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ELECTORAL RULES AND CORRUPTION

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ABSTRACT

Is corruption systematically related to electoral rules? A number of studies have tried to uncover economic and social determinants of corruption but, as far as we know, nobody has yet empirically investigated how electoral systems infuence corruption. We try to address this lacuna in the literature, by relating corruption to different features of the electoral system in a sample from the late nineties encompassing more than 80 (developed and developing) democracies. Our empirical results are based on traditional regression methods, as well as non-parametric estimators. The evidence is consistent with the theoretical models reviewed in the paper. Holding constant a variety of economic and social variables, we find that 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. Altogether, proportional elections are associated with more corruption, since voting over party lists is the dominant effect, while the district magnitude effect is less robust.

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1. Introduction

Elected politicians have ample opportunities to abuse their political powers at the expense 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.

This variation suggests that corruption may be systematically related to political institutions. 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.

Is corruption systematically related to electoral rules? A few theoretical studies have attempted to formally address this important question. We describe the main ideas behind the existing theoretical models and their testable implications in Section 2 below.

The main purpose of the paper is empirical, however. 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 the data we will use. As far as we know, however, nobody has yet investigated how electoral rules correlate with corruption in a large cross section of countries. We try to fill this lacuna in the literature by relating corruption to different features of the electoral system in a sample from the late nineties, encompassing data from more than 80 (developed and developing) democracies. Our corruption variable is based on the data compiled by Transparency International, measuring perceptions of the degree of corruption, as seen by business people, risk analysts and the general public.

We confront these corruption data with data on political institutions in two alternative ways. Section 4 reports on estimates obtained from traditional regression analysis. In Section 5, we instead present non-parametric estimates to address possible selection bias in the choice of electoral rules and to allow for possible non-linearities. Specifically, we use two propensity-score methods that have recently begun to make their way into the tool box of labor economists, but have not yet been applied in the literature on political economics.

The evidence is consistent with the theoretical hypotheses outlined in Section 2. Holding constant a variety of economic and social variables, we find that specific features of proportional electoral rules are associated with more widespread perceptions of corruption. In particular, corruption tends to be higher in those countries where a larger fraction of candidates is elected via voters over party lists rather than over individual candidates, that is where there is less individual accountability. We also find that larger voting districts—implying lower barriers to entry—are associated with less corruption, but this result is less empirically robust. Proportional electoral systems tend to combine these two opposite effects. Thus, they typically have large district magnitude with citizens voting for party lists rather than for individual candidates. But the second dimension is empirically more important than the first: according to the data, proportional electoral rules are associated with more corruption than majoritarian elections.

2. Theory

What can economic and political theory say about the mapping from the electoral rule to corruption or rents for politicians? To the best of our knowledge, only a few studies have tried to model this relation formally.

One idea is that an electoral system promoting the entry of new parties or new candidates better protects voters against corruption. The clearest formalization is perhaps the model suggested by Myerson (1993). He assumes, on the one hand, that candidates (parties) and voters have opposite interests regarding the level of corruption. On the other hand, interests diverge within the set of voters as well as within the set of candidates along another ideological dimension. In this setting, corrupt incumbents may still cling on to power if voters sharing the same ideological preferences cannot find a good substitute candidate (party). Given how other voters behave, an individual voter may also find it too costly to vote for another party representing her own ideological group, as that may raise the probability of victory for a candidate on the other side of the ideological scale. Thus the voters' ability to hold corrupt incumbents accountable is better the lower are the barriers to entry in the electoral system.

In Myerson's model, voting behavior is endogenous to the electoral system, whereas corruptibility is assumed to be an exogenous feature of each candidate (party). 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), however, one can easily reformulate Ferejohn's model such that deterrence of rent extraction takes the place of promotion of effort.

In the model, electoral defeat is less fearsome the higher 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 should be less beneficial in containing corruption than electoral systems with large districts. More specifically, district *magnitude* and *thresholds* for representation become the critical features of the electoral system. Because larger electoral districts and lower thresholds imply lower barriers to entry, they should be associated with less corruption, ceteris paribus.

But electoral systems differ in another important dimension, namely in the *electoral formula* translating vote shares into seat shares. *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 between different 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 distinction between plurality and PR. The main idea is that voting over individual candidates creates a direct link between individual performance and reappointment, which gives an individual incumbent strong incentives to perform well by putting in effort or avoiding corruption. When voters choose among party lists, politicians' chances of re-election primarily depend on their ranking in the list, not on their performance. If lists – as is commonly the case – are drawn up by party leaders, the ranking will most likely reflect criteria unrelated to competence in providing benefits to voters, such as party loyalty, or effort within the party (rather than in office). Then, the incentives to perform well are much weaker. Persson and Tabellini's analysis therefore suggests that corruption should be positively associated with the proportion of representatives elected on lists as opposed to individually assigned seats.[1]

A final set of formal political models of corruption can be found in Polo (1998),

[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.

Svensson (1998) and Persson and Tabellini (1999). These are all models of electoral competition predicting that the extraction of rents is increasing in political instability, as more instability makes the perceived probability of winning less sensitive to rent extraction. Persson and Tabellini (1999) also contrast equilibrium behavior by politicians in two stylized electoral systems: one with PR in a single nation-wide district, another with plurality rule in a number of single-member districts. Electoral competition becomes 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. This prediction is less precise than those above, in that the argument does not distinguish well between district magnitude and the electoral formula.

Countries with "majoritarian electoral systems" typically combine single-member districts and plurality rule, however. At the opposite extreme, some "proportional systems" indeed have large districts and voters choose among party lists (Israel e.g. have just one nation-wide district where all representatives are elected and very low thresholds). But in between these polar cases one finds intermediate systems, involving different district magnitudes, different size thresholds, and multi-tier systems mixing plurality rule and PR.[2]

This institutional variation is fortunate in that it allows us to test separately the different hypotheses outlined above. These can be summarized as follows:

> *H1:* Ceteris paribus, countries with larger district magnitude and lower thresholds for representation should have less corruption (the barriers-to-entry effect).
>
> *H2*: Ceteris paribus, countries with a larger share of representatives elected as individuals rather than as members of lists should have less corruption (the career-concern effect).
>
> *H3*: Ceteris paribus, plurality rule in single-member districts should be associated with less corruption than PR in large districts; moreover, corruption should be larger, the larger is political instability (the electoral-competition effect).

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

3. Data

This section discusses the key variables used in the empirical analysis and our specification, while the Data Appendix gives a precise description of the data sources.

3.1. Corruption

Finding an empirical measure of political corruption and rents is not an easy task. As Tanzi (1998) observes, it is difficult to define corruption in the abstract and – as the act is illegal – violators try to keep secret its specific instances. Furthermore, cultural and legal differences across countries make it hard to investigate corruption without taking country-specific features into account. A good proxy for political corruption should thus offer reliable information on the unlawful abuse of political power, and a high degree of comparability across different countries.

The Corruption Perceptions Index (*CPI*) is perhaps the best measure to meet these requirements. Produced by Transparency International, a world-wide organization and 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. The index ranges between 0 (perfectly clean) and 10 (highly corrupt).[3] Lambsdorff (1998) gives an extensive description of the statistical characteristics of the *CPI*. We have taken an average of *CPI* scores for the three years in 1997-1999, which restricts our sample size to about 85 countries. In the 1997 *CPI*, 7 different surveys are considered from 6 different institutional sources, in 1998, 12 surveys from 7 institutions, and in 1999 14 surveys from 10 sources. For most countries analyzed in this paper, at least 3 surveys are available in the *CPI* for each of the 3 years. 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. This suggests that the surveys, though independently done, really measure some common features of the country in question. 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 weigh the observations with the inverse of the standard deviation among the different surveys available for each country,

[3] The score in the original surveys was rescaled so that all of them rank countries on a range from 0 to 10.

STDEV, in the regression analysis to follow.

A number of recent empirical studies of corruption have employed this index, including Fisman and Gatti (1999), Treisman (2000) and Wei (1997a and 1997b). Our rationale for using *CPI* is that it explicitly includes measures of so-called "grand" or large corruption (see Lambsdorff, 1998, for the specific composition). Corruption at the highest level in the public sector fulfils this particular definition (see Rauch, 1995 and Tanzi, 1998) and approximates illegal political rents, which would be our ideal dependent variable, given the theory discussed in Section 2.[4]

Unfortunately, the *CPI* measure is only available for the last half of the nineties. Meaningful panel data analysis is thus ruled out.

3.2. Political Data

We have developed a few continuous explanatory variables to test the hypotheses formulated in Section 2. Data on legislative institutions were mainly taken from the Inter-Parliamentary Union, based in Geneva, from Kurian (1998), and from the International Institute for Democracy and Electoral Assistance (1997), based in Stockholm. For most of the countries in the sample, our data refer to institutions in the mid-nineties.

To test the barriers-to-entry effect (*H1*), we first develop an index of the average magnitude of each constituency in different countries. District magnitude (*DISMAG*) is a measure of the average number of representatives elected in each district (see e.g., Cox, 1997). As is well known, the lower is district magnitude, the higher a party's electoral strength must be to gain representation in the legislative body. In this paper, we measure average district magnitude by the formula

$$DISMAG = 1 - \frac{CONSTIT}{MPS} \ ,$$

where *MPS* denotes the number of elected representatives in the lower or single house of the Parliament and *CONSTIT* – the number of constituencies – is obtained by adding up the number of single-member and multi-member districts within each country. *DISMAG* thus ranges between 0 and 1, taking a value of 0 for a system with only single-member districts, and close to 1 for a system with a single electoral district. Note that *CONSTIT* (and hence *DISMAG*) does not

[4] A specific justification is a high correlation among the perception of bureaucratic and political corruption. Lambsdorff (2000) reports a correlation of 0.88 between the assessment of politicians and of public administrators in the Gallup International survey, one of the sources of the *CPI*.

distinguish between single tier and upper tier districts (in multi-tier systems). In fact, *CONSTIT* identifies only all "geographic areas within which votes are aggregated and seats allocated" (Cox, 1997).[5] Column 2 in **Table 1** lists the values of *DISMAG* for the countries in our sample.

The career-concern effect (*H2*) instead focuses on the electoral formula. To test this second prediction, we construct another continuous explanatory variable:

$$PLIST = \frac{LISTMPS}{MPS} ,$$

where *LISTMPS* is the number of representatives elected through party list systems. Thus, *PLIST* measures the percentage of representatives elected on a party list. As *DISMAG, PLIST* ranges between 0, under plurality rule in every district, and 1, in a system with full proportionality. Column 1 of **Table 1** lists the values of *PLIST* for the countries in our sample.

By construction, this variable lumps together several different mechanisms for voting over lists of representatives. The Political Science literature usually classifies list systems into one of three different types: closed list, preference (or open list) vote, and panachage (see Cox, 1997). Closed lists do not allow the voters to express a preference for individual candidates. If a preference is allowed, the party list is still the default option for the voter (e.g., in Finland). The panachage is the least restrictive list system, since it allows the voter to express preferences across parties (e.g., in Switzerland). As these alternatives are still quite distinct from the personal selection under plurality rule, they were all included in our variable *LISTMPS.*

A final point is worth noting. Most PR systems use party list allocation formulas in distributing seats within each district (like the D'Hondt, the modified St. Laguë, or the LR-Hare; see LeDuc, Niemi, and Norris (1996) for a comprehensive survey). The precise mechanism does not immediately affect the individual candidate's career concern. But a few PR systems do not rely on party lists. The proportional system adopted for the Dáil Eireann in Ireland e.g. is based on the Single Transferable Vote. Here, we set $PLIST = 0$.

[5] In Greece, for example, the legislative body consists of 300 deputies. By current electoral law, 282 of the total *MPS* are elected by party list vote from 50 multi-seat constituencies, 6 are elected by majority rule from single-seat constituencies, and 12 are elected by party list vote (with a 3% threshold) from a national constituency, in order to warrant proportional representation. In this case, *CONSTIT* would be 57, obtained by adding up 50 multi-member districts, 6 single-member districts, and 1 upper-tier national district, and *DISMAG* 0.81.

The electoral competition effect (*H3*) really combines the two dimensions measured by *PLIST* and *DISMAG*. To test it, we rely on an indicator variable taking a value of 1 only for countries which rely exclusively on plurality (or majority) rule in their legislative elections. Countries with either a fully proportional electoral formula, or a mixed system, we code by 0. This variable, *MAJ*, is thus a broad proxy measure of majoritarian elections.

According to the electoral competition hypothesis outlined in Section 2, corruption should also be positively related to political instability. Here, we use a measure, *INSTAB*, taken from Treisman (2000) which proxies for political instability in the executive by the number of government leaders in a recent period (1980-1993 for almost all countries in the sample).

Finally, we also include a measure of the respect for basic political rights taken from the Freedom House Annual Surveys. We use an average for the years 1990/91-1998/99. Fisman and Gatti (1999) also used this variable, denoted by *POLRIGHT*, as a control in their study of fiscal centralization and corruption. We expect corruption to be higher in less democratic regimes (a higher value of *POLRIGH*T), since the voters find it harder to remove corrupt leaders and to punish corruption in general.

3.3. Other explanatory variables

On the basis of the empirical strategy described in the next section, the other determinants of corruption can be classified in two main categories, namely standard economic and social controls, and legal and colonial history.

Standard economic controls are those included in the basic specification shown in column 1 of **Table 3**. To control for poverty, we consider the logarithm of GNP per capita, adjusted for purchasing power ($LOG(Y)$). The variable *OPEN* is defined as the sum of merchandise exports and imports divided by the value of GDP measured in current US. dollars. Openness of the market was found to be a significant negative determinant of corruption by Ades and Di Tella (1999) (although with doubts about the direction of causation). Data on population (in millions) are converted to logarithms and indicated by $LOG(POP)$. All these data are collected from the World Bank's World Development Indicators for the second half of the nineties (see Data Appendix for details). The population's education level is proxied by the secondary school gross enrolment ratio (for male and female population), taken from UNESCO and indicated by *EDU.* Data on ethno-linguistic fractionalization (*ELF*) are taken from La Porta *et al.* (1999), as are the religious variables. These authors investigated how the ICRG Index of corruption was influenced by religion, while Treisman (2000) found evidence of a significant negative impact of Protestant tradition on corruption measured by *CPI*. We include the population share with a Protestant or Catholic religious tradition. Discrete religious variables (for e.g. Confucian dominance) are from Wacziarg (1996), as are regional dummy variables. Empirical studies of corruption including regional dummy variables can be found in Leite and Weidmann (1999), for Africa, and Wei (1997a), for East Asia.

Legal origin dummies are from La Porta *et al.* (1999), who extensively analyzed their impact on various measures of government efficiency. They found French and Socialist legal origin in particular to have a significant impact on some measures of the quality of government, although not on corruption. Treisman (2000) studied the effect of legal origin on corruption carefully, attempting to separate the legal framework, as such, from colonial influences on a country's "legal culture" (expectations on the efficiency of the legal system as a whole). Colonial variables (for British, French, and Spanish colonies, plus colonies of other types) are from Wacziarg (1996). To adjust the strength of colonial forces, we weight these data by the extent of colonial dominance in the last 250 years.

Table 2 shows the partial correlations among the main variables. Some of them are highly correlated, as expected. Richer economies have less corruption, more education and better political rights. Note, however, that the two political variables of most interest, *PLIST* and *DISMAG*, are not highly correlated with other independent variables, suggesting that multicollinearity may not be a first order problem in interpreting our results. On the other hand, as **Tables 1** and **2** show, *PLIST* and *DISMAG* are highly correlated with each other. Proportional elections tend to have both a large number of candidates elected on party lists and large district magnitudes, while the opposite is true for majoritarian elections. Since these two variables are expected to have opposite effects on corruption, including both of them is important to avoid specification bias due to omitted variables.

4. Regression estimates

This section gives the results of our regression analysis, testing the hypotheses outlined in Section 2 by help of the data described in Section 3. The next section presents our non-parametric estimates.

4.1. Economic and social determinants of corruption

We start by a regression relating corruption, as measured by *CPI*, to the economic and social determinants discussed in Section 3. Estimation is by weighted least squares, the weights being the (inverse) standard deviation of the *CPI* score, *STDEV* – see Section 3 and the Data Appendix for a precise definition. The estimates are reported in **Table 3**, column 1. **Table 4** reports parallel unweighted (OLS) regressions with White-corrected standard errors. The results are very similar for all specifications.

Corruption is lower in richer (Y), more open ($OPEN$) and smaller (POP) economies and in the OECD, in countries where citizens are better educated (EDU) and where there is more fractionalization as measured by (ELF). Religion also has an important effect on corruption: Catholic ($CATH$) countries tend to be more corrupt, Protestant ($PROT$) countries less corrupt, while Confucian ($CONFU$) religion seemingly has no effect – though this last variable becomes statistically significant in the regressions reported below.

The results conform to earlier studies and prior expectations (see, in particular, Treisman, 2000). Altogether, the basic economic and social variables explain between 85 and 90% of the variation in the data. The residual variation is displayed in **Table 1**, where column 3 reports the *CPI* score and column 4 reports the residuals from this regression. The residuals range from - 2.5, for Chile, to + 2.3, for Belgium. Other countries with large residuals include Costa Rica and Israel (both negative) and Czech Republic, Greece and Turkey (all positive). Clearly, our basic controls eliminate the most striking differences across countries. In fact, holding these variables constant, dummy variables for geographic location (such as Africa, Asia and Latin America) do not have a statistically significant impact on corruption.

4.2. Political determinants of corruption

Next, we ask whether political institutions indeed contribute to explaining corruption. We focus on the electoral rule as measured by *PLIST* and *DISMAG*. As suggested by hypothesis *H3,* we also include our measure of political instability *INSTAB*. Finally, we include the extent of political rights by *POLRIGHT*. We continue to control for the same list of economic and social variables as in column 1. The results, displayed in column 2, are consistent with the predictions of the theory. First, the coefficient on *PLIST* is highly significant and positive, suggesting that voting over party lists rather than over individuals leads to more corruption. The standardized beta coefficient of *PLIST* is 0.27, one of the highest in the set of explanatory variables, suggesting that the effect of this variable is quantitatively important, and not just statistically significant. The estimated coefficient on *DISMAG* is negative, suggesting that the barriers to entry due to small districts also lead to more corruption, but it is statistically significant only at the 10% confidence level. *INSTAB* also has an estimated coefficient with the expected positive sign, albeit borderline significant at the 5% level. *POLRIGHT* has the expected sign, but a t-statistic only around 1.5. Finally, coefficients on the other variables remain quite stable, despite the addition of the new variables, suggesting that multicollinearity is not driving the results.

The coefficient on *PLIST* remains stable to changes in the specification, such as dropping the variables in column 2 with the lowest t-statistics, such as *CATH, CONFU, ELF* and *POP*, dropping the political variables *POLRIGHT* and *INSTAB*, and even dropping the variable *DISMAG*. The estimated coefficient on *DISMAG*, on the other hand, is less stable, and its statistical significance is affected by the details of the specification. As **Table 4** shows, we obtain similar results in the unweighted regressions.

So far, we have discussed the effect on corruption of two separate but related dimensions of electoral systems: *PLIST* and *DISMAG*. As already noted, however, these two variables are highly correlated: majoritarian electoral systems typically have small district magnitudes and a large fraction of seats allotted by votes for individual candidates, i.e. they have small values of both *PLIST* and *DISMAG*. Since these two variables are predicted and found to have opposite effects on corruption, it is natural to ask which is the prevailing effect. For this purpose, in column 6 of **Tables 3** and **4** we have replaced *PLIST* and *DISMAG* with the dummy variable *MAJ,* taking a value of 1 in majoritarian electoral systems – see Section 3 and the Data Appendix for a precise definition. This also allows us to test the other aspect of the electoral competition effect (*H3*), derived from models (such as Persson and Tabellini, 1999) that only distinguish crudely between majoritarian and proportional elections. The data suggest that *PLIST* has the stronger influence: the estimated coefficient of *MAJ* is negative and statistically significant. Overall, majoritarian electoral systems thus seem to induce less corruption than proportional elections.[6]

[6] In the case of quite a few countries, the classification between majoritarian and proportional elections is ambiguous. These countries were thus defined as semi-proportional, and included with a separate dummy variable (*SEMI*). The estimated coefficient on this dummy variable, not reported in the Tables, was not significantly different from zero, suggesting that these semi-

4.3. Other institutional determinants of corruption

An important test of whether our results are robust is to check how they survive the inclusion of other institutional variables. As documented in other empirical studies (in particular Treisman, 2000), perceptions of corruption are correlated with dummy variables reflecting a country's legal and colonial origin. Do the effects of *PLIST* and other political institutions survive, once we control for different historical origins in our sample of countries? The answer is displayed in columns 3–5 of **Table 3**. Column 3 adds the legal origin variables; French and socialist legal origin is associated with more corruption compared to UK legal origin. The other political variables, *INSTAB* and *POLRIGHT* now become statistically insignificant, but the estimated coefficient on *PLIST* remains remarkably stable and that on *DISMAG* becomes clearly statistically significant.

Column 4 adds the colonial origin variables. French colonial origin is associated with less corruption, thereby counteracting the positive effect on corruption of having a French legal system.[7] Otherwise, the results are not much affected. The estimated coefficients on *PLIST* and *DISMAG* drop somewhat, but remain statistically significant. The results are also quite similar if colonial origin is measured as a 0–1 variable, irrespective of when independence was obtained. Finally, column 5 reports the effect of colonial origin, without also controlling for legal origin. Now, the estimated coefficient of *PLIST* drops further, though it remains statistically significant at the 10% confidence level, while the estimated coefficient of *DISMAG* becomes insignificant. Curiously, none of the colonial origin variables is statistically significant.

Overall, we conclude that the effect of *PLIST* on corruption is quite robust to the inclusion of these institutional variables, while the effect of *DISMAG*, as before, is less robust. Given the number of right-hand side variables included in these regressions, the statistical significance of *PLIST* is pretty remarkable. The estimated coefficient on *PLIST* is most sensitive to the inclusion of the colonial origin variables without the legal origin dummies, and in particular to UK and French colonial origin. It is really these two variables together that matter for the estimated coefficient of *PLIST*; if either of them is dropped, or if they are entered together with the legal origin variables, the coefficient on *PLIST* is not affected. We do not have a good explanation for this feature of the data, other than that it may reflect collinearity among the regressors.

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proportional countries could be lumped together with the clearly proportional ones.

[7] Several countries have a French-type legal system without being former French colonies.

Do the results of including institutional dummy variables also extend to the blunter classification into majoritarian and proportional elections according to the *MAJ* dummy? When either legal origin dummy variables, or colonial origin dummy variables, or both, are included among the regressors, the t-statistics on *MAJ* drops to just below -1.5 (not shown in the Tables). With this cruder classification it is thus harder to disentangle the effect of the electoral system from that of other institutional variables. The reason may again be multicollinearity, as many countries classified as majoritarian according to *MAJ* also have an UK legal and/or colonial origin.

4.4. Simultaneity problems?

To what extent can we regard our political variables as truly exogenous? This question is obviously highly pertinent for the variables *INSTAB* and *POLRIGHT*. Politicians appearing as more corrupt would behave more myopically, and for this reason, could be thrown out of office more frequently. And more corrupt politicians could be more likely to interfere with the democratic process in order to extract additional rents from their citizens. If so, the estimates of our regressions on these two coefficients would be biased. This is not too troublesome for our main results concerning the electoral rule, however. As already noted, the estimated coefficients on *PLIST* and *DISMAG* are robust to omitting the other two political variables, *INSTAB* and *POLRIGHT* from the specification. Moreover, judging from their pair-wise correlation coefficients in **Table 2**, *INSTAB* and *POLRIGHT* seem to be uncorrelated with our two variables of interest, *PLIST* and *DISMAG*.

But what about the electoral rule itself? If some electoral rules were conducive to *more* corruption, would not malevolent and corrupt politicians be more likely to choose exactly those rules? There could, of course, also be reverse causation from corrupt politicians to electoral rules conducive to *less* corruption if voters fed up with crooked politicians – rather than the crooked politicians themselves – manage to push through electoral reform. The recent electoral reforms in Italy and Japan mentioned in the Introduction seem to be examples of the latter type.

An argument in defence of regarding *PLIST* and *DISMAG* as exogenous is the fact that electoral reforms are very rare. In the last 25 years only about 10 structural changes in the electoral system have been implemented in the 85 countries of our sample. Most of these changes have led to a mixed electoral law combining single-member districts with corrections for proportional representation, but shifts in the direction both of more pronounced PR or Plurality have also been recorded.[8] This stability suggests considerable institutional inertia. Indeed, this inertia has been such a common feature of this century's political history that Political Scientists refer to an "iron law" of political self-preservation in the context of comparative electoral systems analysis. Changing the electoral regime is difficult because it requires support from a large majority in most democracies, even if the constitution does not explicitly say so. For all practical purposes, therefore, we think that the electoral rule may be regarded as determined by chance and history.

This defence of our results is not watertight, however. Even if electoral rules are determined by history and are unlikely to change in response to corruption, how do we know that we have not left out some important historical determinant of both the electoral rule and corruption itself? One standard way of coping with this problem is to rely on instrumental variable estimation. Unfortunately, we have not been able to find any suitable instruments in this case. Any plausible, and observable, historical determinant of the electoral rule we could imagine might also have an independent effect on corruption.

We have shown that our results are robust to controlling for the colonial origin of a country and other historical variables. But what if these observables also influence the choice of the electoral regime? Our OLS estimates are still unbiased under two assumptions: (i) the model is recursive (i.e., the error term of the relation determining the electoral rule as a function of observables is uncorrelated with the error term of the corruption relation); (ii) the relationships are linear with homogenous coefficients. As discussed, the first assumption is critical, but we can do little to relax it in the absence of reliable instruments. The assumption of linearity is also restrictive, however. Suppose that the impact of the electoral rule on corruption is systematically related to some observables that also determine the electoral rule. If this non-linearity is important, the OLS estimates could be severely biased, particularly if these observables differ across countries under different electoral rules. This problem can be addressed, however. We can check whether the results hold up under non-parametric estimates, free from strong assumptions about functional form and we can also allow the historical determinants

[8] We are here considering only radical transformations in the electoral law. That is, we look at changes in the allocation mechanism of at least one third of the total number of legislative seats in the lower or single house for the period 1975-95. Significant recent examples would be the brief electoral reform in France in 1986 (from Majority to PR, and back), the New Zealand electoral reform (from FPTP to mixed member) in 1993, the same year's Italian reform (from PR to mixed member system), or the 1994 Japanese reform (from Plurality with SNTV in 3-5 member districts to mixed-member system).

to influence the choice of electoral rule. The next section deals with these issues.

5. Non-parametric estimates

Non-parametric estimates of the effects of a particular treatment in the absence of experimental data have been used in the medical sciences for some time (see Rosenbaum and Rubin, 1983 for a systematic analysis). More recently, such methods have been introduced into economics, especially as tools for evaluating labor market and education programs (see for instance Dehejia and Wahba, 1999, and Heckman, Ichimura and Todd, 1997). In this section, we apply so-called propensity score estimation to our task of evaluating how electoral rules affect corruption. As the typical reader may not be familiar with such estimation, we begin with a brief summary of the main ideas. A useful and accessible survey, which puts the methodology in context, can be found in Blundell and Costa Dias (2000).

5.1. A brief introduction

For simplicity, we only consider the two groups of countries defined by our binary dummy variable *MAJ*, namely those with strict plurality (or majority) rule, $MAJ = 1$, and all the others with either proportional or mixed systems, $MAJ = 0$. Maintaining the same terminology as in the evaluation literature, we define as "treated" the countries that do *not* have majoritarian elections, and denote this set by T. The set of majoritarian countries is not subject to treatment and will make up our "control" group, denoted by C. As our prior is that treatment causes more corruption, we would like to estimate the *average* effect of "treatment on the treated". Indexing our corruption measure *CPI* for treated and non-treated countries by T and C superscripts, we can define this by

$$\tau = E(CPI_i^T \mid i \in T) - E(CPI_i^C \mid i \in T) , \tag{5.1}$$

where subscripts denote countries and the E operator denotes expectations, conditional on the distribution of *CPI* in the group with majoritarian elections. The problem is that the last term on the right-hand side is not observable: we cannot directly observe the corruption level a country with majoritarian elections would have, if it hypothetically had proportional elections.

How can we exploit the information in our control group, allowing for the fact that – in this non-experimental setting – the choice of the electoral rule is not likely to be random? Suppose selection is affected by a set of observable variables, X, such as colonial origin or religious tradition; variables which could also have an independent effect on corruption. To exploit the control group, we then need a central identifying assumption, "conditional independence" also known as the "selection on observables" assumption (Rosenbaum and Rubin, 1983, Rubin, 1974, 1977). This assumption asserts that, conditional on X, corruption and the choice of electoral rule are independent. In other words, no omitted or unobserved variable influences both the choice of the electoral rule and the corruption outcome, once we have controlled for X, formally:

$$E(CPI_i^T \mid i \in T, , X_i) - E(CPI_i^C \mid i \in T, X_i) \ .$$

This assumption allows us to replace the unobservable counterfactual in (5.1) and write:

$$\tau(X) \equiv E(CPI_i \mid i \in T, X_i) - E(CPI_i \mid i \in C, X_i) \ . \tag{5.2}$$

In fact, we implicitly relied on a version of the conditional independence assumption already in our OLS estimation – the recursiveness assumption (i) in Section 4.4. Here, it is reformulated in a context more general in two respects: we now explicitly consider the possibility of selection into the electoral rule, and we do not impose any precise functional forms on the relation between electoral rules and corruption. Our parameter of interest can thus be written as $\tau = E[\tau(X)]$, where the expectation is now taken over the possible realizations of X. A non-parametric test of our central hypothesis could be obtained from (5.2), by combining observations in T and C with similar values of X, and then evaluating τ. But if X is multidimensional and has non-trivial distributions in T and C this is very hard, particularly in a small sample like ours.

The propensity-score literature (Rosenbaum and Rubin, 1983) shows that, under further assumptions, (5.2) can be restated on a more parsimonious form. Specifically, let $p(X_i)$ be the probability of selection into treatment (i.e., non-majoritarian electoral rule), conditional on the observable variables X_i. Furthermore, assume that $0 < p(X_i) < 1$, for all X_i; that is, the distribution of X_i has a common support. Then, we can rewrite (5.2) as:

$$\tau(X) \equiv E(CPI_i \mid i \in T, p(X_i)) - E(CPI_i \mid i \in C, p(X_i)) \ . \tag{5.3}$$

The probability $p(X_i)$ is also called the propensity score of country i. A non-parametric test of $\tau = E[\tau(X)] > 0$ is obviously much easier if $\tau(X)$ is obtained from (5.3) rather than (5.2), as the propensity score is uni-dimensional and has values constrained to lie between 0 and 1.

In the following two subsections, we discuss estimation of the propensity score and test our central hypothesis using two alternative, non-parametric estimators.

5.2. Estimating the propensity score

The first step is to estimate the propensity score. We do that by running a linear probit regression of the treatment indicator $(1 - MAJ)$ on a number of observed variables, the vector X in the previous section. We include eight variables in X : log per capita income $(LOG(Y))$, our three dummy variables for religious beliefs (*CATH, PROT, CONFU*), and our four dummy variables for colonial origin (*COLOES, COLOFR, COLOUK, COLOTH*). These are the main variables that we think might influence both the choice of electoral rules as well as corruption.

Next, we want to verify that conditioning on the estimated propensity score, as in (5.3), is indeed equivalent to conditioning on the full vector X, as in (5.2). That is, we ask whether the distribution of X is the same across the treatment and control groups, conditional on the propensity score. Following the procedure in Dehejia and Wahba (1999), we rank the full set of countries according to their estimated propensity scores. Based on this ranking, we group the observations into five strata: the first stratum includes countries with an estimated probability between 0 and 0.2 of having the treatment of proportional elections, the next includes countries with an estimated probability of 0.2 to 0.4, and so on. We then test for equality of means between the treatment and the control group, within each stratum, and for each of the eight variables in X. In no case can we reject the null hypothesis that the means are equal, at the 5% confidence level. When the same test is performed on the whole sets T and C, rather than within each stratum, we reject equal means for five out of eight variables.[9]

Before proceeding, we want to verify another aspect of comparability across the treatment $(MAJ = 0)$ and control $(MAJ = 1)$ groups, namely the common support condition discussed in the previous section. For five countries with majoritarian elections (Bangladesh, India, Pakistan, Nigeria, and Tanzania), the estimated propensity score was lower than the lowest score among the proportional countries. These majoritarian countries were thus discarded as non-comparable to any proportional country. There was no need for discarding countries at the top of the ranking.

[9]Results for these tests as well as the probit estimates are available upon request.

5.3. Estimating the treatment effect

In this subsection, we use two different non-parametric estimators to estimate the treatment effect $\tau = E[\tau(X)]$. Details of the estimators and their standard errors are given in the appendix. Here, we describe their properties and report the results of testing $\tau > 0$.

Consider first the *stratification* estimator, which relies on the same grouping into strata as in the prior subsection. This estimator of τ computes the average difference in *CPI* between the proportional (treatment) and majoritarian (control) countries within each stratum and forms the weighted average of these differences, weighing each stratum by the number of treated observation it contains. It thus balances the treatment and control countries group-wise, within the five strata.

Graph 1 illustrates the overlap between control $(MAJ = 1)$ and treatment $(MAJ = 0)$ countries within each stratum by a simple histogram. As expected, we gain treatment observations and lose control observations as the estimated propensity score increases. But some overlap of treatment and controls is present in every stratum. The small overlap in the extreme bins (0-0.2 and 0.8-1) does not bias our estimates, as long as the two groups are homogeneous in terms of the covariates (as is the case here).[10] But the low number of controls relative to treatments in the higher strata raises the standard error of our estimate (see the Appendix).

Consider next the *matching* estimator, or more precisely, the method of nearest matching with replacement of controls. Here, instead of utilizing the full set of controls on the common support (like in stratification), we discard the more distant controls and instead use some controls more than once. In a first step, every treated $(MAJ = 0)$ country is matched with the most similar control $(MAJ = 1)$ country; i.e., the nearest match in terms of propensity score. In our case, this entails dropping 13 majoritarian countries from the control group.[11] We thus obtain 53 pairs, equivalent to the number of treated countries. The matching estimator is just the average difference in corruption outcomes across these pairs of treated and control countries.

The rationale for this estimator is to reduce the bias, due to differences in the observables, by finding the nearest match in the control group for every treated country. As a certain control can be the nearest match for more than one treat-

[10] See Dehejia and Wahba (1999).

[11] The countries excluded in the matching process are Botswana, Ghana, Jamaica, Jordan, Kenya, Malawi, Mauritius, Morocco, Singapore, Uganda, Vietnam, Zambia, and Zimbabwe.

ment country, it should be matched more than once (and then replaced in the control set). **Graph 2** shows that the fit of the propensity score across pairs is generally very close. The flats of the dashed line represent control countries used several times. For instance, the majoritarian country with the highest estimated propensity score (of 0.93) is Chile. Quite intuitively, Chile is the nearest match for most of the remaining countries in South America, which have proportional elections. Similarly, France (with a propensity score of 0.87) is matched with many of the proportional countries in Europe. While such multiple use of certain controls is desirable in terms of reducing bias, it has a cost in terms of less precise estimates (see Appendix).

Table 5 reports the estimates obtained with these two methods. The matching estimator yields a mean difference in corruption of 0.95, while the stratification estimator yields a difference of only 0.28. Recall from **Table 4** (last column) that the OLS estimator of the mean difference in corruption was 0.58 (there, the dummy variable was MAJ, so the sign of the coefficient should be reversed). Our two non-parametric estimates thus confirm the previous finding, namely that non-majoritarian countries are more corrupt. The estimated effect of the electoral rule on corruption is larger than the OLS estimate according to one estimator, smaller according to the other.

We also note that the standard errors are much larger than the OLS standard errors; even though the matching estimator gives a higher estimate than OLS, it is not statistically significant at conventional confidence levels. 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. To obtain more precise estimates, we have to make more restrictive assumptions about functional forms. An example can be found in the second column of **Table 5,** where we report estimates of the treatment effect by linear regression on the balanced samples. The variables in these regressions – in addition to the $(1 - MAJ)$ dummy – are the same as those entering the probit, and we use the matched and stratified samples, respectively.[12]

All in all, we conclude that – subject to the identifying assumptions stated in this section – the inference from our regression analysis in Section 4 appears robust to possible specification bias.

[12] The estimation method is OLS for the stratified sample, WLS for the matched sample (each observation in the control group is weighted by the number of times it is used in the matching).

6. Concluding remarks

This paper has presented new results on how electoral rules affect corruption. Our empirical results are consistent with theoretical models suggesting that voting on party lists (the career-concern effect) or in relatively small electoral districts (the barriers-to-entry effect) reduce the effectiveness with which voters can exploit the ballot to deter corruption. The estimated effects of the electoral system are non-trivial. For instance, they suggest that Chile's low corruption outcome – a CPI 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 ($PLIST = 0$) in two-member districts ($DISMAG = 0.5$). Similarly, Belgium – an outlier with much higher corruption than predicted – could cut its corruption level towards that of France by introducing plurality rule in place of PR. 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 for 75% of the legislature – instead appears as a step in the right direction.

Future work on electoral rules and corruption might consider additional aspects of the electoral law, such as the effects of thresholds for representation. According to the discussion in Section 2, such thresholds should allow for more corruption, ceteris paribus, by raising barriers to entry. It would also be interesting to study the effect of electoral reforms over time in a true panel data set. Unfortunately, this seems infeasible in the light of available data. The problem is not so much to measure changes in the electoral rules over time (even though coding available documentation into time-variable measures corresponding to *PLIST* and *DISMAG* would require a non-trivial amount of work), but the lack of relevant and comparable measures of corruption over time.

Future work should also further investigate the statistical robustness of our results. In particular, other non-parametric estimators than the matching and stratification estimators used here might strike a better balance between bias and efficiency in samples as small as ours.[13] More generally, we believe that this kind of non-parametric approach might be a promising avenue for empirical work on international cross-section and panel data in the field of political economics. Al-

[13] Heckman, Ichimura and Todd (1998) and Heckman et al. (1998) discuss and evaluate the properties of different non-parametric and semi-parametric estimators in the context of the treatment literature.

lowing for systematic selection and non-linearities might be particularly important in the kind of international comparisons considered here.

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DATA APPENDIX

Dependent variable and weight

CPI = Proxy for Political Corruption and "Grand" Bureaucratic Corruption. Corruption Perceptions Index published by Transparency International, NGO for worldwide fight against corruption, describes the level of perceived corruption in the public sector using a poll of political risk indexes. Original scores range from 0 (completely corrupt) to 10 (clean). Average of *CPI* indexes for years 1997, 1998, and 1999. Source: Transparency International. With regard to the 1997 Corruption Perceptions Index, data for a larger sample were taken from Lambsdorff (1998), although the original limit of four surveys was not satisfied for all the observations. The index is inverted in the scale by subtracting values from 10 to make the results more intuitive.

STDEV = The standard deviation mentioned is referred to the different rankings given to a specific country by the different polls considered in the *CPI*. Its inverse is used as a weight to adjust for measurement error in corruption. Source: Transparency International.

Socio-Economic Variables

EDU = Proxy for the expected level of schooling and education in the country. Data show total enrollment in primary and secondary education, regardless of age, expressed as a percentage of the population age-group corresponding to the national regulations for these two levels of education. Average on the period 1994-96. Source: UNESCO.

ELF = Index of Ethnolinguistic Fractionalization approximates for 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. The components are: 1) Atlas Narodov Mira, 1960; 2) Muller, 1964; Roberts, 1962; 4) and 5) Gunnemark, 1991. Source: La Porta et al. (1999). For Central and Eastern Europe countries computations follow Mauro (1995) with data from Quain (1999).

OPEN = Trade as a share of PPP. GDP is the sum of merchandise exports and imports measured in current U.S. dollars divided by the value of GDP converted to international dollars using purchase power parity conversion factors. It is a proxy for the level of openness of the national market to competition (see Ades and Di Tella, 1999). Data are average for years 1996 and 1997. Source: World

Development Indicators (WDI, World Bank). We computed observations for Belgium, Botswana, Iceland, and Tanzania with World Bank's alternative data and same methodology.
POP = Population in millions. It is based on the de facto definition of population, which counts all residents regardless of legal status or citizenship - except for refugees not permanently settled in the country of asylum, who are generally considered as part of the population of the country of origin. The values shown are the average of midyear estimates for the period 1996-1999. Source: World Development Indicators (WDI, World Bank).
Y = Gross National Product converted to international dollars using purchase power parity rates. An international dollar has the same purchasing power over GNP as a U.S. dollar in the United States. The values shown are the average of midyear estimates for the period 1996-1999. Source: World Development Indicators (WDI, World Bank).

Geographic and institutional variables[14]
CATH = Percentage of the total population belonging to the Roman Catholic religion for the period 1980-1990. Source: La Porta et al. (1999).
COLO(*ES, FR, or UK*) = Dummy variable taking value 1 if the country has, for a significant time, been a colony of Spain (or Portugal) (ES), United Kingdom (UK), or France (FR), and 0 otherwise. Source: Wacziarg (1996). The *COLOTH* dummy was computed as *COLOTH* = *EVERCOL* - *COLES* - *COLUK* - *COLFR*. In order to weight for the colonial exposition, we multiplied these dummy variables by (250 - *TIME IND*)/250, where 250 was the default time of independence value for non-colonies.
CONFU = Religious tradition dummy, taking value 1 if the main religious tradition in the country is Confucianism, 0 otherwise. Source: Wacziarg (1996).
EVERCOL = Dummy variable taking value 1 if the country has ever been a colony since 1776, 0 otherwise. Source: Wacziarg (1996)
*LEGOR_*(*UK, FR, GE, SO, SC*) = Dummy variable for the origin of the legal system and, consequently, of 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. (1999).

[14] We are grateful to Rafael La Porto et al. and to Romain Wacziarg for sharing their data with us.

OECD = Dummy variable for OECD member countries, taking value 1 if a country is OECD member, 0 otherwise. Source: Persson and Tabellini (1998).
PROT = Percentage of the total population belonging to the Protestant religion for the period 1980-1990. Source: La Porta et al. (1999).
TIME IND = Years of independence of the country since 1748. (Note that we considered the default value of 250 for the non-colonies and the USA). Source: Wacziarg (1996).

Political Variables
CONSTIT = Total number of primary and secondary (plus tertiary, if indicated) electoral districts in the country. Only territorial districts are considered in the computations. A 1 is added only when national district is explicitly mentioned. Sources: Quain (1999).
INSTAB = Average number of government leaders per year (number of government leaders in the recent period divided by the length of period in years).

Recent period: most countries = Jan. 1980 - Dec. 1993; former USSR = Jan. 1991 - Dec. 1994; post communist Europe = Jan. 1990 - Dec. 1994. Must be $>$ 14 days to count. Leader is PM in parliamentary systems, president or head of state in presidential or non-democracy. Source Rulers database: http//www.geocities.com/Athens/1058/rulers.html.
LISTMPS = Number of legislators in lower or single chamber for the latest legislature that has been appointed through party list voting mechanisms (open and closed) and different formulas (D'Hondt; Saint Lagüe; Hagenbach-Bischoff; LR-Hare; LR-Droop). Note that we had to deal with some ambiguous cases. We included Switzerland's panachage because of the strong weight of party influence, but excluded Chile's dual majority list allocation because of the clear plurality-type rationale. Appointed or ex officio members of the Parliament are excluded. Sources: Quain (1999) and Kurian (1998).
MAJ = Dummy variable taking value 1 in the presence of either a majority or a plurality electoral rule, 0 otherwise. In ambiguous cases we used the presence of party list vote or not to make a distinction between *MAJ* and *SEMI*. For example, dual majority in Chile is classified as 1, while Italy, with a $\frac{1}{4}$ of total seats PR allocated, is classified as 0 (and *SEMI* = 1). Only legislative elections for lower or single house are considered. Sources: Cox (1997), International Institute for Democracy and Electoral Assistance (1997), Quain (1999), and Kurian (1998).
MPS = Number of elected legislators in lower or single chamber for the latest legislature of each country. Appointed or ex officio members of the Parliament are

excluded. Source: International Institute for Democracy and Electoral Assistance (1997), Quain (1999), and Kurian (1998).
POLRIGHT = Proxy for the level of respect of the basic political rights (such as the right of free political association). The index ranges from 1 (max freedom) to 7 (complete absence of political liberties). Average of data from 1990/91 to 1998/99 assessments. Source: Freedom House.
SEMI = Dummy variable taking value 1 in the presence of specific types of semi-proportional representation, 0 otherwise. Semi-proportional electoral rule identifies those mixed electoral systems characterized by both PR and FPTP representation for allocating seats (for example Bolivia, Germany, Italy after the reform, etc.). The share of the total number of seats allocated under the Proportional rule can be greater or smaller than the complementary plurality-allocated share. Only legislative elections are considered. Sources: Cox (1997), International Institute for Democracy and Electoral Assistance (1997), Quain (1999), and Kurian (1998).

Statistical Appendix

The matching estimator
Consider matching (with replacement) on the nearest unit, in terms of estimated propensity scores, yielding a set of controls $i \in C$ matched to the group of treated $i \in T$, on the common support of the propensity score. The estimator for the difference in means is given by:

$$\tau^M = \frac{1}{N^T}\sum_{i \in T} CPI_i^T - \frac{1}{N^T}\sum_{i \in C} w_i CPI_i^C \ , \tag{A1}$$

where N^T denotes the size of the treated group and w_i the number of times a particular control $i \in C$ is used in the matching.

Assume that these observations are independent and treat the weights w_i as fixed. Furthermore, assume that the variance of CPI is the same within each group C and T, but potentially different across these groups. Then we can compute (as in Lechner, 2000) the variance of τ^M as:

$$\begin{aligned} \mathrm{Var}(\tau^M) &= (\frac{1}{N^T})^2[\sum_{i \in T} \mathrm{Var}(CPI_i^T) + \sum_{i \in C} (w_i)^2 \mathrm{Var}(CPI_i^C)] \\ &= \frac{1}{N^T}[\mathrm{Var}(CPI_i^T) + \frac{\sum_{i \in C}(w_i)^2}{N^T}\mathrm{Var}(CPI_i^C)] \ . \end{aligned} \tag{A2}$$

As is evident from (A2), there is a relatively strong penalty from "overusing" some observations, particularly in small samples. Note that if the matching yields a single control for each treated unit, we get the conventional formula:

$$\mathrm{Var}(\tau^M) = \frac{1}{N^T}[\mathrm{Var}(CPI_i^T) + \mathrm{Var}(CPI_i^C)] \ .$$

Our standard errors are computed from (A2). As Lechner (2000) notes, the result is only an approximation as it does not take into account the estimation of the propensity score, and hence the uncertainty about the weights w_i.

The stratification estimator
Consider now the cruder stratification estimator (as e.g., in Deheija and Wahba, 1999), which forms a weighted average of the difference in means across the discrete bins, $b = 1, ...B$, produced by the propensity score estimation. Its formula

is:

$$\tau^{S} = \frac{1}{N^{T}}\Big[\sum_{b} N_b^{T}\Big(\sum_{i\in T_b}\frac{1}{N_b^{T}}CPI_i^{T} - \sum_{i\in C_b}\frac{1}{N_b^{C}}CPI_i^{C}\Big)\Big] \tag{A3}$$
$$= \frac{1}{N^{T}}\Big[\sum_{i\in T} CPI_i^{T} - \Big(\sum_{b}\frac{N_b^{T}}{N_b^{C}}\sum_{i\in C_b} CPI_i^{C}\Big)\Big],$$

where T_b, C_b are the sets of treated and control observations in bin b and N_b^{T}, N_b^{C} the corresponding number of observations.

With the same assumptions as above, we can derive the variance of τ^{S}:

$$\mathrm{Var}(\tau^{S}) = \Big(\frac{1}{N^{T}}\Big)^2\Big[N^{T}\,\mathrm{Var}(CPI_i^{T}) + \sum_{b}\Big(\frac{N_b^{T}}{N_b^{C}}\Big)^2 N_b^{C}\,\mathrm{Var}(CPI_i^{C})\Big] \tag{A4}$$
$$= \frac{1}{N^{T}}\Big[\mathrm{Var}(CPI_i^{T}) + \sum_{b}\frac{N_b^{T}}{N^{T}}\frac{N_b^{T}}{N_b^{C}}\mathrm{Var}(CPI_i^{C})\Big]\ .$$

As is evident from (A4), there is a penalty for a small number of controls relative to treatments in a bin, particularly if that bin includes a significant share of the treated units in the sample. Suppose that $N_b^{T} = N_b^{C}$ for all b. Then, (A4) again produces the conventional formula:

$$\mathrm{Var}(\tau^{S}) = \frac{1}{N^{T}}\Big[\mathrm{Var}(CPI_i^{T}) + \mathrm{Var}(CPI_i^{C})\Big]\ .$$

Our (approximate) standard errors of the stratification estimates are computed from (A4).

Table 1 (begins)

Political and corruption data

Country	PLIST	DISMAG	CPI	RESIDUAL
Argentina	1.00	0.91	7.06	0.89
Australia	0.00	0.00	1.25	-1.13
Austria	1.00	0.95	2.43	-0.46
Bangladesh	0.00	0.00	8.20	-0.71
Belarus	0.00	0.00	6.77	0.71
Belgium	1.00	0.87	4.68	2.30
Bolivia	n/a	0.93	7.55	n/a
Botswana	0.00	0.00	4.73	0.06
Brazil	1.00	0.95	6.11	-0.80
Bulgaria	0.50	0.87	6.62	0.30
Cameroon	0.68	0.68	8.28	1.09
Canada	0.00	0.00	0.83	-1.28
Chile	0.00	0.50	3.42	-2.56
Colombia	1.00	0.80	7.56	0.71
Costa Rica	1.00	0.88	4.28	-2.10
Cyprus (G)	1.00	0.89	3.39	-1.20
Czech Republic	1.00	0.96	5.13	1.56
Denmark	0.98	0.91	0.02	-0.40
Ecuador	0.85	0.74	7.30	0.40
Egypt	0.00	0.50	7.29	0.36
El Salvador	1.00	0.82	6.56	-1.09
Estonia	1.00	0.89	4.15	0.53
Finland	1.00	0.93	0.37	-0.25
France	0.00	0.00	3.35	0.15
Germany	0.50	0.48	1.96	-0.33
Ghana	0.00	0.00	6.91	-0.19
Greece	0.98	0.81	4.95	1.52
Guatemala	0.20	0.71	6.61	-0.86
Honduras	1.00	0.86	8.17	0.63
Hungary	0.54	0.49	4.87	0.96
Iceland	1.00	0.87	0.62	0.46
India	0.00	0.00	7.15	-0.62
Indonesia	1.00	0.94	7.86	1.30
Ireland	0.00	0.75	1.94	-0.56
Israel	1.00	0.99	2.71	-1.55
Italy	0.25	0.20	5.22	1.35
Ivory Coast	0.00	0.12	7.45	0.02
Jamaica	0.00	0.00	6.20	0.97
Japan	0.40	0.38	3.88	0.49
Jordan	0.00	0.75	5.54	n/a
Kenya	0.00	0.00	7.73	0.36
Latvia	1.00	0.95	6.26	0.75
Luxembourg	1.00	0.93	1.30	n/a

Note: The residuals refer to the regression in **Table 3**, Column 1

Table 1 (concludes)
Political and corruption data

Country	PLIST	DISMAG	CPI	RESIDUAL
Malawi	0.00	0.00	5.90	-1.26
Malaysia	0.00	0.00	4.87	-0.01
Mauritius	0.00	0.68	5.05	0.18
Mexico	0.40	0.40	6.88	0.26
Morocco	0.00	0.00	6.25	-0.83
Namibia	1.00	0.68	4.70	0.66
Netherlands	1.00	0.88	0.99	-0.29
New Zealand	0.46	0.45	0.66	-1.39
Nicaragua	1.00	0.80	6.57	-1.26
Nigeria	0.00	0.25	8.25	0.48
Norway	1.00	0.88	1.06	0.82
Pakistan	0.00	0.00	7.52	-0.55
Paraguay	1.00	0.78	8.27	1.32
Peru	1.00	0.99	6.03	-0.68
Philippines	0.00	0.00	6.68	0.15
Poland	1.00	0.88	5.37	0.44
Portugal	0.98	0.90	3.28	-0.65
Romania	1.00	0.88	6.75	0.39
Russia	0.50	0.50	7.64	1.33
Senegal	0.50	0.64	6.65	-1.05
Singapore	0.00	0.74	1.05	-0.47
Slovak Republic	1.00	0.97	6.25	0.63
South Africa	1.00	0.98	4.95	0.52
South Korea	0.15	0.15	5.90	0.76
Spain	0.99	0.85	3.80	0.21
Sri Lanka	1.00	0.88	5.83	-0.85
Sweden	1.00	0.91	0.58	-0.31
Switzerland	0.98	0.87	1.20	-0.29
Taiwan	0.20	0.86	4.69	n/a
Tanzania	0.00	0.00	7.98	-0.74
Thailand	0.00	0.61	6.91	0.51
Tunisia	0.12	0.84	5.32	-0.75
Turkey	1.00	0.86	6.60	1.88
Uganda	0.00	0.00	7.84	-0.52
Ukraine	0.00	0.00	7.33	0.70
United Kingdom	0.00	0.00	1.49	-0.84
United States	0.00	0.00	2.46	0.07
Uruguay	1.00	0.81	5.72	-0.05
Venezuela	0.99	0.88	7.44	0.84
Vietnam	0.00	0.65	7.37	-0.71
Yugoslavia	1.00	0.74	7.18	n/a
Zambia	0.00	0.00	6.84	-0.45
Zimbabwe	0.00	0.00	5.98	-0.37

Note: The residuals refer to the regression in **Table 3,** Column 1

Table 2
Partial Correlations

	CPI	Y	POLRIGHT	EDU	ELF	PROT	CATH	CONFU	PLIST	DISMAG	INSTAB
Y	-0.86										
POLRIGHT	0.66	-0.66									
EDU	-0.69	0.62	-0.66								
ELF	0.40	-0.45	0.55	-0.53							
PROT	-0.57	0.34	-0.31	0.38	-0.05						
CATH	0.06	0.09	-0.26	0.14	-0.22	-0.33					
CONFU	-0.01	0.14	0.13	-0.03	-0.13	-0.13	-0.24				
PLIST	-0.17	0.18	-0.37	0.36	-0.30	0.19	0.32	-0.23			
DISMAG	-0.20	0.20	-0.26	0.31	-0.34	0.08	0.28	-0.03	0.86		
INSTAB	0.02	0.01	-0.27	0.07	-0.17	0.02	-0.04	0.06	0.14	0.14	
MAJ	0.18	-0.21	0.38	-0.35	0.39	-0.11	-0.33	0.12	-0.86	-0.82	-0.23

Table 3
WLS
Dependent Variable: CPI

	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	16.23	14.33	14.25	13.62	15.40	15.55
	(10.45)	(7.74)	(7.60)	(6.23)	(7.05)	(8.21)
LOG(Y)	-0.97	-0.85	-0.84	-0.75	-0.89	-0.96
	(-4.77)	(-3.95)	(-3.63)	(-3.15)	(-3.84)	(-4.49)
LOG(POP)	0.12	0.10	0.13	0.13	0.07	0.14
	(1.39)	(1.10)	(1.47)	(1.29)	(0.72)	(1.56)
EDU	-0.02	-0.02	-0.02	-0.03	-0.02	-0.02
	(-2.18)	(-2.12)	(-2.72)	(-2.98)	(-2.67)	(-1.98)
OECD	-1.59	-1.58	-1.54	-1.27	-1.37	-1.57
	(-4.39)	(-4.59)	(-4.42)	(-0.99)	(-3.55)	(-4.55)
OPEN	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01
	(-2.73)	(-2.79)	(-2.41)	(-3.00)	(-2.66)	(-2.73)
ELF	-0.79	-0.80	-0.50	-0.47	-0.81	-0.67
	(-1.68)	(-1.75)	(-1.04)	(-0.99)	(-1.74)	(-1.47)
PROT	-0.02	-0.02	-0.01	-0.01	-0.02	-0.02
	(3.00)	(-3.81)	(-1.86)	(-1.25)	(-3.77)	(-3.38)
CATH	0.01	0.01	0.01	0.01	0.01	0.01
	(2.36)	(1.45)	(0.93)	(0.79)	(1.21)	(1.62)
CONFU	0.3	0.50	0.81	1.27	0.48	0.16
	(0.60)	(0.961)	(1.51)	(2.42)	(0.88)	(0.33)
PLIST		1.49	1.51	1.35	1.04	
		(2.67)	(2.72)	(2.25)	(1.70)	
DISMAG		-1.10	-1.48	-1.33	-0.86	
		(-1.67)	(-2.17)	(-1.98)	(-1.30)	
POLRIGHT		0.17	0.10	0.19	0.22	0.11
		(1.55)	(0.90)	(1.70)	(1.91)	(1.05)
INSTAB		0.86	0.70	0.44	0.50	0.78
		(1.92)	(1.31)	(0.83)	(1.03)	(1.71)
MAJ						-0.60
						(-2.44)
LEGAL	NO	NO	YES	YES	NO	NO
COLONIES	NO	NO	NO	YES	YES	NO
Adj. R^2	0.87	0.89	0.89	0.90	0.89	0.89
N. Obs.	82	80	80	80	80	81

Notes: Weights are the Inverse of STDEV for CPI observations. T-statistics in parentheses. LEGAL= YES means that we are controlling for legal origin. COLONIES = YES means that we are controlling for colonial origin.

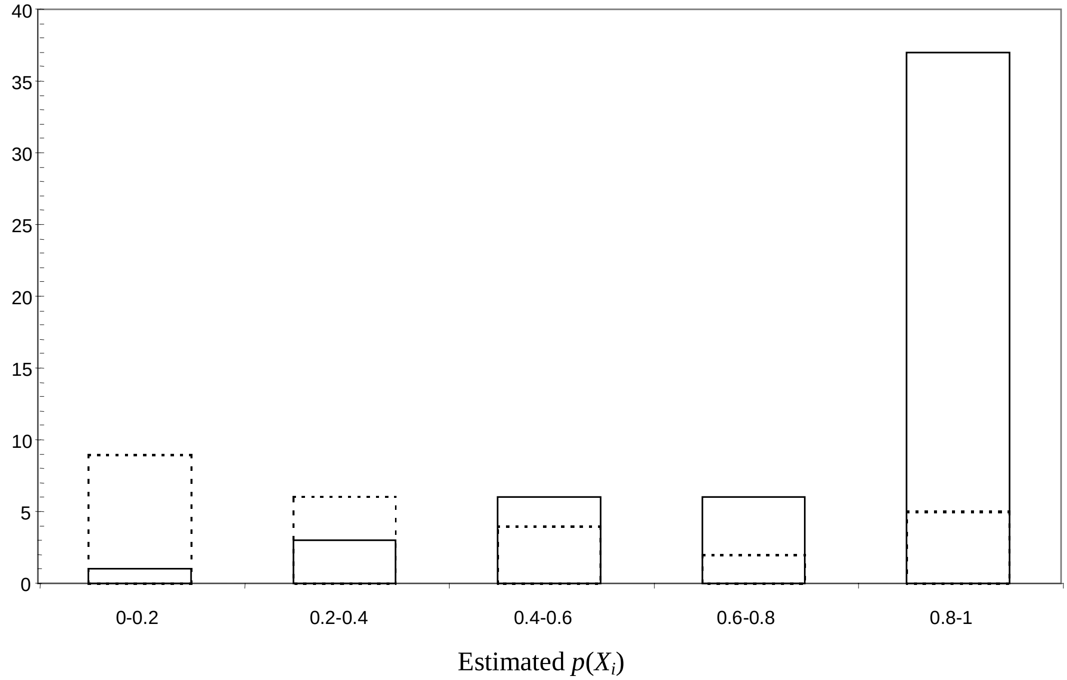
Table 4
OLS
Dependent Variable: CPI

	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	15.74	12.99	13.15	11.94	13.87	14.43
	(9.34)	(6.94)	(7.01)	(5.49)	(6.31)	(7.25)
LOG(Y)	-1.02	-0.82	-0.85	-0.73	-0.84	-0.95
	(-4.26)	(-3.76)	(-3.82)	(-3.14)	(-3.47)	(-4.09)
LOG(POP)	0.16	0.15	0.18	0.20	0.13	0.18
	(2.03)	(1.77)	(2.32)	(2.21)	(1.46)	(2.22)
EDU	-0.01	-0.01	-0.02	-0.02	-0.02	-0.01
	(-1.58)	(-1.89)	(-1.91)	(-2.62)	(-2.53)	(-1.49)
OECD	-1.27	-1.30	-1.28	-1.10	-1.12	-1.30
	(-2.95)	(-3.51)	(-3.52)	(-2.95)	(-2.75)	(-3.31)
OPEN	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01
	(-3.91)	(-3.35)	(-2.54)	(-3.04)	(-2.97)	(-3.51)
ELF	-0.37	-0.25	0.08	0.07	-0.22	-0.11
	(-0.84)	(-0.48)	(0.14)	(0.12)	(-0.38)	(-0.23)
PROT	-0.02	-0.02	-0.02	-0.01	-0.02	-0.02
	(-3.40)	(-3.88)	(-1.95)	(-1.31)	(-3.92)	(-3.33)
CATH	0.01	0.01	0.01	0.01	0.01	0.01
	(2.06)	(1.47)	(1.04)	(1.14)	(1.40)	(1.43)
CONFU	0.48	0.57	0.91	1.18	0.51	0.33
	(1.22)	(1.38)	(1.78)	(2.58)	(1.44)	(0.83)
PLIST		1.43	1.38	1.20	1.03	
		(2.73)	(2.48)	(1.93)	(1.67)	
DISMAG		-1.04	-1.33	-1.12	-0.82	
		(-1.85)	(-2.25)	(-1.76)	(-1.27)	
POLRIGHT		0.19	0.11	0.19	0.23	0.12
		(1.85)	(1.02)	(2.05)	(2.18)	(1.18)
INSTAB		1.14	0.88	0.69	0.72	1.00
		(3.10)	(1.95)	(1.61)	(1.57)	(2.78)
MAJ						-0.58
						(-2.05)
LEGAL	NO	NO	YES	YES	NO	NO
COLONIES	NO	NO	NO	YES	YES	NO
Adj. R^2	0.84	0.87	0.88	0.89	0.87	0.87
N. Obs.	82	80	80	80	80	81

Notes: White corrected t-statistics in parentheses. LEGAL = YES means that we are controlling for legal origin. COLONIES = YES means that we are controlling for colonial origin.

Graph 1

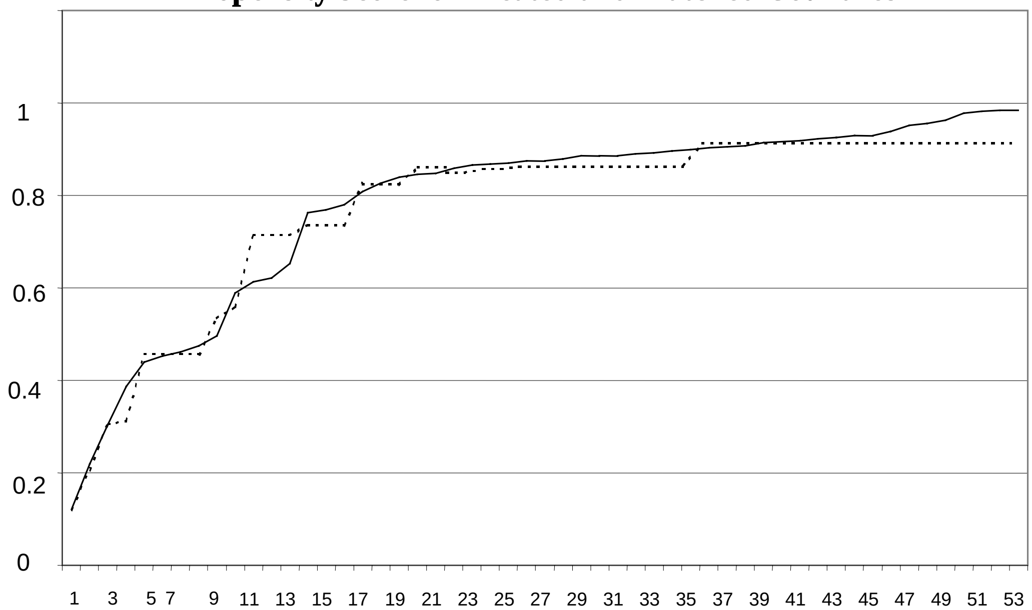
Histogram of Estimated Propensity Score Strata for treated and controls



5 units discarded, first bin contains 9 controls
Solid = Treated (*MAJ* = 0)
Dashed = Control (*MAJ* = 1)

Graph 2

Propensity Score for Treated and Matched Countries



Treated units indexed from lowest to highest estimated $p(X_i)$

Solid = Treated (*MAJ* = 0)
Dashed = Control (*MAJ* = 1)

Table 5

Estimates of Average Effect on CPI of Non-Majoritarian Elections

	Non-parametric **Mean difference**	**Parametric** **Regression coefficient**	**Number of controls**
Matching	0.95 (1.14)	0.43 (0.19)	13
Stratification	0.28 (0.82)	0.17 (0.29)	26

Standard errors in parentheses.
Mean differences computed as in the Appendix.
Regression coefficients from linear regression of *CPI* on (1 –*MAJ*) and all variables that enter the Probit, estimated by OLS on stratified sample and by WLS on the matched sample, the weights on each control reflecting the number of times it is used in the matching.