Electoral Rules and Corruption*

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Abstract

Is corruption systematically related to electoral rules? Some recent theories suggest a positive answer. But little is known about the data, despite several recent empirical studies of the determinants of corruption. We try to address this lacuna, by relating corruption to different features of the electoral system in a sample from the 1990s encompassing about 80 democracies. Our empirical results are based on traditional regression methods, but we also take into account non-random constitution selection and possible non-linearities. We exploit cross country variation as well as the time variation arising from a few 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. These two effects tend to be of the same size and offset each other, so that according to the data switching from strictly proprtional to strictly majoritarian elections has only a small negative effect on corruption.

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1. Introduction

Elected politicians have ample opportunities to abuse their political powers at the expenses of voters. Corruption, or rent extraction, is not only a problem in developing countries and recent democracies, but also in developed and mature democracies. 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, 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-nineties.

Are political rents systematically related to electoral rules? A few theoretical studies have addressed this important question. We describe the main ideas behind existing theoretical models in Section 2. The theory identifies two critical aspects of the electoral system: the ballot structure and the number of legislators elected in each district. With regard to the first aspect, in some electoral systems incumbents are individually accountable to the voters, while in others politicians are elected from party lists. The party list system weakens individual incentives for good behavior, because it creates a free rider problem and a more indirect chain of delegation, from voters to parties to politicians. With regard to the second aspect, district magnitude, smaller electoral districts raise higher barriers to entry. This matters for corruption because it affects the choice set of voters. With small districts, a smaller number of parties (or ideological types) are represented in the legislature. As a result, voters have less opportunities to oust corrupt politicians or parties. A few theories have also emphasized the distinction between strictly majoritarian and purely proportional electoral systems, suggesting that the election outcome is more sensitive to the incumbent's performance in the former than in the latter. Thus, the prediction here is that majoritarian elections are more effective in deterring political rents.

The main contribution of the paper is empirical. A number of 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 as far as we know, nobody has yet investigated how electoral rules correlate with corruption in a large cross section of countries. 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

corruption, all measuring perceptions of the degree of corruption by public officials. The perceptions are those of business people, risk analysts and the general public. We mainly exploit the cross country variation in the data. But since there are a few electoral reforms in the 1990s, we also report some panel estimates exploiting whatever time variation there is in the perceptions of corruption.

We use a variety of statistical methods. Section 4 focuses on the details of the electoral rule, the ballot structure and district magnitude, and reports traditional regressions estimates of cross country data. Section 5 compares majoritarian vs proportional electoral systems, also in cross country data. Here we also take into account possible selection bias in the choice of electoral rules, as well as possible non-linearities – such as heterogenous effects of electoral rules on corruption depending on the cultural or historical environment. Thus we employ a battery of statistical methods, such as instrumental variable estimation, the Heckman procedure, and non-parametric matching estimators. These methods have recently begun to make their way into the tool box of labor economists, but have not yet been applied to cross country comparisons in political economics. Finally, in section 6 we explore the consequences of electoral reforms, by estimating a panel of data on corruption and electoral institutions in the 1990s.

The evidence suggests 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. But consistent with the hypothesis on district magnitude and barriers to entry, countries that elect fewer candidates per district tend to have more corruption. Proportional electoral systems tend to combine these two opposite effects: they typically have large district magnitude as well as citizens voting for party lists. The opposite is true about majoritarian electoral systems, that vote over individuals but elect only one (or few) per district. Hence, not surprisingly, corruption does not differ much across these two crude electoral systems, though majoritarian elections seem to lead to marginally less corruption.

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

Myerson (1993). He assumes that parties (or equivalently, candidates) differ on 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 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 to be elected in equilibrium. But in single member districts, the equilibrium can be very different. Even if honest candidates run for office for all possible ideological types, 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 (for instance Adsera, A., C. Boix and M. Payne 2000). In Ferejohn's model, electoral defeat is less fearsome the higher is the probability that an ousted incumbent will return 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 another important dimension, namely in the

electoral formula translating vote shares into seat shares, and in the implied ballot structure. Plurality rule awards the seats in an M seat district to the individual candidates receiving the M highest vote shares. In proportional representation (PR) systems, voters instead choose among party lists and candidates are selected from these lists depending on the vote share of each party.

Persson and Tabellini (2000, Ch. 9), building on the career-concern model of Holmström (1982), suggest a model of rents and corruption which rests precisely on this difference in the ballot structure between plurality and PR. 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, instead, politicians' chances of re-election primarily depend on their ranking in the list, not on their 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 that political rents and corruption are higher the larger is the proportion of representatives elected on party lists, rather than on individually assigned seats.[1]

In the real world, these two features of electoral rules, district magnitude and ballot structure, are combined in a systematic pattern. Countries with "majoritarian electoral systems" typically combine single-member districts and plurality rule where voters select individual candidates. At the opposite extreme, many "proportional systems" indeed have large districts and voters choose among party lists (Israel e.g. has just one nation-wide district where all 120 representatives are elected and no threshold beyond the vote share for obtaining a single seat).[2] It is thus interesting to know which of these two effects - the district magnitude or the ballot structure- dominates: is corruption higher under purely "majoritarian" elections, or under strictly "proportional" elections? This crude comparison is also motivated by some theoretical work. Persson and Tabellini (1999) study

[1] Recently, Golden and Chang (2000) have suggested that the list system itself may induce more or less corruption. Electoral systems with open lists may induce corruption as they produce intra-party competition for office and thus give candidates from the same party stronger incentives to raise resources, including money from corruption. They find support for this proposition in an empirical study of the Italian Christian Democrats.

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

electoral competition in two stylized electoral systems: one with PR in a single nation-wide district, another with plurality rule in several single-member districts. Electoral competition is stiffer in the latter system, as the 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 in the majoritarian electoral system. This prediction is less precise than the previous ones, in that the argument does not distinguish well between district magnitude and the ballot structure. But it reflects a general and widespread idea: that under majoritarian elections the electoral outcome is more sensitive to the performance of the incumbent. Sometimes this property of majoritarian elections is attributed to the fact that this electoral rule is less likely to lead to coalition governments (and in coalition governments voters may find it more difficult to identify who is responsible for disappointing performance). Alternatively, the argument is sometimes made that, when the electoral contest is close to a tie, marginal swings in the votes can have drastic consequences for the electoral outcome under majoritarian elections but not under proportional elections. Whatever the detailed theoretical reason, it is worth investigating empirically whether political rents are higher under "majoritarian" or "proportional" elections, or whether instead the opposite effects of district magnitude and ballot structure tend to offset each other.

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

> *H1:* Countries with larger district magnitude and lower thresholds for representation have less corruption (the *barriers-to-entry effect*).
>
> *H2*: Countries with a larger share of representatives elected as individuals rather than as members of lists have less corruption (the *career-concern effect*).
>
> *H3*: Plurality rule in single-member districts is associated with less corruption than PR in large districts (the *electoral-competition effect*).

3. Data

This section discusses the key variables used in the empirical analysis and our basic specification. These data have been collected as part of a larger research program on economic policy and comparative politics. The Data Appendix of this

paper gives a precise description of the data sources, while Persson and Tabellini (2003) provide a more comprehensive discussion.

3.1. 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 ($GASTIL$) 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 in the period 1990-98 does not exceed 5. This is a generous definition of democracy, that includes countries such as Zimbabwe. This is done to maximize the number of countries. But we also report results for a narrower sample of better democracies, with an average $GASTIL$ score less than 3.5 in the period 1990-98. 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 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 200 years from the current date until the date of first becoming an independent democracy. In the empirical work that follows, we always control for both the quality of a democracy (as measured by $GASTIL$) and for its age (as measured by AGE).

We now turn to our measures of electoral rules. To test the barriers-to-entry effect (*H1*), we measure the the average size 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 slightly above 0 in an Israel-style system with a single national district where all legislators are elected. In some cases we also rely on an alternative measure of (inverse) district magnitude collected and discussed by Seddon et al (2001). The 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.

The career-concern effect (*H2*) instead focuses on the electoral formula, and in particular on the ballot structure. To capture the theoretical idea of individual

rather than collective accountability, we construct another continuous explanatory variable, *PIND*, defined as the proportion of legislators in the lower house elected with a vote on individuals (as opposed to party lists). Like our measure of district magnitude, $PIND$ ranges between 0 and 1, taking the value of 1 in a plurality system with single-member constituencies and a value of 0 in a pure PR system.[3] On this aspect too, 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 in sub-national (primary) districts. As the emphasis on collective vs. individual accountability may be largest for a politician running on a national party list, we sometimes use $PPROPN$ as an alternative to $PIND$.

All these indicators, *PIND, MAGN, PDM* and *PPROPN,* vary both across countries and over time (as discussed below, there are a few episodes 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 1990-98. In the panel data analysis we also exploit the time variation, and all variables are measured as yearly observations.

Finally, the electoral competition effect (*H3*) really combines the two dimensions measured by *PIND* and *MAGN*. To test it, we classify electoral systems into "majoritarian" vs "mixed" and "proportional" electoral rules, resulting in the binary (dummy) variables 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 in the most recent election exclusively by plurality rule are coded as $MAJ = 1$, whereas those relying on mixed or proportional rule are

[3]For a few countries, constructing this variable entails a number of difficult and arbitrary decisions. Party-lists can be of three types: closed list, preference (or open list) vote, and panachage. Closed lists do not allow the voters to express a preference for individual candidates. When a preference is allowed (as with open party lists), the party list is still the default option for the voter. The panachage is the least restrictive, by allowing voters to express preferences across parties (e.g., in Switzerland). As these alternatives are still quite distinct from the individual election under plurality rule, they are all treated as party lists. The PR system for the Dáil Eireann in Ireland is not based on party lists, relying instead on the Single Transferable Vote. The same electoral formula is used in Malta. In these cases, we set $PIND = 1$. Finland and Poland are strictly proportional systems (employing respectively D'Hondt and St. Laguë modified formulas) which allow voters to only choose among individuals, not parties. Candidates are ranked according to the total of personal votes they have polled, and parties are allocated seats on the basis of the cumulative vote for their candidates. In light of the free rider problem that remains in the ballot structure, we set $PIND = 0$ for these two countries.

coded $MAJ = 0$.

Five countries in our sample undertook electoral reforms in the last decade and changed their classification as coded by MAJ (they are Japan, New Zealand, Philippines, Fiji and 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 $PIND$ and $MAGN$ are more numerous (about 12 and about 15 respectively), but the classification of countries according to MAJ is affected only if the reform is sufficiently significant. We exploit this time variation in the panel analysis in section 6 below, dating the reform with the year in which the first election took place under the new electoral rule. But in the cross sectional analysis, we continue to treat the variable MAJ as binary (0 or 1) and if there was a reform we code its value *before* the reform, on the argument that it could take some time before electoral reform will impact on such slowly moving variables as the average perception of corruption.[4]

The nineties is an exceptional decade in terms of the frequency of electoral reform, however, at least when it comes to the basic features of electoral systems, and in previous periods electoral reform is much more rare. This stability reflects an inertia of political systems that is sometimes referred to as an "iron law" in the political-science literature. We exploit it in the empirical analysis by constructing three dummy variables that date the origin of the current constitution, inclusive of the electoral rule as coded by MAJ.[5] The three dummy variables refer to the periods before 1920, 1921-1950, and 1951-80 (and are called $CON20, CON2150$, $CON5180$ respectively). They take a value of 1 if the origin of the current constitution dates from one of these periods, and 0 otherwise. The period after 1981 is thus the default. This date of origin of the current constitution is indeed related to the current electoral rule. While slightly above one third of our sample has majoritarian elections, this proportion is much lower (one seventh) if the current

[4]Note that with this dating convention only nine countries in the sample have a mixed electoral system in the cross sectional analysis. All the others are either strictly majoritarian or strictly proportional. This makes it difficult to capture any difference between mixed systems and either strictly majoritarian and strictly proportional, since there are just too few observations. Nevertheless, the data suggest that mixed systems are more similar to proportional systems when it comes to the effect on corruption (more on this in section 5).

[5]We define the date of origin of the current constitution as the year in which the current value of MAJ was first acquired, or the current form of government was first acquired, given that the country was a democracy and an indpendent nation. If there was no constitutional or electoral reform since becoming a democracy, the origin of the current constitution coincides with the birth date of the democracy. See Persson and Tabellini (2003) for more details.

constitution originated in the 1921-50 period, but much higher (one half) if it originated in 1951-80. We do not have an explanantion for this specific pattern. But it suggests that the forces shaping constitutional rules – experience by other democracies, prevalent political and judicial doctrines, academic thinking, etc. – may have shifted systematically over time.

3.2. Corruption

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 measures of political rents in the empirical work to follow; three of these refer to corruption, the third to 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 a reliable information on the unlawful abuse of a political power, as well as a strong level of comparability across different countries.

The Corruption Perceptions Index (CPI) goes some way towards meeting these requirements.[6] Produced by Transparency International, a world-wide organization and a leader in anti-corruption research, this index measures the "perceptions of the degree of corruption as seen by business people, risk analysts and the general public". It is computed as the simple average of a number of different surveys assessing each country's performance in a given year. The yearly score thus includes information from many sources. For example, the 1998 score is based on 12 surveys from 7 different institutions, and the 1999 score on 14 surveys from 10 sources. 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 on average exceeds 0.8, 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 making up the CPI. For this reason, we typically weigh observations with the (inverse of the) standard deviation among the different surveys available for each country.

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

We use this variable only in the cross sectional analysis, taking the average of these yearly country scores 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 et al (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 in which fair and predictable rules form the basis for economic and social interactions; perceptions of corruption play a central role. The original surveys range from -2.5 to 2.5, with higher values corresponding to less corruption. We thus invert and re-scale this measure, which we also call *GRAFT*, to the same 0-10 scale as *CPI9500.* 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). The a priori differences notwithstanding, it is strongly correlated with *CPI9500* (the simple correlation coefficient is 0.97). Since it is available for more countries, this is our preferred indicator of corruption and we use it in most of the empirical analysis that follows. **Figure 1a** depicts the distribution of *GRAFT* in our sample.

Another cluster of governance indicators presented by Kaufman et al (1999) instead focuses on 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 re-coded 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, 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 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 1990-98 period, and we use it in the panel analysis with yearly data, to explore the effects of electoral reforms. We rescaled it so that it also varies between 0 and 10, with higher values denoting more corruption. This index has been used in a number of panel studies before, among which 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 cf 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 to formulate our basic specification.

To control for economic development, we consider the logarithm of GNP per capita, adjusted for purchasing power ($LOG(Y)$), and a dummy variable for OECD membership ($OECD$). We expect both of these variables to be associated with less corruption. Because earlier work has shown openness to trade and a decentralized political structure to be negatively correlated with corruption (see Ades and di Tella, 1999, and Fisman and Gatti, 1999, respectively), we include a measure of openness (*TRADE,* defined as the sum of exports and imports as a percentage of GDP) and an indicator for a federal political structure ($FEDERAL$) in the basic specification.

Based on the existing literature, we also include some other country characteristics. One of these is population size, measured in millions and expressed in natural logarithms ($LPOP$). Higher fractionalization of the population in the linguistic or ethnic dimension has been found in several recent studies to be a significant determinant of misgovernance (see e.g., Mauro, 1995 and La Porta et al, 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 frac-

tionalization. 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 two continuous measures of the population shares with a Protestant or a Catholic religious tradition as measured in the 1980s (*PROT80* and *CATHO80*) and an indicator variable for Confucian dominance (*CONFU*).[7]

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, has been investigated by the comprehensive study of La Porta *et al.* (1998), while Treisman (2000) has 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*), in eastern and southern Asia (*ASIAE*), in 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 to more than 250 years ago receives no weight at all. The result is three truncated but continuous measures of colonial origin adjusted for time elapsed since independence, and called: *COL_ UKA*, *COL_ ESPA* and *COL_ OTHA*.[8] Finally, to capture the influence of legal origin, we follow La Porta *et al.* (1998) and classify the origin of

[7] Oher studies have found media diffusion to be correlated with corruption. We have included in our regressors measures of TV per households internet connections, but they did not have additional explanatory power. Hence, we have not retained these variables in our final specification.

[8] Thus, for instance, the variable COL_UKA is defined as: COL_UK * (250 - years of independence)/250.

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*.

Finally, in section 6 we seek to explain the cross country variation in the electoral rule. For this purpose, besides some of the variables mentioned above, we also rely on three variables that Persson and Tabellini (2003) found to be correlated with the political constitution, namely distance from the equator ($LAT01$), and the percentage of the population speaking English ($ENGFRAC$) or another European language ($EURFRAC$) as a mother tongue.[9]

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 country average over the period 1900-98. In the panel analysis, of course, we only include the variables that vary over time and observations are yearly values.

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 partial correlations among the main variables. Some of these variables are highly correlated, as expected. Richer economies have less corruption, more education and better and older democracies. Corruption is also lower in better and older democracies, and where the population is better educated.

The two political variables of most interest, *PIND* and $MAGN$, are highly positively correlated with each other, as expected. Since they are predicted to have opposite effects on corruption, regression analyses should always include both of them to avoid an omitted variable bias. On the other hand, these two variables are not correlated with the other independent variables, suggesting that multicollinearity with the other controls is unlikely to be a problem. Note that these two variables also display little correlation with corruption.

Table 2 shows the means of the main variables, grouped by the electoral rule

[9] The source for these three variables is Hall and Jones (1999), who show that they contribute to explain growth promoting structural policies.

as coded in MAJ. Different electoral rules are certainly not randomly distributed in the sample. In particular, strictly majoritarian countries ($MAJ = 1$) tend to be less developed (lower values of LYP), less democratic (higher values of $GASTIL$), more fractionalized (higher values of $AVELF$), more open (higher values of $TRADE$), and many of them are former UK colonies, some of them located in Africa. As expected, the continuous measures of the electoral rule, $PIND$ and $MAGN$, also vary systematically with the electoral rule. This systematic pattern of the electoral rule in our sample is important, since it affects the inferences we can draw from our estimates. We return to this point again in sections 4 and 5.

Before turning to a systematic analysis of the effect of the electoral rule, we ask how much of the observed cross country variation in corruption can be explained by the social, economic and institutional variables other than the electoral rule. The answer is depicted in **Figure 2**, that displays the distribution of the residuals in $GRAFT$, once we control for the standard determinants discussed in the previous two subsections (the specification omits the measures of the electoral rule and the dummy variable for geographic location and colonial and legal history). Altogether, the basic economic and social variables explain over 80% of the variation in the data. The residuals range from - 1.96, for Chile, to + 1.99, for Belgium (the way we measure $GRAFT$, a negative residual means less corrupt than predicted). Other countries with large residuals include Cyprus and Uganda (both negative), and Venezuela, Paua N. Guinea and Bulgaria (all positive). Clearly, our basic controls eliminate the most striking differences across countries.

The precise specification and the estimated coefficients of the regression that generated these residuals are displayed at the bottom of **Figure 2**.[10] Corruption is lower in richer (LYP) and more open ($TRADE$) economies, in the OECD countries, in countries where citizens are better educated ($EDUGER$). Religion also has an important effect on corruption: Catholic ($CATHO80$) countries tend to be more corrupt, Protestant ($PROT80$) countries marginally less corrupt, while Confucian ($CONFU$) religion seemingly has no effect. These results generally conform to earlier studies and prior expectations (see, in particular, Treisman, 2000). Note however that, contrary to what appeared in the partial correlation coefficients depicted in **Table 1,** the quality and age of democracy (as measured by $GASTIL$ and AGE) do not have a statistically significant estimated coefficient. This could reflect multicollinearity of these variables with income. Other controls such as population size ($LPOP$), fractionalization ($AVELF$) and having a federal

[10] Estimation is by weighted least squares, the weights being the (inverse) standard deviation of $GRAFT$.

structure ($FEDERAL$) are also not statistically significant. Nevertheless, given the findings of previous empirical studies, and to minimize the risk of omitted variable bias, we always include all of these variables in our specification in the analysis that follows.

When we add the indicators for continental location and (discounted) colonial history, but still omitting the measures of electoral rules, the distribution of residuals shrinks further and the R^2 of the regression exceeds 85%. The estimated coefficients displayed at the bottom of **Figure 2** are not much affected, but countries located in Latin America tend to have more corruption, while being a former British Colony significantly reduces corruption.

We also experiment 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. Anglo-Saxon legal origin, not surprisingly, seems to pick up the same features as British colonial origin. For the rest, the results are not much affected. Since the specification with the colonial origin indicators are the least favorable to the results on electoral rules, in the remainder of this paper when controlling for the history of countries we always use colonial origin rather than legal origin. Replacing colonial origin with legal origin would produce a stronger effect of electoral rules on corruption, relative to what is reported below.

4. Career concerns and barriers to entry: cross country evidence

In this section we ask whether hypotheses H1 and H2 outlined in Section 2 are supported by the data on electoral rules. Thus, we focus on the ballot structure (voting over individuals vs parties) and on district magnitude. In the next section we turn to hypothesis H3 and compare strictly majoritarian elections against proportional and mixed systems. Throughout, we control for all the economic and social variables described in the previous section and listed at the bottom of **Figure 2**. We also always control for continental location and colonial origin as defined in the previous section, to minimize the risk of omitted variable bias.

The regression results are reported in **Table 3**. As we have most observations for *GRAFT,* we start with this measure of rents as our dependent variable. To help reduce the noise introduced by measurement error, the estimation method is always weighted least squares, with weights given by the (inverse) standard deviation of *GRAFT* (or of the other perceptions of corruption), except for column

3 where we estimate by OLS.

The data strongly support both the career-concerns and the barriers-to-entry effects. As shown in the first three columns of **Table 3**, inverse district magnitude and voting over individuals are statistically significant (individually and jointly) and with the expected sign: more individual voting (higher values of $PIND$) reduces corruption, but higher barriers to entry associated with smaller districts (higher values of $MAGN$) increases corruption. This result is robust to the estimation method (OLS rather than WLS, in column 3)), even though the standards error are now higher. They are also robust to the sample of better democracies (i.e. those with a $GASTIL$ score smaller than 3.5, in column 2). Moreover, the estimated coefficients of $PIND$ and $MAGN$ are large (both variables are defined so that they lie between 0 and 1) and their standardized beta coefficients are by far the largest of all regressors. For example, switching from a system where all legislators are elected on party lists ($PIND = 0$), to one where all are elected as individuals ($PIND = 1$) is estimated to reduce the perceptions of corruption by well over 20% (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. Omitting the dummy variables for continental location and colonial origin does not affect the estimated coefficient of $PIND$ much, though the estimated coefficient of $MAGN$.becomes a bit smaller and it remains statistically significant only at the 10% level. Finally, note that these variables are not only individually, but also jointly significant, except in the OLS estimation on the default sample. Given the high correlation between them and their opposite effect on corruption, this is a further sign that we are not just picking up a statistical artifact.

According to these estimates, a comprehensive electoral reform, going from a Dutch-style electoral system with party lists in a single national constituency to a UK-style system with first past the post in one-member districts (i.e., moving both *MAGN* and *PIND* from approximately 0 to 1), would have two counteracting effects on corruption, producing a net result close to zero. A better reform from the viewpoint of reducing corruption, would be to switch to plurality rule voting for individuals, but keeping districts with more than one member as in Chile (two-member districts and $MAGN = 0.5$) or Mauritius (three-member districts and $MAGN = 0.33$). Indeed, these countries, especially Chile, turn out to have very low corruption levels as compared to neighboring countries.

The 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 the remainder of **Table 3** we carry out further sensitivity analysis, with alternative measures for our dependent and independent variables. Columns 5 and 7 report on the same specification as in column 1, but with either *GOVEF* or *CPI9500* as the dependent variable. The results are very similar and even stronger when we measure corruption by $CPI9500$ (recall from the previous section that we have re-scaled all these measures to run on a scale from 0 to 10.).

Finally, columns 4, 6 and 8 of **Table 3** replace our own two measures of the electoral system with the alternatives from the data set constructed by Seddon et al (2001) and defined in the previous section. Recall that *PDM* is their measure of district size, defined so that higher values mean larger districts – not smaller as with our variable *MAGN.* Similarly*, PPROPN,* their measure of legislators elected at the national level is an inverted measure of individual accountability, and not a direct measure as our *PIND* variable. Thus, the expected sign of these two variables is the opposite relative to PIND and MAGN. As shown in **Table 3**, the main results hold up equally well with these alternative measures.

Overall, these simple regressions strongly suggest that the details of the electoral rules influence corruption, as predicted by hypotheses H1 and H2. Countries that predominantly vote over individuals tend to have less corruption than those that predominantly vote over parties, as predicted by the career-concerns model. And countries with smaller electoral districts tend to have more corruption, as predicted by the barriers-to-entry models.

5. Majoritarian vs proportional systems

In this section we ask how corruption would be affected by a comprehensive electoral reform, contrasting majoritarian vs proportional and mixed elections as coded by MAJ. This binary variable is defined on the basis of the electoral formula (plurality rule). But most countries with plurality rule also have single member district, hence the indicator MAJ really refers to a hypothetical reform that changes both the electoral formula and district magnitude. The results reported in this section thus shed light on hypothesis H3 mentioned in section 2. Throughout, we only exploit the cross country variation.

Unlike in the previous section, where the electoral rule was measured by two continuous variables, here we want to estimate the effect on corruption of a single binary variable, MAJ. This is an instance of an estimation problem extensively

studied in the program evaluation literature, and referred to as the estimation of the "treatment effect" - see for instance Heckman, Lalonde and Smith (1999). There are several estimation strategies, depending on the specific assumptions.

Our model can be thought of as consisting of two equations. One is a stochastic process that determines corruption in each country i (say as measured by $GRAFT$) as a function of the electoral rule and of a vector of other observable controls ($\mathbf{Z}$), such as per capita income or religious beliefs,

$$GRAFT_i = F(MAJ_i, \mathbf{Z}_i) + u_i \ , \tag{5.1}$$

where u is an unobserved error term. The second is a stochastic process that assigns an electoral rule to each country:

$$\begin{aligned} MAJ_i &= 1 \; if \;\; G(\mathbf{X}_i) + e_i \; > 0, \\ MAJ_i &= 0 \; if \;\; G(\mathbf{X}_i) + e_i \; \leq 0 \end{aligned} \tag{5.2}$$

where $\mathbf{X}$ are observables possibly also included in the vector $\mathbf{Z}$, such as colonial origin or geographic location, while e is an unobserved error term. Our goal is to estimate the effect of the indicator for majoritarian elections on the stochastic process for corruption.

The standard and simplest econometric approach is to assume: (i) that the function F is linear and with constant coefficients; (ii) that the model is recursive, namely that the error term e of the constitution selection equation (5.2) is uncorrelated with the error term u of the corruption relation (5.1). This second assumption is also known as "conditional independence", or "selection on observables". Under these two assumptions, linearity and conditional independence, we can estimate the effect of majoritarian elections on corruption by OLS or some equivalent simple linear regression. This is what we do in the next subsection. The remaining two subsections then relax conditional independence and linearity.

5.1. Simple regressions

Here we estimate the effect of majoritarian elections on corruption by weighted least squares. As in the previous section, the weights are the inverse of the standard deviation of the perceptions of corruption. Columns 1 and 2 of **Table 4** report the estimated coefficient of the dummy variable for majoritarian elections for different measures of the dependent variables, when we do *not* also include indicators for colonial origin and continental location. The other controls are the

same as in **Table 3** above. The estimated coefficients on majoritarian elections are negative and statistically significant, particularly when corruption is measured by $CPI9500$. Also including a binary indicator for mixed electoral system further strengthens this result: the estimated coefficient of MAJ increases in absolute value and in statistical significance, while the binary indicator for mixed systems is not statisticlly significant. Thus, it is really strictly majoritarian elections that seem to be associated with less corruption, while mixed and proportional system apparently can be lumped together.

Nevertheless, the estimated effect of majoritarian elections has a crucial fragility: in columns 3-5 of **Table 4** we also control for continental location and colonial origin, and the effect of majoritarian elections becomes much smaller and it loses significance. Majoritarian electoral rules are often found in former British colonies, and it is difficult to tell apart the influence of these two variables (when we control for continents or legal origin, the constitutional effect remains negative and statistically significant, so it is really colonial origin that makes a difference). The results are similar when we restrict the sample to better democracies (not shown).

5.2. Non-random constitution selection

Could it be that violations of the conditional independence assumption are responsible for the equivalence of proportional and majoritarian elections when it comes to the effects on corruption? This is the question addressed in this subsection.

There are two possible reasons for violation of conditional independence. One is reverse causation: for instance, corrupt politicians might deliberately choose an electoral system that tolerates corruption. But this is unlikely: as argued in section 3, electoral rules are changed infrequently and largely determined by history. This suggests a more serious reason for violations of conditional independence: historical variables determining current electoral rules could also influence current corruption. This is not a problem if all the common historical determinants of corruption and electoral rules are included in the regression (and if the model is linear). This is why the previous section checked the robustness to a non-parsimonious specification of corruption, that also includes variables such as colonial history or geographic location. But how do we know that we have included enough common determinants of corruption and of the electoral rule to really satisfy the conditional independence assumption?

When the variable of interest is a binary variable, such as MAJ, a standard way to cope with this problem is to estimate the correlation coefficient between

the error terms e and u of (5.1) and (5.2), and to correct for the bias in the OLS estimates - the so called Heckman correction. Identification is made possible by a strong functional form assumption: (5.1) and (5.2) are linear, and the error terms u and e are jointly normal.

To implement this estimation procedure, we specify the constitution selection equation (5.2) so that the vector $\mathbf{X}$ consists of the age of democracy (AGE), UK colonial origin, the dummy variable for Latin America, distance from the equator ($LATO1$), and the percentage of the population speaking English ($ENGFRAC$) or another European language ($EURFRAC$) as a mother tongue.[11] This set of variables has considerable explanatory power: the pseudo R^2 of the probit equation for majoritarian electoral systems is 45%. UK colonial origin and a higher fraction of English speakers increase the probability of having majoritarian electoral systems, being in Latin America and close to the equator reduce it. The equation for corruption (5.1) is specified as in the previous section, except that to facilitate convergence of the estimation algorithm we only include UK colonial origin and the dummy variable for Latin America, omitting the other colonial and continental indicators. Estimation is by maximum likelihood, jointly for (5.1) and (5.2).

The results are displayed in **Table 4**, column 6. The estimated correlation coefficient between the error terms of these two equations (rho) is -0.54. Even though it is imprecisely estimated, a negative correlation coefficient imples that the OLS estimates for majoritarian elections reported in the previous columns of **Table 4** are biased downwards. Correcting this bias with the Heckman procedure produces a positive estimated coefficient on MAJ, though not significantly different from zero.

As a further check, we also rely on instrumental variables. The dummy variables $CON21, CON2150, CON5180$ defined in section 3 classify the origin of the current constitution. As discussed in Persson and Tabellini (2003), they have some (weak) predictive power for the current electoral rule as coded by MAJ. There is also no reason to expect these dummy variables to be correlated with the unobserved determinants of corruption, once we control for the AGE of a democracy and all the other regressors. Hence, they can be used as instruments for the electoral rule. We thus estimate the corruption equation by 2SLS. In the first stage we impose the linear probability model and regress the dummy variable for majoritarian elections (MAJ) on the full set of controls for corruption, the

[11] In a more extensive analysis, Persson and Tabellini (2003) found these variables to be good predictors of current constitutional features, including the electoral rule.

dummy variables for UK colonial origin and for Latin America, plus the three dummy variables for constitutional origin. The R^2 of this regression exceeds 60%. In the second stage, we estimate the corruption equation, with the previous set of controls (including the dummy variables for UK colonial origin and for Latin America), letting majoritarian elections (MAJ) be endogenous. The results are displayed in column 7 of **Table 4**. Again, the dummy variable for majoritarian elections has a positive estimated coefficient on corruption, not significantly different from zero. The over-identifing restrictions on the instruments cannot be rejected.

Summarizing, when we seek to relax recursivity, both the Heckman procedure and instrumental variable estimation confirm that there is no effect of majoritarian elections on corruption. Neither procedure is entirely reliable. The Heckman correction hinges on a strong functional form assumption and the correlation coefficient rho is imprecisely estimated; while the instruments for the electoral rule are weak, since most of the explanatory power in the first stage regression is due to other variables. Hence, if conditional independence is indeed violated, we cannot be entirely confident that the estimates reported in the last two columns of **Table 4** are unbiased. Nevertheless, these two estimation procedures yield very similar results, despite very different identification methods. This lends support to the inference that majoritarian and proportional elections are really equivalent when it comes to the effects on corruption.

5.3. Relaxing linearity: matching with the propensity score

All estimation methods reported in the previous two subsections impose the assumption that the function F in (5.1) is linear, and that the electoral rule only affects the intercept of this function. Usually, linearity is taken to be a convenient local approximation of a more general model. But here we are interested in the comparison of two quite different groups of countries: those with majoritarian elections ($MAJ = 1$) vs the others. As shown in **Table 2**, many variables differ considerably across these two groups If the effect of the electoral rule on corruption really interacts with other variables (for instance, it is stronger in better democracies, or in more economically advanced countries), then the local approximation is not tenable and the linear estimates reported in the previous two sub-sections are biased. In this subsection we relax linearity and estimate the effect of majoritarian elections on corruption with non parametric matching

methods based on the propensity score.[12]

The gist of these non-parametric estimators is that they give more weight to comparisons of similar countries, where the non-linearities are unlikely to be relevant. Countries are ranked on the basis of their "propensity score". In our context, the propensity score can be defined on the basis of (5.2), as the conditional probability that country i has majoritarian elections ($MAJ = 1$), given the vector of observable constitutional determinants $\mathbf{X}$. Some countries in this ranking have majoritarian elections, others don't. Countries with similar propensity scores but different electoral rules are then matched and their performance (here corruption) is compared. Comparisons among more similar countries are given more weight. There are many possible ways of doing that, and each method of comparison corresponds to a specific matching estimators.

A critical step in this procedure is the estimation of the propensity score. These non parametric methods are based on two assumptions (cf. Rosenbaum and Rubin 1983). The first one is that conditioning on the vector $\mathbf{X}$ is sufficient to achieve conditional independence between (5.1) and (5.2): once we have conditioned upon $\mathbf{X}$, the unobserved determinants of the constitution and corruption are uncorrelated. Thus, in the specification of the probit or logit model for constitution selection, we should not omit any variables really driving corruption. This speaks in favor of an inclusive probit/logit specification (note that this wa not a concern in the specification of the probit model used for the Heckman correction in **Table 4**).

The second assumption is that the propensity score is strictly between 0 and 1 (the so called *common support* condition). To satisfy this assumption, we must preserve some randomness in constitution selection. If we explain constitutional choice "too well", we shrink the region of overlapping propensity scores between countries having different electoral rules: for some majoritarian countries the estimated propensity score can be very close to 1, for some mixed or proportional countries it can be very close to 0. For these extreme observations, matching becomes difficult because there are no terms of comparisons (i.e., no countries with the opposite electoral rule). Preserving enough randomness in the propensity

[12]These methods were introduced into economics as tools for evaluating labor market and education programs (see for instance Dehejia and Wahba, 1999, and Heckman, Ichimura and Todd, 1997). More recently, they have been applied to cross country-comparisons in a variety of studies -see Persson and Tabellini (2003) for additional references. A useful and accessible survey, which puts the methodology in context, can be found in Blundell and Costa Dias (2000). More general discussions about matching vs. other evaluation methods can be found in Angrist and Kreuger (1999), Heckman, Lalonde and Smith (1999), and Ichino (2001).

scores thus speaks for a parsimonious logit or probit specification.

To satisfy these opposite concerns, we specify the logit equation for constitution selection in two different ways (estimation by probit yields similar results). One specification includes the following variables: per capita income (LYP), protestant religious beliefs ($PROT80$), quality of democracy ($GASTIL$), fractionalization ($AVELF$), plus the dummy variables for UK colonial origin and Latin America. The other specification replaces the dummy variables for UK colonial origin and Latin America with the variables used in the Heckman correction discussed in the previous subsection, namely distance from the equator ($LAT01$), and the fractions of the population speaking English ($ENGFRAC$) and another European language ($EURFRAC$) as a mother tongue. Per capita income, religious beliefs and quality of democracy are included because they are the most important determinants of corruption. Fractionalization does not display much correlation with corruption, but there are strong prior reasons to belive that it could influence both corruption and the choice of electoral rules. The other variables are strongly correlated with the electoral rule, but they could also influence corruption.

Table 5 ranks the countries in our sample by their estimated propensity scores (for the first of the two logit specifications mentioned above) and reports the actual value of the electoral-rule indicator (MAJ).[13] Countries with *low* estimated probabilities of majoritarian elections are mostly located in Europe and Latin America, while countries with higher scores are more often previous British colonies and, as we move down the ranking, rather poor countries not located in Latin America. Not surprisingly, this matches the actual distribution of majoritarian elections in the real world.

All estimated propensity scores lie strictly in between 0 and 1. Nevertheless, to be on the safe side with regard to the common support assumption, we define the *estimated* common support as the interval between the *minimum* estimated propensity score among the $MAJ = 1$ countries, and the *maximum* estimated propensity score among the $MAJ = 0$ countries. All observations outside this estimated common support are discarded as non-comparable. Thus, with reference to **Table 5** we discard the first three mixed or proportional countries ($MAJ = 0$), which all have a score lower than the US, the majoritarian country with the lowest estimated probability of being majoritarian. In the same way, we discard the last nine majoritarian countries ($MAJ = 1$), which all have a higher score

[13] To maximize observations, the propensity score is estimated on a slightly larger sample of 84 democracies, that also includes a few countries on which measures of corruption are not available. These extra democracies, not used in the matching, are not listed in **Table 5**.

than Sri Lanka, the mixed or proportional country with the highest estimated probability of being majoritarian. This procedure reduces our sample size, but it has the advantage of excluding outliers. It reinforces the idea that matching estimation relies on inference from local comparisons among similar countries.

A natural question is whether the countries that are close in the ranking are indeed more similar when it comes to the distribution of observable covariates, irrespective of their actual electoral rule. To check this, we group the countries inside the estimated common support in three strata, corresponding to the three percentiles (between 0-33%, 34-66%, 67-100%). We then test whether the means of all the controls used in the simple regressions of **Tables 3** and **4** are equal in the groups of majoritarian ($MAJ = 1$) and mixed or proportional ($MAJ = 0$) countries in each of these strata. In the first stratum we reject (at 5% level) the nul of equal means for only two variables; in the second stratum , the nul is rejected for five variables; in the third stratum we can never reject the nul. Given the differences in means reported in Table 2, and how parsimonious is how our probit specification, the strata define groups of countries that are much more similar than in the whole sample, though some differences remain.

Now that we have a metric with wich to rank countries, we compare the corruption of similar countries under different electoral rules. We rely on three alternative matching estimators. All three estimate the effect of the electoral rule on corruption for a country drawn at random from our sample. [14]The *stratification estimator* computes the difference in average corruption of majoritarian and non-majoritarian countries inside each stratum (the strata are those mentioned in the previous paragraph). Each stratum is then weighted by the number of countries it contains to produce an overall estimated difference in corruption. The *nearest neighbor estimator* only compares countries that are closest in the ranking of **Table 5**. For each country with $MAJ = 1$, we find its closest twin with the opposite electoral rule and compute the difference in corruption. Thus, with reference to **Table 5**, Switzerland is the best match for the US. The more distant countries with $MAJ = 0$ are neglected, while closest countries can be used several times if they happen to be the closest match for several $MAJ = 1$ countries (in **Table 5**, El Salvador is the best match for both Canada and Bangladesh). Then the procedure is reversed, and for each country with $MAJ = 0$ we find

[14]That is, we estimate what is also known as the *average* treatment effect. This corresponds to what was estimated in the two previuos subsections. Sometimes the literature on program evaluation is interested in other effects, such as the effect of treatment on the treated. See Heckman et al. (1999) for alternative estimators and definitions.

its closest twin with the opposite electoral rule, and compute the difference in corruption. Finally, the *kernel-based estimator* combines the logic of the previous two estimators. Each $MAJ = 1$ country is a matched against a weighted average of all $MAJ = 0$ countries within a certain propensity-score distance (here 0.25), with weights declining in that distance. And conversely when we match the $MAJ = 0$ countries.[15]

Table 6 reports the estimated effects of the electoral rule on corruption, for these alternative matching estimators, for alternative specifications of the logit equation on the propensity score, and for alternative measures of corruption. We give more emphasis to the kernel based estimator which is a bit more stable. All these non-parametric estimates confirm the findings of the simple regressions reported in **Table 4:** the estimated effect of majoritarian elections on corruption is always negative, even though it is rather small in absolute value and not statistically significant except with the stratification estimator (column 2). As already discussed, however, the idea behind our non-parametric estimators is precisely to trade off reduced bias due to specification error against less efficiency. High standard errors thus come as no surprise, particularly in such a small sample of countries.

5.4. Summary

All in all, cross country data suggest that a reform from mixed or proportional to strictly majoritarian elections could have a small negative effect on corruption. But this effect is often not statistically significant and it is not robust. Countries ruled by majoritarian vs proportional elections seem to have about the same level of corruption. The main reason why it is difficult to draw robust inferences from cross country comparisons is that electoral rules are not randomly distributed across countries. When we take this into account, either by allowing for a direct effect of colonial origin on corruption, or by relaxing conditional independence, the effect of majoritarian elections on corruption vanishes or becomes statistically insignificant. Relaxing linearity and allowing for interactions between the electoral rule and other determinants of corruption strengthens the estimated negative effect of majoritarian elections on corruption, but generally not enough to achieve statistical significance.

[15] These estimators are quite common in the applied microeconometric literature. See Ichino (2001) or Persson and Tabellini (2003) for more details.

6. Electoral reforms and panel estimates

As noted in section 3, five countries had significant electoral reforms during the 1990s changing the value taken by MAJ. A few more countries switched from mixed to proportional or viceversa. Over a dozen countries reformed detailed aspects of their electoral rules, resulting in significant changes in our continous variables $PIND$ and $MAGN$. In this section we ask whether these reforms had an impact on corruption, as measured by the time varying index $ICRG$. Exploiting this time variation in the data provides additional information, despite the relatively small number of reforms, because violation of conditional independence due to omitted determinants of both corruption and of the constitution is unlikely to be a problem in this panel. On the other hand, we have to assume that the political events that led to electoral reforms were not also accompanied by significant changes in other unobserved determinants of corruption.

We focus both on the detailed aspects of electoral rules, as measured by $PIND$ and $MAGN$, and on the crude classification coded by the binary variable MAJ. In both cases, we estimate three specifications. To control for time invariant determinants of corruption, such as colonial origin or location, we always include country fixed effects. In the first specification, we only control for the electoral rule besides the country fixed effects; thus, observations are in deviations from country means, and we essentially ask whether the one-time change in the electoral rule has significantly increased or decreased corruption in the countries that underwent reform. In the second specification we add year fixed effects, allowing for common events that influence corruption in all countries. Finally, in the third specification we remove the year fixed effects and replace them with those determinants of corruption that exhibit some time variation, (namely per capita income (LYP), quality of democracy ($GASTIL$), openness ($TRADE$) and population ($LPOP$)).[16]

The results are reported in **Table 7**. The first three columns refer to the continuous measures of electoral rules, $PIND$ and $MAGN$, under each of the three specifications. The last three columns refer to the crude distinction between majoritarian elections vs mixed and proportional systems, as coded by MAJ.

Consider columns 1-3 first. The results are remarkably similar to those reported in the cross country regressions of **Table 3**. Increasing the proportion of legislators individually accountable (increasing $PIND$) reduces corruption, while

[16] Education also varies over time, but time variation is missing for many countries, hence we do not include it among our controls.

reducing district magnitude (increasing $MAGN$) increases corruption. The estimated coefficients are statistically significant and stable across specifications; they are also large, though not as much as in the cross country estimates (where they were closer to 2) - recall that the measure of corruption is different, though on a same scale. These estimates lend further support to both $H1$ and $H2$: both the career-concerns effect and the barriers-to-entry effect are supported also by the time series variation in the data.

Next, consider columns 4-6. An electoral reform from mixed or proportional to majoritarian elections reduces corruption by 0.65-0.7 points (recall that corruption ranges from 0 to 10). Thus, the effect is small, though marginally significant at about the 10% level. Again, the results are remarkably similar to those reported in the simple cross country regressions of **Table 4,** columns 1-5, where the estimated coefficients of MAJ were always negative but smaller than 1 in absolute value (and often close to -0.5). Note that here the non random selection of the constitution due to omitted historical variables is unlikely to bias our estimates, since we include country fixed effects. Hence, collinearity of the electoral rule with former UK colonies is not a problem, and the effect of majoritarian elections appears more strongly compared to columns 3-5 of **Table 4** (that also controlled for UK colonial origin). Overall, the time variation in the data lends some support to hypothesis $H3$, the so called electoral competition effect of a reform from proportional to majoritarian elections.

The effect of the binary variable MAJ on corruption in these panel estimates is only due to the five countries that changed from mixed or proportional to majoritarian elections. To increase the number of countries, we also investigated whether reforms from mixed to proportional or viceversa is also associated with changes in corruption. The answer is negative; according to the time series data, mixed and proportional electoral rules can be lumped together; again, recall that the same result was obtained in the cross country comparisons.

7. Concluding remarks

This paper has presented new results on how electoral rules affect political corruption. The main lesson of the data is that the details of electoral rules, such as the ballot structure or district magnitude, seem to be more important than the crude form of the electoral system. Switching from purely proportional to strictly majoritarian elections could lead to a marginal reduction in corruption. But the overall effect is small and imprecisely estimated. The reason is that such a reform

changes several features of electoral systems at once, with offsetting effects on political corruption.

These empirical results are consistent with the theories that focus on detailed aspects of electoral rules. As predicted by models that emphasize the career concerns of individual politicians, voting on party lists (rather than on individuals) reduces the effectiveness with which voters can exploit the ballot to deter corruption. And as predicted by models that emphasize bareers to entry of new candidates, in districts where only a few candidates are elected it is more difficult to oust dishonest incumbents. Instead, the electoral competition model that predicts that a system with single member districts and plurality rule leads to stiffer political competition and less corruption, relative to a purely proportional system with a unique national district, receives only mixed support from the data.

The estimated effects of these details of electoral rules are non-trivial. For instance, our estimates suggest that Chile's low corruption outcome – a $CPI9500$ value of 3.42 compared to values well over 5 for most other South American democracies – might to a considerable degree be attributed to its electoral rules, combining dual-majority rule ($PIND = 1$) in two-member districts ($MAGN = 0.5$). Similarly, 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 75% of the legislature – instead appears a step in the right direction.

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DATA APPENDIX

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

AGE : age of democracy. Defined as: AGE= (2000-DEM_AGE)/200. Varies 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 1 if a country is East Asian, 0 otherwise.

$AUTOC$: institutionalized autocracy. The indicator of autocracy is 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

$AVELF$: index of ethnolinguistic fractionalization. It approximates the level of lack of ethnic and linguistic cohesion within a country. It ranges from 0 (homogeneous) to 1 (strongly fractionalized) and averages 5 different indexes. Source: La Porta et al. (1998). For Central and Eastern Europe countries computations follow Mauro (1995) with data from Quain (1999).

$CATHO80$: percentage of the population belonging to the Roman Catholic religion in 1980. Source: La Porta et al. (1998).

$COL\_ESPA : COL\_ESPA = COL\_ES * (250 - T\_INDEP)/250$. It represents the combined effect between COL_ES, which describe if a country was a colony of Spain or not, and T_INDEP, years of independence. T_INDEP takes into account for colonial exposition ranging from 0 to 250 where the latter value is for non-colonies countries. Source: Wacziarg (1996).

$COL\_OTHA : COL\_OTHA = COL\_OTH * (250 - T\_INDEP)/250$. It represents the combined effect between COL_OTH, which describe if a country was neither a colony of Spain nor one of UK, and T_INDEP, years of independence. T_INDEP takes into account for colonial exposition ranging from 0 to 250 where the latter value is for non-colonies countries. Source: Wacziarg (1996).

$COL\_UKA : COL\_UKA = COL\_UK * (250 - T\_INDEP)/250$. It represents the combined effect between COL_UK, which describe if a country was a colony of UK or not, and T_INDEP, years of independence. T_INDEP takes into account for colonial exposition ranging from 0 to 250 where the latter value is for non-colonies countries. Source: Wacziarg (1996).

$CONFU$: religious tradition dummy. I equals 1 if the majority of population is Confucian/Buddhist/Zen, 0 otherwise. Source: Wacziarg (1996), CIA-The World

Factbook 2000.

$CPI9500$: corruption perception index. It is the 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/~uwvw).

DEM_AGE : year of birth of the democracy. It 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. This variable does not take in consideration foreign occupation during WWII as an interruption of democracy. Source: See $POLITY$.

$DEMOC$: institutionalized democracy. This index is 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).

$EDUGER$: 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. It is imputed dividing the number of pupils (or students) enrolled in a given level of education regardless of age by the population of the age-group which officially corresponds to the given level of education, and multiplying the result by 100. Source: UNESCO - Education Indicator - Category Partecipation. Available on www.unesco.org

$ENGFRAC$: This variable measures the fraction of the population speaking English. Source: Hall and Jones (1999).

$EURFRAC$: This variable measures the fraction of the population speaking one of the major languages of Western Europe: English, French, German, Portoguese, or Spanish. Source: Hall and Jones (1999).

$FEDERAL$: federalism dummy. Source: Boix 2000.

$GASTIL$: average of gastil index for civil liberties and political rights. It is measured on one-to-seven scale with one representing the highest degree of freedom and seven the lowest. 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. Its purpose is to combine 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. It ranges from around 0 to around 10 (lower values correspond to better outcome). Sources: Kaufmann et al. (1999a.), available at http://www.worldbank.org/wbi/gac.

$GRAFT$: point estimate of "Graft", the sixth cluster of Kaufmann et al.'s governance indicators. Together with "law", it captures the success of a society in developing an environment in which fair and predictable rules form the basis for economic and social interactions. It particularly focuses on perceptions of corruption. It ranges from around 0 to around 10 (lower values correspond to better outcome). Sources: Kaufmann et al. (1999a.), available at www.worldbank.org/wbi/gac.

$LAAM$: regional country dummy. It equals 1 if a country is Latin American, 0 otherwise.

$LAT01$: rescaled variable for latitude. It is the absolute value of latitude divided by 90, so that it returns a number between 0 and 1. Source: see $LATITUDE$.

$LATITUDE$: It measures the distance, in degrees, from the equator and ranges between -90° to 90°. Source: Hall and Jones (1999).

$LEGOR\_(UK, FR, GE, SO, ANDSC)$: dummy variable for the origin of the legal system. Consequently, it also represent the original electoral law for each Country. Five possible origins are considered: 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. (1998).

$LIST$: number of lower-house legislators elected through party list systems. Sources: Quain (1999) and Kurian (1998).

$LPOP$: 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); Easterly's series on www.worldbank.org; The World Bank's World Development Indicators (WDI).

$MAGN$:inverse of district magnitude, defined as $DISTRICTS$ (i.e., the number of primary as well as secondary and tertiary if applicable) over $SEATS$ (i.e.the number of seats in the lower house). Sources: Quain (1999) and Kurian (1998).

MAJ: dummy variable for electoral systems. It equals 1 in presence of either a majority or a plurality rule, 0 otherwise. Only legislative elections are considered. Sources: Cox (1997), International Institute for Democracy and Electoral Assistance (1997), Quain (1999), and Kurian (1998).

$OECD$: Dummy variable for OECD member countries, taking the value 1 if a Country is an OECD member, 0 otherwise. Source: Persson and Tabellini (1998).

PDM : district magnitude (i.e., as seats over districts); 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. 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 $MAGN$.

$PIND = 1 - \frac{LIST}{SEATS}$. Source: see $LIST$ and $SEATS$

$PPROPN$: the share of legislators elected in national (secondary or tertiary) districts rather than sub-national (primary) districts. Source: Seddon et al. (2001)

$POLITY$: The $POLITY$ score is computed by subtracting the $AUTOC$ score from the $DEMOC$ score; the resulting unified polity scale ranges from +10 (strongly democratic) to -10 (strongly autocratic). Source: Polity IV Project (http://www.cidcm.umd.edu/inscr/polity/index.htm).

$PROT80$: percentage of the population of each country belonging to the Protestant religion in 1980. Source: La Porta et al. (1998).

$SEATS$: number of seats in lower or single chamber for the latest legislature of each Country. It is also 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).

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

Figure 1: Histogram of GRAFT

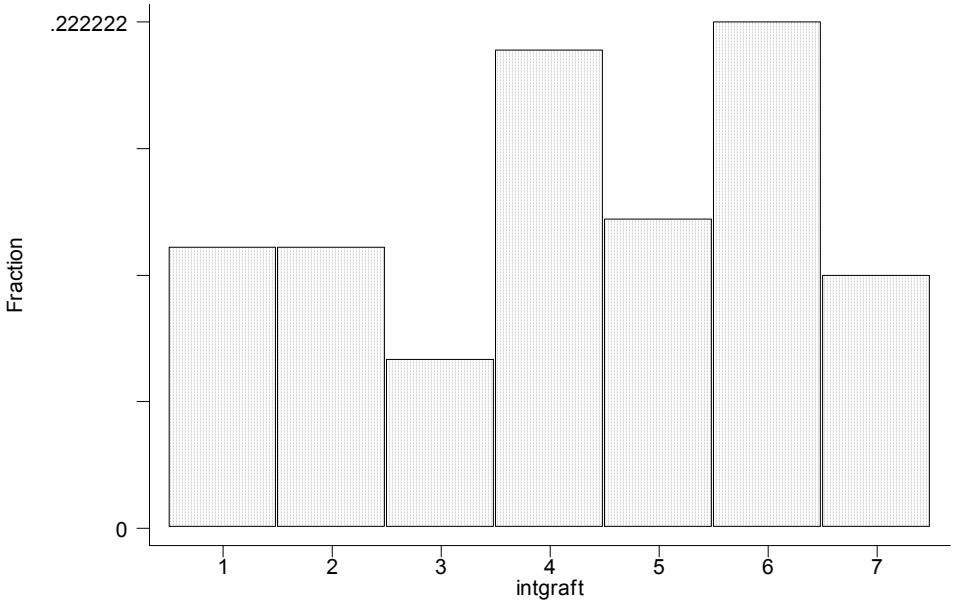
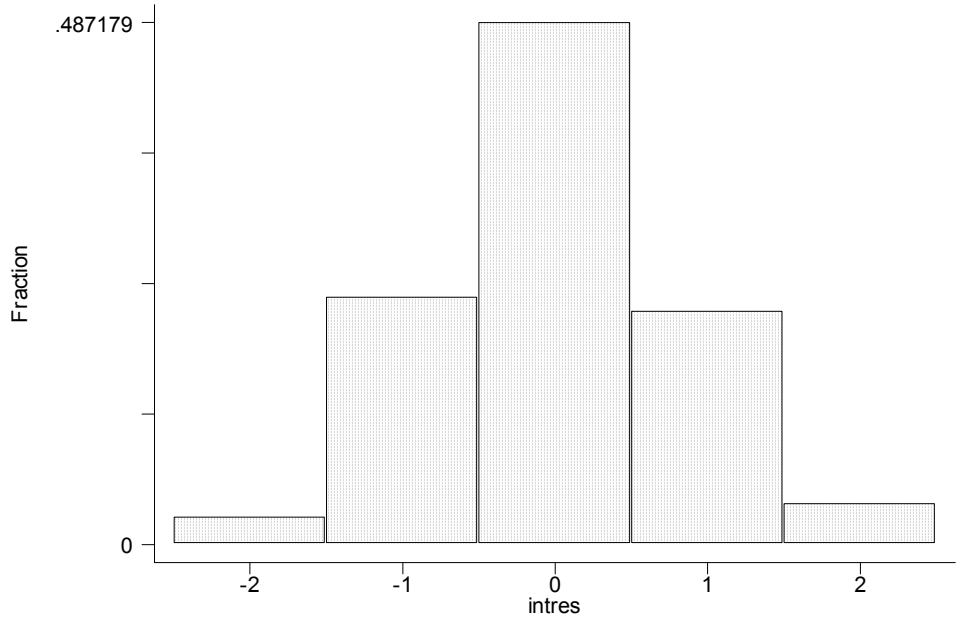


Figure 2: Histogram of residuals of GRAFT



Residuals generated from the following regression (S.E. in parenthesis):

GRAFT = 12.57(2.15) – 0.84(0.25)*LYP* + 0.05(0.11)*LPOP* -1.33(0.39)*OECD* –
– 0.01(0.009)*EDUGER* – 0.006(0.003)*TRADE* – 0.33(0.52)*AVELF* – 0.006(0.005)*PROT80* +
+ 0.007(0.003)*CATHO80* + 0.49(0.47)*CONFU* + 0.07(0.31)*FEDERAL* – 0.36(0.62)*AGE* +
+ 0.10(0.17)*GASTIL*

Adj R-squared: 0.8; Obs: 78; estimation by weighted LS.

Table 1
Partial correlations

	graft	maj	pind	magn	lyp	lpop	gastil	age	avelf	eduger	trade	catho80
maj	0,052											
	0,646											
pind	0,076	0,934										
	0,506	*0,000*										
magn	0,140	0,870	0,929									
	0,217	*0,000*	*0,000*									
lyp	-0,820	-0,220	-0,202	-0,264								
	0,000	*0,043*	*0,066*	*0,015*								
lpop	0,208	0,032	0,092	0,081	-0,081							
	0,063	*0,775*	*0,408*	*0,463*	*0,459*							
gastil	0,703	0,189	0,243	0,222	-0,744	0,296						
	0,000	*0,083*	*0,026*	*0,043*	*0,000*	*0,006*						
age	-0,637	0,026	-0,049	-0,046	0,611	0,057	-0,545					
	0,000	*0,817*	*0,658*	*0,680*	*0,000*	*0,605*	*0,000*					
avelf	0,329	0,284	0,299	0,265	-0,555	0,009	0,524	-0,219				
	0,003	*0,009*	*0,006*	*0,015*	*0,000*	*0,937*	*0,000*	*0,044*				
eduger	-0,642	-0,298	-0,313	-0,298	0,696	-0,062	-0,655	0,428	-0,440			
	0,000	*0,007*	*0,004*	*0,007*	*0,000*	*0,582*	*0,000*	*0,000*	*0,000*			
trade	-0,197	0,084	0,064	-0,043	0,144	-0,580	-0,003	-0,026	0,095	0,046		
	0,078	*0,443*	*0,561*	*0,701*	*0,187*	*0,000*	*0,976*	*0,811*	*0,388*	*0,683*		
catho80	0,166	-0,354	-0,385	-0,319	0,051	0,015	-0,145	-0,022	-0,216	0,097	-0,091	
	0,138	*0,001*	*0,000*	*0,003*	*0,642*	*0,893*	*0,187*	*0,844*	*0,047*	*0,388*	*0,407*	
prot80	-0,458	-0,008	-0,024	0,024	0,265	-0,315	-0,356	0,309	-0,035	0,312	0,037	-0,378
	0,000	*0,943*	*0,830*	*0,832*	*0,015*	*0,003*	*0,001*	*0,004*	*0,751*	*0,004*	*0,739*	*0,000*

Notes: p-values for pairwise correlation below in in parentheses

Table 2

Rent extraction and institutions

Sample means for plurality and non-plurality systems

emaj	graft	pind	magn	lyp	lpop	gastil	avelf	eduger	trade	col_uk	col_esp	laam	africa	age	prot80
1	4,2997	0,9876	0,8951	8,1396	2,3027	2,7351	0,3792	81,8677	83,7425	0,6667	0,0312	0,2121	0,2424	0,2117	17,2121
0	4,0965	0,0972	0,1947	8,5744	2,1891	2,2604	0,2272	92,6608	75,6077	0,1538	0,0749	0,3077	0,0577	0,2054	17,6212
p-value	*0,64*	*0.00*	*0.00*	*0,06*	*0,79*	*0.10*	*0,01*	*0,49*	*0,01*	*0.00*	*0,15*	*0,33*	*0,03*	*0,82*	*0,93*

Notes: p-value denotes the p-value for a means test between the groups emaj = 0 and emaj = 1, the null being equal means, allowing for unequal variances in the two subsamples.

Table 3

Political Rents and Details of Electoral Rules

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	graft	graft	graft	graft	govef	govef	cpi9500	cpi9500
pind	-1,79	-2,29	-1,71		-1,82		-2,72	
	(0.74)**	(0.84)***	(1,10)		(0.84)**		(1.00)***	
magn	2,3	2,9	2,26		1,91		3,2	
	(0.85)***	(0.95)***	(1.28)*		(0.97)*		(1.12)***	
ppropn				1,24		0,98		1,78
				(0.44)***		(0.52)*		(0.61)***
pdm				-1,1		-1		-1,58
				(0.44)**		(0.52)*		(0.60)**
pvalue F-test for joint signif. political variables	0,03	0,01	0,18	0,01	0,11	0.10	0,02	0,01
Continents & Colonies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Default	Gastil <3.5	Default	Default	Default	Default	Default	Default
Estimation	WLS	WLS	OLS	WLS	WLS	WLS	WLS	WLS
Observations	78	59	78	72	78	72	68	62
Adjusted R-squared	0,83	0,86	0,82	0,87	0,75	0,79	0,88	0.90

Notes: WLS weights are 1/std(dependent variable), Robust standard errors in parentheses for OLS regression. Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include the following controls: gastil, age, lyp, lpop, eduger, trade, oecd, federal, avelf, prot80, catho80, confu.

Table 4
Political Rents and Majoritarian Elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable	*GRAFT*	*CPI9500*	*GRAFT*	*CPI9500*	*GOVEF*	*GRAFT*	*GRAFT*
maj	-0.46	-0.92	-0.11	-0.53	-0.31	0.35	0.52
	(0.25)*	(0.31)***	(0.31)	(0.41)	(0.34)	(0.58)	(0.60)
rho						-0.54	
						(0.47)	
Chi-2 test for over-id							4.98
Continents and Colonies	No	No	Yes	Yes	Yes	UK col. Lat Am	UK col. Lat Am
Estimation	WLS	WLS	WLS	WLS	WLS	Heckman ML	2SLS
Observations	78	68	78	68	78	73	78
Adjusted R-squared	0.81	0.84	0.81	0.83	0.74		0.83

Standard errors in parentheses (Robust SE in column 7)
* significant at 10%; ** significant at 5%; *** significant at 1%

WLS weights are 1/std(dependent variable)

Controls always included in the corruption equations: gastil, age, lyp, lpop, eduger, trade, oecd, federal, avelf, prot80, catho80, confu. Continental and Colonial dummy variables are included as indicated. Columns 6 and 7 include dummy variables for UK colonial origin and for Latin America.

Heckman probit specification includes: AGE, *LATO1*, *ENGFRAC, EURFRAC* and dummy variables for Latin America and for UK colonial origin.

2SLS first-stage specification includes: con2150, con5180, con81 plus all the second stage regressors.

Table 5
Estimated Propensity Scores and Electoral Rule

Country	Estimated propensity score	*MAJ*	Country	Estimated propensity score	*MAJ*
Argentina	0.119	0	Bulgaria	0.240	0
Mexico	0.122	0	Philippines	0.255	1
Venezuela	0.123	0	Poland	0.261	0
United States	0.125	1	Czech Republic	0.265	0
Switzerland	0.126	0	Turkey	0.268	0
Uruguay	0.129	0	Senegal	0.298	0
Norway	0.131	0	Russia	0.310	0
Chile	0.131	1	Nepal	0.340	1
Luxembourg	0.134	0	Romania	0.351	0
Germany	0.135	0	Ukraine	0.372	1
Belgium	0.140	0	Belarus	0.373	1
Iceland	0.140	0	Bangladesh	0.376	1
Denmark	0.141	0	El Salvador	0.381	0
Finland	0.142	0	Canada	0.382	1
Ecuador	0.145	0	Australia	0.549	1
Sweden	0.146	0	New Zealand	0.583	1
Bolivia	0.147	0	Trinidad&Tobago	0.623	1
Costa Rica	0.148	0	Bahamas	0.636	1
Brazil	0.149	0	South Africa	0.642	0
Guatemala	0.151	0	Barbados	0.656	1
Peru	0.155	0	Ireland	0.658	0
Netherlands	0.155	0	Belize	0.709	1
Paraguay	0.157	0	Israel	0.713	0
France	0.158	1	Singapore	0.745	1
United Kingdom	0.161	1	Cyprus (G)	0.747	0
Colombia	0.161	0	Mauritius	0.748	1
Spain	0.167	0	Jamaica	0.756	1
Japan	0.167	1	Malaysia	0.758	1
Austria	0.169	0	St.Vincent&Grenadines	0.769	1
Italy	0.175	0	Fiji	0.783	0
Taiwan	0.177	0	Malta	0.793	0
Dominican Rep.	0.189	0	India	0.826	1
Namibia	0.195	0	Sri Lanka	0.840	0
Portugal	0.209	0	Papua New Guinea	0.842	1
South Korea	0.210	0	Pakistan	0.842	1
Greece	0.217	0	Botswana	0.846	1
Honduras	0.218	0	Ghana	0.867	1
Latvia	0.218	0	Zambia	0.884	1
Estonia	0.223	0	The Gambia	0.890	1
Slovak Republic	0.224	0	Uganda	0.894	1
Nicaragua	0.226	0	Zimbabwe	0.899	1
Thailand	0.230	1	Malawi	0.908	1
Hungary	0.237	0			

Logit specification: *LYP, GASTIL, PROT80, AVELF*, dummy variables for UK colonial origin and for Latin America.

Table 6

Political Rents and Majoritarian Elections

Matching estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	graft	graft	graft	cpi9500	govef	graft
emaj	-0,25	-1,09	-0,23	-0,57	-0,28	-0,26
	0,39	0.53**	0,49	0.78	0,47	0.40
Estimation	Nearest	Stratification	Kernel	Kernel	Kernel	Kernel
Logit Specification	[1]	[1]	[1]	[1]	[1]	[2]
Sample	Default	Default	Default	Default	Default	Default
Obs. on control group	52	52	52	47	52	52
Obs. on treatment group	29	29	29	24	29	29

Notes: Standard errors in parentheses estimated by boothstrapping, 200 reps. Logit specifications underlying estimated propensity scores are [1]: lyp, gastil, avelf, prot80, col_uka, laam; [2]: lyp, gastil, avelf, prot80, engfrac, eurfrac, lat01.

Table 7

Political Rents and Electoral Rules
Panel estimates

	(1)	(2)	(3)	(4)	(5)	(6)
dep.var.	ICRG	ICRG	ICRG	ICRG	ICRG	ICRG
maj				-0,73	-0,66	-0,65
				(0.41)*	(0.40)*	(0.41)
pind	-1,69	-1,65	-1,51			
	(0.69)**	(0.68)**	(0.67)**			
magn	1,38	1,48	1,44			
	(0.72)*	(0.71)**	(0.70)**			
pvalue F-test for joint signif. political variables	0,049	0,043	0,058			
Sample	90s panel, default	90s panel, default	90s panel, default	90s panel, default	90s panel, default	90s panel, default
Estimation	Country FE	Country and year FE	Country FE	Country FE	Country and year FE	Country FE
Controls	No	No	Yes	No	No	Yes
Observations	640	640	623	648	648	631
Countries	78	78	78	79	79	79
R-squared	0,01	0,07	0,05	0,01	0,07	0,04

Notes: Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.
Controls include: lyp, lpop, trade, gastil.