Essays on Political Corruption

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Abstract

This dissertation presents three essays offering explanations for the persistence of corruption despite electoral competition.

The first essay, co-authored with Adam Glynn and Nahomi Ichino, asks what the effect of electoral systems is on corruption. Persson, Tabellini and Trebbi (2003) proposed that plurality electoral systems should lead to lower corruption compared to proportional representation (PR) systems because the former creates a direct link between voters and politicians whom voters can hold accountable for corruption. The empirical question remains unresolved, however, in part due to the endogeneity of the electoral institutions and difficulties in measuring corruption. Using nonparametric methods and new data to reduce sensitivity to these problems, we find no evidence for this hypothesis. Instead, we find some evidence in the opposite direction, that PR leads to less corruption.

The second essay makes a theoretical distinction between voters' perceptions of the corruption of the political system and of individual politicians. Evidence from original interviews and focus group discussions, as well as public opinion data shows that many Ugandan citizens perceive their political system to be highly corrupt. In particular, they perceive corrupt acts to be widespread, do not expect perpetrators to be punished, and have difficulty distinguishing "honest" candidates. These characteristics cause voters who perceive the system to be highly corrupt to be less likely to punish overtly corrupt individuals by withdrawing electoral support. In some cases, they even prefer clearly corrupt candidates.

The third essay argues that to understand when voters hold politicians accountable for corruption, it is necessary to understand who they perceive to be corrupt. It presents

evidence from a survey experiment showing that American voters perceive copartisan politicians to be less corrupt than those from the other political party or without a party label. This pattern is consistent with motivated reasoning in which voters expend extra cognitive resources to process information that contradicts their partisan leanings rather than from the use of party labels as heuristics to avoid cognitive burdens. Furthermore, I show that the ideological orientation of the media source reporting allegations of corruption affects whether they are viewed as credible. Counterstereotypical allegations – i.e., those that come from a media source that is ideologically similar to the politician – are taken more seriously by respondents. In fact, when partisans view counterstereotypical allegations, they exhibit less bias toward copartisans. In sum, this research demonstrates that in-group favoritism poses a challenge to democratic accountability, but that motivated reasoning is bounded by the evidence voters view, and thus that media sources with well-known ideological orientations may serve a particularly important role in encouraging democratic accountability among their bases.

Contents

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Introduction

Many theories of democratic accountability expect voters to withdraw support from corrupt politicians, yet high corruption and electoral competition coexist in many countries. This dissertation presents three essays offering explanations for this persistence. First, in a cross-national analysis focusing on electoral institutions, we show that though plurality electoral systems theoretically facilitate accountability for individual legislators, there is no empirical evidence they reduce corruption. The second essay focuses on perceptions of the political system, arguing that when Ugandan voters perceive their system to be highly corrupt they are less inclined to hold individual politicians accountable. The final essay focuses on individuals' cognitive processes, showing that Americans are more reluctant to identify politicians from their own parties as corrupt, and expend cognitive resources to arrive at these more favorable conclusions. Thus accountability is complicated by biased evaluations of who is actually corrupt.

Chapter 1,"Electoral Systems and Corruption" co-authored with Adam Glynn and Nahomi Ichino, asks how electoral systems affect corruption. Persson, Tabellini and Trebbi (2003) proposed that plurality electoral systems should lead to lower corruption compared to proportional representation (PR) systems because the former creates a direct link between voters and politicians whom voters can hold accountable for corruption. We address empirical challenges due to the endogeneity of the electoral institutions and difficulties in measuring corruption in several ways. We focus on within-country changes in corruption following electoral systems transitions, which eliminates issues of post-treatment bias that complicate the interpretation of coefficients in many large-N analyses. Second, we construct

synthetic controls based on each country's levels of corruption before the electoral system transitioned, which helps ameliorate concerns of bias due to endogeneity of electoral systems to political processes. Finally, to test for statistical significance we use the nonparametric signed rank statistic, which is less sensitive to error in measurement of corruption.

We find no evidence that plurality systems reduce corruption. Instead, we find some evidence in the opposite direction, that PR leads to less corruption. These results are robust to using a variety of fixed effects specifications and country samples. Given these results, this chapter concludes that more research is needed to understand the conditions under which citizens hold leaders accountable for corruption. The next two chapters are attempts to answer this call by focusing on how the level of corruption in the political system and individual cognitive processes affect the probability of accountability.

Chapter 2, "The System Matters: Corruption and Vote Choice in Uganda," makes a theoretical distinction between voters' perceptions of the corruption of the political system and of individual politicians. Evidence from original interviews and focus group discussions, as well as public opinion data shows that many Ugandan citizens perceive their political system to be highly corrupt. In particular, they perceive corrupt acts to be widespread, do not expect perpetrators to be punished, and have difficulty distinguishing 'honest' candidates. I argue that these characteristics cause voters who perceive the system to be highly corrupt to be less likely to punish clearly corrupt individuals by withdrawing electoral support. In some cases, they even prefer corrupt candidates. In addition, I present suggestive evidence from Afrobarometer survey data showing that perceptions of corruption affect political behaviors beyond vote choice. In particular, I show that perceptions that the political system is highly corrupt are negatively correlated with measures of political efficacy, reported turnout, and even democratic aspirations. This chapter thus answers the overarching question of when voters hold politicians accountable by arguing that when citizens believe their political system is highly corrupt, they are unlikely to hold politicians accountable for malfeasance through ordinary democratic channels.

Chapter 3, "Who is Corrupt? Cognitive Bases of Biased Perceptions," begins with the

premise that to understand when voters hold politicians accountable for corruption, it is necessary first to understand who they perceive to be corrupt. In particular, this chapter tests the hypothesis that citizens perceive copartisan politicians to be less corrupt and that this effect operates through motivated reasoning in which voters expend extra cognitive resources to process information that contradicts their partisan leanings rather than from the use of party labels as heuristics to avoid cognitive burdens. It also explores the role of media in fostering accountability by testing whether allegations reported in media sources that are ideological similar to politicians (counterstereotypical allegations) are viewed as more credible. To address confounding from citizens selecting their news sources, I use a survey experiment in which respondents view a hypothetical news article reporting allegations of corruption against a politician, with the politician's political party and news source randomized.

I find that Americans do perceive copartisan politicians to be less corrupt than those from the other political party or without a party label. Furthermore, respondents spend longer viewing allegations against their copartisans, which is consistent with motivated reasoning rather than the use of heuristics. Additionally, the ideological orientation of the media source reporting allegations of corruption affects whether they are viewed as credible. Counterstereotypical allegations are taken more seriously by respondents. In fact, when partisans view these allegations, they exhibit less bias toward copartisans. In sum, in-group favoritism poses a challenge to democratic accountability because citizens are motivated to reach favorable conclusions about politicians from their own party. Motivated reasoning is bounded by the credibility of evidence, however, thus ideological media sources may serve a particularly important role in fostering accountability among their bases. Thus this chapter answers the broader question of when voters hold politicians accountable by arguing that copartisanship makes voters less inclined to take allegations seriously, but that sufficiently credible evidence helps overcome this bias.

In sum, each essay represents an attempt to answer the broad question of why corruption persists in electoral democracies. Although these essays offer suggestive evidence, they leave

certain questions unanswered. In particular, future work can confirm chapter 1's findings using additional data after electoral transitions to test the presence of long-term effects of electoral systems, test the theory developed in chapter 2 in contexts with varying levels of perceived system-corruption, and tie the differences in perceived corruption of copartisan candidates documented in chapter 3 to voting behavior. More generally, to explain how corruption affects actual electoral outcomes, more work needs to be done to integrate theories focusing on vote choice and those focusing on turnout.

1 Electoral Systems and Corruption with Adam N. Glynn and Nahomi Ichino

1.1 Introduction

What is the effect of electoral systems on corruption? A *Petition to Restore Power to the People* recently circulated in the Republic of South Africa called for electoral system reform to create a closer connection between voters and their elected representatives in order to help prevent "public servants [... from] us[ing] their position to enrich themselves and their families" (Agang South Africa 2013). This followed a 2009 report from the Independent Panel of Assessment of Parliament, which argued that South Africa's party list system makes Members of Parliament more accountable to party leaders than to the electorate (Independent Panel Assessment of Parliament 2009). Adding a "constituency-based element into the electoral system" to connect representatives more closely to voters is now part of the platform of the opposition party Democratic Alliance (Democratic Alliance 2013).

Corruption – defined as the misuse of public office for private gain (Treisman 2000) – is an urgent problem in South Africa and around the world (Transparency International 2012). It is linked to greater income inequality (Gupta et al. 2002), lower levels of socially-productive innovation (Murphy et al. 1993), inefficient investment by firms (Choi and Thum 2004; Svensson 2003), and lower human capital (Reinikka and Svensson 2005).[1] Corruption is also associated with lower trust in and satisfaction with the political system (Anderson and Tverdova 2003; Chang and Chu 2006; Seligson 2002) and lower voter turnout (Davis

[1]Perhaps due to measurement difficulties, the influence of corruption on economic growth at the country level has not been well-established (Mauro 1995; Graf Lambsdorff 2005; Svensson 2005).

et al. 2004, McCann and Domínguez 1998). Although reformers cannot easily change the cultural and historical-institutional legacies associated with corruption (Serra 2006; Treisman 2000, 2007), changes to the electoral system might dislodge this political-economic equilibrium of low public engagement and high corruption.

The logic behind this call for electoral reform in South Africa echoes the scholarly view that citizens want to vote out corrupt politicians and that political institutions that empower citizens to do so will lead to lower corruption. Persson et al. (2003, 961-2) contends that plurality systems should lead to lower corruption than proportional representation (PR) systems, because the former creates a direct link between voters and politicians whom voters can hold accountable for corruption. In the same framework, Kunicová and Rose-Ackerman (2005) argues that while plurality systems offer more opportunities for corruption to rank-and-file politicians, PR systems offer those opportunities to party leaders. Because voters are less able to monitor and sanction party leaders in a PR system, corruption will be greater in PR systems. Treisman (2007) challenges the robustness of both results, and there is currently little consensus on how electoral systems affect the extent of politicians' misuse of public office for private gain.

Several difficulties leave this question unresolved. First, corruption is difficult to measure (Apaza 2009; Arndt and Oman 2006) and the available indices of perceptions of corruption measure slightly different concepts. Scholars have been rightly cautious about their use. Second, the endogeneity of a country's electoral system to its politics is generally not sufficiently addressed. Although cross-sectional analyses of corruption often include controls for factors that likely affect the choice of electoral systems (Treisman 2007), the calculations of strategic actors highlighted in the theoretical and case-specific literature on electoral system choice are varied and difficult to capture. Consequently, the estimated causal effects may be biased, and the direction of this bias is difficult to characterize. Third, cross-sectional studies generally overlook *when* an electoral system was adopted. Some control variables are therefore measured pre-treatment for some units and post-treatment for other units, and the resulting estimated average effect is a weighted average of different types of treatment

effects. Persson et al. (2003) uses panel data and fixed effects to address some of these problems, but Treisman (2007) questions the reliability of the corruption measure was available to Persson et al. (2003).

We address these difficulties in several ways. First, as in a fixed effects analysis, we focus on within-country changes from one electoral system to another. By noting the timing of the treatment, we know when the confounding variables should be measured. Requiring information on the pre-transition electoral system also effectively limits the analysis to countries that did not simultaneously transition to democracy and change various other political institutions. This leaves us with ten countries that adopted closed list PR systems between 1998 and 2009, and we adopt a Fisherian randomization inference framework for the statistical analysis to accommodate this small number of observations. Second, to ameliorate confounding issues, we use pre-treatment values of corruption to construct a synthetic control unit for each of our treated units (Abadie et al. 2010). A country that changes its electoral system is compared to a weighted average of countries that have the same pre-transition electoral system and are in the same region of the world. Third, to address some of the concerns with measurement of the outcome variable, we use the nonparametric signed rank statistic to test for statistical significance as discussed in Glynn and Ichino (2014). These data issues and methods are described in greater detail in the following sections.

We find no support for the hypothesis that PR leads to more corruption. In fact, we find some evidence suggesting that PR might lead to *less* corruption, although this evidence is not statistically significant at traditional levels. Two robustness checks further strengthen our results. First, we improve the matches with a differences-in-differences approach and obtain similar results. Second, we introduce a dose-weighted version of the signed rank statistic (Rosenbaum 2002, 2009) in order to account for the heterogeneity in the pre-transition electoral systems. Despite using several different approaches, including the fixed effects regressions mirroring Persson et al. (2003)'s specifications (Appendix A), we were unable to find support for the findings of Persson et al. (2003).

There are a number of important caveats to our results. First, they are limited in scope

to the countries for which we observed the adoption of closed list PR. Persson et al. (2003)'s hypothesis may still hold for countries out of this sample. Second, not enough time may have passed for effects to accrue in these ten countries. Third, our analyses use measures of corruption based upon expert surveys. We have used a nonparametric approach so as to be less sensitive to measurement problems, and the results are relatively stable across different indices. But a different measure of corruption that relies less on perceptions may still reverse these findings. Finally, the synthetic control and difference-in-differences approaches may not have eliminated all bias due to unmeasured confounding, which is likely if the measures of corruption we use are poor. A Rosenbaum (2002) style sensitivity analysis on these results is relatively straightforward. However, this would only weaken our confidence that PR might be reducing corruption for these countries, not strengthen the evidence for the Persson et al. (2003) hypothesis.

Despite these caveats, our results suggest that further work is necessary before the Persson et al. (2003) hypothesis should be accepted or used to guide institutional design. Either new measures of corruption need to be developed or extant theories should be re-examined for what purposes and under what conditions voters hold politicians accountable for corruption. In particular, theory more appropriate for conditions in new democracies can contribute to the scholarship on corruption.

In the following section, we discuss the existing theoretical perspectives on how electoral systems affect corruption. We discuss three broad difficulties with current empirical analyses of this question and describe a methodological approach to address these issues, before presenting our results.

1.2 Electoral Rules, Accountability, and Corruption

Electoral rules for legislative bodies define how votes are converted into a set of legislators following an election. These rules may be very complex, but the most basic distinction is between PR systems and plurality systems. In PR systems, legislative seats are allocated

to political parties on the basis of the total votes won by each party. Voters may express preferences over particular candidates within a party in an open list PR system. But in a closed list PR system, party leaders determine the order in which individual politicians are ranked on the party list. Once the total number of seats awarded to a party is determined, that number of candidates from the top of the list are elected. By contrast, in plurality or majoritarian systems, the candidate or party with the greatest number of votes wins all the seats in a district. These districts will often only have one seat, and in some cases, the candidate or party must also meet a majoritarian vote threshold such as 50%. As we describe in more detail in Section 1.5.2, a mixed system uses both of these systems for elections to the same legislative body. While we focus on this broad distinction between plurality and PR systems, electoral systems can also vary in the number of total seats in the legislature, number of districts, and district magnitudes, as well as the exact formula for allocating seats within these broad categories.

Persson et al. (2003, 961–2) proposes that electoral systems affect corruption by changing the incentives for politicians to engage in corruption. Since party leaders determine the placement of politicians on party lists, political parties intervene in the chain of delegation from voters to politicians in PR systems and incentivize politicians to exert effort in and for the party rather than in office for the electorate. Furthermore, because the number of seats won by a party in PR systems depends upon the total number of votes won by the party's candidates, neither the negative electoral consequences of engaging in corruption nor the electoral rewards from refraining from corruption are internalized by an individual politician as fully as they would be in plurality systems. Consequently, it expects PR to lead to more corruption than do plurality systems, and mixed systems to lead to a middle level of corruption, less than with a PR system but more than with a plurality system.[2]

[2] Persson et al. 2003 also examines the role of district magnitude, noting that electoral reforms may have countervailing effects when they change both district magnitude and whether individuals or parties are selected. Nonetheless, the paper offers evidence in favor of the hypothesis that "Plurality rule in small districts should be associated with less corruption than PR in large districts" (963) and interprets real-world differences in corruption as results of plurality rule (983-984). We also run fixed effects specifications controlling for district magnitude and find no evidence that plurality systems reduce corruption.

With a similar logic, Kunicová and Rose-Ackerman (2005) argues that corruption should be worse under closed list than open list PR systems, because political rents are primarily extracted by party leaders rather than by rank-and-file politicians, and voters are less able to hold these party leaders accountable in the former than in the latter. Gingerich (2009) also associates closed list PR systems with more corruption than open list PR systems, proposing that party leaders in closed list PR systems can entice bureaucrats to engage in corruption by promising rewards of positions within the party, a power party leaders lack in open list PR systems.

However, Golden and Chang (2001) and Chang (2005) note that open list PR and plurality systems could lead to *more*, not less, corruption than closed list PR, since the former allow voters to favor or disfavor individual politicians. This gives incentives to politicians to "cultivate the personal vote" (Carey and Shugart 1995) and to turn to corruption to finance these activities, with greater pressure where politicians face greater competition such as in high district magnitude systems (Mainwaring 1991; Reed 1994). This contrasts with Myerson (1993) and Persson et al. (2003) which argue that electoral systems with lower barriers to entry and offer more alternative options to voters, like PR systems which usually have higher district magnitude than plurality systems, create more competitive pressures on incumbents to refrain from corruption.

In statistical analyses of cross-national data, Persson et al. (2003), Kunicová and Rose-Ackerman (2005), and Tavits (2007) have found that plurality systems with personal ballots have the lowest levels of corruption, whereas open list PR systems have more corruption than plurality systems, and closed list PR systems have the highest levels of corruption. However, the electoral system is not among the factors that Serra (2006) finds to be consistently associated with corruption. Similarly, Treisman (2007) notes that this effect of electoral system on corruption appears fragile. These results were not robust to the inclusion of control variables or the use of data from different years than the original analyses (232). In the next section, we describe several general difficulties with assessing the effect of electoral systems on corruption in cross-national analyses before discussing our approach to ameliorating these

issues.

1.3 Challenges for Empirical Analysis

There are three major issues for empirical analysis. The first is an imprecise definition of treatment effects due to electoral systems being adopted at different points in time. The second is unmeasured confounding due to non-random treatment assignment. The third is the difficulty in measuring corruption. Each can contribute to the inconsistency and fragility of empirical assessments of the causal effects of electoral systems on corruption.

1.3.1 Timing of electoral system adoption and post-treatment bias

Most empirical studies of the effects of electoral systems on corruption, including the main analysis in Persson et al. (2003), are cross-sectional analyses with a recent measure of perceived corruption as the outcome (Y), an indicator of some aspect of the electoral system as the treatment (T), and a set of control variables such as degree of democracy, economic development and freedom of the press (X), that previous studies suggest also affect the outcome. One problem with this setup is that the countries in the analysis adopted their electoral systems at different points in time. Some are well-established democracies that have maintained the same type of electoral system for many years (Group 1), like the United States, while others are countries that have recently made a transition to democracy or adopted significant electoral reforms (Group 2), such as the former Soviet republics. By analyzing these two groups together, these studies estimate an average of the long-run effects for Group 1 countries and short-run effects for Group 2 countries.

Moreover, if control variables X are measured at the same point in time for all countries, they are likely measured before the adoption of the electoral system (treatment) for Group 2, but after adoption for Group 1 (Figure 1.1). Many of these covariates are likely affected by the electoral system T, so that the effect of electoral systems on corruption is likely to suffer from post-treatment bias and to be underestimated for Group 1 countries. Under

favorable circumstances, the effect for this set of countries may be interpreted as a controlled direct effect, while the effect for Group 2 can be interpreted as a total effect (VanderWeele and Vansteelandt 2009). However, the assumptions needed for the estimation of controlled direct effects are different from the assumptions needed for total effects, and it is unlikely that both sets of assumptions will simultaneously hold.

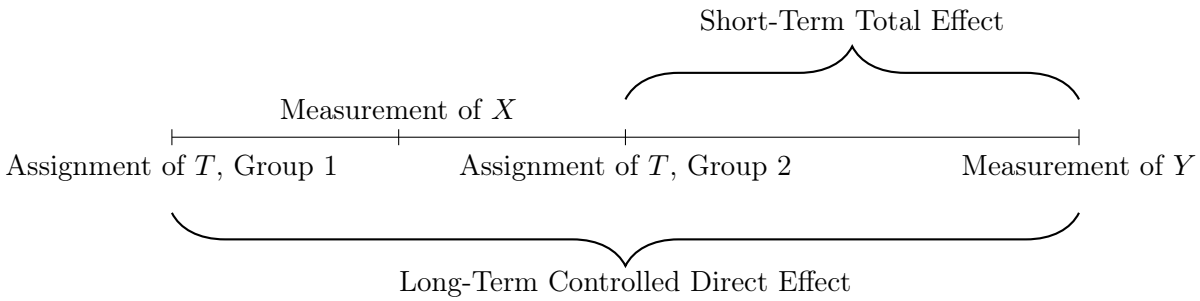

Figure 1.1: Coefficient estimates are an average of long-term controlled direct effects and short-term total effects of electoral systems on corruption.

1.3.2 Unmeasured confounding from non-randomized electoral systems

The second issue is that electoral systems are not randomly assigned, but originate in a political process. For example, colonial heritage and history of communist rule affect what electoral reforms are considered viable options for a country (Blais and Massicotte 1997; Golder and Wantchekon 2004; Luong 2000; Mozaffar 1998). Divided societies might also prefer proportional systems to majoritarian ones (Lijphart 1969), and the number of social cleavages and the size of the country also affect some electoral system choices (Benoit 2007). Analyses of corruption generally include these variables that plausibly affect both the choice of electoral systems and corruption as control variables (Persson et al. 2003, Treisman 2007, Gerring and Thacker 2004).

However, studies of the origins of electoral systems emphasize the politicians' assessments of their future success under alternative electoral systems. The balance of power among these actors and the uncertainty about future power are crucial to understanding the electoral institutions they choose (Andrews and Jackman 2005; Benoit 2004; Luong 2000; Remington

and Smith 1996). Because systematic and reliable information on this uncertainty and politicians' calculations are difficult to obtain, substantial unmeasured confounding may lead to biased estimates.

Persson et al. (2003) addresses this issue with a fixed effects analysis with data from Political Risk Services's International Country Risk Guide (ICRG) as the outcome, the only measure with enough coverage at the time of its writing. But there are sudden, unexplained changes in the ICRG scores for particular countries and inconsistencies in the scoring across countries (Treisman 2007, 221), and it appears to be less reliable than alternative measures. Imai and Kim (2013) has also recently shown that country-year fixed-effects estimators rely on questionable implicit comparisons.

1.3.3 Measurement of corruption

As noted earlier, the enduring challenge to the empirical study of corruption is its definition and measurement. While most scholars agree that corruption is "the misuse of public office for private gain," they disagree on what constitutes misuse and how to measure it (Della Porta and Vannucci 2012; Philp 1997). Those engaged in illicit activities have incentives to hide corruption, compounding the difficulty of measuring corruption.

Most cross-national empirical studies use two indices of corruption: Transparency International's Corruption Perception Index (CPI) and the World Bank's Worldwide Governance Indicator for "control of corruption" (Treisman 2007).[3] Both indices aggregate information from several sources, including expert surveys and business group risk reports. Scholars have registered various concerns with these indicators: they measure perceptions of corruption, not corruption itself; treat expert opinions as independent although experts may rely on the same sources; encourage cross-country comparisons although the relationship between experienced corruption and reported corruption could be heterogenous across countries; and have changed their methodologies thus making inter-temporal comparisons difficult (Apaza 2009; Arndt and Oman 2006). More objective indicators may be available for specific coun-

[3]The latter variable is sometimes referred to as "GRAFT."

tries. For example, Gagliarducci et al. (2011), Golden and Chang (2001), and Golden and Picci (2005) have used legislator absenteeism, formal complaints against legislators, and an index measuring the gap between funds spent on infrastructure and its quality to measure corruption in Italy. But such measures may indicate different things in different contexts and are difficult to obtain, so they are not well-suited for cross-national comparative studies.

1.4 Definitions, Methods, and Data

We take several steps to address these challenges in our analysis. We use the World Bank's Control of Corruption (CCE) index like most cross-national empirical studies of corruption, but we use nonparametric methods and rank-based statistics that are less sensitive to measurement errors in the outcome variable, which we describe in greater detail in the next section. We also define treatment to be applied when a country changes its electoral system, similar in spirit to Persson et al. (2003)'s fixed effects analysis. This allows us to use synthetic control methods (Abadie et al. 2011), which are more transparent about what countries are being compared with each other than a fixed effects analysis. Later we take a differences-in-differences approach to ameliorate confounding and estimate short-run total effects for the treated countries.

First, we simplify our analysis by following Persson et al. (2003) and classify electoral systems as (a) proportional representation (PR), which may be open list or closed list; (b) plurality or majoritarian (plurality, henceforth); or (c) mixed. We define treatment as being applied when there is a change from one of the latter systems to closed list PR, and investigate the effect of this new electoral system on corruption for nominally democratic countries that adopted closed list PR. Data availability directed our analysis to changes to, rather than from, closed list PR and to focus on closed list PR rather than open list PR. We date the treatment to the adoption of these rules rather than when a new legislature is elected and seated under the new rules, because we expect forward-looking politicians to decide whether to engage in or refrain from corruption by considering their consequences for

their prospects for election under the new rules.

We use three variables from the World Bank's Database of Political Institutions compiled as part of the Quality of Governance (QoG) time-series dataset (Teorell et al. 2011, 6 April 2011 version). The first is a dummy variable for *plurality*, which takes the value of 1 if plurality is used to select any member of any chamber of the national legislature or if there is competition for the seats in a one-party state, and 0 otherwise. The second is a dummy variable for *PR*, which takes the value of 1 if proportional representation is used to select any member of any chamber of the national legislature, and 0 otherwise. The third is a dummy variable for *closed list*, which is defined only when the PR variable is 1. We define a country as having a plurality system in a given year when *plurality* is coded 1 and *PR* is coded 0 and as having a PR system in a given year when *plurality* is coded 0 and *PR* is coded 1. When both variables take the value 1, we consider the country to have a mixed system.[4]

Second, we initially examine the difference between plurality and closed list PR, the comparison for which Persson et al. (2003) expects the largest effect, and then supplement the analysis by looking at the difference in corruption between mixed systems and closed list PR. Because of data limitations on the outcome variable described below, we investigate changes in electoral systems between the years 1998 and 2009. A country must have information on both the pre-transition and new electoral systems to be included in the analysis. This effectively excludes countries that transition to democracy and adopt a variety of new political institutions along with a new electoral system. This is in keeping with the cross-national literature that excludes non-democracies and allows us to distinguish the change in electoral system from other changes to political institutions that might affect corruption. In this time period, Kazakhstan, Kyrgyzstan, Mongolia, and Togo changed from plurality to closed list PR systems. We also find changes in Algeria, El Salvador, Macedonia, Niger, Russia, and Ukraine from mixed systems to closed list PR systems.

[4]Teorell and Lindstedt (2010) finds varying results from using different electoral datasets, but no methodological reason to prefer a particular dataset. We use this dataset because it covers the most years.

Third, having defined treatment and identified the treated countries, we must find appropriate control countries because electoral systems were not randomly assigned. We use the `Synth` package v. 1.1-3 (Abadie et al. 2011) in `R` v.2.15.3 to construct a synthetic control unit that is as similar as possible to each of the treated units on pre-treatment values of the outcome variable (corruption) for the three years preceding the electoral system change.[5] Under assumptions that are generally weaker than the assumptions required for linear regression or fixed effects, this approach ameliorates some of the problems due to non-random treatment assignment (Abadie et al. 2010).

The synthetic control is a weighted average of control units drawn from a donor pool of countries in the same region as the treated country that had and maintained the treated country's pre-reform electoral system. This restriction to the same region helps account for factors such as shared colonial heritage, culture, and diffusion of policies that affect both electoral system choice and corruption. We use the alternative region coding ($ht\_region2$) from Teorell and Hadenius (2005), available from the QoG dataset. This coding groups Mongolia with Eastern Europe due to its post-Communist legacy.

The potential donor countries for Kazakhstan, Kyrgyzstan, Mongolia, and Togo are countries in the same region as the treated country that had plurality systems from 1996 through 2011. The potential donor countries for Algeria, El Salvador, Macedonia, Russia, and Ukraine are countries in the same region as the treated country that maintained mixed closed list systems from 1996 to 2011. Niger used open list in its PR tier before the change in its electoral system, but very few African countries used and maintained this system. We prioritize matching on pre-treatment values of the outcome variable and allow the donor pool for Niger to be composed of countries which maintained any mixed system from 1996 to 2011. Niger is consequently matched to Senegal, which had a mixed system with closed list in its PR tier. Only Madagascar and Tunisia among the potential donor countries were unused (received weight of 0) in the synthetic controls. The countries used as synthetic

[5]Kyrgyzstan is an exception. Because it transitioned to a plurality electoral system two years before transitioning to closed list PR, we include only two years preceding its transition in the construction of its synthetic control.

controls and their relative weights are presented in Tables 1.1 and 1.2.

Table 1.1: Synthetic Controls using the World Bank's Control of Corruption Index (CCE) for countries that changed from plurality to closed list PR

Treated Unit	Kazakhstan		Kyrgyzstan		Mongolia		Togo	
Synthetic Control	1	Tajikistan	0.200	Azerbaijan	1	Tajikistan	0.027	Botswana
			0.190	Tajikistan			0.063	Cote d'Ivoire
			0.409	Turkmenistan			0.050	Gabon
			0.201	Uzbekistan			0.038	Gambia
							0.047	Ghana
							0.052	Kenya
							0.042	Malawi
							0.032	Mali
							0.047	Mauritius
							0.048	Uganda
							0.046	Zambia
							0.508	Zimbabwe

Table 1.2: Synthetic Controls using CCE for countries that changed from mixed systems to closed list PR

Treated Unit	Algeria		El Salvador		Macedonia		Niger		Russia		Ukraine	
Synthetic Control	1	Senegal	0.236	Bolivia	0.179	Albania	1	Senegal	1	Albania	0.977	Albania
			0.166	Guatemala	0.167	Armenia					0.012	Armenia
			0.178	Honduras	0.406	Croatia					0.005	Croatia
			0.420	Mexico	0.128	Georgia					0.003	Georgia
					0.121	Lithuania					0.003	Lithuania

As noted above, our primary measure of corruption is the World Bank Worldwide Governance Indicator for Control of Corruption (CCE) (Kaufman et al. 2013).[6] This indicator measures perceptions of the extent of use of public office for private gain, including petty corruption, grand corruption, and state "capture." It aggregates 30 data sources, comprising expert assessments from governmental, commercial and non-governmental organizations and surveys of citizens (Kaufman et al. 2010). The variable CCE ranges from -2.5 to 2.5 with higher values indicating less perceived corruption. It was measured biennially from 1996

[6]The World Bank corrected errors in its 2011 estimates for CCE in February 2013 (Kraay 2013). We use the corrected version. QoG provides this World Bank data series through 2009. We obtained the 2010 and 2011 data directly from the World Bank website.

until 2002 and annually thereafter. For years without data, we take an average of the prior and following years' scores. Because we need the outcome variable measured both before and after treatment, we only examine countries that adopted closed list PR between 1998 and 2009. Tables 1.3 and 1.4 present summaries of balance on pre-treatment values of the outcome variable. In all cases, the average pre-treatment values of the synthetic control are closer to those of the treated units than those of the unweighted regional sample.

Table 1.3: Average Pre-Treatment CCE for Countries with Plurality to Closed List PR Transitions

Treated Unit	Average Pre-Treatment CCE for:		
	Treated Unit	Synthetic Control	Regional Sample
Kazakhstan	-0.89	-0.97	-1.07
Kyrgyzstan	-1.10	-1.10	-1.05
Mongolia	-0.62	-0.97	-1.09
Togo	-0.98	-0.98	-0.54

Table 1.4: Average Pre-Treatment CCE for Countries with Mixed Systems to Closed List PR Transitions

Treated Unit	Average Pre-Treatment CCE for:		
	Treated Unit	Synthetic Control	Regional Sample
Algeria	-0.61	-0.38	-0.13
El Salvador	-0.62	-0.62	-0.69
Macedonia	-0.62	-0.62	-0.41
Niger	-0.88	-0.34	-0.11
Russia	-0.76	-0.74	-0.29
Ukraine	-0.68	-0.68	-0.26

We prefer this CCE measure to Transparency International's Corruption Perceptions Index (CPI), which similarly takes business group and country expert surveys and creates a 0-10 score with higher scores indicating less corruption (Transparency International 2010). While CPI and CCE use similar data sources, CPI differs from CCE in that scores are standardized based on each country's percentile rank within the larger sample of countries. CPI is a relative score by construction and was not designed to be compared across years. CCE also covers more countries for more years than CPI.[7]

[7]Neither measure was available for enough years for Persson et al. (2003) to use in its fixed effects analysis.

In order to assess the effects of the treatment, we take the difference in CCE scores between the treated and control countries, averaged over time. Because the donor pool is relatively small for each treated country, we cannot use the inferential approach presented in Abadie, Diamond, and Hainmueller (2010). Instead we treat each country that adopted closed list PR and its synthetic control as exchangeable – i.e., we treat the synthetic control as if it were a country and assume that treatment was as-if randomly assigned between the treated country and its synthetic control.

This allows us to conduct Fisherian randomization inference (Rosenbaum 2002, Ch. 2) to calculate p-values without assuming a parametric model or assuming that these countries were sampled from some larger population. If the synthetic controls are sufficiently comparable to the treated units, then the exchangeability assumption holds. This allows us to evaluate the sharp null hypothesis of no effect of electoral system for any country with pairwise randomization inference. In this case, sufficiently comparable means that each synthetic control acts as if it were matched to each treated unit in a pairwise randomized experiment. This assumption is strong, but generally weaker than the assumptions required for causal inference with linear regression or fixed effects that are currently employed in cross-national analyses of corruption.

1.5 Analysis

With this synthetic control analysis, we find no support for Persson et al. (2003)'s hypothesis that the move to closed list PR from plurality *increases* corruption. In fact, the analysis suggests that either there is no effect, or even the opposite – that this change leads to *lower* corruption. Additional difference-in-difference and dose-weighted analyses for robustness only strengthen these results.

1.5.1 Plurality Systems to Closed List PR

Figure 1.2 presents our analysis for the four countries that changed from plurality systems to closed list PR systems. The vertical line marks the change in the electoral system for each country. The dark solid line is the country's score on the World Bank's control of corruption (CCE) measure, with higher values indicating less corruption. The dotted line is the CCE score of the synthetic control for each country. The dashed line is CCE for the diff-in-diff adjusted synthetic control. Specifically, if there is a difference between the treated country and the synthetic control in the average pre-treatment outcome values, we adjust the synthetic control by the average pre-treatment difference in order to remove this discrepancy. For Kyrgyzstan and Togo, there is no difference between the dotted line and the dashed line. This indicates good fit for the synthetic control for these countries.

The estimated effect using the `Synth` package for each treated country is the average vertical distance between the solid and the dotted lines after treatment. These average differences are reported in Table 1.5 along with the number of years of data that make up these averages.

Table 1.5: Synth estimates for countries that move from plurality/majoritarian systems to closed list PR

		Synth Estimate	Post-Treatment Years
1	Kazakhstan	0.17	3
2	Kyrgyzstan	0.14	3
3	Mongolia	0.47	2
4	Togo	-0.07	3

If the move to closed list PR from plurality were to *increase* corruption as Persson et al. (2003) expects, then we would expect the estimates in Table 1.5 to be negative, and for the dark solid lines in Figure 1.2 to be below the dashed line after the point of treatment. Only Togo fits this pattern and has the smallest difference with its synthetic control of these four countries. The other three countries indicate positive effects of plurality on CCE, although these effects are also small. Even when the synthetic controls for Kazakhstan and Mongolia are difference-adjusted so as to better match the pre-treatment values of these

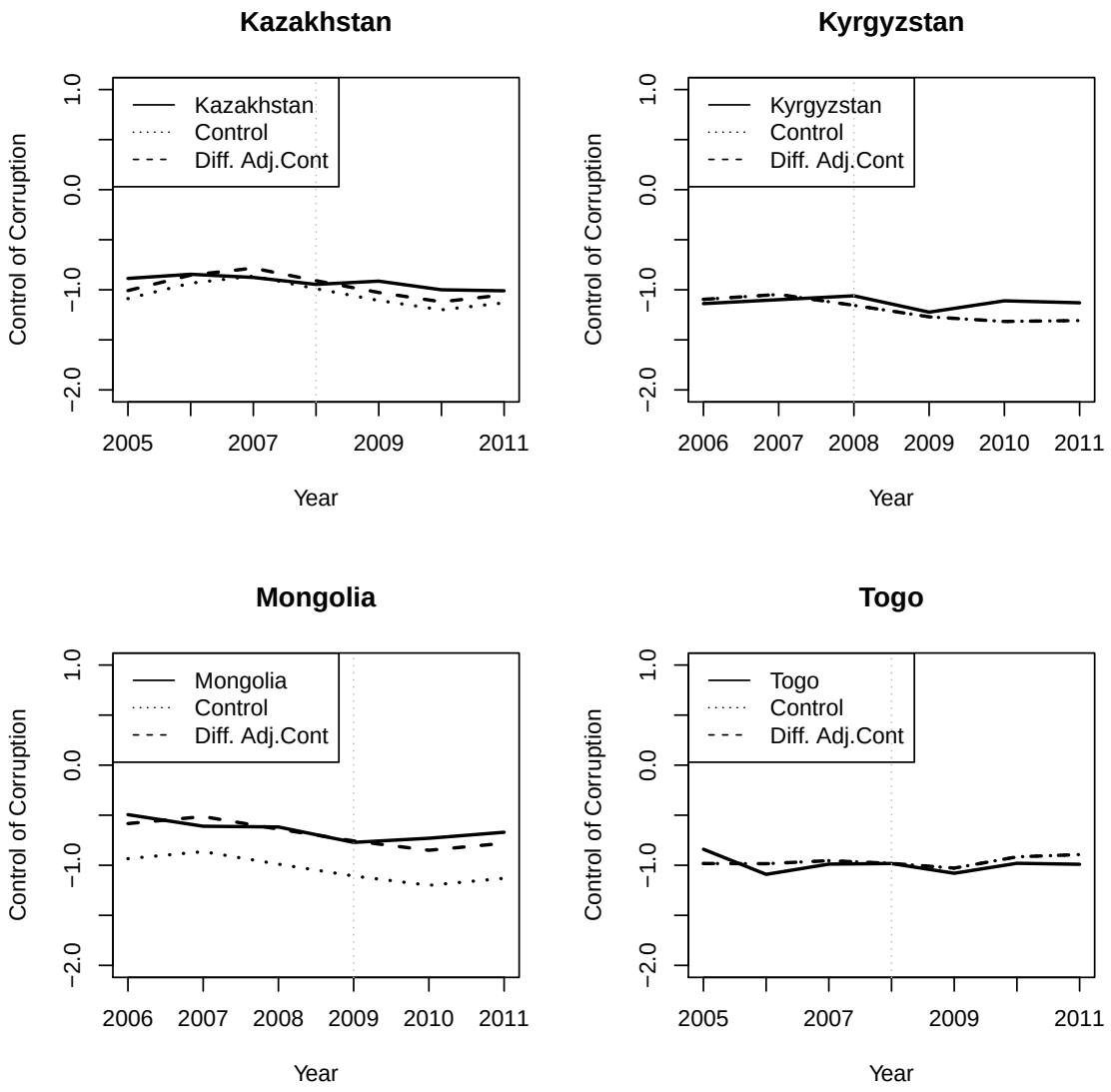

Figure 1.2: Control of Corruption (WB CCE) over time, for each country that moved from plurality/majoritarian systems to closed list PR. The solid line is the treated country, the dotted line is the synthetic control, and the dashed line is the synthetic control adjusted for the average pre-treatment difference in CCE scores.

countries, the estimated effects are still contrary to the Persson et al. (2003) hypothesis (Table 1.6).[8] The results in Figure 1.2 and in Tables 1.5 and 1.6 are fairly strong evidence against the Persson et al. (2003) hypothesis for these countries.[9]

Table 1.6: Diff-in-Diff Synth estimates for countries that move from plurality/majoritarian systems to closed list PR

	Country	Diff-in-Diff Estimate	Post-Treatment Years
1	Kazakhstan	0.09	3
2	Kyrgyzstan	0.14	3
3	Mongolia	0.12	2
4	Togo	-0.07	3

1.5.2 Mixed Systems to Closed List PR

We conduct the same synthetic control analysis for the six countries that changed from mixed electoral systems to closed list PR systems in this period. Mixed systems (or mixed member systems) are those in which both plurality/majoritarian and PR rules are used for election to the national legislature, or where there is more than one chamber, used for elections to the lower chamber (Massicotte and Blais 1999).[10] While Persson et al. (2003) highlights the proportion of seats that are elected by one formula or the other, scholarship on political outcomes in mixed systems has tended to focus on whether and how the seats in the different tiers are linked. It considers whether the different rules apply in geographically distinct areas so a voter votes either in a PR system or in a plurality system, or whether legislators elected in different systems represent the same geographical area (Moser and Scheiner 2012). Because our primary purpose is to compare PR and plurality systems, we

[8]Mongolia moved to closed list PR from a block vote (multiple non-transferable vote) system, which is an extremely disproportional form of plurality system. Under the previous system, fairly small changes in vote share led to large changes in the composition of the legislature (Schafferer 2005), which Persson et al. (2003) note as a condition that should strongly incentivize politicians to refrain from corruption. It may be that the block vote created a collective action problem for incumbent legislators that weakened this incentive, but we do not find any support for the hypothesis that PR increases corruption.

[9]We find similar results using Transparency International's Corruption Perception Index (CPI) instead of CCE (Appendix A).

[10]See Nishikawa and Herron (2004) for a review of other definitions.

treat mixed systems as falling in between these two "pure" systems.

If we restrict the analysis to this set of countries, there appears to be mild but insignificant support for the Persson et al. (2003) argument (Figure 1.3 and Table 1.7). Algeria, Niger, Russia, and Ukraine have lower CCE than their synthetic controls after adopting closed list PR, and Russia has the largest difference in average CCE scores for any pair.[11] The other two countries that moved from a mixed system to closed list PR, El Salvador and Macedonia, had average CCE scores greater than their synthetic controls.

Table 1.7: Synth and Diff-in-Diff Synth estimates for countries that move from mixed systems to closed list PR

	Country	Synth Estimate	Diff-in-Diff Estimate	Post-Treatment Years
1	Algeria	-0.21	0.02	13
2	El Salvador	0.26	0.26	13
3	Macedonia	0.07	0.07	8
4	Niger	-0.12	0.41	4
5	Russian	-0.53	-0.52	5
6	Ukraine	-0.42	-0.42	4

However, there are two complications with the analysis for these countries. First, the synthetic controls for Algeria and Niger do not provide good matches for the pre-treatment outcome values of these countries. Once these synthetic controls are difference-adjusted as in the previous section, the estimated effect reverses sign, as reflected in the solid lines that are mostly above the dashed lines in Figure 1.3. Second, the move from a mixed system to closed list PR is arguably a smaller "dose" of the treatment than the move from a plurality system to closed list PR. Therefore, these six treated countries should likely be given less weight than the four countries considered in Section 1.5.1. These issues tend to provide evidence against the Persson et al. (2003) hypothesis. We formally incorporate these points in the combined analysis of the next section.

[11] Unlike the other treated countries which started out using closed list for its PR seats within a mixed electoral system, Niger moved from a mixed system using open list for its PR seats to "pure" PR using closed lists. Open list systems allow voters to express their preferences over individual politicians, not just parties, so they are more like plurality than closed list systems. Therefore Persson et al. (2003)'s argument may be more likely to hold for Niger than the other countries that start out with mixed systems with closed list in its PR tier. However, once we difference-adjust the synthetic control, we find that CCE is greater for Niger than its synthetic control (Table 1.7).

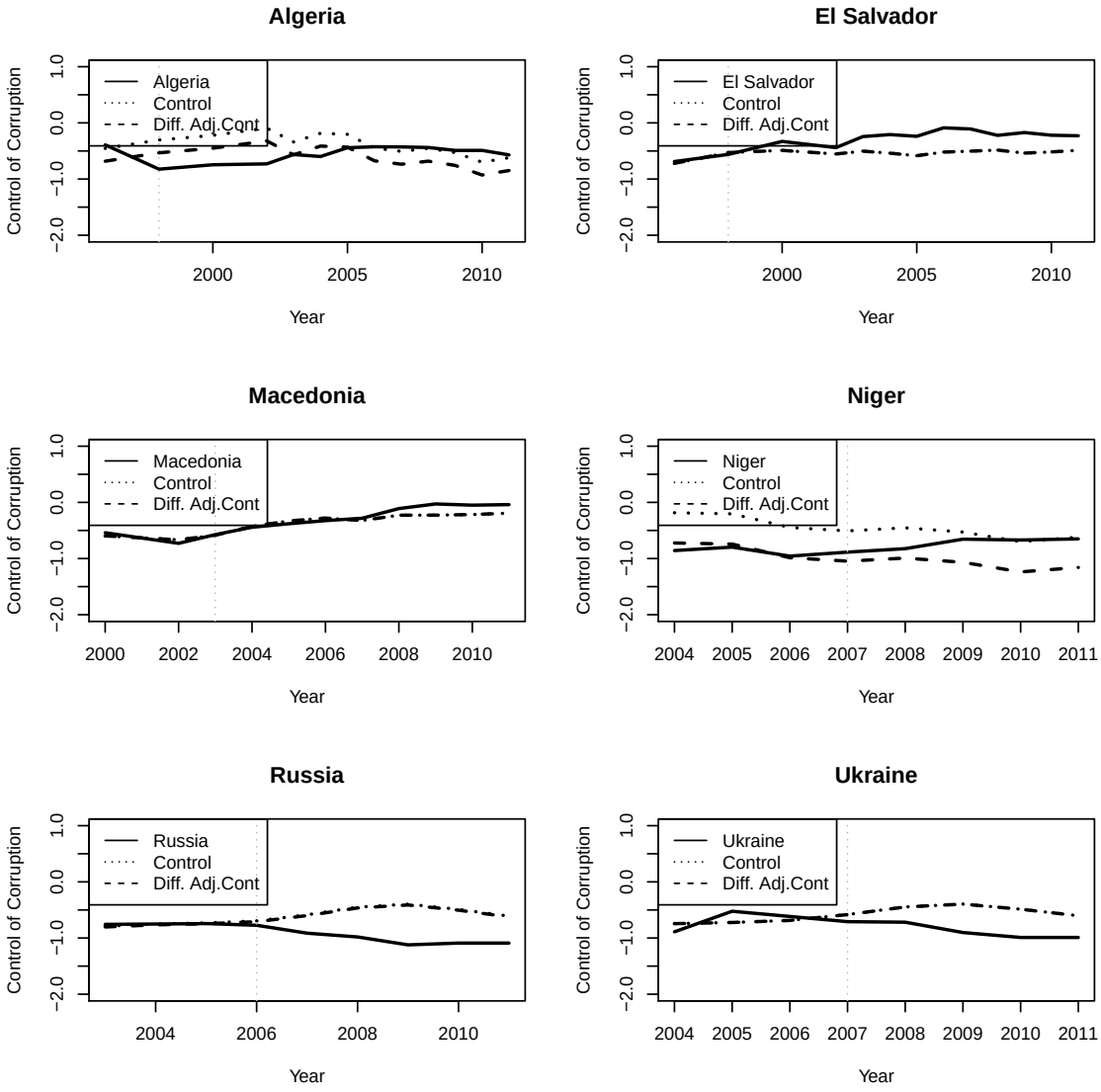

Figure 1.3: Control of Corruption (WB CCE) over time, for each country that moved from mixed systems to closed list PR. The solid line is the treated country, the dotted line is the synthetic control, and the dashed line is the synthetic control adjusted for the average pre-treatment difference in CCE scores.

1.5.3 Combined Analysis

The analyses above suggest that moving to closed list PR systems may actually reduce corruption. To assess the strength of this evidence, we combine the evidence from the plurality and mixed system countries and present nonparametric tests based on weighted and unweighted signed rank statistics. Although we prefer this approach to standard fixed effects models, we note that all of the substantive points here are robust to the use of standard fixed effects models.

We start with the analysis that is most sympathetic to the Persson et al. (2003) hypothesis. The combined mean of the Synth estimates in Tables 1.5 and 1.7 is -0.024 and the median is 0.0, so the overall evidence does not support the Persson et al. (2003) hypothesis. We use the exchangeability assumption discussed earlier to conduct a nonparametric signed rank test, which allows us to assess the strength of this evidence with this small sample size without resorting to parametric assumptions. The Wilcoxon signed rank statistic assigns the ranks 1 (smallest) through 10 (largest) to each of the 10 pairs according to the size of absolute difference in post-treatment CCE scores (averaged over time) between the paired treated country and synthetic control countries. The statistic then sums the ranks only for the pairs where the post-treatment CCE scores are larger for the treated country than for the synthetic control (averaged over time).

This process is presented in Table 1.8, and the signed rank statistic calculated from our data is 27. If all of the ranks had been positive, it would have been 55. This statistic is difficult to interpret directly, but we can generate a p-value under the sharp null hypothesis of "no effect," by provisionally assuming that electoral system is irrelevant for corruption and permuting treatment status between the treated countries and their synthetic controls within pairs. Because we have 10 pairs, there are $2^9 = 1024$ different within-pair permutations of treatment status, of which our observed data represents just one possibility. For each permutation, we re-calculate the signed rank statistic, giving us a permutation distribution of 1024 statistics. Figure 1.4 presents this null distribution based on the analyses in Tables

1.5 and 1.7. The p-value is then determined by calculating the proportion of these 1024 statistics that are more extreme than our observed statistic of 27. This procedure is a nonparametric version of a paired t test and is explained in detail in Glynn and Ichino (2014). As noted above, the evidence is not supportive of the Persson et al. (2003) hypothesis. The two-sided p-value is effectively 1 because as we can see from Figure 1.4 the observed statistic is in the middle of the permutation distribution.

Table 1.8: Signed rank statistic for the combined groups. The absolute value of Macedonia's estimate is slightly larger than that of Togo, so we include an additional decimal place in the analysis to avoid ties in the rank.

	Synth Est	Sign	Absolute Rank	Signed Rank
Kazakhstan	0.17	+	5	5
Kyrgyztan	0.14	+	4	4
Mongolia	0.47	+	9	9
Togo	-0.07	−	1	0
Algeria	-0.21	−	6	0
El Salvador	0.26	+	7	7
Macedonia	0.07	+	2	2
Niger	-0.12	−	3	0
Russia	-0.53	−	10	0
Ukraine	-0.42	−	8	0
			Sum	27

The mean of the difference-adjusted synth estimates is 0.01 and the median is 0.08 – provisional evidence that closed list PR systems may *reduce* corruption. A nonparametric signed rank test now produces a two-sided p-value of 0.56. The evidence against the Persson et al. (2003) hypothesis is strengthened further if we consider that theory suggests larger effects for transitions from plurality to closed list PR systems than from mixed to closed list PR systems. The outcomes from transitions from plurality systems might therefore be weighted more heavily than the outcomes from transitions from mixed systems. If we knew the appropriate doses (weights) to assign to these two types of transitions, then we could use the dose-weighted signed rank test described in Rosenbaum (2002, 2009). For example, if the move from plurality to closed list PR represented a dose of 2, and the move from a mixed system to closed list PR represented a dose of 1, then we would alter the signed rank

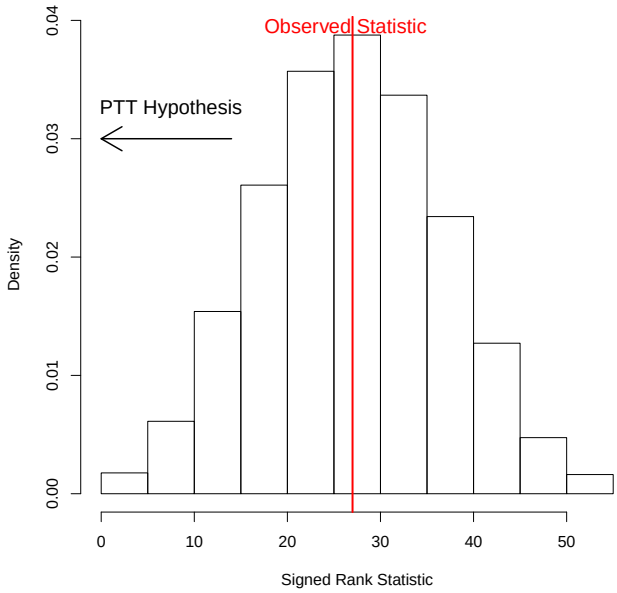

Figure 1.4: Permutation distribution for the signed rank statistic based on unweighted Synth estimates

statistic by multiplying the ranks of the plurality to closed list PR estimates by 2 and the ranks of the mixed to closed list PR estimates by one. The procedure for generating the p-value is otherwise the same as the permutation procedure described above.

Unfortunately, we do not know the doses to assign to the two types of transitions. However, for any arrangement such that the plurality to closed list PR transitions are assigned a dose at least as large as the dose assigned to the mixed to closed list PR transitions, the p-value will be at least as small as those reported above. With doses of 2 and 1 as described above, the p-value would be approximately 0.70 for the synth estimates and 0.375 for the difference-adjusted estimates. With doses of 3 and 1 as described above, the p-value would be approximately 0.625 for the synth estimates and 0.275 for the difference-adjusted estimates. Therefore, depending on how we assess the relative strengths of these two types of transitions, the evidence against the Persson et al. (2003) hypothesis can be reasonably strong.

1.5.4 Discussion

There are a number of important caveats to these results. First, these results are limited in scope to the countries for which the we have changes in electoral systems. Methodologically, it makes sense to limit the analysis to these ten countries where the evidence will be most straightforward to assess. Therefore, the Persson et al. (2003) hypothesis may hold for countries out of this sample, although in the appendix we also fail to replicate this hypothesis using the full sample of countries in a fixed effects analysis.

Second, it may not be surprising that there has been little change in the CCE scores of these countries. None are very democratic, so that citizens may not be able to vote out legislators affiliated with a strong ruling party, and the locus of corruption may be the leaders of these parties rather than individual politicians. Togo did not hold an election under the new rules until 2013 because of a longstanding stalemate between the opposition and government. The situation in Kazakhstan is somewhat better in that although the ruling party held all legislative seats following the 2007 elections under the previous electoral system, some opposition parties were able to win seats in the highly disputed elections of 2012.[12] Similarly, Kyrgyzstan adopted various constitutional reforms in 2007 following the Tulip Revolution, but the ruling party won an overwhelming majority of seats in the legislature. Changes in the electoral systems may have greater effects in more democratic countries, but again the fixed effects analysis does not provide evidence for this hypothesis.

Third, not enough time may have passed since the electoral system change for the effects to accrue in these ten countries, and this may be especially true for the plurality countries for which we only have a few post-treatment years. Our analysis may be replicated once more data becomes available on these countries. Fourth, we have adopted a nonparametric approach in order to reduce our sensitivity to measurement problems with CCE, and we find similar results with Transparency International's CPI. However, these measures rely on surveys of experts for their perceptions of corruption, and more objective measures of

[12] However, note that the data ends in 2011.

corruption may reverse our findings.

Finally, synthetic control and difference-in-differences approaches may not have eliminated all bias due to unmeasured confounding, and the consequences are more problematic with worse measurement. We can conduct a Rosenbaum (2002) style sensitivity analysis on these results which would raise the reported p-values. However, such a sensitivity analysis would only weaken our confidence that PR might reduce corruption for these countries. It would not strengthen the evidence for the Persson et al. (2003) hypothesis.

1.6 Conclusion

Following the pioneering work of Persson, Tabellini, and Trebbi (2003), scholars have proposed several theories linking electoral systems to levels of corruption. But how much empirical support these arguments have found has been debated, due to the difficulty of measuring corruption, non-random assignment of electoral systems, and the combination of different treatment effects.

This paper addressed each of these empirical challenges and found no support for the hypothesis that PR systems cause corruption. Rather, transitions to PR in our sample are weakly associated with *lower* levels of corruption. First, we addressed the issue of timing by focusing on individual country transitions from one system to another. Second, we more clearly defined the treatment effect by comparing treated countries only to countries with the same electoral system as the treated country had before its reform. Third, we address non-random treatment assignment by using synthetic control and differences-in-differences approaches. Finally, we partially addressed concerns about measurement of the outcome by using the nonparametric signed rank statistic.

The immediate implication is a call for a re-examination of the dominant view that plurality systems cause lower levels of corruption and that PR cause higher levels, the theory implicit in the South African petition for electoral reform. At a minimum, our analysis suggests that this is unlikely to be the case in new democracies and poorer countries, and

we find some evidence that PR may even cause an increase in corruption. More generally, we should reconsider the conditions under which citizens hold elected leaders accountable for corruption, as this could yield stronger theory for the effect of electoral systems on corruption.

2 | The System Matters: Corruption and Vote Choice in Uganda

2.1 Introduction

In 2006, Major General Jim Muhwezi lost his position as minister of health for Uganda over allegations of embezzlement and misuse of resources donated by the Global Fund for Aids, Malaria and Tuberculosis; in 2007 he was charged and served a brief stint in prison before being released on bail. Yet, in 2011 Muhwezi handily won reelection with more than 57% of votes cast in his constituency, and the next year all charges against him were dropped. Muhwezi's case is just one in which corruption has taken center stage in Ugandan politics in recent years. Allegations of corruption led a number of donors to suspend bilateral aid in 2012 (*Reuters* 2012), leading civil society members to organize a series of protests and actions against corruption titled 'Black Mondays.'

One theoretical benefit of electoral competition is that it allows voters to 'throw the rascals out.' Yet despite electoral competition, corruption in Uganda is common and persistent. Why do voters fail to sanction politicians for bad performance? To answer this question, I make a theoretical distinction between voters' perceptions of the corruption of the political system and of individual politicians. I argue that a sizable portion of Ugandan citizens perceive their political system to be highly corrupt. In particular, they perceive corrupt acts to be frequent and widespread, do not expect perpetrators of corrupt acts to be punished, and have difficulty distinguishing 'honest' candidates from those whose corruption has not yet been revealed. Contrary to traditional models of democratic accountability, these characteristics cause voters who perceive the system to be highly corrupt to be less

likely to punish clearly corrupt individuals by withdrawing electoral support. In some cases, they even prefer corrupt candidates.

In the next section I present evidence that corruption in Uganda is widespread, and that it has persisted despite significant changes in formal political institutions including multiparty competition in Uganda's last two elections. I then show that existing explanations for the persistence of corruption despite electoral competition – including coercion, barriers to entry, and lack of information – are not sufficient to explain the Ugandan case. The subsequent section presents the theoretical argument that voters' perceptions that their political system is highly corrupt – and in particular, that corruption is frequent and widespread, that corruption is unlikely to be punished and the difficulty of distinguishing 'honest' candidates – combine to make them less likely to withdraw support from overtly corrupt candidates. I then illustrate this argument using data from Uganda – including original data from focus groups and interviews, as well as public opinion and media data. In the subsequent section I present preliminary evidence from Afrobarometer surrey data that the influence of these perceptions extend beyond vote choice, lowering citizens' political efficacy, making them less likely to turn out to vote, and possibly even dampening their aspirations for democracy. I then argue that neither clientelism nor vote buying explain these patterns. The final section concludes.

2.2 The Puzzle of Persistent Corruption in Uganda

2.2.1 Corruption in Uganda

By almost any metric, corruption – the misuse of public office for private gain – is widespread in Uganda. Moreover, high levels of corruption have persisted despite the introduction of multiparty competition in the country. The two most commonly used indicators for international corruption comparisons are produced by Transparency International (TI) and the World Bank. In 2013, Uganda ranked 140 of 175 countries included in TI's Corruption Perception Index. Even within SubSaharan Africa, a region which fares poorly on TI's

index, Uganda ranked 33 of the 48 countries included. Furthermore, in 2013, Uganda was in the bottom quintile of countries in the World Bank's measure of 'control of corruption.' Of course, these indicators are hardly comprehensive, and scholars have argued they are subject to methodological (Apaza 2009; Arndt and Oman 2006; Langbein and Knack 2008) and ideological (Razafindrakoto and Roubaud 2010) biases.

Yet moving beyond these international comparisons, public opinion data indicate that a sizable portion of Ugandan citizens also perceive their country to be corrupt. In one recent poll, 61% of Ugandans stated that they had paid a bribe in the preceding 12 months; the global average was 27% (Transparency International 2013). Furthermore, more than half of Ugandan respondents described their Parliament as 'extremely corrupt' (Transparency International 2013). Data from the Afrobarometer surveys, a comparative series of public opinion surveys based on a national probability sample, allows insight into Ugandan attitudes toward corruption over time. In the most recent Afrobarometer, less than 20% of respondents stated that the government was doing 'fairly' or 'very well' at 'fighting corruption in government' (Afrobarometer 2012); in 2005, just before the first multiparty elections, nearly 30% of respondents answered similarly (Afrobarometer 2005). Qualitative data from focus group discussions also reveals a variety of ways that corruption affects Ugandans' daily lives. For example, participants frequently expressed frustration with shortages of medicines at government-run health clinics which are diverted and sold privately.[1] Another common refrain was discontentment with the misallocation and diverting of funds from the National Agricultural Advisory Service (NAADS). Finally, participants expressed frustration that police and village leaders demand payments for nominally free services or inflate required fees.

In addition to their own experiences with corruption, citizens hear about elite corruption scandals through the media. Documented instances of corruption include maintenance of 'ghost soldiers' to funnel funds to military officers and politicians, favorable deals during

[1]More than half of respondents who had been to a local public clinic or hospital in the 12 months before the most recent Afrobarometer reported encountering a demand for an illegal payment (Afrobarometer 2012).

the privatization of state-run enterprises, and embezzlement of funds from foreign aid and domestic social programs (Tangri and Mwenda 2008).[2] These scandals – and the fact that high ranking politicians are rarely convicted or punished – are widely reported in newspapers and discussed on radio programs. Discussions in focus groups showed that many participants were familiar with recent scandals, with some having closely followed media coverage.

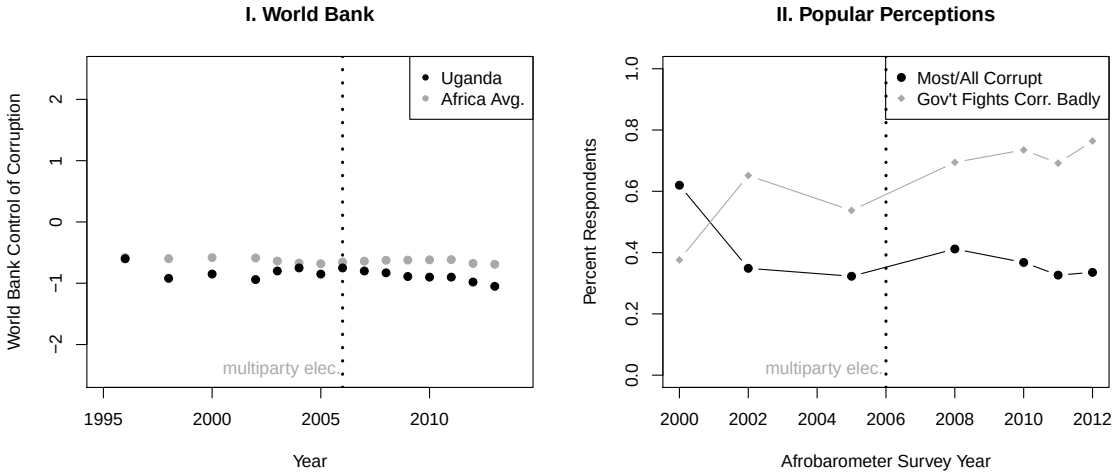

Figure 2.1: *I. Stability of 'Control of Corruption' over Time:* Black circles indicate Uganda's score on the World Bank's Control of Corruption measure, which has a range of -2.5 to 2.5 with lower values indicating more corruption, while the gray circles show the average score for SubSaharan Africa as a whole. *II. Ugandan Perceptions of Government Corruption:* Black circles indicate the percentage of Afrobarometer respondents who answered that 'most' or 'all' elected leaders, government officials, and the president and his office are corrupt.[2] Gray circles indicate the percentage of respondents who answered that the government was doing 'very' or 'fairly' badly at fighting corruption in government.

Although comparing corruption levels over time is challenging[3], the available data indicate no decrease of corruption with multiparty elections in Uganda. For example, the black circles in Panel I of Figure 2.1 show that Uganda's performance on the World Bank's Control of Corruption measure has been largely stable since multiparty competition was

[2]For a timeline of selected corruption scandals which received heavy media coverage, see Appendix B.

[3]The source data used in the World Bank's Control of Corruption measure changes over time, but more generally, perceptions may reflect factors beyond incidence of corruption such as societal norms or availability of information.

introduced. For reference, the gray circles indicate the average score of other SubSaharan African countries.

Panel II of Figure 2.1 shows two measures of citizens' perceptions of corruption over time. The black circles show the percentage of respondents who answered that 'most' or 'all' elected leaders and government officials[4] are involved in corruption. While this proportion dropped notably between the first and second Afrobarometer survey in the early 2000s, it has remained relatively stable since then, with between 32 and 42% of respondents falling in these categories. On the other hand, the proportion of respondents who perceive the government to be doing 'fairly' or 'very badly' in its fight against corruption has risen slightly.

This relative stability of perceptions of corruption stands in contrast with important changes to Uganda's formal institutional landscape. President Yoweri Museveni has been in office since 1986. He successfully seized power after a five-year guerrilla struggle against president Milton Obote and then Gen. Tito Okello, who was ousted a mere six months after overthrowing Obote's government. Museveni initially embraced 'no party' (i.e., single-party) politics, altering the constitution so that every citizen was an automatic member of his National Resistance Movement (NRM). In effect, during these years there was no differentiation between the state of Uganda and Movement structures. However, despite Movement politics being upheld by referendum in 2000, Museveni reintroduced multiparty competition for the 2006 elections (see Makara, Rakner, and Svåsand (2009) and Keating (2011) for discussions of possible motivations for instituting multiparty competition). Museveni and the NRM won elections in 2006 and again in 2011. Although many commentators attributed this victory to the large influx of cash in the election (Izama and Wilkerson 2011), academic studies have not isolated an effect of money on vote choice (Conroy-Krutz and Logan 2011).

In addition to the presidency (elected by popular vote every five years) and the parlia-

[4]Because different surveys asked about different office holders, this figure represents an average of responses regarding: elected leaders (all surveys; Rounds 3-5 distinguish between national and local offices), the president and his office (Rounds 2-5), civil servants (Round 1), and government officials (Rounds 2-5; Round 3 distinguishes between national and local government officials while Rounds 4 and 5 ask only about national officials).

ment (elected by plurality from single member districts), Uganda has five administrative levels called Local Councils (LCs) which are staffed by both elected and appointed officials. Although the NRM has maintained its dominance since multiparty competition was reintroduced, there is significant regional variation in its levels of support, with Museveni's region of origin (the west) lending stronger support to the NRM and the opposition parties gaining more support from the northern and eastern regions of the country.[5]

Politicians from all levels have promised to combat corruption. In the 'Ten Point Programme' which stated its principles of governance upon gaining power, the NRM linked corruption to the overall problem of economic development and stating that it 'must be eliminated once and for all.' The NRM continues to endorse anti-corruption efforts, most recently with the Anti Corruption Bill (2013), which would seize the property of those convicted of corruption. Opposition leaders have critiqued these actions as mere lip service. In fact, criticism of government corruption has been a prominent plank of party platforms in both multiparty elections (Forum for Democratic Change 2006, 2011).

Anti-corruption rhetoric notwithstanding, illicit resources form an important part of electoral campaigns. A report by the Inspectorate of Government notes that 'In practice, the finances of individual candidates' campaigns are never audited, issues of existing limits are routinely bypassed or willfully ignored, and most of the individual contributions to a particular political candidate are made outside of the formal limitation system; there is generally no enforcement of sanctions for violations' (The Inspectorate of Government, The Republic of Uganda 2012, 33). Opposition media outlets claimed that NRM candidates received packets of 20 million shillings (about $8,800 at that time) of government funds to campaign before the 2011 election (estimate cited in Barkan (2011, 11)). About a third of the government budget for the entire year was spent in January preceding the February 2011 presidential and parliamentary elections (Izama and Wilkerson 2011). A supplementary budget had to be passed to allow continuation of government functions. While academic

[5]Opposition parties include the Forum for Democratic Change (FDC) led by Kizza Besigye and the Democratic Party (DP) and Uganda People's Congress (UPC), which have long histories but less recent electoral success.

studies have not identified the effect of money on vote choice, in interviews journalists, politicians and NGO employees frequently stated their beliefs in the importance of resources used to finance campaign rallies, party agents, and small amounts of cash or gifts to voters during elections.

There has been some degree of popular mobilization around dissatisfaction with corruption. The 'Walk to Work' protests in Kampala in 2011 and 2012, organized by opposition politicians and some civil society members, protested both inflation and corruption. Following the 2012 suspension of bilateral aid mentioned above, a number of civil society organizations have advocated for increased transparency; recently they instituted an ongoing protest movement called 'Black Mondays' which seeks to draw attention to the costs of corruption (for instance, see Action Aid (2012)). While protests have sometimes drawn large crowds and activists have been arrested, there is little evidence that the control of corruption is an electorally salient issue. Although Museveni's support was lower in the 2006 elections than in 2001, he received a strong majority of the vote in the 2011 elections. Candidates from his party, the NRM, won 165 of the 237 directly elected seats and 85 of the 112 seats reserved for women (Inter-Parliamentary Union n.d.). Although reelection for politicians is far from guaranteed – 120 incumbent MPs lost their bids for reelection in 2011 (Juma and Saidi 2011) – there is no evidence that corrupt incumbents are being replaced by more honest candidates.[6] As one civil society leader from Katakwi put it: 'They [communities] should design approaches for demanding accountability between elections. [...] We are replacing people with those who have the same intentions as the ones they are replacing. This is not effective.' This raises this chapter's central question: why have voters not used the return to multiparty competition to elect less corrupt candidates?

[6]This is a difficult claim to prove. At a macro-level, there is no evidence that levels of corruption are decreasing. This does not preclude the possibility that certain honest individuals are elected but just not effective. However, the evidence presented below shows that this is not commonly perceived to be the case.

2.2.2 Extant Explanations for Failures in Accountability

Faced with evidence that voters around the world support corrupt politicians (Kurer 2001; Manzetti and Wilson 2007), scholars have devoted considerable attention to explaining why individuals would act in such an apparently deleterious manner. In this section, I review prominent explanations and argue that coercion, barriers to entry, and access to information cannot fully explain how Ugandan voters evaluate individual politicians' levels of corruption.

Much literature in political science assumes that voters will punish corrupt politicians. Corruption includes many actions that diminish voter welfare. For example, a politician who demands bribes in exchange for government contracts may choose a lower quality contractor, embezzlement of state funds leaves fewer resources for public consumption, and hiring bureaucrats based on personal connections may lead to an incompetent civil service. Based on these undesirable outcomes, many models of political accountability assume that voters will punish corrupt politicians (Adserà, Boix, and Payne 2003; Deegan-Krause, Klašnja, and Tucker 2011; Ferraz and Finan 2008). The assumption that voters punish corruption forms the basis of models that predict lower levels of corruption in democracies generally, and particularly when voters can hold a single person accountable (plurality and open-list proportional representation systems) (Persson and Tabellini 2000; Persson, Tabellini, and Trebbi 2003; Kunicová and Rose-Ackerman 2005), as they can in Uganda.

The simplest explanation in the case of Uganda is that because of the electoral dominance of the NRM and Museveni, vote choice is not meaningful. It is true that Uganda is not a liberal democracy by any means. Restrictions on freedom of assembly and the press, and allegations of ballot box stuffing have marred Uganda's multiparty national elections (Election Observation Mission 2011).

Although Ugandan elections have not featured 'a level playing field,' Ugandan voters still face a meaningful choice in casting ballots. First, data from Afrobarometer public opinion polls consistently show that the vast majority of Ugandans believe themselves to be 'completely' or 'somewhat free' to choose who to vote for 'without feeling pressured.'

Furthermore, the proportion of the population who believe this to be true has remained relatively constant over time, showing a slight increase around the 2011 elections, rather than a decrease as one would expect if coercion were prevalent (see Figure 2.2). Second, the majority of respondents believe that it is 'not at all' or 'not very likely' that powerful people could find out their vote choice; thus most Ugandans believe their votes remain private, even if elites desire to influence them (see Figure 2.2). The two Afrobarometer surveys fielded around the 2011 election (Rounds 4.5.1 and 4.5.2) also asked how often 'anyone threatened negative consequences' to either the respondents or people in their village to influence vote choice during campaigns for the 2006 or 2011 elections. Between 85 and 90% of respondents said this had 'never' happened to themselves or those in their village during both campaigns. Thus while Ugandan elections are far from perfect, coercion alone does not appear to sufficiently explain why voters have not selected less corrupt politicians.

Another possibility is that barriers to entry prevent less corrupt candidates from running for office. Most crudely, the ruling party could use its control over the Electoral Commission to prevent such candidates from campaigning. There is no evidence that the government prevented non-party members from running, however. In the 2011 elections, 1,269 candidates contested the 238 seats from directly-elected parliamentary constituencies and 444 candidates contested the 112 women's seats elected at the District level (The Electoral Commission of Uganda 2011). Of these, only 237 candidates were nominated by the NRM in the directly elected parliamentary constituencies, and only four NRM members ran unopposed (The Electoral Commission of Uganda 2011). Of course, the ruling party or the political parties more generally could still ensure that only candidates who are willing to 'play ball' run by controlling party primaries. This is not the case in Uganda, however, which has very modest hurdles for a candidate to appear on the ballot. A parliamentary candidate may run with or without party sponsorship as long as she is a citizen, is a registered voter, has attained a certain level of education, and can produce at least two registered voters from her constituency to support her nomination (The Electoral Commission of Uganda 2010). In fact, many candidates choose to run as independents or switch parties if they lose party

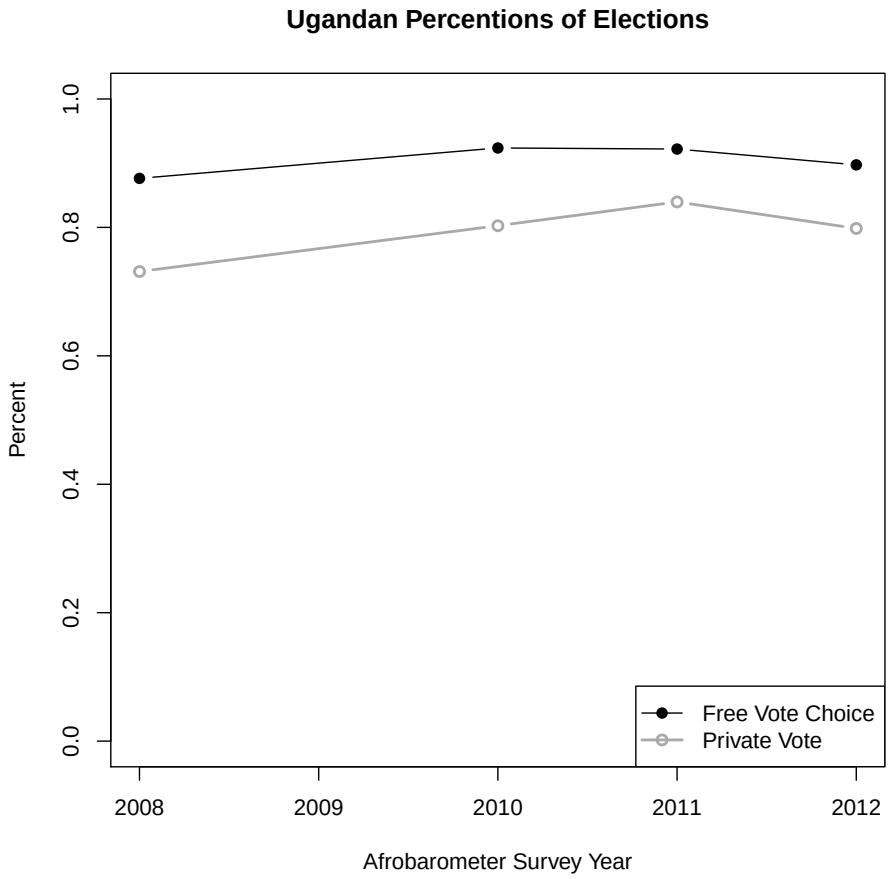

Figure 2.2: *Ugandan Perceptions of Elections:* The filled circles shows the percentage of respondents who answered that they were 'somewhat' or 'completely free' to 'choose who to vote for without feeling pressured.' The unfilled circles show the percentage of respondents who answered that it was 'not at all' or 'not very likely' that 'powerful people could find out how you voted, even though there is supposed to be a secret ballot in this country.' This figure excludes those who refused to answer, said they did not know, or for whom data is missing.

primaries; more than 500 candidates contested for the directly elected seats as independents and many others ran as candidates from very small parties (The Electoral Commission of Uganda 2011). The NRM does not exercise strict control over its own candidates; five NRM ministers decided to run as independents in the 2011 election after losing party primaries (Makara 2010). Furthermore, running as an independent is a viable strategy – 30 independents won seats in the parliamentary constituencies. This is a higher number than were elected from any of the opposition parties (The Electoral Commission of Uganda 2011).

No comprehensive measure of individual candidates' corruption exists, making it difficult to prove the existence of "honest" candidates. However, citizens can identify 'overtly corrupt' politicians (for example, those who are the subjects of corruption scandals). The remainder of the chapter argues that citizens' perceptions that the system is highly corrupt make them unlikely to punish these overtly corrupt politicians. Thus citizens' perceptions of the system would sustain the electoral status quo, even absent barriers to entry.

Given free vote choice and the presence of candidates of varying degrees of corruption, voters might still fail to hold politicians accountable if they lack information about their corruption. For this reason, in many formal models of electoral accountability politicians' rents increase in information asymmetries between politicians and voters (Persson and Tabellini 2000); if voters are perfectly informed, then politicians should cease to extract resources (Adserà, Boix, and Payne 2003). Access to accurate information seems particularly important with regards to corruption, since of course politicians have every incentive to hide corrupt behavior. As Winters and Weitz-Shapiro (2013) notes, many cross-national studies implicitly assume that citizens with accurate information punish corruption (Brunetti and Weder 2003; Treisman 2007)

To more fully establish causality, scholars have begun experimentally and quasi-experimentally testing the effects of information on vote choice (See Pande (2011) for a thorough review), with mixed results. While Ferraz and Finan (2008) find that on average release of audit reports of Brazilian mayors before elections decrease their chance of reelection, they find that audits showing fewer than the median number of corruption violations actually increase may-

ors' chances of reelection. This implies that absent information, Brazilian voters assumed their mayors to be of median corruption and vote accordingly. De La O et al. (Forthcoming) find that randomly distributed information about corruption in Mexico decreases votes for both the incumbent and the challenger as people exposed to that information become less likely to vote at all. Humphreys and Weinstein (2012) find that after random distribution of scorecards detailing MP performance[7] to citizens in Uganda, more poorly performing MPs are *not* more likely to be voted out of office. These seemingly disparate findings can be reconciled by this chapter's argument because because information about an individual candidate's corruption should have different effects on vote choice depending on citizens' perceptions of the political system.

2.3 The System Matters

Previous theories of vote choice focus on individual voters assessing individual politicians; they fail to consider that particular attributes of a politician may be desirable in one type of political system, but neutral or undesirable in another. Specifically, I argue that a large portion of Ugandan citizens perceive the political system to be highly corrupt, which diminishes the value they place on individual candidates' ethics.

By 'political system,' I mean the formal and informal institutions that shape how politics works in a given country. In the next section, I will show that 'grand' corruption (e.g. corruption at the level of political decision-makers) is critically important in maintaining the current Ugandan political system.[8] For my argument to hold, citizens must perceive the political system to rely on what the literature defines as 'grand' corruption (e.g. corruption by political decision-makers) as opposed to 'petty' corruption (e.g. corruption in the implementation or administration of policy). This is because citizens understand that they elect

[7]Note that performance as defined here did not include indicators of corruption

[8]This feature is hardly unique to Ugandan politics. In many African countries, elite-level corruption is pervasive enough as to constitute a 'veritable political institution[]' (Bratton 2007, 98). Nor is this pattern unique to Africa. As an observer of Mexican politics pithily stated, 'Corruption is not a characteristic of the system in Mexico, it is the system' (quoted in Morris (1999, 623)).

politicians to operate in the political rather than bureaucratic realm. Furthermore, people in powerful roles are likely to be seen as symbols of the political system itself (Gusfield and Michalowicz 1984). Thus citizens' perceptions of the system in which their elected leaders will operate are crucial for informing their evaluations of those individual leaders.

Three components of perceptions of the corruption of the political system affect how citizens evaluate corruption of candidates. First and most obviously, citizens who perceive their political system to be highly corrupt expect corrupt actions to be frequent and widespread. Therefore, if voters believe that they are in a highly corrupt political system, electing one 'honest' candidate is unlikely to make a significant difference in the amount of state resources that are inappropriately funneled off. In fact, voters may prefer a corrupt politician because she is likely to be more skilled at delivering benefits to the community within this corrupt overarching system. For example, one study measured graft in the early 1990s in Uganda by comparing central government disbursements to the amounts received by schools; it found that schools received only 13% of funds disbursed, *most* schools received nothing, and that local politicians likely captured the difference (Reinikka and Svensson 2004). In such an environment, any resources from politicians delivered to the community – even if they do not benefit a voter personally – would be preferable to the alternative of receiving nothing. Furthermore, those who are most skilled at capturing also might be those with the most resources to disburse. Together, these limit the incentives and raise the disincentives to electing an 'ethical' politician.

Second, when citizens perceive the political system to be highly corrupt, they do not expect corrupt behavior to be punished consistently or appropriately. This matters because the threat of punishment is one potential deterrent to electing a corrupt politician. If that person might be punished, this could deprive the community of a representative or otherwise negatively affect standing within the broader political landscape. Thus punishment being unlikely not only reduces disincentives for politicians to be corrupt, it also reduces the disincentive to citizens for voting for corrupt representatives.

Third, within a corrupt political system, it is very difficult for politicians to signal to

voters that they are honest. Establishing one's ethics is a fundamentally difficult task in any political system, but in highly corrupt systems clean politicians must somehow counter voters' default assumptions that they are corrupt. How is a voter to distinguish if a candidate is indeed a transparency-minded reformer?

Thus voters who perceive their system to be highly corrupt are less likely to prefer clean politicians, because such politicians may be less effective at channeling resources, it is unlikely that a corrupt politician will be punished, and it is difficult for voters to even distinguish honest politicians from those who have not yet been shown to be corrupt. Of course, no political system is perfectly clean or perfectly corrupt, and individuals within a given system will necessarily perceive the same system differently depending on their individual characteristics. However, the fraction of citizens who perceive their political system to be highly corrupt can reasonably be expected to correlate with actual elite corruption. In turn, this affects how citizens vote, reinforcing equilibria in either ideal type. The next section illustrates these dynamics with evidence from Uganda.

2.4 Perceptions of the Political System and Vote Choice: Evidence from Uganda

This section argues that in addition to the high levels of corruption in general established earlier in the chapter, grand corruption in particular has been a crucial tool to maintain the political system under the NRM. Furthermore, Ugandans who perceive the political system to be highly corrupt also point out the three elements – the frequency of corrupt actions, the small likelihood of punishment and the difficulty of distinguishing honest politicians – highlighted in the previous section. Having established that the political system in Uganda is perceived to be corrupt by a sizable portion of Ugandan citizens, this section will illustrate how these perceptions affect citizens' evaluations of politicians and ultimately their vote choice. These dynamics help underscore a key failing on the democratic accountability models, and show why corruption is often 'sticky' and difficult to dislodge through electoral

competition. This section draws on data from interviews with elites and focus group discussions with average citizens as well as media and NGO reports.[9] Interviews with elected officials, candidates, civil servants, journalists, and NGO employees were conducted in 2012 in Gulu, Lira, Jinja, Katakwi, and Masaka Districts.[10] Focus groups were conducted in 2013. Focus group discussions took place in Bungatira, Gulu; Butansi, Kamuli; Katawki, Katawki and Kakoba, Mbarara.[11] The interview and focus group locations were chosen to capture variation in language, level of urbanization, and partisanship.

Frequency and Extent of Grand Corruption

The opportunity to benefit from state resources – whether through favorable contracts, embezzlement, or other kickback schemes – structures the incentives of nearly all actors in Ugandan politics. Critically, military officers' loyalty to the regime is courted through the opportunity to personally profit from Ugandan military actions and to skim off an expanding military budget which has been subject to periodic procurement and kickback scandals (Tangri and Mwenda 2013; Tripp 2010). For example, senior members of the Uganda People's Defense Force, including the president's brother, profited from Ugandan military actions in the Democratic Republic of Congo by establishing control and guaranteeing continued production of natural (e.g., gold, coltan) and agricultural (e.g., vanilla, coffee) resources (Vlassenroot, Perrot, and Cuvelier 2012). The main actors in these networks were 'recycled' back into the regime after the conflict ended, filling such roles as minister of trade and industry (Vlassenroot, Perrot, and Cuvelier 2012). Similar opportunities have kept key members of the NRM elite loyal to Museveni, with 'only a tiny minority of the state elite' having broken with the regime over the last two decades (Tangri and Mwenda 2013, 47). One such example involved money from the National Social Security Fund (NSSF) being used to pur-

[9]While Afrobarometer data is employed in other sections of the chapter, there are no questions asked in that survey measuring the dynamic explored in this section.

[10]In total, I conducted 50 in-depth interviews using the snowball method. This sample included NRM and opposition supporters and those in rural and urban areas.

[11]Focus groups were led by Ugandan facilitators of the same first language as participants and focus group discussions of men and women were conducted separately.

chase land in Temangalo at an inflated price from a company owned by the then-Security Minister, Amama Mbabazi. Then-Finance Minister Ezra Suruma was also implicated in the scheme, but both politicians were reportedly vigorously defended by Museveni and subsequently only the NSSF managing director was punished (Asiimwe 2013). Furthermore, this pattern of profiting from public office extends well beyond national figures; the opportunity to influence government contracts so that they 'go to family members, friends, or proxy companies [...] was one key reason people sought public office' at LC levels (Tripp 2010, 119). The Anti Corruption Coalition of Uganda reported one representative example when noting that residents of Bala subcounty had collected money to maintain a critical water source, but complained that their LC III (subcounty) chairperson never accounted for these funds (Anti Corruption Coalition Uganda 2013). The systematic nature of corruption in Uganda and the multiple actors who benefit from it makes it difficult to change without significant restructuring of incentives.

Ugandan citizens have the opportunity to observe some consequences of grand political corruption in their daily lives, and its role in the political system through any interactions with local governments as well as media coverage. Thus it is not surprising that many Ugandans perceive their political system, rather than just particular individuals, to be corrupt. When asked whether the political system in Uganda is corrupt or honest, focus group participants emphasized that corrupt behavior is common and characterizes all levels of the government. For example, participants stated 'There is serious corruption in Uganda starting at the LC I [village] level all the way to the presidential level' (Bungatira Subcounty, Gulu) and 'It is extremely corrupt, from LC I to the topmost authorities [...]' (Kakoba Division, Mbarara). In addition, citizens recognize that the systemic nature of corruption makes it more difficult for individuals to act 'honestly.' For example, one participant said 'It's not true that all the *candidates* are corrupt. It's only once people are elected and are in office and start getting access to the money, that's when people become corrupted' (Bungatira Subcounty, Gulu).

The high level of corruption within the political system as a whole is a key reason that

many Ugandans feel justified in voting for specific politicians whom they know to be corrupt. For example when asked why voters support corrupt politicians, one participant stated 'We vote for corrupt politicians just because it's the [nature of the] era. Politics is done on principles of corruption' (Butansi Subcounty, Kamuli). Others noted that since corruption is the norm, for the community to receive any resources is preferable to the potential of receiving nothing. For example, one participant stated: 'As we have been experiencing corruption almost everywhere, government money has become like fodder for the leaders. So if one of them takes it to develop the community, it's 100% right!' (Kakoba Division, Mbarara). Similarly, another said 'Most leaders take all the money for themselves, so if a leader actually brings some back to our community, it is better' (Bungatira Subcounty, Gulu). Beyond just expressing an expectation of corruption, some participants expressed preferences for it. As one woman put it 'In order to bring any development to the community, that politician has somehow been corrupt in order to bring that program successfully to the community, maybe by bribing other politicians. So with development, you must have corruption' (Butansi Subcounty, Kamuli).

Even among citizens who maintain that citizens ought to vote against corrupt politicians, their rationales frequently demonstrate the influence of the perceived level of system corruption on vote choice.[12] In many cases, these citizens simply perceived their system to be less corrupt.[13] First, some citizens specifically objected to politicians diverting funds on the grounds that the intended purpose would go unfulfilled; 'I do not agree that it's OK for a politician to take money even if it develops the community, because that money was budgeted and meant for another purpose. So what becomes of that purpose?' (Katakwi Subcounty, Katakwi). This objection is most potent if the respondent believes that the government would have otherwise implemented that project. Others feared long-term con-

[12]On the other hand, a small number of citizens expressed categorical disapproval of supporting corrupt politicians on moral grounds.

[13]A number of individual characteristics could cause citizens to perceive the system to be less corrupt, even within a corrupt political system. For example, consumption of media likely affects exposure to information about grand corruption. Some empirical evidence shows that people in diverse locations suspect more misappropriation (Olken 2009).

sequences which were based on the possibility of government action to curb corruption. For example, '[If a politician diverts funds] it could also cause the government to terminate the other development funds if those ones have been diverted' (Butansi Subcounty, Kamuli). All of these more practical objections still demonstrate the influence of perceptions of the system.

Politics of Punishment for Corruption

Second, punishment for corruption in Uganda is inconsistent and, as the previously referenced example of the Temangalo land scandal shows, frequently follows patterns of political expediency (also see Tripp (2010, 131-133)). Analyzing the record of the Anti-Corruption Court, Human Rights Watch (2013) finds that the majority (65 of 106) of defendants were local government workers; those holding this office were also convicted at the highest rate. In contrast, of the three central government leaders who were ever tried, only one was convicted, but his conviction was overturned on appeal. As one journalist from Jinja put it 'It is safe to steal billions, but if you steal millions you go to jail.' Some analysts argue that beyond just protecting corrupt members of the elite, Museveni allows corrupt behavior and then uses the threat of prosecution to ensure cooperation from members of the elite (Tangri and Mwenda 2013). One District Information Officer hinted at similar dynamics when he told me that 'We can't disseminate all information here. I am supposed to promote the positive aspect of the district' but that nonetheless, 'the public finds out [about corruption in service delivery] when it leaves the District level [...]. That is one way that public officials are punished, is by the government publishing information about their corruption.' This claim is difficult to prove, however it is indisputable that the president has significant formal power regarding anticorruption institutions, including the right to appoint the Inspector General of Government and the Auditor General, in addition to the potential to exercise influence through informal channels.

Regardless of the actual extent to which Museveni uses the potential for corruption charges to control the elite, the perception that most who commit corrupt acts goes un-

punished is common among citizens. Numerous focus group participants pointed out that high-profile scandals have failed to lead to prosecutions or, when officials are convicted, appropriate jail sentences. For example, one participant explained:

> Politics is very corrupt. In relation to the past ruling regimes, this regime has experienced the highest level of corruption. We have seen high-profile people, the so-called 'untouchables' involved in embezzlement scandals, yet they are not subject to strict rules of law. People talk about the scandals, it becomes a topic, then the culprits walk away without being touched, hence they are called 'untouchables.' (Kakoba Division, Mbarara)

Another participant explained that 'It is impossible to fight corruption, because the agents that are put in place to fight corruption are also corrupt. Look at the police and the courts – they are all corrupt!' (Bungatira Subcounty, Gulu). A different participant explained the arbitrary nature of justice, saying that in Uganda, 'the laws are not good for all; many are unpunished while others are punished' (Katakwi Subcounty, Katakwi). While no public opinion data is available specifically about punishment for corruption, nearly 59% of Ugandans who responded said that officials who commit crimes 'often' or 'always go unpunished' (Afrobarometer 2012). In contrast, only 27% of respondents said the same was true of 'ordinary people.'

Multiple other focus group participants emphasized that while judges should fight corruption, they are dependent on political leaders and thus unable to act independently. While many participants believed that the president could lower corruption if he wanted, the same participant quoted above explaining 'untouchables' observed that 'Most of the people around the president are his relations [part of an ethnic network] so there is no way he can touch an iron hand on them.'

Beliefs about the likelihood of punishment also implicitly inform vote choice calculus. For example, one man refused to endorse a corrupt politicians on the grounds he might embarrass the community, saying 'what if that politician is caught? Then the community will appear corrupt to everyone else' (Bungatira Subcounty, Gulu). But this objection is

only applicable if a politician is likely to be suspected and punished for corruption. No other participants brought up the possibility of punishment as a risk of supporting a corrupt politician. Instead, participant comments, such as those bemoaning the president's support for scandal-ridden elites or others endorsing a ban from office for any politician suspected of corruption, indicated that they perceived little risk to the community from the potential for punishment.

Difficulty of Distinguishing 'Honest' Candidates

Finally, citizens' beliefs that most politicians are corrupt make it difficult for politicians or candidates to effectively signal 'honesty' during a campaign. Citizens can identify 'overtly corrupt' politicians. For example, many focus groups respondents pointed to prominent politicians who had been the subject of media reports on corruption scandals. In addition, corrupt behavior of politicians who are not the subject of scandal might nonetheless be locally known. However, citizens have difficulty distinguishing 'honest' candidates from those who may be corrupt, but have not been identified as such yet.

First, a candidate must spend resources to have a chance of victory during a campaign. But there is a strong understanding among Ugandans that at least some money spent during campaigns is embezzled. For example, when asked how candidates financed their campaigns, focus group participants responded 'Politicians use the taxes that we voters pay and then in turn they use them in their campaigns' (Bungatira Subcounty, Gulu) and 'Some of these people just divert money, especially the politicians who come to stand again but have already been in service. If they're expecting to stand again and are given money for a particular project, they will take some of that money and divert it for their campaign' (Butansi Subcounty, Kamuli). Another participant made an explicit connection between high-profile corruption scandals and campaigns saying 'Many of them swindle government money for their campaigns. For example, the Global Fund money, the money that was supposed to buy bicycles for local councils, the GAVI funds... this is the money they use in campaigns' (Kakoba Division, Mbarara).

When asked how one could tell that a politician was corrupt, respondents pointed out that distributing gifts during campaigns could signal corruption. For example, one participant said that 'If the politician buys the electorate during the campaign by giving them goods like soap, like sugar to get them to vote for him or her, [then you can tell that politician is corrupt]' (Butansi Subcounty, Kamuli). Yet, failing to distribute resources is not an effective signal of honesty. For example, one man noted that 'An MP always gets the constituency development funds. So if we don't see those funds, then it is a clear sign of corruption' (Bungatira Subcounty, Gulu) while another woman similarly stated '[...] the government sends help to the community through the MP, but the members of the community actually don't receive anything. You can tell then that the MP has just eaten all the funds' (Butansi Subcounty, Kamuli). Thus in practice voters have difficulty distinguishing honest and corrupt candidates, which makes corruption a difficult criteria to vote on.

In sum, a sizable portion of Uganda's citizenry believes that their political system is highly corrupt, and in particular that corruption is frequent and widespread, that corrupt officials are unlikely to be punished, and that the proper default assumption is that candidates are corrupt. Citizens who perceive the system to be highly corrupt are unlikely to punish corrupt individuals, and some even prefer them. Together, this evidence indicates that if citizens perceive the political system to be highly corrupt, elections are unlikely to be effective accountability mechanisms.

2.5 Beyond Vote Choice

A significant body of scholarly work argues that corruption is not as problematic as Western scholars are prone to believe, and may even improve the functionality of developing states (see Seligson (2002) for a review of this literature). In this perspective, high corruption may be normatively neutral, even granting this chapter's theory that high perceptions of corruption make democratic accountability less likely. Drawing on evidence from recent Afrobarometer data (2012), I argue that this line of reasoning understates the effects of high

corruption on citizens' political behavior. In fact, preliminary evidence from Afrobarometer data indicates that the perception that their government is highly corrupt is negatively correlated with political efficacy, likelihood of voting, and even democratic aspirations. While the survey data employed in this section has limitations, initial evidence nonetheless suggests that perceptions of corruption are linked with political behaviors beyond vote choice.

One challenge in testing a theory that focuses on perceptions of the political system is the dearth of public opinion data asking about this system per se. To work around this challenge, I performed a principal component analysis (PCA) on four questions measuring prevalence of corruption among the president and his office, members of Parliament, government officials, and local government councillors (the same questions used in Figure 2.1). By definition, the PCA produces dimensions which explain the most statistical variation in responses. I interpret the first dimension as the belief that the political system is not corrupt (i.e., lower values are associated with higher perceived corruption). The resulting variable ('Honest' System PC) is the main one of interest in the following analyses.

High systemic corruption could lower citizens' 'external political efficacy' (the belief that government will respond to citizen participation). Evidence from Latin America (De La O et al. Forthcoming; Davis, Camp, and Coleman 2004; McCann and Domínguez 1998) and Eastern Europe (Kostadinova 2009) is consistent with the claim that if people believe that the system is corrupt, they are less likely to believe that democratic participation will have an impact. Is this the case in Uganda? Controlling for a host of individual characteristics – including age, gender, political interest, general trust, rurality, and poverty – those who perceive the political system to be cleaner are more likely to believe that both members of Parliament and local councillors 'try their best to listen to what people like you have to say' (Table 2.1).

Similar to political efficacy, beliefs that the system is highly corrupt and turnout are plausibly also negatively related. Turnout over the last three elections in Uganda decreased from 70% and 68% of registered voters in 2001 and 2006 respectively to 59% in 2011 (International Institute for Democracy and Electoral Assistance 2011). When multiparty competition was

Table 2.1: Belief in a Low Corruption Political System and Belief that Elected Officials Listen

	(1) MPs Listen	(2) LCs Listen
(Intercept)	-0.73	-2.23***
	(0.52)	(0.52)
"Honest" System PC	0.21***	0.14**
	(0.05)	(0.05)
Rural	0.10	0.48*
	(0.24)	(0.23)
Female	0.05	-0.09
	(0.13)	(0.12)
Age	0.00	0.00
	(0.00)	(0.00)
Political Interest	0.08	0.38**
	(0.15)	(0.14)
Trust Others	0.47**	0.42**
	(0.17)	(0.16)
Sec. Educ.	0.08	-0.25 †
	(0.14)	(0.13)
Poverty PC	0.02	0.06
	(0.05)	(0.05)
Language FE	Y	Y
Region FE	Y	Y
Interviewer Language FE	Y	Y
Interviewer Sex FE	Y	Y
Who Sent Us FE	Y	Y
N	1867	1867

Standard errors in parentheses

† significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

instituted in 2006, it may have caused voters to perceive the potential for systemic changes through electoral mechanisms. However, subsequent results showed more continuity than change. One focus group participant drew the connection between this declining turnout and corruption, saying 'The biggest reason why 30% of Ugandans don't vote is because of corruption. There is nothing to gain from a corrupt leader. People don't take voting as a[n important] point anymore' (Kakoba Division, Mbarara). A journalist in Gulu expressed similar sentiments, saying 'Very few people even turnout because they think voting is just a show. Why vote? It's just a waste of time.' Afrobarometer data also shows this point.

Controlling for individual characteristics, those who perceive the system to be more honest are more likely to have reported having voted (Table 2.2). This finding suggests an additional way in which classic models of democratic accountability could be flawed – those who are most sensitive to information about corruption may also be more likely to become disengaged and thus precluded from helping hold public officials to account.

Table 2.2: Belief in a Low Corruption Political System and Turnout

	(1) Voted in 2011
(Intercept)	1.85***
	(0.56)
"Honest" System PC	0.08†
	(0.05)
Rural	0.19
	(0.25)
Female	-0.46**
	(0.14)
Age	0.00
	(0.00)
Political Interest	0.44*
	(0.18)
Sec. Educ.	-0.30*
	(0.15)
Trust Others	-0.22
	(0.18)
Language FE	Y
Region FE	Y
Interviewer Language FE	Y
Interviewer Sex FE	Y
Who Sent Us FE	Y
N	1884

Standard errors in parentheses

† significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Beyond just political efficacy and turnout, perceptions of high system corruption may even be correlated with aspirations for democracy. Work from around the world has found that higher perceived government corruption is associated with less satisfaction with regimes and less trust in government (Anderson and Tverdova 2003; Chang and Chu 2006; Morris and Klesner 2010; Pharr 2000; Seligson 2002). In the case of Uganda, citizens' perceptions

that the political system is corrupt could well lead to a disenchantment with democracy in general, not just with the current state of electoral competition in Uganda. This is so because citizens' evaluations of democracy and electoral mechanisms in general probably depend upon their evaluations of the functioning of their own electoral systems (Mattes and Bratton 2007; Moehler and Lindberg 2009). There is some evidence from public opinion data that this is the case. On the one hand, nearly all Ugandans say they prefer democracy to any other form of government and support holding elections; belief in a 'clean system' does not predict these direct measures of support for democracy. However, a series of questions asks Ugandans to rate the current degree of democracy in their system and the extent that they would like Uganda to become democratic in the future. In these questions, not only does belief in a clean system predict higher assessments of Uganda's current state of democracy, it also is positively associated with aspirations for *future* levels of democracy.

This section's analyses have a number of important limitations. First, the variable of interest is derived from a PCA rather than being a direct measurement, which raises the possibility that it does not capture beliefs about the political system as such. Second, the survey data cannot establish causality. For example, it could be the case that individuals who are less likely to vote (for whatever reason) are more likely to say the government is corrupt in order to rationalize that decision. For these reasons, the conclusions from this section should be regarded as preliminary. Nonetheless, taken together, this evidence suggests that citizens who perceive their political system to be highly corrupt are likely to have low political efficacy, to be less likely to turnout to vote, and even to aspire to lower levels of democracy in the future. Thus political corruption is an issue which not only affects how citizens vote, but also could change they way they conceive of themselves vis-a-vis the state. These reinforcing dynamics suggest that systemic corruption is an important factor conditioning citizens' relationship with the state and that change in the level of political corruption is unlikely to result from 'ordinary' politics.

Table 2.3: Belief in a Low Corruption Political System and Democratic Aspirations

	(1) Prefer Dem.	(2) Prefer Elec.	(3) Current Dem.	(4) Aspire Dem.
(Intercept)	2.74 ***	2.30 ***	3.80 ***	4.64 ***
	(0.60)	(0.64)	(0.48)	(0.63)
"Honest" System PC	0.02	0.05	0.28 ***	0.12 *
	(0.05)	(0.06)	(0.04)	(0.05)
Rural	0.29	-0.25	0.09	0.08
	(0.25)	(0.32)	(0.19)	(0.26)
Female	-0.03	-0.03	0.24 *	0.03
	(0.14)	(0.17)	(0.11)	(0.14)
Age	-0.00	-0.00	0.00†	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Pol. Interest	0.21	0.31	0.08	0.08
	(0.17)	(0.21)	(0.12)	(0.16)
Sec. Educ.	0.45 **	-0.36 *	-0.26 *	0.21
	(0.15)	(0.18)	(0.12)	(0.15)
Trust Others	-0.45 *	-0.31	-0.02	-0.08
	(0.17)	(0.21)	(0.15)	(0.20)
Language FE	Y	Y	Y	Y
Region FE	Y	Y	Y	Y
Interviewer Language FE	Y	Y	Y	Y
Interviewer Sex FE	Y	Y	Y	Y
Who Sent Us FE	Y	Y	Y	Y
N	1884	1884	1783	1525

Standard errors in parentheses

† significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

2.6 Clientelism or Vote-Buying as Alternative Explanations?

Rather than perceptions of the political system causing citizens to downgrade the importance of corruption, it could be the case that voters choose corrupt politicians because they expect to personally benefit from their corruption. A number of scholarly traditions explore the use of 'material inducements,' defined as personal benefits transferred to voters, to secure support for politicians (Bratton 2008; Nichter 2008; Rundquist, Strom, and Peters 1977; Schaffer 2007; Vicente and Wantchekon 2009). These scholars point out that in addition to being harmed by corruption, voters may also receive illicit individual benefits from politicians. Politicians could funnel ill-gotten resources to constituents during campaigns

(vote buying) or during non-election years. The assumption that politicians act in this manner, and that voters reward them for it, underlies theories which predict that incentives for politicians to cultivate the 'personal vote' (in plurality and open-list proportional representation (PR) systems) will lead to higher levels of corruption (Chang and Golden 2007; Golden and Chang 2001).

Do Ugandan citizens support corrupt politicians solely because of clientelism? The large swings in vote shares between elections provide a priori evidence that networks effectively binding citizens to parties are either non-existent or ineffective. The long-lasting patron-client networks described in much of the literature on clientelism (Hicken 2011; Kitschelt 2000; Kitschelt and Wilkinson 2007) are not present in Uganda. Interviews with politicians, journalists, and NGO employees in Uganda indicated that politicians mainly spend resources in their districts during elections, whereas they lose contact with community members between elections. For example, one journalist in Gulu explained that 'Many of them [politicians] just go and sit in Kampala. Or the people from the deeper country just come to town and stay here.' This assessment is consistent with public opinion data. While almost 74% of respondents thought MPs *should* visit their constituencies monthly or weekly, or spend nearly all their time there, nearly 72% of respondents said their MPs visited their constituency never or once a year (Afrobarometer 2008). Similarly, a civil society leader from Katakwi explained that 'Gifts happen mainly during elections' which people take knowing it is unlikely that a 'politician will come back after they are elected anyway.' While politicians employ agents to distribute resources, interviews indicated that agents who work for multiple politicians are not uncommon. For example, a journalist in Masaka affirmed that politicians 'use different people every year' while another reporter in Jinja said that some agents 'will first go to a candidate and will say 'I will work to get you votes' and then two hours later will go to someone else and say the same.' Furthermore, multiple politicians explained that they minimize visiting constituents in their homes or keeping in touch with agents in order to avoid giving gifts or cash.

While the foregoing discussion might rule out clientelism, it does not eliminate the

possibility of vote-buying. Despite spending significant resources for elections, evidence of Ugandan politicians 'buying votes' is limited. Conroy-Krutz and Logan (2011) use panel Afrobarometer surveys from before and after the most recent Ugandan elections and find no evidence that those who reported receiving NRM resources or benefitting from recent government-provided public goods were more likely to support the president. Furthermore, while Ugandans believe that vote-buying is widespread, the majority respondents report themselves and their neighbors to be likely to 'defect' (take the money but vote for another candidate) if offered a chance to have their votes bought (Conroy-Krutz and Logan 2011). One journalist from Masaka vividly described such defection in an off-cycle parliamentary election, saying 'maybe people have started to understand [the nature of money in politics in Uganda] because they ate everything, they drank everything, but they didn't give them their votes.' An LC III (sub-county) chairman from Katakwi expressed a similar sentiment about the difficulty of identifying voters, saying 'During the campaign its very hard to tell where your support is, especially because the campaign is commercialized. People will jump and chant and wait to receive from a candidate even if they don't support him.' Moreover, given the Afrobarometer data above showing that most Ugandans believe their votes are kept private from powerful people, it is difficult to see how politicians' agents could enforce a strict 'quid-pro-quo' vote buying transaction. Taken together, this evidence suggests that clientelism or vote buying alone cannot explain the lack of electoral sanctions for corrupt politicians.

2.7 Conclusion

This chapter argues that voters consider the corruption of the political system in evaluating individual political candidates. Thus the level of political corruption becomes self-reinforcing, with voters in highly corrupt systems being less likely to punish candidates. The evidence in this chapter is suggestive; challenges in convincingly measuring actual corruption among Ugandan MPs and the generally uniform perception that the Ugandan political sys-

tem is highly corrupt hinder causal inference. Future research can test the theory developed in this chapter by experimentally manipulating perceptions of corruption, further exploring subnational variation in perceptions, or conducting cross-national comparisons of countries which have varying levels of perceived corruption of the political system.

If this chapter's argument is correct, electoral competition alone is unlikely to curb corruption. A natural question is what might change Ugandans' perceptions of the corruption of the political system. Some scholars have argued that scandals can serve a 'functional' role by demonstrating to citizens that wrongdoing is punished (see Maier (2011)). In this scenario, citizens might believe the political system to be less corrupt after hearing that perpetrators of grand corruption have been exposed or prosecuted. This function is unlikely to be served in the Ugandan context, however, because punishment is perceived as arbitrary and not as strengthening norms against corruption. Instead, it seems that both levels of corruption and the overarching electoral trends are likely to continue in Uganda unless a more radical transformation – such as a sudden change in government caused by extra-electoral means – takes place.

3 | Who is Corrupt? Cognitive Bases of Biased Perceptions

3.1 Introduction

In 2006, Congressman Tom DeLay was facing a primary challenger, had just been indicted for money laundering, and was under intense scrutiny for his relationship with convicted lobbyist Jack Abramoff. One of Representative DeLay's supporters dismissed the charges against him as politically motivated, saying "He has no ethics problems. He has Democrat problems" (quoted in Seabrook (2006)). Although DeLay resigned later that year, his supporter's assertion raises the crucial question of how voters form opinions about the credibility and seriousness of accusations of corruption against public officials.

Much scholarly work focusing on corruption begins with the assumption that voters dislike corruption and thus vote against politicians whom they believe to be corrupt. To explain persistence of corruption even in areas with high quality democratic institutions, scholars have argued that voters "trade-off" between a politician's honesty and other desirable traits (Manzetti and Wilson 2007; Myerson 1993; Rundquist, Strom, and Peters 1977) or that limited information about corruption prevents voters from punishing corrupt politicians (see Pande (2011) for a thorough review). In this chapter, however, I focus on the logically prior question of how voters interpret information about corruption. By this I mean both the conclusions that voters draw when given certain information and the cognitive processes that lead to these conclusions. In particular, I expect that voters' partisanship influences how they perceive politicians described as committing ethically ambiguous actions. This implies that democratic accountability is necessarily limited by partisanship. Furthermore,

I provide new evidence consistent with voters engaging in motivated reasoning, rather than using party labels as heuristics as is commonly assumed. Finally, I show that even with a free press, the ideological orientation of a media source affects how seriously allegations are taken.

Despite the large literature on corruption and vote choice, little scholarly work has focused on how voters interpret information about corruption allegations (but see Anduiza, Gallego, and Muñoz (2013) for a recent exception examining perceptions of corruption among Spanish citizens). This omission is significant for several reasons. Most broadly, citizens act based on their interpretations of information. If citizens do not perceive a politician as corrupt even when given information to that effect, then neither a trade-off nor informational framework is sufficient to explain later voting behavior.

In addition to understanding voters' perceptions of politicians' corruption, we must also understand the cognitive processes that affect these perceptions. Theories in political psychology offer two means by which partisanship might affect voters' interpretations of corruption. First, voters might use heuristic judgement, meaning that they use partisanship as a shortcut to avoid expending cognitive resources in processing information. Second, voters might employ motivated reasoning to meet directional goals, in this context meaning that they try to reconcile new information with their partisan beliefs. While each of these cognitive mechanisms would lead partisans to perceive copartisan politicians to be less corrupt, they have different implications for our understanding of democracy and for policy interventions that might lessen corruption. In the case of heuristic reasoning, citizens still wish to make accurate assessments and might respond to a policy intervention designed to encourage the use of more appropriate heuristics. If citizens use motivated reasoning, however, it may be more challenging to design an appropriate policy intervention. Instead, it becomes more important to understand what types of information partisans cannot counterargue or rationalize away. Finally, scholarship on the importance of information tends to focus on either the freedom of the media (Lederman, Loayza, and Soares 2005; Treisman 2007) or the extent of reporting (Costas-Pérez, Solé-Ollé, and Sorribas-Novarro 2012; Pande 2011). This

chapter shows that within a free media environment, citizens' perceptions of the ideology of media sources influences how credible they deem allegations to be. Thus not only the information reported, but where it is reported affects democratic accountability.

This research is also relevant to other studies on the causes and consequences of corruption. The common practice in the scholarly literature of using indices measuring perceptions of corruption as indicators of actual corruption makes it particularly important to understand systematic factors that affect perceptions. Most empirical studies of corruption rely on indices based on perceptions of experts and business analysts (e.g. Transparency International's Corruption Perceptions Index, the Worldwide Governance Indicator for Control of Corruption, or the corruption indicator in the International Country Risk Guide) and more recently on perceptions of ordinary citizens (e.g. Transparency International's Global Corruption Barometer, and certain questions in the regional Barometer public opinion surveys). Without fully understanding the systematic factors[1] that skew such assessments, scholarly work risks attributing changes in corruption to factors that really affect perceptions.

The remainder of this chapter is organized as follows. Section 1.2 provides an overview of levels of corruption in the United States and draws on the scholarly literature to argue that it is reasonable to expect partisanship to affect individual's perceptions of corruption allegations. Section 1.3 outlines the theoretical underpinnings of the hypotheses that citizens are less likely to believe allegations of corruption against copartisan politicians and employ motivated reasoning rather than using party labels as heuristics. It also hypothesizes that counterstereotypical allegations against politicians from ideologically similar media sources are more credible, but that partisanship plays a meaningful role in shaping perceptions, even when voters are exposed to counterstereotypical accusations against politicians. Section 1.4 provides an overview of the survey experiment that tests these hypotheses. Section 1.5 presents results, which are generally consistent with the theory, but provide a more bounded

[1]Noting the importance of understanding how perceptions are formed, several scholars have attempted to identify factors that bias perceptions of corruption. They have found that age, level of education, and diversity of home village (Olken 2009) and trustingness (Wroe, Allen, and Birch 2012) affect perceptions of corruption. Razafindrakoto and Roubaud (2010) compares perceptions of "country experts" and those of citizens in eight African countries and finds that experts report higher levels of corruption than citizens.

scope of motivated reasoning than was hypothesized. Section 1.6 concludes, highlighting the importance of even-handed media coverage for democratic accountability.

3.2 Corruption and Partisanship in the United States

Levels of corruption[2] in the United States are relatively low compared to the rest of the world. Nonetheless, corruption in the American context warrants scholarly attention. First, while corruption is low in the US, it exists. Understanding how voters perceive corruption allegations could help explain circumstances under which voters check corruption. Second, corruption scandals are a subset of a much wider set of ethical issues. Understanding how partisanship affects voters' perceptions of allegations of corruption can offer insight into the prospects for voters holding politicians accountable in more quotidian issue areas.

Corruption is less common in the United States than in many countries, but it is hardly nonexistent. Although measuring corruption presents a perennial challenge, scholars often use the number of federal, state and local public officials convicted of a federal corruption-related crime as a proxy for corruption itself (Glaeser and Saks 2006; Goel and Nelson 1998; Meier and Holbrook 1992). In 2012, more than 1000 public officials were convicted on such charges (United States Department of Justice 2012); the number of public officials accused or suspected is no doubt much higher. This non-trivial level makes it meaningful to ask how American voters interpret information about such accusations. How does the public perceive corruption in the United States? In response to questions about politicians in general, Americans tend to express cynicism. For example, the American National Elections Studies ask respondents how many "people running the government" they think are "crooked." In 2012, nearly 59% of respondents chose the largest response of "quite a few." While there was a partisan divide in these perceptions, about 53% of Democrats chose this response while

[2] Although scholars generally agree that corruption is "the misuse of public office for personal gain" (Rose-Ackerman 1978; Treisman 2000), it is difficult to establish whether certain actions fall within this definition. For example, Redlawsk and McCann (2005) finds that Americans broadly agree that actions which are obviously illegal constitute corruption, while there is more disagreement about the corruption of legal actions that imply favoritism. While this distinction is important, this chapter focuses on unambiguously corrupt actions to devote more attention to the prime variable of interest – the potentially corrupt actor's identity.

about 63% of Republicans and Independents did.[3] In Transparency International's 2013 Global Corruption Barometer, a plurality of American respondents (38%) called public sector corruption "a very serious problem" (Transparency International 2013). The distribution of responses to this question among Americans was roughly similar to that of respondents from Sierra Leone. Of course, it is unclear whether American responses indicate a belief in genuine corruption or rather a general dissatisfaction with government. Studying specific allegations of corruption, rather than general sentiments, helps mitigate this problem of interpretation.

The extensive corpus of scholarship on partisanship's role in shaping public opinion provides good reason to posit that it also should shape perceptions of corruption. Party identification is more stable than issue area opinions (Green et al. 2002) and tends to constrain political values (Goren 2005). Party cues not only affect opinion formation (Baum and Groeling 2009), they also affect whether partisans respond to issue framing (Slothuus and de Vreese 2010), particularly in polarized environments (Druckman, Peterson, and Slothuus 2013). Observational evidence is consistent with partisanship affecting perceptions of corruption. For example, Redlawsk and McCann (2005)'s survey asking respondents to evaluate the corruption of an official offering no-bid contracts happened to be fielded during the 2004 controversy over no-bid contracts for reconstruction in Iraq that were offered to Halliburton. In this survey, Republicans were significantly less likely to judge an official committing such an action as corrupt, consistent with partisanship influencing perceptions of corruption. Despite its importance to political accountability, however, there has been little work examining how partisanship affects perceptions of allegations of corruption (but see Anduiza, Gallego, and Muñoz (2013) in the Spanish context) and no work attempting to identify the cognitive mechanism underlying any observed partisan bias. This chapter addresses those questions.

[3] If survey respondents associated "people running the government" with the Democratic president, this partisan divide could be explained by motivated reasoning as detailed throughout this chapter.

3.3 Theory: Partisanship, Media and Perceptions of Corruption

Building on the preceding literature, I hypothesize that when given information about allegations of corruption, people perceive actions by members of their own groups to be less corrupt.

Hypothesis 1. *In-Group Bias: Citizens assess accusations against copartisan politicians as less credible than those against hypothetical politicians without party labels, which in turn are judged more favorably than accusations against politicians from the other political party.*

There are two cognitive mechanisms which might lead to this outcome. First, people might use identity-based *heuristics* as a cognitive shortcut. Rather than answering a complicated question about an individual's culpability, citizens might instead answer an easier question of how they feel about a particular group. Second, social identity theory suggests that people connect their own self-esteem to the standings of their groups (see Haidt and Kesibir (2010) for a discussion of social identity theory in the context of moral reasoning). Therefore, perceiving behavior of a fellow group member to be more moral could be a way to preserve one's own self-esteem. In this case, copartisans might engage in *motivated reasoning* to meet the goal of preserving esteem for their in-groups. The next subsection discusses these mechanisms in more depth.

3.3.1 Heuristic Judgment or Motivated Reasoning?

Significant bodies of work in political science document the effects of party cues on citizens' evaluations. Most work has assumed that citizens use such cues as heuristics,[4] although an important body of work suggests that these changes result from motivated

[4]For example, papers discussing party labels as heuristics cover subjects like the role of minor party labels as heuristics (Coan et al. 2008), the influence of party labels vs. other information in attributing blame for poor government performance (Malhotra and Kuo 2007), and the limits of the influence of party cues (Bullock 2011).

reasoning (Druckman, Peterson, and Slothuus 2013; Lodge and Taber 2000; Slothuus and de Vreese 2010; Taber and Lodge 2006). While political psychologists have only recently begun adjudicating between these mechanisms (Bolsen, Druckman, and Cook 2014; Petersen et al. 2013), these distinctions – which are discussed more fully below – are important. This is because the use of heuristics implies that "citizens are basically motivated to hold accurate opinions" whereas motivated reasoning implies that "citizens are directly motivated to be biased, making partisan bias inevitable" (Petersen et al. 2013, 844). In the context of information about corruption, this implies that if citizens use appropriate heuristics, information may be able to induce accountability whereas if citizens engage in motivated directional reasoning, information alone is unlikely to lead to in-group accountability.

Heuristics are "common judgmental shortcuts that people use to draw complicated inferences from simple environmental cues" (Lupia, McCubbins, and Popkin 2000, 17). Psychologists developed the idea of heuristic processing in contrast to the notion that people's evaluations of a particular object are the end result of weighted evaluations of each dimension of that object (called "rational processing") (Druckman, Kuklinksi, and Sigelman 2009). A person may come to the same conclusion when using heuristic and rational processing; the crucial difference is the means by which people arrive at a given outcome.

To illustrate, imagine a citizen faced with a news report of an allegation of corruption against a politician. If the citizen were employing ideal rational processing, she would consider a number of dimensions – the credibility of the newspaper itself, the number and credibility of sources it quoted, the action of which the politician is accused and its likely consequences, other elements of the politician's performance, and the politician's party – then evaluate each of the dimensions and weight their importance to determine her final conclusion of how serious she considers the allegation. In contrast, a heuristic judgment would depend on just one dimension as a shortcut to the intensive cognitive work full processing entails. For example, the citizen might use the politician's political party – or even an apparently unrelated characteristic such as the poltician's gender or race – as a heuristic and conclude that the allegation was not credible or that the behavior was not

serious.

In contrast to the use of heuristics, motivated directional reasoning is not a computation shortcut. Motivated reasoning assumes that people have particular goals when considering an object; they may wish to reach the correct conclusion (accuracy goals) or they may wish to confirm prior beliefs (directional goals) (Kunda 1987, 1990, 1999; Lodge and Taber 2000; Redlawsk 2002).[5] When people are engaged in motivated directional processing, they may underweight and/or avoid evidence that contradicts prior beliefs and overweight and/or seek out evidence that confirms prior beliefs (Taber and Lodge 2006).

Scholars who argue that directional motivated reasoning is common in politics do not expect it to be limitless; the realm of possible conclusions that a person may reach is constrained by her own understanding of reality.[6] But the evidence for many allegations of corruption is ambiguous.[7] With regard to accusations of corruption, conflicting or scarce information and the real possibility of a political "witch hunt" make the range of justifiable conclusions quite broad. People who identify with a particular political party may be motivated to perceive that group as honest. Therefore, a citizen faced with an article accusing a member of her own group of corruption may engage in extra cognitive processing in order to dismiss the evidence against that person (for example, by questioning the credibility of accusers or justifying the action). She thus might come to a conclusion that is congruent with her prior attitudes through more complicated reasoning.

Although political science has generally interpreted party cues as working through heuris-

[5]Much work in political science focuses on directional goals, therefore motivated directional reasoning is often shortened to just motivated reasoning (Druckman et al. 2009).

[6]For example, Sanitioso, Kunda, and Fong (1990) found that introverts who were motivated to see themselves as more extraverted did indeed rate themselves as more outgoing. However, those introverts still rated themselves as less outgoing than true extraverts in the same study; while they were motivated to see themselves as more extraverted, they still could not reach an assessment which they could not justify (Kunda 1999).

[7]In contrast, studies finding that partisans update their opinions at similar rates are often based on scandals which featured very convincing evidence. For example, Green, Palmquist, and Schickler (2002) note that while possessing different baseline opinions, members of all parties seemed to adjust their opinions at similar rates in the wake of major scandals like Watergate and Bill Clinton's affair with a White House intern.

tics, the extensive psychological literature (Bersoff 1999; Kunda 1987, 1990, 1999; Kunda and Sinclair 1999) and recent political science studies (Bolsen, Druckman, and Cook 2014; Petersen et al. 2013) provide a strong basis to expect motivated directional reasoning in the presence of party labels.

Hypothesis 2. *Motivated Reasoning: Respondents who view accusations against copartisans will engage in more effortful motivated reasoning (operationalized by longer viewing times) compared to those who view accusations against members of another party or those whose partisanship is not specified.*

3.3.2 Media and Corruption Allegations

In observational data, an aggregate pattern of partisans judging their own less severely does not necessarily imply cognitive bias. One challenge for voters in assessing levels of corruption is that politicians have obvious incentives to accuse opponents of corruption and to claim that any accusations against themselves are "merely partisan tricks" (Rundquist, Strom, and Peters 1977, 955). While scholarly works highlight the importance of freedom of media and extent of corruption reporting in controlling government corruption (Costas-Pérez, Solé-Ollé, and Sorribas-Novarro 2012; Lederman, Loayza, and Soares 2005; Treisman 2007), there has been little attention to how citizens' perceptions of the media source's ideology affects their propensity to take seriously corruption allegations. Many Americans believe that even mainstream media sources are biased against their own ideological stances (Arceneaux, Johnson, and Murphy 2012; Eveland and Shah 2003). Furthermore, many "new media" news sources actually do occupy distinct ideological spaces (Arceneaux, Johnson, and Murphy 2012; Peters 2010). Such sources might be less likely to report corruption accusations against politicians who share their ideological views or more likely question the credibility of evidence against such politicians. Experimental evidence indicates that Americans choose ideologically congruent news sources even on apparently apolitical topics such as crime or travel (Iyengar and Hahn 2009). Taken together these trends could imply that Americans are less likely to see accusations of corruption against their copartisans,

and likely to dismiss as biased those accusations that they do encounter if they come from ideologically dissimilar news sources.

Hypothesis 3. *Counterstereotypical Accusations: Citizens assess allegations against politicians reported in media sources that are ideologically similar to the politician as more credible than those from ideological dissimilar media sources.*

The literature on counterstereotypical position taking suggests that voters should take more seriously accusations against politicians that are reported in ideologically similar news sources than those reported in ideologically dissimilar news sources (Baum and Groeling 2009). The logic here is that the "business-as-usual" approach of attacking one's political opponents is recognized as "cheap talk" by citizens, while targeting one's political allies is recognized as costly by voters and thus taken more seriously. The implications of this for democratic accountability are mixed: while partisans may be unlikely to see reports of corruption by politicians in media of their ideological preference, when they do see them they will know offenses are severe.

Hypothesis 4. *Partisanship Trumps Counterstereotypical Accusations: Citizens assess accusations against copartisan politicians as less credible than those against politicians of another party, even when accusations against the copartisan come from a "friendly" media source.*

I hypothesize that the counterstereotypical allegation effect obtains, but that the effects of copartisanship trump the effects of counterstereotypical reporting. In other words, I hypothesize that a Democrat will consider an accusation against a Republican reported by a liberal media source to be more credible than an identical accusation against a Democrat in the same media outlet.

3.4 Research Design and Data

Because the information that an individual receives about corruption allegations is plausibly endogenous to that individual's partisanship, it is difficult to isolate the effect of parti-

sanship on perceptions of corruption using observational data. To address these challenges, I use a survey experiment that provides respondents with a hypothetical news account of allegations of corruption, but varies the media source and the political party of the relevant politician.

Treatments: The article shown in Figure 1.1, which describes allegations that a representative has accepted bribes from a defense contractor in exchange for awarding his company government contracts, represents one treatment condition. This type of corruption (bribes for government contracts) is chosen because it is a plausible occurrence in the American context[8] and most Americans agree that such an action would qualify as corrupt (Redlawsk and McCann 2005). Results from pilot versions of the treatment indicated that most respondents were inclined to believe the allegations. To avoid ceiling effects, therefore, the article frames allegations as coming from a potentially disgruntled former employee and as not having been fully substantiated.

To test the effects and mechanism of party label on citizens' perceptions, the treatments either identify the politician in question as a Republican, a Democrat, or do not mention a political party. The treatments also vary the purported source of the article between Fox News and MSNBC. Public opinion polling shows that Fox News and MSNBC are perceived as conservative and liberal respectively (The Pew Research Center 2009) and have large and persistent partisan gaps in their "believability" (The Pew Research Center 2012).[9] Because the survey is administered on computers, I use the logos associated with each organization's website (see Figure 1.1 for MSNBC.com logo used). When an accusation comes from an ideologically dissimilar news source (e.g. MSNBC critiques a Republican), it can be viewed as "cheap," since it reflects the usual partisan alignment of the source. Rather than focusing on each respondents' party (and thus modeling different effects for Democrats viewing Democrats than Republicans viewing Republicans), I collapse treatments into copartisan,

[8]For example, Representative Randy "Duke" Cunningham (R, CA) resigned from Congress in 2005 after pleading guilty to several federal charges related to a similar bribery scheme with a defense contractor.

[9]Both MSNBC and Fox News had gaps of approximately 25 between the percentage of Democrats considering the source credible and the percentage of Republicans (The Pew Research Center 2012).

REPRESENTATIVE FACES CORRUPTION ALLEGATIONS

February 4, 2015

A former employee of **Rep. Gregory Martin (Republican)** alleges that he should be investigated for public corruption.

The former employee, whose name is not being published at her request, alleges that Rep. Martin illegally passed confidential government documents to a construction company and lobbied to award that company federal contracts. In exchange for these actions, it is alleged that the company funneled cash to Rep. Myers. While the accusations could not be fully substantiated, the company in question was awarded at least one federal contract.

Rep. Martin vigorously denied these allegations, saying "These rumors are entirely false." Rep. Martin's chief of staff added that "These false allegations are an ill-conceived attempt at revenge by a staffer who was fired for poor performance. Rep. Martin's long and productive record of working for the people of this district speaks for itself."

Figure 3.1: Example of Treatment. Elements in boxes vary across treatment conditions.

opposite party, or no party label as summarized in Table 1.1. I adopt this strategy because theoretically I expect partisans of different parties to act similarly.

Table 3.1: Summary of Treatment Conditions

	Stereotypical Accusation	Counterstereotypical Accusation
Same Party	1A	1B
Other Party	2A	2B
No Party Label	— 3 —	

Dependent Variables: The dependent variables are answers to the following questions: How strongly do you agree with the following statements: "Representative Martin is corrupt" and "The allegations against Representative Martin are likely true." Possible responses range from strongly disagree to strongly agree. These response categories are given values from 1 to 5 with higher values representing more agreement.

In addition, I follow Petersen et al. (2013) and Bolsen et al. (2014) in using processing

time as indicator of either motivated reasoning or heuristic processing. Specifically, the time between the article loading and the respondent advancing to the next page is recorded. The logic for this operationalization is that more effortful cognitive processes require more time (Bassili 1995; Harreveld et al. 2004; Huckfeldt et al. 1999; Huckfeldt and Sprague 2000; Mulligan et al. 2003).[10] If partisan information causes directional motivated reasoning, then response times should be longer when a copartisan politician is accused than when the politician does not have a party label. The respondent uses this extra time to engage in the cognitive work necessary to bring her judgment more into accord with her preexisting orientation. In contrast, if used as a heuristic, then the presence of partisan information should cause less laborious processing, and thus quicker response times than similar information without party cues. This direct measure of processing time also avoids many concerns about bias associated with asking respondents to describe their own thought processes (Petersen et al. 2013).

Survey Protocol and Sample: The survey experiment was administered via Amazon's platform Mechanical Turk (MTurk).[11] Respondents were told that they would take a short survey about political attitudes and information consumption. After completing a series of demographic questions, they advanced to one of the six hypothetical news articles.[12] The dependent variable questions, along with the article, were shown on subsequent pages. Finally, respondents were asked a series of questions measuring political knowledge.

[10]Many papers cited here use response time as a measure of the accessibility of an attitude toward a familiar object (e.g. a well-known politician or abortion rights). This is a less natural interpretation in this study's context since respondents are reading a hypothetical article, featuring a hypothetical politician toward whom they are not expected to have a pre-existing attitude. A more compelling alternative interpretation might argue that longer response times reflect ambivalence when respondents are confronted with objects (political party, media source) toward which they have conflicting attitudes. Future research is needed to distinguish this interpretation from that of motivated reasoning.

[11]MTurk workers tend to be more liberal than the American population at large (Berinksy, Huber, and Lenz 2012). To ensure sufficient representation of Republicans, potential respondents first took a short demographic survey. A subset of respondents, which excluded some liberals and independents, was then invited to take the full survey. The pre-survey also asked for zip codes, offering extra assurance that the respondents recruited on MTurk are, in fact, Americans and not foreign workers using platforms to disguise their locations and gain access to "US Only" jobs (Chandler, Mueller, and Paolacci 2014).

[12]To improve balance, treatment was randomized conditional on partisanship.

3.5 Results

This section presents summary statistics and regression results which provide strong evidence that partisans are less likely to believe allegations against copartisan politicians and that this pattern results from motivated reasoning rather than heuristics. I also find that counterstereotypical accusations (i.e. when the politician and media sources are ideological similar) are deemed more credible. I do not find statistically significant evidence showing that copartisanship affects respondents' evaluations more than whether the accusations are counterstereotypical.

3.5.1 Summary Statistics

Table 1.2 presents average responses to the questions of whether Rep. Martin is corrupt and the allegations against him are true for each of the five treatment conditions of interest, with higher numbers indicating more agreement.[13] Comparing copartisan vs. other party politicians (row 1 versus 2) provides initial evidence in favor of a portion of Hypothesis 1, because copartisans are assessed more favorably. Yet, contra Hypothesis 1, partisans do not seem to view politicians without a party label more favorably than those who are affiliated with the other party. This could be because they assume the "worst" – that an unlabeled politician comes from the other party – or it could indicate that partisans feel positively toward their own party rather than negatively toward the other. Given that the US is dominated by two parties, distinguishing between these two explanations is difficult and of little practical importance. Additionally, comparing stereotypical versus

[13] I include "leaners" (i.e. those who initially claimed to be independents but then state they lean toward one of the two major parties) as partisans. This coding choice follows literature showing that many who claim to be independents act as partisans (Keith et al. 1992; Petrocik 2009). For most analyses, I exclude independent respondents because the majority of hypotheses examined here apply to partisans. Additionally, I exclude independents from most analyses because in their assessments of Republican and Democratic politicians, independents look more like Democrats (i.e. they are more favorable to Democratic politicians). While most (≈64%) of independents describe themselves as moderates, there are more liberals than conservatives among the remaining respondents. This prevalence of liberals may explain their apparent anti-Republican bias. I include independents in analysis for Hypothesis 3 because it concerns the relationship between the politician and media source, not the respondent.

counterstereotypical accusations (columns 1 and 3 versus 2 and 4) provides initial evidence in favor of Hypothesis 3, with counterstereotypical accusations being taken more seriously.[14]

Table 3.2: Summary of Evaluations, Partisan Respondents. Average responses on a 1-5 scale are shown with standard deviations in parentheses. Higher responses indicate more agreement.

	Rep. is Corrupt		Allegations are True	
	Stereotypical	Counterstereotypical	Stereotypical	Counterstereotypical
Same Party	3.30 (0.76)	3.46 (0.77)	3.36 (0.80)	3.52 (0.88)
Other Party	3.56 (0.76)	3.69 (0.71)	3.63 (0.81)	3.80 (0.67)
No Party Label	– 3.63 (0.80) –		– 3.70 (0.91) –	

Table 1.3 presents the average time spent viewing the hypothetical article in each treatment condition. To avoid outliers driving results, I exclude the six respondents who spent more than 200 seconds viewing the hypothetical article from these summary statistics and all analyses. These comparisons provide initial evidence for Hypothesis 2, because respondents spend longer viewing accusations (particularly counterstereotypical accusations) against copartisans (row 1).[15]

Table 3.3: Summary of Viewing Time for Partisan Respondents. Averages are shown with standard deviations in parentheses.

	Seconds	
	Stereotypical	Counterstereotypical
Same Party	43.55 (26.78)	47.89 (40.46)
Other Party	36.52 (24.26)	42.12 (24.75)
No Party Label	– 40.85 (29.94) –	

[14]T-tests confirm that the differences in assessments of copartisan versus other party or unlabeled politicians are statistically significant for both dependent variables. Differences in assessments of stereotypical and counterstereotypical accusations among partisans are marginally significant with p-values of 0.12 (Rep. is corruption) and 0.10 (Allegations are true).

[15]The difference in time spent viewing accusations against copartisans versus all other accusations is significant with a p-value=0.10 in two-tailed T-test.

3.5.2 Regression Results

Hypothesis 1

Regression results for Hypothesis 1 confirm the initial evidence from the summary statistics that partisans are less likely to believe allegations against copartisan politicians. Table 1.4 reports results in which the dependent variables are the extent to which the respondent agrees that the politician is corrupt (columns 1 and 2) or that the allegations against him are likely true (columns 3 and 4), and the main variable of interest is *Same Party Pol.*, which takes a value of 1 when a Democratic (Republican) respondent sees an article about a Democratic (Republican) politician. Conversely, *Opposite Party Pol.* takes the value of 1 when a Democratic (Republican) respondent sees an article about a Republican (Democratic) politician. In all specifications, I control for whether or not the respondent is a Democrat.[16] Thus the baseline category is a Republican respondent viewing an allegation against a politician without a party label in columns (1)-(4).

For ease of interpretation, I present OLS results here. Results using an ordered logit are substantively similar and are presented in Appendix A. In columns (5) and (6) I drop the *Opposite Party Pol.* variable, making the baseline category a Republican respondent viewing an allegation against either a Democrat or a politician without a party label. Coefficient sizes and statistical significance are consistent across all specifications.

Figure 1.2 shows the difference in predicted response for a Republican viewing an allegation against a Republican instead of against a Democrat.[17] In each case, the difference is approximately -0.25 on a 1 to 5 scale.[18] These significant differences exist despite the fact that respondents were told the article was hypothetical and that respondents generally displayed cynicism about government. For example, the modal response to how many people

[16]The coefficient on Democrat is positive and significant, consistent with Democrats in this sample being more suspicious of politicians, or perhaps particularly of the intersection of politicians with business interests.

[17]First differences are calculated using Zelig. All other control variables are set to their means. Results are substantively similar for a Democratic respondent.

[18]This represents approximately 30% of the standard deviations for each dependent variable.

Table 3.4: Ordinary least squares results for H1. Dependent variables are agreement with the statements that Rep. Martin is corrupt and that the allegations against him are probably true (1-5 Scale). Additional respondent characteristics include measures of respondent political knowledge, political interest, education, income, general trust, race, perceived prevalence of government corruption, and whether respondent is registered to vote.

	(1) Corrupt	(2) Corrupt	(3) True	(4) True	(5) Corrupt	(6) True
(Intercept)	3.56^{***}	3.35^{***}	3.59^{***}	3.43^{***}	3.34^{***}	3.43^{***}
	(0.08)	(0.26)	(0.08)	(0.28)	(0.26)	(0.28)
Same Party Pol.	-0.25^{**}	-0.25^{**}	-0.25^{*}	-0.27^{**}	-0.24^{**}	-0.26^{**}
	(0.09)	(0.09)	(0.10)	(0.10)	(0.08)	(0.09)
Opposite Party Pol.	-0.02	-0.03	0.00	-0.01		
	(0.09)	(0.09)	(0.10)	(0.10)		
Dem. Respondent	$0.14^{\cdot}$	$0.15^{\cdot}$	0.22^{**}	0.21^{*}	$0.14^{\cdot}$	0.21^{*}
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Fox		0.03		-0.01	0.03	-0.01
		(0.08)		(0.08)	(0.08)	(0.08)
Additional Resp. Chars.	*N*	*Y*	*N*	*Y*	*Y*	*Y*
Num. obs.	403	403	403	403	403	403

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

running the government are crooked was the highest possible response of "quite a few" and the large majority of responses to the dependent variable clustered in the upper half of the scale (i.e. between neither agreeing nor disagreeing and strongly agreeing that the representative was corrupt). In other words, despite being quite willing to condemn government officials, respondents viewing allegations against copartisans gave them more benefit of the doubt.

Hypothesis 2

To test Hypothesis 2, I use the amount of time between the initial webpage with the hypothetical article loading and respondents clicking forward to the next page as a proxy for their processing time. The dependent variable is measured in seconds rounded to the nearest millisecond. I first test whether the availability of party labels allows respondents to move through an article quicker, which would be consistent with party labels serving as heuristics. Respondents with the longest processing times have been removed from this

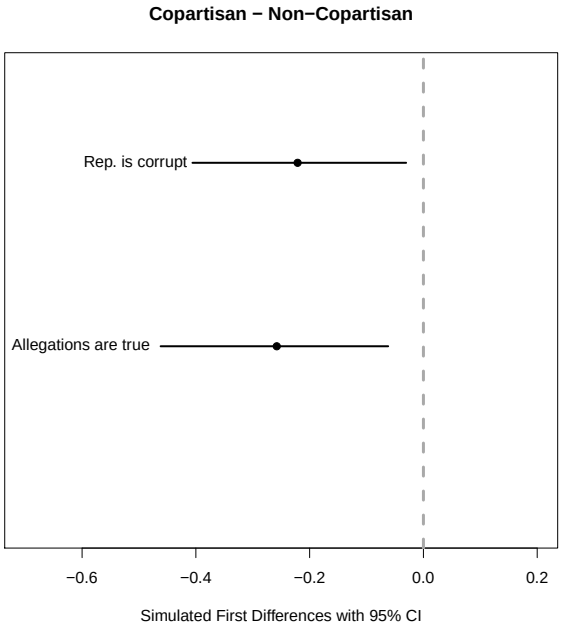

Figure 3.2: Simulated first differences in predicted response when viewing a politician of the same party versus one of the opposite party. Higher numbers indicate more agreement.

sample, but to mitigate concerns about remaining impact of outliers, I present results using the unlogged (columns 1 and 2) and logged (columns 3 and 4) measures of time. Table 1.5 shows that the presence of party labels has no significant effect on processing time in either simple (columns 1 and 3) or full (columns 2 and 4) models. Thus the evidence is not consistent with the use of heuristics to diminish processing burden.

To test for motivated reasoning, I compare the amount of time respondents who view allegations against a copartisan politicians spend reading the article to those who do not. Based on findings for Hypothesis 1 that point to an in-group effect, I also report models that exclude *Opposite Party Pol.* (columns 3 and 6).[19] Additionally, I present results for both unlogged (columns 1-3) and logged (columns 4-6) seconds. As expected, the coefficient on *Same Party Pol.* is positive, however results only approach conventional levels of statistical

[19]In other words, the baseline category is a Republican viewing an article about an unlabeled politician in columns 1, 2, 4, and 5, whereas it is a Republican viewing an article about an unlabeled politician or a Democrat in columns 3 and 6.

Table 3.5: Ordinary least squares results for H2. Dependent variable is time spent on article. Additional respondent characteristics include measures of respondent political knowledge, political interest, education, income, general trust, race, perceived prevalence of government corruption, and whether respondent is registered to vote.

	(1) Unlogged	(2) Unlogged	(3) Logged	(4) Logged
(Intercept)	40.85^{***}	32.29^{**}	3.51^{***}	3.28^{***}
	(2.57)	(10.32)	(0.05)	(0.22)
Party Label Pol.	1.59	1.45	0.04	0.04
	(3.16)	(3.23)	(0.07)	(0.07)
Fox		−0.76		−0.01
Additional Resp. Chars.	*N*	*Y*	*N*	*Y*
Num. obs.	397	397	397	397

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

significance (p-value < 0.10) in models (3) and (6). This could be due to the sample size, which is roughly a third as large as those used in past published work (Bolsen, Druckman, and Cook 2014; Petersen et al. 2013). There is no significant difference in processing time between those viewing an article about a politician from the opposite party and those viewing an unlabeled politician. Thus the effect appears to be a copartisan one, rather than being the result of aversion to the other party.

Substantively, respondents who view an article about a politician without a party label or from the opposite party are expected to spend about 10.54 seconds viewing it; in contrast those who view an article about a politician from their own party are expected to spend about 17.88 seconds viewing it. The expected difference in viewing time is thus about 7.33 seconds (95% CI:-0.25, 14.90).[20] While this may appear to be a small change in processing time, changes of even milliseconds in processing time could indicate different implicit cognitive processes (Bolsen, Druckman, and Cook 2014; Chugh 2004; Greenwald et al. 2009). Furthermore, respondents are MTurk workers who have every incentive to finish the survey as quickly as possible, and who know that they are viewing a hypothetical article. To the

[20]Expected values and first differences are calculated based on model (3) with a Republican respondents and all other variables set to their means using Zelig.

Table 3.6: Ordinary least squares results for H2. Dependent variable is time spent on article. Additional respondent characteristics include measures of respondent political knowledge, political interest, education, income, general trust, race, perceived prevalence of government corruption, and whether respondent is registered to vote.

	(1) Unlogged	(2) Unlogged	(3) Unlogged	(4) Logged	(5) Logged	(6) Logged
(Intercept)	41.15^{***}	31.93^{**}	31.10^{**}	3.52^{***}	3.27^{***}	3.25^{***}
	(3.00)	(10.47)	(10.37)	(0.06)	(0.22)	(0.22)
Same Party Pol.	4.79	5.13	$6.26^{\cdot}$	0.10	0.10	$0.12^{\cdot}$
	(3.66)	(3.72)	(3.25)	(0.08)	(0.08)	(0.07)
Opposite Party Pol.	−1.54	−2.33		−0.02	−0.03	
	(3.65)	(3.76)		(0.08)	(0.08)	
Dem. Respondent	−0.58	−0.15	−0.27	−0.02	0.01	0.00
	(3.00)	(3.15)	(3.14)	(0.06)	(0.07)	(0.07)
Fox		−1.20	−0.98		−0.01	−0.01
		(3.10)	(3.08)		(0.06)	(0.06)
Additional Resp. Chars.	*N*	*Y*	*Y*	*N*	*Y*	*Y*
Num. obs.	397	397	397	397	397	397

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

extent that motivated directional reasoning increases with an individual's engagement with a topic (Strickland, Taber, and Lodge 2011), these results provide a conservative estimate of the motivated reasoning that likely characterizes real political processes.

These results hold in a number of different specifications (see Appendix A). For example, one might be concerned that these models do not control for differences in processing time due to internet speed or unmeasured individual characteristics. Because of randomization, this should not affect the causal estimate, but I nonetheless rerun the models controlling for time spent taking the survey (subtracting the time spent on the article) and find substantively similar results ($p < 0.10$ on coefficients of *Same Party Pol.* when compared to unlabeled or other party politicians). I also find similar results following Bolsen, Druckman, and Cook (2014) by analyzing processing time using a cox proportional hazard model.

One potential alternative explanation that these results cannot address is that partisans are simply more interested in their own parties and thus spend more time reading about them. Future work could attempt to distinguish this mechanism from the motivated reasoning one posited here by comparing processing time when partisans encounter positive or

negative reports about their copartisans.

Hypothesis 3

Are allegations that come from media sources that are ideologically similar to each politician viewed as more credible? Results presented in Table 1.7 provide evidence in support of Hypothesis 3. The key variable of interest is *Counterstereotypical* which takes a value of 1 when a Democrat (Republican) is accused by MSNBC (Fox) and 0 otherwise. I include independents in these regressions because this hypothesis is concerned with the relationship between the media source and politician, rather than the respondent's partisanship. As predicted, the coefficient on *Counterstereotypical* is positive with p-value less than 0.10 across all specifications.[21]

Table 3.7: Ordinary least squares results for H3. Dependent variables are agreement with the statements that Rep. Martin is corrupt and that the allegations against him are probably true (1-5 Scale).Additional respondent characteristics include measures of respondent political knowledge, political interest, education, income, general trust, race, perceived prevalence of government corruption, and whether respondent is registered to vote.

	(1) Corrupt	(2) Corrupt	(3) True	(4) True
(Intercept)	3.61^{***}	3.44^{***}	3.69^{***}	3.35^{***}
	(0.07)	(0.21)	(0.07)	(0.22)
Democrat Pol.	-0.14	$-0.16^{\cdot}$	-0.25^{*}	-0.28^{**}
	(0.09)	(0.09)	(0.10)	(0.10)
Republican Pol.	-0.10	-0.10	-0.13	$-0.15^{\cdot}$
	(0.09)	(0.09)	(0.10)	(0.09)
Fox	0.00	0.02	-0.03	-0.03
	(0.07)	(0.06)	(0.07)	(0.07)
Counterstereotypical	$0.14^{\cdot}$	$0.14^{\cdot}$	$0.17^{\cdot}$	0.18^{*}
	(0.08)	(0.08)	(0.09)	(0.09)
Additional Resp. Chars.	*N*	*Y*	*N*	*Y*
Num. obs.	557	557	557	557

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

Figure 1.3 plots the simulated difference in perceptions of a Republican politician facing allegations reported by Fox rather than MSNBC with an independent respondent. Although

[21] Although the coefficients on *Counterstereotypical* are always positive when using an ordered logit, the p-values are less than 0.10 only for the "allegations are true" questions.

both point estimates are positive, the estimated effects are not as large as those of copartisanship.

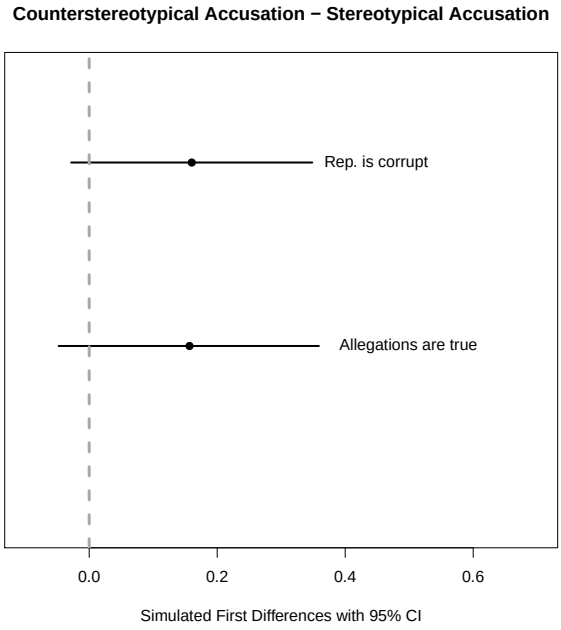

Figure 3.3: Simulated first differences in predicted response when viewing accusation from a media source that is ideologically similar to politician versus those from a media source that is ideologically dissimilar to politician.

Hypothesis 4

The evidence presented thus far has shown that both copartisanship and the costliness of an accusation influence how respondents evaluate allegations against politicians. Table 1.8 presents results for hypothesis 4, which predicted that copartisanship would be more influential than the costliness of an allegation.

For ease of interpretation of this triple interaction, Figure 1.4 presents the predicted difference when moving from a (counter)stereotypical allegation against a politician of the opposite party to a (counter)stereotypical allegation against a politician of the same party. Panels I and II of Figure 1.4 show that when copartisan politicians face stereotypical allegations, respondents are significantly less likely to think copartisans (rather than opposite party members) are corrupt or that the allegations against them are true. On the other

Table 3.8: Ordinary least squares results for H4. Dependent variables are agreement with the statements that Rep. Martin is corrupt and that the allegations against him are probably true (1-5 Scale). Additional respondent characteristics include measures of respondent political knowledge, political interest, education, income, general trust, race, perceived prevalence of government corruption, and whether respondent is registered to vote.

	(1) Corrupt	(2) Corrupt	(3) True	(4) True
(Intercept)	3.54^{***}	3.39^{***}	3.60^{***}	3.47^{***}
	(0.09)	(0.26)	(0.09)	(0.28)
Same Party Pol.	-0.33^{**}	-0.33^{**}	-0.34^{**}	-0.35^{**}
	(0.11)	(0.11)	(0.12)	(0.12)
Opposite Party Pol.	-0.09	-0.09	-0.10	-0.09
	(0.12)	(0.11)	(0.12)	(0.12)
Dem. Respondent	$0.15^{\cdot}$	$0.15^{\cdot}$	0.23^{**}	0.22^{*}
	(0.08)	(0.08)	(0.08)	(0.08)
Fox	0.02	0.02	-0.03	-0.02
	(0.08)	(0.08)	(0.08)	(0.08)
Counterstereotypical	0.15	0.12	0.20	0.16
	(0.13)	(0.13)	(0.14)	(0.14)
*Same Party Pol. * Counterstereotypical*	0.02	0.02	-0.02	0.01
	(0.19)	(0.19)	(0.20)	(0.20)
Additional Resp. Chars.	*N*	*Y*	*N*	*Y*
Num. obs.	403	403	403	403

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

hand, Panels III and IV show that – contrary to Hypothesis 4 – when copartisans face counterstereotypical accusations, there are no significant differences in the assessments of the guilt of copartisan politicians versus opposite party politicians. While the point estimates are still negative, the differences are not statistically significant.

Perhaps with a sufficiently large sample size, the bias in favor of copartisan politicians would be statistically distinguishable. However, it is also possible that these findings show the limits of directional motivated reasoning. When partisans select into consuming ideologically similar news sources, any corruption allegations they view are either stereotypical and directed against politicians from the other political party, or counterstereotypical and directed against copartisans. Given that the latter type of allegations minimize copartisan

bias, it appears particularly important for democratic accountability that media sources with perceived ideological biases do report wrongdoing "against type."

3.6 Conclusion

This chapter demonstrates that partisanship may make democratic accountability more challenging, even within a well-functioning democracy with free media. Consistent with partisanship's strong influence in virtually all realms of American politics, this chapter provides evidence that citizens are less likely to believe copartisan politicians are corrupt or that allegations of copartisan wrongdoing are true. Furthermore, contrary to much scholarship in political science, voters do not just employ party labels as heuristics. Instead, it appears that partisans are motivated to give members of their in-group the benefit of the doubt. Additionally, this chapter shows that even within a free media landscape, allegations of corruption are not equally credible. Instead, when media sources with a perceived ideological bias make allegations against politicians from the ideologically close party, those allegations are viewed as particularly credible. Simulating the effects of these two variables – copartisanship and counterstereotypical accusations – demonstrates the limits of motivated reasoning. Partisans are not unshakeable party cheerleaders. When accusations against copartisans are counterstereotypical, there is no statistically significant difference between evaluations of copartisan and other party politicians. This finding highlights an important role that media sources with ideological positions play in influencing democratic accountability among their base.

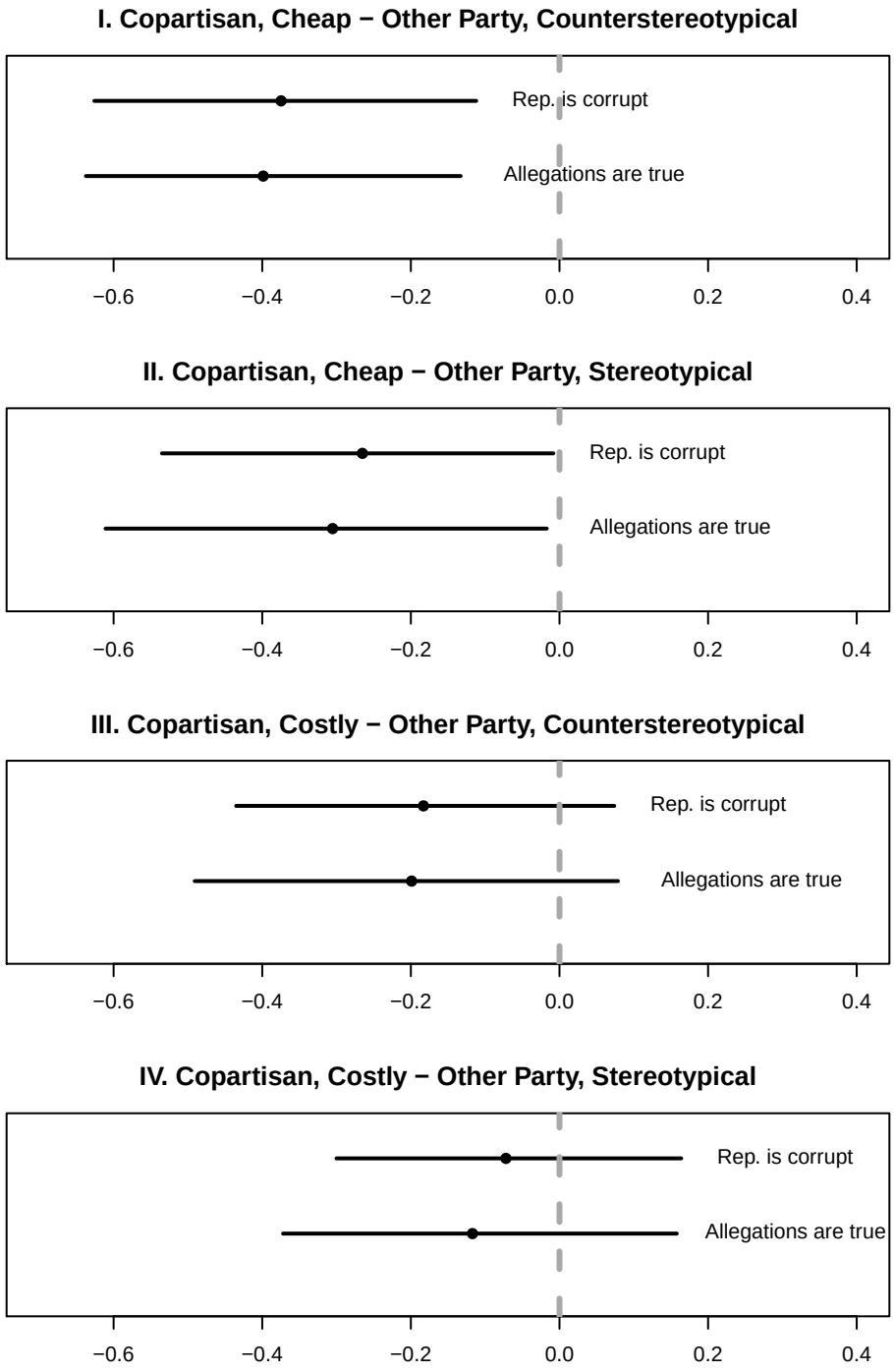

Figure 3.4: Simulated first differences in predicted response based on same/other party and (counter)stereotypical accusations

A | Appendix to Chapter 1

A.1 Fixed Effects Analyses: Control of Corruption

Drawing on a larger dataset, we fail to replicate Persson et al. (2003)'s findings using multiple fixed effects specifications. Data is taken from the Quality of Governance (QoG) Standard Dataset (April 6, 2011). In the models below, *MAGN* is the average district magnitude in the "House" or lower electoral tier (*dpi_mdmh*) and *MAJ* is a dummy variable indicating whether all seats in the lower tier are elected by plurality or majority rule (constructed from *dpi_housesys*). We exclude non-democracies – defined here as countries which score less than four in a combined Freedom House and Polity scale from zero to ten (QoG variable *fh_ipolity2*) – from the first two sets of analyses. In specifications with time-varying covariates, we follow Persson et al. (2003) and include controls for logged per capita GDP (*wdi_gdpc*), quality of democracy (*fh_ipolity2*) and openness to trade (*pwt_openk*).

Our first set of fixed effects specifications closely follow those presented in Table 4 of Persson et al. (2003). To construct time-varying measures of the proportion of legislators elected by party lists, we use the proportion of lower-house seats elected from multi- and single-member districts (*jw_propmmd* and *jw_propsmd*) and the ballot structure used in those elections (*jw_mmdballot* and *jw_smdballot*) from Johnson and Wallack (2006). As in Persson et al. (2003), our measure *PINDP* reflects the proportion of legislators elected on an individual ballot by plurality rule. *PINDO* also includes legislators elected by open party lists. Persson et al. (2003) expects these measures to be negatively correlated with corruption. Included are data from 92 countries between the years 1996 and 2005, the final year for which the independent variables can be constructed.

Table A.1: Replication of Persson et al. (2003) Fixed Effects Analysis with Additional Data

	(1)	(2)	(3)	(4)	(5)	(6)
PINDP	0.05	0.28	−0.03			
	(0.21)	(0.21)	(0.14)			
MAJ 1	-0.17^{*}	-0.18^{*}		-0.17^{**}	$-0.11^{\cdot}$	
	(0.09)	(0.09)		(0.06)	(0.06)	
MAGN	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
PINDO				0.07	$0.11^{\cdot}$	0.09
				(0.07)	(0.06)	(0.06)
Intercept	-0.59^{***}	-6.30^{***}	-6.45^{***}	-0.61^{***}	-6.32^{***}	-6.63^{***}
	(0.12)	(0.81)	(0.81)	(0.09)	(0.81)	(0.79)
Country Fixed Effects	Y	Y	Y	Y	Y	Y
Year Fixed Effects	N	Y	Y	N	Y	Y
Covariates	N	Y	Y	N	Y	Y
Observations	843	816	816	843	816	816

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.1$. Covariates are logged per capita GDP, openness to trade, and a measure of democratic quality.

Table A.1 presents results from this specification, which contrast strongly with those in Persson et al. (2003). We find no statistically significant effects of *PINDP*; *PINDO* reaches statistical significance (p-value=0.08) in the expected direction in only one of three specifications. In contrast, the dummy for plurality rules *MAJ* is significantly associated with *higher* levels of corruption. District magnitude has no estimated effect.

Second, we present results using categorical measures of electoral systems that more closely match those in our main analyses. The specifications without covariates in Columns (1) and (3) contain data from 115 countries from 1996 through 2009; the other specifications with controls contain data from 1996 through 2007. Columns (1) and (2) of Table A.2 present results from specifications in which electoral systems are treated as an ordered categorical variable (plurality, mixed, open list PR, closed list PR, with plurality as the excluded baseline category). It shows that closed list PR systems have less corruption than plurality systems. This difference is statistically significant in both specifications. Columns (3) and (4) compare plurality systems to all others; plurality systems are associated with

significantly more corruption in one of these specifications.

Table A.2: Categorical Electoral Systems Fixed Effects Analysis

	(1)	(2)	(3)	(4)
Mixed	0.02	0.04		
	(0.05)	(0.05)		
OLPR	−0.09	0.16		
	(0.08)	(0.10)		
CLPR	0.11^{*}	0.20^{**}		
	(0.05)	(0.06)		
MAJ			−0.02	-0.12^{**}
			(0.04)	(0.04)
MAGN	0.00	$0.0003^{\dagger}$	0.00	0.00
	(0.00)	(0.0001)	(0.00)	(0.00)
Intercept	-0.69^{***}	-5.46^{***}	-0.65^{***}	-5.08^{***}
	(0.07)	(0.63)	(0.06)	(0.62)
Country Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	N	Y	N	Y
Covariates	N	Y	N	Y
Observations	1326	1086	1326	1086

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.1$. Covariates are logged per capita GDP, openness to trade, and a measure of democratic quality.

Finally, we present results from the 35 countries that are included in our main analysis, either as treated countries (those that transitioned from plurality or mixed to closed list PR electoral systems) or those included as part of synthetic controls. In the models without covariates, the data covers 1996 through 2012. With controls, the data only go through 2007. Columns (1) and (2) of Table A.3 show that closed list PR systems are associated with significantly less corruption than plurality systems within this restricted sample; mixed systems are also associated with less corruption though this result is only significant in one specification. Columns (3) and (4) demonstrate that plurality systems are associated with

significantly more corruption than mixed and closed list PR systems within this smaller sample.

Table A.3: Categorical Electoral Systems, Restricted Sample

	(1)	(2)	(3)	(4)
Mixed	0.05	$0.16^{\dagger}$		
	(0.09)	(0.09)		
CLPR	0.14^{*}	0.30^{***}		
	(0.07)	(0.09)		
MAJ			-0.15^{**}	-0.15^{**}
			(0.05)	(0.05)
MAGN	0.00	0.0003^{**}	0.00	0.00^{*}
	(0.00)	(0.0001)	(0.00)	(0.00)
Intercept	-0.67^{***}	-5.32^{***}	-0.47^{***}	-4.69^{***}
	(0.11)	(0.64)	(0.08)	(0.66)
Country Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	N	Y	N	Y
Covariates	N	Y	N	Y
Observations	585	408	585	408

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.1$. Covariates are logged per capita GDP, openness to trade, and a measure of democratic quality.

A.2 Transparency International's Corruption Perception Index (CPI)

As a robustness check, we conduct both non-parametric and fixed effects analyses using Transparency International's Corruption Perceptions Index (CPI) and again find little evidence to support Persson et al. (2003)'s hypotheses. As noted in the paper, we prefer the CCE analysis because (1) CPI is constructed as a relative measure not designed to be compared across years and (2) CPI includes fewer relevant country years. Specifically, we lack sufficient data to perform the synthetic control and diff-in-diff analysis on three of the countries which transitioned from mixed systems and have fewer observations across all fixed effects specifications.

A.2.1 Non-parametric Analyses

Table A.4 shows the countries that changed from plurality to closed list PR systems and weights that comprise their synthetic controls. For Togo, only two years of pre-treatment CPI was available. While Kazakhstan and Mongolia were matched with Tajikistan when using CCE, they are matched up with Azerbaijan with CPI. The synthetic controls for Kyrgyzstan and Togo comprise the same countries as with CCE, but with slightly different weights.

Table A.4: Synthetic Controls using Transparency Internationals' Corruption Perceptions Index (CPI) for countries that changed from plurality to closed list PR

Treated Unit	Kazakhstan		Kyrgyzstan		Mongolia		Togo (2 years only)	
Synthetic Control	0.999	Azerbaijan	0.279	Azerbaijan	1	Azerbaijan	0.019	Botswana
	0.001	Tajikistan	0.261	Tajikistan			0.392	Cote d'Ivoire
			0.236	Turkmenistan			0.045	Gabon
			0.225	Uzbekistan			0.038	Gambia
							0.066	Ghana
							0.089	Kenya
							0.049	Malawi
							0.023	Mali
							0.051	Mauritius
							0.051	Uganda
							0.052	Zambia
							0.126	Zimbabwe

Table A.5 similarly presents synthetic control countries and weights for the countries that moved from mixed systems to closed list PR. Only Niger, Russia, and Ukraine have sufficient CPI coverage to perform this analysis.

Treated Unit	Niger		Russia		Ukraine	
Synthetic Control	1	Madagascar	0.247	Albania	0.992	Albania
			0.141	Armenia	0.008	Croatia
			0.487	Croatia		
			0.092	Georgia		
			0.033	Lithuania		

Table A.5: Synthetic Controls using CPI for countries that changed from mixed systems to closed list PR

Transparency International's CPI is an index from 0-10 where higher values are associated with lower perceived corruption. Table A.6 shows that balance on the pre-treatment values of CPI improves for each of these countries.

Table A.6: Average Pre-Treatment Transparency International CPI Scores for Countries that Adopted Closed List PR

Treated Unit	Average Pre-Treatment CPI:		
	Treated Unit	Synthetic Control	Regional Sample
Kazakhstan	2.38	2.15	2.04
Kyrgyzstan	2.03	2.03	2.02
Mongolia	2.88	2.17	2.01
Togo	2.47	2.47	3.08
Niger	2.38	3.05	3.67
Russia	2.60	2.60	3.19
Ukraine	2.58	2.60	3.31

Table A.7 shows the estimated difference in average outcomes between each country that adopted closed list PR and its synthetic control. It also shows the diff-in-diff estimate, which adjusts for the pre-treatment differences between the treated unit and the synthetic control. Among the countries that transitioned from plurality systems, only Togo has a lower average CPI than its synthetic control in the Synth estimate. Mongolia also has a lower average CPI than its synthetic control, after adjusting for pre-treatment differences. Of the countries that transitioned from mixed systems, only Niger, Russia, and Ukraine have sufficient data

coverage to perform the analysis. In the Synth estimate, all three have worse CPI scores than their synthetic controls, although the estimated effect for Niger switches signs once differences in pre-treatment values of the CPI are taken into account.

Table A.7: Synth and Diff-in-Diff Estimates using Transparency International's CPI

Country	Synth Est.	Diff-in-Diff Est.	Post-Treatment Years
Kazakhstan	0.41	0.18	3
Kyrgyzstan	0.03	0.03	3
Mongolia	0.30	-0.40	2
Togo	-0.11	-0.11	3
Niger	-0.30	0.37	4
Russia	-1.37	-1.37	5
Ukraine	-0.89	-0.87	4

The mean and median values for the Synth estimate are -0.28 and -0.11 respectively, similar to those for the diff-in-diff estimates (-0.31 and -0.11). These are small differences on a 0-10 scale, but they are in the direction of the Persson et al. (2003) hypothesis. Using the Wilcoxon signed rank statistic as in the paper provides p-values of 0.58 and 0.38 for the synth and diff-in-diff estimates respectively. If we believe that transitions from plurality systems should be more heavily weighted, the evidence in favor of Persson et al. (2003) appears weaker. For any dose such that transitions from plurality systems are weighted at least as heavily as those from mixed systems, the p-values are never lower than those reported, and reach maxima of 1 and .68 for the synth and diff-in-diff estimates respectively.

A.2.2 Fixed Effects Analyses

We also run the fixed effects specifications using the CPI as the dependent variable rather than CCE. All variables are constructed as described in Section A.1. In Table A.8, we attempt to replicate results from Table 4 in Persson et al. (2003) using data from 89 countries from 1996 through 2005. We again fail to replicate most results from Persson et al. (2003) finding no statistically significant effect of the proportion of legislators elected by plurality rule ($PINDP$) or of the dummy for plurality rules (MAJ). Unlike in Table A.1, the proportion elected by plurality or open list PR ($PINDO$) is a statistically significantly associated with lower levels of corruption in all three specifications as Persson et al. (2003) expects.

In Table A.9 we present results using a categorical measure of electoral systems and data from 114 countries from 1996 through 2009 in models without covariates and from 106 countries from 1996 through 2007 in models with covariates. Columns (1) and (3) of Table A.9 conform with expectations from Persson et al. (2003) with open and closed list PR systems associated with higher levels of corruption than plurality systems and majoritarian systems associated with lower levels. However, these associations become insignificant once we include covariates and year fixed effects (Columns (2) and (4)).

Finally, in Table A.10 we present results from the 27 countries used in the nonparametric

Table A.8: Persson et al. (2003) Fixed Effects Analysis with Additional Data, TI CPI

	(1)	(2)	(3)	(4)	(5)	(6)
PINDP	0.53	1.06	0.01			
	(0.88)	(0.93)	(0.53)			
MAJ	−0.43	−0.57		−0.23	−0.16	
	(0.40)	(0.42)		(0.23)	(0.24)	
MAGN	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
PINDO				0.49^{**}	0.49^{**}	0.49^{**}
				(0.17)	(0.17)	(0.17)
Intercept	2.48^{***}	0.90	0.57	2.31^{***}	0.29	−0.22
	(0.42)	(2.72)	(2.72)	(0.31)	(2.72)	(2.61)
Country Fixed Effects	Y	Y	Y	Y	Y	Y
Year Fixed Effects	N	Y	Y	N	Y	Y
Covariates	N	Y	Y	N	Y	Y
Observations	641	630	630	641	630	630

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.1$. Covariates are logged per capita GDP, openness to trade, and a measure of democratic quality.

analysis in Section A.2.1 either as treated countries or as parts of synthetic controls. Models without controls contain data from 1996 through 2012 while those with controls include data through 2007. These results show no significant associations between electoral systems and corruption.

Table A.9: Categorical Electoral Systems Fixed Effects Analysis, TI CPI

	(1)	(2)	(3)	(4)
Mixed	−0.13	−0.04		
	(0.21)	(0.24)		
OLPR	-1.03^{***}	−0.57		
	(0.31)	(0.39)		
CLPR	$-0.36^{\cdot}$	−0.29		
	(0.21)	(0.27)		
MAJ			$0.20^{\dagger}$	0.10
			(0.11)	(0.14)
MAGN	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Intercept	2.82^{***}	−2.15	2.50^{***}	−2.95
	(0.25)	(2.05)	(0.18)	(2.03)
Country Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	N	Y	N	Y
Covariates	N	Y	N	Y
Observations	1070	853	1070	853

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.1$. Covariates are logged per capita GDP, openness to trade, and a measure of democratic quality.

Table A.10: Categorical Electoral Systems Analysis, Restricted Sample TI CPI

	(1)	(2)	(3)	(4)
Mixed	0.13	0.15		
	(0.19)	(0.28)		
CLPR	0.00	0.10		
	(0.14)	(0.34)		
MAJ			−0.05	−0.14
			(0.14)	(0.22)
MAGN	0.00^{*}	0.00	0.00^{*}	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Intercept	2.69^{***}	-10.19^{***}	2.87^{***}	-9.78^{***}
	(0.22)	(1.68)	(0.18)	(1.69)
Country Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	N	Y	N	Y
Covariates	N	Y	N	Y
Observations	344	207	344	207

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.1$. Covariates are logged per capita GDP, openness to trade, and a measure of democratic quality.

B | Appendix to Chapter 2: Selected Ugandan Corruption Scandals

- **"Muyenga House Scandal," Publicized in 2004**

  Details: Media reports accused Gilbert Bukenya (Vice President and MP from Busiro North) of causing the National Social Security Fund (NSSF) to lose sh90m by acquiring a house in Muyenga, Kampala with an improperly low bid. Bukenya sued the reporting newspaper (*The Weekly Observer*) and claimed that investigations were the result of a political plot against him.

- **"Global Fund Scandal," Publicized in 2006**

  Details: Scandal over misappropriation of resources from The Global Fund to Fight Aids, Tuberculosis and Malaria with estimates of missing funds ranging from $10 million (sh25 billion) to $37 (sh95.8 billion). Media reports implicated Health Minister Jim Muhwezi, his then-deputies Mike Mukula and Alex Kamugisha, Director of Economic Affairs (President's Office) Teddy Cheeye (who was arrested October 2008) and Production Manager of Uganda Television, Fred Kavuma. It is alleged that the Project Management Unit displayed nepotism in recruiting and paid inflated salaries to its staff, who also received largely undocumented expense allowances (including for trips abroad). While Muhwezi was indicted in 2007, all charges against him were dropped in 2012.

- **"Temangalo Scandal," Publicized in 2008**

  Details: Reports that Security Minister Amama Mbabazi (Kinkizi City West) and

businessman Amos Nzeyi were paid sh11 billion by the NSSF for 414 acres of land, with each acre going for sh24 million, whereas independent estimates priced acres in that area at between sh14 million and sh18 million. The parliamentary committee investigating the matter concluded that Mbabazi and Finance Minister Ezra Suruma (who was accused of pressuring the NSSF managing director to authorize the sale) had violated sections of the Leadership Code.

- **"Commonwealth Heads of Government Meeting (CHOGM) Scandal," Publicized in 2011**

  Details: Scandal over the mismanagement of approximately sh500 billion of public funds meant for the 2007 CHOGM summit. Media reports implicated Vice-President Gilbert Bukenya, Prime Minister Amama Mbabazi and Minister for Works and Transport John Byabagambi (Ibanda County South), as well as John Nasasira (Kazo County), Mwesigwa Rukutana (Rushenyi County) and Isaac Musumba (Buzaya County). All charges were dropped against Bukenya (who was once seen as top contender to replace Museveni) in November 2011.

- **"ID Scandal," Publicized in 2011**

  Details: National ID cards which were supposed to be used for the 2011 elections were never delivered. The Government borrowed over sh150 billion to finance the project and awarded the contract to the German company Muhlbauer High Tech without following required procurement procedures. Media reports implicated General Duties Minister Kiddu Makubuya (Katikamu South), Internal Affairs Minister Kirunda Kivejinja (Bugweri),Syda Bbumba (Nakaseke North) and the local government Permanent Secretary Stephen Kagoda.

- **"Bicycle Scandal," Publicized in 2011**

  Details: Amman Industrial Tool and Equipment Ltd was contracted by the Ministry of Local Government to supply 70,000 bicycles meant for local councils after an im-

proper procurement process. No bicycles were delivered. The company was reportedly paid $1.7 million (about sh4 billion). Media reports implicated Permanent Secretary for Local Government Muhanguzi Kashaka and various staff members. Kashaka was convicted by the Anti Corruption Court in 2014 and sentenced to ten years in prison.

- **"Microfinance and Specioza Kazibwe," Publicized in 2011**

  Details: An estimated sh60 billion disappeared from the Microfinance Support Centre (founded by President Museveni in 2003). Media reports implicated Former Vice-President Dr. Specioza Wandira Kazibwe and Timothy Lwanga (Kyamuswa County). At the request of the Inspector General of Government, board members and the acting director were suspended to investigate allegations of mismanagement.

- **"Hassan Basajjabalaba compensation scandal," Publicized in 2011**

  Details: Businessperson (and NRM-supporter) Hassan Basajjabalaba was paid sh169 billion as compensation for the loss of business for the city markets. This payment was subject to many allegations of impropriety, including accusations that the original contracts were invalid, that the payment was overinflated, and that there was improper political influence in the eventual payments. Media reports implicated cabinet members Syda Bbumba (Nakaseke North) and Khiddu Makubuya (Katikamu South), and staff. In the wake of the scandal, Bbumba and Makubuya resigned their cabinet positions.

- **"Pensions scandal," Publicized in 2012**

  Details: Sh169 billion meant for pensions of former East African Community workers is said to have been transferred to the private account of the East African Community Beneficiaries Association general secretary. Media reports implicated East African Community Beneficiaries Association National Secretary Peter Ssajjabi, Permanent Secretary of Public Service Jimmy Lwamafa, Director Research and Development (Public Service) Stephen Kiwanuka Kunsa, Principal Accountant (Public Ser-

vice) Christopher Obey and other staff. The named individuals are currently being tried by the Anti Corruption Court.

- **“Prime Minister’s Office," Publicized in 2012**

  Details: Funds allocated for the Peace Recovery and Development Plan for Northern Uganda of sh5 billion allegedly were transferred to an improper account and embezzled. Following the development, the UK, Denmark and Ireland froze development aid to the Office of the Prime Minister. Media reports implicated Prime Minister Amama Mbabazi and staff. At the time of writing, the chief accountant Geoffrey Kazinda is being prosecuted in the Anti Corruption Court.

- **“Mukono Road Scandal," Publicized in 2014**

  Details: The Uganda National Roads Authority awarded a contract to upgrade a road from Mukono to Katosi to a non-existent construction company, which was accused of misappropriating a portion of the funds for the work. Minister for Works and Transportation Abraham Byandala (Katikamu North) was implicated in authorizing the contract without proper due diligence. He contends that the scandal is the result of machinations against him by others who would like his position as minister.

Main Sources: *New Vision* (2012), *Global Integrity* (2011), and the Office of the Inspectorate of Government. A full list of articles consulted is available from the author by request.

C | Appendix to Chapter 3

C.1 Ordered Logistic Regression Results

Table C.1: Ordered logistic regression results for H1. Dependent variables are agreement with the statements that Rep. Martin is corrupt and that the allegations against him are probably true (1-5 Scale). Additional respondent characteristics include measures of respondent political knowledge, political interest, education, income, general trust, race, perceived prevalence of government corruption, and whether respondent is registered to vote.

	(1) Corrupt	(2) Corrupt	(3) True	(4) True
Same Party Pol.	-0.63^{**}	-0.70^{**}	-0.61^{**}	-0.69^{**}
	(0.23)	(0.24)	(0.23)	(0.24)
Opposite Party Pol.	-0.07	-0.10	-0.05	-0.08
	(0.23)	(0.24)	(0.23)	(0.24)
Dem. Respondent	$0.33^{\cdot}$	$0.35^{\cdot}$	0.46^{*}	0.48^{*}
	(0.19)	(0.20)	(0.19)	(0.20)
Fox		0.10		0.01
		(0.20)		(0.20)
Additional Resp. Chars.	*N*	*Y*	*N*	*Y*
Num. obs.	403	403	403	403

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

Table C.2: Ordered logit results for H3. Dependent variables are agreement with the statements that Rep. Martin is corrupt and that the allegations against him are probably true (1-5 Scale). Additional respondent characteristics include measures of respondent political knowledge, political interest, education, income, general trust, race, perceived prevalence of government corruption, and whether respondent is registered to vote.

	(1) Corrupt	(2) Corrupt	(3) True	(4) True
Democrat Pol.	-0.31	-0.37	-0.54^{*}	-0.64^{**}
	(0.23)	(0.23)	(0.22)	(0.23)
Republican Pol.	-0.21	-0.24	-0.28	-0.36
	(0.21)	(0.22)	(0.21)	(0.22)
Fox	0.02	0.03	-0.05	-0.05
	(0.16)	(0.17)	(0.16)	(0.16)
Counterstereotypical	0.30	0.30	$0.36^{\cdot}$	0.40^{*}
	(0.20)	(0.21)	(0.19)	(0.20)
Additional Resp. Chars.	*N*	*Y*	*N*	*Y*
Num. obs.	557	557	557	557

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

Table C.3: Ordered logit results for H4. Dependent variables are agreement with the statements that Rep. Martin is corrupt and that the allegations against him are probably true (1-5 Scale). Additional respondent characteristics include measures of respondent political knowledge, political interest, education, income, general trust, race, perceived prevalence of government corruption, and whether respondent is registered to vote.

	(1) Corrupt	(2) Corrupt	(3) True	(4) True
Same Party Pol.	-0.79^{**}	-0.82^{**}	-0.80^{**}	-0.84^{**}
	(0.29)	(0.30)	(0.28)	(0.28)
Opposite Party Pol.	-0.24	-0.23	-0.30	-0.23
	(0.29)	(0.29)	(0.29)	(0.29)
Dem. Respondent	$0.36^{\cdot}$	$0.37^{\cdot}$	0.51^{**}	0.50^{*}
	(0.19)	(0.20)	(0.19)	(0.20)
Fox	0.09	0.09	-0.03	-0.02
	(0.19)	(0.20)	(0.19)	(0.20)
Counterstereotypical	0.36	0.26	0.46	0.30
	(0.33)	(0.34)	(0.32)	(0.34)
*Same Party Pol. * Counterstereotypical*	-0.04	-0.04	-0.07	0.02
	(0.46)	(0.48)	(0.45)	(0.47)
Additional Resp. Chars.	*N*	*Y*	*N*	*Y*
Num. obs.	403	403	403	403

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

C.2 Robustness Checks for Hypothesis 2

Table C.4: Cox proportional hazard results for H2. Coefficients represent a hazard rate, so higher coefficients indicate quicker processing times. Dependent variable is time spent on article. Additional respondent characteristics include measures of respondent political knowledge, political interest, education, income, general trust, race, perceived prevalence of government corruption, and whether respondent is registered to vote.

	(1) Unlogged	(2) Unlogged	(3) Unlogged	(4) Logged	(5) Logged	(6) Logged
Same Party Pol.	−0.15	−0.17	−0.20$^{\cdot}$	18.98	−0.17	−0.20$^{\cdot}$
	(0.12)	(0.13)	(0.11)	(13.37)	(0.13)	(0.11)
Opposite Label Pol.	0.03	0.06		6.64	0.06	
	(0.12)	(0.13)		(13.38)	(0.13)	
Dem. Respondent	0.00	0.00	0.00	6.71	0.00	0.00
	(0.10)	(0.11)	(0.11)	(10.98)	(0.11)	(0.11)
Fox		0.08	0.08		0.08	0.08
Additional Resp. Chars.	*N*	*Y*	*Y*	*N*	*Y*	*Y*
Num. obs.	397	397	397	397	397	397

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

Table C.5: Ordinary least squares results for H2. Dependent variable is time spent on article. All models control for total duration of survey excluding time spent on article. Additional respondent characteristics include measures of respondent political knowledge, political interest, education, income, general trust, race, perceived prevalence of government corruption, and whether respondent is registered to vote.

	(1) Unlogged	(2) Unlogged	(3) Unlogged	(4) Logged	(5) Logged	(6) Logged
(Intercept)	37.52^{***}	29.37^{**}	28.55^{**}	3.44^{***}	3.21^{***}	3.20^{***}
	(3.20)	(10.41)	(10.32)	(0.07)	(0.22)	(0.22)
Same Party Pol.	4.75	5.03	$6.14^{\cdot}$	0.10	0.10	$0.11^{\cdot}$
	(3.62)	(3.69)	(3.22)	(0.08)	(0.08)	(0.07)
Opposite Label Pol.	−1.55	−2.30		−0.02	−0.03	
	(3.61)	(3.72)		(0.08)	(0.08)	
Dem. Respondent	−0.34	0.12	0.01	−0.01	0.01	0.01
	(2.97)	(3.13)	(3.12)	(0.06)	(0.07)	(0.07)
Total Duration	0.02^{**}	0.01^{**}	0.01^{**}	0.00^{**}	0.00^{**}	0.00^{**}
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
Fox		−1.21	−0.99		−0.01	−0.01
Additional Resp. Chars.	*N*	*Y*	*Y*	*N*	*Y*	*Y*
Num. obs.	397	397	397	397	397	397

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

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