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Unpacking Corruption: The Effect of Veto Players on State Capture and Bureaucratic Corruption

Tanya Bagashka[1]

Abstract

Unpacking corruption has advantages over using aggregate measures of corruption when theory generates different predictions about the effects of political institutions on different kinds of corruption. We take advantage of the Business Environment and Enterprise Performance surveys conducted in 1999, 2002, and 2005 to investigate the effect of veto players on state capture and bureaucratic corruption in the postcommunist countries. According to our results, a greater number of veto players is associated with less state capture. By contrast, the number of veto players does not have a significant impact on bureaucratic corruption.

Keywords

veto players, state capture, bureaucratic corruption

Introduction

Efforts to explain the causes of corruption have focused on the level of democratization, socioeconomic development, and social cleavages. More recent literature emphasizes political institutions such as electoral rules, the form of government, bicameralism, or decentralization. We make the following contributions to this developing field. We investigate the role of veto players, a factor that has not received sufficient attention in the corruption literature. We develop a theory about the impact of veto players on corruption, distinguishing between state capture (bribing to affect the formulation of new laws, rules, and regulations) and bureaucratic corruption (bribing to affect the implementation of already existing rules). We argue that this approach has advantages over using conventional unidimensional measures of corruption because certain political institutions affect different kinds of corruption differently. We argue that state capture, which involves changes in the "rules of the game" should be less likely in systems with a greater number of veto players, as special interests would need to bribe more political actors to secure the passage of policy changes that hurt social welfare. The greater compensation private interests have to provide to public officials might make capture unprofitable where veto players are numerous. We argue that numerous veto players might have a different effect on bureaucratic corruption, which refers mostly to bureaucratic malfeasance. We unpack the concept of corruption not

only theoretically, but also empirically. In contrast to most previous studies, we test the implications of our theory, distinguishing between bureaucratic corruption and state capture. We take advantage of the Business Environment and Enterprise Performance surveys (BEEPS) conducted in 1999, 2002, and 2005 to investigate the effect of veto players on state capture and bureaucratic corruption in the postcommunist countries. In the early 1990s, a single political and economic model dominated the postcommunist countries. Today, the great variation in the development of states and markets is tremendous. While substantively important in its own right, this dramatic change combined with significant institutional cross-country and temporal variation, allows us to address important questions for comparative politics. What kinds of political and economic reforms are conducive to strong state institutions? Can we blame the weak state institutions crippled by corruption in countries like Bulgaria and Russia to constitutional choices and the dispersion of power in the political system? We find that, in line with our expectations, a greater number of veto players is associated with less state capture in the postcommunist countries. By

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contrast, the number of veto players does not have a significant impact on bureaucratic corruption.

Conceptualizing Corruption

Corruption has been extensively studied in economics and political science. The term *corruption* is notoriously vague and has been used to refer to various phenomena. Many studies define corruption broadly as "the misuse of public office for private gain" (Kunicova and Rose-Ackerman 2005; Rose-Ackerman 1999; Sandholtz and Koetzle 2000a; Treisman 2000). With few exceptions, such as Hellman, Jones, and Kaufmann (2000) or Campos and Giovannoni (2007), empirical studies of corruption do not distinguish between different types of corruption. The treatment of corruption as a "generic uni-dimensional phenomenon" (Hellman, Jones, and Kaufmann 2000) is at least to some extent driven by the dearth of publicly available empirical measures that unpack the concept of corruption and have good country coverage. The most widely used corruption measures are the Control of Corruption measure from the World Bank's (WB) Governance Indicators Database and the Corruption Perception Index (CPI) of Transparency International. These indicators aggregate information from multiple surveys and polls. Most components of these composite indices are based on the perceptions of country experts or businessmen about the overall level of perception in a given country. Unlike other corruption indicators, the CPI and WB cover a large number of countries. Despite their popularity, these two measures have been widely criticized for a number of reasons, including their reliance on subjective evaluations and their inherent loss of specificity due to the aggregation of multiple indicators (for a detailed review of corruption measures, see Kaufmann and Kraay 2008; Kaufmann, Kraay, and Mastruzzi 2007; Knack 2007). Is the use of aggregate indices in the context of corruption problematic? Do we need to unbundle the concept of corruption in empirical studies of corruption? Whether the reliance on aggregate indicators is necessary or advantageous depends on the research question. If theory provides little guidance on the effect of a particular variable on different kinds of corruption, an aggregate measure might be preferred (Knack 2007). If, however, different political institutions affect corruption differently, an adequate test of the theory requires disaggregated measures that tap particular kinds of corruption. In this study, we focus on veto players, a concept that has received little attention in the corruption literature, despite its wider popularity. Drawing on recent veto player and the special interest literature, we develop a causal mechanism between veto players and corruption, distinguishing between state capture and bureaucratic corruption.

Veto Players and Corruption

In previous empirical studies of corruption, causal mechanisms underlying arguments about the effects of political institutions often emphasize the policy and political consequences of the number of veto points (see, for example, Gerring and Thacker 2004; Tavits 2007). Veto players or veto points are a central concept in the debate between the proponents of systems that disperse political power and their opponents (see, for example, Gerring and Thacker 2004; Tavits 2007).[1] While theories draw on the veto-player literature, empirical analyses use dichotomous features of political institutions such as presidentialism or federalism. The only exception is Panizza's (2001) empirical study, which uses a direct measure of veto players based on Henisz (2000).[2] Panizza (2001) finds that larger number of veto players is associated with less government graft and corruption, even though the effect is substantively modest.

While a legitimate line of inquiry that has produced many valuable insights, the study of dichotomous features of political institutions such as presidentialism or federalism does not allow an adequate test of the implications of veto-player theory because the effects of the different features of domestic political institutions might cancel each other out. Dichotomous classifications do not lend themselves to comparing the effects of combinations and hybrids such as, for instance, comparing a bicameral parliamentary two-party system with a unicameral presidential multiparty system (Tsebelis 1995). In contrast to the pairwise comparisons, the veto-player concept allows consistent comparisons across political systems.

More importantly, even if the purpose is not to test empirically the implications of veto-player theory, using binary classifications such as parliamentarism/presidentialism and unitarism/federalism might still be problematic, if the same causal logic underlies the effects of narrow features of political institutions on corruption. As Gerring and Thacker (2004) note, even though federalism refers to spatial unity and presidentialism to unity at the national level, both centralize political power. If parliamentarism or unitarism reduce corruption because they centralize political power, empirical tests should use a measure of political centralization that takes into account other features of political institutions that centralize political power and allow for consistent cross-country comparisons. The methodological problems associated with binary indicators aside, this paper goes beyond systematically evaluating the explanatory power of the veto-player concept using a direct measure of veto players. We

develop a theory about the effect of numerous veto players on different kinds of corruption. Drawing on recent veto-player models and the special interest literature, we argue that multiple veto players reduce state capture but do not have the same effect on bureaucratic corruption.

Veto Players and State Capture

The veto-player concept dates as far back as Madison. In the Federalist Papers, Madison argued that the dispersion of power among multiple government institutions strengthens the rule of law as it allows them to serve as a check on each other (Rossiter 1961). The veto-player concept has been rendered more precise in more recent work by Tsebelis (1995, 1999). Standard veto-player models (à la Tsebelis 1995, 1999, 2002), which focus on the difficulty of policy change in systems with multiple veto players, do not imply a necessary link between veto players and corruption. Numerous veto players might contribute to less corruption if policy change involves, for example, the passage of "corrupt" bills. However, the relationship is reversed if policy change involves the passage of anticorruption legislation. In a political system with many veto players, "the corrupt only needs to find the weak link and corrupt that group" (Rose-Ackerman 1999, 144). Without taking into account the preferences of veto players and the nature of policy change, it is difficult to predict the effect of numerous veto players on corruption.

In contrast to standard veto-player theory, more recent models investigate the effect of numerous veto players on the adoption of policies that hurt social welfare (Andrews and Montinola 2004; Gehlbach and Malesky 2010). While following the standard definition of veto players— actors that can block policy change—these models focus on an actor neglected in conventional veto-player theory, namely, special interests who do not have formal veto power but lobby formal veto players to implement policy formation, and generate unequivocal predictions about the effect of veto players.

Gehlbach and Malesky (2010) investigate the interaction between special interests and veto players in the context of economic reform. Contrary to standard veto-player theory, they find that the effect of veto players on policy stability is conditional on the efficiency of policy reform relative to the status quo. Numerous veto players discourage policy changes that lead to less efficient states of the world by weakening the power of rent-seekers. Intuitively, rent-seekers need to compensate veto players to enact policies that hurt the public interest. Veto players concerned with social welfare would support inefficient policies only if rent-seekers provide them with adequate compensation. The greater the number of veto players, the greater the compensation rent-seekers must provide to

buy off veto players, which makes rent-seeking less profitable. In contrast to traditional veto-player theories (à la Tsebelis), which focus on the "separation of purpose" that arises when veto players have divergent policy views, the focus here is on the "separation of powers" (Gehlbach and Malesky 2010, 959) between veto players, concerned with organized and unorganized interests, and rent-seekers, concerned only with the interests of their organized group.[3] The model demonstrates that if the preferences of veto players are not aligned with the preferences of special interests, the number of veto players has an independent and negative effect on the passage of special interest policies even if veto players have homogenous policy preferences because the amount of total compensation special interests must provide increases in the number of veto players. This model has direct implications for state capture, which involves the passage of socially inefficient policies, which benefit narrow and organized interests but hurt the broader national interest.

Although based on a somewhat different dynamic and less directly related to our argument, Andrews and Montinola (2004) also suggest that multiple veto players make rent-seeking by special interests less successful. Building on Rasmusen and Ramseyer (1994), Andrews and Montinola (2004) model the interaction between veto players and special interests that bribe to prevent the passage of legislation in the public interest. In their setting, voters punish all veto players for the failure of legislation in the public interest, regardless of whether they personally voted against or for it. In addition to the general cost to each veto player when public-interest legislation fails, a veto player incurs a personal cost for accepting a bribe to vote against public-interest legislation if the acceptance of bribes is exposed to the public. Political competition motivates veto players to expose each other's corrupt acts and thus inflict high personal costs,[4] unless they all collude and agree to accept the bribes and vote against public-interest policies (thus eliminating the private costs for accepting bribes). Collusion makes bribing a veto player cheaper for rent-seekers due to the elimination of personal costs and at the same time allows veto players to extract rents. As the number of veto players increases, coordination problems increase, which makes collusion less likely, bribes more expensive, and the passage of public-interest legislation more likely. While Andrews and Montinola (2004) focus on the effect of veto players on the passage of public-interest legislation, the logic applies to other strategies veto players might engage in to advance special interests at the expense of social welfare.

These theoretical models have direct testable implications about the effect of veto players on the prevalence of state capture. Following Hellman et al. (2000), we conceptualize state capture as the efforts of firms to affect the

formation of policies (i.e., laws, rules, decrees, and regulations) through illicit and nontransparent private payments to public officials. Through state capture, the private sector "captures" the state legislative, executive, and judicial apparatus, to further its own private interests. State capture involves collusion by private actors with public officials for their mutual private benefit, where they share rents at the expense of social welfare. Several negative externalities could be generated by the capacity of a small number of powerful firms to capture the state and obtain advantages from the developing legal and regulatory systems. Captor firms could use their relationships with public officials to erect barriers to entry or to impose harsh regulatory burdens on their competitors. States that are prepared to sell public goods to individual firms are likely to be undersupplying those goods to the general market.[5]

> *Hypothesis 1*: Multiple veto players contribute to less state capture. The greater the number of veto players in a political system, the greater the bribes special interests must pay to compensate veto players for supporting policies against the public interest.

Veto Players and Bureaucratic Corruption

In contrast to state capture, which refers to corrupt deals between private interests and elected officials, bureaucratic corruption is a "bureaucratic pathology" (Gerring and Thacker 2004). While state capture refers to nontransparent payments to affect the formation of new rules, *bureaucratic corruption* refers to private payments to public officials to affect the implementation of already existing rules. Being a distinct phenomenon, we expect it to be affected differently by the number of veto players in the political system than state capture. Our theoretical expectations about the effect of multiple veto players on bureaucratic corruption are weaker compared with those about state capture. While the literature generates unequivocal predictions about the effects of veto players on state capture, different plausible theories link veto players to bureaucratic corruption positively and negatively. A number of scholars argue that bureaucratic accountability is best achieved through "multiple principals, semi-independent agencies carrying strict and highly specific mandates and overlapping jurisdictions, such that bureaucrats check and balance one another and offer a benchmark to measure each other's performance" (Gerring and Thacker 2004, 314). According to this view, the best way to administer a bureaucracy is to establish multiple principals, which could include the executive, the legislature along with different committees, and the judiciary, to which each bureaucratic agent is accountable. Multiple agencies accountable to multiple principals reduce the monitoring

costs for the principals by creating yardsticks to measure and compare the performance of different bureaucracies (Tirole 1994). If agency monitoring costs stem from the agent's superior information about the relationship between policies and outcomes, the existence of multiple agencies providing the same service can create an information-rich environment and reduce agents' informational rents. These theories predict a negative relationship between multiple veto players and corruption. In a similar vein, Persson and Tabellini (2000) argue that replacing a centralized bureaucracy responsible for a variety of tasks and responsibilities and accountable to a central government to a multitude of bureaucracies each accountable to a number of local governments for a single task in a single jurisdiction facilitates the monitoring of agents (bureaucracies) by the principals (governments). While in a centralized bureaucracy, only the aggregate performance of bureaucrats matters for reappointment, in a decentralized bureaucracy, bureaucrats are held accountable for all of their actions and there is a much stronger link between effort and reward. If increasing the number of veto players entails enhancing direct accountability, this theory would imply that multiple veto players reduce bureaucratic corruption.

> *Hypothesis 2a*: Bureaucratic corruption is less prevalent in political systems with multiple veto players: multiple agents accountable to multiple principals that provide similar services provide yardsticks to measure each other's performance and reduce bureaucratic rents.

On the other side of the debate, a number of works suggest that multiple veto players are conducive to bureaucratic corruption. The American politics literature suggests that when a president and Congress have divergent preferences, the bureaucracy can balance their competing demands, take advantage of presidential–congressional deadlock, and gain autonomy from both institutions (Dahl and Lindblom 1953; Dodd and Schott 1979; McNollgast 1999). Conflict among congressional and presidential politicians over who will control the bureaucracy and the ability of bureaucrats to play one group of politicians against another results in significant bureaucratic discretion (Dahl and Lindblom 1953). In the context of the countries we examine, the Russian experience in the late 1990s period demonstrates how the dispersion of power between different levels of government[6] could contribute to greater bureaucratic corruption. In the 1990s, Russia experienced a devolution of power from the federal center to the regions. The resulting formal and informal division of authority between the central and regional governments invited various abuses by administrative agencies and contributed to Russia's inability to collect taxes, which

turned into the greatest threat to its economic and political stability in the 1992 to 1997 period. While an exclusively federal body,[7] the State Tax Service (STS) occupied an ambiguous political space because its regional branches were financially dependent on regional governments. As planned spending on federal programs, including on state employees' wages, was frequently delayed, STS regional officials relied on regional governments for housing, benefits, and wage supplements. The dual subordination of regional tax collectors allowed them to play one level of government against another. As tax officials used one level of government to shield them against the other, bureaucratic corruption was unlikely to be detected and punished (see Shleifer and Treisman 2000, chap. 6). The existence of such cover allowed bureaucrats to prey on local firms, forcing them to exit the official economy, and exacerbating economic hardship and state weakness.

Multiple veto players could contribute to corruption in another way. If increasing the number of veto players entails introducing additional levels of government with independent rights to regulate economic activity, the aggregate amount of bribes by the separate tiers of government in the absence of collusion between them would be higher under multiple veto players. If different bureaucratic agencies controlled by different levels of government supply complementary goods, the aggregate demand of bribes will be greater compared with a centralized bureaucracy because the different agencies set bribes independently, without internalizing bribe externalities across different levels of government, in an effort to maximize their own revenue rather than the total revenue of all the bribe collectors (Shleifer and Vishny 1993). For instance, registering a startup in Russia in the early 1990s usually involved bribing the regional legislature, the regional executive, the federal ministry, and the regional executive, among others.[8] The dispersion of decision-making powers in Russia resulted in excessive rent extraction in the same manner successive monopolies lead to a price markup above the monopoly level. According to the empirical evidence, harassment of small businesses by corrupt bureaucrats was more prevalent in Russia than in other postcommunist countries (Frye and Shleifer 1997; Frye and Zhuravskaya 2000; Shleifer and Frye 1997). These theories predict a negative relationship between multiple veto players and bureaucratic corruption.

Hypothesis 2b: Bureaucratic corruption is more prevalent in political systems with multiple veto players.

Data and Measurement

Our empirical tests use cross-country data that characterize the postcommunist countries in terms of the level of different kinds of corruption and the number of veto players. To this basic data we add data on other political and economic variables and firm-level characteristics. We focus on the countries in Eastern Europe and the former Soviet Union included in the BEEPS for which we have data for these additional measures discussed below.[9]

Measuring Corruption

Our theory predicts different effects of multiple veto players on different types of corruption. We expect a negative effect on state capture, while the predictions with respect to bureaucratic corruption are not unequivocal. These theoretical expectations necessitate a disaggregated measure of corruption. While the WB and Transparency International corruption indicators have a good cross-country coverage, they are not appropriate for testing theories about particular kinds of corruption. The two indices measure overall perceptions of corruption in a country and aggregate information from various surveys and polls. Most of the questions in the used surveys do not distinguish between high-level political corruption and low-level bureaucratic corruption. To overcome these shortcomings, we take advantage of the BEEPS conducted by the European Bank for Reconstruction and Development (EBRD) and the WB.

The survey covers almost every country in Eastern Europe and Central Asia in each of three survey waves: 1999, 2002, and 2005.[10] Statistical offices in each country were contacted to obtain the total number of firms by industry to ensure representativeness. The investigators also collected information from the statistical offices on the relative contributions of each industrial sector to gross domestic product (GDP), so that, for each country, the composition of the firms in the sample reflects the differences in the relative shares of each sector in GDP.[11] The authors of the survey do not claim the sample is representative of all firms in each country because the information necessary to weigh the sample in proportion to the universe of firms is not available for some countries (Hellman et al. 2000). Given the data constraints, the sample was designed to be as representative as possible of the population of firms, subject to various minimum quotas for the total sample in each country. Quotas were used to ensure sufficient weight in the tails of the distribution of firms for key control parameters such as size, geographical location, exports, and ownership.[12] Firms were randomly sampled from business or telephone directories. The initial screening questionnaire was conducted by phone to select firms fulfilling the quota restrictions. The main questionnaire was conducted in person with a high-ranked officer of the firm.[13]

Questions in the BEEPS emphasize firms' experiences as opposed to corruption perceptions. For the most

sensitive questions in the BEEPS, respondents were asked in the third person (about firms like theirs, rather than about their own firms). The BEEPS have clear advantages and have been extensively used to investigate different aspects of business–state relations in the region (see, for example, Gehlbach 2008; Hellman, Jones, and Kaufmann 2003; Jensen 2003). Respondents in each country were asked identical questions, which allows cross-country comparisons. The survey was conducted at roughly the same point in time in each country, so the international context is roughly similar for all countries in the sample.

Most relevant to our analysis, the surveys unpack the concept of corruption and include questions that target state capture and bureaucratic corruption. To tap state capture, firms were asked how often a firm like theirs would make unofficial payments to affect the content of new legislation, rules, or decrees. Thus, in the models of state capture, the dependent variable is the level of capture at the firm level as measured by the firm response to the above question (never, seldom, sometimes, frequently, usually, always).[14] Following Hellman et al. (2000) and Hellman, Jones and Kaufmann (2003), to measure bureaucratic corruption, we use the question that asks firms how often firms like yours make unofficial payments to public officials "in order to get things done" (never, seldom, sometimes, frequently, usually, always).[15] The surveys list a number of services for which such bribes could be paid, including connection to public services, to obtain licenses and permits, to deal with taxes and tax collection, to deal with customs/imports. That bureaucratic corruption and state capture are distinct phenomena is evidenced by the low bivariate correlation between individual firm-level responses tapping bureaucratic corruption and state capture (.29). The bivariate correlations are .31, .26, and .30 for 1999, 2002, and 2005, respectively. The low correlation between bureaucratic corruption and state capture demonstrates that unpacking the concept of corruption is worthwhile.

The many advantages of the BEEPS notwithstanding, it is restricted to the postcommunist countries. While corruption in the postcommunist countries is substantively important in its own right, the significant cross-country and temporal institutional variation provides us with a great opportunity to investigate in a comparative context the consequences of the dispersion of political power for corruption. The postcommunist countries adopted a great variety of executive and legislative rules and organizations. The constitutional prerogatives vested in the executive changed over time in a number of countries (see Kopecky 2004). A number of countries experienced constitutional reforms, some of which were substantial.[16] For instance, following constitutional changes in Croatia transitioned in 2000 from a semipresidential to a parliamentary system with a closed list proportional voting,

changes that increased the number of veto players. By contrast, upon taking office, President Putin restricted the power of parliament and the governors of the regions,[17] in effect reducing the number of veto players. The substantive importance of corruption in the postcommunist countries notwithstanding, the theoretical argument is not specific to the postcommunist countries, and we are more generally interested in corruption. This begs the question, are there alternatives to BEEPS or the most widely used indices we could use?

The WB has conducted surveys similar to BEEPS, but on a country-by-country basis rather than on a regional basis. An exception is the *Global Competitiveness Report* (GCR) published by the World Economic Forum. The GCR includes several questions that target particular kinds of corruption and state capture and bureaucratic corruption in particular. However, in contrast to BEEPS, the different components of corruption in the GCR surveys are highly correlated (see Tables 9-11 in the supplemental materials in the electronic version at http://prq.sagepub.com for the 2002, 2004, and 2006 GCR).[18] The reason for the high correlation between different measures of corruption in the GCR survey and the lack thereof in the BEEPS does not seem to be GCR's larger sample— the correlations for the subsample of postcommunist countries in the GCR survey are similarly high.[19] A plausible explanation is an important difference between the BEEPS and the GCR survey. While the BEEPS focuses on firms' experiences, the GCR survey aims to provide country-level measures of the business climate based on the opinions of business leaders. The GCR sample in each country includes mostly executives with international experience, who tend to be from larger and export firms. The many advantages of the BEEPS notwithstanding, it is possible that state capture, better tapped by the BEEPS because of its focus on firms' experiences, is a phenomenon typical for former communist countries (or a subset of them) transitioning to the market but rare in other countries. While we would like to investigate whether our theory applies to other parts of the world, we are constrained by the existing data.

Measuring Veto Players

To operationalize the main explanatory variable, the number of political actors with the ability to block policy change, we employ a widely used measure of veto players, namely, the CHECKS variable from the Database of Political Institutions (Beck et al. 2001). As described there and in Keefer and Stasavage (2003), this measure is based on objective criteria. CHECKS is based on a formula that "counts the number of veto players in a political system, adjusting for whether these veto players are independent of each other, as determined by the level of electoral competitiveness in a system, their respective party

affiliations, and the electoral rule" (Beck et al. 2001, 170). This index yields a minimum score of 0 when a country lacks an effective legislature, increasing linearly with the addition of subsequent veto points whose political preferences are closer to that of the opposition than they are to the average government preference, using a three-point scale. The scale is calculated using a different methodology for presidential and parliamentary systems.[20] CHECKS also takes into account the DPI index of electoral competitiveness as constitutional checks are meaningless if the veto players are not subject to electoral competition.

For our purposes, the CHECKS measure is preferable to an alternative, also widely used measure by Henisz (2000), for several reasons. First, the other most widely used measure of veto players (POLCON) is based on a spatial model, which assumes that the marginal effect of veto players decreases in the number of veto players; our theoretical argument implies no such relationship. Second, CHECKS, unlike POLCON, accounts for electoral rules, which affect the number of constitutional veto players. For instance, closed party list proportional voting, such as the electoral rules introduced in Croatia following the constitutional reforms of 2000, allows party leaders to maintain greater party loyalty and tends to turn political parties into strong partisan veto players. Third, unlike POLCON, CHECKS takes into account the diversity of interests in large coalition governments, counting each party in the government coalition as an additional veto player. Because different parties usually represent different constituencies with different interests, special interests faced with multiparty coalition governments and fragmented legislatures may need to buy off more veto players to affect the formation of policy. Because this is fundamental to our argument linking veto players and corruption and state capture in particular, CHECKS is a more appropriate measure. However, we use the alternative measure in the section "Robustness Checks."[21]

Control Variables

In addition to veto players, the analysis includes several control variables previously identified as important predictors of corruption. We control for *trade openness* measured by the sum of imports and exports as a percentage of GDP, and *foreign direct investment* (FDI; Sandholtz and Gray 2003), measured by net inflows as a percentage of GDP.[22] The ability of officials to provide protection or other privileges to firms might be conditional on external competition from imports (Treisman 2007). Ades and Di Tella (1996, 1999) find that countries more open to foreign trade are less corrupt. We use a measure of political competition (*Democracy*) based on the PARCOMP variable from the Polity IV data set (Marshall and Jaggers

2006). This variable measures to what extent competing policy views can be pursued in a country's political arena (the composite POLITY indicator includes a measure of constraints on the executive, which is similar to the measure of veto players). We also control for *GDP per capita (logged)* measured in U.S. dollars. These variables are rather well-accepted controls for cross-national studies of corruption (see, for example, Chang and Golden 2007; Gerring and Thacker 2004; Treisman 2007). To account for the possibility that the level of state capture is low in some countries because the private sector remains small and the political regime is highly authoritarian, thus making policy change and state capture unlikely (see Hellman et al. 2000), we control for the progress of economic reform. To measure the success of economic reform, we employ a widely used measure based on an index provided by the EBRD (2005), for example. EBRD evaluates reform progress on a scale from 1 to 4.3 along eight policy dimensions: large-scale privatization, small-scale privatization, governance and enterprise restructuring, price liberalization, trade and foreign-exchange system, competition policy, banking reform and interest-rate liberalization, and securities markets and nonbank financial institutions. The highest score along each dimension is roughly equivalent to the level typical of a developed market economy. To facilitate interpretation of results, we rescale this variable to take values from 0 to 100, where 100 indicates the highest level of reform. We call this variable *economic reform*.

The discussion so far has focused on the independent effects of numerous veto players on state capture and bureaucratic corruption. However, recent studies suggest that a firm's reliance on state capture might depend on firm-level characteristics such as firm size and ownership. Hellman, Jones, and Kaufmann (2003) investigate state capture and its determinants at the firm level. They find that large new private firms competing with incumbent firms with ties to the state tend to rely on state capture as opposed to bureaucratic corruption. By contrast, small private firms tend to become victims of bureaucratic corruption. To address this, we control for firm-level characteristics, namely, firm size and firm origin, percentage of foreign ownership, and the year in which the firm was founded, using data from the BEEPS. Similar to Hellman et al. (2000), to measure size, we include the dummy variable *small firm* equal to one if a firm has 50 or less full-time employees.[23] Following Hellman et al. (2000), to capture the effect of origin, we include dummy variables for De novo, privatized, and state-owned firms. De novo firms are those that were private since the time of establishment with no state-owned predecessor; privatized firms are those that were previously state-owned; state-owned firms are those in which the state has majority ownership. *Year founded* is the year

in which the firm started production. To explore the possibility that firms with a significant percentage of foreign ownership are less corrupt, we include the variable *foreign ownership* (percentage). We used the variance inflation factor (VIF) method to test for the presence of multicollinearity between the independent variables. In all models, the VIF score of the independent variables does not exceed 10, and the mean VIF is within reasonable limits. Thus, based on the VIF score, multicollinearity is not a serious problem.[24]

Statistical Methods

We combine data from different levels of analysis. We integrate cross-national variables into the BEEPS data because the level of state capture or corruption in a country may be influenced by country-level factors, such as the level of economic development, and firm-level characteristics such as firm size. The country-level variables were collected in the following way: the respective country-level data from the same year as the BEEPS were combined with the BEEPS firm-level data for the respective wave. Because the data are from different levels of analysis, the statistical model must recognize that. Failure to do so and including firm-level and country-level data in a standard single-level model is problematic, because this approach assumes that the firm-level observations are independent of each other, while they are likely to depend on country-level characteristics. This may lead to overconfidence in the precision of estimates or to identifying a relationship between two levels that does not exist (Snijders and Bosker 1999; Steenbergen and Jones 2002). In addition, using a single-equation model does not take into account the possibility that the intercepts might vary across countries, which might overestimate the effects of country-level variables. For example, the variable for economic development might be capturing the effect of economic development on state capture, as well as some other country-level characteristics (Anderson and Tverdova 2003). This will overestimate the effect of ethnic economic development. To address these concerns, we use a hierarchical model that takes into account firm- and country-level characteristics (using STATA's **glamm** command). We use an ordered logit model as the dependent variable is ordinal and ranges from 1 to 6. We estimate the following model, using maximum likelihood.

Level 1:

$$\text{State Capture}_{ij} = \beta_{oj} + \beta_{1j}\text{Small Firm}$$
$$+ \beta_{2j}\text{ De Novo Firm}$$
$$+ \beta_{3j}\text{ Privatized Firm}$$
$$+ \beta_{4j}\text{ Year Founded}$$
$$+ \beta_{5j}\text{ Foreign Ownership} + r_{ij}.$$

Level 2:

$$\beta_{0j} = \gamma_{oj} + \gamma_{1j}\text{Veto Players}$$
$$+ \gamma_{2j}\text{Democracy} + \gamma_{3j}\text{Trade}$$
$$+ \gamma_{4j}\text{GDP Per Capita}$$
$$+ \gamma_{5j}\text{Economic Reform}$$
$$+ \gamma_{6j}\text{Foreign Direct Investment}$$
$$+ U_j.$$

The term r_{ij} represents the error term for firm-level equation. The term U_j captures effects of error operating in the country-level variables. We also add dummy variables for the respective waves of the survey (1999, 2003, and 2005).[25] Notwithstanding the advantages of multilevel models, they can be unstable because any misspecifications at the first level will be passed at the second level (Achen 2005). In addition, the relatively small number of second-level cases (the regressions are based on observations from 18 countries) raise the standard issues about regression analysis with a small number of observations. As a second estimation technique, we used "ordinary" ordered logit regressions with robust standard errors. For all models, we compute robust (White–Huber) standard errors and robust standard errors clustered on country.

Empirical Results

As a first cut, in Table 1 we compare the frequency of state capture to that of bureaucratic corruption in a given country, using BEEPS. To measure the prevalence of state capture at the country level, we calculate the percentage of firms in a country that reported they made payments to affect the formation of new decrees, laws, rules, or regulations sometimes, frequently, usually, or always. To measure the prevalence of bureaucratic corruption, we calculate the percentage of firms that reported they made payments "to get things done" sometimes, frequently, usually, or always. As evidenced by Table 1, bureaucratic corruption and state capture are distinct phenomena. State capture is less prevalent than bureaucratic corruption, but which predominantly varies across countries. While there are countries where bureaucratic corruption and state capture are highly correlated, whether they are both low, as in Belarus or Slovenia,[26] or both high, as in Albania or Macedonia, in a number of countries, one type of corruption clearly predominates. The main findings with respect to the determinants of state capture are presented in Table 2. The results provide strong support for Hypothesis 1. The coefficient of *veto players* is negative and significant in Models 1 to 4, which implies that a greater number of veto players is associated with lower propensity to rely on state capture. The magnitude of the effect ranges from −0.09 to −0.12. Regarding the effects of the other

Table 1. Corruption in the Postcommunist Countries, 1999, 2002, 2005.

	Bureaucratic corruption[a]	Classification[b]	State capture	Classification
Albania	70	High	26	High
Armenia	44	Low	17	Low
Azerbaijan	60	High	19	High
Belarus	40	Low	13	Low
Bulgaria	44	Low	19	Low
Croatia	38	Low	22	High
Czech Republic	40	Low	14	Low
Estonia	30	Low	26	High
Georgia	41	Low	25	High
Hungary	37	Low	13	Low
Kazakhstan	55	High	11	Low
Kyrgyzstan	75	High	20	High
Latvia	36	Low	19	Low
Lithuania	47	Low	17	Low
Macedonia	50	High	22	High
Moldova	45	Low	49	High
Poland	42	Low	18	Low
Romania	56	High	19	Low
Russia	60	High	39	High
Slovakia	53	High	16	Low
Slovenia	22	Low	16	Low
Ukraine	54	High	17	Low
Uzbekistan	50	High	12	Low
Average	48		19	

Source. Business Environment and Enterprise Performance Survey.

[a] Firms were asked how common it is for firms like theirs to make irregular payments "to get things done." Column 1 reports the percentage of firms reporting that such payments were made sometimes, frequently, mostly, or always. Firms were also asked how often a firm like theirs would make unofficial payments to public officials to influence the content of new laws, decrees, or regulations. Column 2 reports the percentage of firms reporting that such payments were made sometimes, frequently, mostly, or always.

[b] Classified as "low" if below cross-country and as "high" if above or equal to cross-country average.

country-level characteristics, FDI, trade, and economic reform have the expected effects. *FDI* has a negative and significant coefficient in the multilevel models (Models 3 and 4), which are the estimates we consider more reliable because they take into account the multilevel structure of the data. Similarly, *trade* has a negative and significant coefficient in Models 1, 3, and 4. By contrast, *economic reform* has positive coefficients in all models, which are significant statistically and substantively. As expected, more advanced economic reforms provide more opportunities for state capture by private interests. The results with respect to the rest of the country-level characteristics are more mixed. Democracy as captured by the PARCOMP variable does not have a significant effect. Economic development is not a good predictor of state capture—GDP Per Capita (logged) has an insignificant coefficient in all models except Model 1, where its coefficient is negative and significant. Regarding firm-level characteristics, we find that small firms are less likely to engage in state capture than large firms. The effect of firm size is negative and significant, statistically and

substantively, in all models. This finding is consistent with Hellman et al.'s (2000) argument that larger firms are more likely to engage in state capture than small firms, while smaller firms are more likely to become victims of bureaucratic corruption. Similar to Hellman et al. (2000), we find that de novo firms and privatized firms are more likely to engage in state capture than state-owned firms (the excluded category), who have strong ties to public officials and can influence them without recourse to private payments. *Year founded* has a positive and significant coefficient in Models 1, 3, and 4. *Foreign ownership (percentage)* does not have a significant effect. As hypothesized, *veto players* have different effects on state capture and bureaucratic corruption (Table 3). The *veto players* variable has an insignificant coefficient in all models except Model 5, where its coefficient is positive and significant. There is no strong support for Hypothesis 2a or Hypothesis 2b, which indicates that veto players do not affect bureaucratic corruption, or that both of the suggested effects are at play and cancel each other out. While distinguishing between those two explanations requires

Table 2. Veto Players and State Capture, BEEPS 1999 to 2005.

	Model 1	Model 2	Model 3	Model 4
	Ordered logit (robust standard errors)	Ordered logit(standard errors clustered by country)	Multilevel ordered logit (robust standard errors)	Multilevel ordered logit(standard errors clustered by country)
Firm-level variables				
Small firm	−0.1388** (0.0642)	−0.1474* (0.0804)	−0.1433** (0.0699)	−0.1434** (0.0701)
New (de novo) firm	0.7726*** (0.2432)	0.7435*** (0.1702)	0.8023*** (0.1566)	0.8023*** (0.1565)
Privatized firm	0.5457** (0.2462)	0.5236** (0.1765)	0.5923*** (0.1559)	0.5924*** (0.1559)
Year founded	0.0044* (0.0024)	0.0046 (0.0026)	0.0045* (0.0024)	0.0045*(0.0025)
Foreign ownership (percentage)	0.0002 (0.0009)	0.0001 (0.0014)	0.0005 (0.0013)	0.0005 (0.0013)
Country-level variables				
Veto players	−0.1171*** (0.0245)	−0.1116* (0.0588)	−0.0913** (0.0363)	−0.0913** (0.0363)
Democracy	0.0019 (0.0469)	0.0107 (0.1794)	−0.1924 (0.1729)	−0.1925 (0.1737)
GDP per capita (logged)	−0.2529*** (0.08)	−0.2636 (0.2112)	0.0099 (0.2839)	0.0099 (0.2853)
Trade	−0.0034** (0.0012)	−0.0033 (0.0035)	−0.0106*** (0.0032)	−0.0106*** (0.0032)
Foreign direct investment	−0.0077 (0.0069)	−0.0072 (0.0168)	−0.0563*** (0.0149)	−0.0563*** (0.0149)
Economic reform	0.6999*** (0.0852)	0.6792*** (0.2016)	0.47701*** (0.1256)	0.477*** (0.1256)
Cut-points	10.09, 10.86, 11.57 12.27, 12.97	10.35, 11.12, 11.83 12.53,13.23	10.44, 11.22, 11.94 12.64, 13.34	10.44, 11.23, 11.94 12.65, 13.34
Year fixed effects (country level and firm level)	Yes	Yes	Yes	Yes
Variance and covariance of random effect			0.2349 (0.0525)	0.2349 (0.0526)
Firm-level N	9793	9793	9793	9793
Country-level N	18	18	18	18
Wald χ^2	135.90***	303.20***		
Log-likelihood	−7210.9116	−7211.6584	−7144.6226	−7144.62

BEEPS = Business Environment and Enterprise Performance Survey; GDP = gross domestic product.

additional research, our results show that *veto players* have different effects on state capture and bureaucratic corruption. Regarding the effects of the country-level variables, state capture and bureaucratic corruption appear to have distinct determinants. While *Democracy* did not have an effect on state capture, it does have a negative and significant effect on bureaucratic corruption in the multilevel models (Models 7 and 8), in line with the findings of the corruption literature. While more advanced economic reforms are associated with more state capture, *economic reform* does not have a significant effect on bureaucratic corruption. Similar to its effect on state capture, *trade* has a negative and significant coefficient in most models (Models 5, 7, and 8), while FDI does not have an effect. Somewhat surprisingly, while economic development *GDP per capita (logged)* did not have a significant effect on state capture, it has a positive and marginally significant effect in the multilevel models (Models

7 and 8). However, its coefficient in single-equation models (Models 5 and 6) is negative and significant, and substantively large. With this exception, our results, and most importantly, our findings about the effects of veto players are not sensitive to the statistical model used. Regarding the firm-level variables, as expected, de novo firms are more likely to be victims of bureaucratic corruption than state firms. While *year founded* did not have a significant effect on state capture, it does have a significant effect on bureaucratic corruption—more recently established firms are more likely to become victims of bureaucratic corruption.

Robustness Checks

We subject our results to a number of robustness checks. First, we use an alternative measure of State Capture that focuses on the capture of the legislative branch. Firms in

Table 3. Veto Players and Bureaucratic Corruption, BEEPS 1999 to 2005.

	Model 5	Model 6	Model 7	Model 8
	Ordered logit (robust standard errors)	Ordered logit(standard errors clustered by country)	Multilevel ordered logit (robust standard errors)	Multilevel ordered logit(standard errors clustered by country)
Firm-level variables				
Small firm	−0.0436 (0.044)	−0.0436 (0.0574)	0.0334 (0.0573)	0.0334 (0.0573)
New (de novo) firm	0.662*** (0.1418)	0.662** (0.2789)	0.8699** (0.3405)	0.8699** (0.3405)
Privatized firm	0.2664* (0.1441)	0.2664 (0.282)	0.4868 (0.3642)	0.4868 (0.3642)
Year founded	0.0049*** (0.0014)	0.0049*** (0.0015)	0.0039*** (0.0011)	0.0039*** (0.0011)
Foreign ownership (Percentage)	−0.0009 (0.0006)	−0.0009 (0.0009)	−0.0007 (0.0039)	−0.0007 (0.0008)
Country-level variables				
Veto players	0.0485*** (0.0152)	0.0485 (0.0601)	0.0227 (0.0404)	0.0227 (0.0404)
Democracy	−0.0236 (0.0336)	−0.0236 (0.1511)	−0.1866** (0.0692)	−0.1866** (0.0692)
GDP per capita (logged)	−0.4873*** (0.0572)	−0.4873** (0.2011)	0.2805* (0.1667)	0.2805* (0.1667)
Trade	−0.0018** (0.0008)	−0.0018 (0.0027)	−0.0045** (0.0018)	−0.0045** (0.0018)
Foreign direct investment	−0.003 (0.0049)	−0.003 (0.0146)	−0.0109 (0.0094)	−0.0109 (0.0094)
Economic reform	−0.0806 (0.0522)	−0.0806 (0.1868)	0.2103 (0.1444)	0.2103 (0.1444)
Cut-points	4.99, 5.74, 6.74 7.52, 8.66	4.99, 5.74, 6.74 7.52, 8.66	9.67, 10.44, 11.47 12.28, 13.44	9.67, 10.44, 11.47 12.28, 13.44
Year fixed effects (country-level and firm-level)	Yes	Yes	Yes	Yes
Variance and covariance of random effect			0.3962 (0.0799)	0.3962 (0.0799)
Firm-level N	10,670	10,670	10,670	10,670
Country-level N	18	18	18	18
Wald χ^2	644.55***	157.05***		
Log-likelihood	−17,081.349	−17,081.349	−16,856.086	−16,856.086

BEEPS = Business Environment and Enterprise Performance Survey; GDP = gross domestic product.

the 2002 and 2005 survey were asked to what extent the sale of parliamentary votes on laws to private interests has had an impact on their business.[27] The possible responses varied from "no impact" to "very significant" or "decisive" impact, with intermediary answers of "minor," "moderate," and "significant" impact.[28] In contrast to the behavioral measure of state capture, we used in the previous analysis, that identifies captor firms (firms that report they have made payments to public officials to influence the content of laws, rules, and decrees), this is an impact measure of the capture economy based on the speculations of firms that other firms are involved in state capture. While perhaps less reliable than the behavioral measure, the impact measure is still a useful indicator of how perceptions of the extent of state capture vary across countries. As Table 4 in the supplemental materials demonstrates, our results are robust to the use of this alternative measure of state capture. *Veto players* have a negative and significant effect on the perceived prevalence of the

sale of parliamentary votes to private interests. The magnitude of the effect is similar to the one identified using the behavioral measure of *state capture*. As the results demonstrate, where the legislature is divided or other institutional actors can block policy chance, the capture of the legislative branch by private interests is less common.

Previous studies of corruption have included only democracies and partial democracies (see, for example, Gerring and Thacker 2004). Because it might be problematic to expect that power-sharing institutions will have substantial effects in newly democratized or non-democratic countries, we replicate the results including only countries with a *Democracy* (measured by PARCOMP) score of 4 or 5.[29] The results are displayed in Table 5 in the supplemental materials. As the results demonstrate, our results are stronger if we restrict the sample to only democratic countries and exclude countries such as Belarus, Kyrgyzstan, or Uzbekistan. *Veto players* have

a negative and significant coefficient in all models predicting *state capture* (Models 13, 14, and 17, 18) and now the effect ranges from −0.17 to −0.19. We also investigate whether there is a substitutive or a complementary relationship between state capture and corruption by including bureaucratic corruption as an explanatory variable in the equation predicting state capture (the *corrupt firm* variable) and by including state capture as an explanatory variable in the equation predicting bureaucratic corruption (*captor firm*). We find evidence of a complementary relationship—firms that engage in corruption are also more likely to engage in state capture (see Tables 7 and 8 in the supplemental materials). This relationship does not affect the significance of the negative effect of veto players on state capture in any important ways. While an adequate explanation of the complementary relationship between state capture and bureaucratic corruption requires additional research, a plausible explanation is that the prevalence of state capture facilitates bureaucratic corruption: if the state is captured by firms, law enforcement and the rule of law are undermined, which makes bribing bureaucrats easier (see Damania, Fredrikson, and Mani 2004).

We replicate our analysis using the alternative measure of veto players (POLCON).[30] When this measure is used, *veto players* lose their statistical significance in most models predicting *state capture* (see Table 12 in the supplemental materials). *Veto players* have the expected negative effect in 4 out 8 models (Models 29–30 and 33–34), but the effect is statistically insignificant. *Veto players* have a positive and statistically significant coefficient in 2 models (Models 31 and 32) but the coefficients in the most comprehensive models and in all multilevel models are statistically insignificant. Regarding *bureaucratic corruption* (see Table 13 in the supplemental materials), *veto players* have a significant (and negative) effect only in one model (Model 40). We would like to note, however, that due to the fact that the POLCON measure does not take into account changes in electoral rules and, most importantly, does not count each party in coalition governments as an additional veto player, it understates the significant temporal within-country variation in the postcommunist countries. In our sample, unlike CHECKS, the POLCON measures exhibit very little temporal within-country variation, and the existing changes are minimal. This difference between the two measures is largely due to the fact that many governments in our sample were coalition governments.[31] Because the CHECKS measure reflects these changes, while the POLCON measure does not, the correlation between the two measures is weaker in the countries with coalition governments.[32] Thus, we surmise that the reason why veto players have an insignificant effect on state capture if the POLCON measure is used is that the POLCON measure understates

the variation in electoral rules and the number of partisan veto players in our sample.[33]

Conclusion

Previous studies tend to conceptualize corruption as a one-dimensional phenomenon and rely on aggregate empirical measures. Here, we show that "unpacking" corruption is worthwhile because political institutions affect different kinds of corruption differently. Using empirical measures that distinguish between state capture and corruption, we find that numerous veto players contribute to less state capture but have no significant impact on corruption. The existence of multiple veto players discourages special economic interests to bribe politicians to affect the formation of new rules: buying off many political actors whose agreement is necessary for policy change might make corruption unprofitable. By contrast, veto players do not have a significant effect on bureaucratic corruption. While our empirical analysis is restricted to the postcommunist countries, our findings have broader implications for the corruption literature. Our results demonstrate that using aggregate measures of corruption might be problematic. For instance, if in a given sample of countries, bureaucratic corruption is widespread in many countries, while state capture is rare, empirical studies using measures of corruption that do not distinguish among capture and corruption might not identify important effects of political institutions, even if those exist in the context of state capture.

Declaration of Conflicting Interests

Funding

Notes

1. However, the term is usually used loosely to refer to "the power of individuals and groups to influence government actions, even if they cannot, by themselves put an end to such actions" (Gerring and Thacker 2004, 313), which deviates from the original definition of a "veto player" in Tsebelis (1995).
2. Gerring and Thacker (2004) investigate the effects of unitarism and parliamentarism on corruption, which are measured continuously rather than dichotomously.
3. According to standard veto-player theory (à la Tsebelis), greater ideological distance between veto players generally makes policy change less likely. In the context of the Gehlbach and Malesky (2010) model, the effect of ideological divergence on the passage of socially inefficient

policies is conditional. If ideological divergence is greater due to the addition of a veto player whose preferences are closely aligned with special interests, ideological divergence among multiple veto players will not reduce the likelihood of special interest policies (see Gehlbach and Malesky 2010, 964). However, if ideological divergence is greater due to the addition of a veto player whose ideological preferences are different, but still more closely aligned with unorganized interests (and social welfare) than with special interests, greater policy divergence will make the passage of special interest policies less likely.

4. This argument is along the lines of Madison's.

5. Hellman et al. (2000) base the concept of state capture on Grossman and Helpman's model (1994) where policymakers and firms conduct an auction over policies. Certainly, private firms are not the only actors that engage in state capture. However, in developing countries and emerging economies, capture of the state by private firms has had a significant impact on the success of political and economic reform, and the design of political institutions.

6. Regional governments were formally a veto player as they could veto or delay legislation through their representation in the Council of the Federation (the upper house of parliament). From 1995 up to Putin's reform of the upper chamber in 2000, the Federation Council consisted of two ex-officio members from each of Russia's eighty-nine regions. In 2000, Putin ratified a new law that replaced (from January 2002) them with delegates, chosen by the regional assemblies and chief executives.

7. Regional branches were subordinate to the State Tax Service (STS) headquarters in Moscow.

8. Prior to the reform of business regulation in the 2001 to 2004 period under Putin, any startup had to register with a number of different agencies, among which the tax ministry, the pension fund, the social security fund, and the local administration. The 2001 to 2004 reforms introduced the so-called "one-stop shop," according to which, registering a business involves a visit to just one government agency.

9. The data set includes all countries in Eastern Europe and the former Soviet Union except Bosnia-Herzegovina and Yugoslavia.

10. The World Bank (WB) has conducted similar surveys in other countries and regions, but they were done on a country-by-country basis rather than on a regional basis.

11. This is the reason why, for example, about 5 percent of the Bulgarian firms interviewed in 1999 operated in manufacturing, while about 40 percent of those firms interviewed in the Czech Republic operated in the service sector.

12. The investigators note that for various reasons, interviews with large and state-owned companies were more difficult to arrange. This firm characteristic is likely to be correlated with the dependent variable (the likelihood to engage in state capture), especially in light of Hellman, Jones, and Kaufmann's (2000) finding that larger and private firms with no state-owned predecessor are more likely to resort to capture. To address this possible bias, we control for firm size and other relevant firm characteristics. On a related note, due to the European Bank for Reconstruction and Development (EBRD) and the WB's interest in foreign investment, the sample included slightly more firms with foreign ownership than is typically found in most countries in the sample. We control for foreign ownership.

13. The samples were drawn independently in each country. The 1999 Business Environment and Enterprise Performance Survey (BEEPS) sample included at least 125 respondents from each country; the number of firms surveyed per country varies from 125 to 166 for the 1999 survey, from 170 to 268 for the 2002 survey, and from 200 to 343 for the 2005 survey. Russia, Poland, and Ukraine had significantly more firms in the sample, which is perhaps reasonable given the larger populations of those countries. Generally, the data have not been weighted based on population size, which implies that the views of enterprises in small countries were as important as those of the major countries in the region.

14. 1 corresponds to *never*; 2 corresponds to *seldom*; 3 corresponds to *sometimes*; 4 corresponds to *frequently*; 5 corresponds to *usually*, 6 corresponds to *always*.

15. Similarly, 1 corresponds to *never*; 2 corresponds to *seldom*; 3 corresponds to *sometimes*; 4 corresponds to *frequently*; 5 corresponds to *usually*, 6 corresponds to *always*.

16. According to Armingeon and Careja 2008, until 2002, in four countries (Belarus, Croatia, Moldova, and Tajikistan) there were significant constitutional changes, and some changes in other five (Albania, Kyrgyzstan, Macedonia, Poland, and Ukraine).

17. The latter are not included in most measures of veto players.

18. The patterns for other years are very similar. All bivariate correlations are above 0.8 and significant at the 99 percent confidence level.

19. While the bivariate correlations for the postcommunist subsample in the Global Competitiveness Report (GCR) survey are slightly lower, all bivariate correlations are above 0.6 and significant at the 95 percent confidence level.

20. For Presidential systems, the opposition is defined as the largest opposition party. The index's value then increases by one point for each legislative chamber and for the president, unless elections are held under closed lists and the president's party is the largest one in a particular chamber, in which case the president is not considered a check. For parliamentary systems, the opposition is defined as the three largest opposition parties. The index's value increases by one point for the prime minister and for each party in the government coalition, including that of the prime minister, unless elections are held under closed lists.

21. The measure of political fragmentation provided by Frye, Hellman, and Tucker (2000), which could be used as an alternative measure of veto players, is available only for 1999.

22. Source: World Development Indicators.

23. Hellman et al. (2000) and Hellman, Jones, and Kaufmann (2003) distinguish among small, intermediate, and large firms in their analysis of the 1999 BEEPS. Because we use all three surveys (1999, 2002, and 2005), we can only

distinguish between small and large firms using a fifty-employee cutoff point due to changing cutoff points in the three surveys.

24. The variance inflation factor (VIF) score is given by $1/(1-R^2 \text{ auxiliary})$ where R^2 auxiliary is the R^2 from regressing one independent variable on all the other independent variables. VIF shows how the variance of an estimator is inflated by the presence of multicollinearity. As a rule of thumb, a variable is considered to be highly collinear if the VIF of that variable exceeds ten.

25. We use fixed as opposed to random year effects because there are observations from three years only. Multilevel models produce poor estimates of the variance components of random effects when there are fewer than ten and especially fewer than five clusters (in our context years; see Snijders and Boskers 1999; Austin 2010). Estimates of fixed effects in a multilevel context when the number of clusters is small are not as problematic (Clarke and Wheaton 2007; Maas and Hox 2004, 2005; Mok 1995; Newsom and Nishishiba 2002).

26. It might seem puzzling that Slovenia, one of the most successful reformer, and Belarus, where reform has hardly progressed, are both classified as countries with low level of bureaucratic corruption and state capture. As Hellman et al. (2000, 9) note, the likely explanation for the low levels of bureaucratic corruption and state capture in Belarus is the disproportionate state control of the economy.

27. This question was also included in the 1999 BEEPS; however, a different scale was used for the responses, which makes comparisons to the 2002 and 2005 surveys problematic.

28. 0 corresponds to *no impact*; 1 corresponds to *minor impact*; 2 corresponds to *moderate impact*; 3 corresponds to *major impact*; 4 corresponds to *significant impact*.

29. This includes only countries with transitional or competitive systems.

30. In the models presented in Tables 12 and 13 in supplemental materials (http://prq.sagepub.com), we used POLCONV. POLCONIII and POLCONV are similar measures. POLCONIII focuses on the alignment of preferences across legislative and executive branches of government. POLCONV is derived in the same way but also takes into account the judiciary and subnational units. The results are very similar to those presented in Tables 12 and 13 if we use POLCONIII.

31. There are thirty country years of governments with more than two parties in the governing coalition and thirty-four country years of governments with one or two parties.

32. The correlation is .79 for governments with one or two parties and .58 for governments with more than two parties.

33. Because the "ordinary" ordered logit models artificially inflate the number of observations used to assess the effect of country-level factors, we assess the robustness of our results by resorting to two-stage estimation. At the first stage of the analysis, we estimate the first-level model separately for each country year. At the second stage of the analysis, we use the predicted probabilities of outcome 1 as the dependent variable and the country-level predictors as

independent variables (1 designated the response "never" to the question "How often do firms like yours make unofficial payments to affect the content of new legislation, rules, or decrees/to get things done?"). We find that our results are robust with respect to our main variable of interest. *Veto players* has a positive and significant coefficient in the state capture models (t statistic = 1.77), implying that a greater number of veto players increase the probability of a "never" response, and an insignificant coefficient in the bureaucratic corruption models. Regarding the control variables, *economic reform* is the only control variable that achieves statistical significance in the two-stage state capture models (t statistic = −2.52). In the models of bureaucratic corruption, the control variables that achieve statistical significance at the second stage are *trade* (t statistic = 1.78) and *foreign direct investment* (t statistic = 2.04).

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