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Elections as instruments for punishing bad representatives and selecting good ones



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ABSTRACT

Many theories of democracy point out that voters make their choices based on two goals: the retrospective assessment of incumbents and the prospective choice between incumbents and challengers. Do voters react to malfeasance on the part of their elected representatives? If they abandon corrupt incumbents, are they able to select more virtuous replacements? In this paper, we assess the effects of corruption on voter loyalty and, conversely, of voter defection on subsequent malfeasance. We examine these relationships with data drawn from 169 elections across 72 countries. Our results show that malfeasance does indeed provoke voter defection, but that electoral volatility is not followed by lower levels of perceived corruption. We conclude by discussing the appropriate interpretation of our results, the future research they suggest, and their meaning for related, emerging literatures.

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1. Introduction

In *Federalist no. 57*, Madison wrote that “the aim of every political constitution is ... first to obtain for rulers men who possess most wisdom to discern, and most virtue to pursue, the common good of society, and in the next place, to take the most effectual precautions for keeping them virtuous whilst they continue to hold their public trust.” To put Madison’s aims into action, voters must be able to choose wise representatives (selection) and to provide them incentives to remain virtuous (assessment) after they have been elected (Manin et al., 1999a).

In another idealized account of the role of elections, Maravall (2007) explains, “[e]lections work like this. (1) Politicians compete, transmitting prospective messages

about their future policies and signals about their competence. (2) Voters select those candidates closer to their ideal policy positions and more able to implement their program. (3) Politicians, once in office, adopt policies and dedicate effort to carry them through. (4) Policies and effort, under particular exogenous conditions, produce outcomes that modify the welfare of citizens. (5) At the time of the next elections voters assess retrospectively such outcomes, and attribute them to policies and effort of the incumbent and to the influence of exogenous conditions. (6) Voters update their preferences about policies and candidates. (7) Voters re-elect or reject the incumbent. Elections, thus, both select and assess.”²

While Maravall’s theory stresses a policy dimension, it is not difficult to imagine “virtuousness” as a second

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² Both Madison and Maravall, as well as countless others in between, offered their idealized versions so that they could explore the ways in which the relationships they rested upon might fail to come to pass in real democracies. Below we will review some of the challenges involved in using elections as instruments of selection and/or assessment.

dimension critical to voters³ or as part of what he identifies as “competence.” In empirical terms, scholars have recently begun to focus on whether corruption – as opposed to economic performance or specific legislative outcomes – is a governmental output for which voters hold representatives electorally accountable (Chang et al., 2010; Peters and Welch, 1980; Tavits, 2007).

This depiction of elections as instruments for punishing bad representatives and selecting good ones is normatively appealing and has simple face validity, but both theoretical and empirical works have shown that the steps summarized by Maravall (2007) are fraught with challenges (as he himself discusses in detail). The prospective messages sent by politicians can be noisy or misleading; policy proximity may be one of multiple decision criteria used by voters; voters may lack, for a variety of reasons, knowledge about government outputs; it may not be clear whether or which incumbents are responsible for an outcome; acceptable replacements for underperforming representatives may not be available, etc. In this paper, we will summarize and evaluate some of the challenges facing voters when trying to use their vote choice to retrospectively assess and prospectively select legislators with respect to how corrupt they are perceived to be.

We begin by elaborating on the use of elections as instruments of retrospective assessment and prospective selection, noting several works that have made clear why these tasks may prove challenging if not impossible. As this literature makes clear, reasons abound for why punishing corrupt politicians and selecting virtuous ones may not be a straightforward process. Then, empirically, in order to determine whether elections can serve these dual purposes, we test for a reciprocal relationship between electoral volatility (as captured by the Pedersen Index of vote shares moving between parties) and political corruption (as captured by citizen responses to The Global Corruption Barometer).⁴ We estimate Vector Auto Regression (VAR) models designed to account for any reciprocal relationship between the two using data from 169 elections in 72 countries. Whereas the potential for reverse causality is something that typically frustrates scholars, we explicitly focus on this possibility both theoretically and empirically.

³ We discuss in greater detail below why voters might privilege some criteria over policy proximity in their vote choice decision.

⁴ Elected officials may be voted out of office for a variety of reasons. Voters may punish incumbents for exogenous shocks (Powell and Whitten, 1993; Anderson, 2000). A new policy dimension might be introduced, dividing groups in the electorate along new axes that result in a drop in support for incumbents (Chhibber and Torcal, 1997). Inexperienced voters after a recent (re)establishment of democratic rule may be uncertain of their options or their preferences over them (Bielasiak, 2002). Given the multitude of sources of vote volatility, in an online appendix we provide a model explaining current perceptions of corruption where we examine the impact vote volatility conditional on past perceptions of corruption so that we can sort out the effect of volatility that follows high levels of past corruption from volatility that follows from other factors. The substantive results of those models do not vary substantially from those reported in the paper. Also in the online appendix, we provide additional models where we explicitly control for other sources of volatility as gleaned from the existing literatures on party systems and on economic predictors of vote choice (in a VARX type model); our substantive findings remain unchanged.

We find that, as theories of retrospective democratic accountability would predict, where voters perceive politicians to be corrupt, they take their electoral support elsewhere, thereby increasing electoral volatility. However, contrary to the expectation of prospective selection (screening or mandate sending), the extent of electoral volatility does not reduce (perceptions of) corruption in the future. We conclude by discussing the appropriate interpretation of our results, future research to which they point, and their meaning for emerging literatures.

2. Elections as instruments of assessment and selection

With slightly different terminology, Manin et al., (1999b) offer a characterization of elections similar to the one from Maravall (2007). According to them, “[m]andates’ are particular kinds of signals that are emitted in elections: they constitute a choice among proposals ... offered by competing teams of politicians ... Once elected, the victorious politicians adopt policies. These policies become transformed into outcomes under the noise of conditions. As the electoral term ends, voters evaluate the outcomes and decide whether or not to retain the incumbent government” (p. 8). As with Maravall’s account, voters are both prospectively selecting among options while retrospectively assessing performance. Unfortunately, despite the importance of elections for the functioning of democracy, after some reasoning Manin et al., (1999a) conclude that “citizens’ control over politicians is at best highly imperfect in most democracies” (p. 50). Let us briefly summarize some of the reasons why it may be difficult for elections to serve as instruments of retrospective assessment or prospective selection, let alone both.

2.1. Challenges to retrospective assessment

One set of challenges to assessment are identified in the literature on *economic voting*, much of which relies at its heart on an understanding of voting as the practice of holding politicians accountable (Fiorina, 1981; Powell and Whitten, 1993; Kiewit, 2000; Sattler et al., 2008).⁵ Voters assess the state of the economy, for example, and make a decision about whether the incumbent government should be rewarded with reelection. These evaluations on the part of individual voters need not necessarily correspond to some underlying objective reality (Lewis-Beck and Stegmaier, 2000). However, debates remain about whether voters accommodate economic constraints imposed on policy makers when the economy is relatively open (Alcañiz and Hellwig, 2011; Hellwig and Samuels, 2007); whether assessments are based on the voters’ personal conditions or on general conditions (Lewis-Beck and Stegmaier, 2000); what balance should be struck between modeling voters as retrospective assessors versus prospective choosers (Alesina and Rosenthal, 1995; Duch and Stevenson, 2008); the clarity with which voters can assign responsibility for an

⁵ In an experimental setting, Woon and Anderson (2012) found that voters are much more likely to rely on retrospective assessment than on prospective selection, in part because retrospection is easier or less uncertain.

undesirable outcome (Tavits, 2007); and just how much information voters need (and have) about economic conditions (Lohmann, 1999; Anderson and O'Connor, 2000).

Indeed, Barro (1973) showed that, even with perfect information, electoral control is only partially effective as a mechanism for inducing the officeholder to advance the interests of his constituents. Several models of democracy since have pointed out that voters are even more disadvantaged than Barro suggests because of incomplete information – and particularly because of informational asymmetries with respect to elected officials (Ferejohn, 1986).⁶ The task of assessing imposes costs on voters, meaning that obtaining the complete information necessary to vote without mistakes is unlikely. If so, on occasion, good representatives are likely to get tossed out and bad ones are likely to be re-elected. For example, if an elected official knows what will satisfy a majority of voters, thus guaranteeing re-election, she can provide that level of effort or policy outcome even when more and better were possible. As long as politicians have an informational advantage in terms of what is satisfactory and what is possible, they can provide only what is satisfactory and keep the difference between possible and satisfactory as a rent (Banks and Sundaram, 1993).

Another problem plaguing the relationship between voters and elected officials is the possibility of time-inconsistent preferences. Voters may oppose policies that they end up eventually deeming to have been a smart decision. If politicians estimate that an election is going to occur between the adoption of the policy and its payoff, how should they behave? Should they implement the will of the people or should they do what is best for them? In the late 1980s and early 1990s, some presidential candidates in Latin America promised to deal with the economic downturns in their countries by providing citizens security through government intervention. Once in office, a subset of these candidates engaged in “mandate switching” by changing direction and implementing neoliberal austerity measures (Stokes, 2001). Assuming they cared about reelection, these presidents were gambling that they could produce results before the next election or that they could convince voters that, though the payoff had yet to result, it eventually would. Those who gambled correctly won re-election while those who did not went down to defeat (or impeachment).⁷

On a related note, the time horizons of politicians are key to the extent to which they worry about the

retrospective accounting that will occur at an upcoming election. If they place too little value in re-election, they have little incentive to deliver on their promises. Under such circumstances, we might expect sustained corruption, and perhaps voter disaffection. Based on a similar line of reasoning, in the study of term limits across U.S. state legislatures, there is empirical evidence to suggest that term limits fundamentally reshape sitting legislators' policy priorities (Gurwitt, 1996; Hansen, 1997) and that they also undermine the extent to which individual politicians are responsive to their constituencies (Besley and Case, 1995; Carey et al., 2006; Carey, 1994; Zupan, 1990). Ferraz and Finan (2011), for example, find a significant difference in corruption levels between municipalities where mayors are eligible for reelection and those where they are not. Like imposed limits on career length, it is possible that vote volatility, even in response to corrupt behavior, sends a perverse signal to elected officials that they should enrich themselves while the opportunity presents itself.⁸

2.2. Challenges to prospective selection

“If honesty and competence are at stake, we should expect politician quality to be what political scientists call a valence issue – every citizen wants more of it regardless of the policy implemented” (Besley, 2005, 47–48). Unfortunately, prospective selection of good representatives is not without its own set of challenges.⁹

First, incentives must be in place to entice virtuous citizens to run. Assuming that politicians are interested in implementing their preferred policies, competent individuals will only run where they are given the leeway or authority to enact their will. Unfortunately, that same leeway may provide, for example, the opportunity to represent special interests or to use the office for “self-dealing” (Besley, 2005, 43). Voters may see that competence and policy proximity do not line up. When multiple dimensions figure into their calculus, selection may entail choosing bad politicians who profess preferred policies.

In addition, modern democracies involve political parties, and those parties may have interests of their own – including the extraction of rents from the state. This will shape the quality of candidates from which voters can choose. If the supply of quality candidates is not endless, parties have incentives to place low quality candidates in safe districts, reserving their best candidates for contested districts (Galasso and Nannicini, 2011). Almost

⁶ This disadvantage can be particularly acute if access to mass media is limited (Besley and Burgess, 2002).

⁷ It may be hard to imagine that voters would at some point in the future deem an incident of corruption to have been a good thing. Still, say that corruption was necessary to get majority support for a president's program and that program turns out to be successful in producing economic results, would voters look the other way? Workers' Party (PT) President Luis Inacio Lula da Silva of Brazil was accused of buying opposition legislator support in congress (literally paying them monthly salaries). However, despite the scandal, with the government's policies producing impressive economic results, he was re-elected in 2006. When he left office, his hand-chosen successor was elected. The scandal eventually led to several successful prosecutions – including that of the Lula's first chief-of-staff. Even so, it has been suggested that nominating Lula is his party's “Plan B” should his successor falter before the next set of elections.

⁸ Short time horizons have been shown to lead to perverse, instrumental behaviors in a variety of settings. Many game theoretic outcomes are based on the assumptions that play is iterative and that the players do not know when play will end (Fudenberg and Maskin, 1986). Often, when the end of play is known, chances for cooperative or virtuous behavior disappear as by backward logic both players decide to defect in the initial round.

⁹ In *Principled Agents? The Political Economy of Good Government* (Besley, 2006), Besley is relatively optimistic about the possibility of elections serving as mechanisms capable of generating wise selection. For more pessimistic views, see the contributions to a special issue of *The Review of Austrian Economics* (February 2009) where a series of public choice scholars react to Besley's argument.

paradoxically, then, districts with the greatest number of loyal party members will be provided lower quality options.

Finally, voters may find wise selection particularly challenging when incumbents and challengers appear very similar. When challengers fail to articulate distinct positions, projecting their likely performance forward is made difficult. This also means that a poorly performing incumbent has an incentive to make himself look as much like the challenger as possible. By minimizing their differences, the incumbent decreases the likelihood that challenger will be seen as superior and undermines voters' control (Hellwig, 2012).

2.3. Challenges to combining assessment and selection

Thinking about elections as instruments for simultaneously retrospectively assessing incumbents and prospectively selecting among candidates points to the possibility of a true "nightmare" (Manin et al., 1999a). Incumbents know that there is some prospect that their challengers will win the next election by promising voters what they want to hear. As a result, discounting the future, the rents incumbents take are greater relative to what they would extract if they thought voters only engaged in retrospective assessment. As incumbents speculate that their chances of defeat are increasing, they will demand more now because they reason that they will not be in office next term to reap further rewards. To make matters worse, as the rents they take increase, it makes it increasingly easy for incumbents to promise a better future – one with less rent-taking (Ferejohn, 1986; Banks and Sundaram, 1993). Put strictly in terms of malfeasance, anticipating they might be defeated by a future challenger who will promise voters less corruption, sitting politicians have every incentive to enrich themselves prior to leaving office. By doing so, they make their proximate defeat even more certain. The challengers brought to office then face the same dilemma. The democracy is left in a steady state where corruption provokes vote volatility but corruption continues unabated afterward.

Finally, note that for the line of reasoning behind any of the idealized depictions of elections to hold, voters must be able to find virtuous candidates for whom they are willing to cast their votes. Depending on the distribution of options, this may require that voters cast their votes for candidates who are more distant from them on some salient dimension than were their original, now proven corrupt, choices. If voters are unwilling to make this compromise, politicians may feel free to engage in corrupt practices because they are betting that voters will be unable to identify viable, virtuous alternatives. This would result in a sort of *moral hazard*, where corrupt politicians do not bear the costs of their malfeasance.¹⁰ In response to this possible short circuit, Kselman and Niou (2011) offer one formalization of why voters will be willing

¹⁰ To pursue the implications of this line of reasoning further, we used the effective number of parties as indicator of voters' alternatives. We allow the effect of volatility on future corruption to be a function of the scope of choices if this effect increased as options became more plentiful. We found that there is no evidence to support this claim. We present only the more general model here, but we make the results of the qualified model (and replication instructions) available in an [online appendix](#).

to make such a compromise, refusing to vote for the candidate most proximate to their preferences. They develop "a model of protest voting in which unsatisfied voters may abandon their most-preferred candidate even though he or she has a good chance of winning, in the hope that this signal of disaffection will lead to downstream improvements in that candidate's performance" (395). Voters can therefore be willing to protest against corruption by taking votes away from their preferred, seemingly corrupt candidates.

In sum, the complexities of assessment and selection through elections can result in equilibria that differ markedly from the ideal of "out with the bad, in with the good." However, as a foil, we state our hypotheses in terms of these idealized versions summarized above, and test their validity in the face of empirical evidence¹¹:

H1. Higher levels of perceived corruption lead to higher subsequent levels of electoral volatility.

H2. Higher levels of electoral volatility induce subsequently lower levels of perceived corruption.

If we find support for both hypotheses, it will be evidence that the normatively appealing, idealized characterization of elections as instruments of both assessment and selection holds true. Voters can effectively punish badly behaving elected representatives and select better behaving ones, generating virtuous governments. If we fail to find support for either hypothesis, it may mean that voters are unwilling or unable to shift their electoral support to alternative parties. Perhaps they deem the alternatives too distant in ideological terms to give them their support despite the corruptness of more proximate parties.¹² This would create the type of moral hazard discussed above. If we find support for H1 but not H2, it would support the reasoning we summarized above about a nightmarish situation in which fearing defeat by challengers, incumbents take high rents, making their proximate defeat still more likely. Finally, if we find support for H2 but not H1, it would suggest that voters do not punish politicians for corruption but punishing politicians for any reasons leads to less malfeasance. This pattern seems least likely given that it rests

¹¹ Following many prominent studies of volatility and retrospective accountability, we test these hypotheses at the country-election level (Adserá et al., 2003), Kiewit (2000), Powell and Whitten (1993), and Roberts and Wibbels (1999). These dynamics could also be addressed with individual-level data. Individual members of parliament must decide if they are going to engage in or refrain from corrupt practices. Likewise, individual voters must decide on what grounds they will decide whether they wish to vote differently than they have in the past. As a result, a few studies have very effectively made use of individual-level survey data about the vote choice to examine assessment and selection. Not surprisingly, individual level data on corrupt practices by elected representatives is scarce. Only somewhat less scarce are individual politician's assessments of how voters will react to their actions. Even data on individual respondents' perceptions of corruption and the impact of those perceptions on their vote choice is not available for very many times or places (for example, the CSES does not capture the necessary data). Therefore, like the works cited above, taking a country-election level approach allows us to cast our net widely in order to test our theory. It does, however, come at the expense of the fine-grained leverage that an individual-level approach could give over related questions.

¹² As we report below, we do in fact find support for H1. So, voters do find acceptable alternatives when they perceive incumbents to be corrupt.

on politicians offering voters virtuous government even though malfeasance is largely tolerated.¹³

3. Data

By definition, corruption is illegal. Those engaged in the practice go to great lengths to conceal their behavior. Not surprisingly, then, it has proven very challenging to develop objective measures of corruption (Treisman, 2007). Because we are interested in citizens' judgments regarding the conduct of elected representatives, however, objective measures of corruption are less central to our theorizing than citizens' perceptions of that corruption. While the World Bank and Transparency International publish well known measures of perceived corruption, these standard measures are based primarily on the perceptions of country experts, not citizens. Moreover, those measures reflect the level of corruption in a country as a whole and do not distinguish levels of corruption in different institutions.

Fortunately for us, since 2004 Transparency International has collected annual survey data on citizens' perceptions of corruption in a broad, and growing, cross-section of countries. The Global Corruption Barometer (GCB) asks citizens their perceptions of corruption not only for the government sector as a whole but for a variety of more specific institutions, including the national legislature. Given our focus on efforts to assess and select elected officials, we use the GCB question that asks citizens: "To what extent do you perceive the parliament/legislature in this country to be affected by corruption?"¹⁴ Responses were recorded on a five-point scale ranging from "not at all corrupt" (1) to "extremely corrupt" (5). The GCB reports the country average (mean) response on this scale. Thus the aggregated variable is, practically speaking, a continuous variable with values between 1 and 5.

Due to the fact that our causal reasoning rests on the assumption that a country's democratic institutions are at least functional, we include in the analysis only those countries that are at least partially democratic. However, we were also concerned that if we selected only perfectly healthy and well-developed democracies we would run the risk of ending up with a dataset full of cases across which

corruption does not vary. Conventional wisdom led us to suspect that better performing democracies would, virtually by definition, have lower levels of corruption (Treisman, 2000, 2007; Montinola and Jackman, 2002; Brunetti and Weder, 2003; Adserá et al., 2003). As a result, we chose to focus on democratic regimes identified with a fairly permissive inclusion criterion (however, our concern was misplaced, with results holding under a more restrictive case selection criteria).¹⁵ For all of the countries covered by the GCB, we collected Freedom House data for the years 2000–2010. If a particular country scored a "Free" or "Partly Free" designation in more than 75% of its observations, we include it in our study.¹⁶ In the end, we collected corruption data for 72 regimes from around the world.¹⁷

We sought vote distributions amongst parties at the national level in each country starting two elections prior to 2004 (the first year of the GCB corruption data). This allowed us, in most cases, to calculate one observation of electoral volatility prior to the first observation of corruption perceptions.¹⁸ Our electoral data come from a variety of sources. For most elections in Europe and other OECD countries, we drew from the online *European Elections Database* which is provided by the National University of Ireland. For elections in Africa, we relied on the online *African Elections Database*, which is an aggregator of electoral data garnered from electoral authorities in each country on the African continent. For many elections in Latin America and Asia and a few elections in Europe, we drew data from the electoral handbook series edited by Dieter Nohlen Nohlen (2005), Nohlen et al. (2001) and Nohlen and Stover (2010). The remaining electoral data was taken directly from electoral commissions in each respective country.¹⁹ We collected vote data for 249 elections which allow us, after the lags necessary to calculate a

¹³ Several recent strands in the literature have made the case that the mere presence of regularly scheduled elections does not mean that all democracies function equally well. It could be the case that in some regimes politicians do not fear the loss of office as a result of unrepresentative behavior. Using terms like "delegative democracy" (O'Donnell, 1994), "electoral authoritarianism" (Caijgaert, 2006; Schedler, 2002), and "democracy with adjectives" (Collier and Levitsky, 1997), several scholars have pointed out that elections as instruments of assessment and selection may breakdown or at least work poorly even in regimes that are considered formally democratic. To account for this possibility, we constructed a model in which the effect of volatility is a function of the quality of democracy (as captured by Polity scores) to determine whether vote volatility has a differential effect as a function of the quality of accountability mechanisms. We did not find support for this idea. In addition, the findings we report below hold across democracies above various Polity score thresholds. Thus, we present only the more general model here, but we make the results of models qualified by the quality of democracy (and replication instructions) available in an [online appendix](#).

¹⁴ The responses to this question were correlated at $r = 0.92$ with the responses to an identical question asking about corruption of political parties.

¹⁵ When we only examine democracies with a polity score greater than 7, we get the same substantive results we report below. In [Table 1](#) of an online appendix we provide an empirical model accounting for the quality of democracy and the possibility that corruption only declines after vote volatility in the best functioning regimes. It shows the same substantive results as the more general model presented in the paper.

¹⁶ We dropped an additional eight countries from this initial group due to the unavailability of vote data.

¹⁷ The countries included are: Albania, Argentina, Armenia, Australia, Austria, Bangladesh, Bolivia, Brazil, Bulgaria, Canada, Chile, Colombia, Costa Rica, Croatia, Czech Republic, Denmark, Dominican Republic, Ecuador, El Salvador, Fiji, Finland, France, Georgia, Germany, Ghana, Greece, Guatemala, Hungary, Iceland, India, Indonesia, Ireland, Israel, Italy, Japan, Latvia, Lithuania, Luxembourg, Malaysia, Mexico, Moldova, Mongolia, Morocco, Netherlands, New Zealand, Nicaragua, Nigeria, Norway, Panama, Paraguay, Peru, Poland, Portugal, Romania, Senegal, Sierra Leone, Singapore, Slovenia, South Africa, South Korea, Spain, Sweden, Switzerland, Taiwan, Turkey, Ukraine, United Kingdom, United States, Uruguay, Vanuatu, Venezuela, Zambia.

¹⁸ We chose to focus on volatility in votes rather than seats. This has the advantage of picking up on relatively subtle changes in the electorate's preferences that, while registered in vote fluctuations, might not be registered in seat fluctuations. Practically speaking, however, the choice between the two is generally immaterial as vote and seat volatility tend to be very highly correlated.

¹⁹ Even though electoral datasets typically group votes for very unpopular parties into an "other" category, thereby compromising our ability to calculate precise levels of volatility. Fortunately, for the country-years in our dataset, the average proportion of votes in the "other parties" category was less than 3.7%.

measure of electoral volatility, to include 169 observations in the regression analysis.

Taagepera and Grofman (2003) evaluate several indices of disproportionality and inter-election volatility and conclude that the Pedersen Index (Pedersen, 1983) and the Gallagher Index (Gallagher, 1991) “satisfy more criteria than any other” in terms of their ability to capture the dynamics of electoral volatility (p. 673). For the observations in our dataset, the two indexes produce volatility figures that are highly correlated with one another ($r = 0.94$). We estimate our models with the Pedersen Index, which is mathematically defined as:

$$\text{Pedersen} = \frac{1}{2} \sum_{i=1}^N |p_{i,t} - p_{i,t-1}|$$

where $p_{i,t}$ is party i 's vote share at time t and $p_{i,t-1}$ is party i 's vote share at time $t - 1$.²⁰

In order to evaluate the proposed relationship between our variables of interest – electoral volatility and perceived legislative corruption, we need to keep track of the temporal nuances involved in using each series both in explanatory and outcome roles. Only temporally antecedent values of each series are used to predict current states of the other phenomenon and, ideally, both series should be composed of measurements generated at the same intervals. For our series, it is usually the case that corruption measures come in shorter intervals than our measures of volatility, because the surveys on which the former are based are not constrained to election years. For this reason, we take the inter-election period as our unit of analysis, and generate values of political corruption at the appropriate level of aggregation by averaging all corruption scores that were obtained in the years between elections, excluding scores for election years themselves. As a result, each observation is composed of two values – one of the electoral volatility observed in the year that marks the beginning of the inter election period, and one of political corruption observed throughout the years strictly between elections.²¹

²⁰ Reasoning that perhaps politicians only reformed their behavior when they saw votes going to entirely new parties, we also measured volatility based solely on this dynamic (what Tucker and Powell (2010) refer to as “Volatility A”). In Table 2 of our on-line appendix, using just new party volatility, we derive the same substantive results we report below, although model fit is slightly worse because the residuals are not bivariate normally distributed. Table 3 in the appendix reports results using only volatility among previously existing parties (“Volatility B”). Given the strong correlation between type A volatility and type B volatility (and volatility calculated using both) the results remain basically unchanged.

²¹ When trying to establish the simultaneous effects of phenomena over time, the literature often relies exclusively on the predictive power of the time trends of the phenomena of interest. As a result, models of these dynamics often lack the types of statistical controls common to other modeling techniques, focusing on the joint significance of self- and cross-lagged values of the endogenous variables of interest. We follow this practice in the model we discuss and present in the next section (for other examples of this, see Brandt and Jones, 2006; Enders and Sandler, 1993; Edwards and Wood, 1999). However, nothing in the theory of vector autoregressions precludes the inclusion of exogenous variables (Lütkepohl, 2005), and their use may even be desirable in order to rule out spuriousness. Consequently, in an online appendix we present the results of a model estimation which includes a battery of exogenous variables (Table 5). Our substantive findings regarding the relationships of greatest theoretical interest do not change.

Take, for example, the case of Lithuania, which enters our regression analysis in the years 2004 and 2008. Electoral volatility in 2004 is calculated as a comparison between the distribution of party support in 2004 and in 2000 while electoral volatility in 2008 is a comparison between distributions in 2008 and 2004. We have measures of legislative corruption from 2004, 2005, 2007, 2009, and 2010. We model whether the 2004 election observation of volatility is a determinant of the average of the 2005 value and the 2007 value for corruption. We then use that same average value for perceived corruption as a possible determinant of electoral volatility as measured at the 2008 election observation. Finally, the 2008 election observation of volatility is used as a potential determinant of the average of the value of corruption in 2009 and the value in 2010. In sum, this setup allows us to estimate the effect of volatility in the election year on the subsequent average levels of corruption across all years leading up to the next election year and the effect of average levels of corruption across all years leading up to the next election year on vote volatility in that election.

4. Analysis

Testing our hypotheses is akin to establishing whether (1) electoral volatility can be better predicted when a temporally antecedent value of political corruption is used for generating the prediction; (2) the same is true about political corruption with respect to electoral volatility; (3) it is the case that larger values of temporally preceding corruption are expected to increase the values of subsequent volatility; and (4) it is the case that larger values of the temporally preceding volatility are expected to decrease values of subsequent corruption. The mechanism we propose, therefore, generates a feedback, or simultaneity relationship, between corruption and electoral volatility.

A common modeling strategy to account for this type of simultaneous relationship consists of using structural equations (Freeman et al., 1989). In general, instruments are incorporated in a two-stage estimation process in order to ‘purge’ presumably endogenous variables from the variation that is attributed to the very phenomenon they are expected to affect. In practice, however, finding appropriate instruments is often problematic. In order for the instrumental variables approach to yield correct estimates of the hypothesized relationship, instruments must both (1) have a non-zero effect on the instrumented phenomenon and (2) be unrelated to the explained variable once the instrumented phenomenon is taken into account (Angrist et al., 1996). Because verifying the latter – the so-called *exclusion restriction* – is inherently difficult in an empirical setting, the validity of the results obtained through this procedure hinge on an assumption that often leads to inconclusive and contradictory results (Freeman et al., 1989).

A modeling alternative which circumvents these potential problems is vector autoregression (VAR) – a technique that generalizes autoregressive models (i.e. models of time series that make current states a function of the series’ historic trend) to enable simultaneous analysis of multiple, interdependent series. By relaxing the need to know the specific functional form relating the two endogenous phenomena, VAR models are able to produce estimates of

the general, historic dependency between them. Hence, although VAR models restrict our ability to directly test the specific *mechanism* through which two phenomena are caught in an interdependency, they allow us to establish whether the proposed feedback relationship is actually present, when relevant conditions are met²² (Freeman et al., 1989; Freeman et al., 1998).

In addition, VAR models can accommodate data that display dependencies that come from sources *other* than the temporal dimension of a given set of series. Specifically, when the time series come from multiple units – in our case, countries – VAR models can (and should!) be extended to account for the heterogeneities that can be expected to exist across units. Such extensions, which can comprise anything from allowing each set of series to have unit-specific means (see, for instance, Holtz-Eakin et al., 1988) to letting all parameters in the model (including covariances between the time series) vary by unit (see Canova and Ciccarelli, 2013), are usually labeled *panel VAR*.²³

For instance, a reduced-form panel bivariate VAR of order 1 (i.e. a model in which a single lag of each of two series are used to predict their current values), which accounts for cross-sectional heterogeneity by allowing parameters to vary by country, could be defined by:

$$y_{tc} = \mathbf{a}_c + \begin{bmatrix} \phi_{12} & \gamma_{12} \\ \gamma_{21} & \phi_{21} \end{bmatrix}_c \begin{bmatrix} y_{1,t-1} \\ y_{2,t-1} \end{bmatrix}_c + \mathbf{u} \quad (1)$$

where \mathbf{a} is a vector of constants, \mathbf{u} is a vector of residuals drawn from a bivariate distribution (usually a bivariate Normal with a zero mean vector), the ϕ 's are self-lag coefficients and the γ 's are cross-lag coefficients (i.e. coefficients corresponding to lags of one series we hypothesize is causing the other series).

VAR models are especially well suited for evaluating Granger causality, which is said to exist between phenomena Y and X if their future is better predicted when accounting for *both* the history of X and of Y , rather than simply the trend of either one by itself (Freeman, 1983).²⁴ When including the appropriate number of self-lags as predictors, and accounting for any source of heterogeneity in the dynamics of the relationship under study, VAR models provide the most natural and direct test of Granger causality between any number of time series.²⁵

²² More specifically, the conditions are that the series being evaluated are either stationary or cointegrated. Substantively, these conditions insure that we can infer something generalizable about the series under study by using their observed behavior. For a detailed explanation of what these two conditions entail, see Hamilton (1994).

²³ VAR models can even include variables that are 'exogenous', in the sense that their values are believed to be the result of dynamics that are outside the system under consideration. Models that include such variables, which serve a purpose akin to that of statistical control, are called conditional 'VARX' models (Lütkepohl, 2005). Our substantive results remain largely unchanged (see Table 5 in our online appendix).

²⁴ When it is the case that the future of Y can be better predicted by past values of X and Y than by values of Y alone, it is said that X Granger-causes Y . Similarly, when the same can be said about X with respect to Y , it is said that Y Granger-causes X , establishing a Granger feedback between X and Y .

²⁵ Examples of political science works that evaluate Granger causality abound (see Edwards and Wood, 1999; Enders and Sandler, 1993; MacKuen et al., 1992).

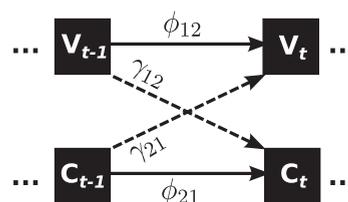


Fig. 1. Depiction of Granger Feedback Characterized Using VAR model parameters.

As a result, these models are ideal for testing theories that pose the existence of a feedback relationship that occurs over time and across multiple countries such as the one we have posed between electoral volatility and legislative corruption. Fig. 1 depicts the way in which the modeling strategy works for the bivariate VAR model defined above, within any given country. The dotted lines represent the effects we are interested in (e.g. in our case the effects of corruption on volatility and vice versa), whereas the solid lines represent the impact of a series' immediate history on its current value. In a sense, then, the VAR model allows us to gauge each series' effect on the other after filtering out the explained series' historic trend.

To test the empirical validity of our hypotheses, we implement such a panel (reduced-form) VAR model of order 1 to test whether there is a Granger feedback between electoral volatility and political corruption. The model we specify (which contains only one self and one cross-lag for each series) proves enough to remove all serial autocorrelation (be they the result of temporal or cross-sectional dependencies) of residuals, justifying the use of a VAR(1) model.²⁶ We rely on a multilevel Bayesian specification to obtain our model's coefficients, and we use MCMC simulations to describe their posterior distributions.²⁷

MCMC Bayesian estimation techniques of VAR models are particularly useful for the purpose at hand for three main reasons. Firstly, as is the case in many applications of panel VAR models, our data is characterized by having a moderate-sized cross-section of countries and relatively short, uneven time series within each panel. This poses important inferential challenges, as the consistency of other, non-Bayesian estimators relies on asymptotic conditions imposed on the length of the time series, the size of the cross-section, or both (e.g. Holtz-Eakin et al., 1988; Binder et al., 2005). Bayesian estimates obtained through a well defined Markov Chain circumvent these problems by

²⁶ Direct tests of Granger causality require an inclusion of as many self-lags as needed to eliminate serial autocorrelation. Although we cannot test *all* possible orders of autocorrelation, we are confident that the number of self-lags we include in our model are sufficient to control for the predictive power of each variable's historic trend for both theoretical (volatility that is far removed in the past should not affect current electoral volatility) and empirical reasons. See our discussion of model fit below.

²⁷ All estimations were conducted in R v. 2.15.0 in combination with JAGS v. 3.3.0. The relevant replication code is available at ***.edu.

providing as-if random samples from the posterior distributions of the model's parameters of interest.²⁸

Secondly, unless some restrictions are imposed, the country-specific parameters of the proposed panel VAR model can only be estimated at a very high cost in efficiency and at the risk of overfitting the dynamics in each country (as it would effectively require estimating a separate model for each country in our sample). A reasonable compromise between a model that imposes homogeneity and a model that allows for complete heterogeneity in the parameters is a model that allows for partial pooling of the effects (Gelman and Hill, 2007). By letting each country's set of parameters be a sample from a common distribution (i.e. by letting both intercepts and slopes in the VAR be modeled as country-by-country random effects) such partial-pooling can be achieved. This random effects VAR modeling strategy, which has been suggested before (see, for instance, Binder et al., 2005; Canova and Ciccarelli, 2009, 2013), is very easily implemented in an MCMC framework.

Finally, the analysis of dynamic, long-term effects for which VAR models are best suited requires us to obtain measures of uncertainty around highly non-linear functions of the estimated model parameters – a task for which, once again, we would have to rely on hard to justify asymptotic assumptions. The matter, however, is very easily resolved once samples from the parameters' posterior distributions have been obtained through the MCMC procedure. In particular, inferences regarding the significance of impulse response functions – which are the long-term effects of a single shock to one of the variables on the entire system – require very little additional work once a Markov sampler is finished. This simulation-based strategy is not new in the VAR world, and can sometimes be preferable to the analytical approach for obtaining measures of uncertainty of impulse response functions (Lütkepohl, 2005).

Our model assumes that the (standardized) phenomena of interest (viz. electoral volatility and legislative corruption during any given legislative term) are random draws from a bivariate normal distribution, the mean vector of which is composed of functions of a constant term, one self-lag and one cross-lag (with variables constructed in the manner described in the previous section). As discussed earlier, and in order to account for within-country dependencies in our panel-structured dataset, we allow the model to include both a normally distributed random intercept and normally distributed coefficients per equation per country.²⁹ As a result, the model is exactly as that

defined in Equation (1) above. The model estimates the (contemporaneous) correlation between the two phenomena of interest, thereby relaxing the assumption of independence of residuals across the two processes.³⁰ For more details on the estimation procedure, which involves the imputation of missing data in our time-series, see the Appendix.

The bivariate Normal model is a relatively good fit for the data – although we cannot reject the null of normality for the observed marginal distribution of residuals for the corruption series using a Shapiro–Wilk test (p -value of 0.09), the same test conducted on the volatility residuals yields a p -value small enough to reject the null of normality.³¹ Partial R^2 measures, however, indicate very good linear fits to both series, with values of 0.95 and 0.87 for the corruption and volatility equations, respectively). Additionally, all serial autocorrelation appears to be accounted for by the specified model, justifying the number of lags chosen. Fig. 1 in the online appendix displays these autocorrelations as they span a number of lags – that is, the correlation between each residual and the residual before it, and the one before that, etc.³² Furthermore, goodness-of-fit tests (conducted by comparing differences in deviances) indicate that the models with two and three self-lags – which are effectively nesting the model we report – are not better fits than our one-lag specification (with differences of -8.63 and -7.71 , respectively). Finally, our model allows us to estimate the contemporaneous correlation of the two variables of interest, which (if being of discernible size) is of major importance for the estimation of long-term effects. The estimated median of the posterior distribution of this correlation, however, suggests that the (contemporaneous) linear association between corruption and volatility is negligible (viz. equal to 0.038, with a 90% credible interval between -0.12 and 0.32).

The results of the estimation are presented in Table 1. In it, we report the medians (and 90% highest posterior density around them) of the marginal distributions from which country-specific intercepts, self-, and cross-lag coefficients are drawn. These estimates comprise a direct test of hypotheses H1 and H2 above. We also report point estimates (and credible intervals) of the variances of said distributions. In the interest of saving space, we do not report the specific values of random intercept and slope parameters for any individual country (although they are easily accessible using our replication code). For substantive purposes, these summary statistics can be interpreted as one would interpret the point estimates and confidence intervals produced by classical, non-Bayesian estimation techniques

²⁸ In addition, when combined with appropriate prior information, Bayesian estimates appear less susceptible than their classical counterparts to estimation issues raised by non-stationarity of the time-series involved in the autoregressive process (Sattler et al., 2008; Sims, 1989). Most of these benefits are likely to be reaped when non-flat priors (such as the ones we adopted) are used.

²⁹ Although our model does not incorporate them, more complex dependencies in the data, such as those that are expected when there are “spillover” effects across units, can (at least in theory) be modeled in the VAR framework. One could, for instance, model the covariance in the random intercepts of a panel VAR such as ours using regional indicators as explanatory variables. In the interest of parsimony, however, we must leave this matter – which is interesting of its own accord – for future research.

³⁰ The specified model assumes that, after accounting for country heterogeneities and time-serial dependencies, the errors ought to be identically distributed white noise. An evaluation of the residual's autocorrelation function, provided in the on-line appendix, supports this idea.

³¹ Although taking the log of the volatility series improves the fit of the data to the bivariate Normal model, the transformation adds complexity to the interpretation and leaves substantive results unchanged. As a result, we have opted not to take the logarithm of the volatility series.

³² The panels show how these correlations are all negligible, and we offer this as evidence in favor of our choice with respect to the number of lags.

Table 1

Posterior Medians and 90% Credible Intervals for Mean and Variance Parameters of Country-By-Country Random Effects in panel VAR Model (A Test of Hypotheses H1 and H2).

	Response variable	
	Electoral volatility _t	Political corruption _t
Mean of volatility _{t-1,country}	-0.083 (-0.245,0.097)	-0.117 (-0.281,0.048)
Variance of volatility _{t-1,country}	0.075 (0.047,0.126)	0.79 (0.048,0.13)
Mean of corruption _{t-1,country}	0.375 (0.218,0.535)	0.783 (0.523,0.941)
Variance of corruption _{t-1,country}	0.030 (0.023,0.041)	0.021 (0.016,0.028)
Mean country random intercepts	-0.768 (-1.645,0.066)	1.513 (0.630,2.845)
Variance random intercepts	0.131 (0.065,0.299)	0.129 (0.065,0.293)
Partial R ²	0.87	0.95
N	169	
Contemp. correlation	0.038 (-0.21, 0.32)	
Difference in DIC w.r.t. model without cross-lags:	-53.06	

(although these correspond to quantities derived from the observed, as opposed to theoretical, sampling distributions of the coefficients of interest), and they correspond to average effects of country-specific dynamics.

The sizable (*viz.* -53.06) difference in deviances suggests that the model using a single cross-lag for predicting each series provides a much better fit of the data than a model that includes self lags only. Results presented in Table 1, however, indicate that Granger causality can only be reliably posited when considering the effect of corruption on electoral volatility (first column). In support of H1, then, the probability that increasing immediately past perceptions of corruption leads to greater electoral volatility is high (*i.e.* greater than 0.9). In other words, voters are likely to express their discontent in the face of high perceived corruption by expressing preferences for different alternatives. More specifically, Fig. 2 shows predicted values (and 90% highest posterior density intervals for each prediction) of vote volatility at time t for different levels of perceived corruption at the receding time $t - 1$, holding the past level of vote volatility constant at its observed average. The predicted values can be construed as the effects of an exogenously achieved level of perceived corruption on immediately future levels of vote volatility. Our model predicts that exogenously increasing perceptions of corruption from about 0 to 5 (the variable's theoretical range) can result in a dramatic increase in volatility – from a vote volatility level of about 4% to a vote volatility level of over 40% in an average country.

We can see this trend borne out in individual countries as well. For example, in the lead up to the 2004 election and 2008 elections, incumbent politicians in Lithuania were beset by corruption scandals (Velykis, 2010). The GCB report scored Lithuania at 4.2 on a 5-point scale leading up to the 2004 election and at 4.0 leading up to the 2008 election. In both cases, the Lithuanian people responded to these high profile corruption cases by voting for the opposition parties (Lithuania: Constitution and Institutions, 2007; Country Report: Lithuania (2008), N.d.). Indeed,

total volatility figures in these elections were 86.2 and 65.1, respectively. These figures are much larger than one standard deviation above the mean volatility score for our cross-national dataset. Additionally, these figures specifically reflect Lithuanians' acute sense of disappointment in persistent levels of corruption (Velykis, 2010). In national electoral surveys in 2008, for example, half of respondents indicated that both the parliament and the government were "very corrupt" entities and, furthermore, 83% responded that these national politicians should be held more responsible for the level of corruption.

Similar to our results, though not focusing on members of parliament,³³ Ferraz and Finan (2008) show that the revelation of corrupt practices had a discernible impact on incumbents' electoral performance in Brazilian municipalities. They gain particular leverage on the relationship between corruption and vote choice due to the federal government's random selection of municipalities for audit. They compare vote choice in municipalities where corrupt practices were revealed prior to the election to vote choice in municipalities where corruption was not revealed until after the election. They find that mayors in municipalities where corrupt practices were exposed prior to voting were much less likely to get reelected and that the likelihood of reelection decreased even more with each count of corruption reported by the audit.³⁴

The same support for H1 is evident when we move from the effects of shocks to the immediate past to effects of shocks over time. VAR models are particularly well suited for evaluating how a single, exogenous shock to the system (*e.g.* an increase in volatility due to the sudden death of a prominent leader, or a surge in corruption prompted by a sudden increase in regional oil royalties) can affect values of the variables in the system over a longer time horizon. These analyses, usually based on Impulse Response Functions (IRFs), allow us to investigate the ripple effects of a surge in each of the variables on the entire system over time, assuming no further surges occur and subsequent values are as predicted by the model's parameters. Effectively, IRFs allow us to compare two chains of predicted values for each series – one where there is an initial external shock and another where no such shock occurs; their differences can be interpreted as the responses of the system to the initial shocks, or impulses. Fig. 3 displays these IRFs for the variables in our model, along with simulated 90% regions of uncertainty around them. It shows responses, over time, of the two variables comprising our system to exogenous surges of each.

The bottom left panel of Fig. 3, which corresponds to the response of electoral volatility to a surge in corruption, is consistent with H1: after an initial, one standard deviation

³³ Peters and Welch (1980) and Welch and Hibbing (1997) examine the effects of corruption charges on the impact of incumbents' reelection prospects in U.S. House of Representatives. They find that allegations of corruption do lead to a loss in electoral support. Reelection rates are much lower for members charged with fraud than the average incumbent, but corruption charges do not automatically lead to defeat.

³⁴ Related to our discussion above regarding information and the mass media, they also find that the impact of the audits were enhanced where there was a local radio station to report the audit results.

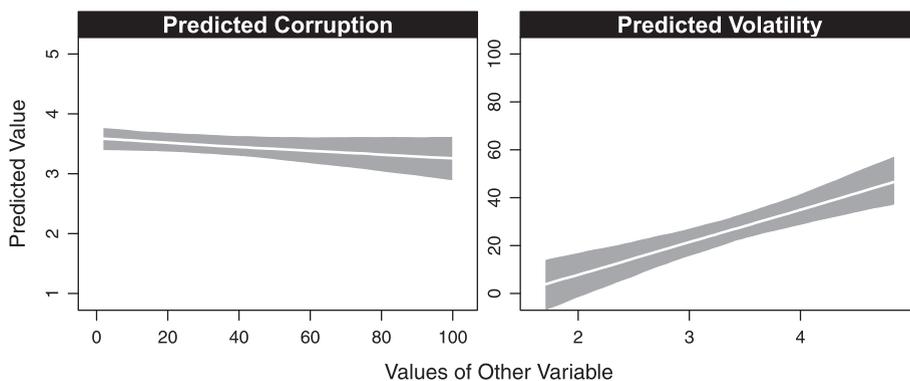


Fig. 2. Predicted Values (in Standard Deviations) of Corruption at time t as a Function of Lagged Volatility (Left Panel) and of Volatility at time t as a Function of Lagged Corruption (Right Panel). Shaded regions represent 90% Credible Intervals for the Predictions.

increase in perceptions of corruption, it is estimated that the system will see a corresponding surge in volatility that is both statistically discernible and substantively large, and which takes a long time to taper off – about six election cycles. This long lasting effect of corruption on volatility, which can be construed as the type of political alienation prompted by acts of malfeasance, is in line with the pattern of sustained dissatisfaction we alluded to in the case of Lithuania, and appears now to be a cross-country phenomenon. We will return to this persistence in the ripples of corruption in our conclusion.

The evidence from the model in Table 1 and in Figs. 2 and 3 does not support H2. The average predicted levels of perceived corruption do decline slightly as the amount of dismay previously displayed by voters increases, but the credible intervals around our predictions when the amount of vote volatility is high are far too broad (barely excluding zero) to conclude that there is any relationship. The left panel of Fig. 2, which displays the next-term predicted values of corruption as immediately preceding volatility spans its observed range, shows no statistically discernible changes as a result of the increase in volatility. A similar conclusion is reached by examining the top-right panel of Fig. 3, which shows no statistically discernible evidence of any long-term effects of an exogenous surge in volatility on perceptions of corruption.³⁵ Even if we relaxed our criterion for deciding which results are statistically significant, the estimated sizes of these cross-sectional and long-term effects are so small that they could hardly be deemed *substantively* significant. In general, then, we find little evidence in favor of H2.

The results of the cross-national statistical analysis evaluating H2 do, however, affirm what we know anecdotically of several individual countries around the world: vote volatility fails to curb (perceived) corruption. For example, Israel averaged a 4.15 on the GCB's 5-point scale of citizen

perceptions about the extent of corruption in parliament between 2004 and 2010. There were three parliamentary elections during this time period with volatility figures of 49.1 in 2003, 74.2 in 2006, and 43.5 in 2009. While the spike in volatility in 2006 can be partially attributed to the emergence of the new Kadima Party, the persistent level of volatility reflects voters' sustained dissatisfaction with the corrupt practices of elected politicians. Galia Sagy, the Head of Transparency International in Israel, argues that the string of public prosecutions of corrupt politicians in Israel conveys information to the voters about the overall level of corruption in the political system (Dattel, 2011). She notes a direct tie between voters' perceptions about corruption and the accrual of legal cases against national-level politicians (Dattel, 2011). There is also reason to believe that these perceptions are, in turn, informing vote choice. Certainly this was the case years earlier in 1977, when corruption charges forced Prime Minister Yitzhak Rabin to close down his campaign and prompted voters to move their support from the Labor Party to more right-leaning parties (Benn, 2009). It has also been the case in more recent years, where Ehud Olmert, anticipating dire reelection prospects, specifically cited corruption accusations in his announcement that he would not run in his party's primary in 2008 (Olmert, 2008). Despite his admission that his corrupt practices were shaping his electoral prospects, however, electoral volatility did little to actually curtail future perceptions of corruption in Israel. In 2009, the average Israeli respondents' perception of corruption in parliament was at 4.0 – among the highest levels in the world.

The same holds true in Bosnia-Herzegovina – an out-of-sample case. A very high level of perceived corruption in 2005 (4.5) helped prompt a 123% increase in total volatility between 2002 (20.4) and 2006 (45.5). Despite the upswing in volatility, it does not appear that politicians received the message, as subsequent measures of perceived corruption in 2005 and 2007 showed the figure holding steady at 4.5 and 4.4, respectively. Transparency International's National Integrity System Study for Bosnia-Herzegovina in 2007 noted that perceptions of corruption in the country were being driven mainly by “critically problematic pillars” in society such as political parties and “the highest levels of elected” office (Transparency-International, 2007). While

³⁵ These results (as well those regarding H1) are robust to alternative model specifications (reported in our online appendix). Specifically, we study whether the corruption-curbing effect of vote volatility is conditional on (i.e. is a function of) previous levels of malfeasance, the choice set available to voters (the effective number of electoral parties), and the quality of democracy (as measured by a country's Polity score). We still do not find support for effective prospective selection.

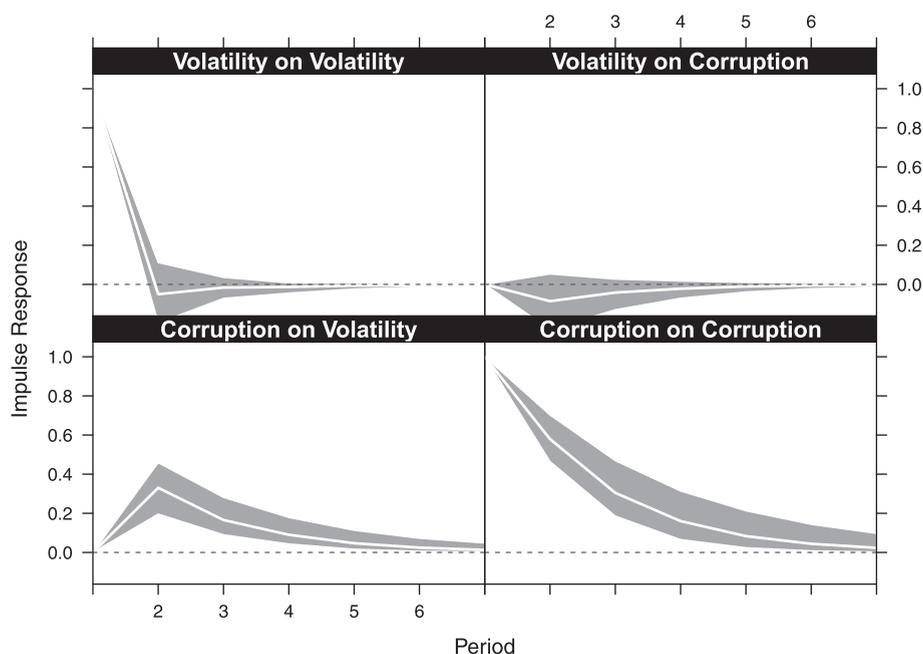


Fig. 3. Impulse response functions of perturbations to corruption and volatility. Shocks correspond to a standard deviation increase in each series at time t . Shaded regions represent 90% credible intervals for the impulse responses.

several issues were salient in the 2006 election, we have good cause to suspect that citizens were also concerned about the extent of corruption among national politicians. A survey conducted by the World Bank in 2000 concluded that there was “a high level of public concern” regarding corruption with citizens believing that corruption was responsible for greater inequality, higher crime rates, and reduced foreign investment (World-Bank, 2000). Furthermore, more than 97% of survey respondents indicated that corruption “leads to very serious consequences” for their country (World-Bank, 2000). Despite the fact that citizens in Bosnia-Herzegovina both (1) identified national politicians specifically as the source of corruption and (2) indicated that corruption was a driving issue in the country’s politics, their efforts at curbing corruption were ineffective. Of the 163 countries ranked by the Global Perceptions Index on a yearly basis, Bosnia-Herzegovina fell from 70th place in 2003, to tied for 88th place in 2005, to tied for 98th place in 2006 (Transparency-International, 2007).

In summary, both cross-national and anecdotal evidence seems to suggest that, although more corruption is expected to lead to more (and persistent) electoral volatility, increasing volatility does not seem to reduce subsequent levels of (perceived) corruption. As a result, then, the case for the idealized depiction of elections as instruments of assessment and selection is found wanting: corruption provokes harsh retrospective assessments, but prospective efforts at selection fail to generate clean government.

5. Discussion and conclusion

We began this paper by summarizing idealized characterizations of elections as instruments of assessment and

selection on which common understandings of *representative government* heavily depend. According to those accounts, voters hold elected officials accountable for their performance in office by voting out of office anyone suspected of corrupt behavior. This presumably purges government of the worst officials, reminds continuing representatives of the voters’ power, and selects newly elected officials who are more virtuous than their predecessors. The empirical evidence we have reported, like the more advanced theorizing summarized above, seems to indicate that the role of elections is not as straightforward and simple as we might like. Our findings show that voters respond as predicted (by at least some models) to perceived corruption by voting to throw their representatives out of office. Nevertheless, the observation that increasing volatility in votes does not reduce perceived corruption undermines an understanding of elections as mechanisms for selection (or screening).

Theories of spatial voting assume that voters identify the party closest to them on some dimension (or dimensions), typically in a left-right policy space, and cast their votes for that party. However, there is a well established literature – going all the way back to Duverger (1954) – explaining why voters might be willing to abandon their most preferred candidate or party when they suspected it was not likely to win. Exercising retrospective accountability, casting one’s vote for a party other than the previously preferred one makes sense if its actions in office have shown it to be located further from the voter than originally estimated or have rendered it likely unviable. It is not clear that allegations of corruption would necessarily do either. Yet there is evidence suggesting that long-term instrumental voters could potentially be willing to

abandon a preferred, viable party in order to signal that they want still better performance from it in the future (Kselman and Niou, 2011). Our empirical results seem to indicate that voters are willing to abandon corrupt representatives perhaps either because they estimate that their corruptness has rendered them unviable or because they are willing to use their votes to send a signal that they want better (more virtuous) representation in the future.

Our results with respect to the prospective selection properties of elections are more dismal. Manin et al. (1999a, 45–46) note that voters may be “swayed by the prospect of electing better governments. Voters may believe that the challenger is ... more honest, willing to accept lower rents in exchange for holding public office. ... [I]f voters always think that the challenger is better, then the incumbent can never be reelected, and he will always choose to extract high rents. In turn, if incumbents extract high rents, voters will never vote for them. The incumbent knows that voters will always be swayed by the promises of the challenger and always extract maximal rents, which means that if voters believe that political are not all the same, they are certain that the challengers will be better for them. In this situation, voters’ control breaks down completely.” The “nightmare” they describe is based on formal theoretic work by Ferejohn (1986) and Banks and Sundaram (1993), and it appears to capture the empirical patterns we have uncovered here.

One question that remains to be addressed is whether there is something unique about corruption that makes it different from other failures of representation. Whereas it is difficult to know decisively that corruption is not covertly occurring (Besley and Burgess, 2002; Vivyan et al., 2012), other legislative behaviors are more transparent. A legislator who signs a no-new-taxes pledge and reneges by voting for tax increases does so in full public view as do legislators who fail to introduce or support other policies that voters want. Legislators who are inept at constituency service, providing pork, or engaging in symbolic representation (Pitkin, 1967) also are visible to their constituents to different degrees. It may simply be that the largely invisible nature of corruption, when carried out competently, short-circuits the assessment-to-selection link. However, even if future research shows that corruption is unique, the failure of elections to provide voters a remedy for corruption is a significant shortfall. Corruption, after all, is a fundamental threat to political representation as evidenced by Madison’s emphasis on virtue and public trust.

Our findings present opportunities, in particular, for studies positing a relationship between voters’ ability to perceive and assign blame for shortcomings in representation and their subsequent ability to do something effective about them. The mechanism behind this so-called “clarity of responsibility” link – electoral volatility – is theoretically obvious, yet has never been tested in a cross-national context. Our findings suggest that the clarity of responsibility provided by some institutional designs is only a *necessary* but not *sufficient* cause for subsequent improvements in representation. Our results lead us to conclude that incumbent politicians are either misunderstanding or ignoring the signals voters send regarding corruption. Or, perhaps, while politicians take steps to

remedy the situation, voters’ perceptions remain pessimistic. A next step in the clarity of responsibility literature might be to examine when, given any level of clarity of responsibility, elections actually serve to bring the actions of elected officials back in line with the preferences of voters. Also, it would make sense to investigate how mechanisms governing clarity of responsibility affect voter disaffection, especially with their own parties.

On a related note, our results also speak to the literature on sustained political disaffection and the low esteem in which elected officials are held. Recall from the bottom right panel of Fig. 3 that perceptions of corruption recover relatively slowly from a surge in corruption at some point in the past. This is consistent with previous theoretical treatments of this matter. For example, in his model of the optimal choice set of candidates, Fedderke (2010) concludes that as long as experience is valued but the costs of abuse of privilege increase with time served, an equilibrium proportion of candidates with an appropriate level of prior experience may simply not exist. “The consequence is that dissatisfaction with the political class and system will be endemic, in the sense that the dissatisfaction cannot be eliminated: there is simply no optimum that is available to the society. No amount of policy intervention can resolve social disaffection under these circumstances” (Fedderke, 2010, 146). Empirically, Söderlund (2008) shows that even after controlling for general levels of dissatisfaction with the political system, dissatisfaction with one’s most preferred party is statistically discernible cause of vote volatility. Similarly, Bélanger (2004) finds that disenchantment with specific (traditional) parties drives voters to third parties while disenchantment with parties generally generates abstention. Clarke and Kornberg (1996) noted that such shifts could be sufficiently dramatic as to remake a political landscape, even in institutional settings least favorable for providing a diverse array of party options.

Thinking about our findings in terms of this literature, future research might tackle whether corruption scandals ratchet up political disaffection, which then only dissipates gradually – if at all – as the scandal fades. This is the driving focus of a burgeoning literature on electoral participation in the face of corruption. Stockemer et al. (2012) and Stockemer (2013), for example, find that increased corruption decreases voter turnout in, respectively, legislative and presidential elections around the world³⁶ (for similar evidence from Mexico, see Chong et al. (2012)). But does this abstention or disaffection from the (institutionalized) electoral arena go hand-in-hand with participation in the (non-institutionalized) non-electoral arena? Machado et al. (2011), for example, find that increased corruption drives up an individual’s propensity to participate in street-level protests, while – in the specific case of Bolivia – Gingerich (2009) finds similar effects. Future work at the individual level could focus on how corruption levels affect individuals’ vote-versus-protest calculation, which would, as in the case of contemporary events in Brazil, help us

³⁶ although they note that these findings are somewhat preliminary and contingent on how corruption is measured).

understand when and under what circumstances protest activity is substituted for vote volatility.

In general, we do not know enough about the impact of voter behavior on representatives' behavior. Although the literature on retrospective assessment has explored the relationship from the perspective of voters' reaction to representatives' behavior, we cannot sufficiently situate this dynamic in the broader literature on representation without more thoroughly examining two additional steps in the process: do politicians get the message?³⁷ and are voters subsequently satisfied? As we have discussed, our provisional answer to these questions is "no", and this should prompt additional investigations along these lines.

Appendix. Empirical Model Specifics

Let $V_{t,c}$ and $C_{t,c}$ stand for Volatility and Corruption at time t in country c . Our model is defined by

$$\begin{bmatrix} V_{t,c} & C_{t,c} \end{bmatrix}' \sim MVN \left(\begin{bmatrix} \alpha_{Vol,c} + \phi_{12,c} V_{t-1} + \gamma_{21,c} C_{t-1} \\ \alpha_{Cor,c} + \phi_{21,c} C_{t-1} + \gamma_{12,c} V_{t-1} \end{bmatrix}, \begin{bmatrix} \sigma_{Vol}^2 & \rho \sigma_{Vol} \sigma_{Cor} \\ \rho \sigma_{Cor} \sigma_{Vol} & \sigma_{Cor}^2 \end{bmatrix} \right)$$

$$[\alpha_{Vol} \quad \alpha_{Cor} \quad \phi_{12} \quad \phi_{21} \quad \gamma_{21} \quad \gamma_{12}]_c' \sim MVN(\boldsymbol{\mu}, \boldsymbol{\tau}^{-1})$$

where the α , ϕ and γ are normally distributed random intercepts and slopes, respectively, and ρ is the (contemporaneous) correlation between volatility and corruption. All calculations and estimations were conducted in R v. 2.15.0, and the MCMC sampling was implemented in JAGS v. 3.3.0.

We give the $\boldsymbol{\mu}$ mean vector (which corresponds to the mean of the common distribution from which country-specific effects are believed to be drawn, and which is the vector we report in our tables) a single, flat multivariate Normal prior, with a mean hyperparameter vector $\mathbf{0}$ and a covariance hyperparameter \mathbf{I}_{6100} . In turn, we give the $\boldsymbol{\tau}$ precision a Wishart prior, with scale matrix \mathbf{I}_6 and prior degrees of freedom equal to 7. This set up induces a uniform prior distribution on the correlation between these random effects, while remaining agnostic regarding prior information about the mean values that govern the dynamics we are studying (Gelman and Hill, 2007). The precision matrix for the observation-level multivariate Normal was also given a Wishart prior, with scale matrix \mathbf{I}_2 and degrees of freedom equal to 3. We let the Gibbs sampler explore the parameter space for 5000 iterations, after which we record 10,000 samples from the reached posterior distributions.

16.4% of our time serial data is missing. Rather than listwise deleting these observations, we chose to use multiple imputation by chained equations to generate five complete datasets. We then use each of these datasets to obtain samples from the parameters' posteriors using two Markov chains per dataset. After evaluating convergence of these chains for each complete dataset, we randomly pick one set

of samples (i.e. one chain) for each of the 5 complete datasets (in order to keep memory requirements at a manageable size). We combine the samples derived from these five chains, evaluate convergence of all chains once again (all Gelman–Rubin statistics were well under 1.5, and Geweke statistics were smaller than 2 for the coefficients of interest), and proceed to use all these samples to obtain the point estimates and measures of uncertainty reported in tables and figures.

The procedure is effectively equivalent to Rubin's repeated imputation inference (1987), and only different from Tanner and Wong (1987) Data Augmentation algorithm in that we let $P(Y_{\text{miss}}|Y_{\text{obs}},\theta) = P(Y_{\text{miss}}|Y_{\text{obs}})$, allowing us to sample from the posterior predictive distribution of Y_{miss} independently of our samples from the posterior of the model's parameters. As a result, our procedure builds on these tested approaches, increasing our confidence in its adequacy. In general, and simply through the combination of independent chains, our strategy allows us to incorpo-

rate both within-imputation and between-imputation variances of the parameter estimates while keeping computation time and memory requirements minimal.

Finally, direct tests of Granger causality depend on the specifying the correct number of self lags for each of the outcome variables. When plotting the autocorrelation function for lags 0 through 4 (since this is the maximum amount of observations per country) of residuals corresponding to the corruption and volatility series (after a single self lag has been included in each equation) we find an indication that there is no significant autocorrelation (the ACF is calculated on countries that have enough observed histories in our dataset). This lends credibility to our choice of lag number, which is further justified by the fact that adding more lags does not significantly improve the fit of the model: adding an additional self lag reduces deviance by merely -8.63 , whereas adding 2 additional self lags reduces deviance by -7.71 (these models are based on fewer observations, however, as there are 8 countries for which there are fewer than 4 observations; analytically, however, all three models remain nested, which allows goodness of fit comparisons).

Appendix A. Supplementary data

Supplementary data related to this article can be found at <http://dx.doi.org/10.1016/j.electstud.2013.08.017>.

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³⁷ For excellent work on this question in the U.S. context, see Sulkin (2005) and Sulkin (2011).

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