Institutional Checks and Corruption: The Effect of Formal Agenda Access on Governance

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Abstract

Legislative checks give whoever wields them influence over policy making. We argue that this influence implies the ability not only to affect legislative content but also to direct public resources toward private ends. Rational politicians should use access to checks to make themselves better off, e.g., by biasing policy toward private interests or creating opportunities to draw directly from the public till. Disincentives exist only to the extent that those able to observe or block corruption do not themselves benefit from it. Political opponents thus can use checks to stymie each other, but legislative checks controlled by political allies create conditions for collusion and corruption. We find, testing our claim against data from a sample of 84 countries, strong support for our hypothesized relationship between institutional checks and corruption.

Keywords Veto players, checks, corruption, governance, panel data

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INTRODUCTION

Most analyses of the causes of corruption start with the presumption that the abuse of public office for private gain grows from voters' inability to rein in their representatives. In the face of government malfeasance, scholars ask whether and how political institutions affect it. Answers in the literature vary in specifics, often in contradictory ways, but generally boil down to a statement that corruption is lowest where political institutions give voters the wherewithal to punish politicians who fail to perform to expectations.[1]

We take a different tack. We treat the electoral connection as a background condition affecting legislative actors' incentives. The reasons we do so are two. In order to punish malfeasance, voters have to both know that it exists and be able to identify its source. Those involved, however, want to keep it hidden. Investigative reporters need leaks and leads, for instance, without which much information will remain hidden no matter how free the media are. Further, elites who know of bad behavior might not have any reason to reveal it. Indeed, and this is where we part from the existing literature, elites sometimes might be willing not only to tolerate others' bad behavior but also to take advantage of their influence to behave badly themselves.

We begin with an observation that leads into a question. Actors want to hide their malfeasance. Thus, the people best situated to know that policy makers or their agents are behaving badly are other participants in the policy process. The question is whether those who observe bad behavior do anything about it. The answer, we argue, depends on context.

A legislator who observes corruption can reveal it to others (i.e., voters) or otherwise oppose it if it is costly to her or if doing so gives her an advantage. If she gains nothing from opposing it, by contrast, she can ignore or tolerate it or use the same authority that allows her to *observe* corruption to participate in it. Elites with opposing preferences such that one's policy gain implies the other's policy loss should do all they can to block each other. The more elites share policy preferences or the less they have to gain by differentiating themselves from each other (Kam 2009; cf. Martin and Vanberg 2011), however, the more they should be willing both to cooperate publicly in policy making and either collude or at least tolerate each other's efforts in their quest for private gain.

The key institutional feature of our argument is legislative agenda authority, which we operationalize as legislative checks or, more loosely, vetoes—the formal

[1] For example, Montinola and Jackman (2002), Bäck and Hadenius (2008), and Charron and Lapuente (2010) have looked at how the level of democracy affects this principal-agent problem while Panizza (2001), Persson et al. (2003), Chang (2005), Kunicovà and Rose-Ackerman (2005), and Chang and Golden (2006) examine how electoral rules affect how accountable political candidates are to voters. Tavits (2007) explains that the principle-agent relationship is tightened in the presence of clear lines of responsibility for enacting anti-corruption policies (see also, Gerring and Thacker 2004). Evidence from the 2009 UK MPs' expenses scandal suggests, however, that even in the best of circumstances (and for reasons that are both unclear and beyond the scope of this paper) voters might not punish questionable behavior (Pattie and Johnston 2012; Vivyan et al. 2012).

authority to delay, block, or amend bills.[2] Checks provide influence over outcomes by injecting those who control them into legislative process (Tsebelis 1995; 2002; and cf. Gerring and Thacker 2004: 313). Tsebelis distinguishes theoretically between institutional checks, derived from formal rules of procedure that allocate authority to specific offices or bodies, and partisan checks, which grow from the preferences of those who control those institutional checks. Tsebelis's absorption rule, however, implies that institutional vetoes are meaningless when controlled by copartisans or close allies. Basically, the operating assumption is that copartisans hold identical preferences (Laver and Schofield 1990, ch. 2; but see Heller 2001; Laver and Shepsle 1990; 1999; VanDusky-Allen and Heller 2014). This approach thus sets aside the possibility of intraparty politics and as a consequence ignores a lot of interesting action.

We agree with Tsebelis's logic, but not the absorption rule. Our theoretical contribution lies in the assertion, which stands up well to empirical scrutiny, that legislative checks function *independent* from partisan concerns. *How* they function depends on veto players' preferences, as we show in the next section and as is to be expected if outcomes are a product of both institutions and preferences (Hinich and Munger 1997: 17; Plott 1991: 904-905). We test our argument using a new measure that focuses solely on formal, constitutionally defined legislative rules (Branduse and Heller 2013). This indicator counts the number of formal checks and weights them by the extent to which they can be weakened (as for example when the legislature can remove the executive from office or the executive can dissolve the legislature). We show, by separating partisan concerns from formal checks and drawing on a sample of up to 84 countries for the period 1984-2005, that stronger formal checks lead on average to higher levels of corruption. Our empirical results stand even after we account for a range of potentially important covariates and apply a host of robustness checks and, importantly, control for partisan competition by interacting checks with a seat-shares sensitive measure of parliamentary polarization.

INSTITUTIONAL CHECKS AND INDIVIDUAL SELF-INTEREST

The ability to keep agents in check is meaningless absent information about whom to check and when. Because corrupt agents might be able to keep their perfidy hidden, a more reliable foundation for good government requires that those who would like to see corruption punished be in a position to observe, report, or block undesirable behavior (Brown el al. 2011). These elements—effective monitoring and institutional checks—are fundamental to well-designed principal-agent relationships (Kiewiet and McCubbins 1991).

Checks facilitate monitoring, but they do not automatically make it effective. In most legislatures control over checks accrues to parties in the governing coalition. Whether actors who control checks are willing to reveal malfeasance should depend on how their fates are linked to the ability to cooperate with other veto players in passing legislation. To anticipate, we expect veto players who observe bad behavior by copartisans to want to keep it hidden; the incentive to turn a blind eye decreases for malefactors in other parties, but should be greater for coalition partners than for policy rivals or where survival in office is independent from the ability to pass policy (Shugart and Carey 1992).

[2] The power to propose bills, if solely held, also implies the power not to do so (i.e., "gatekeeping", Shepsle and Weingast 1987; or "negative agenda setting", Cox and McCubbins 2005).

Cooperation in politics, of which collusion in corruption or tolerance thereof can be one aspect, yields both costs and benefits. One clear benefit in the legislative context is the ability to pass bills, but that can come at a cost since cooperation among coalition partners makes it difficult for voters to distinguish them from each other (cf. Martin and Vanberg 2011), particularly for smaller parties (Fortunato and Adams 2015). Collusion also can yield benefits directly (e.g., overly liberal use of expense accounts, see for example Kelso 2009),[3] or indirectly (e.g., encouraging bureaucratic corruption to advantage allies or legislating benefits for friends). In some circumstances, moreover, failure to cooperate can be costly, as for example within a party or a coalition where individual behavior can both undermine collective benefits and incur disciplinary costs.

In this vein, the question of cooperation boils down to a set of costs and benefits. Define actor i's policy benefits from cooperation as her utility from policy proposal s, $u_i(s)w = -\|x_i - s\|$, where x_i is i's ideal policy, $\| \quad \|$ is the Euclidean norm, and w is a salience weight for the policy dimension or dimensions covered by s. Benefits from cooperation can extend beyond policy however, inasmuch as it manifests as unity that might enhance a party or coalition's reputation for efficacy. Define these additional benefits from cooperation as g. Further define b as side benefits—from over-liberal use of parliamentary expense accounts to legislative favors for friends to kickbacks—c as the electoral costs of cooperation, and d as the costs of noncooperation. Thus, i's payoff from cooperating with j is $u_i(s) + g_{ij} + b_i - c_{ij}$ and her payoff from not cooperating is $u_i(q) - d_{ij}$, where q is the reversionary policy that obtains absent legislative action. Here, i prefers to cooperate with j if and only if

$$u_i(s) + g_{ij} + b_i - c_{ij} > u_i(q) - d_{ij}. \tag{1}$$

Benefits and costs are not constant, but rather should vary depending on the relationship between i and j. Specifically:

- g_{ij} increases as the policy distance $\|x_i - x_j\|$ between i and j decreases and is highest for copartisans and lowest for members of parties with opposing policy positions;
- c_{ij} increases with policy distance $\|x_i - x_j\|$, which is easily observed, and for self-serving behavior (which yields payoffs b_i) *when observed*;
- b_i is increasing in g_{ij} and, inasmuch as there is a tradeoff between policy goals and rent seeking, decreasing in $u_i(s)$ and w; and
- d_{ij}, like b_i, increases with g_{ij} and decreases with $u_i(s)$.

If i doesn't care about the policy dimensions in question ($w = 0$) or there is no bill on the table—so that $u_i(s) = u_i(q)$—then Equation (1) reduces to

$$g_{ij} + b_i > c_{ij} - d_{ij}, \tag{2}$$

which will be true as long as c_{ij} is not too large. In the extreme case, for example, of the classic view of the US Congress where partisan concerns are irrelevant and legislators engage in universalistic logrolls (Weingast 1979) designed to protect their electoral prospects and access to legislative resources, all legislative activity, while not

[3] We thank an anonymous reviewer for highlighting this particular form of corruption.

necessarily *illegal*, is directed at benefiting incumbents (Katz and Mair 1995; Mayhew 1974).[4]

Actors i and j in this model are individuals with influence over legislative output, whether for the public good or their own. They are veto players; if they were not (unless they somehow were able to exercise influence over veto players), their preferences would be irrelevant to legislative output. Where they want distinctly different policies, they should be less willing to engage in horse trading over either legislative content or side payments and rents. Where policy differentiation is less of an issue, as in particular for copartisans or in matters divorced from policy considerations, such as the 2009 MPs' expenses scandal in the UK or the 1992 US House banking scandal, colluding for personal benefit become more attractive. By implication, formal institutional checks in the absence of partisan or programmatic concerns should, all else equal, increase the incidence of malfeasance (cf. Diermeier and Myerson 1999 with respect to the creation of legislative "hurdles" for extracting resources from lobbyists). In formal terms, $\|x_i - x_j\| \to 0$ and in the limit Equation (2) reduces to $g_{ij} + b_i > -d_{ij}$, which is always true.

We focus on constitutional checks in the legislative process[5] because a state's resources are as a rule allocated legislatively. The key elements of these checks are defined by the number of legislative chambers, the role of the executive in the legislative process, and executive-legislative relations (cf. Shugart and Carey 1992), where one actor or set of actors' ability to impose costs on another—as for example when a president can dissolve the legislature—should affect the latter's willingness to use formal checking authority. Opportunities for realizing rents from the policy process come from influence over the process. Such influence is most obviously rooted in the ability to stop the process, i.e., by controlling legislative checks. The power to block corruption, in a word, also implies the power *not* to block it.

Members of the political opposition of course would like any evidence of misbehavior in government to come to light. Similarly, evidence of malfeasance is the kind of news a free press thrives on. But willful malfeasance is hard to spot. Opposition parties *might* discover and reveal corruption, but they are not well-placed to do so; investigative reporters *might* find evidence of wrongdoing, and our empirical analysis does indicate that a free press correlates with better governance, but they are most likely to do so when pointed in the right direction by someone in the know (see, e.g., Kelso 2009: 333). Outsiders might discover evidence of wrongdoing, but the less those involved are willing to reveal it the harder it will be to see.

No one who wields power—and vetoes are a source of power—can credibly commit to do anything but use it to her own advantage (see, e.g., Laver and Shepsle 1996; Osbourne and Slivinski 1996; Shepsle 1979; 1991). This is true for partisan rivals and copartisans alike, as well as in a regime ostensibly without parties. We argue that legislative checks give actors leverage to make themselves better off in the process of passing legislation (Heller 1997; cf. Osbourne and Rubinstein 1990; see also Tsebelis and Money 1997).

When players compete over policy, following veto-player theory and as argued in most of the literature, we agree that corruption should be rare. But shared

[4] The 2009 MP expenses scandal in the UK is similar, inasmuch as expansive use of expense accounts, even if improper, was widespread and generally accepted among MPs (see, e.g., Kelso 2009: 331-332).

[5] We leave extending our argument into the minutia of legislative rules for future work.

partisanship should not render checks inoperative. Where a single party controls multiple veto points the *individuals* who operate the levers of power—the veto players—cannot be assumed to set aside their own self-interest. We expect them to use their institutional authority to increase their own influence in intraparty decision making. They thus can undermine the quality of governance not only by blocking good policy (Gerring and Thacker 2004), but also by letting bad policy pass or using their leverage in the process to direct government resources to their own ends. This observation straightforwardly yields the following hypothesis:

Hypothesis 1: *All else equal, the number of institutional legislative checks should be positively correlated with corruption.*

All else rarely is equal, of course. A relatively polarized competitive regime where incumbent veto players face a high probability of being replaced by partisan rivals—who then might discover their predecessors' malfeasance—offers incentives very different from one where opposition parties cannot expect to control checks of their own. Where competing veto players face either political rivals who also control institutional checks in the legislative process or a high probability of being replaced by partisan rivals, as shown in Figure 1, anticipation of punishment ex post should keep improper collusion relatively low. That said, however, our argument suggests that where there are many checks and rivalry (or polarization) across veto players is low—the lower left cell in Figure 1—the opportunities and potential gains from malfeasance should be high and, at least probabilistically, we should be able to observe relatively high levels of corruption.[6] Where polarization is low and checks are few (the lower right cell of Figure 1), by contrast, the combination of fewer veto players in a position to leverage their authority for personal benefit *and* increased clarity of responsibility (Tavits 2007) should yield lower levels of corruption, even though relatively tame competition might lower the expectation of punishment and so keep some level of corruption attractive.

		Institutional Checks	
		Many	Few
Polarization	High	Corruption low	Corruption low
	Low	Corruption middling to high	Corruption low to middling

Figure 1: Legislative Partisan Differences, Institutional Checks, and Expected Corruptiion

Figure 1 adds nuance to Hypothesis 1 and indicates that the effect of checks should vary depending on the nature of and level of polarization embedded in political competition. Specifically, in democratic regimes—political competition and the existence of a viable opposition capable of replacing the government being a *sine qua non* of democracy (Cheibub et al. 2010)—we would expect corruption to be highest

[6] We stress here the "all else equal" nature of our argument. Any number of factors might affect a country's effective baseline level of corruption, e.g., banking laws, financial disclosure laws, or whether the economic or political system rewards funneling resources to teams of friends or supporters (cf. Wolf and Hansen 1967).

when checks are many and partisan differences are few so that polarization is low. This yields

Hypothesis 1a: *Institutional legislative checks increase corruption in democracies when polarization is low and decrease it when polarization is high.*

This hypothesis suggests an interaction between the number of checks and polarization. Any non-zero level of polarization in government of course requires that the government comprise at least two parties.

Our approach is related to, but distinct from, previous efforts that have explored how veto player interactions relate to corruption. Gerring and Thacker (2004) also argue that politically decentralized and presidential systems increase corruption, but we differ from them in two key respects. First, we focus on *legislative* process, leaving federalism out of the analysis. Second, we incorporate Tsebelis's (2002) theoretical veto-players framework, not least in the utilization of an aggregated measure of veto points, though our conception of checks is more expansive than strict vetoes. Lederman et al. (2005) argue that institutions, by creating lines of accountability, structure incentives for good behavior. They do not consider whether accountability might work better in some contexts than in others. Shleifer and Vishny (1993) argue that the level of corruption will be higher the greater the number of veto players (they call them monopolists), assuming that the veto players ignore each other and focus on maximizing their private gains independently (see also Brennan and Hamlin 1994). Their veto players are independent public administrators, however, decidedly different from actors in the legislative process. Andrews and Montinola (2004), by contrast, suggest that increasing the *total* number of veto players (not just in the legislative arena) makes collusion more difficult and reduces actors' capacity to collude to accept bribes, which in turn should reduce corruption. That we arrive at the opposite conclusion is neither surprising nor necessarily inconsistent because, again, we restrict our analysis to institutional *legislative* checks while they count all checks in a system. Legislating is by nature a collective endeavor, in which independent actors get nowhere; but actors who are willing to abide each other's ethical lapses stand to benefit personally. If anything, collusion helps make the collective activity of legislating possible. Cooperation should indeed be more difficult outside the legislative arena, as Andrews and Montinola show, because extra-legislative veto players (such as courts and subnational governments) are both much more easily characterized as independent from each other and, importantly, better equipped to accept or reject decisions made in the national legislature than to amend their content.

To test our hypothesis we need a measure of institutional or formal checks distinct from partisan preferences. Ideally, a measure of checks also should capture the "security" of veto players' authority, as this is likely to affect their capacity to hold out for personal gain. Existing empirical work on the impact of checks on governance employs measures that conflate formal and partisan checks (for a full discussion see Branduse and Heller 2013). For instance, Panizza (2001) uses Henisz's (2000; 2002) POLCON index, which collapses to 0 if all veto points are controlled by a single party, thus operationalizing the presumption that checks are irrelevant in such contexts. Panizza (2001) finds that the presence of veto points controlled by political rivals is associated with better governance. Andrews and Montinola (2004) build on data from the Database of Political Institutions (DPI; Beck et al. 2001) and party seat shares from various sources to generate a measure of veto players that combines both institutional and partisan influences, which they use to show that more veto players lead to higher levels of rule of law. And Brown et al. (2011) account for rival veto points by way of the ideological distance or polarization among the largest parties and the executive and find that the partisan divide seems to keep corruption under control. In all cases the measure of veto points or checks is defined in terms of partisan competition. This is tantamount to assuming that having institutional checks controlled by a single party is equivalent to having no checks at all.

We thus turn to a new measure of checks which accounts for both formal checks and, significantly, factors such as the ability to call new elections that might make veto players reluctant to use their authority. This measure is invariant with respect to electoral outcomes or coalitional composition—i.e., it explicitly ignores Tsebelis's (1995) partisan veto players and absorption rule—capturing formal institutional checks, not partisan ones. The measure is essentially additive, but weights specific checks by their exposure to "censure" authority, i.e., players formally endowed with the ability to impose costs on veto players who cross them. The ability of the executive to block legislation thus amounts to less when he can be removed from office by the legislature, for example, and the ability of the legislature to act against the executive's desires is weakened when the executive unilaterally can call new elections (cf. Shugart and Carey 1992).

DATA AND EMPIRICAL APPROACH

Our measure of formal constitutional agenda powers, *instchecks*, comes from Branduse and Heller (2013). The underlying idea is to count checks, but weight them according to the institutional relationship between those who wield checks and those whom they might affect. This is consistent with the well-known finding that the ability of governments in parliamentary systems to link the passage of bills to the survival of the government or the entire legislature makes it more difficult for parliaments to reject government proposals (Huber 1996). To this end, Branduse and Heller employ twelve constitutional-level variables (all dummies) from the Institutions and Elections Project (IAEP; Regan, Frank and Clark 2009). They combine these variables into a raw indicator x, which counts checks and weights them according to the possible costs of using them, as follows:

$$x = legcham$$

number of legislative chambers, important because even weak second chambers can affect legislative outcomes (Heller 2007; Heller and Branduse 2014; Tsebelis and Money 1997)

$$+\frac{execveto^2}{1 + execveto + removexec}$$

executive ability to veto legislation is weakened if legislature can remove executive

$$+\boldsymbol{legveto} * ([\mathbf{1} - \boldsymbol{callpm}] + \boldsymbol{callnone} + [\mathbf{1} - \boldsymbol{callpres}])$$

legislature's ability to block executive actions is weakened if executive can call new elections

$$* \boldsymbol{removeexec} * ([\mathbf{1} - \boldsymbol{removeleg}] + [\mathbf{1} - \boldsymbol{legpres}] + [\mathbf{1} - \boldsymbol{legpm}])$$

executive ability to initiate legislation is weakened if legislature can remove the executive is weakened, a power that is itself weakened if the executive can retaliate in kind

$$+\boldsymbol{removeleg} * ([\mathbf{1} - \boldsymbol{legveto}] + [\mathbf{1} - \boldsymbol{legpres}])$$

executive ability to dissolve the legislature weakens legislative veto ability but executive cannot initiate legislation if legislature is dissolved[7]

$+\boldsymbol{execindep} * ([\mathbf{1} - \boldsymbol{removeexec}] + \boldsymbol{legpres} + [\mathbf{1} - \boldsymbol{removeleg}])$

executive chosen independently from the legislature strengthens executive ability to initiate legislation but is weakened if executive or legislature can remove the other

$+legpres * ([1 - callpres] + removeexec)$

legislature's ability to remove the executive is more important if executive can initiate legislation but weakened if executive can call new elections

$+legpm$

executive ability to propose legislation is an institutional check, by our definition

$-exleg$

executive ability to check or punish legislature is weakened if executive is member of legislature

$+callnone$

vetoes are strengthened/less costly to wield if no one can call new elections

$+callpres * (1 - legpres)$

president's ability to call new elections negates potential vetoes to president's legislative proposals

Because this method can overcount checks, Branduse and Heller then create a 0-1 index by taking the result and transforming it to $instchecks = 1 - \frac{1}{x}$. The transformation reduces the weight of each additional check.[8]

Tables A1 and A2 in the online appendix present, respectively, the summary statistics and data definitions and sources of all the variables employed in the analysis. Our *instchecks* indicator is available over the period 1984-2005. It ranges from 0.700 (for Hungary in 1990; other countries in our sample with weak institutional checks include Finland, Greece, Mozambique, Sweden, and Tanzania) to 0.919 (Latin American presidential systems like Colombia, Chile, and Brazil) with a mean value of 0.856.[9] Table A4 shows mean, minimum, and maximum values for *instchecks* for all

[7] The IAEP dataset includes measures of both executive ability to call new elections and to dissolve the legislature. The two are in principle different, e.g., an appointed legislature can be dissolved with no new elections necessary. To the extent that the measures are the same the result would be an overcount of checks, which should bias results against our argument.

[8] We ran our estimations using the raw count as well as the transformed *instchecks* variable we report. The results are essentially the same (after taking into account the order-of-magnitude difference between the two).

[9] The range appears small, but recall that the marginal effect of each new check in the index is declining, by construction. The range for the untransformed variable is 3.333 to 12.333.

countries in our sample. The difference between our measure and other commonly used indicators is brought home by the correlations among them: *instchecks*-CHECKS, 0.219; *instchecks* and POLCON, 0.268; CHECKS-POLCON, 0.602. Clearly the three indicators are measuring checks in different ways (see, Table A3 of the online appendix).

To measure corruption we draw from the International Country Risk Guide (ICRG) as published by the Political Risk Services Group and the World Governance Indicators (WGI) from the World Bank (Kaufmann et al. 2006). The ICRG corruption measure captures in-house experts' perceptions of actual or potential corruption. It ranges from 0 to 6 and higher values reflect a lower perceived risk of corruption or, in other words, perceptions that the government is cleaner. Another measure of corruption is the World Bank's Control of Corruption indicator which varies from -2.5 to +2.5. Again, higher values indicate stronger control of corruption. The ICRG measure is available since 1984 while the World Bank measure was first published for 1996. It is for this reason that we mostly rely on the ICRG measure in our empirical analysis and employ the World Bank measure for robustness purposes. According to the ICRG, the countries in our sample with the lowest risk of corruption are Canada, Denmark, Finland, Netherlands, New Zealand, Norway, Singapore, and Sweden.

Hypothesis 1 suggests a fairly straightforward test, which we estimate as follows:

$$Corruption_{it} = \alpha_1 + \alpha_2 instchecks_{it} + \alpha_3 X_{it} + U_{it}$$

Where *instchecks* represents our measure of constitutional checks, *X* is a vector of control variables and *U* is the error term. Given our previous discussion we expect $\alpha_2 < 0$, i.e., the presence of stronger constitutional checks should worsen corruption. Our data take a time-series–cross-section structure covering some 84 countries with up to 22 time periods, so we estimate the model using Beck and Katz (1995) panel-corrected standard errors. We run the estimation both with a lagged dependent variable (as recommended by Beck and Katz 1996) and without. We include as well a time-trend indicator (*year)* to account for the influence of unknown or unobservable factors evolving over time and affecting all our cross-section units.

We control for partisan preferences (Brown et al. 2011; and cf. Tsebelis 1995). The available measure for the full sample is POLARIZ from the DPI dataset, which measures the maximum ideological difference between the chief executive's party and the four largest parties of the legislature based on seat shares (see also, Keefer and Stasavage 2003).[10] This measure is problematic, however: It is, for example, a constant 2 for Denmark for the entire period covered by the data, where we think the dynamics of multiparty competition should *reduce* polarization. Similarly, the DPI polarization measure for Canada is 0 until it jumps to 2 in 2005, indicating that polarization increased dramatically when the government went from majority to minority status. This seems unrealistic. Thus, to test for an interaction effect between *instchecks* and polarization in competitive democracies, in line with Hypothesis 1a, we turn to a more appropriate, fine-grained and clearly interpretable polarization-in-parliament measure calculated from party positions as measured in the Manifesto Project Dataset (MPD;

[10] DPI polarization is coded zero if the executive's party controls an absolute majority in the legislature or if elections are deemed "uncompetitive"; one if elected bodies only feature center–left or center–right representation among the largest parties; and two in states featuring a large left and right wing presence among elected officials.

Budge et al. 2001; Klingemann et al. 2006). This MPD polarization measure ranges between 1.4 and 2.9 for Denmark in our data and between 1.144 and 2.332 for Canada, with the highest values corresponding to the late 1980s and early '90s. We use the DPI polarization measure as a control for models using the full sample—the MPD measure covers far fewer cases—but the MPD measure is both a better indicator of intraparliamentary competition and more appropriate for our argument. This measure, specifically designed to get at "the quality of party competition" (Dalton 2008: 900) and adapted to MPD seat-share data, is calculated as $\sqrt{\sum_{i=1}^{n}\left(s_i \frac{[RILE_i - \overline{RILE}]^2}{100}\right)}$, where s_i is party i's seat percentage and $RILE$ is the MPD right-left score.[11] For the estimations using interaction effects we are limited to countries included in the MPD, reducing our sample to 24 countries, all democratic, but covering the same number of time periods.

Finally, we control for the effective number of parties in government. The notion of partisan veto players (Tsebelis 1995; 2002) indicates that more parties in government implies more checks, which our argument suggests should increase corruption for parties with similar goals and decrease it as their goals diverge. Kunicová and Rose-Ackerman (2005) have argued that party competition is strongest (and thus incentives to collude weakest) in two-party systems where the opposition has a credible chance of winning the election. Moreover, the multiplicity of parties characteristic of proportional-representation (PR) systems may create incentives for tolerating malfeasance. The problem as they see it comes in two parts. First, parties that reveal corruption are at a disadvantage when it comes to forming coalitions with parties they have exposed (parties, in other words, hold grudges). Second, a collective dilemma emerges if exposing one party's malfeasance can benefit any or all other non-exposed parties, not necessarily solely or even primarily the party providing the information. This leads to the claim that a large number of parties in parliament reduce incentives to reveal bad government. Charron (2011) tests for this in a sample of democratic and semi-democratic countries. He constructs a dummy variable differentiating legislatures with less than three effective parties from legislatures with more than three parties. He finds that corruption is higher in countries in the latter category. To control for the number of parties in government in the full-sample estimations in Table 1, we use the inverse of the DPI's Herfindahl index of government (HERFGOV); for the estimations in Table 2, we calculate the effective number of parties in government using seat totals from the MPD. The correlation between the two ranges from 0.766 to 0.840, depending on the sample.

In line with previous work on the determinants of corruption we include a number of standard control variables. We control for real GDP per capita and the size of government because wealthier countries both can afford better government quality and face citizen demands for good governance (Islam and Montenegro 2002; La Porta et al. 1999; Treisman 2000; Van Rijckeghem and Weder 2001), while a large public sector implies greater institutional capacity as well as greater scope for diverting public funds (Tanzi 1998). We control for ethno-linguistic fractionalization on the strength of the

[11] Running the models using raw left-right party positions from the MPD yields essentially the same result. Results available on request.

possibility that voters are more likely to support corrupt politicians from their own ethnic group in exchange for public benefits (La Porta el al. 1999; Kimenyi 2006).

We also control for the level of democracy, since democracy is likely to reduce corruption by tightening principal agent relationships. First, it increases the likelihood of alternation in office of political parties or—in polities with dominant political parties—of individual leaders within parties (Montinola and Jackman 2002). Second, it strengthens incentives of political elites to reveal information on malfeasance by opponents (Treisman 2000). We consider the possibility of a quadratic relationship between democracy and corruption in line with Bäck and Hadenius (2008), who have suggested that the control of public officials is lowest at intermediate levels of democracies, higher in the context of dictatorships because of top-down hierarchical control and highest in developed democracies because press freedoms and electoral participation facilitate bottom-up control. Like these authors, we account for democracy using the Polity IV DEMOC measure (Marshall et al. 2010).

The capacity of voters to discipline malfeasant politicians also might depend on electoral rules (see Persson et al. 2003; Kunicová and Rose-Ackerman 2005; Chang and Golden 2006). We thus control for plurality or majoritarian electoral rules and district magnitude (which Persson et al. 2003 argue could reduce corruption; and Chang and Golden 2006 argue might increase it). We control for electoral rules using a DPI measure of mean district magnitude for the lower or sole chamber and a dummy variable for plurality rules.[12]

Finally, in light of arguments addressing the relationship between executive type and political accountability (e.g, Kuniková and Rose-Ackerman 2005; Moe and Caldwell 1994; Persson et al. 1997; Shugart 1999), we control for regime type. To this end we use the DPI SYSTEM variable, equal to 0 for presidential systems, 1 if there is an assembly-elected president, and 2 for parliamentary systems. Importantly for our purposes, strong presidential systems are characterized by fixed terms and restrictions in replacing a president mid-term; other things equal, this should worsen the risk of malfeasance (cf. Shugart and Carey 1992). By our measure of checks presidential systems like those in Latin America score high, so the regime-type control allows us to account for any impact of presidential systems on corruption by means distinct from those captured by our measure of formal checks.[13]

EMPIRICAL FINDINGS

Table 1 presents our basic results. We first discuss models 1-4, full-sample estimations testing for the effect of constitutional checks in the legislative process on corruption. Models 5-6 are for a restricted sample of democracies only. We checked for robustness by including and withholding, separately, the lagged dependent variable and a quadratic *Democracy* term (Bäck and Hadenius 2008). The coefficient for our *institutional checks* indicator is negative, as predicted, and strongly statistically

[12] We checked as well for the impact of list type (open or closed; see Kunicová and Rose-Ackerman 2005; Chang and Golden 2006) using a measure from the DPI, but found no effect. More importantly, as reported in the next section, the estimated impact and significance of *instchecks* does not change.

[13] The simple correlation between the institutional checks index and the DPI SYSTEM variable is only -0.3589, indicating that *instchecks* is not proxying for presidential systems.

Table 1: Panel Estimates (1984-2005)

	Full Sample				Democracies	
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**
Institutional checks	**-8.287** (0.573) *0.000*	**-0.817** (0.388) *0.035*	**-8.224** (0.560) *0.000*	**-0.844** (0.392) *0.031*	**-9.259** (0.684) *0.000*	**-1.460** (0.492) *0.003*
Polarization	**0.281** (0.048) *0.000*	**0.037** (0.019) *0.047*	**0.243** (0.045) *0.000*	**0.034** (0.019) *0.071*	**0.281** (0.054) *0.000*	**0.051** (0.021) *0.014*
Eff. parties in gov't	-0.040 (0.029) *0.162*	-0.004 (0.014) *0.760*	**-0.057** (0.030) *0.057*	-0.006 (0.014) *0.654*	**-0.120** (0.035) *0.001*	-0.013 (0.019) *0.500*
GDP per capita (log)	**0.424** (0.031) *0.000*	**0.033** (0.019) *0.079*	**0.348** (0.036) *0.000*	0.026 (0.017) *0.133*	**0.451** (0.041) *0.000*	0.052 (0.036) *0.152*
Government size	-0.424 (0.345) *0.219*	0.050 (0.239) *0.834*	-0.392 (0.332) *0.238*	0.052 (0.239) *0.826*	**-0.690** (0.384) *0.072*	-0.199 (0.242) *0.410*
Ethnic fractionalization	**0.761** (0.076) *0.000*	0.025 (0.052) *0.629*	**0.791** (0.073) *0.000*	0.032 (0.054) *0.560*	**0.921** (0.119) *0.000*	0.057 (0.089) *0.520*
District magnitude	0.001 (0.001) *0.264*	0.000 (0.001) *0.918*	**0.002** (0.001) *0.012*	0.000 (0.000) *0.722*	**0.003** (0.001) *0.000*	0.000 (0.000) *0.501*
Plurality	0.077 (0.058) *0.181*	**0.067** (0.028) *0.016*	0.073 (0.054) *0.175*	**0.066** (0.028) *0.017*	0.111 (0.069) *0.107*	**0.085** (0.032) *0.008*
Regime type	**0.132** (0.040) *0.001*	0.003 (0.025) *0.894*	**0.081** (0.039) *0.039*	-0.002 (0.025) *0.941*	-0.006 (0.053) *0.907*	-0.031 (0.030) *0.307*
Democracy	**0.099** (0.014) *0.000*	**0.014** (0.007) *0.036*	**-0.112** (0.033) *0.001*	-0.008 (0.015) *0.573*	**0.262** (0.026) *0.000*	**0.043** (0.015) *0.005*
Democracy^2			**0.023** (0.003) *0.000*	**0.003** (0.002) *0.098*		
Year	**-0.074** (0.008) *0.000*	**-0.014** (0.004) *0.001*	**-0.072** (0.008) *0.000*	**-0.014** (0.004) *0.001*	**-0.087** (0.008) *0.000*	**-0.019** (0.005) *0.000*
Lagged corruption		**0.899** (0.030) *0.000*		**0.895** (0.031) *0.000*		**0.874** (0.038) *0.000*
Adjusted R^2	0.614	0.928	0.629	0.928	0.706	0.933
N obs	1052	1020	1052	1020	708	684
N countries	84	84	84	84	56	56

Panel corrected standard errors in parentheses; p-values in *italics*; coefficients with $p<0.1$ in **bold.** All regressions include a constant (not shown). The dependent variable is the ICRG control of corruption measure. Polarization is the POLARIZ measure from the Database of Political Institutions. Countries in the full sample (models 1-4) include: Albania, Angola, Argentina, Australia, Austria, Azerbaijan, Bangladesh, Belgium, Bolivia, Botswana, Brazil, Bulgaria, Burkina Faso, Cameroon, Canada, Chile, Colombia, Congo, Costa Rica, Cote d'Ivoire, Denmark, Ecuador, Egypt, El Salvador, Finland, Gabon, Gambia, Ghana, Greece, Guatemala, Guinea, Honduras, Hungary, India, Indonesia, Iran, Iraq, Ireland, Israel, Italy, Jamaica, Japan, Jordan, Kazakhstan, Kenya, Latvia, Lebanon, Liberia, Lithuania, Madagascar, Malawi, Mali, Mexico, Moldova, Mongolia, Morocco, Mozambique, Namibia, Netherlands, New Zealand, Niger, Nigeria, Norway, Panama, Paraguay, Peru, Philippines, Senegal, Sierra Leone, Singapore, Slovakia, Slovenia, Sri Lanka, Sweden, Syria, Tanzania, Thailand, Togo, Tunisia, Turkey, Uganda, Uruguay, Venezuela, and Zambia.

significant. The coefficient is, unsurprisingly, smaller with the lagged dependent variable included (models 2 and 4), but still significant. The DPI political polarization variable (Keefer and Stasavage 2003; Brown et al. 2011) also is significant and, as expected, reduces corruption. The effective number of parties in government, by contrast, has a negative effect but is significant only in Model 3. In short, increasing the number and strength of constitutional legislative checks leads to higher levels of corruption, just as we hypothesize. This effect is substantively important: taking the smallest coefficient for *institutional checks* from Table 1 (Model 2), a one standard deviation increase in institutional checks increases corruption perceptions by 27 percent of a standard deviation. A comparable increase in polarization raises perceptions of clean government by nearly 24 percent. Regressions 5 and 6 in Table 1 test the robustness of our *institutional checks* measure to a restricted sample of 56 democratic countries as defined by Cheibub et al. (2010). As can be seen, the results stand and indeed are stronger despite the substantially reduced sample size.

The regressions in Table 2 more directly test the claim in Hypothesis 1a by interacting our checks variable with a polarization-in-parliament variable calculated using data from the MPD. Focusing only on models 2 and 4 (with the lagged dependent variable), two things stand out. First, our checks measure is negative, as expected, significant, and suggests a substantively meaningful effect. Second, while the interaction term itself is not significant, a closer look at the marginal effect of *institutional checks* in Figure 2 nicely and definitively supports our Hypothesis 1a.[14] Increasing the number of checks in the absence of political polarization increases corruption, an effect that fades as polarization increases. At the minimum level of parliamentary polarization in the sample, a one standard deviation increase in *instchecks* yields an 11.9 percent standard deviation increase in corruption (p-value 0.004); when polarization is at two standard deviations, a one standard deviation increase in *instchecks* still increases corruption, albeit less sharply, by 8.6 percent of a standard deviation (p-value 0.001). At 2.589 standard deviations of polarization measure the effect of *instchecks* is smaller still, at 4.6 percent, (p value 1.00); at higher levels of polarization checks cease to have a significant effect on corruption.[15] In terms of veto-player theory, the implication is stark: partisan differences matter for how vetoes work, but to ignore vetoes in the absence of those differences will lead to erroneous and—for efforts to fight corruption—harmful inferences.

We further check the robustness of our results in several ways. First, we include in the regressions two alternative measures of veto points discussed above—POLCON (Henisz 2000; 2002) and DPI CHECKS—to ensure that the estimated impact of institutional checks on corruption is not picking up the influence of rival political parties or partisan preferences in the legislative process. Our results are maintained for models using the DPI polarization measure to test Hypothesis 1 and using the MPD-derived

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[14] The fact that the interaction term is not statistically significant does not mean that marginal effect is not significant for substantively relevant values of the modifying variable (Brambor et al. 2006)

[15] Recall that our sample includes only democracies included in the Manifesto Project Dataset. Marginal effects without the squared *Democracy* variable are essentially unchanged; they are much more striking without the lagged dependent variable, but yield the same inference with respect to institutional design.

Table 2: Panel Estimates (1984-2005) with Interaction

	Model 1	Model 2	Model 3	Model 4
Institutional checks	**-22.247** (2.730) 0.000	**-4.342** (1.655) 0.009	**-22.739** (2.763) *0.000*	**-4.440** (1.655) *0.007*
Polarization	**-5.726** (1.264) 0.000	-0.838 (0.643) 0.192	**-6.121** (1.253) *0.000*	-0.895 (0.634) *0.158*
*Institutional checks*Polarization*	**7.173** (1.504) 0.000	1.062 (0.768) 0.167	**7.641** (1.494) *0.000*	1.129 (0.759) *0.137*
Eff. parties in gov't	-0.066 (0.041) 0.105	0.015 (0.025) 0.562	-0.054 (0.039) *0.168*	0.016 (0.025) *0.520*
GDP per capita (log)	**2.045** (0.192) 0.000	**0.396** (0.165) 0.016	**2.015** (0.207) *0.000*	**0.396** (0.165) *0.016*
Government size	**4.693** (1.394) 0.001	0.617 (0.805) 0.444	**5.479** (1.422) *0.000*	0.718 (0.801) *0.371*
Ethnic fractionalization	**1.624** (0.157) 0.000	**0.248** (0.114) 0.029	**1.545** (0.153) *0.000*	**0.243** (0.112) *0.030*
Democracy	0.132 (0.112) 0.238	0.007 (0.057) 0.905	**-4.057** (1.595) *0.011*	-0.450 (1.032) *0.663*
Democracy^2			**0.235** (0.090) *0.009*	0.026 (0.058) *0.657*
District magnitude	**0.006** (0.001) 0.000	0.001 (0.001) 0.266	**0.005** (0.001) *0.000*	0.001 (0.001) *0.283*
Plurality	**-0.239** (0.066) 0.000	-0.029 (0.044) 0.501	**-0.194** (0.069) *0.005*	-0.025 (0.044) *0.574*
Regime type	-0.201 (0.170) 0.238	-0.108 (0.080) 0.177	-0.124 (0.182) *0.496*	-0.100 (0.080) *0.213*
Year	**-0.120** (0.009) 0.000	**-0.022** (0.006) 0.000	**-0.118** (0.009) *0.000*	**-0.022** (0.006) *0.000*
Lagged corruption		**0.850** (0.043) 0.000		**0.848** (0.044) *0.000*
Adjusted R^2	0.679	0.918	0.685	0.918
N obs	375	367	375	367
N countries	24	24	24	24

Panel corrected standard errors in parentheses; p-values in *italics*; coefficients with p<0.1 in **bold.** All regressions include a constant (not shown). The dependent variable is the ICRG control of corruption measure. The polarization measure is constructed using data from the Manifesto Project Dataset. Countries in the sample include: Australia, Austria, Belgium, Bulgaria, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Latvia, Lithuania, Netherlands, New Zealand, Norway, Poland, Slovakia, Slovenia, Sweden, Switzerland, and Turkey.

polarization indicator to test Hypothesis 1a (see Table A5 and figures A1 and A2 in the online appendix). Recognizing that the degree of press freedom should affect the extent to which information on corruption reaches voters, we controlled for newspaper circulation per 1000 people (see Adserà et al. 2003). We also ran our estimations using an alternative measure of corruption from the World Governance Indicators. While our results are maintained for Hypothesis 1, they cease to be significant or meaningful for Hypothesis 1a, likely due to the fact that crossing the MPD-derived measure of polarization with the newspaper circulation indicator or the alternative measure of corruption cuts our sample by about half to between 143 and 149 observations.[16]

[16] Results available on request.

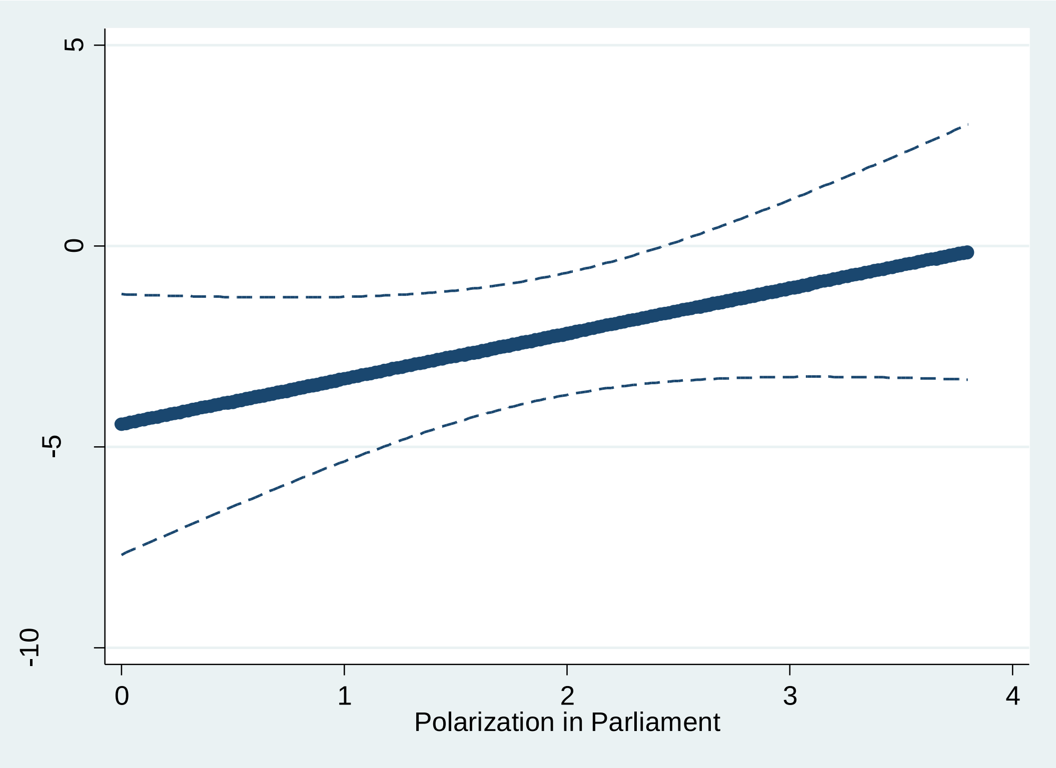

Figure 2: The Marginal Effect of Institutional Checks on Corruption

CONCLUSION

The principal-agent perspective highlights corruption as agency loss. Where the principal is the voting public, corrupt agents use public resources for their own ends. Most scholarship on corruption builds on this observation to focus on, on one hand, voters' ability to hold officials accountable and, on the other hand, the role of political competition as measured through the existence of partisan checks in limiting agents' discretion to act on their own behalf.

We build on this characterization of corruption as a principal-agent problem and hone in on the role of elites. Arguments that elites might collude to benefit themselves at the expense of their erstwhile principals are not difficult to find (particularly in the American politics "iron-triangles" literature on collusion among congressional committees, executive agencies, and they special interests they are supposed to oversee; see, e.g., Heclo 1978; and for a comparative-politics perspective, see Katz and Mair 1995). The question boils down to the age-old one of *quis custodiet ipsos custodies?* And though political scientists tend to respond with checks, a better response is *balanced* checks—that is, checks in the hands of rivals. James Madison put it well, in *The Federalist* 51, when he wrote that "ambition must be made to counteract ambition." When ambition leads to collusion, as we have shown, checks instead are counterproductive. Our point is that checks matter, but whether they matter for good or ill depends on who is doing the checking. Thus, while we take exception to the notion that checks matter only when controlled by opposing parties, we agree wholeheartedly that the preferences of those who wield checks—veto players—matter. They matter as much (or even more) when veto players agree as when they disagree, but they matter differently.

When institutions are controlled by rivals, i.e., when they effectively comprise partisan checks, veto players check each other; when institutions are controlled by allies, by contrast, veto players collude. All else equal, agenda power provides those who wield it with the wherewithal to direct public resources to private ends. Whether they do so depends in no small measure on whether agenda power is shared and, if it is, how misdirecting public resources affects those who share it. In short, the multiplication of institutional checks increases opportunities for corruption, but populating those checks with political rivals mitigates their effect.

We test our claimed link between checks and corruption using a measure of checks that explicitly counts formal, *constitutional* checks independent from partisan considerations. The measure accounts for both the number of formal checks and the extent to which these can be weakened by the legislative and constitutional provisions in place. Our results strongly support our claimed link between checks and corruption. Formal checks are associated to increased corruption even after controlling for the independent effect of partisan checks as well as a host of additional explanatory variables that can impinge on the principal-agent relationship.

We close with an observation and a comment. Our results indicate that institutional checks facilitate corruption, which suggests that efforts to build checks into political systems are ill-conceived. This is true, but only to a point. First, we look only at the effect of *legislative* checks and leave to the side others that come into play ex post, such as constitutional courts, certain types of referendums, or anti-corruption agencies. And second, the broad nature of constitutional checks might obfuscate checks below the constitutional level, specifically legislative committee systems, that could serve to bring political rivals into the legislative process (cf. Strøm 1990). More generally, our results suggest that robust, open political competition—the existence of multiple viable rival parties seeking distinctly different policy packages and in position to benefit electorally and in policy terms from other parties' weaknesses—is the best safeguard against corruption and foundation for good governance. Parties control checks, but often do so by giving the authority to exercise checking authority to individuals; the less incentive those *individuals* have to collude in their private interest, the better. We believe the incentive to collude is minimized when voters focus on parties rather than individual candidates, and when candidates depend on their party leaders, who in turn can lead only subject to the good will of their copartisan legislators, for their legislative influence and electoral prospects (cf. Cox 1987).

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