product of agentic greed. I also introduce a treatment vignette which combines information about both personal and institutional types of corruption by jointly identifying bribery and lobbying as the root causes of corruption. After receiving the treatment, respondents are confronted to a randomized battery of questions, which requires them to report their level of trust in different political entities, such as the core principles of the democratic system of government, different sets of institutions, and also politicians. In the post-treatment period, I also test the respondent's level of political sophistication by asking them a series of factual political questions. Lastly, I ask a series of questions that seek to test the causal mechanism that I developed in the previous chapter of this thesis. In brief, I inquire about how early in their lives respondents were first exposed to certain political concepts, and also ask them to identify whether they value these concepts for primarily instrumental or non-instrumental reasons. A detailed examination of the all variables captured in the survey is provided in the following sections. Table 4.1 summarizes the survey's experimental conditions.

Table 4.1 Experimental Conditions

Experimental Group	**Treatment**
Control	None
T_1	General information about costs of corruption
T_{1a}	General information about costs of corruption and identification of bribery as most common act of corruption
T_{1b}	General information about costs of corruption and identification of lobbying as most common act of corruption
T_{1c}	General information about costs of corruption and identification of both bribery and lobbying as most common acts of corruption
Placebo	Neutral information about federal aid grants and how they are spent

Given the fact that, in this study, the treatment is randomized, the difference-in-group-means will be an unbiased estimator of the average treatment effect. I use linear regression models to calculate the mean difference in trust scores of subjects in the treatment and control groups. Formally, the effect of the treatment on the respondents' reported level of political trust is measured with the regression model below:

$$Y_{ip} = \alpha + \beta_1 Treatment_i + \varepsilon_i$$

Where Y_i is the outcome of interest (i.e. level of political trust) measured at the level of the individual respondent i and across different dimensions of political trust p (i.e. specific or diffuse); T_i is a treatment indicator that takes the value of 1 for individuals that have received the treatment (i.e. were exposed to a vignette containing information about corruption) and 0 otherwise (i.e. control group); α is the intercept, which measures the average level of political trust of the respondent before the treatment was assigned; ε_i is an idiosyncratic error term; lastly, β_1 is a coefficient measuring the average treatment effect for each of the four possible treatment arms, which in this case, should be equivalent to the difference-in-group-means.

I hypothesize that the magnitude of the treatment effect will be conditioned by different respondent attributes, including their partisan and ideological preferences, level of political sophistication, and cultural characteristics. I account for treatment heterogeneity by using separate models predicting political trust with interaction terms between treatments and each of the covariates of interest.

$$Y_{ip} = \alpha + \beta_1 Treatment_i + \beta_2 Covariate + \beta_3 Treatment_i \times Covariate + \varepsilon_i$$

4.2 Data Collection, Sampling Method, and Case Selection

This paper's population of interest is composed of all the permanent residents who are aged 18 – 85 in a given democratic country. To test my theory, I use data collected from an original survey fielded in the United States in March of 2020. I used Amazon's Mechanical Turk platform to administer my survey on a non-representative US sample composed of 1,843 subjects. The average duration of the survey was 14 and a half minutes. Further details regarding scope and case selection are presented below. I choose to run the survey in the US for convenience reasons, however, the argument presented in this paper applies to any advanced democratic country.

Table 4.2 Overview of Data Source

Country	United States
Mode of collection	Online survey
Duration	14:30 minutes
Number of respondents	1,843
Number of experimental groups	6
Covariates	Socioeconomic profile, partisan and ideological preferences, political sophistication, cultural attitudes
Nationally representative	No
Platform	Amazon Mechanical Turk

4.3 Dependent Variables

Political trust (specific and diffuse). For the purpose of this thesis, trust is defined as an individual's belief that it is in the interest of others (individuals or institutions) to not do them harm. The primary dependent variable I focus on is political trust, which refers to the confidence individuals place in various political objects. I disaggregate the concept of political trust into its specific and diffuse components, based on the theoretical work performed by Easton (1965) and Norris (1999). I gauge the respondent's reported level of political trust at four different levels of abstraction, namely trust in the politicians who are holding or running for office, a number of important political offices that these politicians might occupy (e.g. the presidency, party leadership etc.), the overarching public institutions that they operate under (e.g. the executive, legislative, and judiciary branches of government) and lastly, the core principles of democratic government, such as the rule of law or equality. Trust attitudes can be conceptualized as either a binary judgment, in which case the outcome of interest is interpreted as an individual's propensity to trust, or in a more graded fashion, meaning one can trust to a greater or lesser degree. In this thesis, I adopt the latter approach as it produces an easier to interpret indicator. I therefore measure the outcomes using an 11-point Likert scale with responses ranging from "not trusting at all" to "fully trusting" a given political entity. The questions employed to determine the respondent's willingness to trust are based on World Values Survey questionnaires. Some scholars describe trust as a latent variable, meaning that it is never directly observed, but rather inferred from other variables, such as responses to attitudinal questions. Although the survey format better approximates real world environments compared to laboratory experiments, it is unable to accommodate behavioral measures of trust, which could otherwise be included in traditional laboratory experiments and are generally believed to offer a more accurate reflection of one's actual propensity to trust.

Social trust (particularized and generalized). The secondary dependent variable I integrate into my analysis is social trust. I employ measures of both generalized trust, which refers to the willingness of individuals to trust other members of their community in general, and particularized trust, which refers to one's willingness to trust people who they interact with regularly (e.g. neighbors, co-workers). Since the theoretical argument developed in this thesis is partly based on the institutional branch of the politically endogenous theory of trust formation, I expect factors that deteriorate political trust to also affect generalized and particularized social trust. Both of these concepts are measured using 11-point Likert scales with response options ranging from "needing to be very careful when dealing with others" to "most people can be trusted". Prior scholarship has argued that people extrapolate the trustworthiness of other members of their national community based on the perceived

quality of their country's formal institutions (Tao et al., 2013). This theoretical argument is grounded in the political psychology literature, which claims that individuals use the quality of formal institutions (and implicitly, their level of corruption) as a mental heuristic, or a cognitive shortcut, to produce social trust judgments (Richey and Ikeda, 2009). Empirically, analysis of cross-sectional survey data from multiple waves of the World Values Survey reveals consistent positive associations between particularized, generalized, and political trust across countries and across time. It is important to note that particularized social trust remains a relatively broad category that encompasses a number of social relationships that are not necessarily equivalent to each other. For instance, trust in a family member or close friend is likely to be stickier and less susceptible to change under the pressure of exogenous shocks, compared to the trust individuals have in people outside their immediate social circle, such as their neighbors. For this reason, the question used to gauge particularized social trust excludes family and close friends.

4.4 Control Variables

I collect a series of pre-treatment controls in order to test the robustness of treatment effects and analyze how they vary depending on the socioeconomic, cultural, psychological, and political features of the respondents. The covariates that I seek to measure can be categorized into four broad groups, namely socioeconomic attributes, partisan attachments and ideological preferences, political sophistication, and cultural and sociopsychological factors. Where applicable, I employ 7-point Likert scales to gauge the variables of interest. I discuss and justify the implementation of each of the covariate categories in the following subsections. All of the sensitive covariates that could potentially prime treated respondents or accidentally treat the control group, such as those that gauge levels of political knowledge, are measured after the treatment is administered.

Socioeconomic background. I attempt to account for the sample's diverse range of social and economic backgrounds by measuring variables such as the respondent's gender, marital status, geographical location, and ethnic background, as well as indicators of their economic status, including their income level, employment status and sector. All of these variables are typically included in studies of trust formation and scholars expect trust-related attitudes to differ quite strongly depending on one's socioeconomic profile. In fact, there is a large body of literature that illustrates how trusting attitudes are shaped by complex social dynamics that determine not only how one interacts with the world but also how people absorb and process factual information. For instance, studies find that women are generally more trusting and less likely to lose trust in others following transgression compared to men (Haselhuhn et al.,

2015). Similarly, ethno-racial differences have also been found to explain variations in trust patterns due to enduring historical and contemporary discriminatory structures (Smith, 2010).

Partisan attachments and ideological preferences. I introduce a series of measures to identify the respondents' political preferences, which include their ideological leaning and the direction of their partisan attachment. I expect all of these covariates to condition the treatment effect. Indeed, there is an extensive body of literature on motivated reasoning and affective polarization that describes how individuals interpret pieces of factual information differently depending on how these align with their pre-existing biases or positions. This argument stems primarily from the literature on voting but can be extended to other types of political value judgments and decisions, such as evaluations of trustworthiness. Campbell et al. (1980) were among the first scholars to identify partisan identification as the main explanatory force of vote choice and although this idea has been disputed intensely throughout the decades, it has not lost its relevance. In fact, several recent studies support the notion that individuals filter factual information differently depending on pre-existing opinions. For instance, Zaller et al. (1992) argue that individuals are more or less receptive to the discourse of elites on political matters depending on whether the message is in concordance with their own prior beliefs. To give another example, Gerber and Huber (2009) find that partisanship represents a strong moderator of voters' economic expectations and behavioral intentions; he finds that when Democrats win over Congress, partisans are more optimistic about the future of the economy and report higher planned consumption, while the opposite is true for Republicans. Similar mechanisms have been found to apply to trust judgments. According to Miller (1974), dissatisfaction with the policy decisions of centrist political parties was the main reason why distrust in incumbent authorities and the institutions of the state was the most common among Left and Right extremist groups in the US in the 1970s. Citrin (1974) finds that respondents who identify as Democrats are more trusting towards the president if the president is also a Democrat and less trusting if the president is a Republican. Overall, the proposition according to which the subjective interpretation of factual information varies greatly across individuals of different partisan colors, even when understood accurately, receives a significant amount of empirical support (Gaines et al., 2007; Listhaug, 1995; Taber and Lodge, 2006).

Political sophistication. Beyond ideological preferences, political sophistication also shapes individual reactions to external events. Political sophistication is a multidimensional concept, which is why I implement several distinct measures to account for all of its facets. At the most general level, I inquire about the respondent's education level – a variable which has consistently been found to be positively related to civic engagement and political knowledge (Agerberg, 2019). Previous studies have found that more highly educated people are more

likely to possess deeper factual knowledge about the negative consequences of corruption and also be more critical towards deviations in the professional conduct of public officials (Agerberg, 2019). Empirically, education has been found to be strongly associated with increasing political distrust in the face of deteriorating institutional quality or corruption (Hakhverdian and Mayne, 2012). Turning to specifically political covariates, I gauge the respondents' level of interest in politics and also implement a more direct measure of the respondents' level of political knowledge. More specifically, I use an additive index of correct answers to a series of three factual political questions of varying difficulty that cover more or less specific aspects of the country's political system. In this case, I ask respondents about the name of the Vice President of the US, the electoral method used in US elections, and the number of amendments of the Constitution. I expect these measures to condition the treatment effect in a similar way as education. Individuals who demonstrate a greater level of awareness and involvement in political affairs should be more capable of understanding the direct and indirect costs associated with corruption and therefore also be more critical towards the entities that are responsible for violating the democratic order. I expect more politically sophisticated respondents to react more strongly to exposure to information about corruption and, in particular, I predict that they will be more sensitive to the more morally complex instances of legal corruption, compared to their less politically sophisticated peers.

Cultural and psychological factors. Lastly, trust judgments depend to a large extent on the cultural framework that respondents operate in, as well as their psychological predisposition. Drawing upon the core findings associated with the sociopsychological and civil society models of trust formation, I measure the respondents' level of optimism, post-materialist outlook, as well as their personal level of awareness of corruption. According to proponents of the sociopsychological model of trust, individual predispositions, such as their level of optimism, play a decisive role in shaping their willingness to trust (Uslaner, 2000). The measure of optimism is a valuable addition to the analysis of heterogenous effects since it acts as a proxy for other important micro-level preferences, such as appetite for risk. Another key sociopsychological condition that I seek to capture in my experiment is postmaterialism. According to Inglehart (1977), postmaterialism refers to the generational transformation of individual values from predominantly materialist and economic to more abstract ones, such as autonomy or self-expression. Inglehart argued that rising prosperity in advanced industrialized democracies has gradually liberated people from the stress of basic materialistic needs, therefore creating a new set of existential goals and personal objectives in their lives. Among other things, postmaterialism has been associated with a more developed sense of social justice and critical assessment of government performance, as well as a more profound examination of the moral underpinnings of entrenched institutional frameworks, including

the democratic one. I therefore expect postmaterialist values to amplify the treatment effect in all circumstances, including when the corruption is personalized or institutionalized (Joon, 2004). To measure postmaterialist attitudes, I employ a refined version of Inglehart's postmaterialism index, which has originally been developed in the 1970s. The original index is measured through a question asking respondents to choose the two most important goals that their country should be pursuing in the near future from a list of four. The four items include the following options: maintaining the order of the nation, giving the people more say in important government decisions, fighting rising prices, and protecting freedom of speech. Respondents are asked to rank their choices in order of importance. The items relating to maintaining order and combating inflation are meant to reflect acquisitive or materialistic values, while the other two reflect postmaterialist value-orientations. Respondents can therefore be categorized as either "materialists", "postmaterialists", or "mixed", depending on the combination of items that they picked. I update Inglehart's postmaterialism index by adding two more options to the list of four, namely keeping unemployment low and reducing corruption. This allows me to introduce a pre-treatment measure of the respondents' concern for corruption in a relatively discreet manner, without running the risk of accidentally treating the control group or priming those who will receive the treatment. The unemployment option is added to ensure balance in the number of materialist and postmaterialist options given to respondents. It is a well-established fact that corruption can be a more or less culturally accepted phenomenon, which primarily depends on the frequency and nature of one's prior experiences with corrupt behavior (Lee and Guven, 2013; Rothstein, 2003). Although this argument is generally formulated with regard to cross-national comparisons, this paper acknowledges that such variations can also occur across individuals living in the same country, which is why the measure of corruption tolerance is introduced.

4.5 Time Effects

Time is a crucial factor in explaining variations in political behaviors. When dealing with the effects of early socialization, in particular, it is important to inquire whether socialization experiences that occur during the impressionable years of one's life persist until seniority or are overwritten by other experiences. The temporal dynamics of socialization can be analyzed from three distinct angles, namely age, cohort, and period effects.

Age effects. The life-cycle approach focuses on the role that biological age plays in determining people's patterns of political preferences and behaviors. According to this view, the relationship between age and political behavior follows an inverted U-shaped curve. In other words, people are most politically active in the middle of their lives, and the least active

during their youth and latest stages of the life cycle. This argument relies on the assumption that younger people are more politically inexperienced and busier with setting up their lives (Jankowski and Strate, 1995), while the elderly are less concerned with things that are out of their immediate reach and prefer to focus on more urgent issues such as health (Cutler and Bengtson, 1974). Conversely, in middle life, people tend to experience the greatest levels of mobilization pressures from society (Kinder, 2006; Lane, 1959; Strate et al., 1989), as well as the most stability (Billari and Piccarreta, 2005), which enables them to get more involved politically. Although it is unclear to what extent this approach maps on to the dynamics of trust formation, however, age remains an important conditioning factor, which is why a measure of it is also include in this paper's experiment.

Cohort effects. Despite the relevance of biological aging effects, age itself fails to account for the changing social, political, or economic events that can impact political activity across time. This is why scholars often turn to cohort effects, which refer to the behavioral and attitudinal patterns that emerge as a result of cohort members sharing similar socializing experiences (Jennings et al., 1983; Ryder Norman, 1965). In the context of this thesis, cohort effects would be particularly relevant when expanding the scope of the argument to include non- or semi-democratic regimes, or when taking into account different degrees of democratic development. For instance, this paper's theoretical model predicts that in the case of a country that has experienced a recent regime transition, younger people, who have been socialized under a new established democracy, should express greater levels of political trust than the older generation, who has been socialized to a different regime, all things being held equal. The literature on regime transitions seems to align with this prediction. For instance, Fuks et al. (2018) find that countries with stronger authoritarian legacies exhibit less support for democracy, while stronger democratic legacies have the opposite effect. Mishler and Rose (2007) find that cohorts who lived under the communist regime in Russia show a certain nostalgia for it and report to be less satisfied with democracy, compared to younger cohorts. This phenomenon has also been interpreted as the post-honeymoon disillusionment effect caused by unmet expectations (Catterberg and Moreno, 2006). However, other scholars have express skepticism with regard to these findings. While some studies find few differences between pre- and post-transition generations (Bratton et al., 2005; Chu et al., 2008), others claim that people who grew under authoritarian regimes – and experienced transition at the age of maturity – display greater commitment to democracy due to being more capable of recognizing its virtues (Moreno and Lagos, 2016).

Period effects. Lastly, patterns in political behaviors and attitudes may also result from period effects, which refer to idiosyncratic social, political, and economic events that punctuate the lives of individuals (Inglehart and Norris, 2003; Nieuwbeerta and De Graaf,

1999). Period effects are particularly relevant when they occur at the time when the attitudes are being measured. The data collected for this thesis will therefore inevitably be colored by highly salient recent events, such as the impeachment trial of President Donald Trump, or the outbreak of the coronavirus. Although time effects, including age, cohort, or period-related ones, are difficult to disentangle, they are important to acknowledge in the study of political trust judgments.

4.6 Testing the Validity of the Causal Mechanism

This thesis seeks to demonstrate that although both specific and diffuse dimensions of political trust judgments contain a socialized and evaluative component, these dimensions are not weighed equally across the two types of political trust. Diffuse political trust is more stable compared to specific trust primarily because of the attention it receives during the early political socialization process that citizens living in advanced democracies experience. In other words, a smaller fraction of one's diffuse political trust propensity will depend on objective evaluations of political performance, relative to specific trust. I therefore expect diffuse political trust to be both naturally greater and slower to change in response to external events, such as corruption, compared to specific trust. In order to strengthen the proposition that any post-treatment differences in specific and diffuse political trust levels that I identify between experimental groups is indeed caused by differences in the emphasis placed on diffuse as opposed to specific political concepts in early socialization, I introduce an additional series of questions that will allow me to test the validity of the posited mechanism.

The core of this paper's causal mechanism lies in the idea that diffuse political trust is more strongly (although not fully) dependent on early socialization compared to specific trust. To test whether this is accurate or not, I ask respondents to rank a series of more or less specific (or diffuse) political entities based on how early in their lives they learned about them. The options range from specific political actors and political parties to formal institutions (such as Congress), and finally, democracy as a political regime. I predict that the more diffuse items on the list will also be the ones that respondents came into contact with during the earlier stages of their lives. I interpret this as a signal that their early political socialization experience is likely to have revolved around the more diffuse dimensions of the political system. To increase the robustness of this causal mechanism test, I also ask respondents to rank the ideas that they associate most strongly with the democratic form of government in general and with the role played by elected public officials. I provide respondents with a list of four items, which make reference to two instrumental and two

non-instrumental expectations that people might have from the aforementioned entities. In the case of democracy as a form of government, respondents must rank items that mention democracy's ability to produce more effective laws and policies, promote social stability, protect individual rights and freedoms, and guarantee equality of opportunity. In the case of elected officials, respondents must rank items that identify the role of politicians as either effective administrators of public resources, managers of the bureaucratic apparatus, incarnations of the general will, or moral examples for society. Since in my theory I argue that democratic socialization primarily targets the non-instrumental, rather than instrumental, values of political entities, I predict that respondents will prioritize the non-instrumental value of the democratic regime as a whole and the instrumental value of elected officials.

4.7 Methodological Limitations

There are several potential issues with testing my hypotheses through an online survey experiment. Online vignette-based experiments have been criticized on accounts of both internal and external validity. The internal validity of randomized online experiments may be questioned for several different reasons. First, there is the possibility of subjects violating treatment assignment by participating in a given task more than once. In order to ensure that the same subjects do not participate in the experiment multiple times, I verify that each IP address only produces a single response. Second, subjects might be inattentive or reject the treatment, in which case, subsets of the treatment samples would end up not being properly exposed to the experimental stimuli. In order to address this problem, a manipulation check is introduced to verify whether respondents paid attention to (and assimilated the content of) the treatment. The manipulation check consists of two questions asking respondents to identify the statement which is most likely to be true among a list of four options. The first question inquiries about the cost of corruption, which relates to the general corruption vignette, and the second asks about the most common forms of corruption and thus relates to all the other treatment arms. To avoid any kind of involuntary priming of treated participants or the accidental treatment of control group subjects, the manipulation checks are only introduced after the outcomes of interest are measured. Lastly, the visual aspect of the vignette might also act as a confounding factor. To address this issue and control for the effect of the vignette format, I add a placebo treatment arm which uses the same format as the other treatment vignettes and contains an identical amount of text but presents information that is completely unrelated to corruption (i.e. a description of what federal aid funds are and how they are spent by states in the US).

Online survey experiments can also suffer from external validity issues. Having low external validity means that the conclusions that such studies can produce are not necessarily reflective of the way in which individuals behave and think in general. First, individuals who take surveys for financial returns may not be sufficiently invested in the subject matter given the context of the survey. I attempt to mitigate this issue by designing visually appealing vignettes which are modelled upon a realistic format that people would be more likely to encounter in their daily lives and want to engage with. The vignettes are modelled after the executive summaries of the yearly reports published by Transparency International. Second, using non-nationally representative samples might bias the results away from the population of interest, which in the case of this experiment is composed of all the citizens of a given country. Interestingly, scholars have previously found that the demographic characteristics of respondents recruited through online survey collection tools, such as Amazon's Mechanical Turk, are fairly representative of the population at the national level. For instance, Berinsky et al. (2012) replicate experimental studies previously conducted using convenience and nationally representative samples and find that the estimates of the average treatment effects are similar in Mechanical Turk and original samples. I try to minimize the bias created by non-representativeness by engaging a relatively large sample size of over 1,843 respondents. Lastly, I perform balance tests by comparing the differences in observable pre-treatment covariates across experimental groups via t-tests in order to ensure that the randomization was indeed successful.

This paper's empirical strategy faces other limitations that cannot be as easily overcome but that need to be acknowledged, nevertheless. First, vignette experiments may overestimate the effect of a treatment through the priming of respondents, which would call their generalizability into question. For instance, it might be that individuals who encounter the same kind of information in their daily lives would not think twice about it. In fact, individuals rarely make decisions, such as choosing whether or not to trust a politician or an institution, based on single issues, such as corruption, which in this case will be the only piece of information that is presented to them. Second, the attitudes and preferences reported in a survey are not necessarily genuine reflections of people's thoughts but might simply indicate their willingness to conform to the expectations of the survey-giver (Krupnikov and Levine, 2011). Unlike surveys conducted face-to-face or through the phone, however, online surveys are more impersonal, which could reduce the social desirability bias that is heavily present in traditional surveying techniques. Third, information leakage can also be problematic in the context of vignette experiments. For instance, exposing subjects to information about a country's level of corruption may lead them to draw inferences about other features of the respective country, such as whether it is poor, safe, or ethnically fragmented, which might

in turn contaminate the dependent variable measurements. I partially mitigate the risk of respondents extrapolating other attributes of the country by maintaining a tight focus on corruption issues. Fourth, vignettes may also generate framing bias depending on the way in which information is presented to the respondents. I attempt to minimize this risk by adopting a neutral and purely descriptive tone in the content of the vignettes. Lastly, there is a concern regarding the size of the treatment effect in the context of vignette experiments: when dealing with highly salient issues, it is possible for respondents to have already been confronted to similar kinds of information beforehand, with their attitudes and beliefs shifting in response to them, which would predict a substantively small, if not null, treatment effect. Although corruption itself is a salient issue on mainstream news media platforms, people are relatively poorly informed with regard to its economic costs and most common manifestations. Exposing them to a credible measure of corruption's actual cost and connecting it to either personal or institutional acts of corruption is therefore likely to offer them a new perspective on an otherwise mainstream issue, which could plausibly shift their attitudes about the trustworthiness of various political objects.

Chapter 5

Results

This thesis argues that the effect of corruption revelations on political trust depends both on the dimension of political trust that is being measured and the ease with which individuals can attribute blame on the basis of the revelation. At the core of this argument lies the idea that political trust can be disaggregated along a spectrum of abstraction, which includes the trust that individuals have in specific political actors as well as their confidence in institutions and more diffuse political concepts, such as the values or foundational principles of a given political regime. In my theory, I argue that specific and diffuse types of political trust are both the products of socialization, on the one hand, and objective evaluation efforts, on the other. However, these two factors are weighed differently depending on the level of abstraction at which trust judgments are made: trust in diffuse political objects depends more heavily on early socializing experiences relative to trust in specific political objects. This in turn implies that diffuse trust will also be less reliant on objective evaluations of performance. To justify this, I posit that early political socialization in democracies focuses primarily on developing an understanding and appreciation for the non-instrumental value of the diffuse, rather than specific, dimensions of the political regime. The following sections of this chapter are therefore dedicated to exploring the data in order to test whether the aforementioned relationships and mechanisms find empirical support. This chapter is organized as follows: first, I analyze the composition of my sample and perform randomization checks to ensure that the distribution of covariates is balanced across experimental groups; second, I demonstrate that the treatments used in my experiment have indeed been assimilated by respondents by performing a series of manipulation checks; third, I measure average treatment effects using linear regressions, while also performing multiple robustness checks, which include incorporating control variables and replicating the core models with different modelling techniques; lastly, I perform a series of auxiliary analyses, which including measuring

potential heterogenous treatment effects through interaction terms and testing the validity causal mechanism.

5.1 Descriptive Statistics

A few key facts deserve to be pointed out with regard to the composition of the sample. First, the sample is predominantly composed of males, which make up over 60% of each experimental group. This indicates that my results will be to a certain extent skewed with relation to gender, which has been shown to be a strong determinant of trust formation. In terms of age, the median value of the sample is approximately 37 years old, with the youngest respondents being aged 18 and the oldest 76. This figure is almost completely in line with the national US median age of 38, which means that although this sample is slightly younger than a nationally representative one, age will not necessarily represent a strong source of bias. The sample is also quite ethnically diverse, with 62.5% of respondents identifying as White, only 8.1% identifying as Black, and 23.5% identifying as Asian. These figures indicate a meaningful degree of imbalance in terms of ethnic background, as Whites and Blacks are underrepresented in comparison to the racial composition of the US as a whole, while Asians are heavily overrepresented, given that they only account for 5.6% of the total US population. In terms of education, it is clear that the sample covers the more highly educated share of the population, as 47.4% of respondents declare having obtained a bachelor's degree and 22.5% a master's degree, which both represent meaningful increases from the national figures of 37% and 9.3% respectively. Conversely, only 7.4% of the sample chose high school as the highest level of education attainment. Turning now to the respondents' economic profiles, a vast majority of the sample is composed of either full-time employees or self-employed individuals, which represent 67.9% and 12.3% of the sample respectively. Only 4.1% of the sample claimed to be unemployed, which is in line with the 4.4% US national unemployment figure for March 2020. In terms of income, 52.6% of respondents situate themselves in the category whose income ranges from $25,000 to $75,000. However, it is worth nothing that almost 30% of the sample have an annual income of under $25,000, compared to approximately 20% of US households, which indicates that the lower income groups are disproportionately represented in the sample. Lastly, in terms of political preferences, the sample also seems to skew heavily towards the liberal side of the ideological spectrum, as 42.2% of respondents report having a preference for the Democratic Party, compared to only 28.6% of them leaning towards the Republicans. To sum up, the demographic composition of this sample seems to be skewed towards relatively young, highly educated, and low-income liberals. Beyond the demographic profile of the respondents, the data does not seem to suffer

from any functional problems. None of the demographic variables with the exception of the question measuring the respondents' sense of national pride have any missing values. Table 5.1 reports the descriptive statistics for a selection of the main demographic covariates across all six experimental groups.

Table 5.1 Descriptive Statistics

Variable	**Levels**	**n**	%	∑%
Gender	Female	680	36.9	36.9
	Male	1159	62.9	99.8
	Other	4	0.2	100.0
	all	1843	100.0	
Education Level	Associate Degree	152	8.2	8.2
	Bachelor's Degree	874	47.4	55.7
	Doctorate or professional degree	41	2.2	57.9
	High school	137	7.4	65.3
	Master's degree	415	22.5	87.8
	Some college	224	12.2	100.0
	all	1843	100.0	
Race	Arab	3	0.2	0.2
	Asian	433	23.5	23.6
	Black	150	8.1	31.8
	Latino	67	3.6	35.4
	Multiracial	25	1.4	36.8
	Other	13	0.7	37.5
	White	1152	62.5	100.0
	all	1843	100.0	
Income	$25,000 to $49,999	530	28.8	28.8
	$50,000 to $74,999	439	23.8	52.6
	$75,000 to $99,999	195	10.6	63.2
	Less than $25,000	550	29.8	93.0
	More than $100,000	129	7.0	100.0
	all	1843	100.0	
Ideology	Conservative	782	42.4	42.4
	Liberal	683	37.1	79.5
	Moderate	378	20.5	100.0
	all	1843	100.0	

As a preliminary precaution, I perform a set of randomization checks in order to ensure that the distribution of covariates across experimental groups is indeed balanced. First, I use a series of t-tests to measure whether there is any statistically significant difference in the means of the core demographic covariates included in Table 5 across treated and non-treated

groups. Although these calculations are not directly reported in this paper, I find that there is no significant difference in the means of the aforementioned demographic variables. I also build a plot of the absolute mean differences of a selection of the survey's covariates to facilitate the visual detection of potential imbalances across treated and control groups. Figure A.1 demonstrates that randomization was indeed successful as variations in absolute mean differences of covariates are essentially non-existent.

5.2 Primary Results

In this section, I test the paper's central hypotheses. H_1 claims that corruption revelations that do not offer sufficient information for individuals to effectively attribute blame to more or less specific political objects will disproportionately affect specific, more so than diffuse, political trust. H_2 predicts that when the corruption information that individuals are exposed to contains information in which responsibility can be directly assigned to individual agents, specific political trust should decrease even further compared to diffuse trust. H_3 claims that the opposite outcome should be observed when the corruption revelations highlight institutional or procedural shortcomings, rather than individually reprimandable actions. Lastly, H_4 predicts that when the corruption revelation includes a mix of all the aforementioned types of information, political trust should be strongly negatively affected at all levels. I test these hypotheses experimentally by exposing survey respondents to a series of vignettes containing different kinds of information about the state of corruption in their country. The basic treatment vignette simply reports the economic cost of corruption over the year 2019 (in the US) but does not make any reference to the institutional processes, political officeholders, or private interest groups that might have been responsible for the economic loss associated with that level of corruption. Three additional vignettes build upon the first one and include a breakdown of the most common corruption type. I use bribery as the prime example of an instance of corruption in which blame can be directly attributed to individuals operating within the system, rather than impersonal entities, such as institutions, or more abstract values. Conversely, I use lobbying as the prime example of an instance of corruption in which blame is more likely to be placed on the institutions, or the system as a whole, given that it represents an entirely legal practice.

5.2.1 Manipulation Checks

In order to test whether the vignettes were successful in updating the respondents' beliefs about the cost and most common types of corruption in their country, I include several post-

treatment manipulation checks in the form of questions asking respondents to identify the statements most likely to be true among a list of four options. I use one question that concerns the costs of corruption, with the response option "the cost of corruption amounts to 5% of GDP" being the correct one, and I use another which contains two response options claiming that either lobbying or bribery are "the most common forms of corruption". Based on the respondents' answers to these two questions, I calculate differences in average responses across all experimental groups in order to observe whether those who were treated were more likely to choose the correct options compared to those who were not. Table 5.2 reports differences in average responses to the manipulation check with the control condition. First, it is clear from this table that the vignette fragment concerning the economic cost of corruption as a whole was successful in updating the respondents' knowledge of the cost of corruption, as there is a statistically significant difference in the average correct response rates between all treatment and control groups. In line with my expectations, there is no statistically significant difference in the average responses of the placebo and control groups. It is interesting to note that respondents in the core treatment group were the most likely ones to answer this question correctly, which can be explained by the fact that they had to assimilate a lower quantity of information, which would be easier to memorize, compared to subjects assigned to any of the other treatment arms. The coefficients presented in the table also demonstrate that the three other treatment vignettes had the intended effects on the respondents' corruption perceptions. With regard to the other manipulation checks, respondents who were assigned to the bribery vignette were the most likely to identify bribery as the most common form of corruption, and the least likely to claim that it was lobbying. Conversely, respondents in the lobbying treatment arm were the most likely to identify lobbying as the most common form of corruption and the least likely to claim that it was bribery. Lastly, respondents in the mixed treatment arm were more likely to pick one of the two correct options from the list of four but there is no significant preference in either one, which means that the vignette was successful in exposing them to two equally strong informational stimuli.

5.2.2 Frequency of Political Trust Responses

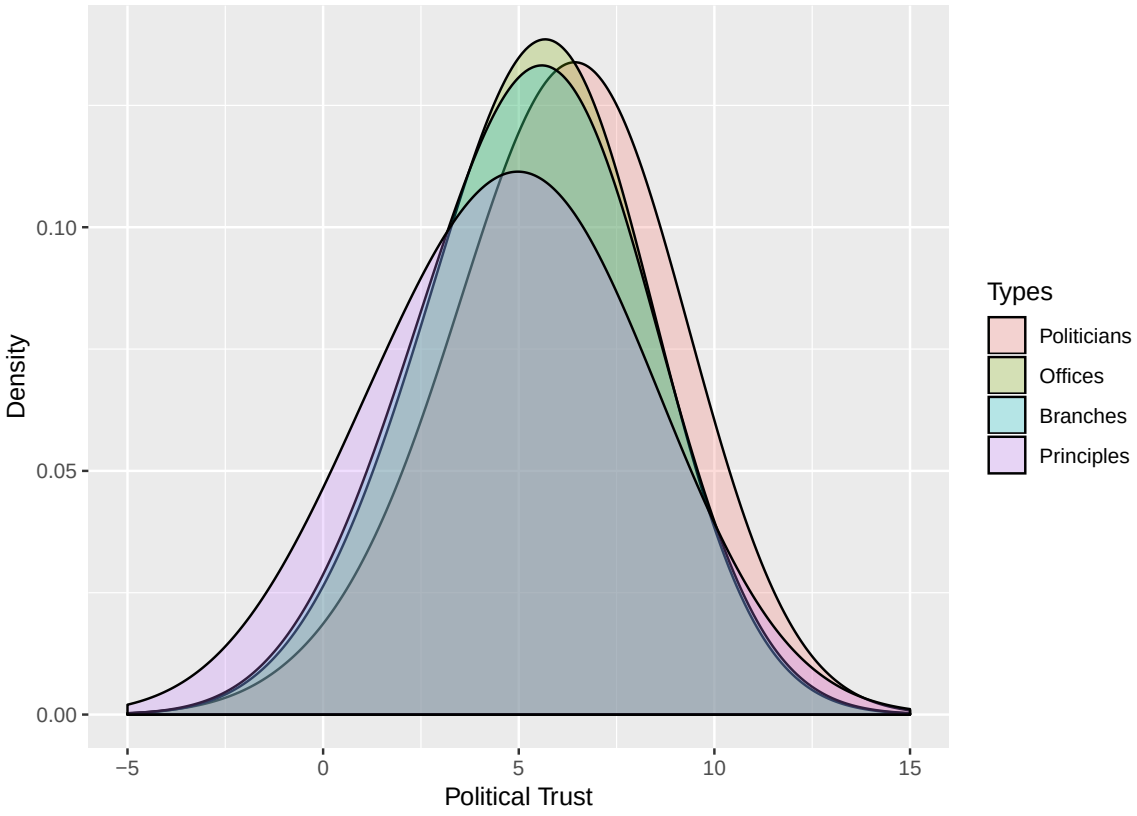
The first corollary of the theoretical argument developed in this paper is that we should expect diffuse political trust to be naturally higher than specific trust. If early political socialization focuses more strongly on building confidence in diffuse political objects, thus leaving less room for evaluation as a driver of trust formation, we should expect reported levels of diffuse political trust to be higher than more specific ones. Figure 5.1 plots the density of political trust responses alongside the four dimensions along which I measure the dependent variable. These categories range from the most specific political objects, namely

Table 5.2 Manipulation Checks for Mechanical Turk Experiment

Experimental Group	**Cost of Corruption**	**Bribery**	**Lobbying**
Control	0.128	0.272	035
Placebo	0.119	0.222	0.427
T_1	0.469**	0.216**	0.436
T_{1a}	0.438**	0.429**	0.268**
T_{1b}	0.39**	0.163**	0.511**
T_{1c}	0.452**	0.3	0.41

the politicians who are either running for or currently holding office, to the most diffuse ones, namely the core principles of the democratic system of government. I measure trust attitudes at two additional intermediary levels of abstraction, which include political offices, such as the Presidency or party leadership, and the overarching institutions that compose the main branches of government, such as Congress or the Supreme Court. Although the conceptual distinction between political offices and actors might seem unnecessary, prior scholarship has demonstrated that individuals are capable of distinguishing between political actors and the institutional structures that they represent (Alcañiz and Hellwig, 2011; Norris, 1999). The density plots effectively illustrate how the mean levels of political trust, which I measure using 11-point Likert scales, diverge depending on the level of abstraction that is being measured: as predicted by theory, diffuse trust in the principles of democracy benefits from the highest level of confidence on average across all experimental groups (5.34), followed by trust in the main branches of government (5.36), trust in political offices (5.31), and lastly, trust in politicians (4.78).

Fig. 5.1 Density of Political Trust Responses Across Experimental Groups



5.2.3 Linear Regressions of Political Trust by Experimental Condition

I run several linear regression models to measure average treatment effects on different levels of political trust. I gauge the dependent variable of political trust at four levels abstraction: I ask respondents to report their level of trust in the most specific kind of political object, namely active politicians, political offices, such as the Presidency or Governorship, overarching formal institutions, such as Congress or the Supreme Court, and lastly, the core principles of the democratic system of government, such as the rule of law and equality. By breaking down the measurement of political trust into these key four indicators, I seek to cover the dependent variable's full conceptual range, moving progressively from its most specific aspect, to its most diffuse. I measure trust on an 11-point scale and build multi-item indices of trust scores for the two measures of institutional trust. The regressions are structured as follows: first, I regress the dependent variables on dummy variables that categorize respondents by the experimental group to which they are assigned; second, I replicate these models to incorporate all the relevant control variables, which include those relating to the respondents' socioeconomic background, their ideological and partisan preferences, their cultural attitudes, and degree of political sophistication.

Table 5.3 presents eight models in which the four measures of political trust, which I treat as continuous variables, are regressed on treatment group dummies and other covariates. Models (1) and (2) refer to trust in politicians who are either holding or running for office, models (3) and (4) cover trust in political offices as institutions rather than individuals, models (5) and (6) refer to trust in the executive, legislative, and judiciary branches of the federal government, and lastly, models (7) and (8) refer to the respondents' confidence in the abstract principles of democratic government, such as the rule of law or equality. The cell entries represent coefficients from ordinary least squares regressions of political trust scores on treatment groups, using the control group as the reference category. I also calculate robust standard errors, which are shown in parentheses. The coefficients estimated in this table paint a complex picture of the relation between corruption revelations and political trust judgments. The first result to note in the experiment is that across all treatment arms and dependent variable measurements, exposure to information about corruption has a negative effect on reported levels of political trust. This finding aligns with the conclusions of prior scholarship, which claim that revealing information about corruption has a corrosive effect on political trust and is unlikely to inspire hope and boost confidence in the political system. Secondly, it is worth noting that the value of the adjusted R-squared ranges between 0.002 and 0.01 across all of the models, which indicates that only a small proportion of the variance in the dependent variable is explained by the independent effect of the treatments. This value

increases significantly to 0.15 in model (8) and jumps to 0.3 in models (2), (4), and (6), in which all the covariates are added.

Table 5.3 Linear Regression Models of Political Trust by Experimental Condition

	Politicians OLS		Offices OLS		Institutions OLS		Principles OLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment 1 (General)	−0.09	−0.33*	−0.26	−0.37*	−0.13	−0.17	−0.40*	−0.39*
	(0.17)	(0.16)	(0.17)	(0.16)	(0.17)	(0.16)	(0.19)	(0.19)
Treatment 1a (Bribery)	−0.46**	−0.44**	−0.38*	−0.42**	−0.25	−0.11	−0.72***	−0.65**
	(0.18)	(0.17)	(0.17)	(0.16)	(0.18)	(0.17)	(0.20)	(0.20)
Treatment 1b (Lobbying)	−0.24	−0.44**	−0.27	−0.38*	−0.30	−0.31	−0.62**	−0.64***
	(0.17)	(0.16)	(0.17)	(0.16)	(0.17)	(0.16)	(0.19)	(0.19)
Treatment 1c (Mixed)	−0.36*	−0.50**	−0.39*	−0.47**	−0.39*	−0.35*	−0.71***	−0.66***
	(0.17)	(0.16)	(0.17)	(0.15)	(0.17)	(0.15)	(0.20)	(0.19)
Age		−0.005		0.002		−0.002		0.01*
		(0.01)		(0.01)		(0.01)		(0.01)
Female		−0.23*		−0.09		0.01		−0.07
		(0.12)		(0.11)		(0.11)		(0.14)
Education		0.001		0.05		−0.0004		−0.03
		(0.03)		(0.03)		(0.03)		(0.04)
Income		−0.11**		−0.10*		−0.04		−0.02
		(0.04)		(0.04)		(0.04)		(0.05)
Constant	5.01***	10.51***	5.56***	4.89**	5.56***	5.72***	6.58***	5.42*
	(0.12)	(1.17)	(0.12)	(1.71)	(0.12)	(1.60)	(0.14)	(2.23)
N	1,535	1,187	1,535	1,187	1,535	1,187	1,535	1,518
Adjusted R^2	0.002	0.31	0.002	0.35	0.002	0.31	0.01	0.14

*p < .05; **p < .01; ***p < .001

I begin the analysis of the treatment effects by focusing on the coefficients associated with *Treatment 1*, which exposes respondents to general information about their country's level of corruption, without identifying its causes. The hypothesis according to which agnostic corruption revelations should primarily affect the specific dimensions of political trust finds only partial empirical support. Surprisingly, it appears that trust in the diffuse principles of democracy is the most strongly affected by *Treatment 1*, as the absolute value of the treatment effect calculated in model (7) is substantively larger (– 0.40) than for every other dimension of the dependent variable, while also reaching strong statistical significance at the 5% level. This means that the average degree of trust in the principles of democracy reported by respondents who were exposed to the vignette informing them of the total cost of corruption in their country was 6.18 out of 11, compared to the control group's average of 6.58. The absolute value of the treatment effect increases slightly in the fully specified model (8), which includes all the control variables, reaching – 0.43. However, H_1 seems to

find some level of support when comparing the average effects of *Treatment 1* on the other dimensions of political trust. The absolute value of the treatment effects on *Politicians* and *Offices*, which represent the two most specific dimensions of political trust, are – 0.31 and – 0.38 respectively. These values are in fact substantively larger than the treatment effect on trust in the main branches of government, which is only – 0.19. It is also important to note that the latter effect fails to reach statistical significance in both the core and fully specified models. This falls in line with the expectation that presenting individuals with information about corruption that is not directly associated with a certain category of political objects pushes people to attribute blame on the basis of their preexisting beliefs about the importance of scrutinizing more or less specific political entities. Although the – 0.07 point difference in the magnitude of the treatment effect between the fully specified models (4) and (2), which regress trust in political offices and politicians on treatment dummies and covariates, casts some doubt over whether individuals confronted with responsibility-agnostic corruption revelations indeed prioritize political agents when attributing blame, the stark contrast between either of those coefficients and the absence of a significant effect on trust in the branches of government indicates that individuals are less inclined to conceptualize corruption as a systemic phenomenon unless it is specifically described as such. To sum up, it seems that exposure to information about corruption has a meaningful negative effect on reported levels of political trust in politicians and the offices that they occupy, as well as in the broader principles of democratic government. However, trust in the executive, legislative, and judiciary branches of government does not appear to deteriorate when the corruption revelation remains impartial with regard to the attribution of blame. Overall, this finding partly aligns with the expectations formulated in H_1 with regard to the prioritization of specific over diffuse political objects when attempting to attribute blame in contexts of limited information. However, the size and significance of the treatment effect on the most diffuse measure of political trust leads me to question whether abstract principles and formal institutions should be treated as belonging to the same conceptual scale.

The coefficients associated with *Treatment* 1_a, which exposes respondents to a vignette in which the information regarding the cost of corruption is supplemented by the identification of active bribery as the most common form of corruption, seem to reflect a similar narrative as those of *Treatment 1*. For this treatment arm, I also expected to witness a negative effect across the board accompanied by an even more profound relative deterioration in the more specific dimensions political trust compared to the more diffuse ones. Similar to the previous treatment arm, this hypothesis finds partial empirical support. Yet again, trust in the principles of democracy is the most negatively affected quantity, as the average treatment effects range from – 0.72 in the unspecified model to – 0.73 in the fully specified one. These

results also achieve statistical significance at the 1% level. While this finding goes against the prediction formulated in H_2, the rest of the coefficients behave entirely as expected. Excluding the final two models, models (1) and (2), which concern the respondents' trust in political candidates and officeholders, showcase the largest average treatment effects: the magnitude of the *Treatment* 1_a effect is – 0.46 in model (1), decreasing slightly to – 0.43 after the inclusion of covariates in model (2) but remaining statistically significant at the 5% level. These figures are slightly larger than those presented in models (3) and (4), which hold trust in political offices as the dependent variable and amount to – 0.38 and – 0.42, while also achieving statistical significance at the 5 and 1% levels respectively. Trust in the various branches of federal government are once again unaffected by the treatment vignette: not only are the coefficients substantively small in comparison the all the other models but they also fail to reach statistical significance. This can be interpreted as evidence that when individuals are exposed to corruption information in which the blame can be directly attributed to specific political entities, distrust in those categories of political objects will increase, without necessarily affecting one's confidence in the country's institutional framework as a whole. Nevertheless, the magnitude of the *Treatment* 1_a effects on the most diffuse measure of political trust remains difficult to explain. Ultimately, it seems that the expectation that democratically socialized individuals are less intransigent when it comes to updating their trust in abstract political values fails to gather meaningful empirical support. To summarize, the experimental findings related to *Treatment* 1_a fall broadly in line with those of *Treatment 1* and partially confirm the expectations formulated in H_2: although exposing survey respondents to corruption information in which responsibility is personal rather than institutional does indeed have a more profound corrosive effect on the specific dimensions of political trust, diffuse trust in the most abstract aspects of the democratic system is also negatively impacted despite the absence of any clear indication that corruption is in fact systemic.

The coefficients produced by *Treatment* 1_b, which exposes survey respondents to information about the economic cost of corruption that is supplemented by the identification of institutional processes as the key enabling factor, also align partially with the predictions formulated in H_3. First, it is interesting to note that although the absolute value of the average treatment effect on trust in the principles of democracy remains substantively large and strongly statistically significant (at the 1% level), the magnitude of the coefficient is slightly smaller than the one calculated for the bribery treatment arm (– 0.62 compared to – 0.72). This is unexpected since my theory predicted that instances of corruption in which responsibility is assigned to impersonal entities, such as institutional procedures or structures, should trigger a more profound deterioration in the people's confidence in the diffuse aspects

of democracy compared to instances in which the corruption is primarily presented as the product of agentic misbehavior. This slight discrepancy might be explained by the fact that despite being described as corrupt, lobbying is not perceived as a perversion of the democratic theoretical framework to the same extent that bribery is. Interestingly, however, the other coefficients associated with *Treatment* 1_b appear to largely support my theoretical predictions. The absolute value of the effect of the lobbying treatment arm on the respondents' trust in political candidates and officeholders is the smallest across all core models: – 0.24 compared to – 0.27 for trust in political offices and – 0.30 for trust in the three branches of government. Additionally, this coefficient is the only one in the list that fails to reach statistical significance, while all of its counterparts achieve it, albeit at the 10% level. In the fully specified models, however, the absolute value of the coefficient for trust in politicians almost doubles to – 0.46 and reaches statistical significance at the 5% level, which is surprising, and casts doubt over whether lobbying is indeed primarily conceptualized as a procedural flaw, rather than an individual-level issue. The most meaningful change compared to the previous treatment arm is the increase in the absolute value of the treatment effect on trust in the institutions forming the executive, legislative, and judiciary branches of government: in the fully specified model (6), this value goes up to – 0.32 and reaches statistical significance at the 5% level. This shows that lobbying, which is identified in the treatment vignettes as a legal yet corrupt practice, does indeed trigger a meaningful deterioration of trust in the overarching institutions of the state, while no such effect is observed when lobbying is not mentioned. Summing up, *Treatment* 1_b seems to confirm the pattern outlined across all treatment arms, namely that trust in the abstract principles of democracy is highly sensitive to corruption information. Additionally, these results also show us that trust in institutions tends to be relatively resilient and only goes down when the corruption is explicitly associated to the permissiveness of a system's procedures and rules. Additionally, judgments concerning trust in political actors are also quite sensitive to corruption revelations, regardless of the type of corruption that is being described, but to a lesser extent than institutional trust.

Lastly, the average effects of *Treatment* 1_c, which exposed respondents to a vignette that simultaneously identified bribery and lobbying as the most common forms of corruption, fall in line with my theoretical expectations. As predicted by H_4, the coefficients associated with this treatment arm combine the results produced by the previous vignettes. First, the absolute values of the treatment effect on the two measures used to capture trust in specific political entities, namely political actors and offices, are largely similar to the effects of *Treatment* 1_a, amounting to – 0.46 and – 0.47 in the fully specified models. Trust in the overarching institutions of the state also follows the same trend, decreasing by 0.35 points. All of these coefficients achieve statistical significance at the 5% level. Lastly, trust in the principles

of democracy is yet again the most deeply affected value, decreasing by 0.63 points and achieving strong statistical significance at the 1% level. It is interesting to note that with the exception of models (7) and (8), the absolute value of the treatment effects measured in each of the other fully specified models is in fact slightly larger compared to their equivalent quantities in the other treatment arms. This indicates that there is an interactive effect that is generated by the simultaneous exposure to different types of corruption. Although the difference in the magnitude of *Treatment* 1_c's effects compared to the highest values achieved by other treatment arms is substantively small (– 0.03 for trust in *Politicians*, – 0.05 for trust in *Offices*, and – 0.9 for trust in *Branches*), it demonstrates that revelations which cover a broader range of corruption modalities can amplify feelings of frustration and influence respondents to be even more critical in their evaluations of the trustworthiness of various political objects.

The question then remains why all of the treatment effects on trust in the abstract principles of democracy are the largest in absolute terms. Unlike institutional trust, variations in trust in the principles of democracy do not seem to depend on signals associating corruption to institutional structures or procedures. This finding contradicts my theory, according to which individuals are strongly socialized to the non-instrumental value of abstract political concepts and therefore less likely to adjust their levels of diffuse political trust when confronted to corruption revelations, unless they can be interpreted as evidence of systemic dysfunction. There are multiple possible explanations for this surprising finding. First, it could be that early political socialization does not actually emphasize abstract concepts, as much as it does institutional structures, which could account for the discrepancy between the two most diffuse measures of political trust employed in the experiment. This explanation remains unsatisfactory, however, since reported levels of trust in the abstract principles of democracy are the highest in the control group, meaning that people do indeed seem to naturally value them to a greater extent than the other more specific political objects. Another possible explanation is that individuals might expect abstract values to play a greater role in regulating the behavior of agents within a political system and preventing them from engaging in morally reprimandable acts of corruption compared to formal institutional structures, which they might judge on more practical rather than moral ground. In other words, specific political agents are indeed the first ones to shoulder the blame for corruption, especially in the case of bribery, however, individuals value the moral dimension of abstract principles more so than that of formal institutions, which means that the frustration caused by unmet expectations will also be stronger. This could ultimately mean that the evaluative dimension of diffuse political trust is not only disproportionately sensitive to evidence of systemic dysfunction, but

also to moral, rather than practical, outcomes. In any case, it is clear that this topic requires further theoretical inquiry.

5.2.4 Linear Regression Models of Social Trust by Experimental Condition

Although this paper focuses primarily on the relation between corruption and political trust, I also conduct an auxiliary analysis of the treatments' effect on two measures of social trust. Table 5.4 contains four models which describe the relationship between the various treatment arms and reported levels of generalized and particularized social trust. Structurally, the regressions use the same formula applied in Table 5.3. This piece of analysis is important because it allows me to better gauge the societal implications of corruption revelations Theoretically, it would be plausible to assume that individuals use the performance of the leaders and institutions of the state to infer the trustworthiness of other members of their society. This idea is rooted in the literature on cognitive heuristics. In psychology, heuristics are described as simple strategies that people use to form judgments and make decisions when confronted with complex problems. These decision-making rules-of-thumb tend to be the most useful in contexts where information is limited or too complex to be processed efficiently by humans, such as it is the case for the formulation of trust judgments. A particular kind of heuristic, namely representativeness, claims that people expect instances of a broader category to possess the characteristics of the category which they belong to (Tversky and Kahneman, 1974). Representativeness is a form of associative thinking, which entails that if the salient prototypical features of a given category change, so should the people's attitudes towards the instances that are being categorized by it. In this case, the conceptual category whose performance we are interested in assessing is the state, and its citizens represent the instances upon which those value judgments may or may not translate. Therefore, I expect exposure to information about corruption to principally negatively impact generalized social trust, as individuals update their assessment of the trustworthiness of other members of their community based on the salient features of the agents and institutions that represent the state. Particularized trust, however, should not be as strongly affected since the representativeness heuristic is should not be required when dealing with people that one knows personally. Given the fact that the analysis of social trust is beyond the theoretical scope of this paper, I refrain from formulating a set of formal hypotheses and approach this exercise as a descriptive complement to the core analysis.

The coefficients presented in Table 5.4 reveal that exposure to information about corruption always has a corrosive effect on reported levels of social trust, as the direction of

the average treatment effects is negative across all treatment arms and model specifications. It is interesting to note, however, that the treatment effects on particularized social trust, which refers to the kind of trust individuals have in people they interact with on a regular basis, fail to reach statistical significance, while the effects of the treatments on generalized social trust reach statistical significance at the 10% or 1% level more often than not. This broad observation indicates that generalized trust is more sensitive to corruption revelations, compared to particularized trust, which is a theoretically consistent outcome since particularized trust is likely to be more reliant on direct, repeated interactions compared to its generalized counterpart. Generalized social trust judgments tend to be more dependent on heuristics because of the cognitive and informational barriers associated with the evaluation of the trustworthiness of a large group of people. Particularized trust, however, is far more limited in its scope and therefore relies primarily, if not exclusively, on direct face-to-face interactions, rather than complicated inferences based on cognitive shortcuts. Despite the clear separation between these two types of social trust however, it is interesting to observe that the magnitude of the average treatment effects on generalized trust differ quite strongly depending on the treatment arm. First, *Treatment 1*, which only informs respondents of the economic cost of corruption in their country, without including any reference to the agents or processes that are responsible it, reduces the respondents' level of reported generalized trust by 0.38, as shown in the fully specified model (4). This shows that corruption information at its most general level indeed leads people to re-evaluate the trustworthiness of distant fellow co-nationals. Interestingly, this effect is far more pronounced in the lobbying treatment arm (– 0.72) than in the bribery one (– 0.21), which could be interpreted as evidence that people do not extrapolate beliefs in the trustworthiness of others on the basis of the individual misbehavior of political agents, but only system-wide valence cues. This might also imply that institutions are generally seen as more genuine manifestations of a community's values and behaviors, while political agents are perceived as a separate entity that is not necessarily representative of the community as a whole. The mixed treatment arm produces a strongly statistically significant effect of – 0.58, which implies that exposure to information about both bribery and lobbying attenuates the negative impact of institutional corruption. This could occur because the aforementioned separation between politicians and citizens in general is transferred to a certain extent on to the institutions when these are associated with specific political actors. In sum, Table 5.4 reveals that exposure to information about corruption, in cases where the blame can and cannot be attributed to a certain category of political objects, does not have a significant impact on the kind of trust judgments that individuals apply to people they interact with regularly. However, corruption revelations do seem to have a

meaningful corrosive effect on trust in members of a national community, in particular when the responsibility for said corruption is more institutional than personal.

Table 5.4 Linear Regression Models of Social Trust by Experimental Condition

	Particularized OLS		Generalized OLS	
	(1)	(2)	(3)	(4)
Treatment 1 (General)	−0.15	−0.04	−0.31	−0.39
	(0.18)	(0.20)	(0.19)	(0.22)
Treatment 1a (Bribery)	−0.20	−0.13	−0.19	−0.23
	(0.17)	(0.20)	(0.19)	(0.21)
Treatment 1b (Lobbying)	−0.08	−0.15	−0.37	−0.72***
	(0.18)	(0.20)	(0.19)	(0.21)
Treatment 1c (Mixed)	−0.26	−0.30	−0.35	−0.60**
	(0.17)	(0.19)	(0.19)	(0.21)
Age		0.02**		0.01
		(0.01)		(0.01)
Female		−0.18		−0.31*
		(0.14)		(0.15)
Education		0.02		0.01
		(0.04)		(0.04)
Income		−0.03		−0.09
		(0.05)		(0.05)
Constant	6.44***	4.76	5.49***	7.95*
	(0.13)	(2.99)	(0.14)	(3.28)
N	1,535	1,187	1,535	1,187
Adjusted R^2	−0.001	0.08	0.001	0.12

*p < .05; **p < .01; ***p < .001

5.3 Robustness Checks

5.3.1 Ordinal Logistic Regression Models of Political and Social Trust by Experimental Condition

In Table 5.5, I estimate identical models to those presented in Table 5.3 but employ ordinal logistic regression instead of a linear modelling technique. Ordered logit models are often considered to be the optimal estimation technique for ordered dependent variables, such as those measured through Likert scales. Given that all of the core dependent variables of this paper are measured on 11-point ascending scales, I complement my core analysis with models that treat the outcomes of interest as ordinal Also, in order to simplify their interpretation, I transform the outcomes of interest into 5-point scales by reassigning their original values to new categorical levels, which range from "Very low" trust in a given kind of political object, to "Very high".

At first glance, it seems that the findings described in the previous section of this chapter broadly align with the results of the ordered logit with regard to the direction and statistical significance achieved by the treatment effects. Most importantly, the treatment effects on the most diffuse measure of political trust, namely trust in the principles of democracy, are the highest ones across all models and achieve statistical significance at the 5% for *Treatment 1* and at the 1% level for all the other treatment arms. With regard to the validation of my hypotheses, Table 5.5 leads me to almost the exact same conclusions. First, exposure to general information about corruption does not have a significant negative effect on the most specific dimension of political trust, namely trust in politicians, but does meaningfully deteriorate trust in both institutions and abstract principles of democracy. Second, corruption revelations that associate the economic cost of corruption with bribery have a strong corrosive effect on the two most specific types of political trust, as well as on trust in democratic principles; however, this treatment has no significant effect on trust in the executive, legislative, and judiciary branches of government – which supports the hypothesis that specific political trust judgments are more sensitive to revelations concerning individual misbehavior rather than dysfunctional institutions or procedures. Similarly, the third treatment echoes the results produced by the linear regression models, as the two most diffuse measures of political trust, namely trust in the branches of government and the principles of democracy, are the most profoundly affected by the lobbying treatment arm (– 0.62 and – 0.56 compared to – 0.42 and – 0.41 for trust in politicians and offices respectively). The amplifying effect of combining corruption information is only partly present in the ordinal logit models, as the treatment effect in the fully specified model (6), which measures the effect of the treatment on trust in the overarching institutions of the state is the only one whose absolute value

increases compared to the other treatment arms. Nevertheless, all of the effects associated with *Treatment* 1_c are statistically significant and substantively large.

Table 5.5 Ordinal Logistic Models of Political Trust by Experimental Condition

	Politicians Ordinal Logit		Offices Ordinal Logit		Institutions Ordinal Logit		Principles Ordinal Logit	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment 1 (General)	−0.04	−0.24	−0.24	−0.47*	−0.16	−0.34	−0.35*	−0.34
	(0.15)	(0.18)	(0.15)	(0.19)	(0.16)	(0.20)	(0.15)	(0.18)
Treatment 1a (Bribery)	−0.36*	−0.43*	−0.32*	−0.45*	−0.29	−0.26	−0.59***	−0.59**
	(0.15)	(0.18)	(0.15)	(0.18)	(0.16)	(0.21)	(0.15)	(0.18)
Treatment 1b (Lobbying)	−0.19	−0.43*	−0.26	−0.40*	−0.42*	−0.63**	−0.57***	−0.53**
	(0.15)	(0.18)	(0.15)	(0.18)	(0.16)	(0.21)	(0.15)	(0.18)
Treatment 1c (Mixed)	−0.21	−0.38*	−0.34*	−0.53**	−0.41*	−0.52*	−0.62***	−0.56**
	(0.15)	(0.18)	(0.15)	(0.18)	(0.16)	(0.21)	(0.15)	(0.18)
Age		−0.01		0.002		−0.003		0.01*
		(0.01)		(0.01)		(0.01)		(0.01)
Female		−0.20		0.004		0.03		−0.08
		(0.13)		(0.13)		(0.15)		(0.13)
Education		−0.02		0.07*		0.01		−0.01
		(0.03)		(0.04)		(0.04)		(0.03)
Income		−0.10*		−0.10*		−0.01		−0.01
		(0.04)		(0.05)		(0.05)		(0.04)
N	1,534	1,187	1,534	1,187	1,254	959	1,534	1,187

*p < .05; **p < .01; ***p < .001

In Table 5.6, I estimate ordinal logistic models regressing the two measures of social trust on treatment dummies. I also transform the two indicators of social trust in 5-point ascending scales. This table employs the same structure as Table 5.5, with the first two models measuring treatment effects on particularized social trust with and without control variables, while the final two models calculate treatment effects for generalized social trust. In line with the results of the linear regressions, generalized trust seems to be far more sensitive to corruption revelations than particularized trust, as the coefficients associated with the fully specified model (4) are both substantively larger than those produced in model (2) and also achieve strong statistical significance more often than not. The differences in the absolute values of the treatment effects shown in model (4) are almost identical to those observed in the linear regression table. Indeed, exposure to information about lobbying appears to most strongly deteriorate generalized trust, while the bribery treatment arm does not produce significant results. The mixed treatment vignette effect is yet again attenuated. However, the effect of this treatment on particularized trust manages to achieve statistical significance, unlike in the linear regression models. To sum up, the average treatment effects calculated

with linear regression models are broadly robust to alternative modelling techniques, such as the ordinal logit.

Table 5.6 Ordinal Logistic Models of Social Trust by Experimental Condition

	Particularized Ordinal Logit		Generalized Ordinal Logit	
	(1)	(2)	(3)	(4)
Treatment 1 (General)	−0.17	−0.12	−0.28*	−0.36*
	(0.14)	(0.17)	(0.14)	(0.17)
Treatment 1a (Bribery)	−0.24	−0.23	−0.16	−0.19
	(0.14)	(0.17)	(0.14)	(0.17)
Treatment 1b (Lobbying)	−0.11	−0.22	−0.30*	−0.61***
	(0.14)	(0.17)	(0.14)	(0.17)
Treatment 1c (Mixed)	−0.29*	−0.42*	−0.32*	−0.56***
	(0.14)	(0.17)	(0.14)	(0.17)
Age		0.02**		0.01
		(0.01)		(0.01)
Female		−0.10		−0.23*
		(0.12)		(0.12)
Education		0.02		0.01
		(0.03)		(0.03)
Income		−0.02		−0.06
		(0.04)		(0.04)
N	1,534	1,187	1,534	1,187

*p < .05; **p < .01; ***p < .001

5.3.2 Replicating the Core Models with Restricted Samples

In Table 5.2, I demonstrated that there is a statistically significant difference in the correct response rate to manipulation check questions between treated and control groups, which I argued was a sign that the survey's treatments were successful in manipulating the respondents' knowledge about corruption. However, a relatively large share of treatment group participants did not in fact respond correctly to the manipulation checks, which could be caused by either inattentiveness or voluntary rejection of the experimental stimuli. I therefore replicate the analysis carried out in the first part of this chapter but instead of calculating average treatment effects on the entire sample, I filter out any non-control or placebo group respondent who answered incorrectly on the relevant manipulation check questions. It is important to note, however, that subsetting the sample to only include respondents who passed the manipulation

checks considerably reduces its overall size. For instance, the models presented in Table B.1, which are structurally identical to my original linear regression models, only take into account treatment group respondents who correctly answered the all of the relevant manipulation check questions and are thus based on sample size of 1055, which further decreases to 820 in the fully specified models (2), (4), (6), and (7). Given the reduction in sample size, these results should be interpreted with caution. I hypothesize that the main reason why filtering by manipulation check responses reduces the sample size by such a large amount is that the checks are situated after a relatively lengthy section dedicated to the measurement of the outcome variables, in order to avoid the accidental treatment of control group respondents and the priming of those who were treated.

The linear models calculated using the restricted sample broadly align with the core findings of this paper. First, the relative size, direction and statistical significance of the coefficients associated with *Treatment 1* are highly comparable to those observed in Table 5.3. Looking at the fully specified variants of the regression models, we can see that the treatment exposure appears yet again to most heavily deteriorate trust in the diffuse principles of democracy (– 0.69), while also having a significantly negative impact on trust in politicians (– 0.43). The average treatment effects on the measures of institutional trust are both substantively smaller than the two aforementioned figures (– 0.26 and – 0.05 for trust in political offices and branches of the government respectively) and also fail to reach statistical significance. The treatment effects of the bribery and lobbying treatment arms also mirror the results of the original regression models. In the bribery arm, the absolute value of the treatment effect on trust in politicians (– 0.78) is the second largest after trust in the principles of democracy (– 1.05). This therefore confirms that the most diffuse measure of political trust is also the most sensitive to instances of corruption where the blame can be most easily attributed to individuals. However, there also appears to be a clearer demarcation between the size of the treatment effect on trust in politicians and trust in political offices (– 0.26), when comparing to the original models. The more pronounced difference in the magnitude of these treatment effects implies that bribery is indeed perceived as an individually blameworthy phenomenon, more so than an institutional one. The coefficients produced by the lobbying treatment are fairly different to the ones observed in the models estimated using the full sample. Although it is clear that the attribution of blame in the case of lobbying is both personal and institutional, the absolute value of the effect of the treatment on trust in political offices actually decreases by 0.25 points relative to the bribery treatment arm, while the effect on trust in the executive, legislative, and judiciary branches of government fails to reach statistical significance. While the absence of a significant treatment effect on institutional trust is puzzling, it is interesting to note that the absolute value of the treatment effect on trust

in politicians decreases by 0.52 points, which indicates that political agents are less likely to receive direct blame for it compared to cases of bribery. Also, as observed in the original linear regression models, the lobbying treatment effect on trust in the principles of democracy is substantively smaller than for the bribery treatment arm. This finding seems to corroborate the idea that individuals are more intransigent when evaluating abstract concepts rather than institutional structures. Lastly, the average effects of *Treatment* 1_c, which associates the phenomenon of corruption with both bribery and lobbying, also mirrors the findings presented in Table 5.3, with the exception of the effects on trust in politicians and the three branches of government, both of which fail to reach statistical significance.

In Table B.2, I replicate the original linear models regressing social trust on the treatment dummies, using restricted samples. In this case, several significant differences are worth noting. First, while none of the treatment effects on particularized social trust reached statistical significance in the original models, the coefficients associated with *Treatment* 1_a achieve statistical significance albeit only at the 10% level. This could be interpreted as evidence that being confronted to the individually blameworthy behavior of political agents could push people to reconsider the trustworthiness of other members of their own social circles. Interestingly, all the other average treatment effects align entirely with the findings of the full sample models, as exposure to vignettes that identify lobbying as the root of corruption deteriorates generalized social trust most strongly (– 1.05), followed by the mixed treatment vignette (– 0.77) – with both coefficients achieving statistical significance at the 1% level. It is also worth mentioning that although the order of importance of these coefficients is constant across models, regardless of the type of sample used, the ones calculated on the basis of the restricted sample are far larger in magnitude, which implies that the original models might have underestimated the potential impact of exposure to information about political corruption on social trust. To sum up, it seems that the initial findings presented at the beginning of this chapter are robust to alternative estimations based on restricted samples. Bar some minor differences, the direction, relative magnitude, and statistical significance of the treatment effects is maintained even when regression coefficients are calculated using only data collected solely from respondents who passed all the relevant manipulation checks.

5.3.3 Placebo Tests

In order to account for the potential confounding effect of being exposed to an informational vignette in the survey, some of the respondents were randomly assigned to a placebo group. The placebo arm consisted of a vignette whose format was identical to the one used in the other treatment arms but whose content differed. Instead of being confronted to information about corruption in their country, respondents were presented with a few paragraphs describ-

ing the process by which the federal government distributes grants to states in order to support the financing of various social services and projects. Additional information was provided concerning the average value of federal aid grants, as well as a breakdown of how they are typically spent by states. The quantity of information supplied to survey takers assigned to the placebo group matched the bribery and lobbying treatment arms, rather than the shorter vignette of Treatment 1. The idea for the content of the placebo vignette was inspired by the experiment performed by Chong et al. (2015) in Mexico, which sought to measure the impact of exposure to corruption information on voter turnout. Given the association of the vignettes with the Transparency International brand, their format could generate unwanted independent effects on reported trust attitudes and thus process of isolating the average treatment effects.

In Table C.1, I regress the four indicators of political trust on a dummy variable indicating whether the respondent was assigned to the placebo or control group. The structure of the table is the same as all the previous ones, with the core models being presented first, followed by their fully specified variants. The results presented in this table indicate that the placebo vignette does not have any effect on the first three dimensions of political trust, namely trust in politicians, political offices, and the three branches of the federal government, as none of the coefficients achieves statistical significance. However, it appears that the placebo does have a substantively meaningful and strongly statistically significant effect (at the 5% level) on the indicator of trust in the principles of democracy, which moves from – 0.42 to – 0.39 in models (7) and (8). This is a surprising result since the placebo vignette was not designed to contain information that could influence respondents to reconsider the trustworthiness of the foundational principles of their political system. However, the distribution and usage of federal aid grants may nevertheless be a politically sensitive topic, particularly as there might be partisan or ideological disagreement with regard to how these grants are managed. Another possible explanation for this result could be that the format of the vignette, and more specifically the association with Transparency International, does have an independent effect of its own on diffuse political trust. This could in part explain why the absolute value of the average treatment effects analyzed in the previous sections of this chapter were consistently the largest for the most diffuse dimension of political trust – a pattern that contradicts the original theoretical direction of this paper's hypotheses. Adjusting the results to take into account the independent effect of the vignette format could potentially reduce the magnitude of the treatment coefficients on diffuse political trust.

In Table C.2, I regress the indicators of particularized and generalized social trust on the placebo treatment dummy. Looking at the fully specified models (2) and (4), the placebo vignette appears to only have a statistically significant effect at the 5% level on generalized social trust, which it decreases by 0.49 points. Yet again, this is a theoretically unexpected

finding, which casts doubt over whether the magnitude of the treatment effects on generalized trust in the core regression models is accurate or not. With regard to particularized trust, the coefficient associated with the placebo treatment arm in model (2) loses its statistical when transitioning to the fully specified variant.

I also perform a second placebo test to identify whether the way in which the outcome variable is measured has the potential to confound the treatment effect. To do so, I regress trust in the United Nations on all treatment dummies, with the expectation of observing null effects. In the case of this experiment, the United Nations are an irrelevant political object, since the matters described in the vignettes do not concern international organizations. Therefore, if the respondents' confidence in the UN is unaffected by their exposure to the treatments, this would solidify the claim that the treatments do not simply act as triggers for political or institutional distrust in general, regardless of the attribution of blame and the domestic scope of the vignettes. Table C.3 shows that none of the treatment dummies have a statistically significant effect on trust in the UN, which confirms that the treatments were indeed successful in accurately assigning responsibility to the relevant political objects.

5.3.4 Clustering and Principal Component Analysis of Multi-Item Institutional Trust Scales

In this paper, I measure institutional trust at two different levels, namely trust in political offices and in the branches of government. I treat the former as the more specific instance of political trust, while the latter is considered more diffuse, as it encompasses the most far-reaching institutional structures of the state. Both of these variables are calculated on the basis of multi-item scales that make reference to a variety of political offices and institutions (e.g. the legislature, the mayor's office etc.). Thus, in order to verify that the way in which these scales are aggregated into two factors is justified, I perform clustering analysis on the dependent variables of my dataset, which I expect will inform me of whether the institutional trust measures used in the survey can in fact be separated into two clusters. After having rescaled the variables, I use multiple indices to identify the best clustering scheme. This computation tells me that the best number of clusters is two. I then use K-means clustering, which minimizes total within cluster-variation summed over all the clusters, to produce the two clusters, which are plotted in Figure D.1. The existence of two distinct clusters indicates that the conceptual distinction regarding the level of specificity or diffuseness of different sets of institutional structures that I describe in this paper is mirrored in the data. Alternatively, I also perform Principal Components Analysis and find that two principal components explain approximately 70% of the variance in the data. In Figure D.2, I plot the two principal

components against each other in order to visually inspect the loadings of each variable in the principal components. In this case, it seems that trust in political parties, as well as the legislative, judiciary, and executive branches of government all belong to the second principal component, while trust in political offices, such as the presidency, the governorship, and mayorship are relatively more important for the first principal component. Although trust in party leaders seems to be more strongly associated with the second principal component, replicating the core regressions of this paper with the variable of trust in party leaders being included in the more diffuse variant of institutional trust has not significant effect on the main results.

5.4 Heterogeneous Effects

The way in which political information is absorbed and interpreted by individuals could vary greatly depending on their pre-existing political and cultural preferences and attitudes. As discussed in Chapter 4, the literature on motivated reasoning has demonstrated that people update their beliefs and adjust their behaviors in a strongly biased way. I therefore argue that the same dynamics can be applied to more accurately reflect the way in which people produce political trust judgments. This section aims to explore how the effect of being exposed to corruption information on political trust changes depending on different respondent profiles. I measure treatment heterogeneity across three distinct categories, namely partisan and ideological preferences, political sophistication, and cultural attitudes. I calculate interaction effects between each treatment dummy and the corresponding covariates in order to identify how the interpretation of treatment vignettes changes depending on the respondents' ideational, psychological, and cultural profile. In each of the tables discussed below, I present four models in which all the relevant measures of political trust are regressed on treatment dummies interacted with variables that captures different dimensions of the respondents' partisan and ideological leaning, level of political sophistication, and psychological and cultural background. Table 5.7 below summarizes the main potential sources of heterogeneity as well the variables used to measure them.

Table 5.7 Sources of Treatment Heterogeneity and their Measurements

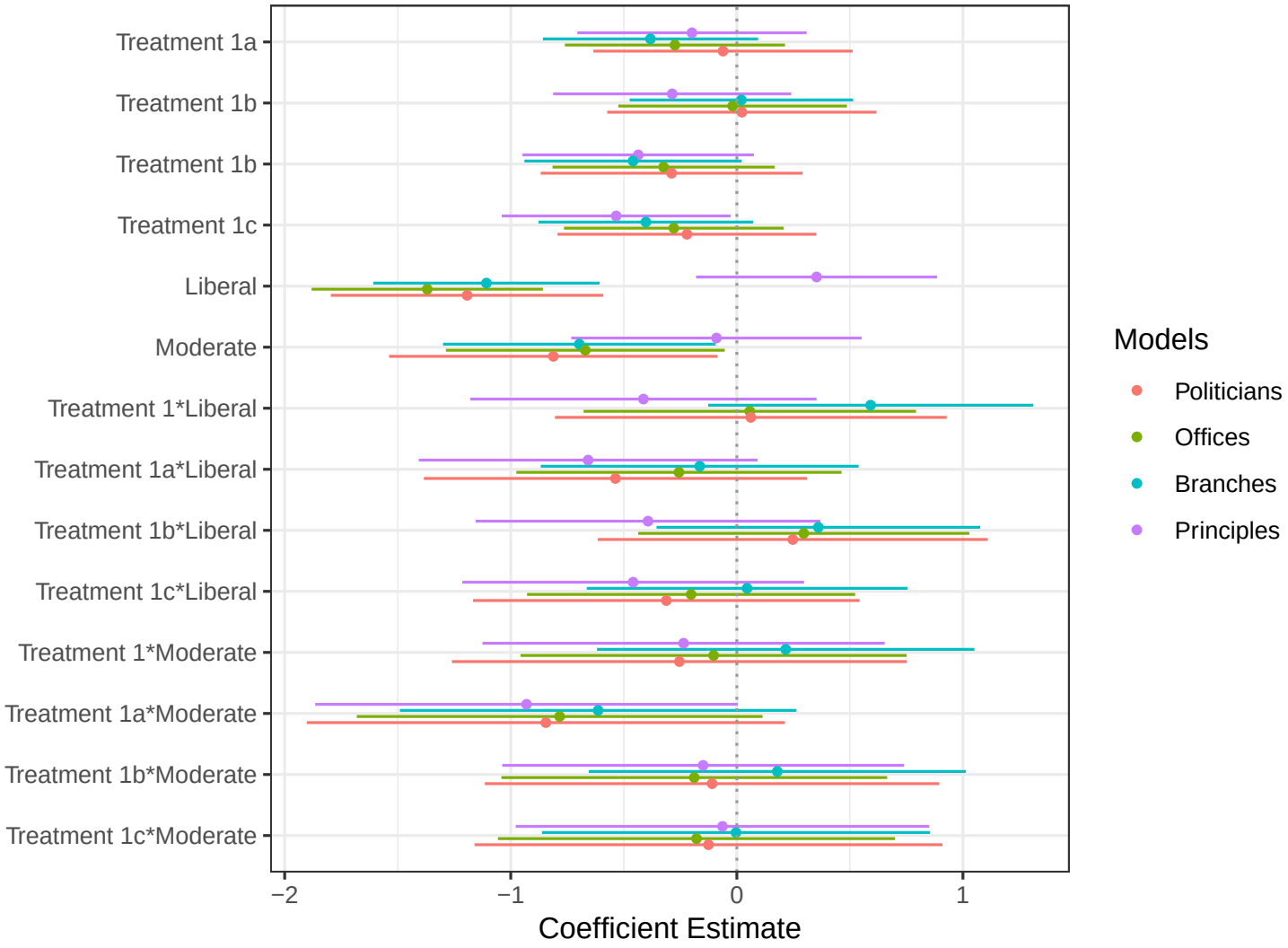
Variable Category	**Relationship**	**Indicators**
Partisan and ideological preferences	Moderating	Party attachment, ideological leaning
Political sophistication	Moderating	Education level, political interest, political knowledge
Psychological attributes	Moderating	Optimism
Cultural preferences	Moderating	Postmaterialism, awareness of corruption

5.4.1 Partisan and Ideological Preferences

Given the importance of motivated reasoning for the interpretation of political information, I first look at how my treatments interact with the respondents' ideology and partisan attachments. Although the survey measures ideology on a 7-point scale ranging from "Very conservative" to "Very liberal", I collapse the original variable into a 3-point factor categorizing respondents as either Conservatives, Liberals, or Moderates. As shown in Table E.1 (whose coefficients are also plotted in Figure 5.2), the interaction terms between treatment dummies and the respondents' ideological preferences, which seek to capture how the interpretation of the treatment varies when the respondents identify as Liberals or Moderates compared to when they identify as Conservative, are mostly negative and substantively large. In particular, the magnitude of the interaction term between the treatments and the respondents' ideological leaning appears to be the largest for the coefficients associated with trust in politicians and in the diffuse principles of democracy. For example, in the bribery treatment arm, trust in politicians decreases by a further 0.65 when the respondent identifies as Liberal compared to when they identify as Conservative. Similarly, a 0.7 deterioration in specific political trust can also be observed when comparing Moderate respondents to Conservatives. The differences in the magnitude of the treatment effects across ideological positions can be interpreted as evidence that Liberals and Moderates are perhaps naturally more idealistic and therefore less tolerant of corrupt behaviors and institutions in general, or they can be seen as a form of protest against the pre-eminence of Conservative discourse in the current American political climate. Despite these theoretical implications, it is important to note that the coefficients illustrated in this table largely fail to achieve statistical significance, with the exception of a handful which are only statistically significant at the 10% level. Although this

casts doubt over whether ideology acts as a meaningful filter for the interpretation of factual corruption-related information, the absence of a strongly statistically significant interaction effect could be explained by small sample sizes or insufficient variation in the variables of interest. In Table E.2, I replicate the same interaction models but replace the ideology variable with the respondents' partisan attachment in the interactions. This set of interactions is substantively consistent with the results of the ideology analysis. Holding the Republican Party as the baseline reference category, it is clear that the respondents' partisan affiliation strongly moderates the relationship between the treatment and outcome variables. The interaction effects for the major left-wing parties, such as the Democrats or the Green Party, are all negative, which means that the treatment effect on trust is amplified when respondents associate themselves with the more liberal side of the political spectrum. However, only a handful of these coefficients achieve statistical significance. For instance, trust in politicians, political offices, branches of government, and diffuse principles of democracy decreases by 3.42, 4, 3.56, and 3.36 points respectively, when the respondents who identify with the Green Party receive the bribery treatment, relative to those who identify as Republican (all of these coefficients achieve statistical significance at the 5% level). In sum, the size and direction of the interaction terms appears to indicate that holding a liberal or centrist ideological position amplifies the treatment effects compared to identifying as a conservative. However, since these coefficients largely fail to achieve statistical significance, I suggest that further study, with larger samples, might be required to better gauge the extent to which ideology bias and motivated reasoning interferes with the way in which people process of corruption revelations.

Fig. 5.2 Coefficient Plots of Interactions between Treatment Condition and Political Ideology



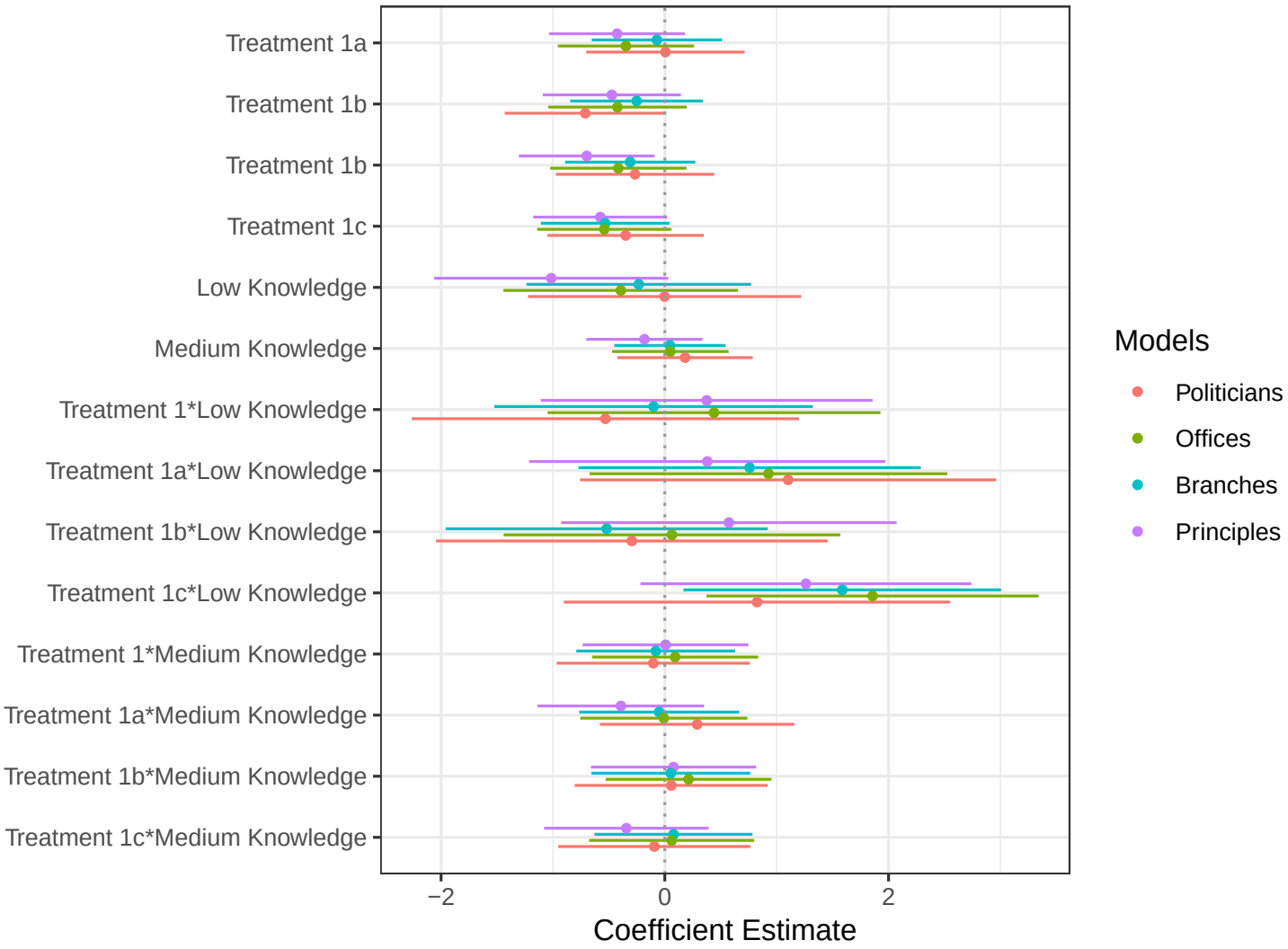
5.4.2 Political Sophistication

Beyond partisan biases, political sophistication can also modify the way in which individuals absorb and interpret factual political information. Since political sophistication is a multi-dimensional concept, I interact the treatment dummies with multiple covariates, which serve as alternative measurements of the respondents' level of political sophistication. I begin by analyzing interactions with the respondents' education level. I recode the original education variable as a three-level factor, with the value of "Low" being assigned to respondents whose highest level of educational attainment was high school, "Medium" for those who only completed undergraduate studies, and "High" for those who pursued postgraduate studies.

Theoretically, the more highly educated individuals are expected to be better equipped to understand the consequences of corruption and therefore be more critical of it. Table E.3 reveals that education may indeed be the source of treatment heterogeneity. In particular, the effect of the interaction between the *Treatment* 1_a and 1_b dummies and the medium level of education on trust in politicians produces substantively large negative coefficients, which are also statistically significant at the 5% level. Thus, exposure to the bribery treatment vignette decreases the respondent's reported level of trust in politicians by 1 full point when their highest level of educational attainment is the undergraduate college compared to those who pursued postgraduate studies. This effect is almost identical for the lobbying treatment arm, with a relative decrease in trust in politicians of 0.91 points. This indicates that individuals who did not pursue further study after graduating from college are particularly critical of the individual behavior of politicians compared to those who did – this effect being slightly stronger in the bribery treatment arm. Interestingly, all of the coefficients measuring the treatment effect at the level of high school graduates fail to reach statistical significance, however, this is possibly yet again caused by insufficient variation in educational attainment, as a large majority of respondents have in fact completed at least some form of undergraduate studies. Although education is typically positively correlated with political sophistication, it may not be a sufficient measure of it. I therefore incorporate two additional variables in my analysis, including the respondents' reported level of interest in politics, as well as an additive index of political knowledge, measured on the basis of three factual questions that I ask respondents at the end of the survey. Table E.4, which presents the results from the interaction of the treatment dummies with the variable of political interest, reveals that moving across different levels of political interest does not alter the treatment effect significantly, with the exception of the interaction between the mixed treatment arm dummy and middling political interest. Despite the absence of statistically significant results, the negative direction and substantive size of the majority of interaction coefficients appears to indicate that people who are moderately interested or not interested at all in politics are in fact slightly more sensitive to corruption revelations compared to those who are very interested. These findings contrast with the interaction effects of the treatment dummies and the variable measuring the respondents' political knowledge. As shown in Table E.5 (whose coefficients are also plotted in Figure 5.3), the direction of the interaction coefficients at low and medium levels of political knowledge is generally positive, holding high political knowledge as the baseline category, which could mean that having a better knowledge and understanding of the political system does make corruption in all of its manifestations less tolerable. For instance, being assigned to the bribery treatment arm and being poorly politically informed increases reported levels of trust in politicians by 2.27 points compared to those who are highly informed. This

result falls in line with the expectation that better-informed individuals are more critical of political dysfunctions and also more capable of attributing blame. However, it is important to note that the coefficients calculated in this table also fail to achieve statistical significance. In sum, interacting the treatment dummies with measures of political sophistication reveals surprising trends, as only the political knowledge index interactions behave as predicted, while education and political interest conflict with the theoretical expectation that more politically sophisticated individuals are more sensitive to corruption information.

Fig. 5.3 Coefficient Plots of Interactions between Treatment Condition and Political Knowledge

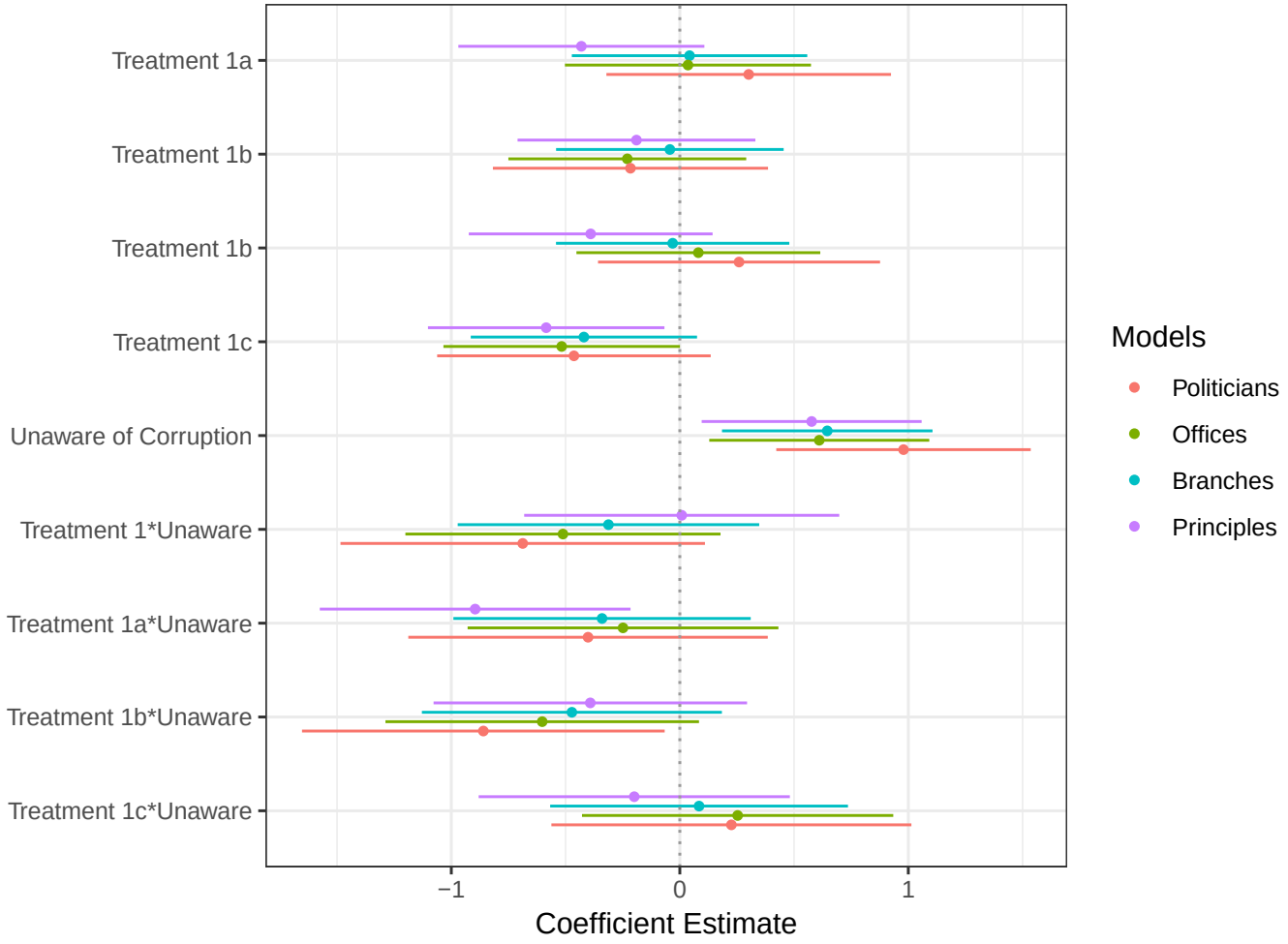


5.4.3 Psychological and Cultural Attitudes

Cultural attitudes are also likely to play a moderating role in producing political value judgments. First, the decision to trust may be partly related to an individual's embedded personality traits, such as their level of optimism. These traits are developed in early childhood and influence the way in which individuals interact with the world. In Table E.6, I present a series of models based on interactions between treatment dummies and a three-level factor variable gauging the respondents' reported level of optimism. First, it appears that at low and medium levels of optimism, the negative effect of exposure to general information about corruption, as well as information about bribery, on political trust is attenuated relative to the baseline category of high optimism, as the direction of the respective interaction coefficients is positive. For the lobbying and mixed treatment arms, the signs of the interaction effects on trust in politicians change to negative. This pattern could be interpreted as resulting from a potential disenchantment effect with regard to the individual behavior of politicians on behalf of more optimistic respondents. In other words, individuals who are more positive about the future might have higher expectations with regard to the conduct of their representatives, while those who are more pessimistic do not, which implies lower potential for disappointment. However, causal identification remains difficult given the fact that most of these interaction terms fail to reach statistical significance. In Table E.7, I perform a similar analysis but this time I interact the treatment dummies with an index of postmaterialism. I divide respondents in four different categories, which range from "Postmaterialist" to "Materialist", holding the latter as the baseline for the categorical interaction effects. Despite not showing signs of statistical significance, the vast majority of interaction terms are positive and substantively large. The direction of the coefficients could signify that although postmaterialists are less trusting of authorities and institutional structures in general (as indicated by the negative independent effect of postmaterialism on political trust), materialists are more cognizant of the damage caused by corruption when exposed to information that makes reference to its economic cost. In the case of this survey, for instance, postmaterialists do not seem to be as deeply affected by the claim that the yearly costs of corruption amount to 5% of GDP. Lastly, I interact the treatment dummies with a variable indicating whether the respondent considers corruption to be one of the most important issues that their country must tackle in the near future. This variable is based on the order of the responses given to the postmaterialism index questions. More specifically, if the respondent ranks the option "Reducing corruption" as one of the top three priorities for their country's future, then they are categorized as being aware of corruption – and vice versa. As shown in Table E.8 (whose coefficients are plotted in Section 5.4.3), most of the interaction terms between the treatment and corruption awareness dummies

are negative and substantively large (although failing to reach statistical significance in the majority of cases). This means that receiving one of the four treatments while being unaware of corruption further decreases one's political trust levels compared to those who acknowledge corruption as a high priority issue. This effect can result from the unpleasant sentiment that gets triggered when individuals find out that things are worse than they had anticipated. Assuming that those who rank corruption as a relatively low priority issue are also likelier to underestimate its magnitude, exposing them to information about its cost can generate stronger sense of frustration or disappointment compared to those who were more concerned about it in the first place. This explanation is supported by the positive direction of the independent effect of being unaware of corruption on all four measures of political trust, which suggest that corruption ignorance is associated with greater political trust. The direction of Table E.8 coefficients are also consistent with those produced by the optimism interaction terms, which similarly demonstrated the magnifying effect of worse-than-expected outcomes. To sum up, the direction and size of the treatment interactions with psychological and cultural attributes, such as the respondents' optimism, postmaterialist outlook, or awareness of corruption, appear to indicate that such features could affect the way in which confrontation with corruption facts influences political trust judgments. However, the scope of this survey's sample is insufficiently large to produce statistically significant conclusions, which is why these results should be interpreted with caution. Further theorizing on the potential impact of psychological and cultural variables on political trust formation is also required, since expecting to observe the opposite effects, such as optimism attenuating the treatment effect and postmaterialism amplifying it, would have also been theoretically justified (e.g. optimists being more forgiving and postmaterialists more critical with unethical conduct).

Fig. 5.4 Coefficient Plots of Interactions between Treatment Condition and Awareness of Corruption



5.5 Causal Mechanism

In my theory, I argue that the diffuse and specific dimensions of political trust are both reliant on socialized beliefs as well as evaluative efforts, but unequally so. I make the claim that diffuse political trust is primarily socialized and therefore relatively less evaluative compared to specific trust because diffuse political objects are the conceptual category that receives the most attention during early socialization in advanced democracies. This is due to the fact that the early democratic socialization process aims at strengthening individual attachments to the non-instrumental value of political concepts in order to cultivate a healthy form of citizenship. I posit that diffuse political objects are preferred over specific ones because

their meaning and purpose is easier to grasp from an early age. Therefore, since diffuse political trust relies to a relatively greater extent on embedded beliefs, it should also be less sensitive to external informational stimuli about the performance of the political system, which is the core hypothesis that I tested in this paper. In this section, I perform several tests to verify whether the causal mechanism that links corruption revelations to political trust is correct. To do so, I first analyze when respondents first learned about different political concepts, expecting them to report having first encountered diffuse rather than specific concepts. Second, I compare the degree to which survey respondents associate diffuse and specific political objects with non-instrumental values more so than instrumental ones, expecting them to value the non-instrumental value of diffuse objects more highly than that of specific ones. I would interpret this as evidence that diffuse political concepts and their non-instrumental value were indeed the centerpiece of the respondents' early political socialization process. Third, I measure how the treatment effects vary depending on how early in their lives respondents first learned about the objects that they are asked to report their confidence in. I expect that the earlier one has familiarized with a particular political concept, the more robust will their trust in that concept be. Lastly, I measure how the treatment effects on trust in politicians and principles of democracy vary depending on whether the respondent values the respective political objects for their instrumental or non-instrumental value, expecting those who prioritize non-instrumentalism to report relatively higher levels of political trust, since their political value judgments rely more heavily on socialized beliefs, rather than ongoing performance assessments. I would interpret this as evidence that beliefs that become embedded during socialization persist through life and moderate the evaluative dimension of political judgments. As a side note, (non)instrumentalism and (post)materialism are not synonymous to each other. Instrumentalism refers to the expectation that a given political object will perform well along a different set of dimensions, such as ethics or economic welfare. Conversely, non-instrumentalism is simply the belief that a given political object possesses certain inherent qualities, which are valued regardless of the outcomes they produce. Materialism and postmaterialism, on the other hand, refer to one's preference for the different types of outcomes that a political system can produce.

I build two variables to measure the degree to which respondents associate diffuse and specific categories of political objects with instrumental or non-instrumental values. These variables are based on questions asking survey takers to rank four ideas by order of importance, with two of them capturing the instrumental dimension of the respective political object, and the other two its non-instrumental one. Using a similar coding strategy as for the postmaterialism index, I categorize respondents as either "Instrumentalists", "Rather Instrumentalists", "Rather Non-instrumentalists" and "Non-instrumentalists" based on the

combination of their responses. Given the limited survey duration, respondents were only asked to perform this task for two political objects, namely democracy as a political regime and elected officials. I also create dummy variables that capture how early respondents first learned about different political objects in their lives. These dummies are based on questions asking respondents to rank different political concepts according to the chronological order in which they found out about them: the dummy takes the value of 1 if a given concept is ranked first or second (i.e. learned earlier in life), and 0 if otherwise (i.e. learned later in life). For the sake of clarity, I only generate dummies for the most diffuse and specific categories of political objects, namely democracy in principle and elected political leaders.

In Section 5.5, I show the distribution of responses relating to how early the survey participants first learned about democracy as a political regime and specific political leaders compared to the other political objects. First, the bar plot seems to indicate that the majority of the sample has indeed learned about the most specific kind of political object, namely political leaders, later on in life compared to more diffuse concepts, such as the institutions of the state or democracy as a political regime. However, contrary to my expectations, the same observation also applies to the diffuse notion of democracy, which respondents also appear to have come in contact with relatively later on in their lives. Although this data is insufficient to reach a definitive conclusion, it nevertheless casts doubt over the validity of the theoretical claim that diffuse political concepts are indeed prioritized in the early political socialization process. In Section 5.5, I plot the variables that measure the extent to which individuals associate non-instrumental values with diffuse or specific political concepts. The lower values reflect the prioritization of instrumentalist arguments, while higher values capture non-instrumentalist positions. Surprisingly, this figure indicates that, on average, respondents associate the role of political leaders more strongly with non-instrumentalist goals compared to the purpose of democracy as a political regime. Similar to Section 5.5, this finding seems to also indicate that early political socialization might not be centered on the diffuse dimensions of the political system as much as I expected. However, the theoretical mechanism elaborated in this paper could still be relevant inasmuch as those whose socialization process prioritized diffuse concepts over specific ones could still respond differently to the experimental stimuli.

Fig. 5.5 Socialization Timing for Diffuse and Specific Political Objects

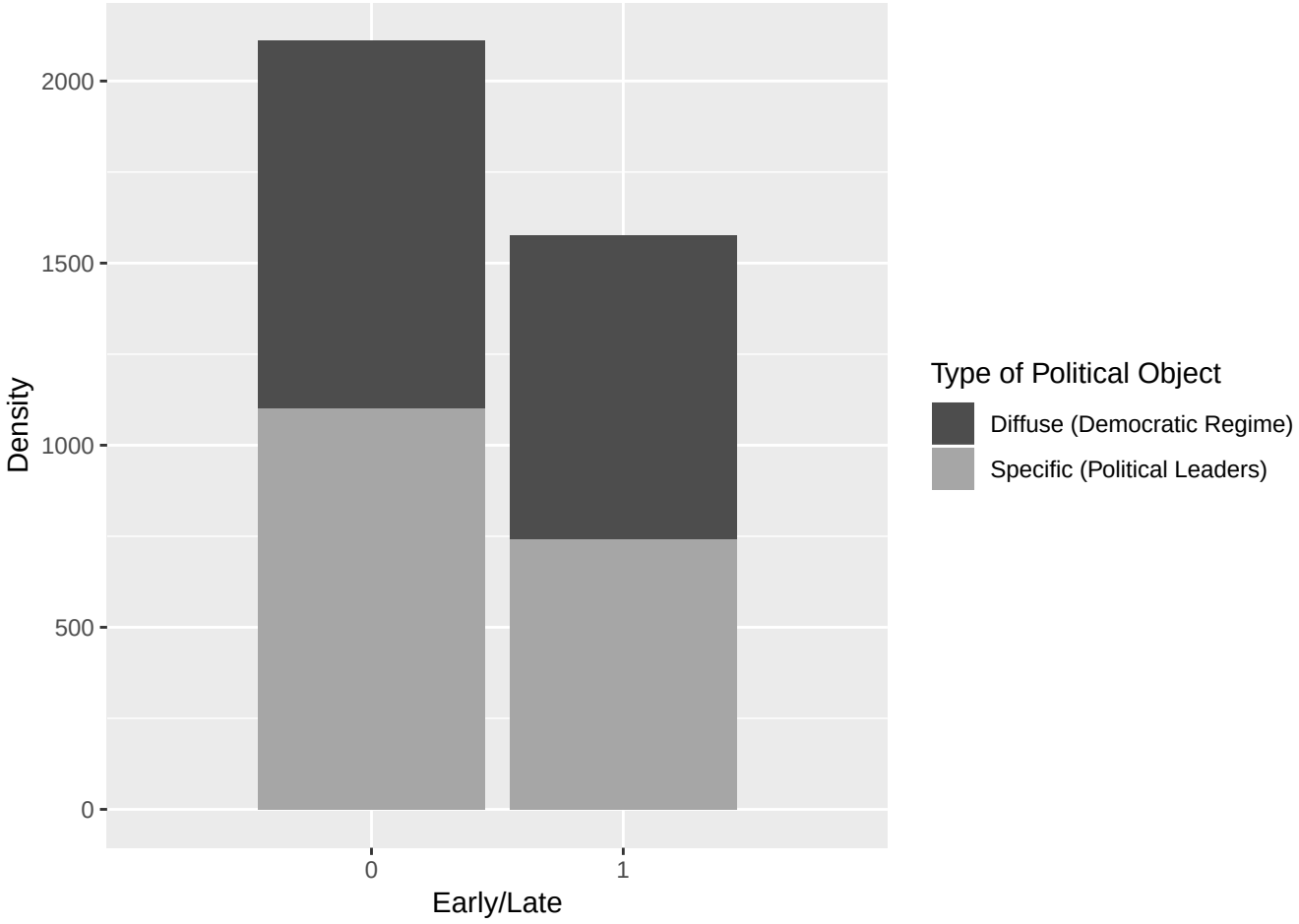


Fig. 5.6 Importance of Non-Instrumental Value of Diffuse and Specific Political Objects

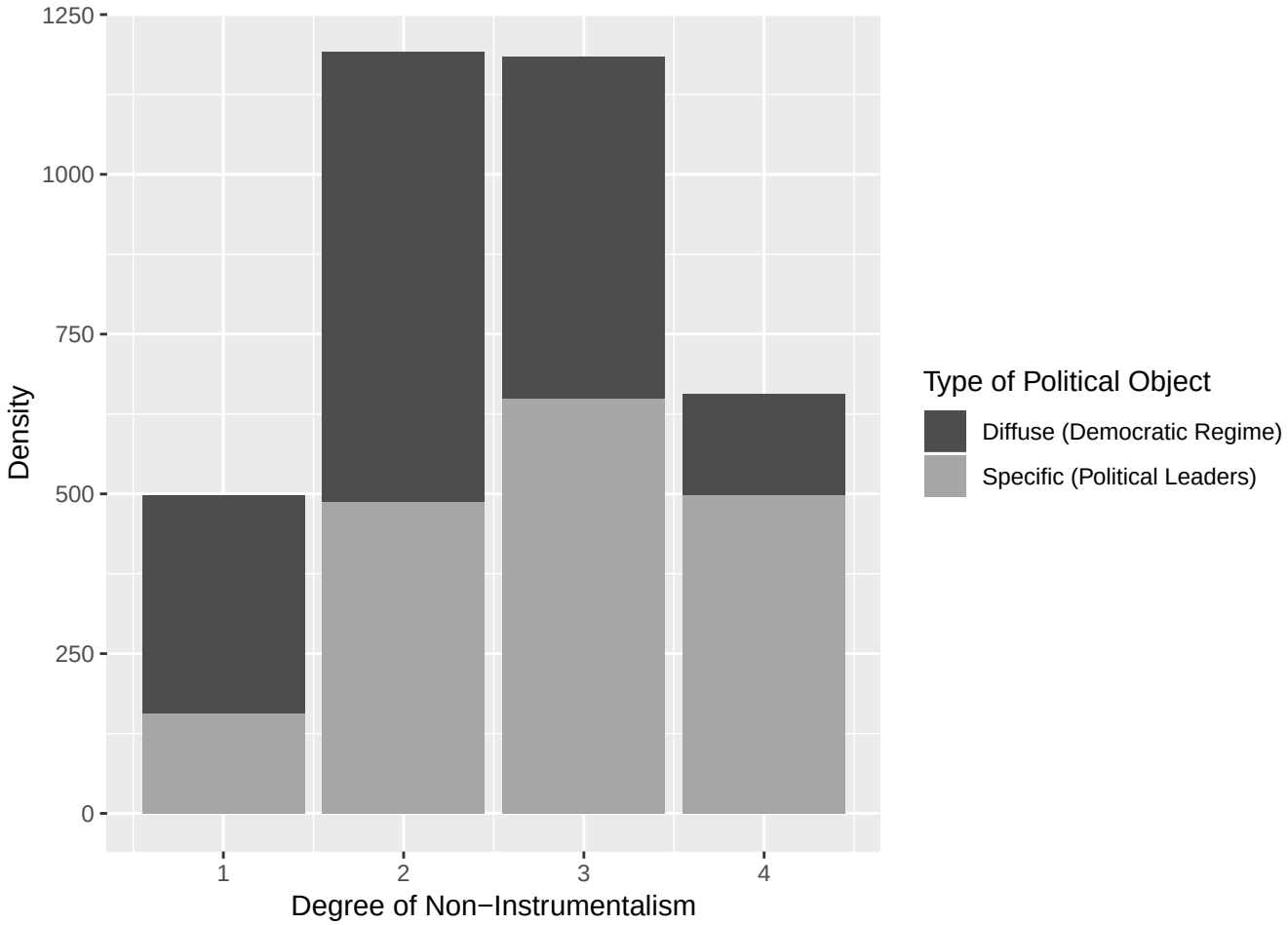


Table F.1 shows the results from a series of linear regression models in which the variables of trust politicians and in the principles of democracy are regressed on the treatment dummies interacted with the two socialization chronology dummies. First, the positive direction of the coefficients produced by the interactions between the bribery and mixed treatment dummies and the specific socialization chronology dummy indicate that the specific political trust levels of respondents who learned about the role of politicians earlier in their lives are more resilient to information about corrupt acts where the blame is primarily attributable to individuals compared to the respondents who learned about politicians at a relatively later point in their lives. This finding aligns with the notion that even though political trust is partly evaluative, it tends to be more stable when the object whose trustworthiness is being assessed has received privileged attention during the early socialization process. However, this effect does not seem to apply to the interaction between the general and lobbying treatment arms and the socialization dummy, as their coefficients have a negative direction. Also, both of the positive coefficients are substantively small (0.2 and 0.06 for *Treatment* 1_a and *Treatment* 1_c respectively), while also failing to achieve statistical significance. Therefore, these figures cannot be interpreted as definitive evidence that the socialization variable does indeed moderate the treatment effect. The interaction coefficients with the diffuse socialization dummy are even more puzzling, as three of them have a negative direction, with only the one associated with the lobbying treatment arm being positive. The direction of the coefficients appears to conflict with the theoretical mechanism posited in this paper, however, the results fail yet again to achieve statistical significance, thus casting doubt over how they should be interpreted.

In Table F.2, I investigate the moderating effect of associating more or less instrumental roles to politicians on political trust. First, it appears that respondents who conceptualize the purpose of elected officials as being primarily non-instrumental are likely to report slightly higher levels of trust in politicians when confronted to corruption information, compared to those who value politicians mainly for instrumental reasons. Holding the "Instrumental" category as the baseline reference, almost all of the interaction terms between the treatment dummies and the other levels of the instrumentalism factor are positive and substantively large. To give an example, being assigned to the core treatment vignette and valuing the non-instrumental purpose of elected officials the most increases one's specific political trust by 0.69. However, these results yet again broadly fail to achieve statistical significance.

Lastly, Table F.3 investigates the moderating effect of valuing democracy for more or less instrumental reasons on political trust. In this case, the direction of the interaction coefficients is negative across the board, which can be interpreted as a sign that the more one is attached to the non-instrumental value of the democratic system as a whole, the

stronger can we expect the negative impact of corruption revelations to be on diffuse political trust. Similar to the previous table, these coefficients fail to achieve statistical significance. However, their direction and magnitude conflict with the theoretical expectations of this paper's mechanism, which predicted that a more non-instrumental conceptualization of the democratic system should minimize the extent to which diffuse political trust deteriorates in response to corruption revelations. To sum up, the causal mechanism developed in this paper does not seem to find strong empirical backing. However, the results presented in this section are likely to be affected by the insufficiently large size of the sample, which suggest that further theorizing is required to properly account for the effects of the treatments on political trust.

Chapter 6

Conclusion

6.1 Summary of Findings

In this paper, I employed a survey experiment based on a sample of 1,843 respondents from the United States to analyze how exposure to information about corruption affects different dimensions of political trust. In my theory, I argued that both diffuse and specific types of political trust contain a socialized and evaluative component that are, however, weighted differently. I posited that diffuse political trust is more sensitive to evidence of systemic dysfunction, while specific political trust is most strongly affected by individual level transgressions. In terms of results, I found partial support for my hypotheses. First, when the attribution of blame was either undefined or primarily focused on the individual misbehavior of political agents, specific political trust decreased most strongly while trust in the legislative, executive, and judiciary branches of the government was unaffected. Second, when the corruption vignette highlighted institutionally tolerated instances of corruption, such as lobbying, institutional trust decreased significantly, compared to other treatment arms, even though trust in politicians remained the most severely affected quantity. Surprisingly, trust in the principles of democracy was the most sensitive outcome variable across all treatment arms. These patterns are robust to different model and sample specifications, as well as alternative modelling techniques, including ordinal logistic regression. Auxiliary analyses indicated that the treatment effect might be moderated by the respondents' partisan and ideological attachments, political sophistication, and psychological and cultural attributes. However, the analysis of heterogeneous effects failed to produce statistically significant results, which I hypothesize was caused by an insufficiently large sample size. Lastly, despite also being statistically inconclusive, the testing of the causal mechanism appeared to indicate that socialization does necessarily not attenuate the effect of exposure to corruption information on diffuse or specific types of political trust.

6.2 Discussion

The contributions of this paper to the literature are twofold. First, I complement existing studies of corruption and political trust by approaching this topic from an experimental angle. Second, I develop an original theoretical argument, which seeks to provide a more nuanced model of political trust formation by suggesting that the trust-defining forces of socialization and evaluation are complementary, rather than mutually exclusive. Although the empirical validity of the causal mechanism laid out in this paper is questionable, the process of attempting to identify complementarities across multiple theories of trust formation is an undoubtedly worthwhile endeavor.

Beyond their academic relevance, the findings presented in this paper may also be of interest to policymakers who seek to better understand the way in which political trust can be cultivated. The results of this paper suggest that while confidence in institutions is remarkably resilient in the face of corruption revelations, the same cannot be said for trust in politicians and in the diffuse principles of democracy. This means that the release of information about corruption is a sensitive endeavor that requires careful planning to minimize its trust-eroding effects. My paper also shows that legal forms of corruption that are tolerated in advanced democracies, such as lobbying, can also have a damaging impact on trust levels.

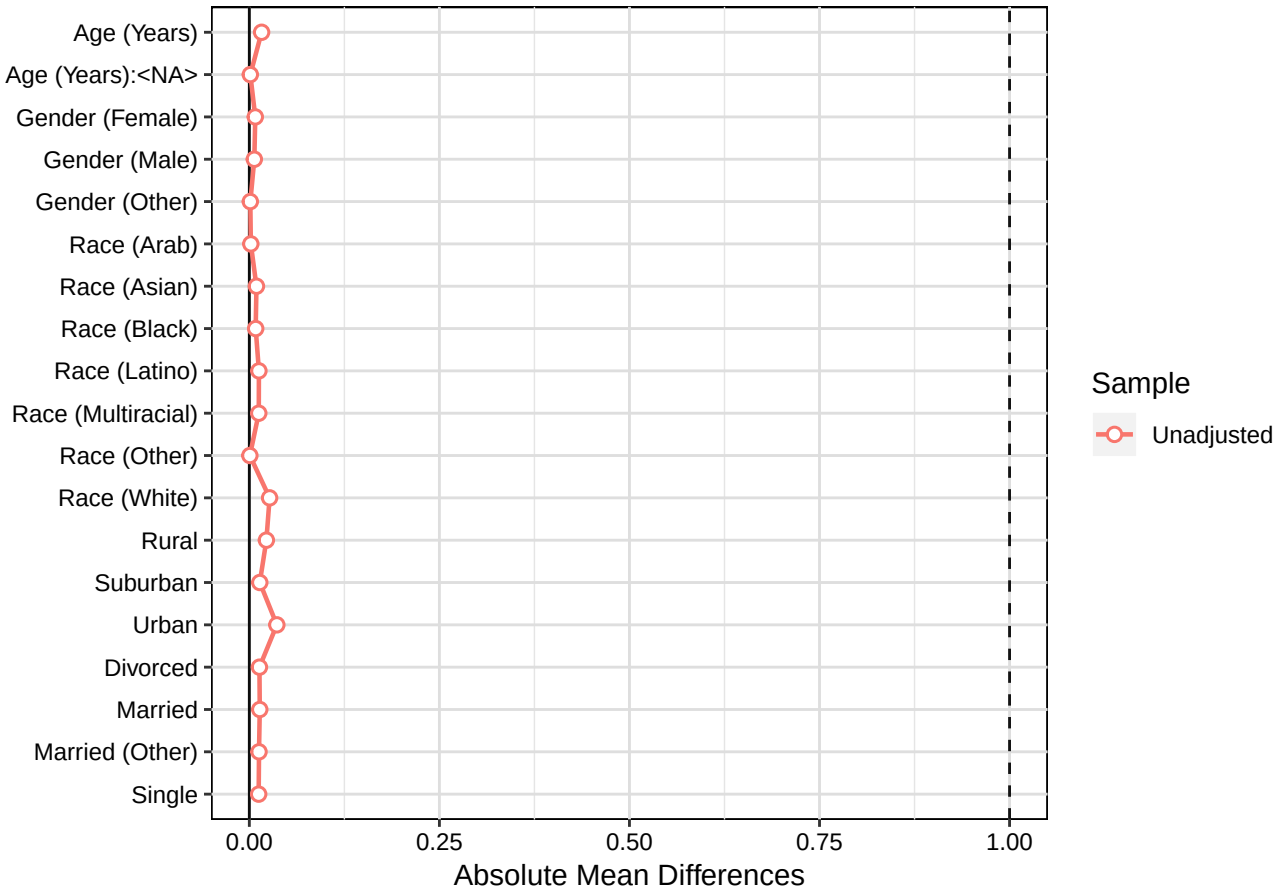
This paper faces a number of key limitations. First, it does not offer an inter-temporal measurement of the outcome variable, which means that it is hard to know whether the measured treatment effects should be expected to decay or not. Second, the relatively small sample size and high number of experimental groups poses a significant obstacle for the analysis of heterogeneous effects, as the lack of variation in key covariates makes it difficult to gauge the extent to which they moderate treatment effects. Lastly, the unexpected direction of some of this paper's results indicates that the causal process by which revealing information about corruption generates distrust in different categories of political objects should be revisited. In terms of avenues for future research, I believe it would be useful to broaden the geographical scope of corruption survey studies since it is likely that the size of the treatment effects will vary depending on the respondent country's pre-existing level of corruption, as well as its democratic experience. In particular, I believe it would be beneficial to target countries with differing levels of corruption and perhaps also include a wider range of political regimes. Second, I believe that replicating this study in a laboratory setting might offer researchers more flexibility with regard to the implementation of behavioral measures of political trust, which are difficult to include in surveys. Third, exploiting natural instances of political regime transitions would also enable researchers to compare the inter-generational evolution of political trust alongside changing corruption levels and socializing contexts. Lastly, further research is also needed to better understand how variations in the strength of

the informational stimuli as well as the tone used to communicate political facts can moderate the effect of exposure to corruption on political trust.

Appendix A

Randomization Checks

Fig. A.1 Love Plot of Covariate Balance Across Treatment and Control Groups



Appendix B

Model Replication with Restricted Samples

Table B.1 Linear Models of Political Trust by Experimental Condition (Restricted Sample)

	Politicians OLS		Offices OLS		Institutions OLS		Principles OLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment 1 (General)	−0.15	−0.49*	−0.20	−0.28	−0.03	−0.03	−0.57*	−0.66**
	(0.20)	(0.23)	(0.19)	(0.20)	(0.20)	(0.19)	(0.23)	(0.25)
Treatment 1a (Bribery)	−0.85***	−0.68**	−0.55*	−0.48*	−0.54*	−0.27	−0.84**	−0.88***
	(0.24)	(0.26)	(0.22)	(0.21)	(0.23)	(0.21)	(0.26)	(0.26)
Treatment 1b (Lobbying)	−0.43*	−0.70**	−0.49*	−0.61**	−0.40	−0.40	−0.61*	−0.73**
	(0.22)	(0.24)	(0.22)	(0.22)	(0.23)	(0.23)	(0.26)	(0.25)
Treatment 1c (Mixed)	−0.33	−0.34	−0.34	−0.34	−0.29	−0.11	−0.61*	−0.52*
	(0.22)	(0.25)	(0.20)	(0.19)	(0.22)	(0.20)	(0.26)	(0.26)
Age		−0.003		0.01		0.004		0.01
		(0.01)		(0.01)		(0.01)		(0.01)
Female		−0.43*		−0.14		−0.12		−0.19
		(0.19)		(0.15)		(0.15)		(0.19)
Education		−0.07		0.01		−0.03		−0.11*
		(0.05)		(0.04)		(0.04)		(0.05)
Income		−0.18**		−0.15**		−0.04		−0.03
		(0.07)		(0.05)		(0.05)		(0.07)
Constant	5.01***	10.96***	5.56***	6.98***	5.56***	6.56***	6.58***	5.14***
	(0.12)	(1.48)	(0.12)	(1.64)	(0.12)	(1.57)	(0.14)	(1.49)
N	844	662	844	662	844	662	844	662
Adjusted R^2	0.01	0.32	0.01	0.35	0.01	0.31	0.02	0.15

*p < .05; **p < .01; ***p < .001

Table B.2 Linear Models of Social Trust by Experimental Condition (Restricted Sample)

	Particularized OLS		Generalized OLS	
	(1)	(2)	(3)	(4)
Placebo	0.01	0.14	−0.35	−0.49
	(0.23)	(0.26)	(0.24)	(0.28)
Treatment 1 (General)	−0.20	−0.15	−0.31	−0.25
	(0.22)	(0.26)	(0.25)	(0.29)
Treatment 1a (Bribery)	−0.16	−0.32	−0.64**	−1.15***
	(0.24)	(0.26)	(0.24)	(0.26)
Treatment 1b (Lobbying)	−0.16	−0.31	−0.53*	−0.78**
	(0.22)	(0.26)	(0.24)	(0.28)
Treatment 1c (Mixed)		0.01		0.01
		(0.01)		(0.01)
Age		−0.24		−0.22
		(0.19)		(0.19)
Female		0.02		−0.02
		(0.05)		(0.05)
Education		−0.04		−0.06
		(0.07)		(0.08)
Income	6.44***	7.46***	5.49***	8.61***
	(0.13)	(1.64)	(0.14)	(1.78)
N	844	662	844	662
Adjusted R^2	−0.003	0.09	0.01	0.11

*p < .05; **p < .01; ***p < .001

Appendix C

Placebo Tests

Table C.1 Linear Regression Models of Political Trust by Placebo Dummy

	Politicians OLS		Offices OLS		Institutions OLS		Principles OLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Placebo	−0.20	−0.16	−0.18	−0.07	−0.12	0.06	−0.14	−0.39*
	(0.33)	(0.19)	(0.16)	(0.17)	(0.17)	(0.17)	(0.19)	(0.20)
Age		0.003		0.02		0.01		0.02*
		(0.01)		(0.01)		(0.01)		(0.01)
Female		−0.002		0.10		0.03		−0.07
		(0.22)		(0.19)		(0.19)		(0.21)
Education		−0.03		0.03		−0.02		−0.08
		(0.06)		(0.05)		(0.05)		(0.06)
Income		−0.08		−0.08		−0.06		−0.01
		(0.08)		(0.06)		(0.06)		(0.07)
Constant	5.01***	6.35**	5.56***	6.65*	5.56***	5.91*	6.36***	8.95***
	(0.23)	(2.15)	(0.12)	(2.75)	(0.12)	(2.55)	(0.14)	(2.57)
N	617	478	617	478	617	478	617	478
Adjusted R^2	0.0002	0.31	0.0001	0.34	−0.001	0.31	0.03	0.16

*p < .05; **p < .01; ***p < .001

Table C.2 Linear Regression Models of Social Trust by Placebo Dummy

	Particularized OLS		Generalized OLS	
	(1)	(2)	(3)	(4)
Placebo	−0.33	−0.30	−0.31	−0.49*
	(0.17)	(0.20)	(0.19)	(0.22)
Age		0.03**		0.03*
		(0.01)		(0.01)
Female		−0.18		−0.16
		(0.23)		(0.25)
Education		0.02		−0.03
		(0.06)		(0.07)
Income		−0.05		−0.11
		(0.08)		(0.09)
Constant	6.44***	7.18***	5.49***	9.96***
	(0.13)	(1.83)	(0.14)	(2.04)
N	617	478	617	478
Adjusted R^2	0.004	0.10	0.003	0.11

*p < .05; **p < .01; ***p < .001

Table C.3 Linear Regression Models of Trust in the UN by Experimental Condition

	Trust in the United Nations
	OLS
Treatment 1 (General)	−0.40
	(0.23)
Treatment 1a (Bribery)	−0.36
	(0.23)
Treatment 1b (Lobbying)	−0.53*
	(0.23)
Treatment 1c (Mixed)	−0.48*
	(0.23)
Age	−0.01
	(0.01)
Female	0.04
	(0.17)
Education	−0.02
	(0.04)
Income	−0.07
	(0.06)
Constant	8.62**
	(2.70)
N	1,187
Adjusted R^2	0.16

*p < .05; **p < .01; ***p < .001

Appendix D

Clustering and Principal Component Analysis

Fig. D.1 K-Means Cluster Plot of Survey Dataset

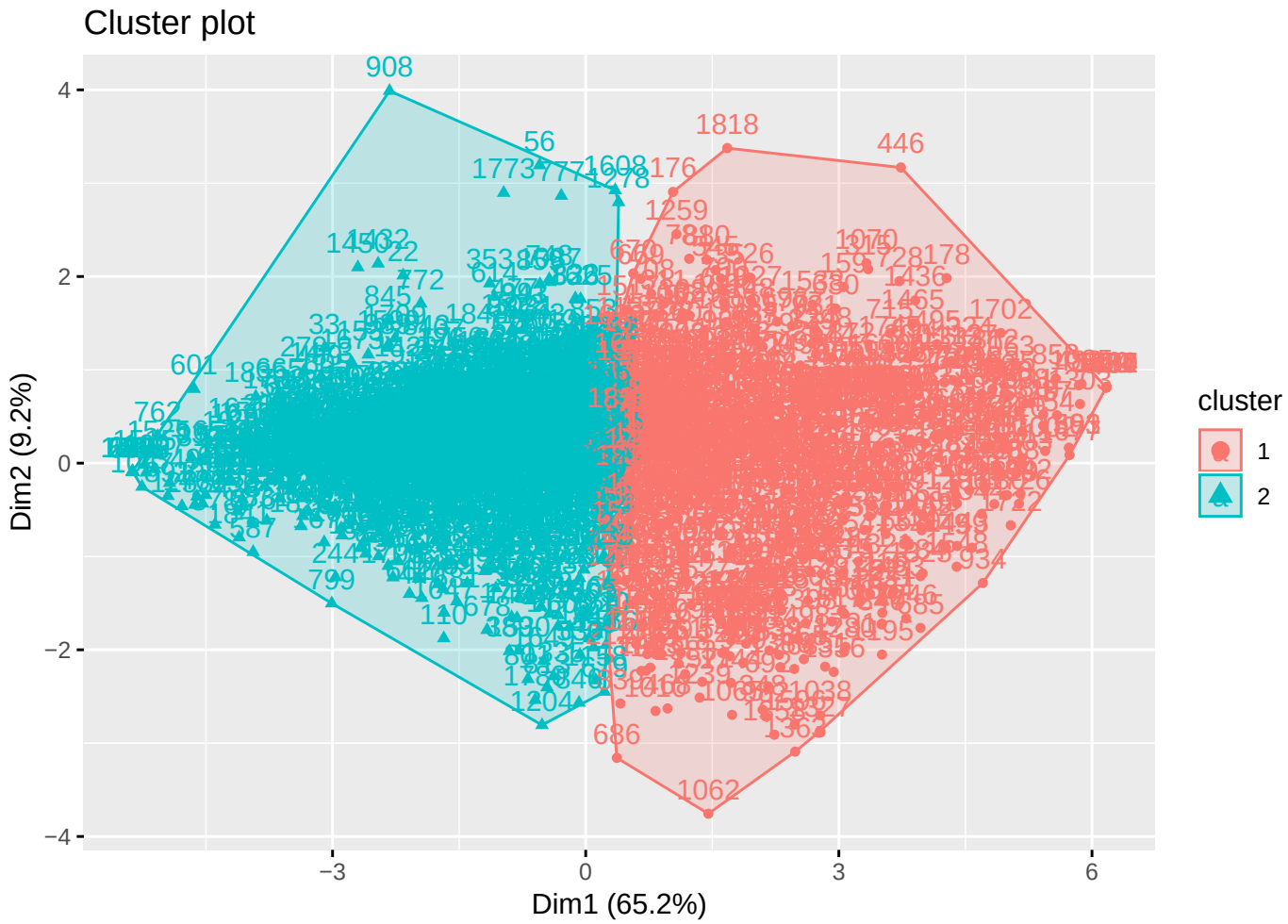
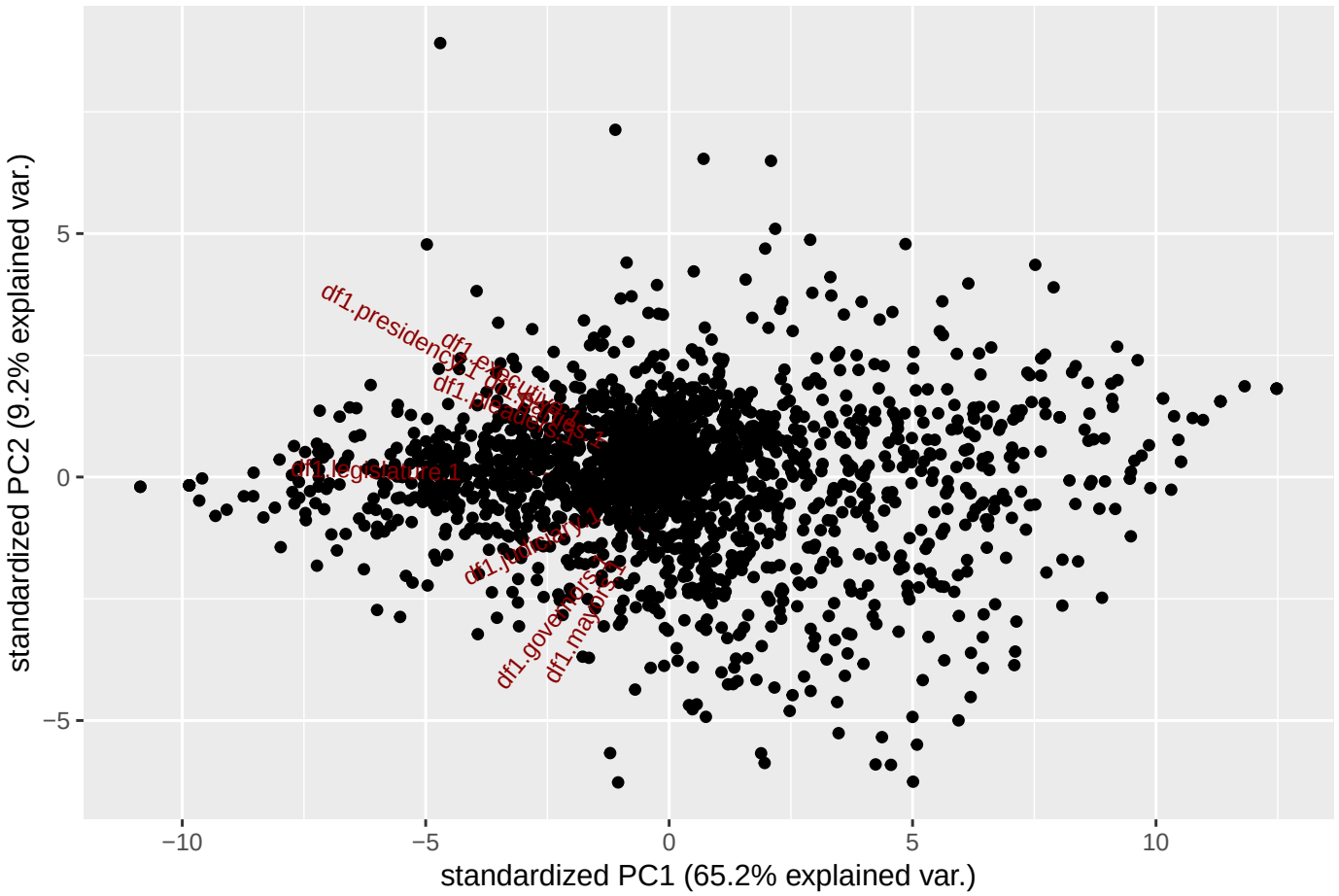


Fig. D.2 Principal Component Plot with Corresponding Variables



Appendix E

Heterogeneous Effects

Table E.1 Interaction Effects of Treatment*Political Ideology

	Politicians OLS	Offices OLS	Institutions OLS	Principles OLS
	(1)	(2)	(3)	(4)
Treatment 1 (General)	0.07	−0.22	−0.31	−0.26
	(0.29)	(0.23)	(0.23)	(0.26)
Treatment 1a (Bribery)	−0.14	−0.21	−0.09	−0.43
	(0.28)	(0.24)	(0.22)	(0.29)
Treatment 1b (Lobbying)	−0.24	−0.29	−0.35	−0.34
	(0.28)	(0.23)	(0.23)	(0.28)
Treatment 1c (Mixed)	−0.12	−0.26	−0.37	−0.47
	(0.27)	(0.22)	(0.22)	(0.28)
Political Ideology (Liberal)	0.29	0.01	0.12	0.69*
	(0.31)	(0.27)	(0.28)	(0.31)
Political Ideology (Moderate)	0.38	0.34	−0.02	0.44
	(0.35)	(0.33)	(0.31)	(0.36)
Treatment 1*Political Ideology (Liberal)	−0.64	−0.35	0.19	−0.46
	(0.43)	(0.36)	(0.37)	(0.41)
Treatment 1a*Political Ideology (Liberal)	−0.64	−0.34	−0.11	−0.53
	(0.43)	(0.37)	(0.36)	(0.44)
Treatment 1b*Political Ideology (Liberal)	−0.25	−0.05	−0.05	−0.58
	(0.43)	(0.36)	(0.38)	(0.42)
Treatment 1c*Political Ideology (Liberal)	−0.76	−0.62	−0.19	−0.56
	(0.42)	(0.34)	(0.35)	(0.44)
Treatment 1*Political Ideology (Moderate)	−0.83	−0.31	0.35	−0.38
	(0.50)	(0.45)	(0.42)	(0.48)
Treatment 1a*Political Ideology (Moderate)	−0.75	−0.85	−0.21	−0.98
	(0.58)	(0.48)	(0.48)	(0.53)
Treatment 1b*Political Ideology (Moderate)	−0.66	−0.42	0.18	−0.56
	(0.51)	(0.47)	(0.45)	(0.52)
Treatment 1c*Political Ideology (Moderate)	−0.54	−0.18	0.30	−0.20
	(0.51)	(0.46)	(0.42)	(0.51)
Constant	8.08***	4.69**	5.23**	4.08**
	(2.41)	(1.63)	(1.79)	(1.28)
N	1,180	1,180	1,180	1,180
Adjusted R^2	0.31	0.34	0.30	0.13

*p < .05; **p < .01; ***p < .001

Table E.2 Interaction Effects of Treatment*Party

	Politicians OLS	Offices OLS	Institutions OLS	Principles OLS
	(1)	(2)	(3)	(4)
Treatment 1 (General)	−0.09	−0.10	−0.25	−0.38
	(0.35)	(0.28)	(0.28)	(0.35)
Treatment 1a (Bribery)	−0.13	−0.25	−0.17	−0.84*
	(0.34)	(0.28)	(0.25)	(0.37)
Treatment 1b (Lobbying)	−0.28	−0.19	−0.34	−0.65
	(0.32)	(0.26)	(0.26)	(0.34)
Treatment 1c (Mixed)	0.08	−0.14	−0.25	−0.33
	(0.32)	(0.25)	(0.25)	(0.34)
Democratic Party	0.10	−0.41	−0.44	0.41
	(0.31)	(0.26)	(0.27)	(0.31)
Green Party	0.49	0.37	−0.62	−0.39
	(0.83)	(0.69)	(0.40)	(0.68)
Independent	−0.28	−0.05	−0.16	0.66
	(0.72)	(0.73)	(0.58)	(0.62)
Libertarian	−0.33	−0.36	0.21	−0.04
	(0.45)	(0.37)	(0.38)	(0.43)
Other	−0.60	−0.23	−0.02	0.12
	(0.45)	(0.38)	(0.36)	(0.47)
Treatment 1*Democratic Party	−0.13	−0.05	0.17	0.11
	(0.43)	(0.35)	(0.37)	(0.43)
Treatment 1a*Democratic Party	−0.70	−0.36	−0.12	−0.41
	(0.43)	(0.35)	(0.35)	(0.45)
Treatment 1b*Democratic Party	−0.96	−1.54	−0.16	0.08
	(1.14)	(0.99)	(0.91)	(1.30)
Treatment 1c*Democratic Party	−3.42***	−4.07***	−3.56***	−3.35*
	(0.95)	(0.89)	(0.61)	(1.47)
Treatment 1*Green Party	−0.90	−0.12	0.40	0.42
	(1.29)	(1.06)	(0.75)	(1.06)
Treatment 1a*Green Party	−1.39	−0.80	−0.06	−0.41
	(1.04)	(0.89)	(0.72)	(1.14)
Treatment 1b*Green Party	−0.37	−1.23	−0.74	−1.58*
	(0.97)	(0.89)	(0.72)	(0.77)
Treatment 1c*Green Party	−0.77	−1.46	−0.85	0.12
	(1.05)	(1.02)	(0.85)	(0.88)
Treatment 1*Libertarian Party	−0.20	−0.45	−0.24	0.55
	(0.89)	(0.97)	(0.83)	(1.03)
Treatment 1a*Libertarian Party	−1.37	−1.55	−1.04	−1.90
	(0.97)	(0.93)	(0.86)	(1.13)
Treatment 1b*Libertarian Party	8.10**	5.32**	5.69**	4.28**
	(2.53)	(1.72)	(1.96)	(1.35)
N	1,180	1,180	1,180	1,180
Adjusted R^2	0.31	0.35	0.31	0.13

*p < .05; **p < .01; ***p < .001

Table E.3 Interaction Effects of Treatment*Education

	Politicians OLS	Offices OLS	Institutions OLS	Principles OLS
	(1)	(2)	(3)	(4)
Treatment 1 (General)	−0.05	−0.57*	−0.29	−0.23
	(0.34)	(0.28)	(0.28)	(0.36)
Treatment 1a (Bribery)	0.04	−0.28	−0.11	−0.40
	(0.33)	(0.27)	(0.27)	(0.36)
Treatment 1b (Lobbying)	0.09	−0.23	−0.23	−0.29
	(0.31)	(0.26)	(0.27)	(0.33)
Treatment 1c (Mixed)	−0.38	−0.59*	−0.35	−0.24
	(0.33)	(0.26)	(0.24)	(0.35)
Education (Low)	−0.17	−0.62	0.32	0.43
	(0.50)	(0.46)	(0.42)	(0.51)
Education (Medium)	0.59*	−0.05	−0.05	0.46
	(0.28)	(0.24)	(0.24)	(0.29)
Treatment 1*Education (Low)	−0.44	0.50	−0.52	−0.94
	(0.68)	(0.63)	(0.61)	(0.70)
Treatment 1a*Education (Low)	0.05	0.38	−0.35	−0.37
	(0.72)	(0.68)	(0.56)	(0.71)
Treatment 1b*Education (Low)	−0.30	−0.12	−0.78	−0.71
	(0.75)	(0.62)	(0.59)	(0.81)
Treatment 1c*Education (Low)	−0.60	−0.27	−0.83	−1.14
	(0.66)	(0.59)	(0.56)	(0.66)
Treatment 1*Education (Medium)	−0.38	0.20	0.25	−0.31
	(0.43)	(0.36)	(0.35)	(0.43)
Treatment 1a*Education (Medium)	−1.01*	−0.42	−0.06	−0.66
	(0.42)	(0.35)	(0.34)	(0.43)
Treatment 1b*Education (Medium)	−0.91*	−0.26	−0.06	−0.53
	(0.41)	(0.35)	(0.36)	(0.41)
Treatment 1c*Education (Medium)	−0.11	0.19	0.07	−0.65
	(0.42)	(0.33)	(0.32)	(0.43)
Constant	7.82***	4.86**	5.47**	3.91**
	(2.33)	(1.61)	(1.79)	(1.31)
N	1,180	1,180	1,180	1,180
Adjusted R^2	0.31	0.34	0.30	0.13

*p < .05; **p < .01; ***p < .001

Table E.4 Interaction Effects of Treatment*Political Interest

	Politicians OLS	Offices OLS	Institutions OLS	Principles OLS
	(1)	(2)	(3)	(4)
Treatment 1 (General)	−0.32	−0.48	−0.41	−0.60
	(0.40)	(0.33)	(0.35)	(0.40)
Treatment 1a (Bribery)	−0.14	−0.33	−0.06	−0.64
	(0.40)	(0.35)	(0.33)	(0.42)
Treatment 1b (Lobbying)	−0.21	−0.41	−0.44	−0.74**
	(0.32)	(0.27)	(0.29)	(0.37)
Treatment 1c (Mixed)	−0.02	−0.11	−0.08	−0.45
	(0.35)	(0.27)	(0.29)	(0.38)
Political Interest (Low)	0.43	0.22	0.09	0.05
	(0.32)	(0.28)	(0.29)	(0.36)
Political Interest (Medium)	0.01	0.13	0.04	−0.20
	(0.34)	(0.29)	(0.30)	(0.36)
Treatment 1*Political Interest (Low)	0.002	0.25	0.34	0.17
	(0.49)	(0.40)	(0.42)	(0.49)
Treatment 1a*Political Interest (Low)	−0.75	−0.40	−0.26	−0.24
	(0.50)	(0.44)	(0.41)	(0.52)
Treatment 1b*Political Interest (Low)	−0.25	0.23	0.35	0.23
	(0.44)	(0.38)	(0.39)	(0.48)
Treatment 1c*Political Interest (Low)	−0.38	−0.51	−0.44	−0.40
	(0.46)	(0.37)	(0.38)	(0.48)
Treatment 1*Political Interest (Medium)	0.02	−0.08	0.22	0.13
	(0.51)	(0.44)	(0.44)	(0.52)
Treatment 1a*Political Interest (Medium)	−0.12	0.10	0.05	−0.09
	(0.52)	(0.45)	(0.42)	(0.53)
Treatment 1b*Political Interest (Medium)	−0.44	−0.17	−0.09	0.07
	(0.51)	(0.41)	(0.42)	(0.51)
Treatment 1c*Political Interest (Medium)	−0.96**	−0.61	−0.39	−0.29
	(0.48)	(0.39)	(0.39)	(0.49)
Constant	7.81***	4.54***	5.31***	4.21***
	(2.41)	(1.70)	(1.83)	(1.25)
N	1,180	1,180	1,180	1,180

*p < .1; **p < .05; ***p < .01

Table E.5 Interaction Effects of Treatment*Political Knowledge

	Politicians OLS	Offices OLS	Institutions OLS	Principles OLS
	(1)	(2)	(3)	(4)
Treatment 1 (General)	−0.21	−0.45	−0.18	−0.39
	(0.32)	(0.26)	(0.27)	(0.32)
Treatment 1a (Bribery)	−0.87*	−0.59*	−0.28	−0.76*
	(0.35)	(0.30)	(0.27)	(0.33)
Treatment 1b (Lobbying)	−0.25	−0.41	−0.34	−0.69*
	(0.31)	(0.26)	(0.26)	(0.32)
Treatment 1c (Mixed)	−0.49	−0.49	−0.51*	−0.44
	(0.30)	(0.25)	(0.25)	(0.32)
Political Knowledge (Low)	−0.11	0.49	−0.02	−0.67
	(1.18)	(0.95)	(0.84)	(1.33)
Political Knowledge (Medium)	0.18	0.01	−0.05	−0.11
	(0.27)	(0.24)	(0.24)	(0.28)
Treatment 1*Political Knowledge (Low)	−1.02	−0.59	−0.88	−0.29
	(1.32)	(1.08)	(1.01)	(1.37)
Treatment 1a*Political Knowledge (Low)	2.18	0.45	1.46	1.58
	(1.34)	(1.16)	(1.12)	(1.57)
Treatment 1b*Political Knowledge (Low)	0.84	0.01	0.65	0.71
	(1.59)	(1.25)	(1.11)	(1.44)
Treatment 1c*Political Knowledge (Low)	0.42	0.27	0.85	0.84
	(1.37)	(1.04)	(0.91)	(1.44)
Treatment 1*Political Knowledge (Medium)	−0.03	0.14	0.09	−0.08
	(0.40)	(0.34)	(0.33)	(0.40)
Treatment 1a*Political Knowledge (Medium)	0.49	0.17	0.13	−0.08
	(0.44)	(0.37)	(0.35)	(0.42)
Treatment 1b*Political Knowledge (Medium)	−0.32	0.04	−0.01	0.05
	(0.39)	(0.33)	(0.33)	(0.39)
Treatment 1c*Political Knowledge (Medium)	−0.01	−0.06	0.15	−0.47
	(0.39)	(0.33)	(0.32)	(0.40)
Constant	8.30***	4.98**	5.71***	4.52**
	(1.91)	(1.54)	(1.42)	(1.39)
N	1,180	1,180	1,180	1,180
Adjusted R^2	0.31	0.34	0.31	0.13

*p < .05; **p < .01; ***p < .001

Table E.6 Interaction Effects of Treatment*Optimism

	Politicians OLS	Offices OLS	Institutions OLS	Principles OLS
	(1)	(2)	(3)	(4)
Treatment 1 (General)	−0.48	−0.79*	−0.75*	−0.64
	(0.41)	(0.32)	(0.33)	(0.38)
Treatment 1a (Bribery)	−0.69	−0.78*	−0.70*	−0.78
	(0.46)	(0.37)	(0.32)	(0.44)
Treatment 1b (Lobbying)	−0.13	−0.35	−0.63*	−0.39
	(0.40)	(0.31)	(0.32)	(0.39)
Treatment 1c (Mixed)	−0.22	−0.42	−0.68*	−0.97*
	(0.42)	(0.31)	(0.33)	(0.47)
Optimism (Low)	−0.26	−0.33	−0.72**	−0.30
	(0.34)	(0.29)	(0.28)	(0.37)
Optimism (Medium)	−0.56	−0.76*	−1.05***	−0.24
	(0.35)	(0.31)	(0.30)	(0.39)
Treatment 1*Optimism (Low)	0.07	0.54	0.71	0.06
	(0.50)	(0.40)	(0.41)	(0.46)
Treatment 1a*Optimism (Low)	0.12	0.27	0.56	−0.002
	(0.56)	(0.46)	(0.40)	(0.53)
Treatment 1b*Optimism (Low)	−0.53	−0.29	0.19	−0.35
	(0.50)	(0.41)	(0.41)	(0.48)
Treatment 1c*Optimism (Low)	−0.23	−0.17	0.45	0.60
	(0.51)	(0.40)	(0.40)	(0.55)
Treatment 1*Optimism (Medium)	0.50	0.53	0.83	0.42
	(0.51)	(0.43)	(0.42)	(0.51)
Treatment 1a*Optimism (Medium)	0.40	0.57	0.88*	0.02
	(0.56)	(0.47)	(0.43)	(0.56)
Treatment 1b*Optimism (Medium)	−0.30	0.26	0.67	−0.31
	(0.52)	(0.42)	(0.43)	(0.52)
Treatment 1c*Optimism (Medium)	−0.43	0.005	0.38	0.06
	(0.51)	(0.41)	(0.42)	(0.55)
Constant	7.89**	4.56**	5.44**	4.26***
	(2.49)	(1.59)	(1.73)	(1.23)
N	1,180	1,180	1,180	1,180
Adjusted R^2	0.31	0.34	0.31	0.13

*p < .05; **p < .01; ***p < .001

Table E.7 Interaction Effects of Treatment*Postmaterialism

	Politicians OLS	Offices OLS	Institutions OLS	Principles OLS
	(1)	(2)	(3)	(4)
Treatment 1 (General)	−0.68	−1.29*	−0.88	−0.54
	(0.74)	(0.64)	(0.58)	(0.73)
Treatment 1a (Bribery)	−1.30	−1.69*	−0.94	−1.31
	(0.79)	(0.74)	(0.60)	(0.69)
Treatment 1b (Lobbying)	−1.12	−1.05	−0.79	−0.89
	(0.68)	(0.60)	(0.66)	(0.68)
Treatment 1c (Mixed)	−1.52*	−1.70**	−1.46*	−2.27**
	(0.68)	(0.61)	(0.68)	(0.83)
Postmaterialist	−0.64	−1.54*	−1.37*	−0.21
	(0.74)	(0.63)	(0.58)	(0.69)
Rather Materialist	−0.05	−0.64	−0.71	−0.64
	(0.57)	(0.48)	(0.51)	(0.55)
Rather Postmaterialist	−0.30	−1.03*	−0.90	−0.83
	(0.57)	(0.49)	(0.51)	(0.55)
Treatment 1*Postmaterialist	0.65	1.10	0.73	−0.55
	(0.98)	(0.85)	(0.75)	(0.96)
Treatment 1a*Postmaterialist	−0.13	1.70	−0.12	−0.03
	(1.18)	(0.99)	(0.84)	(1.19)
Treatment 1b*Postmaterialist	0.93	1.17	0.99	0.86
	(1.05)	(0.86)	(0.83)	(1.14)
Treatment 1c*Postmaterialist	0.63	0.48	0.68	1.13
	(0.95)	(0.83)	(0.83)	(1.11)
Treatment 1*Rather Materialist	0.37	0.92	0.80	0.07
	(0.79)	(0.68)	(0.64)	(0.79)
Treatment 1a*Rather Materialist	0.86	1.38	1.01	0.45
	(0.85)	(0.78)	(0.64)	(0.75)
Treatment 1b*Rather Materialist	0.57	0.55	0.40	0.18
	(0.74)	(0.65)	(0.71)	(0.74)
Treatment 1c*Rather Materialist	1.07	1.28*	1.25	1.56
	(0.73)	(0.65)	(0.71)	(0.88)
Treatment 1*Rather Postmaterialist	0.37	0.94	0.67	0.12
	(0.79)	(0.69)	(0.63)	(0.78)
Treatment 1a*Rather Postmaterialist	0.95	1.16	0.71	0.77
	(0.85)	(0.78)	(0.65)	(0.76)
Treatment 1b*Rather Postmaterialist	0.86	0.83	0.56	0.36
	(0.74)	(0.65)	(0.71)	(0.75)
Treatment 1c*Rather Postmaterialist	1.26	1.45*	1.15	1.89*
	(0.74)	(0.67)	(0.73)	(0.89)
Constant	8.69***	5.54***	5.93***	4.72***
	(2.46)	(1.65)	(1.80)	(1.29)
N	1,180	1,180	1,180	1,180
Adjusted R^2	0.31	0.34	0.30	0.13

*p < .05; **p < .01; ***p < .001

Table E.8 Interaction Effects of Treatment*Corruption Awareness

	Politicians	Offices	Institutions	Principles
	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)
Treatment 1 (General)	0.09	−0.09	−0.06	−0.54
	(0.32)	(0.25)	(0.26)	(0.32)
Treatment 1a (Bribery)	−0.36	−0.43	−0.10	−0.22
	(0.31)	(0.25)	(0.23)	(0.30)
Treatment 1b (Lobbying)	0.03	−0.08	−0.15	−0.41
	(0.32)	(0.25)	(0.27)	(0.32)
Treatment 1c (Mixed)	−0.41	−0.42	−0.25	−0.34
	(0.29)	(0.23)	(0.22)	(0.30)
Corruption Awareness (Unaware)	0.73**	0.38	0.34	0.56*
	(0.27)	(0.23)	(0.23)	(0.28)
Treatment 1*Corruption Awareness (Unaware)	−0.64	−0.52	−0.20	0.10
	(0.41)	(0.33)	(0.34)	(0.39)
Treatment 1a*Corruption Awareness (Unaware)	−0.24	−0.08	−0.11	−0.94*
	(0.40)	(0.34)	(0.33)	(0.40)
Treatment 1b*Corruption Awareness (Unaware)	−0.78	−0.50	−0.29	−0.38
	(0.40)	(0.33)	(0.34)	(0.40)
Treatment 1c*Corruption Awareness (Unaware)	−0.10	−0.14	−0.22	−0.64
	(0.38)	(0.31)	(0.31)	(0.39)
Constant	8.18***	4.67**	5.24**	3.88**
	(2.42)	(1.62)	(1.77)	(1.24)
N	1,180	1,180	1,180	1,180
Adjusted R^2	0.31	0.34	0.30	0.14

*p < .05; **p < .01; ***p < .001

Appendix F

Causal Mechanism Analysis

Table F.1 Interaction Effects of Treatment*Socialization

	Politicians OLS	Principles OLS	Politicians OLS	Principles OLS
	(1)	(2)	(3)	(4)
Treatment 1 (General)	−0.34	−0.31	−0.24	−0.39
	(0.28)	(0.30)	(0.26)	(0.25)
Treatment 1a (Bribery)	0.07	−0.58	−0.56*	−0.60*
	(0.29)	(0.31)	(0.28)	(0.28)
Treatment 1b (Lobbying)	−0.19	−0.65*	−0.16	−0.41
	(0.27)	(0.30)	(0.27)	(0.26)
Treatment 1c (Mixed)	−0.30	−0.41	−0.53*	−0.54
	(0.29)	(0.32)	(0.26)	(0.28)
Earlier (Democracy)	0.67*	0.25		
	(0.27)	(0.28)		
Earlier (Politicians)			−0.02	0.18
			(0.26)	(0.26)
Treatment 1*Early (Democracy)	0.08	−0.15		
	(0.38)	(0.39)		
Treatment 1a*Early (Democracy)	−0.93*	−0.21		
	(0.40)	(0.41)		
Treatment 1b*Early (Democracy)	−0.42	0.16		
	(0.38)	(0.38)		
Treatment 1c*Early (Democracy)	−0.38	−0.32		
	(0.39)	(0.40)		
Treatment 1*Early (Politicians)			−0.17	−0.02
			(0.39)	(0.38)
Treatment 1a*Early (Politicians)			0.25	−0.21
			(0.40)	(0.39)
Treatment 1b*Early (Politicians)			−0.60	−0.36
			(0.38)	(0.37)
Treatment 1c*Early (Politicians			0.06	−0.10
			(0.39)	(0.38)
Constant	10.13***	3.52**	10.42***	3.38*
	(2.35)	(1.34)	(2.18)	(1.39)
N	1,187	1,187	1,187	1,187
Adjusted R^2	0.31	0.15	0.31	0.15

*p < .05; **p < .01; ***p < .001

Table F.2 Interaction Effects of Treatment*Instrumentalism (Specific)

	Politicians OLS	Principles OLS
	(1)	(2)
Treatment 1 (General)	−1.01	−0.21
	(0.59)	(0.57)
Treatment 1a (Bribery)	−0.41	−0.41
	(0.54)	(0.55)
Treatment 1b (Lobbying)	−0.54	−0.74*
	(0.40)	(0.37)
Treatment 1c (Mixed)	−0.86	−0.95
	(0.65)	(0.66)
Rather Instrumental	−0.71	−0.26
	(0.43)	(0.46)
Rather Noninstrumental	−0.63	0.26
	(0.42)	(0.47)
Noninstrumental	−0.59	0.67
	(0.47)	(0.50)
Treatment 1*Rather Instrumental	1.22	0.25
	(0.69)	(0.67)
Treatment 1a*Rather Instrumental	0.32	0.33
	(0.65)	(0.64)
Treatment 1b*Rather Instrumental	0.43	0.54
	(0.55)	(0.51)
Treatment 1c*Rather Instrumental	0.60	0.75
	(0.75)	(0.74)
Treatment 1*Rather Noninstrumental	0.55	−0.52
	(0.67)	(0.65)
Treatment 1a*Rather Noninstrumental	−0.34	−0.66
	(0.62)	(0.65)
Treatment 1b*Rather Noninstrumental	0.13	0.05
	(0.50)	(0.50)
Treatment 1c*Rather Noninstrumental	0.34	0.20
	(0.72)	(0.74)
Treatment 1*Noninstrumental	0.69	−0.33
	(0.71)	(0.68)
Treatment 1a*Noninstrumental	0.003	−0.61
	(0.69)	(0.69)
Treatment 1b*Noninstrumental		
Treatment 1c*Noninstrumental	0.34	0.30
	(0.77)	(0.78)
Constant	11.37***	3.79**
	(2.28)	(1.44)
N	1,150	1,150
Adjusted R^2	0.31	0.15

*p < .05; **p < .01; ***p < .001

Table F.3 Interaction Effects of Treatment*Instrumentalism (Diffuse)

	Politicians OLS	Principles OLS
	(1)	(2)
Treatment 1 (General)	−0.36	0.26
	(0.38)	(0.34)
Treatment 1a (Bribery)	−0.60	−0.42
	(0.39)	(0.39)
Treatment 1b (Lobbying)	−0.49	−0.52
	(0.71)	(0.60)
Treatment 1c (Mixed)	−0.24	−0.28
	(0.35)	(0.34)
Rather Instrumental	−0.46	0.19
	(0.34)	(0.35)
Rather Noninstrumental	−0.06	0.65
	(0.34)	(0.35)
Noninstrumental	−0.31	0.05
	(0.63)	(0.50)
Treatment 1*Rather Instrumental	0.26	−0.60
	(0.51)	(0.47)
Treatment 1a*Rather Instrumental	0.65	−0.21
	(0.50)	(0.53)
Treatment 1b*Rather Instrumental	−0.20	−0.40
	(0.79)	(0.71)
Treatment 1c*Rather Instrumental	−0.33	−0.30
	(0.48)	(0.48)
Treatment 1*Rather Noninstrumental	−0.51	−1.37**
	(0.50)	(0.49)
Treatment 1a*Rather Noninstrumental	−0.03	−0.70
	(0.55)	(0.55)
Treatment 1b*Rather Noninstrumental	0.35	−0.15
	(0.79)	(0.70)
Treatment 1c*Rather Noninstrumental	−0.42	−0.77
	(0.50)	(0.52)
Treatment 1*Noninstrumental	0.88	−0.22
	(0.80)	(0.68)
Treatment 1a*Noninstrumental	−0.73	0.05
	(0.85)	(0.69)
Treatment 1b*Noninstrumental		
Treatment 1c*Noninstrumental	0.14	0.38
	(0.92)	(0.82)
Constant	10.77***	3.51*
	(2.07)	(1.50)
N	1,106	1,106
Adjusted R^2	0.32	0.14

*p < .05; **p < .01; ***p < .001

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