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 late nineties encompassing more than 80 (developed and developing) democracies. Our empirical results are based on traditional regression methods, as well as non-parametric matching methods. The evidence is consistent with the theoretical priors. 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, because the effect of party lists appears to dominate the effect of larger districts.

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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.

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 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 electoral formula and the size of voting districts. With regard to the first aspect, plurality rule provides stronger incentives against rent extraction compared to proportional representation (PR). Under plurality rule, incumbents are individually accountable to the voters. In a PR system, instead, politicians are elected from party lists, which introduces a free rider problem and a more indirect chain of delegation, from voters to parties to politicians, weakening individual incentives for good behavior. With regard to the second aspect, district magnitude, small electoral districts raise higher barriers to entry than large districts. 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.

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 the data we will use. 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 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 among business people, risk analysts and the general public.

We confront these data on corruption with data on electoral rules using two different statistical methods. Section 4 reports on estimates obtained from traditional regression analysis. Section 5 presents estimates obtained by three non-parametric matching estimators, to address possible selection bias in the choice of electoral rules and to allow for possible non-linearities – such as heterogenous effects of electoral rules on corruption depending on the cultural or historical environment. Specifically, we use 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, corruption tends to be higher in those countries where a larger fraction of candidates is elected via voting over party lists rather than on individual candidates. We also find that larger voting districts are associated with less corruption, but this result is empirically less robust. Proportional electoral rules tend to combine these two opposite effects: they typically have large district magnitude as well as citizens voting for party lists. 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 analytical studies have addressed this question.

One idea is that electoral rules promoting the entry of many parties or candidates protect voters against corruption in a better way. 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 an 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 rule, 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 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 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), 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.

In the real world, these two features of electoral rules, district magnitude and the electoral formula, are combined in a systematic pattern. Countries with "majoritarian electoral systems" typically combine single-member districts and plurality rule. 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). 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

[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.

lower thresholds for representation 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 have less corruption (the *career-concern effect*).

H3: Ceteris paribus, plurality rule in single-member districts is associated with less corruption than PR in large districts; moreover, corruption is larger the larger is political instability (the *electoral-competition effect*).

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". Our rationale for using *CPI* is that it explicitly includes measures of so-called "grand" 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.[3] A number of other recent empirical studies of corruption have em-

[3] A specific justification is a high correlation among the perception of bureaucratic and po-

ployed this index, including Fisman and Gatti (1999), Treisman (2000) and Wei (1997a and 1997b). Unfortunately, the *CPI* measure is only available for the last half of the nineties. Meaningful panel-data analysis is thus ruled out.[4]

The *CPI* 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).[5] 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. Moreover, the survey results differ not only across countries in different continents and at different levels of development, but also across closely located countries with similar economic characteristics. 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, *STDEV*, in the regression analysis to follow.

Column 3 in **Table 1** lists the *CPI* values for the countries in our sample. As the Table reveals, the values are distributed quite evenly over the 0-10 scale. Moreover, we find no particular clustering at very low or very high values (7 observations in the 0-1 cell and no observations in the 9-10 cell), suggesting that truncation is not a major problem.

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litical 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*.

[4] Other recent studies, such as Adsera, Boix and Paine (2000), have used a similar measure compiled by Kaufmann, Kraay and Loido-Zabatòn (1999), called GRAFT, based on similar sources but a different aggregation method.

[5] The score in the original survey was converted by deducting it from 10, to obtain a 0 to 10 score ranking countries from low to high corruption. Note that CPI is not measured in integers but in real numbers. The reason is that CPi is an average of several polls and by averaging a number with decimal points is often obtained.

3.2. Political Data

We have developed some continuous explanatory variables to test the hypotheses formulated in Section 2. Data on electoral and 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 political 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 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).[6] 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:

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

[6] 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.

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 $PLIST = 0$.

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. There are 31 countries with $MAJ = 1$ and 55 countries with $MAJ = 0$.

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 *POLRIGHT*), 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 economic development, 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 as a percentage of GDP. 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.

Tables 2a and **2b** show the partial correlations among the main variables, and their means grouped by electoral rule. Some of these variables are highly correlated, as expected. Richer economies have less corruption, more education and better political rights. When it comes to the two political variables of most interest, *PLIST* and *DISMAG*, they have a maximum correlation of 0.2-0.3 with the other independent variables. Different electoral rules is thus not completely random, suggesting that in the analysis to follow we should take seriously issues of selection. **Tables 1** and **2** also show that *PLIST* and *DISMAG* are highly correlated with each other. As previously mentioned, certain "proportional" electoral rules tend to combine a large number of candidates elected on party lists and large district magnitude, while the opposite is true for "majoritarian" electoral rules. Since *PLIST* and *DISMAG* are expected to have opposite effects on corruption, regression analyses should include both of them to avoid specification bias.

Finally, some variables have the same means irrespective of the broad electoral rule, while other variables take different means depending on the electoral rule. In particular, strictly majoritarian countries ($MAJ = 1$) tend to be more fractionalized (higher mean value of *ELF*), less democratic (higher mean value of *POLRIGHT*), and tend to have a more educated population, have fewer Catholics and more unstable governments (higher mean value of *INSTAB*).

4. Regression estimates

This section gives the results of our regression analysis, testing the hypotheses outlined in Section 2 with the data described in Section 3. The next section presents our non-parametric matching 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 similar, not only here but in all other 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 (the way we measure CPI, a negative (positive) residual means less (more) corrupt than expected). 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 the electoral-competition 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 multi-collinearity 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.

There are a few outlying observations: Chile, Jamaica, Israel and Mauritius. Dropping all of them together increases the statistical significance of $PLIST$ and $DISMAG$. If instead only Chile is dropped from the sample, the estimated coefficient of $DISMAG$ becomes smaller and not significantly different from 0, but $PLIST$ retains a t-statistic of 1.9. Finally, as discussed more at length in Section 5, the 80 or so countries included in our sample differ in important ways along many potentially relevant dimensions. The analysis in the next section identifies 67 countries that are more easily comparable (in a precise way to be explained). Restricting our regressions to the sample of these countries leads to very similar estimates of the coefficients of $PLIST$ and $DISMAG$ on corruption as those reported in **Tables 3** and **4**.

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. the values of *PLIST* and *DISMAG* are both 0 or close to zero – cf. **Table 2b**. 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 replace *PLIST* and *DISMAG* with the binary variable *MAJ,* taking a value of 1 in plurality (majority) electoral systems – see Section 3 and the Data Appendix for a precise definition. This also allows us to test the electoral-competition hypothesis (*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.[7]

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 the different histories 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 origins are 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, counteracting the positive effect on corruption of a French legal system.[8] 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 binary (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 now 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

[7] 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-proportional countries could be lumped together with the clearly proportional ones.

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

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.

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-statistic 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; many countries coded with *MAJ = 1* also have a UK legal and/or colonial origin.

4.4. Simultaneity (selection) problems?

All the analysis so far has treated our political variables as truly exogenous and uncorrelated with the error term in the regression. This is certainly highly questionable 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* appear 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, giving rise to *reverse causation*? Of course, reverse causation could also run 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 against reverse causation is that electoral reforms are very rare. In the last 25 years only about 10 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 PR, but shifts in the direction of more pronounced PR or Plurality have also been recorded.[9] This stability suggests considerable inertia: changing electoral rules is difficult because it requires support from a large majority in most democracies, even if the constitution does not explicitly say so. 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 comparative electoral systems analysis. For practical purposes, therefore, broad features of electoral rules might be seen as decided by chance and history.

This argument may rule out direct reverse causation, but it does not rule out other forms of simultaneity. Electoral rules may be determined by history and slow to respond to current changes in corruption. But how do we know that the historical variables determining current electoral rules do not also determine current corruption? That is, how can we rule out problems caused by *non-random selection* of electoral rules?

For simplicity, consider the electoral rule in its crude binary form: MAJ_i equal to 0 or 1. Suppose that the link between corruption (CPI) and the electoral rule takes the form:

$$CPI_i = F_i(\mathbf{Z}_i, MAJ_i) + u_i \ , \tag{4.1}$$

where the i subscript denotes countries, $\mathbf{Z}$ is a vector of observable controls, u is an unobserved error term and $F(\cdot)$ is a function to be estimated. Suppose further that countries select their electoral rule based on the relation:

$$MAJ_i = \ G_i(\mathbf{X}_i) + e_i \ , \tag{4.2}$$

where $\mathbf{X}$ is a vector of observables, such as colonial origin or location, possibly included in the vector $\mathbf{Z}$, while e is an unobserved error term.

[9] 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).

Our OLS estimates are unbiased under two assumptions: (i) the model is recursive: i.e., the error term e of the selection relation (4.2) is uncorrelated with the error term u of the corruption relation (4.1); (ii) the functions $F(\cdot)$ and $G(\cdot)$ are linear with homogenous coefficients.

Assumption (i) is strong and rules out selection bias due to *unobservable* (omitted) historical variables. A standard way to relax this assumption is to rely on instrumental-variable estimation. Unfortunately, we have not been able to find any suitable instruments in this case. Every plausible historical determinant of the electoral rule we could imagine might also have an independent effect on corruption. Hence we cannot credibly relax assumption (i), and must retain the assumption that u and e are uncorrelated – the so called "selection on *observables"* assumption.

If (i) and (ii) both hold our OLS estimates are unbiased despite the non random selection of the electoral rule, as long as we have not omitted from the *CPI* regressions any variable included in the intersection of **Z** and **X**. This is the rationale for showing that our results are robust to controlling for the colonial origin of a country and other historical variables that might also influence the selection of electoral rule.

But the linearity assumption on F and G is restrictive. For instance, it rules out an impact of the electoral rule on corruption systematically related to observables – such as historical, social or religious variables – which also influence the choice of electoral rule. The OLS estimates could be severely biased if this non-linearity is important *and* the observable covariates differ systematically across different electoral rules. Indeed, **Tables 2a** and **2b** show that some observables are systematically, if moderately, correlated with *MAJ, PLIST* and *DISMAG* in our data. And in the previous subsection, we noted that the *MAJ* binary variable is correlated with variables reflecting a country's colonial and legal history.

This second problem can be addressed, however. We can check whether the results hold up under non-parametric estimates, which are free from strong assumptions about functional form while allowing observable variables to influence selection. The gist of such estimators is that they force us to give more weight to comparisions of similar countries, where the effect of the non-linearities is unlikely to be important. The next section deals with these issues.

5. Matching estimators

Non-parametric estimators of the effects of a particular treatment in the absence of experimental data have been used in the medical sciences for some time. More recently, such matching 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 matching, 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). More general discussions about matching vs. other evaluation methods can be found in Angrist and Kreuger (1999) and Heckman, Lalonde and Smith (1999).

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 be the source for our "control" group. In our context, the choice of how to label the two groups is not self-evident. Our chosen convention gives a larger group of treated countries, however, which exploits more of our scarce data – we have 53 proportional and 30 majoritarian electoral systems countries in our sample (see further below).

Our prior is that the treatment (being non-majoritarian) *causes* more corruption. A precise interpretation of this hypothesis is the following: picking a country at random among the treated countries in our sample, and administering the treatment (i.e. giving it a non-majoritarian electoral rule), we should observe a level of corruption above the level we would observe in the absence of treatment (i.e., maintaining a strictly majoritarian electoral rule). In the labor literature, one measures this causal effect by a parameter known as the *average effect of treatment on the treated.*

In Section 4, we made three assumptions allowing us to our causal hypothesis by evaluating the t-statistic on the dummy variable MAJ in a linear regression of corruption. Two of those assumptions were spelled out at the end of the previous subsection: recursivity (or "selection on observables") and linearity. A third implicit assumption is that the electoral rule only has *direct* causal effects on corruption. For instance, we are not allowing the electoral rule to have a causal effect on income and through that channel an indirect effect on corruption. By linearity and this third "no-indirect-effect" assumption, the causal effect of the electoral rule on corruption, as defined above, is captured by the (constant) partial derivative of F with respect to MAJ. And by recursivity, this partial derivative is correctly estimated by OLS.

We now want to estimate the average effect of treatment on the treated, without any restrictions on the functional form F. Specifically, indexing our corruption measure *CPI* for treated and non-treated countries by T and C superscripts respectively, we want to estimate:

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

where subscripts denote countries and the E operator denotes expectations, conditional on the distribution of *CPI* in group T, the group with proportional elections.[10]

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 proportional elections would have, if it hypothetically had majoritarian elections. How can we exploit the data from countries with majoritarian elections to replace this unobservable counterfactual? And how can we allow for the fact that – in this non-experimental setting – the choice of the electoral rule is not likely to be random?

As in Section 4, we need to retain the recursivity (or selection on observables) assumption, also known as "conditional independence" (Rosenbaum and Rubin, 1983, Rubin 1973, 1977). Specifically, suppose that selection of countries into treatment and control groups is affected by a set of observable variables, $\mathbf{X}$, such as colonial origin or religious tradition, that also have an independent effect on corruption. To construct a control group, we then need the following central identifying assumption: conditional on $\mathbf{X}$, (mean) corruption and the choice of electoral rule are independent. In other words, no omitted or unobserved variable influences average corruption in the absence of treatment, once we have condi-

[10] Note that, even with this methodology, we have to retain the assumption that the electoral rule only has direct causal effects on corruption, ruling out other indirect effects operating through some control variables (see Heckman, 1998 for further discussion of this point).

tioned on $\mathbf{X}$:

$$E(CPI_i^C \mid i \in T, \mathbf{X}_i) - E(CPI_j^C \mid j \in C, \mathbf{X}_j) = 0, \text{ for } \mathbf{X}_i = \mathbf{X}_j. \tag{5.2}$$

This assumption allows us to replace the unobservable counterfactual in a conditional version of (5.1), writing:

$$\tau(\mathbf{X}_i) \equiv E(CPI_i^T \mid i \in T, \mathbf{X}_i) - E(CPI_j^C \mid j \in C, \mathbf{X}_j = \mathbf{X}_i) , \tag{5.3}$$

where C denotes the control (majoritarian) group. From here on, we drop the T and C superscript, since it is understood that when $i \in T$ we only observe CPI under treatment, and when $j \in C$, we only observe CPI in the absence of treatment. Our parameter of interest can thus be written as $\tau = E[\tau(\mathbf{X})]$, where the expectation is taken over the distribution of $\mathbf{X}$ in the group of treated (non-majoritarian) countries.

A non-parametric test of our central hypothesis could thus be obtained from (5.3) by a process of *matching*. The basic idea is very simple: we directly compare observations $i \in T$ with observations $j \in C$ that have similar values of $\mathbf{X}$, ideally $\mathbf{X}_i = \mathbf{X}_j$. This will rule out biased estimates due to non-linearities, e.g., heterogenous treatment effects systematically related to $\mathbf{X}$. But as $\mathbf{X}$ is multidimensional and has non-trivial distributions in T and C, finding close enough observations is very hard, particularly in a small sample like ours.

Luckily, the propensity-score literature (Rosenbaum and Rubin, 1983, 1984) shows how (5.3) can be restated on a more parsimonious form. Specifically, let $p_i = p(\mathbf{X}_i)$ be the probability of selection into treatment (i.e., non-majoritarian electoral rule), conditional on the observable variables $\mathbf{X}_i$. Furthermore, assume that $0 < p(\mathbf{X}_i) < 1$, for all $\mathbf{X}_i$. Then, we can rewrite (5.3) as:

$$\tau(p_i) \equiv E(CPI_i \mid i \in T, p_i - E(CPI_j \mid j \in C, p_i = p_j) .$$

The probability p_i is also called the propensity score. We can compute the average effect of treatment on the treated from

$$\tau = E[\tau(p_i) \mid i \in T] , \tag{5.4}$$

where the expectation is taken over the distribution of $p_i = p(\mathbf{X})$ in the treated group ($i \in T$). Estimating (5.4) is obviously much easier than estimating $E[\tau(\mathbf{X})]$ based on (5.3), 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 three alternative matching 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_i in the previous section.[11] The essence of the identifying assumption (5.2) is that we should not leave out any variable known to influence corruption. We thus include all covariates for which we found a significant effect on corruption in Section 4. The union of those variables include log income, OECD membership, openness, ethno-linguistic fractionalization, education (*LOG*(Y), *OECD*, *OPEN*, *ELF*, *EDU*), our variables for religious beliefs (*CATH, PROT, CONFU*), and for colonial origin (*COLOES, COLOFR, COLOUK, COLOTH*).[12]

Next, we want to verify that matching on the estimated propensity score, as in (5.4), indeed is comparable to matching on the full vector X, as in (5.3). To do that, we ask whether the distribution of X is balanced 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 of the 5 strata, and for each of the 12 variables in X. We cannot reject the null hypothesis that the means are equal, at the 5% confidence level, except in very few special cases.[13] When the same test is performed on the whole sets T and C, rather than within each stratum, we reject equal means for 7 out of 12 variables, namely for (*LOG*(Y), *OECD*, *ELF*, *EDU*, *CATH, COLOUK, COLOTH*) - see **Table 2b**.[14]

Before estimating the treatment effect, we also want to take care of the com-

[11] We have also estimated the propensity score with a logit. Matching on these alternative propensity scores yields similar results as those based on the probit estimation.

[12] We have also estimated our probit with a more parsimonious set of covariates. The resulting propensity score yields results similar to those repeated beow (see Persson, Tabellini and Trebbi, 2000).

[13] In the case of a few categorical variables (4 stratum-variable cells out of 60) the stratum means in the two groups are different, while the standard error in either group is zero.

[14] Results for the tests not shown in **Table 2b** as well as the probit estimates are available upon request.

mon support assumption implicit in (5.4), verifying that we indeed compute $E[\tau(p(X_i))]$ on the support of $p(X_i)$ in the group of treated (proportional) countries. For 14 countries with majoritarian elections, the estimated propensity score was lower than the lowest score among the proportional countries. These countries were thus discarded as non-comparable to any proportional country. There was no need for discarding countries at the top of the ranking.

5.3. Estimating the treatment effect

In this subsection, we estimate the treatment effect $\tau = E[\tau(p(X))]$. We can perform the matching underlying the expectation in different ways, each one corresponding to a different estimator of the treatment effect. As the precise costs and benefits of these different estimators are not well established, especially in small samples, we choose to report the results of three different methods. The formulas for these estimators and their standard errors are given in the Statistical appendix. Here, we describe their properties and report the results.[15]

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 matches 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 treatment and controls are present in every stratum. The smaller overlap in the outer bins does not bias our estimates, as long as the two groups are homogeneous in terms of the covariates (as is the case here).[16] 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 *nearest-neighbor matching* estimator. In a first step, every treated ($MAJ = 0$) country is matched with the most similar control ($MAJ = 1$)

[15] We are grateful to Andrea Ichino and Barbara Sianesi, both of whom generously shared with us their STATA programs for estimating treatment effects with methods like those considered in this section.

[16] In the $0 - 0.2$ stratum, all the controls ($MAJ = 1$) have a lower estimated propensity score than the two treated ($MAJ = 0$) countries. Imposing the common support condition, we are forced to discard this stratum since it does not have any overlapping observations.

country; i.e., the nearest match in terms of propensity score. In our case, this entails dropping 2 majoritarian countries from the control group (in addition to the 14 countries discarded to ensure a common support). This matching process yields 53 pairs, equivalent to the number of treated countries. The nearest-neighbor 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 treatment 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 – cf. Dehejia and Wahba (1998) for a similar graph. 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 many other countries in South America, which have proportional elections. Similarly, majoritarian France (propensity score 0.89) is matched with several 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).

The *radius* estimator, finally, combines features of the other two. Each treated observation is matched with all members of the control group that have an estimated propensity score within a certain distance (radius). Thus, each treated observation is potentially matched with more than one control (as in stratification), and each control is potentially used more than once in the matching process (as in nearest-neighbor matching). In our estimates below we use a radius of 0.05.

The first three columns of **Table 5** report the estimates obtained with these three methods. The highest estimate of the mean difference in corruption, namely 1.15, is given by the nearest-neighbor matching estimator. Using the stratification estimator, we obtain an estimate of 0.81, whereas the radius estimator predicts a treatment effect of 0.72. Recall that the WLS and OLS estimates of the coefficient on MAJ on the full sample in **Tables 3** and **4** were 0.60 and 0.58 (we reversed the sign of the regression coefficient to facilitate the comparison, given the definition of treatment as non-majoritarian). Our three non-parametric estimates thus confirm the previous finding, namely that non-majoritarian countries are more corrupt. Indeed, the estimated treatment effect of the electoral rule on corruption is larger than the regression estimates according to all the three estimators.[17]

[17] Note that differences between the two sets of estimates could depend on the fact that the

We also note that the standard errors are larger than the standard errors obtained in the linear regressions, so that the estimates are no longer statistically significant at usual confidence intervals. 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. Higher standard errors thus come as no surprise, particularly in such a small sample of countries. To lower the standard errors we have to make stronger functional-form assumptions. As an illustration, we also obtain a regression estimate of the (negative of the) MAJ coefficient on the 66 country sample obtained from nearest neighbor matching (which by construction has a more balanced distribution of covariates than the full sample used in Section 4). The right-hand side variables in the regression are the same as in the probit estimation. In the last column of Table 5 we estimate by WLS, weighing the control observations by the number of times they are used in the matching. The point estimate remains 0.80, close to the values in the first three columns, but the standard error is now much smaller.[18]

Finally, we should note that our labeling the proportional countries as the ones subject to treatment does matter for these results. If we reverse the labelling, we do not match on the 53 proportional countries, but instead on the 30 majoritarian countries. In this case, we obtain an average effect of treatment on the treated around zero with all the three estimators. A likely reason for the different results is that, with the reverse labelling, we exploit much fewer observations, so that small sample size becomes even more of a problem.

All in all, we conclude that – subject to the identifying assumptions stated in this section and the small sample size – the inference from our regression analysis in Section 4 do not appear fragile, even when we allow for possible specification bias due to a wrong functional form.

OLS estimator weighs covariate-specific treatment effects with variance weights whereas the matching estimators weigh them with probability weights (on this point, see e.g., Angrist and Kreuger, 1999).

[18] To further highlight the differences in estimation methods, we have also estimated the effect of MAJ on the matched sample, but now weighing all observations by the inverse of STDEV, as in the WLS estimates reported in Table 3. The point estimate of MAJ falls to 0.54, very similar to the estimated reported in Table 3, and the standard error remains small, 0.287.

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, our regression estimates 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 in a true panel data set. Unfortunately, this seems infeasible in the light of available data. The data problem concerns a lack of relevant and comparable measures of corruption over time. But it also concerns changes in the electoral rules over time; while this information is in principle available from first-hand sources, collecting it and coding it into time-variable measures corresponding to $PLIST$ and $DISMAG$ will require a non-trivial amount of work.

Future work should also further investigate the statistical robustness of our results. In particular, other non-parametric estimators than the specific matching estimators used here might strike a better balance between bias and efficiency in samples as small as ours. 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. Allowing for non-random selection and non-linearities might be particularly important in the kind of international comparisons considered here. The non-parametric methods illustrated in this paper force the researcher to give more weight to "local" comparisons among more similar countries. This might be highly appropriate if the effects of political institutions are non-linear, in the sense that they interact with other observable social or economic features.

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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[19]
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).

[19] 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).

The nearest-matching estimator
Consider matching (with replacement) on the nearest neighbor, in terms of estimated propensity scores, yielding a set of controls $j \in C$ 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:

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

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

Assume that these observations are independent and treat the weights w_j 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:

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

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:

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

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 radius estimator
The formulas for the radius estimator are identical to (A1)-(A2), but the weights $\{w_i\}$ are computed in a different way. To see this, note that under radius matching a member of the treated group $i \in T$, with propensity score p_i^T, is matched with all

non-treated observations $j \in C$ with a propensity score closer than a pre-defined distance δ ; i.e., such that $p_i^T - \delta < p_j^C < p_i^T + \delta$. (If no observation can be found within the given radius, the nearest observation is used.) Denote the number of controls matched with observation $i \in T$ by N_i^C and define the weights $w_{ij} = \frac{1}{N_i^C}$ if $j \in C$ is matched with $i \in T$, and $w_{ij} = 0$ otherwise. Then, the formula for the radius estimator can then be written

$$\begin{aligned} \tau^R &= \frac{1}{N^T}\left[\sum_{i \in T} CPI_i^T - \sum_{i \in T}\sum_{j \in C} w_{ij} CPI_j^C\right] \\ &= \frac{1}{N^T}\sum_{i \in T} CPI_i^T - \frac{1}{N^T}\sum_{j \in C} w_j CPI_j^C \end{aligned}$$

if the weights w_j are defined by $w_j = \Sigma_i w_{ij}$. As this formula is identical with (A1) up to the definition of the weights, the variance of the radius estimator is given by (A2).

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:

$$\begin{aligned} \tau^S &= \frac{1}{N^T}[\sum_b N_b^T (\sum_{i \in T_b} \frac{1}{N_b^T} CPI_i^T - \sum_{j \in C_b} \frac{1}{N_b^C} CPI_j^C)] \qquad (A3) \\ &= \frac{1}{N^T}[\sum_{i \in T} CPI_i^T - (\sum_b \frac{N_b^T}{N_b^C} \sum_{j \in C_b} CPI_j^C)], \end{aligned}$$

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:

$$\begin{aligned} \mathrm{Var}(\tau^S) &= (\frac{1}{N^T})^2 [N^T \mathrm{Var}(CPI_i^T) + \sum_b (\frac{N_b^T}{N_b^C})^2 N_b^C \mathrm{Var}(CPI_j^C)] \qquad (A4) \\ &= \frac{1}{N^T}[\mathrm{Var}(CPI_i^T) + \sum_b \frac{N_b^T}{N^T} \frac{N_b^T}{N_b^C} \mathrm{Var}(CPI_j^C)] \ . \end{aligned}$$

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:

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

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 a
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 2b
Means

	MAJ = 0	MAJ =1	p-value		MAJ = 0	MAJ =1	p-value
CPI	5.1	4.3	0.126	**PROT**	17.1	12.1	0.371
Y	10860	7218	0.057	**CATH**	46.1	21.4	0.003
POLRIGHT	2.3	3.6	0.001	**CONFU**	0.1	0.1	0.466
EDU	92	79.1	0.001	**PLIST**	0.82	0	0.000
ELF	0.2	0.4	0.001	**DISMAG**	0.79	0.15	0.000
OPEN	40.7	36.4	0.630	**INSTAB**	0.38	0.26	0.029

P denotes the p-value for a means test between the groups *MAJ* = 0 and *MAJ* = 1, the null being equal means, under the assumption of equal variances.

Table 3
Regression Estimates - 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

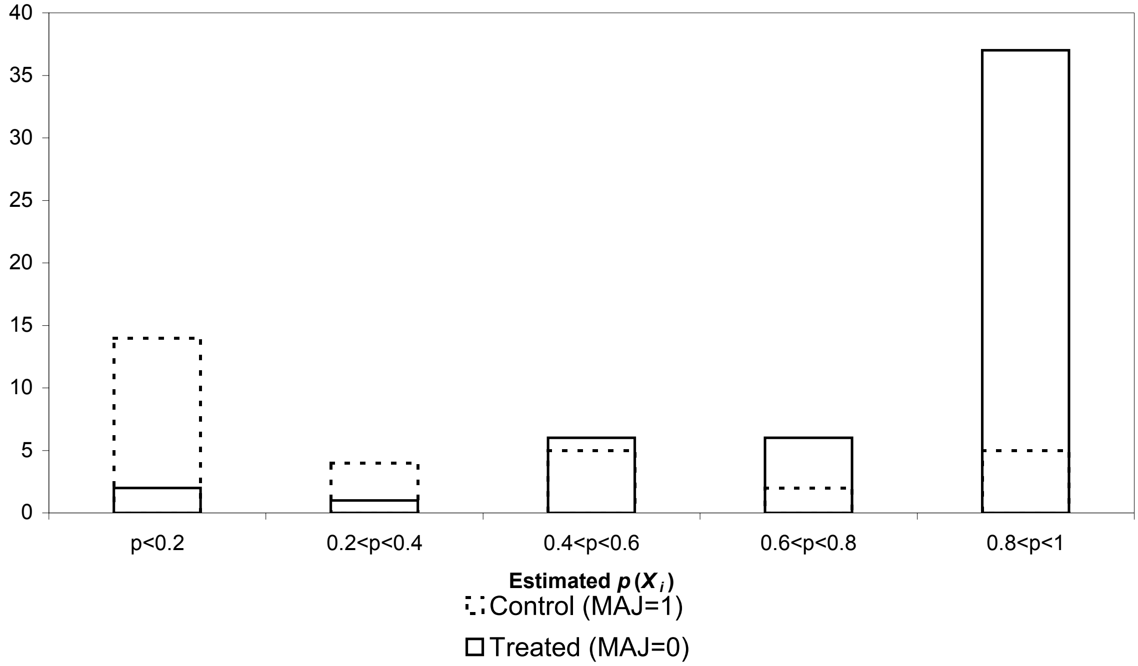
Notes: Weights are the Inverse of STDEV for CPI observations. T-statistics in parentheses. LEGAL= YES means that we are controlling for legal origin. COLONIES = YES means that we are controlling for colonial origin.

Table 4
Regression Estimates - OLS
Dependent Variable: CPI

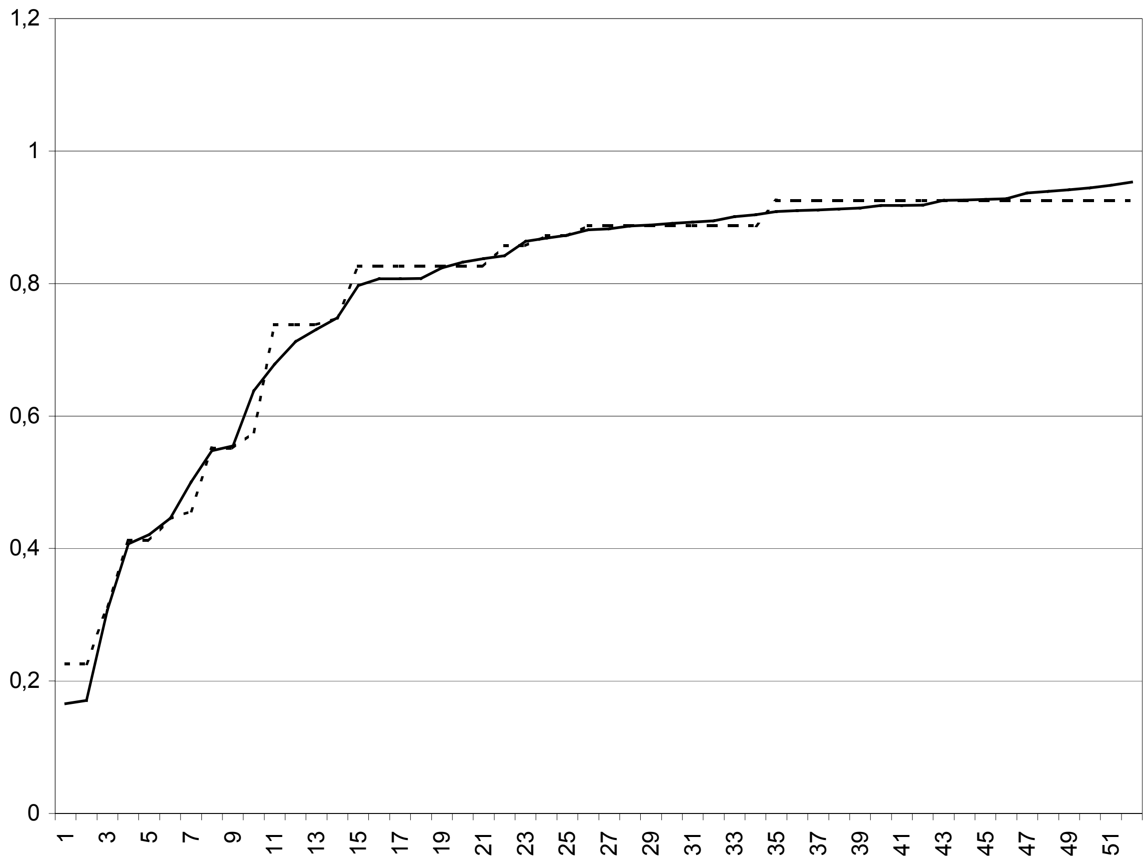
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	15.74	12.99	13.15	11.94	13.87	14.43
	(9.34)	(6.94)	(7.01)	(5.49)	(6.31)	(7.25)
LOG(Y)	-1.02	-0.82	-0.85	-0.73	-0.84	-0.95
	(-4.26)	(-3.76)	(-3.82)	(-3.14)	(-3.47)	(-4.09)
LOG(POP)	0.16	0.15	0.18	0.20	0.13	0.18
	(2.03)	(1.77)	(2.32)	(2.21)	(1.46)	(2.22)
EDU	-0.01	-0.01	-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
Estimated Propensity Score Strata for Treated and Controls



Graph 2
Propensity Score for Treated and Matched Countries



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

Table 5

Matching Estimates of Treatment Effect
Proportional Elections on CPI

Stratification	Nearest Matching	Radius Matching	Regression Matched Sample
0.81 (0.917)	1.15 (1.122)	0.72 (0.940)	0.80 (0.260)

Three first columns give treatment effects and standard errors in brackets computed as described in statistical appendix.
Last column gives estimate of the *MAJ* coefficient from a linear regression on the sample obtained by nearest-neighbor matching with the same right-hand side variables as in the Probit-estimates of the propensity score and control (*MAJ* = 1) observations weighted by the number of times they are used in the matching.