ELECTORAL RULES AND CORRUPTION

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Abstract
Is corruption systematically related to electoral rules? Recent theoretical work suggests a positive answer. But little is known about the data. We try to address this lacuna by relating corruption to different features of the electoral system in a sample of about eighty democracies in the 1990s. We exploit the cross-country variation in the data, as well as the time variation arising from recent episodes of electoral reform. The evidence is consistent with the theoretical priors. Larger voting districts—and thus lower barriers to entry—are associated with less corruption, whereas larger shares of candidates elected from party lists—and thus less individual accountability—are associated with more corruption. Individual accountability appears to be most strongly tied to personal ballots in plurality-rule elections, even though open party lists also seem to have some effect. Because different aspects roughly offset each other, a switch from strictly proportional to strictly majoritarian elections only has a small negative effect on corruption. (JEL: E62, H3)

1. Introduction

Elected politicians have ample opportunity to abuse their political powers at the expense of voters. Corruption— or, more generally, extraction of political rents—is not only a problem in developing and young democracies, but also in developed and mature ones. Moreover, available measures indicate that the incidence of corruption varies substantially among countries with similar economic and social characteristics. As voters can hold their elected representatives accountable at the polls, it is natural to ask whether different electoral rules work more or less well in imposing accountability on incumbent politicians. Indeed,

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perceptions among voters of widespread abuses of power by the ruling political elite were a major factor behind the electoral reforms in Italy and Japan during the mid-1990s.

Are political rents systematically related to electoral rules? A few theoretical studies have addressed this important issue. We describe the main ideas behind existing theoretical models in Section 2. Briefly, the theory identifies three critical aspects of the electoral system: (1) the ballot structure, (2) district magnitude, and (3) the electoral formula. With regard to (1), some electoral systems make incumbents individually accountable to the voters, while others elect politicians from party lists. A party-list system weakens individual incentives for good behavior, because it creates free-rider problems and more indirect chains of delegation, from voters to parties to politicians. As for (2), fewer legislators elected in a typical electoral district (low district magnitude) may increase corruption because it raises barriers to entry. A smaller number of parties (or ideological types) present themselves at the polls and voters have less opportunity to oust corrupt politicians or parties. When it comes to (3), the electoral formula may also shape rent extraction through the sensitivity of election outcomes to incumbent performance. Since incumbents may be more severely punished under plurality rule than under proportional representation (PR), the former may be more effective in deterring corruption.

A number of empirical studies have tried to uncover economic and social determinants of corruption: we outline some of their results in Section 3, when describing our data. But the question of how electoral rules correlate with corruption in a large cross section of countries still remains unanswered.[1] Trying to fill this lacuna in the literature, we relate corruption to electoral rules as suggested by theory in a sample from the 1990s encompassing data from about 80 democracies. We use several indicators of rent extraction, measuring perceptions of the degree of corruption by public officials and ineffectiveness in the delivery of government service. The perceptions are those of business people, risk analysts and the general public.

We present results for alternative measures obtained by alternative methods. Section 4 first provides results from conventional cross-sectional estimates, where we try to identify the effect of different aspects of the electoral system. Recognizing that independent and dependent variables may be measured with error, we report on sensitivity analysis where political rents and electoral rules are measured in alternative ways. But drawing inferences from cross-sectional estimates is difficult, because omitted-variable bias is always possible. Based on the electoral reforms occurring in the 1990s, we also report on panel estimates exploiting time variation in the perceptions of corruption. We also briefly

1. In a recent paper, Kunicova and Rose-Ackerman (2002) investigate the effect of the political constitution on perceptions of corruption in a large cross section of countries. They emphasize the role of party selection of candidates and contrast electoral systems with closed and open lists. Their finding (that closed list systems are associated with more corruption) is consistent with our empirical results.

discuss the empirical results presented in Persson, Tabellini, and Trebbi (2002) and Persson and Tabellini (2003): a binary classification of electoral formulas allows these to take into account possible selection bias and nonlinearities—such as heterogeneous effects of electoral rules on corruption, depending on the cultural or historical environment.

Our results suggest that the details of electoral rules have a strong influence on political corruption. Consistent with the theoretical hypothesis on the ballot structure, corruption is higher the larger is the fraction of candidates elected on party lists. The combination of individual ballots and plurality rule seems the most effective in reducing corruption, but open-lists systems under PR (where voters may express preferences for certain names on the list) also appear to reduce corruption. Consistent with the hypothesis on district magnitude, and controlling for the ballot structure, corruption is also higher in countries electing fewer candidates per district. As systems based on PR electoral formulas tend to combine large district magnitude and citizens casting their ballot for party lists, while plurality systems tend to have small districts where citizens cast their ballot for individuals, however, corruption may not change much across a crude classification of electoral systems.

2. Theory

What can economic and political theory say about the mapping from the electoral rule to corruption or rents for politicians? Some recent analytical studies have addressed this question.

One idea is that electoral rules promoting the entry of many parties or candidates reduce rents captured by politicians. The clearest formalization is perhaps that in Myerson (1993). He assumes that parties (or equivalently, candidates) differ in two dimensions: their intrinsic honesty and their ideology. All voters prefer honest candidates but disagree on ideology. Dishonest incumbents may still cling on to power if voters sharing the same ideological preferences cannot find a good substitute candidate. The availability of good candidates depends on district magnitude. With PR and large districts (meaning that several candidates can be elected in each district), an honest candidate is always available, for all ideological positions. Dishonest candidates thus have no chance of being elected in equilibrium. But in single-member districts, the equilibrium can be very different. Even if honest candidates run for office for all possible types of ideology, only one candidate can win the election. This implies that voters vote strategically, and may vote for the dishonest but ideologically preferred candidate if they expect all other voters with the same ideology to do the same. Switching to the honest candidate risks giving the victory to a candidate on the other side of the ideological scale. In other words, small district magnitude together with strategic voting increases the barriers to entry in the electoral system, and makes it more difficult to oust dishonest incumbents from office.

In Myerson's model, voting behavior is endogenous to the electoral rule, whereas dishonesty is an exogenous feature of candidates. Ferejohn (1986) instead endogenizes the behavior of incumbents, by letting them choose a level of effort, given that voters hold incumbents accountable for their performance through a retrospective-voting rule. As shown by Persson, Roland, and Tabellini (2000), one can easily reformulate Ferejohn's model such that rent extraction is equivalent to exerting little effort, and other papers have used Ferejohn's model to analyze the determinants of corruption (e.g., Adsera, Boix, and Payne 2000). In Ferejohn's model, electoral defeat is less fearsome the higher is the probability of an ousted incumbent returning to office in the future. While Ferejohn treats this probability as an exogenous parameter, he points out that it is likely to be negatively related to the number of parties, or the number of candidates. This brings us back to the barriers of entry raised by the electoral system.

To summarize, these analyses predict that voting in single-member constituencies is less effective in containing corruption, compared to electoral systems with large districts. District magnitude and thresholds for representation are the critical features of the electoral system. Larger electoral districts and lower thresholds imply lower barriers to entry, and thus lead to less corruption and lower rents for politicians.

But electoral systems differ in two other important dimensions, namely in the electoral formula translating vote shares into seat shares, and in the ballot structure. Plurality rule awards the seats in an M seat district to the individual candidates receiving the M highest vote shares. Under PR, voters instead choose among party lists and the number of candidates elected from each list depends on the vote share of each party. Moreover, different electoral systems afford voters different degrees of choice over the candidates nominated on each list.

Persson and Tabellini (2000, Ch. 9), building on the career-concern model of Holmström (1982), suggest a model of rents and corruption resting precisely on these differences in the ballot structure associated with plurality and PR systems. The main idea is that voting over individual candidates creates a direct link between individual performance and reappointment. Individuals have strong incentives to perform well in office, by exerting effort or avoiding abuse of power. When voters choose among party lists, politicians' incentives are instead diluted by two effects. First, a free-rider problem arises among politicians on the same list. The reason is that under PR, the number of seats depends on the votes collected by the whole list, rather than the votes for each individual candidate. Second, if the list is closed and voters cannot choose their preferred candidate, an individual's chance of re-election depends on his rank on the list, not his individual performance. If lists are drawn up by party leaders (as is commonly the case), the ranking is likely to reflect criteria unrelated to competence in providing benefits to voters, such as party loyalty, or effort within the party (rather than in office). Then, individual incentives to perform well are much weaker. Persson and Tabellini's analysis therefore predicts political rents and corruption to be higher, the lower is the proportion of representatives elected via individually assigned seats, rather than party lists. Implicitly, their results also suggest that rents ought to be higher if the list is closed (i.e., voters have no choice over the ranking of individual candidates on the list) than if it is open.[2]

Finally, Persson and Tabellini (1999) suggest another mechanism whereby the electoral system may affect rent extraction. Their model studies electoral competition in two stylized systems: a "proportional system" with PR in a single nation-wide district, and a "majoritarian system" with plurality rule in several single-member districts. Electoral competition is stiffer in the latter, as candidates are induced to focus their attention on winning a majority, not in the population at large, but in "marginal districts" containing a large number of swing voters. As these voters are more willing to switch their votes in response to policy, candidates become more disciplined and extract less equilibrium rents. This prediction is somewhat imprecise on the critical features of the electoral system, in that the argument does not distinguish well between district magnitude and the electoral formula. But a similar distinction between majoritarian and proportional elections is a general and widespread idea in the political science literature.[3] This literature emphasizes the idea that the electoral outcome is more sensitive to the performance of the incumbent under majoritarian elections. Sometimes, this is attributed to the fact that this electoral rule is less likely to lead to coalition governments (and that voters find it more difficult to identify who is responsible for disappointing performance in coalition than single-party governments). Alternatively, it is argued that swings in vote shares have much more drastic consequences for seat shares and the electoral outcome under majoritarian than under proportional elections.[4]

Summarizing, the hypothesis we would ideally want to take to the data can be stated as follows:

2. Carey and Shugart (1995) have suggested that the ballot structure is important for yet another reason. In some open-list systems, as well as the SNTV, candidates compete to win preference votes against other candidates belonging to the same party. This kind of intraparty competition may induce more (rather than less) corruption. One reason is that candidates offer personal favors to voters (e.g., influence in specific public sector activities). A second reason is that they may need to raise additional electoral financing—perhaps through illegal means. At the core of this idea lies an implicit distinction between intraparty and interparty competition. Interparty competition is good for voters, because it encourages politicians to produce good legislation and good policies; intraparty competition is bad, because it may encourage illegal behavior and thus, promote corruption. Reed and Thies (2001) suggest that one motive behind the recent electoral reforms in Japan was to eliminate this kind of undesirable intraparty competition. In this paper, we do not investigate the empirical validity of this idea. But Golden and Chang (2000) find support for it in an empirical study of the Italian Christian Democrats.

3. For example, such a distinction figures prominently in the well-known work by Lijphart (1994, 1999) and Powell (2000).

4. PR makes vote and seat shares proportional almost by definition. But with plurality and single-member constituencies, the seat share changes much more rapidly with the vote share, a phenomenon often refereed to as the "cube law" in the comparative-politics literature.

H1: Larger district magnitude and lower thresholds for representation should be associated with less corruption (the *barriers-to-entry effect*).
H2: A larger share of representatives elected on an individual ballot, rather than on party lists, should be associated with less corruption (the *career-concern effect*).
H3: Plurality rule in small districts should be associated with less corruption than PR in large districts (the *electoral-competition effect*).

These theoretical predictions are complementary, in the sense of each model emphasizing a different mechanism, and more than one of them can be consistent with the same data. For example, Persson and Tabellini (1999) take the number of parties/candidates as given and do not consider the incentives of individual politicians.

Moreover, our three predictions concern the effects of combinations of the three main features of real world electoral systems, namely: (1) district magnitude, (2) ballot structure, and (3) the electoral formula. *H2* e.g., relies on a model distinguishing between different electoral systems on the basis of both (2) and (3), while *H*3 relies on a model making a distinction in terms of (1) and (3).

Finally, district magnitudes, ballot structures and electoral formulas are not independent features of real-world electoral rules, but combined in a systematic pattern. Countries with "majoritarian electoral systems" typically combine single-member districts and plurality rule where voters select individual candidates (as the archetypal British first-past-the-post system). At the opposite extreme, many "proportional systems" indeed have large districts and PR, where voters choose among party lists (Israel e.g., has just one nationwide district where all 120 representatives are elected via party lists).[5]

Because of these correlations, precise empirical testing of the three hypotheses is not a trivial task. We discuss our empirical strategy in Section 4. Before that, however, we turn to the question of how to measure the relevant aspects of electoral rules in a sample of contemporary democracies.

3. Data

We now turn to a discussion of the key variables used in the empirical analysis. These data have been collected as part of a larger research program on economic policy and comparative politics. The Data Appendix gives a succinct description of the data sources, while Persson and Tabellini (2003) provide a more comprehensive discussion.

5. Cox (1997), as well as Blais and Masicotte (1996), give recent overviews of the electoral systems across the world's democracies.

3.1 Electoral Rules and Political Institutions

Our sample consists of about 80 democracies in the 1990s. To define a democracy we rely on the surveys published by Freedom House. The so-called Gastil indexes of political rights and civil liberties, respectively, vary on a discrete scale from 1 to 7, with low values associated with better democratic institutions. For the countries included in our default sample, the average of these two indexes (*GASTIL*) in the period 1990–98 does not exceed 5. To maximize the number of countries, we adopt a generous definition of democracy, which includes countries such as Zimbabwe (although the main deterioration of democratic rights occurred after 1998). But we also report results for a more narrow sample of better democracies, with an average score of less than 3.5 in the 1990–1998 period.

The countries in our sample also differ in how long they have been democracies. This could matter: older democracies might have a better system of checks and balances to fight corruption and the abuse of power. For this reason, we record the age of each democracy (*AGE*), defined as the fraction of time of uninterrupted democratic rule going back in time (for a maximum of 200 years) from the current date until the date of first becoming an independent democracy. In the empirical work, we always control for both the quality (as measured by *GASTIL*) and age (as measured by *AGE*) of the democracy.

How do we measure the different aspects of electoral rules, given the theoretical predictions summarized in the previous section? To test the barriers-to-entry effect (*H1* in Section 2), we measure the average magnitude of voting districts (*MAGN*), defined as the number of districts (primary as well as secondary or tertiary, if applicable) divided by the number of seats in the lower house. Thus, *MAGN* is the inverse of district magnitude as commonly defined by political scientists; it ranges between 0 and 1, taking a value of 1 in a UK-style system with single-member districts and a value close to 0 in an Israel-style system with a single national district. Its expected effect on corruption is positive, according to *H1*.

In some cases, we also rely on an alternative measure of district magnitude collected and discussed by Seddon, Gaviria, Panizza, and Stern (2001). Their variable *PDM* is defined as traditional measures of district magnitude (i.e., as seats over districts), except that district magnitude is now a weighted average, where the weight on each district magnitude in a country is the share of legislators running in districts of that size. We use the measure provided by Seddon et al. (2001) as is, except that we divide it by 100 so as to constrain its value to the (0, 1) range. This variable has an expected negative effect on corruption.

The career-concern effect (*H2* in Section 2) instead focuses on the ballot structure and, indirectly, on the electoral formula. Here, the empirical counterpart to the theory is somewhat less straightforward. The theory identifies two (related) effects on corruption, due to party lists rather than individual ballots.

The first is the free-rider problem among politicians on the same list. The second is the effect of a closed list (where the ranking of candidates on the list is predetermined and cannot be changed by voters). To capture these two different effects, we use two alternative continuous measures of the ballot structure.

The first and stricter measure, called *PINDP,* is designed to reflect the free-rider effect only. It is thus defined as the proportion of legislators in the lower house who are elected on an individual ballot by plurality rule. All individuals elected via party lists are lumped together and coded as 0, irrespective of whether the list is open or closed, since they are all affected by the free-rider problem. Thus, this variable takes a value of 1 in the UK (where all legislators are elected by individual votes under plurality rule), a value of approximately 0.5 in Germany (where only about half the legislators are elected in that way), and a value of 0 in Poland (where all legislators are elected via party lists, even though voters' preferences determine the which candidates on the list get elected). Its expected effect on corruption is negative, according to *H2.*[6]

The second measure, called *PINDO,* is designed to reflect the effect of closed lists only. It is defined as the proportion of legislators in the lower house elected individually or on open lists. The legislators elected in closed lists are instead coded as 0. This different definition of individual accountability discriminates between ballots where voters choose among individuals and those where they do not, irrespective of whether there is a free-rider problem. By this measure, the UK is still coded as 1 and Germany as 0.5, but now Poland (with open lists) is coded as 1. The variables *PINDP* and *PINDO* take on different values in thirteen countries: those using a semi-proportional STV-system, plus those where voters must vote for individual politicians in open-list or panachage systems. For this variable as well, the expected effect on corruption is negative, according to *H2.*[7]

Also on this feature of the electoral rule, we refer to an alternative variable compiled by Seddon et al. (2001): it is called *PPROPN* and measures the share of legislators elected in national (secondary or tertiary) districts rather than sub-national (primary) districts. As the emphasis on collective vs. individual

6. This definition still gives rise to a few borderline cases. Thus, we classify the SNTV system used for the lower houses in Taiwan (part of the house) and Japan (before the mid-1990s reform) as individual voting under plurality rule and set *PINDP* = 1. The hybrid system in Chile formally has party lists in two-member districts. But the seats are won based on individual votes (open list), and the system has plurality in the sense that a first-ranked list that collects more than twice the number of votes as a second-ranked list wins both the seats in a district. We treat also this case as plurality rule on individual ballots and set *PINDP* = 1.

7. Party-list voting can be of three types: closed lists, open lists (or preference vote), and panachage. Closed lists do not allow voters to express a preference for individual candidates, so we set *PINDO* = 0. A preference vote on open lists may be prescribed (as in Finland), or allowed (as in Sweden) with the party list still being the default option for the vector. We code as *PINDO* = 1 only those systems where the ranking on the party lists is exclusively decided by the preferential votes (Brazil, Cyprus, Estonia, Finland, Greece, (prereform) Italy, Poland, Slovakia, and Sri Lanka). Among other PR-systems, the panachage (used in Luxembourg and Switzerland) also gives voters the option of expressing preferences across parties. Here, we set *PINDO* = 1. Finally, the PR system in Ireland is not based on party lists, but on the Single Transferable Vote, which is also used in Malta. In these cases too, we set *PINDO* = 1.

accountability may be largest for a politician running on a national party list, we sometimes use *PPROPN* as an alternative to *PINDP* or *PINDO*. The expected sign of this variable is positive (since it is defined in the opposite way relative to *PINDP* and *PINDO*).

Finally, the electoral competition effect (*H3* in Section 2) refers to a discontinuous change in both district magnitude and the electoral formula. When taking this hypothesis to the data, we classify electoral systems into "majoritarian" vs. "mixed or proportional" electoral rules, resulting in the binary (dummy) variable *MAJ*. We base the classification upon the electoral formula, but given the predominance of the two polar cases a classification based on district magnitude would not be very different. Thus, countries that elected their lower house exclusively by plurality rule, in the most recent election, are coded as $MAJ = 1$, whereas those relying on mixed or proportional rule are coded $MAJ = 0$.

All these indicators of the electoral system vary both over countries and time, due to the occurrence of electoral reforms in the 1990s. In the cross-country analysis, we only exploit the cross-sectional variation and measure each variable as the country average over the period 1990–1998. In the panel-data analysis, we measure all indicators in each year. In the last decade, five countries in our sample undertook electoral reforms significant enough to change their classification as coded by *MAJ* (Fiji, Japan, New Zealand, Philippines, and the Ukraine). A few more countries changed from proportional into mixed, but this does not affect our classification of *MAJ*. The countries where we observe significant changes in the continuous measures *PINDP, PINDO,* and *MAGN* are more numerous (the above countries plus Bolivia, Guatemala, Italy, South Korea, Venezuela, among others). We exploit this time variation in the panel estimation, dating the reform by the year of the first election under the new rules.

As already mentioned, different aspects of electoral systems are strongly correlated across countries. Table 1 lists the simple correlation coefficients among the main variables in our cross-sectional data set. The correlation coefficients among the two continuous measures *PINDP, MAGN* and the binary measure *MAJ* are all around 0.9, whereas the correlation between these three measures and *PINDO* is between 0.6 and 0.7. But while *PINDP, PINDO,* and *MAGN* are continuous measures of different features of electoral rules, the variable *MAJ* is a binary measure lumping together proportional and mixed electoral rules into one group, and those countries relying on plurality for the whole lower house in the other.

3.2 Corruption and Political Rents

It is not easy to find an empirical counterpart to rent extraction by politicians. Real-world abuse of higher political office can show up both in outright corruption and, more generally, in misgovernance. We use four different mea-

TABLE 1. CORRELATION COEFFICIENTS

	GRAFT	*ICRG*	*GOVEF*	*CPI9500*	*PINDP*	*PINDO*	*MAJ*	*MAGN*	*LYP*	*AGE*	*GASTIL*	*EDUGER*	*TRADE*
ICRG	0.809												
	0.000												
GOVEF	0.947	0.751											
	0.000	*0.000*											
CPI9500	0.970	0.844	0.911										
	0.000	*0.000*	*0.000*										
PINDP	0.101	0.307	0.085	0.068									
	0.371	*0.006*	*0.449*	*0.574*									
PINDO	−0.092	0.092	−0.049	−0.066	0.712								
	0.413	*0.421*	*0.664*	*0.583*	*0.000*								
MAJ	0.060	0.277	0.088	0.004	0.914	0.674							
	0.598	*0.014*	*0.434*	*0.974*	*0.000*	*0.000*							
MAGN	0.140	0.246	0.158	0.101	0.920	0.657	0.889						
	0.217	*0.030*	*0.163*	*0.404*	*0.000*	*0.000*	*0.000*						
LYP	−0.820	−0.655	−0.830	−0.794	−0.207	−0.055	−0.246	−0.264					
	0.000	*0.000*	*0.000*	*0.000*	*0.057*	*0.615*	*0.023*	*0.015*					
AGE	−0.629	−0.601	−0.614	−0.677	−0.061	0.017	−0.017	−0.040	0.605				
	0.000	*0.000*	*0.000*	*0.000*	*0.580*	*0.876*	*0.879*	*0.720*	*0.000*				
GASTIL	0.703	0.619	0.681	0.682	0.272	0.104	0.227	0.222	−0.744	−0.539			
	0.000	*0.000*	*0.000*	*0.000*	*0.012*	*0.345*	*0.036*	*0.043*	*0.000*	*0.000*			
EDUGER	−0.642	−0.657	−0.614	−0.642	−0.349	−0.199	−0.321	−0.298	0.696	0.425	−0.655		
	0.000	*0.000*	*0.000*	*0.000*	*0.001*	*0.073*	*0.003*	*0.007*	*0.000*	*0.000*	*0.000*		
TRADE	−0.197	−0.090	−0.197	−0.238	0.052	0.182	0.124	−0.043	0.144	−0.025	−0.003	0.046	
	0.078	*0.429*	*0.077*	*0.046*	*0.640*	*0.095*	*0.259*	*0.701*	*0.187*	*0.823*	*0.976*	*0.683*	
COL_UKA	0.012	0.225	0.123	−0.037	0.503	0.492	0.595	0.522	−0.198	−0.085	0.170	−0.272	0.340
	0.918	*0.046*	*0.275*	*0.763*	*0.000*	*0.000*	*0.000*	*0.000*	*0.070*	*0.439*	*0.119*	*0.014*	*0.002*

Note: p-values for significant pair-wise correlation in italics below coefficient.

sures of political rents; three of these refer to corruption, the fourth to (in)effectiveness in the provision of government services.

As Tanzi (1998) observes, it is difficult to define corruption in the abstract. Moreover, as corruption is generally illegal, violators try to keep it secret. Cultural and legal differences across countries make it hard to investigate corruption without taking country-specific features into account. Good proxies for political corruption should thus offer reliable information on the unlawful abuse of political power, as well as a strong level of comparability across different countries.

The Corruption Perceptions Index goes some way towards meeting these requirements.[8] It is produced by Transparency International, an NGO heavily involved in raising the public awareness about corruption and ways of combatting it. This index measures the "perceptions of the degree of corruption as seen by business people, risk analysts and the general public" and is computed as the simple average of a number of different surveys assessing each country's performance in a given year. For example, the 1998 score is based on 12 surveys from 7 different institutions. Each score ranges between 0 (perfectly clean) and 10 (highly corrupt). As discussed at length in Lambsdorff (1998), the results of these surveys are highly positively correlated: the pair-wise correlation coefficient among different surveys exceeds 0.8 on average, suggesting that the independent surveys really measure some common features. Dispersion in the ranking for an individual country is an indicator of measurement error in the average score. For this reason, we typically weigh observations with the (inverse of the) standard deviation among the different surveys available for each country.

We use this variable only in the cross-sectional analysis, taking the average of yearly country scores, available from 1995 to 2000. This variable, called *CPI9500,* is one of our measures of corruption. It is available for 72 countries, with a mean of 4.8 and a standard deviation of 2.4. The lowest recorded value is 0.3 (for Denmark) and the highest 8.3 (for Honduras and Paraguay).

An alternative corruption measure is based on a similar survey of surveys presented and discussed in Kaufman, Kraay, and Zoido-Lobatòn (1999). Here, the original surveys refer to the years 1997 and 1998. The observed survey results are combined into different clusters of governance indicators by a statistical, unobserved-components procedure. We use their sixth cluster called "Graft." According to the authors, this particular cluster captures the success of a society in developing an environment where fair and predictable rules form the basis for economic and social interactions; perceptions of corruption also play a central role. The original surveys range from -2.5 to 2.5, with higher values corresponding to less corruption. We invert and re-scale this measure to the same 0–10 scale as *CPI9500,* while keeping the same name, *GRAFT,* as in the

8. A number of recent empirical studies of corruption have employed this index, including Fisman and Gatti (1999), Treisman (2000) and Wei (1997a and 1997b).

original source. In this case as well, we weight the observations with the standard deviation of the original surveys.

Since this variable has no time variation, we only use it in the cross-sectional analysis. While *GRAFT* is based on a shorter time interval and is less focused on "grand political corruption" than *CPI9500,* it has the advantage of being available for 82 countries. It has a mean of 4.2, a standard deviation of 1.9, a minimum of 0.7 (for Denmark), and a maximum of 6.9 (for Paraguay). In spite of the a priori differences, *GRAFT* is strongly correlated with *CPI9500* (the simple correlation coefficient is 0.97). Since *GRAFT* is available for more countries, this is our preferred indicator of corruption and we use it in most of the empirical analysis. Figure 1 depicts the distribution of *GRAFT* in our sample in the form of a simple histogram.

Another cluster of governance indicators presented by Kaufman et al. (1999) instead summarizes surveys of government effectiveness (again referring to the average of 1997–1998, and not varying over time). Thus, the purpose is to combine perceptions of the quality of public-service provision, the quality of the bureaucracy, the competence of civil servants and their independence from political pressures. These scores are also recoded on the same 0–10 scale as the other measures, with higher values meaning lower effectiveness, producing the variable *GOVEF*. Like *GRAFT,* it is available for 82 democracies. *GOVEF* has the same average as *GRAFT* (4.2), a slightly lower standard deviation (1.7), and ranges from 0.8 (for Singapore) to 7.3 (for Zimbabwe). While supposedly measuring other aspects of government performance, it is still highly correlated

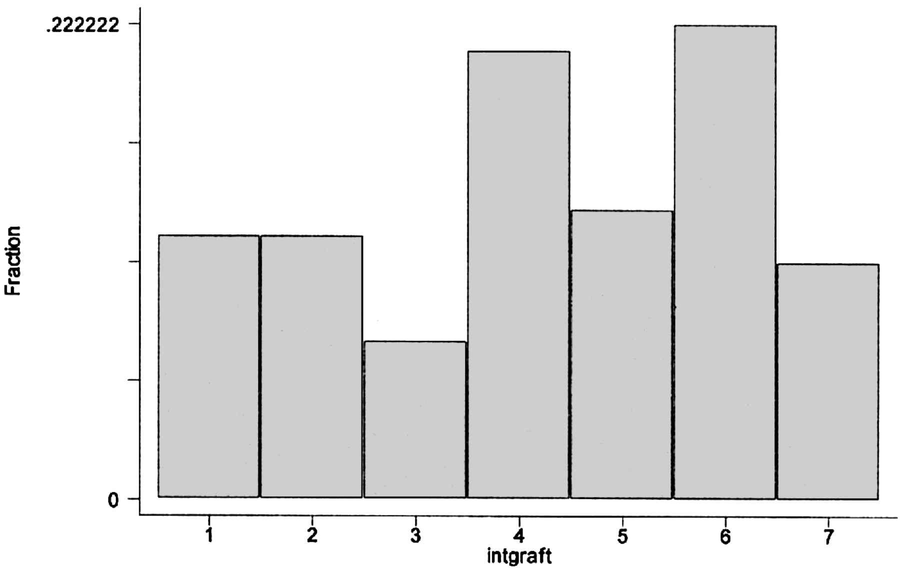

FIGURE 1. Histogram of *GRAFT*

with the corruption measures (the correlation is 0.91 with *CPI9500* and 0.95 with *GRAFT*).

Finally, the International Country Risk Guide (*ICRG*) corruption index is the only one spanning the whole 1990–1998 period, and we mainly use it in the panel analysis, to explore the effects of electoral reforms. Like the other measures, we re-scaled it to vary between 0 and 10, with higher values denoting more corruption. This index has been used in some earlier studies, including Ades and Di Tella (1999). It is released by Political Risk Services, a private think tank specialized in international political and economic country-risk assessments. The index is based on the opinion of a pool of country analysts and refers to the following issues: "high government officials are likely to demand special payments"; "illegal payments are generally expected throughout lower levels of government" in the form of "bribes connected with import and export licences, exchange controls, tax assessments, police protection, or loans."

3.3 Other Explanatory Variables

Earlier empirical work based on cross-country data has identified a number of economic, social, cultural, historical and geographical variables that correlate with the incidence of corruption. We follow these earlier studies in formulating a basic empirical specification.

To take account of economic development, we consider the logarithm of GNP per capita, adjusted for purchasing power (*LYP*), and a dummy variable for OECD membership (*OECD*). We expect both these variables to be associated with less corruption. Because earlier work has shown openness to trade to be negatively correlated with corruption (see Ades and Di Tella 1999), we also control for a measure of openness (*TRADE*), defined as the sum of exports and imports as a percentage of GDP).

Based on the existing literature, we also try some other country characteristics. Several recent studies have found a higher fractionalization of the population in the linguistic or ethnic dimension to be a significant determinant of misgovernance (see e.g., Mauro 1995 and La Porta, Lopez-De Silanes, Shleifer, and Vishny 1999). We use one widely available measure of linguistic and ethnic fractionalization, which itself is put together as an average of five different indexes (*AVELF*). This measure goes from 0 to 1, with higher values corresponding to more fractionalization. It is also likely that a more educated population will suffer less from rent extraction by politicians. To allow for this possibility, we measure the country's level of education by the secondary school gross enrolment ratio (for male and female population) (*EDUGER*). Several authors have also found religious beliefs to be significantly associated with more or less corruption (see e.g., Treisman 2000). To allow for this possibility, we use a continuous measure of the population shares with a Protestant religious tradition as measured in the 1980s (*PROT80*) and an indicator variable for Confucian dominance (*CONFU*).

Previous studies have found that perceptions of corruption are also explained by variables measuring the geographic location and the colonial and legal history of a country. Empirical studies of corruption including regional dummy variables can be found in Leite and Weidmann (1999), for Africa, and Wei (1997a), for East Asia. The effect of legal history on economic performance, including corruption, was investigated in the comprehensive study of La Porta et al. (1999), while Treisman (2000) focused on colonial history, attempting to separate the legal framework, as such, from colonial influences on a country's "legal culture." To capture the geographical aspects, we use three dummy variables for continental location. They refer to countries in Africa (*AFRICA*), Eastern and Southern Asia (*ASIAE*), and Southern and Central America including the Caribbean (*LAAM*). To measure the influence of colonial history, we partition all former colonies in our sample into three groups (the source is Wacziarg 1996): British, Spanish-Portuguese, and Other colonial origin. We then define three binary (0, 1) indicator variables for these groups (called *COL_UK, COL_ESP, COL_OTH*). Since the influence of colonial heritage is likely to fade with time, we weigh these (0, 1) indicators by the fraction of time elapsed since independence, giving more weight to colonial history in young independent states. Colonial history dating more than 250 years back receives no weight at all. The result is three truncated but continuous measures of colonial origin adjusted for the time elapsed since independence, and called *COL_UKA, COL_ESPA* and *COL_OTHA.*[9] Finally, to capture the influence of legal origin, we follow La Porta et al. (1999) and classify the origin of legal systems into five different categories: Anglo-Saxon common law, French civil law, German civil law, Scandinavian law and Socialist law. We use the first four of these categories, creating four dummy variables: *LEGOR_UK, LEGOR_FR, LEGOR_GE,* and *LEGOR_SC.*

We have also tried including other control variables suggested by the literature, such as population size (*LPOP*), the fraction of Catholics (*CATHO*80) and a federal constitution (*FEDERAL*). But these rarely turn out to be statistically significant. To preserve some parsimony in the specification, these variables are not included in our basic specification (although the results of interest remain virtually unchanged if they are).[10]

Some of the variables listed above vary over time, some do not. In the cross-sectional analysis, observations of all variables always correspond to the

9. Thus, for instance, the variable *COL_UKA* is defined as *COL_UK* $*$ (250 $-$ years of independence)/250.

10. Because other studies have found media diffusion to be correlated with corruption, we have also included among our regressors measures of the number of TVs or internet connections per household. But as these variables did not have additional explanatory power, we did not retain them either in our final specification.

country average over the period 1990–1998. Naturally, in the panel analysis we only include annual observations of the time-varying variables.

3.4 Preliminary Analysis

In this subsection, we report some preliminary statistical analysis for the cross-sectional data. To save space, and given the high correlation among all measures of corruption, in this subsection we focus exclusively on the variable *GRAFT* which is available for more countries. Results for the other indicators of political rents and corruption are very similar.

Table 1 shows the correlation coefficients among some of the main variables. A number of these are highly correlated, as expected. Richer economies have more educated populations and are better and older democracies. Judging from the simple correlations, corruption is lower in richer economies, in better and older democracies, and countries where the population is better educated.

As mentioned earlier, the electoral variables of most interest, *PINDP* (alternatively *PINDO*), *MAJ,* and *MAGN,* are highly positively correlated with each other. Multicollinearity may thus be a problem, particularly if the variables are predicted to affect corruption in the same direction (as *PINDP* and *MAJ*). On the other hand, these variables are not very strongly correlated with other independent variables (with the exception of *COL_UKA*), which suggests that multicollinearity with the other controls is unlikely to be a major problem. Note that the electoral variables display little direct correlation with corruption.

Before turning to a systematic analysis of electoral rules, we ask how well the observed cross-country variation in corruption can be explained by other social, economic and institutional variables. A concise summary of the answer is given in Figure 2. Here, we display the distribution of the residuals in *GRAFT* from a regression encompassing the standard determinants of corruption discussed in the previous subsection, including colonial history (the specification omits the measures of the electoral rule and the dummy variable for geographic location and legal history). Altogether, the basic economic, social and historical variables explain over 85 percent of the variation in the data. The residuals range from −1.86, for Chile, to +2.11, for Papua New Guinea (the way we measure *GRAFT,* a negative residual means less corruption than predicted). Other countries with residuals close to 1.5 or more in absolute value include Cyprus and Senegal (both negative), Belgium, Bahamas, Venezuela, and Jamaica (all positive). Clearly, our basic controls eliminate the most striking differences across countries.

The precise specification and estimated coefficients of the regression generating these residuals are displayed at the bottom of Figure 2.[11] Corruption is

11. Estimation is by weighted least squares, the weights being the (inverse) standard deviation of *GRAFT*.

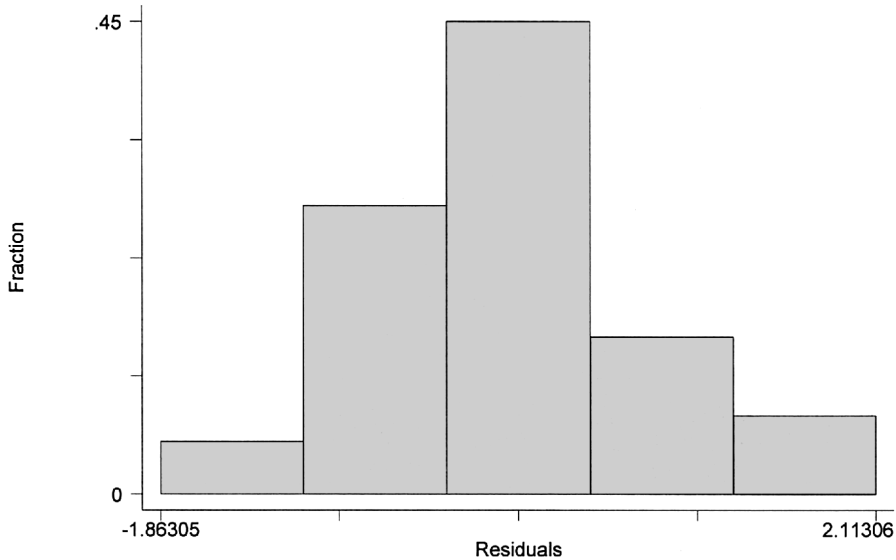

FIGURE 2. Histogram of *GRAFT* Residuals

Notes: Residuals generated from the following regression (standard errors in parenthesis):

GRAFT = 13.10(1.82) − 0.86(0.21)*LYP* − 1.24(0.39)*OECD* − 0.01(0.008)*EDUGER* − 0.006(0.002)*TRADE* − 0.05(0.52)*AVELF* − 0.01(0.004)*PROT*80 + 0.12(0.39)*CONFU* − 0.06(0.59)*AGE* + 0.18(0.14)*GASTIL* − 0.94(0.36)*COL_UKA* + 0.15(0.79)*COL_ESPA* + 0.10(0.32)*COL_OTHA*

Adj. R2: 0.82; Obs.: 80; Estimation by WLS.

lower in richer (*LYP*) and more open (*TRADE*) economies, in the OECD (*OECD*) and old British colonies (*COL_UKA*), and in countries with better educated (*EDUGER*) or many Protestant (*PROT*80) inhabitants. These results generally conform to earlier studies and prior expectations (see, in particular, Treisman 2000). Contrary to the impression from the simple correlation coefficients in Table 1, however, the quality and age of democracy (as measured by *GASTIL* and *AGE*) do not have estimates significantly different from zero, perhaps because these variables are so collinear with income and education.

When we add the indicators for continental location still omitting the measures of electoral rules, the distribution of residuals shrinks and the R^2 of the regression rises further. The estimated coefficients displayed at the bottom of Figure 2 are quite unaffected, but countries located in Latin America tend to have more corruption than others. To be on the safe side, we include the continental dummy variables in our basic specification.

We have also experimented with replacing colonial history by legal origin. The overall effect is similar, with Anglo-Saxon and Scandinavian legal origin having the strongest negative effects on corruption, relative to the default of Socialist legal origin. Not surprisingly, Anglo-Saxon legal origin seems to pick up the same features as British colonial origin. For the rest, the results are not affected much. Since the specification with the colonial origin indicators are the least favorable to the results on electoral rules, we always use colonial rather than legal origin.

4. Results

4.1 Specification

As discussed in Section 2, the predictions we want to take to the data are not mutually exclusive, each prediction emphasizing a different aspect of electoral rules. This suggests that we ought to estimate a comprehensive specification, where we include all three measures discussed in Section 3, namely the indicators for district magnitude (*MAGN*), ballot structure (either *PINDP* or *PINDO*), and the electoral formula (*MAJ*). Given that *PINDP, PINDO,* and *MAGN* vary continuously, while *MAJ* is a binary measure of features of the electoral formula correlated with district magnitude and ballot structure, this comprehensive specification can also be considered as a test for nonlinear effects (i.e., does a change in the electoral rule for the whole legislature have additional effects on corruption, besides those captured by the continuous indicators). Our first benchmark specification thus includes all three measures of the electoral rule.

As already noted, however, these three indicators do not have much independent variation. In particular, it is difficult to disentangle the effect of the electoral formula and that of the ballot structure when the latter is measured by *PINDP,* since the correlation coefficient between *PINDP* and *MAJ* is 0.91, and these two variables have the same expected effect on corruption. For this reason, we systematically try a more parsimonious specification, where we always include the measure of district magnitude (*MAGN*), but drop one of the other two indicators, either for the electoral formula or the ballot structure. While the estimated coefficient of district magnitude (*MAGN*) unambiguously captures the barriers-to-entry effect (hypothesis *H1*), the estimated coefficient of the other included variable could capture the effect of either the career-concern effect (*H2*) or the electoral formula (*H3*), irrespective of how it is measured. We also systematically experiment with our two different measures for the ballot structure, *PINDP* and *PINDO.* Naturally, the interpretation of the results changes with the specification.

For the rest, the cross-sectional specification always includes the economic, social and historical variables described in Section 3 and listed at the bottom of Figure 2. Generally, we also control for continental location to minimize the risk of omitted-variable bias. To help reduce the noise introduced by measurement error, the estimation method is weighted least squares, with weights given by the (inverse) standard deviation of the perceptions of corruption. The results when we estimate with OLS are similar, although the (robust) standard errors of the estimates are slightly higher. If the standard deviation of the perceptions of corruption is not available, as when corruption is measured by *ICRG,* we estimate by OLS and report robust standard errors.

4.2 Basic OLS estimates

Our first regression results are reported in Table 2. As we have the largest number of observations for *GRAFT,* we start with this corruption measure as our dependent variable. In the first four columns, we measure the ballot structure with the strict measure of voting over individuals, *PINDP*. Column 1 reports on the most comprehensive specification, where we include all three indicators for the electoral rule. All three estimated coefficients have the expected sign, i.e., negative for *PINDP* and *MAJ* and positive for *MAGN*. While the coefficient on district magnitude is clearly significantly different from zero (p-value of 0.015), the coefficients on the ballot structure and the electoral formula are not (p-values of 0.22 and 0.33, respectively). As noted previously, the latter might be due to the high correlation between the variables *PINDP* and *MAJ*. As shown in Columns 2 and 3, when we drop either of these variables, the remaining one becomes significantly different from zero, while district magnitude also remains strongly significant. Column 4 shows that the coefficients become larger in absolute value and more precisely estimated when the sample is cut by 25 percent, restricting it to better democracies (i.e., those with an average *GASTIL* score smaller than 3.5).

Notice that the estimated coefficients of *PINDP* (alternatively *MAJ*) and *MAGN* are large (all variables are defined so that they lie between 0 and 1) and their standardized beta coefficients are the largest of all regressors. For example, switching from a system where all legislators are elected by PR on party lists ($PINDP = 0$), to one where all are elected by plurality as individuals ($PINDP = 1$) is estimated to reduce the perceptions of corruption by about 20 percent (2 points out of 10) in the sample of good democracies. This is about twice the effect of not being a Latin-American country. The estimated effect of inverse district magnitude (also taking positive values below 1) is even larger, though it is a bit less stable to the specification. Due to the strong correlation between *PINDP* and *MAJ,* it is hard to give an unambiguous interpretation of these estimates, however. They are consistent with a negative career-concern effect, as well as a negative electoral-competition effect (or with both effects being operative at the same time).

While the variable *PINDP* captures the free-rider problem associated with list voting under PR, it does not discriminate between open and closed lists. To capture this second effect of electoral rules suggested by the career-concern model, we replace *PINDP* by the other measure of ballot-structure, *PINDO.* As discussed in Section 3, this means that we now also treat as individually elected

TABLE 2. POLITICAL RENTS AND ELECTORAL RULES; CROSS-SECTIONAL ESTIMATES

Dependent variable	(1) *GRAFT*	(2) *GRAFT*	(3) *GRAFT*	(4) *GRAFT*	(5) *GRAFT*	(6) *GRAFT*	(7) *GRAFT*	(8) *GRAFT*
PINDP	−0.81	−1.48		−1.91				
	(0.83)	(0.62)**		(0.74)**				
MAJ	−0.67		−1.03		−0.90	−1.01		−1.03
	(0.54)		(0.41)**		(0.53)**	(0.44)**		(0.44)**
MAGN	1.94	2.07	1.36	2.61	1.55	1.82	0.77	1.22
	(0.77)**	(0.77)***	(0.49)***	(0.90)***	(0.47)***	(0.53)***	(0.36)**	(0.52)**
PINDO					−0.42	−0.51	−0.52	−0.53
					(0.27)	(0.26)*	(0.27)*	(0.28)*
F-test	4.43**	3.68**	4.01**	4.31**	4.89**	6.05***	2.73*	3.62**
Continents	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Colonies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Sample	Default	Default	Default	Good democracies	Default	Good democracies	Default	Default
Estimation	WLS	WLS	WLS	WLS	WLS	WLS	WLS	WLS
Observations	80	80	80	60	80	60	80	80
Adj. R2	0.85	0.85	0.85	0.87	0.85	0.88	0.84	0.82

Notes: Standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.
F-test refers to the joint significance of the electoral variables.
Good democracies have values of *GASTIL* smaller than 3.5.
All specifications include the variables *LYP, AGE, GASTIL, EDUGER, OECD, TRADE, AVELF, PROT80,* and *CONFU.*

those politicians obtaining their seats via (semiproportional) STV systems and via open lists in PR party-list systems. Recall that *PINDO* indeed has a lower correlation (0.67) with our measure of the electoral formula (*MAJ*) than *PINDP*. Column 5 reports on the same comprehensive specification as Column 1. Inverse district magnitude continues to have a significant positive effect on corruption. The coefficient on the indicator for plurality rule (*MAJ*) goes up by a third (in absolute value) and now becomes significantly different from zero. On the other hand, the coefficient on individual ballots (*PINDO*) falls somewhat (in absolute value) but also becomes more precisely estimated (p-value of 0.12). When the sample is restricted to good democracies in Column 6, the point estimates remain about the same, but now all three coefficients are significantly different from zero. When we omit *MAJ* from the regression, as in Column 7, the coefficient on the ballot structure stays roughly the same. But the coefficient on district magnitude drops by more than half its previous value: as district magnitude is strongly correlated with the electoral formula, this coefficient is forced to pick up the negative effect of the omitted variable.

Finally, Column 8 shows the effect of dropping the indicators of continents and colonial history from the specification in Column 5. The effect of all three electoral indicators now becomes statistically significant. If we instead replace colonial history by our measures of legal history in the specification of Column 5, this also raises the precision of the estimates such that all three electoral variables are once more significantly different from zero (results nor shown). These results are reassuring, because a history as a former British colony or a British-style legal system, in particular, not only appears to reduce the current propensity for corruption. It also tends to exert a strong influence on a country's electoral institutions, making a British style first-past-the-post system much more likely (*MAJ* has a correlation of 0.60 with *COL_UKA* and 0.70 with *LEGOR_UK*). Thus, the results had better be robust to including these variables as controls. As a final check, the results are also robust to cutting the most influential observations, following the approach recommended by Belsley, Kuh, and Welsh (1980).[12]

Judging from these estimates, the free-rider problem captured by the indicator *PINDP* seems to have a stronger and more robust effect on corruption, compared the closed-list system captured by *PINDO*. If the ballot structure indeed shapes corruption, the effect seems to go through the incentive problems associated with free riding, while the distinction between open and closed lists

12. There is one important fragility in the estimates reported in Table 2, however. It concerns Chile, a country with low corruption (and a highly negative estimated residual) and a peculiar electoral system. As noted in Footnote 6, Chile's electoral system is hard to classify. Unfortunately, this single observation and our classification matter for our results. Dropping Chile from the sample, or reclassifying its electoral system so that $MAJ = 0$ (rather than 1) and $PINDP = 0$ (rather than 1), the estimated effects of the electoral variables on corruption become less precisely estimated and lose significance. Chile is not the only outlier observation, however, and dropping Chile together with other influential observations does not significantly affect the results reported in Table 2.

seems less important. As noted before, however, the variable *PINDP* might also reflect the importance of the electoral formula, as emphasized by the electoral-competition effect (hypothesis *H3*).

4.3 Estimates with Alternative Measurement

We next report on further sensitivity analyses of the basic cross-sectional results carried out at the cost of cutting the size of our sample. In Table 3, we first rerun the basic specification in Column 1 of the previous table, now using the Seddon et al. (2001) measures of the electoral rule. Recall that *PPROPN* measures the share of legislators elected in national (as opposed to local) districts and is thus an inverse measure of individual accountability (not a direct measure as *PINDP*) and that *PDM* is a direct measure of district magnitude (not an inverse measure as *MAGN*). The results in Column 1 are thus similar to the results in Table 2.

Our dependent variable, being a survey of surveys, is clearly measured with error. This is the rationale for our WLS estimation, attaching lower weights to observations where the different components of the perception index are more divergent. In Table 3, we carry out further sensitivity analysis, with alternative measures for our dependent and independent variables. We first use *CPI9500* as the dependent variable (recall from Section 2 that we have re-scaled all the rent

TABLE 3. POLITICAL RENTS AND ELECTORAL RULES; ALTERNATIVE MEASUREMENT

Dependent variable	(1) *GRAFT*	(2) *CPI9500*	(3) *CPI9500*	(4) *CPI9500*	(5) *GOVEF*	(6) *GOVEF*	(7) *GOVEF*	(8) *ICRG*
PPROPN	0.89			1.70			0.92	
	(0.44)*			(0.61)***			(0.51)*	
PDM	−0.87			−1.81			−0.97	
	(0.43)**			(0.64)***			(0.52)*	
MAJ	−0.25		−1.22	−0.39		−0.90	−0.24	0.75
	(0.26)		(0.57)**	(0.38)		(0.49)*	(0.28)	(1.09)
PINDP		−2.12			−1.54			
		(0.86)**			(0.72)**			
MAGN		2.83	1.67		1.79	1.00		0.19
		(1.02)***	(0.66)**		(0.88)**	(0.59)*		(1.30)
PINDO			−0.13			−0.16		−0.17
			(0.36)			(0.31)		(0.36)
F-test	2.34	3.84**	3.39**	4.13**	2.33	1.82	2.02	2.54*
Continents	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Colonies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Estimation	WLS	WLS	WLS	WLS	WLS	WLS	WLS	OLS
Sample	Default	Default	Default	Default	Default	Default	Default	Default
Observations	73	70	70	63	80	80	73	78
Adj. R2	0.84	0.88	0.88	0.89	0.77	0.76	0.79	0.70

Notes: Standard errors in parentheses (robust errors in Column 8).
* significant at 10%; ** significant at 5%; *** significant at 1%.
F-test refers to the joint significance of the electoral variables.
All specifications include the variables *LYP*, *AGE*, *GASTIL*, *EDUGER*, *OECD*, *TRADE*, *AVELF*, *PROT*80, and *CONFU*.

extraction measures to run between 0 and 10). Columns 2 and 3 correspond to columns 2 and 5 in Table 2, while Column 4 corresponds to Column 1 in Table 3. Basically, the results are the same as before. The results in the specifications with *PINDP* and the Seddon et al. variables indicate slightly stronger effects of the electoral system than the results for *GRAFT*. The results in the specification with *PINDO* indicate even more strongly than in Table 2 that it is the lack of a free-rider problem under plurality rule rather than open vs. closed lists that has bite on corruption. Alternatively, they indicate that the electoral-competition effect is stronger than the career-concern effect.

Columns 5–7 show the results of the same specifications when we instead use *GOVEF,* the measure of ineffectiveness in the provision of public services, as the dependent variable. The general pattern of results is the same as for *CPI*9500, even though the coefficients of interest are less precisely estimated and the general fit of the regression is poorer.

Finally, Column 8 shows an example of the results when we use the average of our time-varying corruption measure *ICRG* as the dependent variable. Here, the results are more disappointing, given our theoretical hypotheses. None of the measures of interest has a statistically significant coefficient (even though the three variables together are marginally significant). Individual accountability appears to be important when measured by *PPROPN* (result not shown). As for *GOVEF,* the fit of the regression is considerably poorer than previously, indicating that *ICRG* is a noisy measure of corruption. Unfortunately, as the *ICRG* measure is derived from a single source, we cannot use the WLS approach to adjust our estimates for measurement error in this case.

4.4 Panel Estimates

As noted in Section 3, a number of countries undertook electoral reforms during the nine years of the 1990s for which we have data. Five countries pursued reforms changing our indicator variable for the electoral formula, *MAJ*. In addition to some marginal adjustments, about a dozen countries reformed their electoral systems so as to produce significant changes in our continuous variables for the ballot structure *PINDP* or *PINDO,* and district magnitude, *MAGN*. In this subsection, we ask whether these reforms had an impact on corruption, as measured by the time varying index *ICRG* (the only of our rent indexes with enough time variation).

Exploiting this time variation in the data provides additional information, despite the relatively small number of reforms, because it allows us to relax the assumption of conditional independence underlying our cross-sectional estimates. Specifically, (non time-varying) omitted variables jointly determining the corruption levels and the structure of the electoral system are unlikely to cause problems in this panel. While providing a useful check on our earlier estimates, these panel estimates are no panacea: time variation does not guarantee exogeneity. Obviously, we must still assume that the political events leading to the observed electoral reforms were not accompanied by significant changes in unobserved determinants of corruption.

As for the cross-sectional estimates, we start by a comprehensive specification including changes in all aspects of electoral rules, ballot structures, electoral formulas, and district magnitudes, and then continue by restricting the specification to only two electoral variables. We have considered three specifications. To control for time-invariant determinants of corruption, such as colonial origin or location, we always include country fixed effects. In a first specification, we just include the measures of electoral rules besides the country fixed effects; thus, observations are in deviations from country means, and we essentially ask whether one-time changes in the electoral rule have significantly increased or decreased corruption in the countries that underwent reform. In a second specification, we add year fixed effects, allowing for common events that influence corruption in all countries. Finally, in the third specification, we also add those determinants of corruption that exhibit some time variation, namely per capita income (*LYP*), quality of democracy (*GASTIL*), and openness (*TRADE*).[13] Because the second specification produces results similar to the first and third one, we do not report the estimates from that specification. Note that since the estimates only reflect the time variation in the data, the results concerning the electoral formula are not so interesting: the binary variable *MAJ* varies over time for only five countries, perhaps too few to draw any precise inference. As the other indicators of electoral rules display more time variation, their estimated coefficients are more meaningful.

The results are displayed in Table 4, where the first three columns use the stricter measure of individual ballots (*PINDP*) and the last three the broader measure, including STV and open-list PR (*PINDO*). Columns 1 and 2 are comparable to the comprehensive specification in Column 1 of Table 2, whereas Column 3 is similar to the restricted specification of Column 2 of the earlier table. The panel estimates are remarkably similar to the cross-sectional estimates. Increasing the proportion of legislators individually accountable under plurality rule (increasing *PINDP*) thus significantly reduces corruption, as does increasing district magnitude (reducing *MAGN*), while changes in the electoral formula have no additional effect beyond their effect through *PINDP*. Our estimated coefficients of interest are significantly different from zero and stable across specifications; the point estimates are about as large as the cross-sectional estimates. (When included, the time-varying covariates with significant coefficients have the expected effect, namely higher incomes and improving democracies are correlated with lower corruption.)

The results in columns 4–6 are, again, quite stable across specifications.

13. Among the variables in our basic specification, education (*EDUGER*) also varies over time. Because we lack data on its time variation for a number of countries, we do not include it among our time-varying controls, however.

TABLE 4. POLITICAL RENTS AND ELECTORAL RULES; PANEL ESTIMATES

Dependent variable	(1) *ICRG*	(2) *ICRG*	(3) *ICRG*	(4) *ICRG*	(5) *ICRG*	(6) *ICRG*
PINDP	−1.52	−1.41	−1.61			
	(0.74)**	(0.70)**	(0.65)**			
MAJ	−0.46	−0.33		−0.19	−0.15	
	(0.44)	(0.44)		(0.47)	(0.46)	
MAGN	1.40	1.67	1.72	0.46	0.82	0.80
	(0.72)*	(0.68)**	(0.68)**	(0.55)	(0.53)	(0.53)
PINDO				−1.99	−1.70	−1.82
				(0.76)***	(0.72)**	(0.64)***
F-test	2.64*	3.49**	3.84**	0.39	3.94**	4.83***
Country effects	Yes	Yes	Yes	Yes	Yes	Yes
Time effects	No	Yes	Yes	No	Yes	Yes
Covariates	No	Yes	Yes	No	Yes	Yes
Observations	640	623	623	640	640	623
No. of countries	78	78	78	78	78	78
Within R2	0.01	0.12	0.12	0.02	0.12	0.12

Notes: Standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.
F-test refers to the joint significance of the electoral variables.
Time-varying covariates are *LYP, GASTIL,* and *TRADE.*

These estimates paint a slightly different picture. When it comes to district magnitude, the coefficients—albeit less precisely estimated—resemble the cross-sectional results in Columns 6–8 of Table 2. The estimated effect of the ballot structure is now much stronger, however, suggesting that a switch from closed lists to individual ballots would reduce corruption as measured by *ICRG* by as much as 2 points out of 10 (a full standard deviation in our sample). As in Columns 1–3, the introduction of plurality rule has no additional effect on corruption beyond its effect through the ballot structure. Thus here, contrary to what we found in the cross-sectional estimates, the career-concern effect does not seem to work exclusively through the free-rider problem, but also through the distinction between open and closed lists.

The estimates reported in Table 4 are entirely based on the time-series variation in electoral rules and corruption. Their similarity with the earlier cross-sectional estimates lend further support to the hypotheses of a career-concerns effect, as well as a barriers-to-entry effect.

4.5 Discussion

We noted in Section 3 that the pair-wise cross-country correlation between each measure of the electoral rule and corruption is not significantly different from zero (recall Table 1). In this section, we instead document strong and significant effects of the electoral variables on corruption. It might be conjectured that the difference emanates from our controlling for a variety of economic and social determinants of corruption (in the cross-sectional estimation) or exploiting the time variation (in the panel estimation). But this conjecture is false, or at best only half-true. Consider first the cross-country regressions. If we retain the basic specification of Table 2 (including the dummy variables for continental location and colonial origin) and add the electoral variables one by one in isolation, none of them is ever significantly different from zero, in consistency with the insignificant binary correlations. A strong and statistically significant effect of the electoral variables is only detected if we condition simultaneously on two features of the electoral system (district magnitude and either the ballot structure indicators or the binary indicator for the electoral formula). Similar results are obtained if we enter the electoral indicators one by one in the panel analysis of Table 4, although here a drop in statistical significance does not always occur, or is less stark than in the cross-country regressions.

A previous version of the paper (Persson, Tabellini, and Trebbi 2002), studied more in depth the effect on corruption of the electoral formula alone, as captured by the variable *MAJ*.[14] Because this is a binary indicator, we can estimate its effect on corruption from cross-country data exploiting now standard techniques in the econometric literature on the treatment effect—see, e.g., Wooldridge (2002, Ch. 18) for a textbook overview.[15] These estimation techniques are more general and allow us to relax the assumptions of linearity and conditional independence implicitly underlying the cross-country regressions of Tables 2 and 3. Conditional independence essentially means that the electoral rules are randomly assigned to countries, given the other included regressors. This is clearly a strong assumption. Even though our regressors always include colonial origin and continental location, it is always possible that we have omitted some other historical or social variable influencing both corruption and the electoral rule. To relax this assumption in our previous work, we estimated the effect of *MAJ* on corruption by instrumental variables as well as the Heckman estimation procedure allowing for nonrandom treatment.[16] To relax the linearity assumption, we rely on nonparametric matching methods, based on the propensity score. These more general estimation methods confirm the negative results of the simple regressions: the electoral formula alone does not have any significant effect on corruption.

While the sensitivity to the set of conditioning variables reduces the generality of our inference, it has a plausible explanation. The alternative theories summarized in Section 2 are complementary, each emphasizing a different feature of electoral rule. Moreover, district magnitude is strongly

14. See also Persson and Tabellini (2003).

15. Even though the variable *MAJ* is binary on yearly data, when we take averages over the period 1990–1998, it becomes a fraction for the few countries undertaking reform in that period. In Persson, Tabellini, and Trebbi (2002) we thus code *MAJ* according to its value before the reform, to preserve it as a binary variable.

16. Our instruments are based on the date of origin of the current electoral rule, and exploit historical waves (or "fashions") in the design of electoral rules.

negatively correlated with the other electoral indicators in the real world. Thus, if we only include one feature of the electoral rule in our specification, a standard omitted-variable problem biases the estimate of the included variable towards zero. Indeed, when at least two electoral variables are included, their estimated coefficients are jointly statistically significant, as shown by the F-statistics in Tables 2 and 4.

This interpretation of our results also suggests that a comprehensive electoral reform, going from a Dutch-style electoral system with closed party lists in a single national constituency to a UK-style system with first past the post in one-member districts—i.e., moving *PINDP* (or *PINDO*), *MAJ,* and *MAGN* from (approximately) 0 to 1—would have counteracting effects on corruption, producing a net result close to zero.

5. Conclusion

This paper has presented new empirical results on how electoral rules affect political corruption. The main lesson of the data is that corruption is affected by electoral institutions, but what matters is the comprehensive design of the electoral rule, not just a single feature.

Our empirical results differ somewhat depending on exactly how we measure the dependent and independent variables of interest, and whether we exploit the cross-sectional or time-series variation in the data. Overall, however, the results are broadly consistent with all three theoretical hypotheses, *H1–H3,* summarized in Section 2, even though discriminating sharply among the three may be difficult because of the collinearity among our electoral indicators. Countries with smaller electoral districts tend to have more corruption, as predicted by the barriers-to-entry hypothesis. Countries predominantly voting for individuals tend to have less corruption than those predominantly voting for party lists, as predicted by the career-concern hypothesis. There is also some support for an additional effect of plurality elections. This can be viewed as evidence for the electoral-competition hypothesis, or as stronger individual career concerns in plurality systems than in open-lists systems.

The estimated effects of these details of electoral rules are nontrivial. For instance, they may suggest a reason why Estonia has so much less corruption than neighboring Latvia—a *GRAFT* value of 3.8 vs. 5.5—a puzzle for some observers (see e.g., Bennich-Björkman 2002). According to the cross-sectional estimates in Table 2, 30 percent of the difference is due to Estonia's open-list system, while according to the panel estimates in Table 4, the entire difference is. Another case of stark differences between neighboring countries is Chile and Argentina with *GRAFT* values of 2.9 vs. 5.5. The cross-sectional estimates attribute about 30 percent of the difference to Chile's use of plurality rule in two-member districts, with the caveat about classification in Footnote 6. If one alternatively treats Chile as an open-list PR system (Argentina has closed lists), the panel estimates attribute more than half the corruption difference to the electoral rule.

We believe the results to be relevant for the design of real-world electoral systems. For instance, our estimates suggest that Belgium—an outlier with much higher corruption than predicted—could cut its corruption level towards that of France by holding its legislators individually accountable at the elections. Our results also suggest that each feature of Japan's recent electoral reform—scrapping plurality rule in some districts and diminishing average district magnitude—might actually increase corruption. Italy's electoral reform—abandoning PR in favor of plurality rule with direct elections of individuals in single member districts for 75% of the legislature—should instead have a mixed effect on corruption. While more direct individual accountability with plurality voting is a step in the right direction, the reduction of average district magnitude has a countervailing effect. Similarly, New Zealand's reform of its strictly majoritarian elections, towards a mixed system with less individual accountability but higher average district magnitude, might have an ambiguous effect on corruption.

Data Appendix

AFRICA: regional dummy variable for African countries, taking the value of 1 if a country is African, 0 otherwise.

AGE: age of democracy. Defined as: $AGE = (2000\text{-}DEM\_AGE)/200$ and varying between 0 and 1, with US being the oldest democracy (value of 1). Source: see *DEM_AGE.*

ASIAE: regional dummy variable for East Asian countries, taking the value of 1 if a country is East Asian, 0 otherwise.

AUTOC: institutionalized autocracy indicator on a scale from 0 to 10. Derived from codings of the competitiveness of political participation, the regulation of participation, the openness and competitiveness of executive recruitment, and constraints on the chief executive. Source: Polity IV Project ⟨http://www.cidcm.umd.edu/inscr/polity/index.htm⟩

AVELF: index of ethnolinguistic fractionalization. Approximates the level of lack of ethnic and linguistic cohesion within a country. Ranges from 0 (homogeneous) to 1 (strongly fractionalized) and averages 5 different indexes. Source: La Porta et al. (1999). For Central and Eastern Europe countries computations follow Mauro (1995) with data from Quain (1999).

*CATHO*80: percentage of the population belonging to the Roman Catholic religion in 1980. Source: La Porta et al. (1999).

CLIST: indicator for closed party lists. Sources: see *LIST* and *SEATS.*

COL_ESPA: $COL\_ESPA = COL\_ES * (250 - T\_INDEP)/250$. Combined effect of *COL_ES,* describing if a country was a colony of Spain or not, and

T_INDEP, coding years of independence from 0 to 250 (latter value used for countries that were never colonized). Source: Wacziarg (1996).

COL_OTHA: COL_OTHA = *COL_OTH* ∗ (250 − *T_INDEP*)/250. Defined analogously to *COL_ESPA*. Source: Wacziarg (1996).

COL_UKA: COL_UKA = *COL_U*K ∗ (250 − *T_INDEP*)/250. Defined analogously to *COL_ESPA*. Source: Wacziarg (1996).

CONFU: religious tradition dummy, equal to 1 if the majority of the population is Confucian/Buddhist/Zen, 0 otherwise. Source: Wacziarg (1996), CIA—The World Factbook 2000.

*CPI*9500: corruption perception index. Average of the *CPI* Index over the period 1995–2000. Source: Transparency International (www.transparency.de) and Internet Center for Corruption Research ⟨www.gwdg.de/ũwvw⟩.

DEM_AGE: year of birth of the democracy. Corresponds to the first year of an uninterrupted string of positive yearly *POLITY* values until the end of the sample, given that the country is also an independent nation (foreign occupation during WWII not considered an interruption of democracy). Source: see *POLITY.*

DEMOC: institutionalized democracy indicator on a scale from 0 to 10. Derived from codings of the competitiveness of political participation, the regulation of participation, the openness and competitiveness of executive recruitment, and constraints on the chief executive. Source: Polity IV Project ⟨http://www.cidcm.umd.edu/inscr/polity/index.htm.⟩

DISTRICTS: number of primary as well as secondary and tertiary (if applicable) districts in elections to the lower house. Sources: Quain (1999) and Kurian (1998), Cox (1997), and national constitutional documents.

EDUGER: total enrolment in a specific level of education, regardless of age, expressed as a percentage of the official school population corresponding to the same level of education in a given school-year. Computed by dividing the number of students at a given level of education regardless of age, by the population of the age-group officially corresponding to the given level of education, and multiplying the result by 100. Source: UNESCO—Education Indicator—Category Participation ⟨www.unesco.org⟩.

FEDERAL: federalism dummy. Source: Adsera, Boix and Payne (2000).

GASTIL: average of Gastil indexes for civil liberties and political rights. Measured on a scale from 1 to 7 with 1 representing the highest degree of freedom. Countries whose combined averages for political rights and for civil liberties fall between 1.0 and 2.5 are designated "free," between 3.0 and 5.5 "partly free" and between 5.5 and 7.0 "not free." Source: Freedom House, Annual Survey of Freedom Country Ratings.

GOVEF: point estimate of "Government Effectiveness," the third cluster of Kaufmann et al.'s governance indicators. Combines perceptions of the quality of public service provision, the quality of the bureaucracy, the competence of civil servants, the independence of the civil service from political pressures, and the credibility of the government's commitment to policies into a single grouping.

Re-scaled to range from 0 to 10 (lower values correspond to better outcomes). Sources: Kaufmann et al. (1999); ⟨http://www.worldbank.org/wbi/gac⟩.

GRAFT: point estimate of "Graft," the sixth cluster of Kaufmann et al.'s governance indicators. Captures the success of a society in developing an environment where fair and predictable rules form the basis for economic and social interactions, focusing particularly on perceptions of corruption. Re-scaled to range from 0 to 10 (lower values correspond to better outcome). Sources: Kaufmann et al. (1999); ⟨www.worldbank.org/wbi/gac⟩.

LAAM: regional country dummy, equal to 1 if a country is Latin American (including the Caribbean), 0 otherwise.

LEGOR_(UK, FR, GE, SO, ANDSC): dummy variable for the origin of the legal system, among five possible origins: Anglo-Saxon Common Law (uk), French Civil Law (fr), German Civil Law (ge), Socialist Law (so), and Scandinavian Law (sc). Source: La Porta et al. (1999).

LIST: number of lower-house legislators elected through a party list system. Sources: Quain (1999), Kurian (1998), Cox (1997), and national constitutional documents.

LPOP: natural log of the total population. Source: World Bank.

LYP: natural log of the per capita real GDP. Sources: Penn World Tables—mark 5.6 (PW) and The World Bank's World Development Indicators (WDI) ⟨www.worldbank.org⟩.

MAGN: inverse of district magnitude, defined as *DISTRICTS* over *SEATS*. Sources: Quain (1999) and Kurian (1998), Cox (1997), and national constitutional documents.

MAJ: indicator for electoral formula, equal to 1 if either majority or plurality rule is the only formula used in the elections of the lower house, 0 otherwise. Sources: Cox (1997), International Institute for Democracy and Electoral Assistance (1997), Quain (1999), and Kurian (1998), and national constitutional documents.

OECD: dummy variable for OECD member countries, equal to 1 if a country is an OECD member, 0 otherwise. Source: Persson and Tabellini (1999).

PDM: alternative indicator of district magnitude, measured as a weighted average, where the weight on each district magnitude in a country is the share of legislators running in districts of that size. Relative to the original variable in Seddon et al. (2001), this variable is divided by 100 so that it takes values comparable to those of *MAGN*.

PINDO: continuous measure of the ballot structure defined as $1 - (LIST/SEATS)CLIST$. Source: see *LIST* and *SEATS*.

PINDP: continuous measure of the ballot structure, defined as $1 - (LIST/SEATS)$. Source: see *LIST* and *SEATS*.

PPROPN: alternative indicator of ballot structure, measured as the share of legislators elected in national (secondary or tertiary) districts rather than subnational (primary) districts. Source: Seddon et al. (2001).

POLITY: score of democracy. Computed by subtracting the *AUTOC* score from the *DEMOC* score, and resulting in a unified measure ranging from +10 (strongly democratic) to −10 (strongly autocratic). Source: Polity IV Project ⟨http://www.cidcm.umd.edu/inscr/polity/index.htm⟩.

*PROT*80: percentage of the population belonging to the Protestant religion in 1980. Source: La Porta et al. (1999).

SEATS: number of seats in lower or single house of the latest legislature. Related to the number of districts in which primary elections are held. Source: International Institute for Democracy and Electoral Assistance (1997), Quain (1999), and Kurian (1998), and national constitutional documents.

TRADE: openness, defined as the sum of exports and imports of goods and services as a share of gross domestic product. Source: The World Bank's World Development Indicators CD-Rom 2000.

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