

Legislative Malfeasance and Political Accountability

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Abstract

Utilizing a unique dataset from the Italian Ministry of Justice reporting the transmission to the Chamber of Deputies of more than 3,000 requests for the removal of parliamentary immunity from deputies suspected of criminal wrongdoing, we analyze the political careers of members of the Chamber during the first eleven postwar legislatures (1948–1994). We find that judicial investigation typically did not discourage deputies from standing for reelection in Italy’s large multimember electoral districts. We also show that voters did not punish allegedly malfeasant legislators with loss of office until the last (Eleventh) legislature in our sample. To account for the dramatic change in voter behavior that occurred in the early 1990s, we examine the effects of the judiciary and the press. Our results are consistent with a theory that a vigilant and free press is a necessary condition for political accountability in democratic settings. An independent judiciary alone is ineffective in ensuring electoral accountability if the public is not informed of political malfeasance.

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One compelling justification for democracy is that public officials can be held accountable. Elected representatives who betray the public trust by extracting excessive rents are vulnerable to electoral retribution with loss of office. Perhaps surprisingly, however, evidence from a variety of countries documents that elected officials who are charged with or convicted of criminal wrongdoing are typically reelected rather than being repudiated by the electorate. This finding resonates with studies suggesting in a variety of different ways that accountability may be only imperfectly realized even in well-established democracies because voters either hold politicians responsible for things patently not under their control or because voters fail to take unresponsive or malfeasant representatives to task. In this paper, we argue that electoral retribution for allegations of criminal behavior by national legislators hinges on the dissemination of relevant information by the mass media. Without media reports that inform voters of judicial allegations that public officials have engaged in criminal activities, the electorate's response to charges of malfeasance is one of apparent indifference. We thus identify the informational environment as a critical factor that affects the ability of voters to hold politicians accountable.

We study these issues with an inquiry into those members of the Italian lower house of parliament, the Chamber of Deputies, who were subject to investigation by the judiciary for suspected malfeasance over the first eleven legislatures of the postwar era, elected in the years starting in 1948. Our dataset contains information on all persons in Italy's lower house in the eleven legislatures that sat between 1948 and 1994, as well as thousands of judicial requests to remove parliamentary immunity in order to proceed with investigations for suspected criminal wrongdoing. We also have information on all candidates listed by two of Italy's major parties, the long-dominant ruling party of Christian Democracy (DC) and the smaller government-allied Italian Socialist Party (PSI). This allows us to examine candidates as well as members. Our dataset ends in 1994 with the early election of Legislature XII under a new, mixed electoral system, shortly after amendment of the constitutional provision regulating parliamentary immunity.

The postwar Italian legislature is an especially interesting setting in which to investigate issues of electoral accountability for reasons both empirical and theoretical. Among the world's wealthy democracies, postwar Italy enjoyed the dubious distinction of ranking first on any scale of corruption of which we are aware. The ultimate outcome was that more than a third of the members of Legislature XI (1992–94) were charged with serious wrongdoing. Importantly for present purposes, Italy is the only country we know where voters turned on a whole class of allegedly corrupt

national political leaders and ejected them from public office. It thus provides a unique empirical setting in which to identify the triggers for the wholesale repudiation of a malfeasant political elite. What caused Italian voters, who had tolerated political corruption for many decades, to suddenly “throw the rascals out”? The empirical puzzle motivating this research lies with the dual observations of longstanding and widespread high level political corruption and its sudden, dramatic, and unexpected ending.

Theoretically, our study extends current research on the conditions for effective electoral accountability. Theories of accountability contend that democratic political institutions are essential to responsive government: political competition gives voters resources to enforce political control over politicians (Schumpeter 1962, Ferejohn 1986, Przeworski, Stokes, & Manin 1999). A particularly vivid illustration comes from the observation that famines occur only in non-democratic polities (Sen 1981). However, empirical evidence is surprisingly inconclusive regarding the impact of democracy on political corruption. Multiple studies report that only after many decades of established democracy are voters apparently able to exercise successful political control that results in substantially reduced levels of corruption (Treisman 2000, Montinola & Jackman 2002). A related literature investigating the impact of institutional variation within democratic polities on rent-seeking and political corruption produces contradictory findings about whether institutional features such as presidentialism or parliamentarism allows greater political control and likewise whether single-member district or proportional representation electoral systems is more effective in this regard (Chang & Golden 2006, Gerring & Thacker 2004, Kunicovà & Rose-Ackerman 2005, Lederman, Loayza, & Soares 2005, Persson & Tabellini 2003, Manzetti & Wilson 2007). Our understanding of why and how democratic political institutions enforce political accountability is thus still radically incomplete.

In this study, we direct attention to another factor that may explain why democratic institutions do not necessarily promote political accountability: the quantity of information available to the electorate. Specifically, we attribute the structural break in the response by Italian voters to political corruption that occurred in the early 1990s to the increase in information that grew out of the Clean Hands operation, when judicial inquiries originating in Milan ultimately implicated thousands of politicians including five former prime ministers. These investigations dominated the news in Italy. Even in a democratic polity, there are variations in the extent to which the press publicizes evidence of wrongdoing by elected officials. Sometimes information is deliberately

withheld by politicians in efforts to protect their careers. At other times, the press may not have access to complete information, perhaps because it is not collected or assembled in a form useful to voters. In Brazil, Ferraz & Finan (2008) show that once information was assembled and released documenting expenditure misappropriation by local governments, voters retaliate by significantly reducing their support of those incumbent mayors most likely to have been culpable. Similarly, Reinikka & Svensson (2005) report that in Uganda, where most money allocated by the federal government to schools was lost to corruption before ever reaching the classroom, parents successfully and substantially reduced the amounts of monies embezzled once local radio stations publicized the extent and location of corruption. Finally, Gentzkow, Glaeser, & Goldin (2004) argue that historically the growth of a nonpartisan press played a vital role in uncovering and monitoring corruption in the United States. Other micro-level studies whose findings corroborate the general intuition of the importance of the press in enforcing political accountability include Besley & Burgess (2002) and Cole, Healy, & Weker (2008). These are complemented by a growing body of cross-national literature statistically documenting a relationship between press freedom and corruption reduction (Adserà, Boix, & Payne 2003, Brunetti & Weder 2003, Chowdhury 2004, Freille, Haque, & Kneller 2007).

Our study also documents that a sudden significant increase in information made available by the press to the public was followed by a substantial change in voter behavior. Our findings thus intersect with the research that shows the importance of the press in providing the information permitting voters to coordinate electoral retaliation for ongoing political corruption. In addition, our results are consistent with work by McMillan & Zoido (2004) that documents that corrupt politicians fear a free press more than they do judicial inquiry.

Our analysis proceeds as follow. To guard against potential selection bias, we first explore whether a charge of malfeasance discourages a deputy's party from including his name on the party list in the subsequent elections and whether, once relisted, alleged malfeasance discourages voters from reelecting the incumbent. We find no evidence that charges of malfeasance decrease the likelihood that a deputy will be listed as a candidate in the subsequent election. We then study voter response to allegations of criminal wrongdoing on the part of national legislators over the eleven legislative periods covered by our data. We find that only in the Eleventh Legislature, which sat from 1992 to 1994, is a charge of serious malfeasance negatively associated with the probability of reelection. In the other ten postwar legislatures, the effect is either statistically insignificant, or

— in the First Legislature after World War II, that elected in 1948 — significant and positive. Our aggregate results across all legislative periods show that when a deputy is charged with a potentially serious crime, he nonetheless enjoys greater than even reelection rates. Over the eleven periods, fifty-one percent of charged deputies are reelected, compared with 58 percent of the putatively honest.

In the third section of our paper, we investigate the two legislatures when Italian electoral behavior appears aberrant. In Legislature I, voters reelected deputies who were charged with malfeasance at significantly higher rates than deputies who were not charged. We investigate whether leftwing deputies in the First Legislature were politically targeted by a still heavily fascist judiciary, and produce evidence showing they were. Following that, we study the unusual numbers of allegations of wrongdoing that were lodged in the final legislative period of our analysis. We show that voters punished deputies charged with malfeasance during the Eleventh Legislature by failing to reelect them. We also show that voters punished deputies who were not (yet) charged with wrongdoing but whose partisan, political, and individual characteristics suggested that they would have been charged had the legislature not been dissolved early.

To interpret these findings about the sudden change in voter behavior with the elections to the Twelfth Legislature, we present evidence documenting a dramatic increase in the number of newspaper reports concerning political corruption by the national press. This evidence is consistent with a theory of political accountability that sees the informational environment as crucial in facilitating electoral control. If voters do not have the relevant information about the behavior of politicians or if the information is not salient or credible, perhaps because it is disseminated only by local newspapers with known partisan leanings, voters will not use it when deciding whether to reelect a corrupt, malfeasant, or unresponsive incumbent. Changes in public opinion arise when the informational environment undergoes significant alteration (Zaller 1992).

Political Corruption among Italian Deputies: Some Background

Over the forty-five years that comprise our dataset, and that Italians call “the First Republic,” Italy elected eleven legislatures and 6,800 deputies.¹ During these eleven legislatures, the Ministry of Justice transmitted to parliament 3,149 official documents requesting the removal of parliamentary immunity from members of the lower house in order to proceed with investigations into possible

criminal wrongdoing — or more than the number of individuals who served as deputies. These requests, known as *richieste di autorizzazioni a procedere* (RAP), form the basic information that we analyze.² Many deputies were charged more than once, either within the life of the same legislature or in different legislatures, so that in total, 1,588 individual deputies (or 54 percent of all deputies) were charged at least once during the eleven legislatures we study.³ As we document shortly, however, 256 of these deputies were subject to allegations of malfeasance during the Eleventh Legislature, making charges especially prevalent in that period.

Many RAP were relatively trivial, involving allegations that arise naturally in the life of a professional politician. Following a study by Luca Ricolfi (1993) of RAP lodged during Legislature XI, we class charges as either “minor” (which we also call “opinion” crimes) or “major” (the latter we interchangeably label “serious,” “malfeasance,” or “criminality”). We class as minor all charges involving opinion crimes because these are especially likely to arise during the process of political campaigning. This category includes libel, slander, defamation, and related items, including anything related to fascist activities.⁴ We class as serious all other crimes. These involve a variety of allegations running from the mundane to the genuinely severe, extending even to murder. A great many involve explicit allegations of political corruption, involvement in illegal party financing, or abuse of office. In the Eleventh Legislature, for instance, the modal allegation was “violazione norme finanziamento pubblico dei partiti,” (violation of the regulations on public financing of political parties) with “abuso di ufficio” (abuse of office) and “corruzione per un atto contrario ai doveri di ufficio” (corruption for an act contrary to official duties) running close behind (Ricolfi 1993, p. 24, table 3). In cases where kickbacks are received, the charges do not contain information on how the funds were then used, so we cannot distinguish activities for personal gain from those conducted with political or partisan goals in mind.

But as others have argued (Golden & Chang 2001), precisely because it was so difficult to strip parliamentarians of their immunity — doing so required a majority vote by the relevant chamber — and because parliamentarians were aware that they were likely to be protected by their colleagues from prosecution, almost any criminal activity could involve elements of abuse of office. Unlike ordinary citizens, deputies and senators were largely protected from the legal consequences of breaking the law. Chronic difficulties in successfully prosecuting members of parliament could easily have encouraged wrongdoing on their part. In such an environment, any classification that interprets “corruption” simply as pocketing public monies or accepting illegal campaign contribu-

tions is excessively narrow.⁵ In the analysis to follow, we restrict our work to major (non-opinion) allegations, since these are a more reasonable way to investigate how voters respond to charges of criminality than including phenomena such as libel and slander.

Before we proceed, we first address the issue of measurement validity. Obviously, RAP reflect allegations of wrongdoing, not convictions. As far as we are aware, data on convictions are not available. We know that they are few, and that most deputies charged with wrongdoing over the postwar era were never convicted.⁶ Given that, how do we know the RAP accurately capture malfeasance among elected representatives rather than judicial activism or merely judicial prejudice?⁷ Do we have any way to estimate the “real” incidence of political corruption? If the RAP are manufactured by a politicized judiciary, they are not likely to be credible to voters and the issue of accountability is moot.

These are important questions, to which we offer a variety of answers. No single answer will satisfy all, but we hope that taken together they cumulatively reassure skeptical readers that our data on the number of deputies accused of wrongdoing: (i) do not contain systematic biases; and (ii) are appropriate for the purposes for which we use them. Our answers to these questions are fourfold. First, if charges against deputies are mainly a function of judicial zeal, not actual malfeasance, we might observe a high correlation between the two types of charges — charges for minor and serious crimes — across electoral districts and legislative periods.⁸ Where charges mainly reflect judicial activism, the judiciary might be expected to manufacture as many as possible, hardly discriminating between the serious and the trivial. The correlation coefficient between the two classes of charges over electoral districts and legislatures is $+0.55$. While we cannot know just how high it would be if charges were mainly a function of judicial zeal, graphical evidence (see Figure 1, below) also documents that the two types of charges are not temporally coincident.

Second, we incorporate a measure of judicial activism and effectiveness into our statistical analysis as a covariate. The measure of judicial effectiveness we use is the annual average length of civil trials in the lower courts, a useful measure in a country known for its slow-moving judicial procedures.⁹ Including judicial effectiveness does not fundamentally affect our statistical results, and provides some reassurance that the RAP capture genuine malfeasance.

Third, other research (Golden & Picci 2006) finds a strong relationship between the number of deputies named in RAP during the Eleventh Legislature and an objective measure of corruption that is based on the extent of “missing infrastructure” in public works as of the 1990s. The measure

of corruption in public works used in that study is an index based on the difference between amounts of physically existing infrastructure (kilometers of roads, numbers of hospital beds, power plants, etc.) and the cumulative amounts of government money spent to construct new infrastructure. The index is a more precise measure of actual wrongdoing by politicians, since it exposes the diversion of public funds. That it is significantly correlated with the number of deputies incriminated by RAP in the Eleventh Legislature provides additional reassurance that RAP comprise a reliable proxy for overall legislative malfeasance, especially that involving corruption.

Finally, our purpose here is to study how judicial investigations and public allegations of wrongdoing affect the political careers of elected officials. Due to an absence of data, we necessarily remain agnostic about whether officials are guilty, and focus instead on whether allegations have electoral effects. The information that a RAP has been lodged against a deputy is a matter of public record. Our study aims to assess whether the electorate, once the judiciary officially implicates a deputy in wrongdoing, exacts electoral retribution. We study the political credibility of the RAP separately when we examine the First and Eleventh Legislatures.

Description of the Data

We turn now to an initial description of our data regarding charges of malfeasance. In Figure 1, we present the proportions of deputies charged with opinion crimes and with serious malfeasance for each legislature. Smaller proportions of deputies are charged with minor (opinion) crimes than with serious malfeasance for every legislature except the First. A more important characteristic is that the numbers of charges for serious malfeasance are not uniform across legislative periods. An especially large number are lodged in Legislature XI, when 35 percent of deputies are named in RAP for serious wrongdoing. The extraordinary number of judicial investigations during Legislature XI demands attention to whether the overall results are driven by this legislature. In the analysis that we report below, we document that overall results are driven by the final legislature and present evidence of a period break.

[Figure 1 about here]

We next examine patterns of charges for serious crimes lodged against legislators affiliated with the governing parties and against those representing parties of the opposition. Figure 2 shows the numbers of deputies who are charged divided into government and opposition, where “government”

is defined as those deputies elected to any party holding ministerial posts at any time during the life of the legislature.¹⁰ The data show a reversal in the partisan identity of the accused over the postwar era. In the first part of the period, parliamentarians affiliated with opposition parties were much more likely than those affiliated with governing parties to be incriminated by the judiciary. By the Sixth Legislature (seated in 1972), the pattern had switched, and deputies in governing parties — dominated by Christian Democracy and including by that time three small centrist parties as well as the PSI — were more frequently charged. We also compared the proportions (not just absolute numbers) of deputies charged by government and opposition and found a similar pattern. These data suggest that we will want to control for the partisan identity of the accused in examining the effects of charges of malfeasance on incumbency.

[Figure 2 about here]

Even without losing office, in an open-list system of proportional representation deputies may lose individual (preference) votes because of electoral retribution for alleged criminality. However, our data show that deputies who are charged with serious criminal activities see increases of 14 percent in the number of individual preference votes they receive in the election following the RAP. Deputies who are not charged with illegal activities typically gain 15 percent in the number of preference votes from one election to the next. The difference between the two classes of deputies is not statistically significant.¹¹ Vote gains by incumbents presumably reflect successful cultivation of political clienteles as well as constituency service. Preference votes are politically important both because they order the candidates who are seated in parliament from each multimember district (where the number of party list votes determines the seats allocated each party) and because they are used within parties to allocate resources and influence.

Over the eleven legislatures we study, an average of 57 percent of Italian deputies gain reelection into the subsequent legislature. Despite the commonly held view that Italy's "First Republic" enjoyed an unusually entrenched political elite, this is not a comparatively high reelection rate for incumbent legislators. A cross-national analysis shows that in 25 democratic countries over 116 elections held between 1979 and 1994, the average proportion of incumbents returned to the lower house is 68 percent, and Italy ranks in the middle of 25 countries (Matland & Studlar 2004, p. 92, table 1). Thus Italy, during the period we examine, does not appear to be a country with an unusually high (or low) incumbency advantage.

What is the political fate of those charged with malfeasance? Figure 3 shows the proportion of deputies in each legislature who are reelected in the subsequent period, separating deputies into those against whom RAP for serious crimes are lodged and all other deputies. Over the forty years that we study, the average reelection rate for those investigated by the judiciary for serious malfeasance is 51 percent, whereas it is 58 percent for other deputies. All national legislators have more than a 50 percent chance of serving again in Italy's lower house regardless of allegations of malfeasance, although those subject to allegations of serious offenses see a somewhat reduced and statistically significant probability of retaining public office.

[Figure 3 about here]

Effects of Charges on the Probability of Relisting of Incumbents

We are interested in identifying whether voters hold legislators accountable for wrongdoing. To study this, we analyze the probability of defeat for an incumbent deputy against whom a RAP is lodged compared with his uninvestigated peers. But if members of parliament against whom allegations of criminality are lodged fear losing office, they may decide not to run in the subsequent election rather than face defeat. Alternatively their party may decide not to relist the charged deputy on the ballot because of electoral concerns. If legislators who are more likely to be defeated because of RAP are not relisted, our attempts to estimate the effect of charges of malfeasance may be biased: those who chose to stand again may do so precisely because they enjoy a greater probability of being reelected than their charged peers who do not appear on the ballot.

A solution to the selection bias problem is to analyze what in the U.S. context are referred to as "strategic retirements" and reelections in a unified framework (as in Kiewiet & Zeng 1993). We are unable to do this because, while we have data on every member of the lower house in the postwar era, we have data on all candidates for only two parties, Christian Democracy and the Socialists.¹² As a result, we separate our analysis of retirements from reelection, confining the former to these two parties. Legislators affiliated with these two parties comprise approximately 43 percent of the 1,191 deputies charged with serious malfeasance over the eleven legislative periods that we study. Hence, even limiting our investigation to these two parties still captures a large part of the phenomenon of concern.¹³

In this section, we document empirically that, for deputies in the DC and PSI, there is no

statistically conclusive evidence of “strategic retirements” by those against whom RAP are lodged. We interpret this as evidence that Italian deputies do not expect to face electoral defeat as a consequence of judicial inquiry, and that therefore our subsequent results are not driven by selection bias. Our results are in direct contrast with those reported for the U.S., where legislators involved in scandal tend to retire rather than face electoral defeat (Kiewiet & Zeng 1993).

Failure to reappear on the ballot after serving in the Chamber occurs frequently for Italian legislators affiliated with the DC and the PSI during the postwar era. Of the 3,615 deputies in the Chamber affiliated with these two parties during the first ten legislatures, 418 persons, or about one in eight, was not listed for subsequent reelection. An average of 13 percent of DC and PSI legislators do not appear on their party’s ballot after any given legislature, with a high of 19 percent failing to relist out of the Sixth Legislature.¹⁴

Bivariate analysis finds that deputies in these two parties who are charged with serious wrongdoing are not relisted at a rate of 13.2 percent compared to a non-charged peer rate of 12.9 percent ($p = 0.833$).¹⁵ Lacking any other information, this simple comparison suggests that deputies charged with wrongdoing are as likely as other incumbents to see their names on their party’s ballot for election into the next legislature.

To assess potential confounding, we estimate a multiple variable model with appropriate covariates. We ground this model in the literature on career decisions in the U.S. House (Kiewiet & Zeng 1993, Groseclose & Krehbiel 1994, Hall & Van Houweling 1995, Moore & Hibbing 1998, Theriault 1998), in a study of legislative careers in Italy (Cotta, Mastropaolo, & Verzichelli 2000), and on the assumption that parties relist incumbents for reasons that are both politically strategic and based on demographic attributes. Our dependent variable, *RELIST*, is coded 1 if a deputy relists and 0 otherwise (but did not die in office or is not seeking higher public office). Age should be related to the probability of relisting, and we include both a linear (*AGE*) and a quadratic term (*AGESQ*) to pick up possible nonlinearity. We measure age in years at the beginning of the legislative period. We also control for cumulative tenure in office (*SENIORITY*)¹⁶, gender (*FEMALE*), outside professional options (measured by the professional background of the deputy, labeled *JOB*), electoral uncertainty (*UNCERTAINTY*), party leadership positions (*PARTY ELITE*), and whether the legislator’s party is currently in government (*RULING PARTY*). We measure professional background with a dummy variable coded 1 if the legislator had non-legal, non-political and high status employment before becoming a deputy. Following Chang (2005), we measure electoral

uncertainty as the inverse of the ratio of the incumbent's preference votes in the last election to the minimum number of votes needed to be seated. Hence, higher numbers indicate greater uncertainty. We measure *PARTY ELITE* as deputies who are members of their party's executive or central committee at any time during the postwar period. We assume that legislators who are older, who have served more previous terms, who had relatively good jobs prior to entering parliament, who feel less electorally secure, who are not members of elite party bodies, and whose party is not currently in government are less likely to be relisted. Results are presented in Model 1 of Table 1.

[Table 1 about here]

The effect of a RAP on the decision to relist Christian Democratic and Socialist deputies is estimated to be one-third the size of its standard error while coefficients on control variables behave as expected. Increasing age is associated with an increasing propensity to not relist. Also decreasing the propensity to be listed on the ballot at statistically significant levels is being female, having served more previous terms, and electoral uncertainty. Being a member of an elite party body or being in the ruling coalition is associated with a statistically significant increasing propensity to be relisted. Running the model by legislative period, in no legislature are charges of malfeasance associated with an decreased likelihood of relisting (results not presented). Figure 4 documents the almost complete overlap of 95 percent confidence bars for DC and PSI deputies who are charged with serious malfeasance (light lines) with their peers who are not charged (dark lines). There is no substantive or statistical difference in the expected rates of being relisted for these two groups of deputies.

[Figure 4 about here]

We interpret these results as evidence that incumbents and their parties do not fear voter retaliation when charged with criminality. Deputies who are charged are not more likely than deputies who are not named by the judiciary to withdraw their names or have their names withheld from their party list at the subsequent election.

Effects of Charges on the Probability of Reelection

Results reported in the previous section document that Christian Democratic and Socialist deputies charged with serious wrongdoing are relisted at rates that are statistically indistinguishable from

their non-charged peers, suggesting no selection bias in our sample. We now assess the electoral consequences of charges of malfeasance. We ask whether legislators charged with criminal wrongdoing are more or less likely to win reelection than their peers who are not charged. Our analysis here proceeds in three steps: (1) a survival analysis of all eleven legislative periods together; (2) logitistic regressions of each legislative period separately; and (3) non-parametric matching analyses of each legislative period separately.

Our pooled survival analysis uses a grouped duration approach to model the underlying data generating process. Our data take the form of cross-sectional (individual legislator) time-series (eleven postwar Italian legislatures) with a binary dependent variable. The unit of analysis is the incumbent in each legislature, and the binary dependent variable, *REELECT*, takes a value of 1 if the incumbent wins reelection and holds office in the next legislative period and 0 otherwise.¹⁷ Our analysis proceeds in two steps. First, we include in our regression model individual-level covariates to test the effects of legislators' characteristics on their reelection probability. Then we expand the model to incorporate district-level and legislative period variables to examine whether these exert influence on individuals' electoral outcomes.¹⁸

The measure of legislative wrongdoing that we use is *MAJOR*, which indicates that a deputy is charged with a major offense rather than the kind of trivial (opinion) charge discussed earlier. This variable takes the value of 1 if the incumbent is charged in a given legislature and 0 if he is not. We include potentially relevant characteristics of individual deputies that may affect reelection propensities as controls, some of which we have already used in our evaluation of strategic retirements. First, it is likely that deputies who control more political resources are more likely to get reelected. To capture this, we incorporate information on deputies regarding (1) the total number of previous legislatures served (*SENIORITY*); (2) whether the deputy belongs to a governing or an opposition party (*RULING PARTY*); and (3) whether the deputy is an elite legislator or a backbencher. For the latter, we use data on membership in the any of the parties' various executive bodies (*PARTY ELITE*). We expect that more terms in office, being a member of a governing party, and being an elite legislator equips a deputy with more resources, thereby increasing the probability of his reelection. We also include the (logged) number of individual preference votes (*PREFERENCE*) the deputy received in the election to the legislature in which he sits and for which we assess the impact of *MAJOR*. By considering the preference votes received in what was effectively the election prior to standing for reelection, we incorporate into the model the size of the incumbent's

preexisting clientele. Deputies with larger numbers of preference votes may enjoy more protection from electoral retaliation if preference votes signal a client base that is loyal mainly on the basis of material inducements or ideological commitment. Either suggests an electorate that may be unresponsive to new information regarding the moral rectitude of the incumbent.

Results of this specification are reported in Table 1 under Model 2. The coefficient on the RAP variable is significant and negative, indicating that members of the Italian legislature are less likely to win reelection if charged with malfeasance. However, the size of the effect is modest at best: the results suggest that a judicial investigation for a potentially serious offense decreases a legislator's probability of reelection by roughly 5 percentage points. Holding other variables at their mean, the predicted probability of winning reelection for tainted legislators is estimated to be 52.9 percent compared with a 58 percent reelection rate for their non-charged counterparts. Reelection depends more on being a member of a party elite body and establishing a larger clientele (the marginal effect of *ELITE* and *PREFERENCE* is 14.5 and 11.6 percent, respectively). Allegedly criminal legislators still enjoy greater than even reelection rates.

We now incorporate some geographically and temporally variable characteristics of the electoral environment into the analysis. A large literature suggests that macroeconomic conditions affect voter behavior. We control for the (log of the) annual average growth rate during each legislative period (*GROWTH*). It is also likely that in contexts of higher electoral volatility (*VOLATILITY*), incumbents are likely to suffer greater loss of office. We measure volatility v for each electoral district and election using the standard index (Pedersen 1979), which captures net change in electoral behavior across parties from one election to the next.¹⁹

A potential problem with using judicial charges as proxies for malfeasance is that charges could reflect judicial activism rather than the frequency of genuinely corrupt or illegal activities. To separate out the possible effects of judicial activism, we include the average length of judicial processes in the lower courts as a control variable (*JUDICIAL*).²⁰ We aggregate the measure from Italy's 29 judicial districts to its 32 electoral districts. This variable captures the relative efficiency of a notoriously highly variable judiciary; we believe it is reasonable to assume that judicial efficiency is tightly linked to the degree of overall judicial activism. Because the data on judicial efficiency are available only from 1970 onwards, however, including this variable requires dropping from the model the first four postwar legislatures.

Results of the expanded regression specifications that include contextual factors are reported

in Model 3 of Table 1. The specification of Model 3 does not substantively change the estimated effect of a major charge. The other coefficients suggest that party elites, deputies affiliated with ruling parties, and deputies who had received more individual preference votes in the prior election are more likely to be reelected than others. Reelection probabilities are enhanced where net electoral volatility in the district is lower and when economic growth is higher. Total terms in office, *SENIORITY*, is negatively associated with reelection probabilities, holding all else equal.

Recall from Figure 1 that the proportion of deputies charged varies greatly by legislative period, and in particular that RAP rose drastically and reelection rates fell in the Eleventh Legislature. This suggests the importance of examining the potential impact of RAP on a period-by-period basis to complement the pooled analysis just reported. We do so using two different statistical techniques — logistic regression and a non-parametric matching technique — and we perform each type of analysis by legislative period. Our logistic regression results are presented in Table 2.

[Table 2 about here]

Results from almost all of the eleven legislative periods confirm the importance of assembling a large client base by receiving a high number of preference votes (*PREFERENCE*) as well as elite status (*PARTY ELITE*) in the party apparatus for reelection probabilities. These two variables are statistically significant and positive in virtually all legislatures. We interpret this as documenting the importance of control of personal and party resources for retaining public office. Note, however, that party elites no longer enjoy reelection advantages starting in Legislature IX, and that deputies in the parties of government — whose reelection prospects are usually enhanced by their political affiliation — become significantly more likely to lose office in the final legislative period. We interpret this as evidence that the electorate finally turned on the parties that had governed Italy uninterruptedly since the end of World War II. District level electoral volatility and judicial efficiency do not exhibit consistent or statistically significant effects on reelection probabilities across the various legislative periods.

Our key independent variable, *MAJOR*, emerges as statistically significant in only two of the eleven legislatures, the First and the Eleventh. In Legislature I, being charged with a potentially serious offense is associated with a 24 percentage point *improvement* in the predicted probability of reelection, holding all other variables at their means. Only at the end of Italy's First Republic is a charge of serious criminal wrongdoing associated with a reduction in the probability of reelection. In

1994, incumbents who had been charged experience a 9 percentage point reduction in the predicted probability of reelection compared with their counterparts not named in RAP, holding all other variables at their means. In all other legislatures, our logit estimates indicate that allegations of criminality fail to affect voting outcomes at levels that are statistically significant.

As a robustness check of these results, we also estimate the effects of a malfeasance charge on reelection using a non-parametric matching technique (described in Abadie, Drukker, Herr, & Imbens 2004). Our data provide an ideal setting for a matching procedure. We have a binary observational treatment variable (charge/not charge) and a set of covariates that we think are related both to treatment and outcome. Among other advantages of matching is that we do not have to assume the probit or logit functional form, nor do we need to assume that the covariates are additive rather than interactive. Within each legislative period, we select deputies who look as similar to each other as possible on the various characteristics that we use as control variables in the logit specification just reported — affiliation with a party of government, member of an elite party body, number of preference votes received in the election, and legislative seniority — but who vary on whether a RAP was lodged against them. We then identify the effect on reelection between matched deputies within each legislature who were charged and their counterparts who were not charged.

The results of the legislature-by-legislature matching analysis are consistent with those reported for the logit models. The effect of being investigated for possible criminal activity in the First Legislature is estimated to increase the probability of reelection by 20 points, with a 95 percent confidence interval that does not include 0. Effects in all other legislatures are not significant at $p < .05$; the closest is the estimated effect in Legislature XI, where we find a decrease in reelection probability of 7.6 percentage points ($p = .059$.) We display the results in Figure 5. Using a matching procedure corroborates that deputies who are charged by the judiciary with major crimes do not suffer electorally in most legislatures, and in the First Legislature that criminal charges provide an electoral boost in the subsequent election.

[Figure 5 about here]

Why Did Legislators Charged With Wrongdoing Get Reelected after Legislature I?

To account for the finding that a RAP in the First Legislature enhances reelection probabilities, we hypothesize that the judiciary, not fully purged of those sympathetic to Benito Mussolini's failed regime, targeted leftwing incumbents. To test this, we model the likelihood of being charged with either a minor or major charge in Legislature I as a function of the legislator's party using dummy variables for deputies affiliated with the DC, the PCI, or the PSI.²¹ Our hypothesis is that deputies on the left were more often the target of a still heavily fascist and strongly anti-communist and socialist judiciary.

Results reported in Table 3 corroborate the hypothesis. The coefficient estimates for the two leftwing parties are positive and statistically significant whereas the coefficient for Christian Democrats indicates a lower probability of being charged. The coefficients indicate that the average Christian Democrat has a 6 percent predicted probability of being charged, whereas a Socialist incumbent has a 37 percent predicted probability of being charged and a Communist a 51 percent predicted probability. The data suggest that leftwing incumbents were either massively more prone to illegal activities than their fellow deputies or that the judiciary was not an impartial enforcer of the law in the First Legislature. A politically unreformed judiciary seems to us the more likely explanation.

[Table 3 about here]

We infer, therefore, that the positive coefficient in Table 2 for major charges in the First Legislature is spurious. Incrimination by a still-fascist judiciary was not the product of actual malfeasance by legislators. Instead, the judiciary targeted deputies of the left. In fact, in the early postwar legislatures incumbents from left-wing parties were some of the most popular politicians in the country because of their reputations for having led the anti-fascist Resistance movement. When we run the same model reported in Table 2 for Legislature I but exclude incumbents associated with the PSI and PCI, the coefficient size is quartered and statistical significance lost.²² This suggests that voters were not responding positively to corrupt representatives. Rather, a fascist judiciary spuriously charged incumbents with the greatest probability of reelection.

Why Did Legislators Charged with Wrongdoing Not Get Reelected from Legislature XI?

Our finding that charges of malfeasance have significant negative effects on the probability of reelection only in the Eleventh Legislature raises the question of whether some uncontrolled covariate is at work in that legislative period. To investigate if features of Legislature XI are genuinely unusual and not within a reasonable range of variation, we implement a nonparametric bootstrap analysis. We randomly draw with replacement from our pool of deputies 1000 pseudo-legislatures of 620 deputies each, ignoring the actual legislative period of each deputy in the resampling, and we calculate statistics on each for comparison to statistics from the actual Eleventh Legislature. If that legislature indeed represents a structural break, we should find statistics calculated from Legislature XI reside in the tails of a distribution of such statistics from our 1000 pseudo-legislatures.²³

Figure 6 presents results for two statistics of interest: the reelection rate and the logit coefficient on the major malfeasance charge variable. It is clear that the reelection rate for incumbents from Legislature XI to Legislature XII (when only 25.3 percent of deputies were reelected) represents a structural break with earlier reelection rates despite the density distribution including deputies from Legislature XI in the resampling.

[Figure 6 about here]

The second frame of Figure 6 presents the distribution of logit coefficients estimated on each pseudo-legislature with the specification of Table 2, Column 1.²⁴ While the coefficient from the actual Eleventh Legislature is not as clearly an outlier as the reelection rate, it does reside in the punitive tail, between the first and fifth empirical percentiles. This suggests that the relationship of RAP to reelection probabilities in Legislature XI represents a break from the postwar period.²⁵

These findings suggest that voters' behavior underwent a substantively important change when they went to the polls in 1994. Not only did voters throw the rascals out — they threw out sitting deputies altogether. However, Legislature XI sat for only two years. During those two years, more than a third of the lower house was named in RAP. Like Ricolfi (1993), we now ask what would have happened if the legislature had not been dissolved and early elections called. How many more deputies might have been named in RAP?

To estimate whom else in Legislature XI might have been charged had the legislature continued and the pattern of charges followed that established during the first two years, we use a classification

tree method.²⁶ Classification trees take a categorical dependent variable and explanatory variables (numeric or categorical) and make recursive binary partitions to most efficiently explain variation in the dependent variable.²⁷ We are solely interested here in predicting who is likely to be charged, given what we know of the characteristics of those who were actually charged over the existing two year legislative period. We need not assume that these characteristics are “causal.” Instead, we want to use the available information to predict who else would have been charged had the legislature sat in session longer.

We model charges of major malfeasance for deputies in Legislature XI with explanatory variables seniority, ruling party, judicial activism, and gender. The first partition splits deputies on ruling party: those not in the ruling parties are estimated to have a 22 percent probability of being charged. The tree makes further splits on the gender and judicial activism variables for ruling party members, and returns for each deputy a probability that s/he is charged.²⁸ We then use this probability to estimate the likelihood that uncharged incumbents would have been charged had the legislature been longer-lived. Our model indicates that 108 deputies who were not charged, or 27.3 percent of the nominally “innocent,” had a greater than 50 percent probability of being charged. If we add these 108 to the 222 actually charged, we estimate that the Eleventh Legislature would have had 52 percent of its seated deputies charged had it sat indefinitely.

We next add the estimated charge probability, *CHARGEABILITY*, as an explanatory variable to the logit model reported in column 11, Table 2 and present the results in Table 4. Even controlling for ruling party affiliation and actual major charges, the results provide evidence that voters turned against deputies who *looked like* corrupt deputies. The coefficient on the charge probability is large, negative, and substantively significant: the model presented in Table 4 predicts a mean reelection probability for the actual deputies of 25.2 percent.²⁹ When we disturb each deputy’s charge probability by one standard deviation (16 percentage points), the model predicts a mean reelection probability of 20.3 percent.³⁰ More guilty-looking deputies experience lower reelection probabilities. We take this as evidence that voters acted to overturn the corrupt equilibrium that had existed since the 1950s: in 1994, they voted out not just those tainted by requests for the removal of parliamentary immunity but also incumbents who looked like they were corrupt even though the judiciary had not (yet) assembled enough evidence to lodge a RAP against them.

[Table 4 about here]

Interpretation of the Results

The core result of our empirical analysis is that judicial allegations of criminal malfeasance negatively affect reelection prospects for Italian national legislators in only a single legislature, the Eleventh. Why do we find that voters display apparent indifference to charges of malfeasance for decades and then turn on not only allegedly corrupt incumbents but also those who look like they might be charged, removing them from office in a single election?

The shift in electoral behavior with the elections to the Twelfth Legislature followed immediately on dramatic changes in the information environment due to the “Clean Hands” investigations that began in 1992. Such investigations stem back at least to 1974 and a series of bank collapses in Italy, Switzerland, and New York linked to the financier Michele Sindona. They were followed by other banking and political exposés, including the P2 case, when it became public that many important Italian politicians belonged to a secret Masonic organization whose goals could plausibly be interpreted as involving subversion of the Italian constitution (Ferraresi 1992). In these earlier cases, small numbers of magistrates, flanked by a handful of journalists, were usually at the center of activity, occasionally receiving help from the opposition parties, especially those to the left of the PCI (De Luca & Giustolisi 1993).

In this earlier period, a small number of newspapers, often local in readership — *La Voce della Campania*, for instance, a Neapolitan monthly — had repeatedly tried to investigate political corruption (Bechis 1994, p. 200). Efforts to expose networks of political corruption that led to the highest echelons of power were inevitably frustrated, however. The prosecutors were blocked, routinely defamed in the press, sometimes transferred to different jurisdictions without their cases, and the newspapers that publicized the investigations were sued (see De Luca & Giustolisi (1993, p. 103), Bechis (1994, p. 200), Gilbert (1995, p. 128)). One of the most prominent Milanese public prosecutors eventually involved in Tangentopoli, as the Clean Hands investigations were known in Italian, subsequently claimed that it would have been possible to expose the far-reaching political corruption that only came out in 1993 and 1994 as much as ten years earlier if some of his investigations in the 1970s or 1980s had been allowed to go forward (Colombo 1996, p. 12). Similarly, the most visible of the Milanese public prosecutors — Antonio Di Pietro — had personally been attempting to uncover illicit ties between politicians and businessmen since 1985 (Di Pietro 2000, p. 5).

Only in 1992 did the Clean Hands investigations carry with them a wave of media attention. RAP were reported almost daily in the national press, and some of the most important trials were fully televised. The revelations documented in meticulous detail a level of political corruption that scandalized even hardened Italians. For at least a year and a half, political corruption was the single most important issue discussed in the national media in Italy.

To document the surge in media coverage of corruption as well as the impressive relationship of that surge with the shift of voter response to charges of corruption in Legislature XI, we collected story counts from Italy's major national newspaper as a measure of the informational environment surrounding corruption. Our measure is the annual average number of articles appearing in *Corriere della Sera* that report on corruption and party financing.³¹ As a check on the validity of our measure of the information environment for political corruption, we also include a separate measure of criminality. The latter consists of the annual average number of articles in *Corriere della Sera* that concern the Mafia and organized crime. Because *Corriere's* electronic index begins in 1984, we have press coverage information during only three legislatures, the Ninth, Tenth, and Eleventh.

To visualize the changing informational and judicial environment in which legislators operated, Figure 7 presents the annual figures on judicial efficiency and press reports of corruption and organized crime. Although the judicial data are available for each electoral district, which is the measure used in our statistical analysis, the graph presents the national average.

[Figure 7 about here]

The data depicted in the figure show an extraordinary jump in the reporting of corruption in 1992 and 1993. The number of *Corriere's* articles on corruption and party financing exhibit a nearly fivefold increase in 1992 over the previous year, and that number then doubled in 1993. The total number of articles that appeared in *Corriere* in 1990 on the topics of corruption and party financing was 51. It was 2,603 in 1993. This contrasts with the steady reporting on the Mafia and organized crime over the period, the numbers of which show little variation. The latter suggests that the increase in attention by the newspaper to issues relating to political corruption was not a function of a more general commitment to reporting on criminality.

Figure 7 also graphs the national average of judicial efficiency measured as the average length of time required for a case to get through the lower courts. The measure shows a dramatic but temporary increase in the mid-1980s followed by an equally severe decline in 1990. By this measure,

the Italian judiciary did not become more efficient in the early 1990s. These data thus fail to support the idea that a more efficient judiciary was responsible for the shift in voter behavior that occurred with the elections in 1994 to the Twelfth Legislature.³²

The data presented in Figure 7 corroborate instead the importance of the press. The Clean Hands investigations, public exposure, and resulting collapse of the postwar party system undid habitual voting patterns. The PSI, which had received over 13 percent of the vote in 1992, won a mere 2 percent of votes in 1994, thereby failing to clear the new 4 percent hurdle required for parliamentary representation. The Christian Democrats, long Italy's dominant party, changed its name to the Italian Popular Party (PPI) in an effort to cut their ties to old associations but even so won only 5 percent of seats in the lower house. The small traditional parties of the center, which had helped govern the country for four decades, were wiped out (Sani 1995).

Nor were these changes merely nominal. More than 70 percent of those elected in 1994 had never served before, compared with a previous parliament one third of whose deputies had already served three or more terms (Katz & Ignazi 1996, p. 22). The 1994 elections thus mark a clear transition to an almost entirely inexperienced and new political elite (Lanza 1995, pp. 220ff). Indeed, as far as we are aware, there is no other instance in a stable democratic society of as thorough and far-reaching a collapse of an established party system and entrenched political elite as occurred in Italy with the 1994 elections.

What other story might explain our empirical results? One possibility is that the reliability of the RAP as a signal of the probity of legislators may have changed over time, perhaps as a function of an improvement in the judiciary's public image.³³ The data presented in Figure 2 shows a gradual reversal in the partisan affiliation of those charged over the course of the postwar era, as the incriminated shifted from being overwhelmingly affiliated with the Communist and Socialist parties to being affiliated with the parties of government. If RAP are concentrated on deputies of the left while other sources of information inform voters that deputies affiliated with the ruling parties, especially the dominant Christian Democrats, are more likely to be involved in malfeasance, then voters may discard the RAP as unreliable information about past behavior. Only when the distribution of RAP are consistent with what voters believe about the distribution of actual malfeasance across the partisan spectrum might the RAP be incorporated into the voter's electoral decision. Only in the Eleventh Legislature were RAP overwhelmingly concentrated on members of the ruling parties; perhaps this finally made them credible to the electorate.

We see two major weaknesses with this line of thought. First, evidence from the U.S. tells us that voters are partisan in how they evaluate and react to information about possible criminal malfeasance on the part of elected officials (Peters & Welch 1980). Although we lack any direct evidence for Italy in this regard, the country's deep partisan divide and well-known ideological polarization suggests that voters would have filtered their evaluations of RAP through a strongly partisan lens, probably to a larger extent than in the U.S. Charges against deputies affiliated with a voter's own partisan bloc would have been substantially discounted. If we assume that some proportion of voters affiliated with the parties of government would never take charges of criminality into consideration when electing legislative representative, it becomes difficult to explain the massive anti-incumbency vote of 1994 without invoking additional explanatory variables.

Second, although this line of argument accurately captures the gradual shift in accusations from parties of the opposition to parties of government, the shift in voting behavior is not equally gradual. While accusation distributions move gradually towards ruling party members (Figure 2), we only see a significant response by voters after the wave of press attention to corruption surrounding the Clean Hands investigations. Nor do we find any evidence that judicial effectiveness (measured at the district level) is related to reelection rates of district deputies.

The variable that does seem to mirror the dramatic change in voting behavior is the time-series of press reports depicted in Figure 7. Drawing on previous theory and empirical evidence on the importance of the press (Zaller 1992, Besley & Burgess 2002, Adserà, Boix, & Payne 2003, Brunetti & Weder 2003, Chowdhury 2004, Reinikka & Svensson 2005, Gentzkow, Glaeser, & Goldin 2004, Freille, Haque, & Kneller 2007, Cole, Healy, & Weker 2008, Ferraz & Finan 2008), we believe it is more likely that a change in the national informational environment about political corruption generated by the Clean Hands investigation caused the material change in voter behavior towards corrupt and potentially-corrupt representatives. When major national newspapers repeatedly and insistently covered corruption as a major and ongoing story, the voters added corruption charges to their voting calculus. While one might speculate that the press followed voters, it is hard to imagine how voters would have coordinated a national shift in electoral behavior prior to press coverage. We conclude that the media was crucial.

A few issues remain. First, our study considers the behavior of political elites in the legislature, judiciary, and press, but we are ultimately interested in the Italian electorate. We assume that political accountability rests in the voters' hands. We argue that Italian voters changed the way

they responded to allegations of corruption over the legislatures covered by our study. An ideal research design for such an argument would be a panel study of a set of Italian voters over the postwar period, allowing us to observe how the same individuals respond to allegations of legislative malfeasance at different points in time. Unfortunately, to our knowledge no such individual level data exists. Instead, we examine the aggregate voter response to each incumbent deputy measured by the election outcome in each legislative period. We combine information about deputies, political parties, and the temporal context to make inferences about the Italian voter.

The precision with which we can attribute specific responses by district-level electorates depends upon the variation we observe in our theoretical causal variables. We are able to attach charges of malfeasance to individual deputies, allowing relatively precise estimates of the effects of such charges. We observe judicial effectiveness at the district level in only one half of our eleven legislatures and we are not able to assess variation of the measure across deputies within the same electoral district, lessening our power to measure the effect of judicial effectiveness. And we observe press coverage in national newspapers for only the final few legislatures, providing little variation to attribute press coverage to the outcomes for individual deputies. Despite these data limitations, we find evidence consistent with other research that the press is a crucial actor in electoral accountability in democratic polities. Although criminal charges against deputies were levied in all legislatures, and although judicial activism increased and decreased in the 1980s, it was only in the early 1990s that press coverage of corruption rose dramatically at the exact moment when the electorate voted out the corrupt legislative elite. We conclude that this change in the informational environment was crucial to the change in voter behavior.

Conclusions

Our findings arguably reflect a broader and “widely observed paradox: unpopular corruption and popular corrupt politicians” (Kurer 2001, p. 63). Prior to 1994, our data show that Italian voters tolerated allegations of criminality at rates that were not much different than voters in other wealthy democratic societies. In Japan, Reed (2005) finds that legislators lose only a few percentage points over their previous vote shares when they are indicted for corruption, and that those who are convicted actually see increases in their vote shares. Sixty-two percent of Japanese legislators convicted of corruption between 1947 and 1993 were subsequently reelected. Candidates to the

U.S. House of Representatives charged with corruption, although losing 6 to 11 percent in their expected vote shares depending on whether they are Democratic or Republican, are likely to be reelected to public office (Peters & Welch 1980). From 1982 to 1990 charges of corruption affected U.S. House reelection probabilities more severely, but again those charged were more likely than not to be reelected (Welch & Hibbing 1997). Dimock & Jacobson (1995) estimate that the ultimate impact of the 1992 House bank scandal, when legislators were exposed as having written bad checks and that resulted in the greatest turnover in the U.S. House of Representatives in 40 years, was to reduce incumbents' vote shares by 5 percentage points, although increased electoral uncertainty gave many incumbents an incentive to retire rather than run again (Groseclose & Krehbiel 1994). The survival rate of those implicated in bad check writing was 80 percent compared with 98 percent for those with no overdrafts.

During the first ten legislative periods we study, our data reveal similar patterns. Italian deputies for whom the judiciary request a removal of immunity on serious criminal charges typically ran for reelection, experienced almost identical reelection rates as their counterparts who were not charged, and saw no difference in the share of preference votes they received than others who are not charged.

Only when the press began reporting on political corruption on a daily basis did the issue become sufficiently salient to voters that they altered their habitual electoral behavior and refused to reelect incumbents who were likely to be involved in wrongdoing. The importance of corruption to voters in the 1994 elections is underscored by our finding that legislators who looked as though they would be likely to be charged lost their seats even if they had not already been charged. After decades of chronic high-level political malfeasance, with the help of serious judicial action and, crucially, a strident press corps, Italian voters finally threw the rascals out.

Notes

¹Because many deputies held office repeatedly, 2,973 different individuals filled these 6,800 parliamentary seats in the lower house during the 45-year period.

²The dataset we use is available from Golden (Versions posted 6/13/2006).

³A single RAP may name multiple deputies (in which case, our dataset records the RAP once for each deputy named) and a single RAP may list multiple statutes from the penal code.

⁴We code as opinion crimes charges that list articles from the Italian penal code numbers 269, 272, 278, 286, 290, 303, 340, 341, 342, 403, 405, 595, and 596, and statutes related to fascist activities. We are grateful to Davide Petrini for help developing this classification.

⁵Coding obstacles also preclude any valid classification of judicial charges into those involving political corruption and other types of wrongdoing.

⁶The Chamber failed to lift immunity for most deputies; see the data presented in Cazzola (1988, p. 113). In the postwar era through 1987, only about 20 percent of RAP were conceded by the Chamber, thereby allowing the judiciary to proceed. An even smaller proportion would have gone to trial.

⁷Accusations that the Italian judiciary is partisan in its pattern of allegations are made by Burnett & Mantovani (1998).

⁸The judicial office in a specific area of Italy investigates alleged criminal activities that occur only within its jurisdiction. There are 29 judicial districts across the Italian peninsula. The constitution requires the public prosecutor to open an investigation if criminal wrongdoing is suspected.

⁹Data are available only from 1970 onwards.

¹⁰In Italy, the average length of a government was 10 months during the forty-odd years we study. Governments frequently fell without new elections being called. Hence, multiple governments were inaugurated during most legislative periods. Occasionally these involved the addition or withdrawal of a political party.

¹¹These calculations necessarily exclude deputies serving in Legislature XI because the electoral system was modified from pure PR to a mixed-member system beginning with elections to the Twelfth Legislature, and preference votes thereby abolished. We also exclude deputies reelected from Legislature X to Legislature XI in 1992, when preference votes were reduced to one from three (or in large districts, four). These calculations refer only to deputies serving in one legislature who also served in the subsequent. However, if we analyze all Christian Democratic and Socialist deputies who served in one legislature and stood for reelection, whether they won their seat back or not, results are identical. There is no statistically significant difference in the average preference vote change for deputies who are charged and deputies who are not. The first see increases of 11 percent and the second increase of 13 percent in the number of preference votes won over the number won in the election prior to the legislative session in which the RAP is lodged (two-sample two-tail t-test, $p = 0.35$).

¹²During the period under study, Italian legislative elections were typically contested by more than a dozen parties. With the exception of regionally-specific parties, every party advanced as many candidates as seats in each of Italy's 32 electoral districts. With an average of 20 seats per district, this means there were more than 80,000 candidates over the eleven elections we study. This makes it infeasible for us to input data on all candidates for all parties.

¹³Between 1948 and 1994, 29 percent of DC deputies and 17 percent of PSI deputies were investigated by the judiciary for criminal wrongdoing at least once during their legislative careers.

¹⁴We have excluded from our coding deputies who died in office or who sought a seat in the Senate and not the Chamber.

¹⁵The p -value is calculated for a χ^2 test with one degree of freedom; $\chi^2 = 0.0445$.

¹⁶By cumulative tenure, we mean the number of previous terms served even if these are not continuous.

¹⁷Because we analyze the entire universe of members of the Chamber of Deputies, we do not know in this part of the analysis whether the 0 represents retirement or failure to win reelection.

¹⁸We correct for possible temporal dependence underlying the data using the suggestion of Beck, Katz, & Tucker (1998), and including the spell-identification natural cubic spline variables in the logit specification. The spline variables are simply a smooth function of a series of time dummy variables indicating the number of periods since the previous occurrence of the event.

¹⁹We calculate volatility as

$$v = \frac{1}{2} \sum |p_{t+1-t}^1| + |p_{t+1-t}^2| + |p_{t+1-t}^3| + |p_{t+1-t}^4| + |p_{t+1-t}^5| + |p_{t+1-t}^6| + |p_{t+1-t}^7|$$

where p^1 is the share of votes going to the PCI, p^2 to the DC, p^3 to the Italian Social Democratic Party (PSDI), p^4 to the Italian Socialist Party, p^5 to the Italian Republican Party (PRI), p^6 to the Italian Liberal Party (PLI), p^7 to the Italian Social Movement (MSI), and t refers to the year of the initial legislative election. For convenience, this formulation discards information on all parties other than these seven. The share of votes going to these parties is 93 percent nationally over the nine elections for which we calculate net electoral volatility. Net electoral volatility is 7 percent over all districts and elections for which we are able to compute it. Because the PCI and the PSI ran jointly for the election to Legislature I, we calculate volatility using returns to the Second Legislature as our initial base. Our measure of volatility therefore begins with the Third Legislature.

²⁰Other studies that have used this variable include Bianco, Jappelli, & Pagano (2005), Fabbri (2001), and Guiso, Sapienza, & Zingales (2004).

²¹In the immediate aftermath of World War II, the PSI was allied with the Communists rather than serving as a party of government, which started in 1963.

²²Results not presented. The coefficient estimate is 0.33 with a standard error of 0.66.

²³We follow Kousser, Lewis, & Masket (2007) in the method used to assess legislative period breaks.

²⁴We use the more limited specification so that the sampling does not exclude legislators with missing values.

²⁵The distribution also demonstrates the rarity of the coefficient from Legislature I.

²⁶Classification trees are a nonparametric binning method (see Ripley 2007). We chose this over duration models because we were uncomfortable with the assumptions required to “continue” the legislature past what we believe is a structural break in the Italian electoral environment.

²⁷For example, to explain a dichotomous dependent variable, “lung cancer,” given a host of demographic and behavioral variables, the tree might make a first binary partition on the explanatory indicator for cigarette smoker before making subsequent splits on other variables. As an instance of nonparametric modeling, classification trees require few assumptions about possible relationships between explanatory and response variables.

²⁸The probability might also be interpreted as how much the deputy “looks” like a charged deputy. A graphical representation of the classification tree is available from the authors on request.

²⁹Adding the chargeability variable to the model that estimates the probability of reelection for deputies seated in Legislature XI improves the model: a likelihood ratio test indicates a statistically significant improvement of fit with a p -value of 0.06.

³⁰The actual reelection rate to the Twelfth Legislature is 25.3 percent.

³¹Our measure of corruption combines what is called “reati, concussione” with “reati, corruzione.” The former refers to the extraction of kickbacks by public officials; the latter to receiving monies or other goods by public officials that are not properly due. Although the two types of activities appear closely connected, the former involves the collusion of the private agent paying the kickback.

³²While we would ideally like to include media coverage in a statistical model of reelection, we do not have enough variation. *Corriere's* electronic index covers only the final three legislatures and is only available as a single national measure. We therefore have no cross-sectional variation within legislatures and an n of only 3.

³³We are indebted to one of our reviewers for suggesting this line of argument.

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Table 1: Logit Estimation Results for the Effects of Serious Charges on Relistments and Reelection

	1	2	3
	RELIST	REELECT	REELECT
MAJOR	0.059	-0.205***	-0.278***
	[0.180]	[0.067]	[0.086]
AGE	0.091*		
	[0.055]		
AGESQ	-0.002***		
	[0.001]		
SENIOR	-0.171***	-0.199***	-0.188***
	[0.040]	[0.018]	[0.021]
FEMALE	-0.741***		
	[0.255]		
JOB	0.022		
	[0.139]		
UNCERTAINTY	-1.601***		
	[0.292]		
PARTY ELITE	0.488***	0.608***	0.445***
	[0.130]	[0.060]	[0.076]
RULING PARTY	0.544**	0.035	0.132*
	[0.212]	[0.055]	[0.072]
PREFERENCE		0.476***	0.431***
		[0.036]	[0.046]
VOLATILITY			-0.044***
			[0.012]
JUDICIAL EFF			0.000
			[0.001]
GROWTH			24.890***
			[2.377]
Constant	2.298	-4.246***	-4.171***
	[1.410]	[0.342]	[0.441]
Observations	3088	6828	4436

Notes: Standard errors in brackets.

The dependent variable in Model 1 is relistment on party list for next legislature; the dependent variable in Models 2 and 3 is reelection. Model 1 is estimated for candidates in the Christian Democratic and Italian Socialist Parties only. Models 2 and 3 are estimated for all incumbents.

Data on judicial efficiency is available only from Legislature V onwards. Data on seniority is available only from Legislature II onwards. Data on net volatility is available only from Legislature III onwards. Values for measures of economic growth and preference votes have been logged.

* significant at 10%; ** significant at 5%; *** significant at 1%

All tests are two-tailed. Coefficients for spline variables not reported.

Table 2: Logit Estimation Results for the Effects of Judicial Inquiry on Reelection by Legislative Period, All Deputies

	1	2	3	4	5	6	7	8	9	10	11
	REELECT	REELECT	REELECT	REELECT	REELECT	REELECT	REELECT	REELECT	REELECT	REELECT	REELECT
MAJOR	1.131*** [0.351]	0.292 [0.229]	0.107 [0.228]	0.068 [0.239]	0.285 [0.291]	-0.085 [0.217]	-0.373 [0.320]	0.200 [0.240]	0.038 [0.222]	-0.262 [0.238]	-0.626** [0.252]
RULING PARTY	-0.174 [0.209]	0.353* [0.199]	0.242 [0.214]	0.361** [0.181]	0.245 [0.206]	-0.061 [0.187]	0.679*** [0.203]	0.547*** [0.183]	0.570*** [0.182]	0.936*** [0.194]	-2.711*** [0.289]
PARTY ELITE	1.177*** [0.230]	0.903*** [0.217]	0.776*** [0.206]	0.520*** [0.188]	0.458** [0.205]	1.064*** [0.204]	0.592*** [0.218]	0.506*** [0.195]	0.090 [0.207]	0.142 [0.196]	0.294 [0.324]
PREFERENCE	0.447*** [0.155]	0.247* [0.136]	0.432*** [0.141]	0.355*** [0.118]	0.852*** [0.145]	0.565*** [0.141]	0.308** [0.145]	0.132 [0.114]	0.575*** [0.119]	0.449*** [0.125]	0.631*** [0.147]
SENIOR		-0.262 [0.186]	-0.144 [0.110]	-0.247*** [0.079]	-0.301*** [0.070]	-0.311*** [0.064]	-0.273*** [0.059]	-0.163*** [0.053]	-0.076 [0.052]	-0.131*** [0.047]	-0.203*** [0.075]
VOLATILITY			-0.042 [0.040]	-0.034 [0.052]	0.064 [0.070]	0.011 [0.047]	-0.070 [0.059]	0.013 [0.044]	-0.008 [0.065]	0.022 [0.040]	0.025 [0.046]
JUDICIAL EFF					-0.009* [0.005]	0.004 [0.003]	0.001 [0.003]	0.000 [0.002]	-0.003* [0.002]	-0.000 [0.004]	0.000 [0.005]
Constant	-4.508*** [1.605]	-2.150* [1.271]	-3.628*** [1.266]	-2.794** [1.191]	-7.344*** [1.477]	-5.765*** [1.295]	-1.693 [1.431]	-1.175 [1.048]	-4.876*** [1.118]	-4.885*** [1.209]	-5.727*** [1.685]
Observations	557	590	595	638	634	638	629	632	632	639	632

Notes: Standard errors in brackets.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3: Logit Estimation Results for the Effects of Electoral Uncertainty on the Probability of Being Charged in Legislature I

	CHARGE
PCI	1.794***
	[0.358]
PSI	1.199***
	[0.451]
DC	-0.908**
	[0.389]
Constant	-1.749***
	[0.313]
Observations	560

Notes: Standard errors in brackets.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: Logit Estimation Results for the Effects of Judicial Inquiry and Chargeability on Reelection, All Deputies, Legislature XI

	REELECT
CHARGEABILITY	-2.19*
	[1.28]
MAJOR	-0.56**
	[0.26]
RULING PARTY	-2.23***
	[0.383]
PARTY ELITE	0.265
	[0.325]
PREFERENCE	0.638***
	[0.147]
SENIORITY	-0.201***
	[.075]
VOLATILITY	0.018
	[0.047]
JUDICIAL EFF	0.002
	[0.005]
Constant	-5.47**
	[1.699]
Observations	632

Notes: Standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

All tests are two-tailed.

Chargeability derived from classification tree model on major charge dependent variable.

Figure 1: Proportion of Deputies Charged by Class of Allegation and Legislature

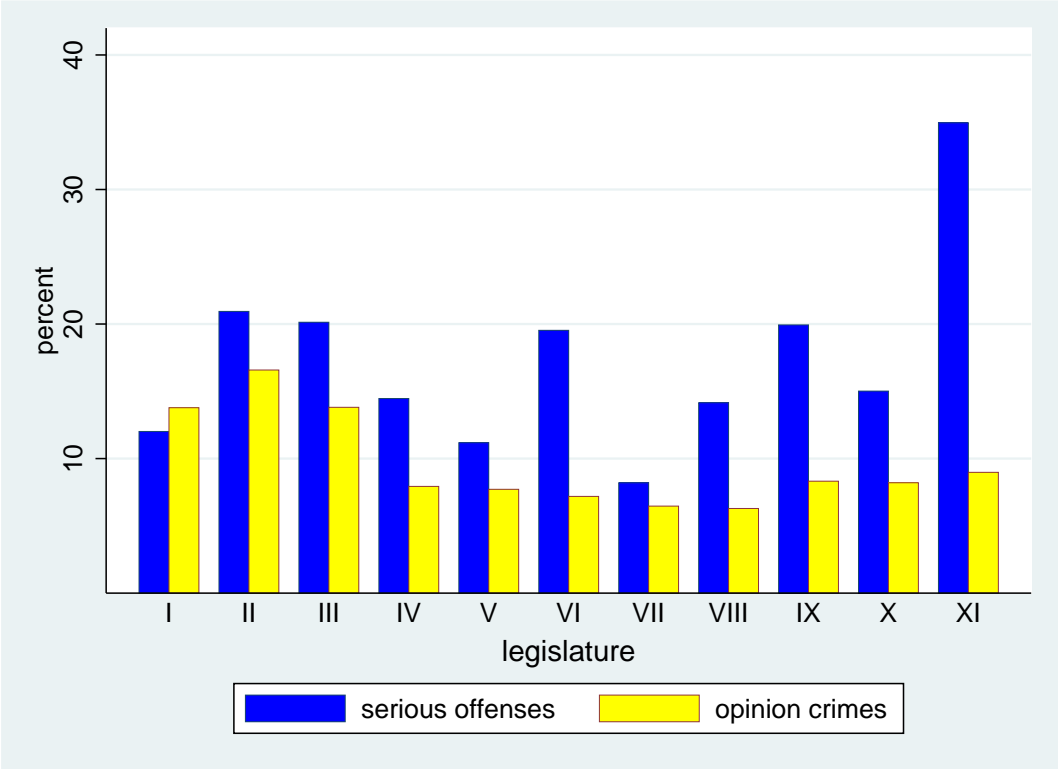


Figure 2: Numbers of Deputies Charged with Serious Offenses in Government and Opposition by Legislature

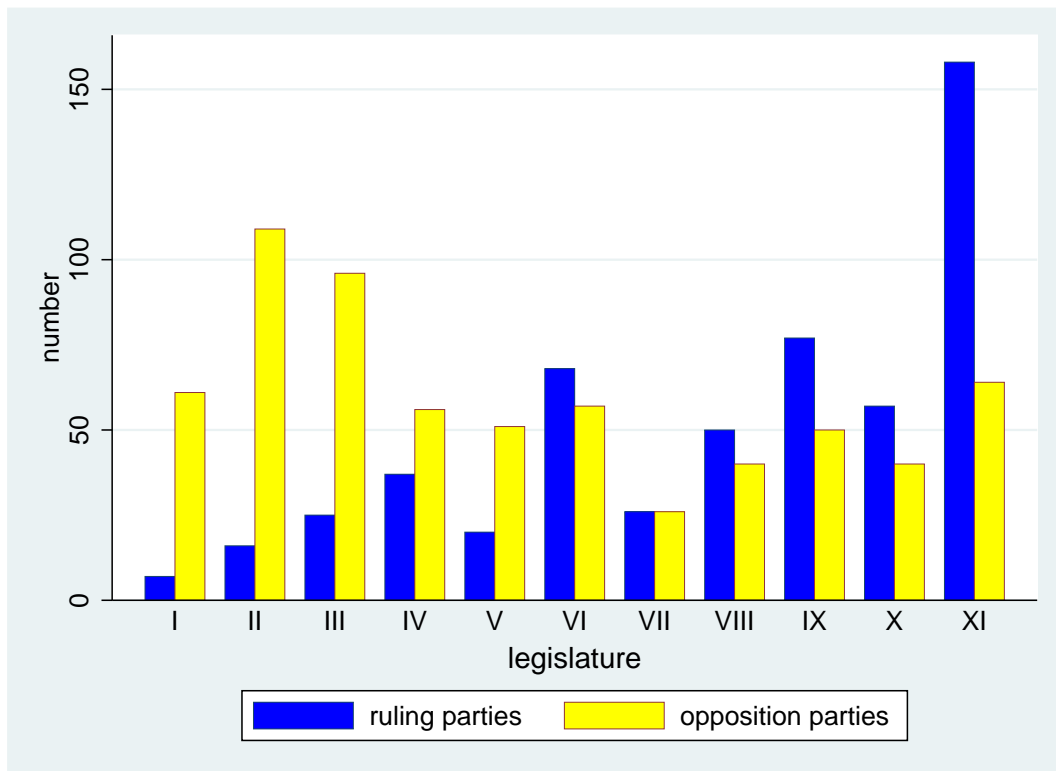


Figure 3: Proportions of Charged and Not Charged who are Reelected by Legislature (Serious Offenses Only)

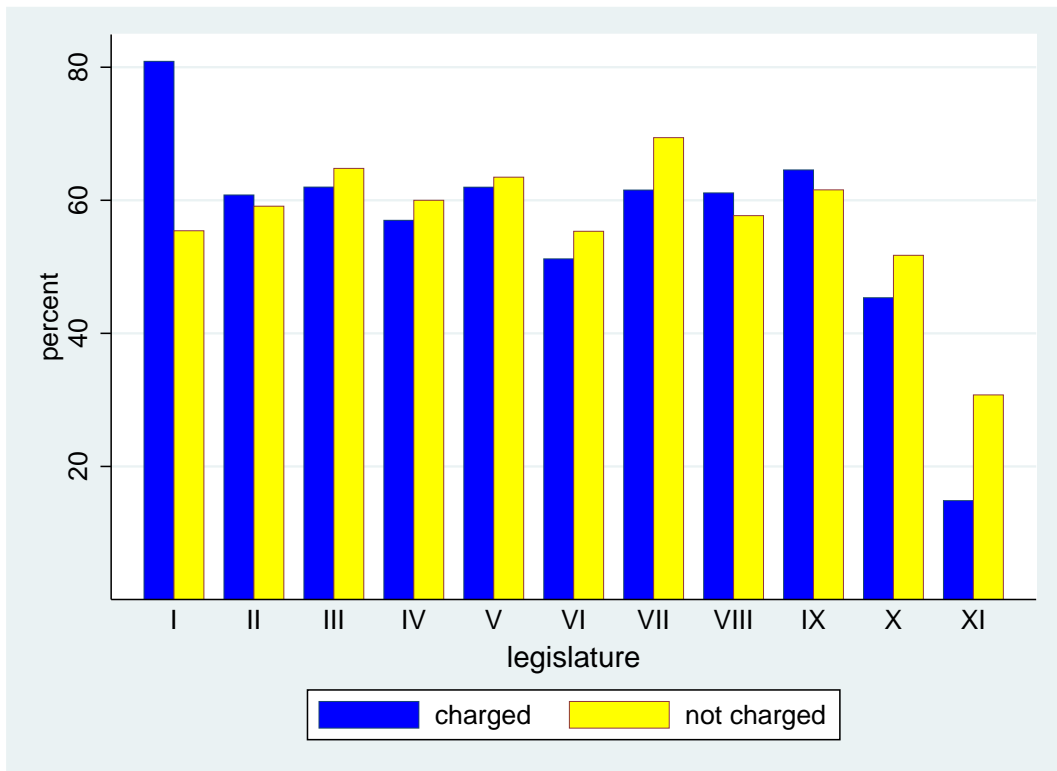


Figure 4: Probability of Reslistment by Age and Charge for DC and PSI Deputies

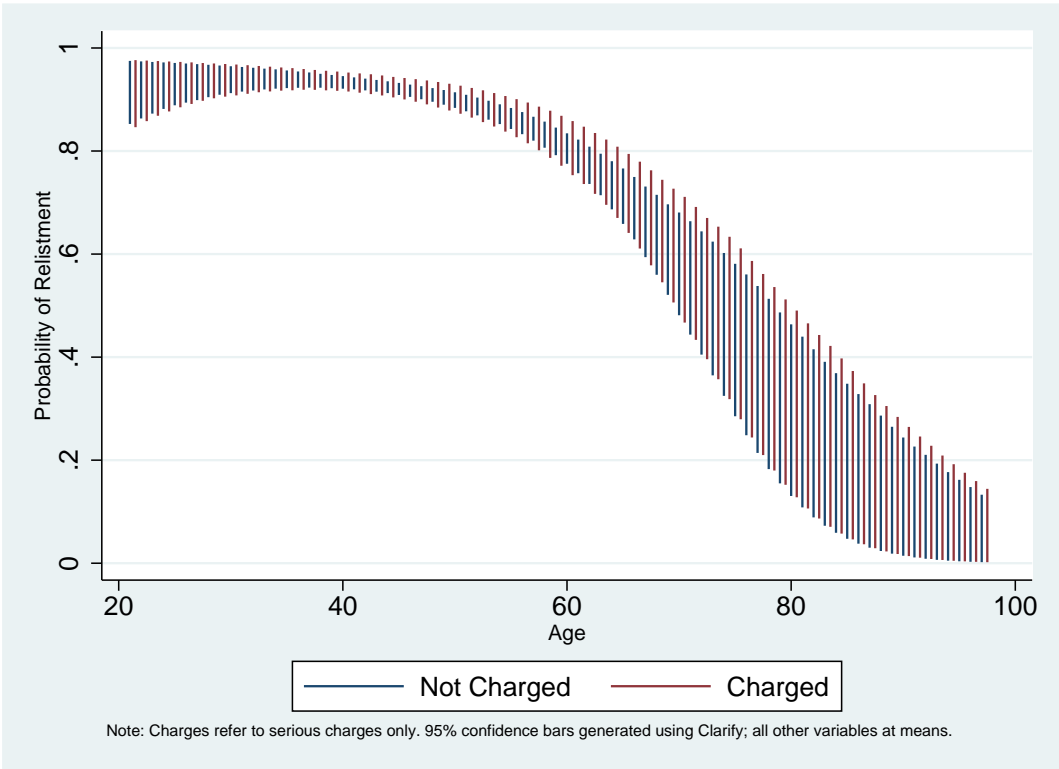


Figure 5: Matching Results Showing Effects of Being Charged on Reelection Probabilities by Legislature (Serious Offenses Only) with 95 Percent Confidence Intervals

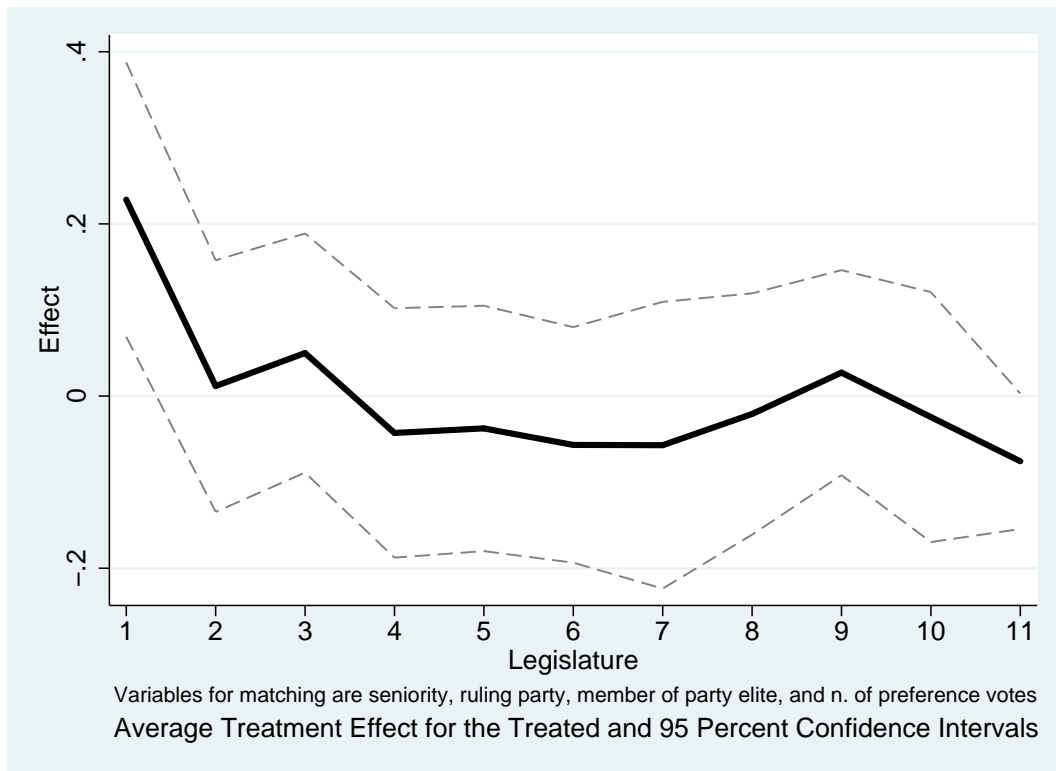


Figure 6: Distribution of Reelection Rates and Major Charge Coefficients of Deputies across Real and Pseudo Legislatures

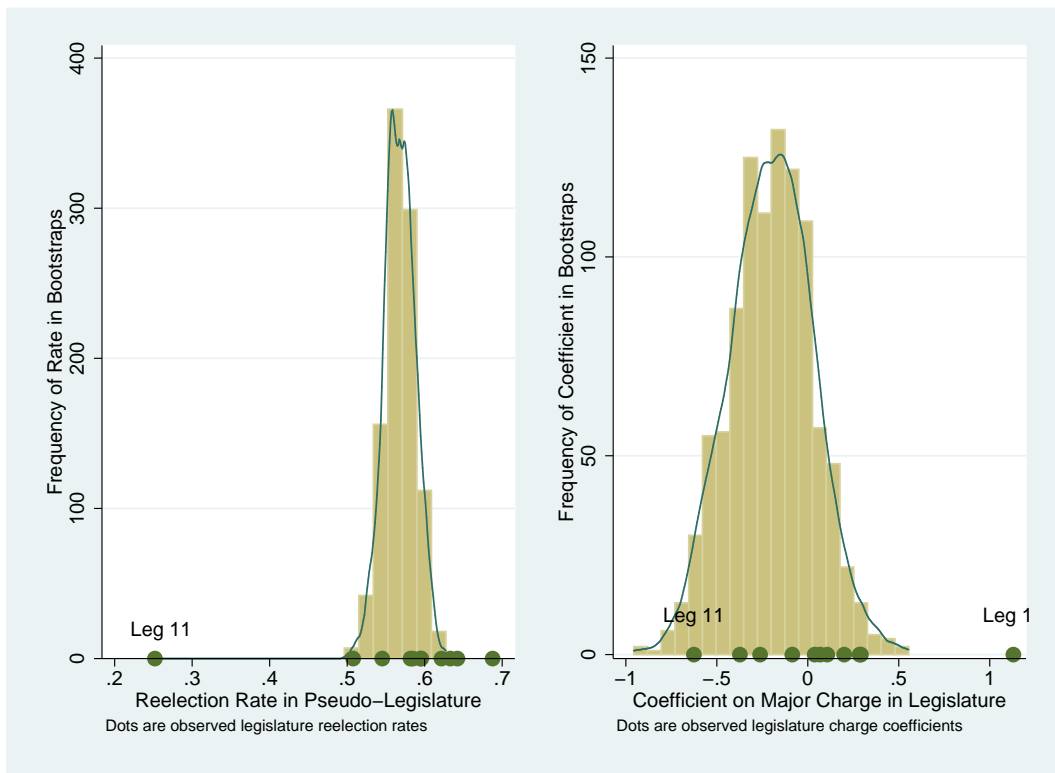
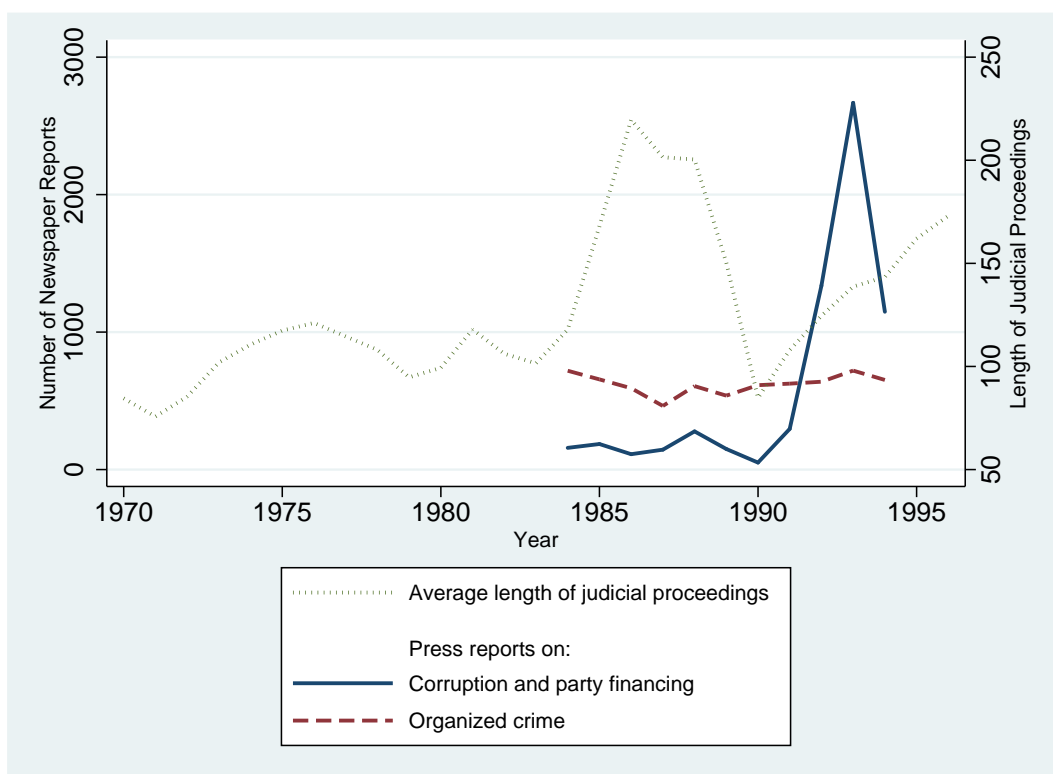


Figure 7: Numbers of Newspaper Reports of Corruption and Organized Crime by Year, 1984–1997 and Judicial Efficiency, 1970–1996



Notes: Newspaper reports from *Corriere della Sera*. Articles on corruption include those on corruption, bribe-paying, and party financing. Legislature XI met from 1992-1994, Legislature X from 1988-1992, and Legislature IX from 1984-1988.