LEGISLATIVE MALFEASANCE AND POLITICAL ACCOUNTABILITY

By ERIC C. C. CHANG, MIRIAM A. GOLDEN, and SETH J. HILL*

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 re-elected rather than repudiated by the electorate. This finding resonates with studies that suggest in various ways that political accountability may be realized only imperfectly even in well-established democracies, either because voters hold politicians responsible for things patently not under their control or because voters fail to take unresponsive or malfeasant representatives to task. In this article 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

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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 data set contains information on all persons in Italy's lower house in the 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 legislators. Our data set 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.[1]

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 one-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 long-standing 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.[2] A particularly vivid illustration comes from the observation that famines occur only in nondemocratic polities.[3] However, empirical evidence is surprisingly inconclusive regarding the impact of democracy on political corruption. Multiple studies report

[1] Because of this constitutional alteration as well as the change in the electoral system, it is neither possible nor desirable to extend the analysis after 1994.

[2] Schumpeter 1962; Ferejohn 1986; Przeworski, Stokes, and Manin 1999.

[3] Sen 1981.

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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.[4] A related literature investigates the impact of institutional variation within democratic polities on rent seeking and political corruption. The research produces contradictory findings about whether institutional characteristics such as presidentialism or parliamentarism allow greater political control and likewise whether systems of single-member districts or proportional electoral systems are the more effective in this regard.[5] Our understanding of why and how democratic political institutions enforce political accountability is thus still 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. 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 trying 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. For example, Ferraz and Finan show that once information documenting expenditure misappropriation by local Brazilian governments was assembled and released, voters retaliated by significantly reducing their support of those incumbent mayors most likely to have been culpable.[6] Similarly, Reinikka and Svensson 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.[7] Finally, Gentzkow, Glaeser, and Goldin argue that the growth of a nonpartisan press played a vital role in uncovering and monitoring corruption in the United States.[8] When the press is owned or controlled by political parties, it is likely to be partisan in its reporting and as a result may cover up allegations of wrongdoing by affiliated politicians. Other microlevel studies that corroborate the general intuition of the importance of the press in enforcing political accountability include Besley and Burgess and Cole, and Healy and

[4] Treisman 2000; Montinola and Jackman 2002.

[5] Chang and Golden 2006; Gerring and Thacker 2004; Kunicovà and Rose-Ackerman 2005; Lederman, Loayza, and Soares 2005; Persson and Tabellini 2003; Manzetti and Wilson 2007.

[6] Ferraz and Finan 2008.

[7] Reinikka and Svensson 2005.

[8] Gentzkow, Glaeser, and Goldin 2004.

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Weker.[9] These microlevel studies are complemented by a growing body of cross-national literature statistically documenting a relationship between press freedom and reduction of corruption.[10] The results we report are consistent with these macrolevel, general findings that a free press is instrumental in corruption control.

Our study also documents that a substantial change in voter behavior may follow a sudden significant increase in information made available by the press. Specifically, we attribute the structural break in the response by Italian voters to the media coverage of the early 1990s political corruption that grew out of the Clean Hands operation, in which judicial inquiries originating in Milan ultimately implicated thousands of politicians, including five former prime ministers. When the press bombards the public with stories about political corruption, we speculate that two results follow. First, political corruption becomes a more salient issue for the public, temporarily displacing other political issues in importance. Second, the extent of press attention serves as a coordination signal to voters, who can then coordinate electoral retaliation for ongoing political corruption. In addition, our results are consistent with work by McMillan and Zoido which documents that corrupt politicians fear a free press more than they do judicial inquiry.[11]

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 subsequent elections. 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 51 percent of charged deputies are reelected, compared with 58 percent of the putatively honest.

[9] Besley and Burgess 2002; Cole, Healy, and Weker 2008.

[10] Adserà, Boix, and Payne 2003; Brunetti and Weder 2003; Chowdhury 2004; Freille, Haque, and Kneller 2007.

[11] McMillan and Zoido 2004.

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In the third section of the article 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 left-wing deputies in the First Legislature were politically targeted by a still heavily fascist judiciary, and we produce evidence that they were. Following that, we study the unusual numbers of allegations of wrongdoing 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. We also show that voters in electoral districts with more access to information, measured by higher than median per capita newspaper and magazine circulation, voted out allegedly corrupt incumbents at higher rates than did those in lower-information environments. 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.[12]

Political Corruption among Italian Deputies: Some Background

Over the forty-five years that comprise our data set and that Italians call "the First Republic," Italy elected eleven legislatures and 6,800 deputies.[13] During these eleven legislatures the Ministry of Justice transmitted to parliament 3,149 official documents requesting the removal of

[12] Zaller 1992.

[13] Because many deputies held office repeatedly, 2,973 different individuals filled these 6,800 parliamentary seats in the lower house during the forty-five-year period.

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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.[14] 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.[15] As we document shortly, 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 course of professional politics. Following a study by Luca Ricolfi 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").[16] 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 other, similar items, including anything related to fascist activities.[17] 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.[18] 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.

As others have argued, precisely because it was so difficult to strip Italian parliamentarians of their immunity—doing so required a majority

[14] The data set we use is available from Golden (versions posted June 13, 2006).

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

[16] Ricolfi 1993.

[17] We 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.

[18] Ricolfi 1993, 24, table 3.

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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.[19] 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 encouraged wrongdoing on their part. In such an environment, any classification that interprets "corruption" simply as pocketing public monies or accepting illegal campaign contributions is excessively narrow.[20] In the analysis to follow, we restrict our attention to major (nonopinion) allegations, since these offer a more reasonable way to investigate how voters respond to charges of criminality than including phenomena such as libel and slander.

Before proceeding, 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 course of the postwar era were never convicted.[21] Given that, how do we know the RAP accurately capture malfeasance among elected representatives rather than judicial activism or merely judicial prejudice?[22] Do we have any way of estimating the *true* 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 four separate answers. No single answer will satisfy everyone, but we hope that taken together they cumulatively reassure skeptical readers that our data on the number of deputies accused of wrongdoing (1) do not contain systematic biases and (2) are appropriate for the purposes for which we use them. 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.[23] Where

[19] Golden and Chang 2001.

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

[21] The Chamber failed to lift immunity for most deputies; see the data presented in Cazzola 1988, 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.

[22] Accusations that the Italian judiciary is partisan in its pattern of allegations are made by Burnett and Mantovani 1998.

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

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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 +.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) 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.[24] If RAP were a function of a dedicated and effective judiciary and not a reflection of genuine political malfeasance, we would expect to see that greater judicial activity significantly lowered the reelection probabilities for malfeasant legislators. However, our data do not support this interpretation, giving us added confidence that the RAP serve as a valid proxy for political corruption.

Third, other research finds a strong relationship across Italy's thirty-two electoral districts 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.[25] 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, and so on) and the cumulative amounts of government money spent to construct new infrastructure. The index is a more precise measure than the RAP of actual wrongdoing by politicians, since the index captures the financial quantities involved in the diversion of public funds. That it is significantly correlated with the number of deputies incriminated in RAP in each electoral district in the Eleventh Legislature provides additional reassurance that RAP constitute 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

[24] Other studies that have used this variable include Bianco, Jappelli, and Pagano 2005; Fabbri 2001; and Guiso, Sapienza, and Zingales 2004. Data are available only from 1970 onward.

[25] Golden and Picci 2006. The index of missing infrastructure is available only for one cross-section, not over time.

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have electoral effects. We aim to assess whether the electorate, once the judiciary has officially implicated a deputy in wrongdoing, exacts electoral retribution. We bring in additional information to isolate the political credibility of the RAP when we examine specific features of the First and Eleventh Legislatures. Our analysis supports the interpretation that RAP were far less credible in the immediate postwar period than they became as the judiciary shed its fascist personnel over the years. The postwar constitution guarantees that the Italian judiciary is one of the world's most independent, and our study uncovers no evidence to contradict this except in the First Legislature after World War II. Thus, so long as the voting public believes that judicial investigations are based on adequate evidence, we expect to observe an electoral reaction.

Description of the Data

We turn now to an initial description of our data regarding charges of malfeasance. In Figure 1 we present for each legislature the proportions of deputies charged with opinion crimes and with serious malfeasance. Smaller proportions of deputies are charged with minor (opinion) crimes than with serious malfeasance for every legislature except the First. A more important feature of the data 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 our analysis below we document that aggregate results are driven by the final legislature and we present evidence of a period break in electoral behavior.

We next examine patterns of charges for serious crimes lodged against legislators affiliated with the governing parties compared with those representing the opposition. Figure 2 shows the numbers of deputies who are charged divided into government and opposition, where "government" is defined as deputies elected to any party holding ministerial posts at any time during the life of the legislature.[26] The data show a reversal in the partisan identity of the accused over the

[26] In Italy the average length of a government was ten 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.

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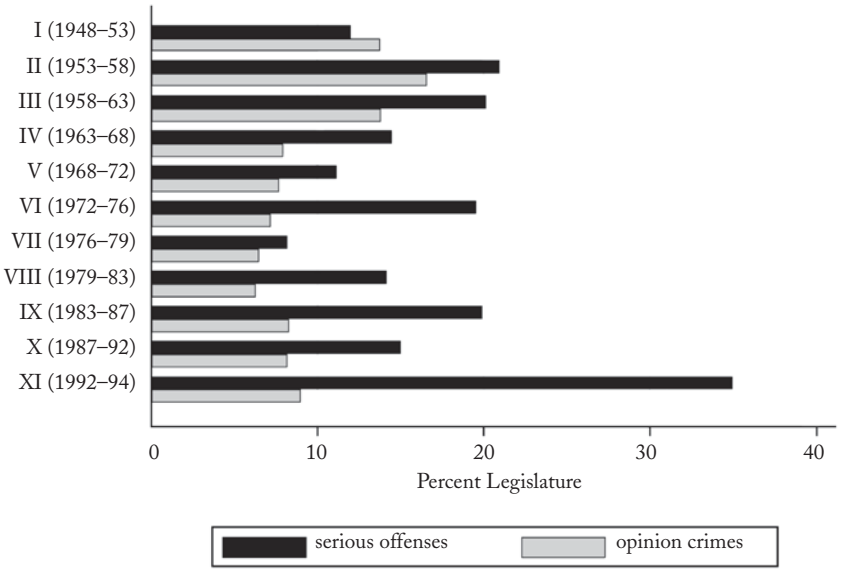


FIGURE 1
PROPORTION OF DEPUTIES CHARGED BY CLASS OF ALLEGATION AND LEGISLATURE

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 reelection.

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. 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 number of seats allocated each party) and because they are used within parties to allocate resources and influence. Vote gains by incumbents presumably reflect successful

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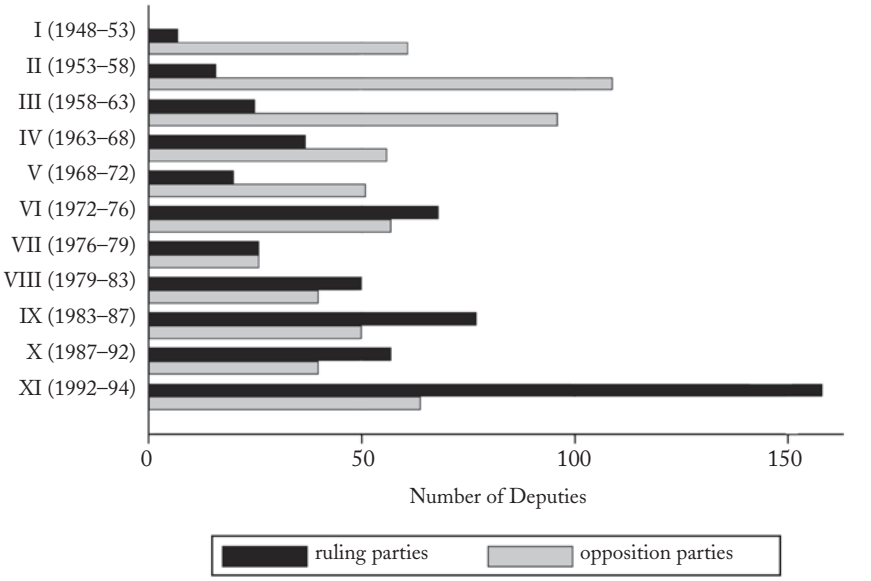


FIGURE 2
NUMBERS OF DEPUTIES CHARGED WITH SERIOUS OFFENSES
IN GOVERNMENT AND OPPOSITION BY LEGISLATURE

cultivation of political clienteles as well as constituency service. 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 in the change of preference votes received after being investigated between the two classes of deputies is not statistically significant.[27]

Over the eleven legislatures we study, an average of 57 percent of Italian deputies gain reelection into the subsequent legislature. Despite

[27] These 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 one. 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).

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the commonly held view that Italy's "First Republic" enjoyed an unusually entrenched political elite, this is not a high reelection rate for incumbent legislators. A cross-national analysis shows that in 25 democratic countries with over 116 elections held between 1979 and 1994, the average proportion of incumbents returned to the lower house is 68 percent; Italy ranks in the middle of the 25.[28] 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.

Effects of Charges on the 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 are referred to in the U.S. context as *strategic retirements* and reelections in a unified framework.[29] We are unable to do this because, whereas we have data on every member of the lower house in the postwar era, we have

[28] Matland and Studlar 2004, 92, table 1.
[29] As in Kiewiet and Zeng 1993.

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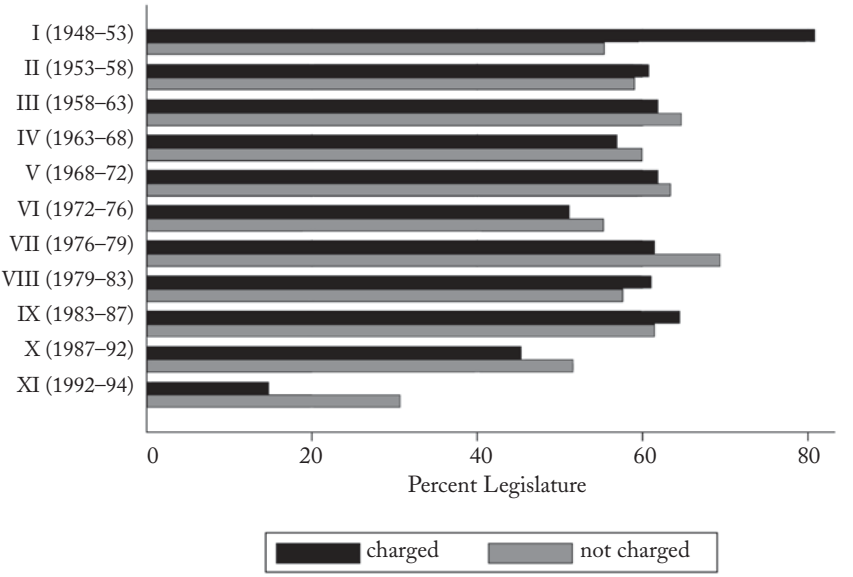


FIGURE 3
PROPORTIONS OF CHARGED AND NOT CHARGED
WHO ARE REELECTED BY LEGISLATURE (SERIOUS OFFENSES ONLY)

complete data on candidates for only two parties, Christian Democrats and the Socialists.[30] 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.[31]

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

[30] During 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 thirty-two electoral districts. With an average of twenty seats per district, this means there were more than eighty thousand candidates over the eleven elections we study. This number makes it infeasible for us to input data on all candidates for all parties.

[31] Between 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.

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results are not driven by selection bias. Our results are in direct contrast to those reported for the U.S., where legislators involved in scandal tend to retire rather than face electoral defeat.[32]

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, were 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.[33]

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 with a noncharged peer rate of 12.9 percent (p = 0.833).[34] 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, in a study of legislative careers in Italy, and on the assumption that parties relist incumbents for reasons that are both politically strategic and based on demographic attributes.[35] 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*),[36] 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

[32] Kiewiet and Zeng 1993.

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

[34] The p-value is calculated for a Chi square test with one degree of freedom; p = 0.0445.

[35] Kiewiet and Zeng 1993; Groseclose and Krehbiel 1994; Hall and Van Houweling 1995; Moore and Hibbing 1998; Theriault 1998; Cotta, Mastropaolo, and Verzichelli 2000.

[36] By cumulative tenure, we mean the number of previous terms served even if these are not consecutive.

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the legislator had nonlegal, nonpolitical, and high-status employment before becoming a deputy. Following Chang, 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.[37] 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 stand for reelection. Results are presented in model 1 of Table 1.

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 not to appear on the subsequent ballot. 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. The first may be due to self-selection or discrimination and the third is a reasonable strategic response to a more marginal electoral situation. While we do not have a complete explanation for why more senior incumbents are more likely to withdraw from continued legislative careers, we note that in the Italian Chamber seniority carries with it none of the political benefits and resources that it does in the U.S. Congress.[38] Results also show that being a member of an elite party body or being affiliated with a party in government is associated with a statistically significant increasing propensity to be relisted. Both are intuitive: more powerful incumbents or incumbents affiliated with more powerful parties have greater incentives to seek to continue their careers in the Chamber. Running the model by legislative period, in no legislature are charges of malfeasance associated with a decreased likelihood of being relisted (results not presented). Figure 4 shows that the predicted relistment rates of DC and PSI deputies charged with serious malfeasance (upper frame) and not charged (lower frame) are essentially the same. There is no substantive or statistical difference in the expected rates of being relisted for these two groups of deputies.

We interpret these results as evidence that incumbents and their parties do not fear voter retaliation when charged with criminality. Deputies

[37] Chang 2005.
[38] Della Sala 1993, 176.

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Table 1
Logit Estimation Results for the Effects of Serious Charges of Relistments and Reelection [a]

	Model 1 RELIST	*Model 2 REELECT*	*Model 3 REELECT*
MAJOR	0.059	–0.205***	–0.278***
	[0.180]	[0.067]	[0.086]
AGE	0.091*		
	[0.055]		
AGESQ	–0.002***		
	[0.001]		
SENIORITY	–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(L)		0.476***	0.431***
		[0.036]	[0.046]
VOLATILITY			–0.044***
			[0.012]
JUDICIAL EFF			0.000
			[0.001]
GROWTH(L)			24.890***
			[2.377]
Constant	2.298	–4.246***	–4.171***
	[1.410]	[0.342]	[0.441]
Observations	3088	6828	4436

* significant at 10%, ** significant at 5%, *** significant at 1%; all tests are two-tailed; coefficients for spline variables not reported; standard errors in brackets

[a] The dependent variable in model 1 is relistment on the ballot 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 are available only from Legislature V onward. Data on seniority are available only from Legislature II onward. Data on net volatility str available only from Legislature III onward. Values for measures of economic growth and preference votes are logged.

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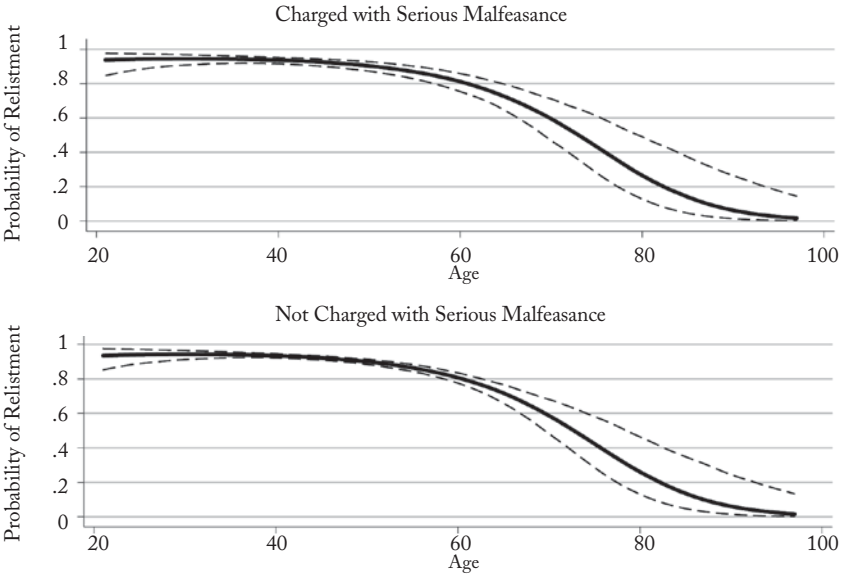


FIGURE 4
PROBABILITY OF CANDIDATE RELISTMENT BY AGE AND CHARGE FOR DC AND PSI DEPUTIES[a]

[a] Charges refer to serious charges only; dashed lines are 95 percent confidence intervals generated using Clarify with all other variables at means.

who are implicated in malfeasance by the judiciary 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 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 noncharged peers, suggesting against 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) logistic regressions of each legislative period separately; and (3) nonparametric matching analysis of each legislative period separately.

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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.[39] 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.[40]

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, or 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 more preference votes may enjoy greater protection from electoral retaliation if preference votes signal a client base

[39] Because 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.

[40] We correct for possible temporal dependence underlying the data using the suggestion of Beck, Katz, and 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.

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that is loyal mainly on the basis of material inducements or ideological commitments. 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, 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 when charged with malfeasance. However, the size of the effect is modest: 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 noncharged counterparts. Reelection depends more on being a member of a party elite body and establishing a large clientele (the marginal effect of *ELITE* and *PREFERENCE* is 14.5 and 11.6 percent, respectively). Allegedly criminal legislators still enjoy reelection rates that are greater than even.

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 a standard index, which captures net change in electoral behavior across parties from one election to the next.[41]

We have already remarked that 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

[41] Pedersen 1979. Following Pedersen, we calculate volatility as

$$v = \frac{1}{2} \times \left(\left| p_t^1 - p_{t-1}^1 \right| + \left| p_t^2 - p_{t-1}^2 \right| + \left| p_t^3 - p_{t-1}^3 \right| + \left| p_t^4 - p_{t-1}^4 \right| + \left| p_t^5 - p_{t-1}^5 \right| + \left| p_t^6 - p_{t-1}^6 \right| + \left| p_t^7 - p_{t-1}^7 \right| \right)$$

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), and p^7 to the Italian Social Movement (MSI); and t refers to the year of the 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 in the elections 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.

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control variable (*JUDICIAL EFF*). 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 activity. Where the judicial process moves slowly, public prosecutors are likely to be generally less efficient and therefore less able to gather preliminary evidence against malfeasant politicians sufficient to file the request with the Ministry of Justice to investigate further. Hence, if judicial effectiveness is contributing to the manufacturing of RAP, we would expect a significant positive coefficient on this variable. To rule this out, we would like the relationship between this variable and the probability of reelection to prove insignificant. Because the data on judicial efficiency are available only from 1970 onward, however, including this variable requires dropping the first four postwar legislatures from the model. We aggregate the measure from Italy's twenty-nine judicial districts to its thirty-two electoral districts.

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. These results are all as expected and intuitively easily interpretable. Total terms in office, *SENIORITY*, is negatively associated with reelection probabilities, holding all else equal, a result we find surprising compared with the situation in the U.S., where seniority is known to be very important. Since seniority carries no benefits in the legislature, however, we interpret this result as documenting that even experienced legislators are not more effective in using their office to secure reelection, conditional on their age, party leadership positions, and other covariates in the model.

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 nonparametric matching technique—and we perform each type of analysis by legislative period. We present logistic regression results in Table 2.

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TABLE 2
LOGIT ESTIMATION RESULTS FOR THE EFFECTS OF JUDICIAL INQUIRY ON REELECTION BY LEGISLATIVE PERIOD, ALL DEPUTIES

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

* significant at 10%, ** significant at 5%, *** significant at 1%; standard errors in brackets

Results from almost all of the eleven legislative periods confirm the importance of assembling a large client base and having received 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 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 nonparametric matching technique.[42] 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 to outcome. Among other advantages of matching is that we do not have to assume a 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 observable characteristics that we use as control

[42] Described in Abadie, Drukker, Herr, and Imbens 2004.

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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.

We display the matching results in Figure 5. They 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 twenty points, with a 95 percent confidence interval that does not include zero. Effects in all other legislatures are not statistically 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 ($p=.059$). The results of the matching procedure corroborate that deputies who are charged by the judiciary with major crimes do not suffer electorally in most legislatures. In the First Legislature, however, criminal charges provide an electoral boost in the subsequent election.

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, still not purged of those sympathetic to Benito Mussolini's failed regime, deliberately targeted left-wing 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.[43] Our hypothesis is that deputies on the left were more often the target of a still heavily fascist and strongly anticommunist, antisocialist judiciary.

Results reported in Table 3 support the hypothesis. The coefficient estimates for the two left-wing parties are positive and statistically significant, whereas the coefficient for Christian Democrats indicates that these deputies experience 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 and a Communist a 51 percent predicted probability of

[43] In the immediate aftermath of World War II, the PSI was allied with the Communists rather than serving as a party of government. The PSI entered government for the first time in 1963.

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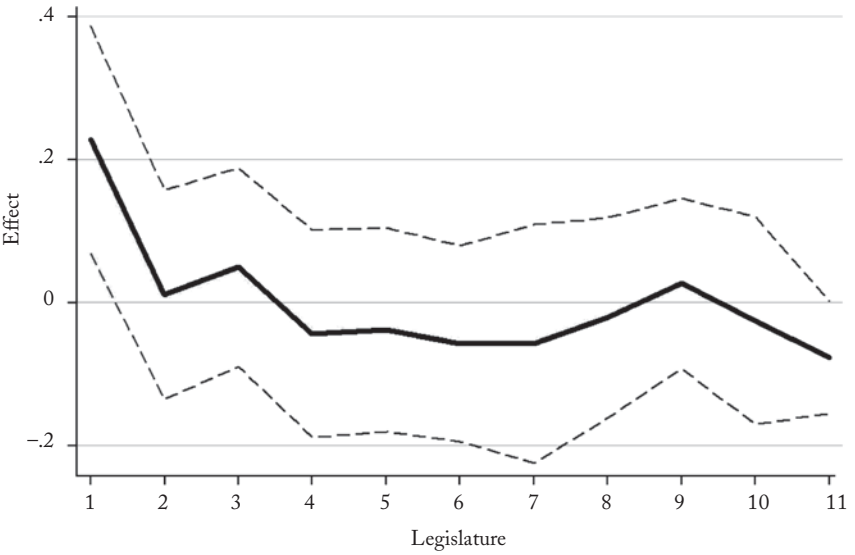


FIGURE 5
MATCHING RESULTS SHOWING EFFECTS OF BEING CHARGED ON REELECTION PROBABILITIES BY LEGISLATURE (SERIOUS OFFENSES ONLY) WITH 95 PERCENT CONFIDENCE INTERVALS[a]

[a] Matching variables used are seniority, ruling party affiliation, member of party elite, and number of preference votes.

being charged. The data suggest that left-wing 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 believable explanation. This effect subsequently disappeared as the newly democratic republican regime installed a new generation of magistrates and as fascist magistrates became politically more isolated and circumspect.

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 Resistance against the Germans. 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

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TABLE 3
LOGIT ESTIMATION RESULTS FOR THE EFFECTS OF PARTISAN AFFILIATION 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

* significant at 10%, ** significant at 5%, *** significant at 1%; standard errors in brackets

is quartered and statistical significance lost.[44] In other words, the significant result for Legislature I is driven entirely by the fact that so many very popular left-wing legislators were charged by what was arguably a vindictive and politicized judiciary. Voters were not reelecting corrupt political representatives. Rather, a fascist judiciary spuriously charged those left-wing incumbents who had the greatest probability of reelection, that is, those whom voters continued to support.

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 is intriguing. How different from other legislatures is Legislature XI? What would have happened had the Eleventh Legislature not been dissolved after only two years? How many more deputies might have been named in RAP if the legislative session had continued? In this section we provide answers to these questions. The following section then considers why Italian voters did not penalize political malfeasance any earlier.

We first investigate whether features of Legislature XI are genuinely unusual rather than falling within a reasonable range of variation. To do this, we implement a nonparametric bootstrap analysis. We randomly draw with replacement from our pool of deputies 1,000 pseudo-legislatures of 620 deputies each, ignoring the actual legislative period

[44] Results not presented. The coefficient estimate is 0.33 with a standard error of 0.66.

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of each deputy in the resampling, and we calculate statistics on each for comparison with statistics from the actual Eleventh Legislature. If that legislature indeed represents a structural break, we should find that statistics calculated from Legislature XI reside in the tails of a distribution of such statistics from our 1,000 pseudolegislatures.[45]

In Figure 6 we present bootstrapped results for two statistics of interest: the reelection rate and the logit coefficient on the major malfeasance charge variable. 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 bootstrap distribution including deputies from Legislature XI in the resampling.

The second frame of Figure 6 presents the distribution of logit coefficients estimated on each pseudolegislature with the specification of Table 2, column 1.[46] While the coefficient from the actual Eleventh Legislature is not as much 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 that relationship in the remainder of the postwar period.[47]

We infer 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, we now ask what would have happened if the legislature had not been dissolved and early elections called.[48] How many more deputies might have been named in RAP?

To estimate who 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 statistical method.[49] Classification trees take a categorical dependent variable and explanatory variables (numeric or categorical) and make recursive binary partitions to explain variation in the dependent variable most

[45] We follow Kousser, Lewis, and Masket 2007 in the method used to assess legislative period breaks.

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

[47] The distribution also demonstrates the rarity of the coefficient for Legislature I.

[48] Ricolfi 1993.

[49] Classification 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.

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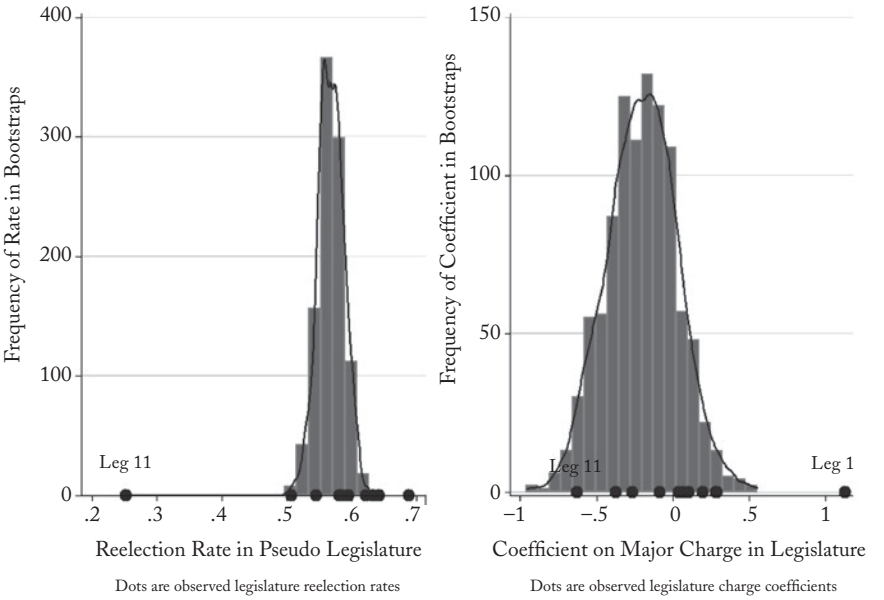


FIGURE 6
DISTRIBUTION OF REELECTION RATES AND MAJOR CHARGE COEFFICIENTS OF DEPUTIES ACROSS REAL AND PSEUDO LEGISLATURES

efficiently.[50] 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 which other deputes would have been charged had the legislature sat in session longer.

We model charges of major malfeasance for deputies in Legislature XI using four explanatory variables: seniority, ruling party, judicial activism, and gender. The first partition splits deputies on ruling party: those not affiliated with 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.[51]

[50] For 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.

[51] The 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.

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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 seen 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.[52] 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.[53] Deputies who look more guilty 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 removal of parliamentary immunity but also incumbents who looked like they were corrupt even though the judiciary had not (yet) assembled evidence to lodge a RAP against them.

Interpreting the Results for Legislature XI: The Role of the Press

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 not only on allegedly corrupt incumbents but also on 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 informa-

[52] Adding 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.

[53] The actual reelection rate to the Twelfth Legislature is 25.3 percent.

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TABLE 4
LOGIT ESTIMATION RESULTS FOR THE EFFECTS OF JUDICIAL INQUIRY AND "CHARGEABILITY" ON REELECTION, LEGISLATURE XI

	REELECT
CHARGEABILITY[a]	–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

* significant at 10%, ** significant at 5%, *** significant at 1%; standard errors in brackets; all tests are two-tailed

[a] "Chargeability" is derived from classification tree model on major charge dependent variable. It measures the likelihood of being charged.

tion environment due to what are known as the "Clean Hands" investigations that began in 1992. Similar investigations stem from at least as far back as 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.[54] 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.[55]

In this earlier period a small number of newspapers, often local in readership—*La Voce della Campania*, for instance, a Neapolitan

[54] Ferraresi 1992.
[55] De Luca and Giustolisi 1993.

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monthly—repeatedly tried to investigate political corruption.[56] 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, and sometimes transferred to different jurisdictions without their cases. In addition, the newspapers that publicized the investigations were sued.[57] 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 came out only in 1993 and 1994 as much as ten years earlier had some of his investigations in the 1970s or 1980s been allowed to go forward.[58] Similarly, Antonio Di Pietro, the most visible of the Milanese public prosecutors, had personally been attempting to uncover illicit ties between politicians and businessmen since 1985.[59]

Only in 1992 did the Clean Hands investigations arouse a national wave of media attention. The national press reported on RAP almost daily, and some of the most important trials were televised in their entirety. 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 number of articles appearing in *Corriere della Sera* that report on corruption and party financing.[60] As a check on the validity of our measure of the information environment for political corruption, we also include a separate measure of newspaper stories involving criminality. The latter consists of the annual 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.

[56] Bechis 1994, 200.
[57] See De Luca and Giustolisi 1993, 103; Bechis 1994, 200; Gilbert 1995, 128.
[58] Colombo 1996, 12.
[59] Di Pietro 2000, 5.
[60] Our 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.

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To visualize the changing informational and judicial environment in which legislators operated, Figure 7 presents the annual measures of 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. The numbers of press reports in *Corriere* are obviously available only at the national level and not at the district level.

The data depicted in the figure show an extraordinary jump in *Corriere*'s reports of corruption in 1992 and 1993. The number of articles on corruption and party financing exhibited 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 number of reports 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 or to a greater interest in "soft" as opposed to hard news.

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 become more efficient in the early 1990s but only relative to a temporary surge of inefficiency that had occurred the 1980s. The measure in the early 1990s looks about the same as it had for the 1970s and much of the 1980s.

These data fail to support the claim that a newly efficient judiciary was responsible for the shift in voter behavior that occurred with the elections in 1994 to the Twelfth Legislature.[61] Instead, the data presented in Figure 7 corroborate the importance of the press in delivering a barrage of new information to the electorate. However, the press figures are available only at the national level and require assumptions about temporal comparability. To further test our assertion that media coverage was a central trigger in voters' willingness to remove corrupt deputies from office after Legislature XI, we collected newspaper circulation counts for each province in Italy from 1986 to 1994 and

[61] While we would ideally like to include media coverage of corruption in our statistical models of reelection, we do not have enough variation to do so. 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 only three observations.

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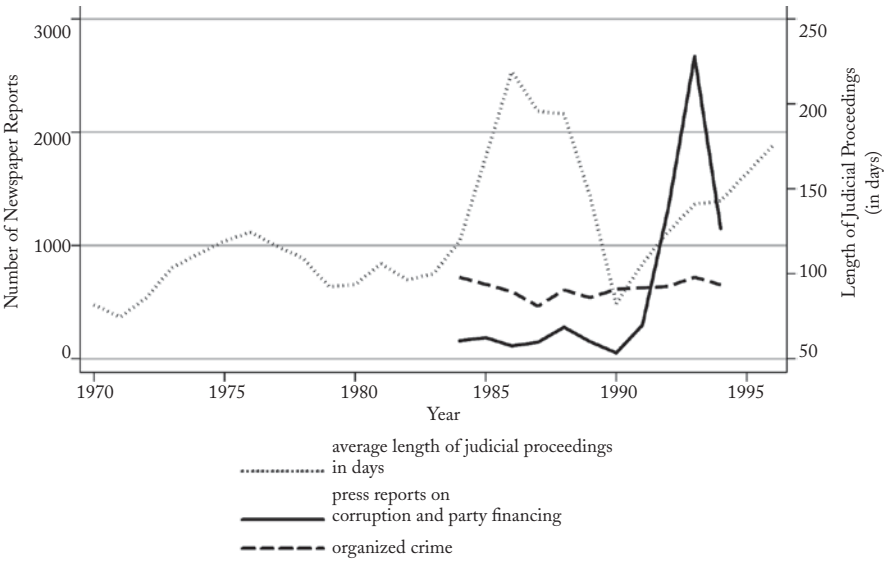


FIGURE 7
NUMBERS OF NEWSPAPER REPORTS OF CORRUPTION AND ORGANIZED CRIME BY YEAR (1984–97) AND JUDICIAL EFFICIENCY (1970–96)[a]

[a]Newspaper reports from *Corriere della Sera*. Articles on corruption include those on corruption, bribe paying, and party financing. Average length of judicial proceedings is in days.

aggregated these to Italy's thirty-two electoral districts.[62] We were thus able to gather circulation measures that covered the periods during which Legislatures IX, X, and XI sat. We divided the number of total newspapers and weekly magazines by the resident population in each electoral district to create a measure of media exposure. Since we know that *Corriere della Sera* increased its reporting of corruption at the end of the Tenth Legislature, we assume a similar pattern for Italian newspapers and magazines generally. Using these data allows us to test whether districts with higher newspaper circulation were more likely to eject their allegedly criminal legislators from public office. We hold time constant and consider cross-sectional variation in media coverage in contrast to the analysis based on the time-varying measures reported in Figure 7. Of course, the circulation measure does not capture exposure to other forms of media, including television and radio, except to the extent that other media consumption may be correlated with newspaper and periodical circulation.

[62] We were not able to locate provincial-level data prior to 1986. The data include daily newspapers and weekly magazines.

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In Table 5 we present a logit regression analysis of the probability of reelection for deputies into the Tenth, Eleventh, and Twelfth Legislatures with the same specification as in Table 2 but with two additional variables. We create a dummy variable indicating whether the district has below or above median per capita newspaper and magazine circulation. This allows us to test the assertion that places with more media exposure were more likely to turn against incumbent legislators. We also model the interactive effect of high circulation and major charge. If more press coverage increases the likelihood that voters remove allegedly corrupt incumbents from office, this should result in a negative coefficient on the interaction of above median circulation per capita and major charge. We expect this to be especially true in Legislature XI where, as we documented earlier, the number of newspaper reports about corruption increased dramatically.[63]

The effect of the variable that interacts high newspaper circulation and major charge is estimated with large uncertainty and with wrongly signed coefficients for Legislatures IX and X. We interpret this as meaning that the press was not delivering enough information to voters about political wrongdoing in these years for media exposure to be associated with any systematic effects on the reelection probabilities of accused legislators. In the Eleventh Legislature, by contrast, we find a correctly signed although statistically insignificant coefficient. The two charge coefficients (the interaction variable and the direct effect of high circulation) are jointly significant at p=.04 in Legislature XI but are not jointly significant in either of the two earlier legislatures for which we have data. In addition, the substantive effect of the point estimate on the interaction term for Legislature XI is not small. A deputy charged in the Eleventh Legislature from a district of below-median per capita circulation has a predicted reelection rate that is 1 percentage point higher than a similarly charged incumbent from a district of above-median per capita circulation, all other variables set to average values.[64] Malfeasant deputies were thus especially penalized electorally in areas of the country with greater newspaper and periodical circulation.

While the effect of cross-district media exposure is estimated with uncertainty, we take the pattern across the three legislatures and the

[63] Of course, newspaper circulation per capita may be related to other important political characteristics having to do with civic participation and social capital. These characteristics are mostly controlled for by the other covariates in the model, specifically preference votes, judicial efficiency, and electoral volatility. We do not have the data or design to test a social capital hypothesis directly.

[64] The predicted effect is the combined effect of the direct effects of major charge and circulation along with their interaction. Note that the interaction term complicates the interpretation of the direct effect coefficient on major charge.

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TABLE 5
LOGIT ESTIMATION RESULTS FOR THE EFFECTS OF NEWSPAPER CIRCULATION AND CORRUPTION CHARGES ON REELECTION, LEGISLATURES IX–XI

	Leg 9 *REELECT*	*Leg 10* *REELECT*	*Leg 11* *REELECT*
MAJOR	–0.212	–0.476	–0.344
	[0.281]	[0.325]	[0.361]
SENIORITY	–0.077	–0.133***	–0.199***
	[0.052]	[0.047]	[0.075]
RULING PARTY	0.581***	0.929***	–2.713***
	[0.183]	[0.194]	[0.291]
PARTY ELITE	0.096	0.152	0.286
	[0.208]	[0.197]	[0.325]
PREFERENCE	0.593***	0.448***	0.637***
	[0.123]	[0.126]	[0.147]
VOLATILITY	0.001	0.031	0.017
	[0.066]	[0.043]	[0.048]
JUDICIAL EFF	–0.003*	–0.001	0.003
	[0.002]	[0.005]	[0.006]
HIGH CIRC[a]	–0.059	–0.176	0.368
	[0.198]	[0.229]	[0.312]
HIGH CIRC*MAJOR	0.663	0.448	–0.499
	[0.460]	[0.475]	[0.501]
Constant	–5.087***	–4.703***	–6.302***
	[1.191]	[1.289]	[1.828]
Observations	632	639	632

* significant at 10%, ** significant at 5%, *** significant at 1%; standard errors in brackets; all tests are two-tailed

[a] Districts with "high circulation" are defined as those above the median in the criculation of newspapers and periodicals.

one-point estimated effect on reelection rates for Legislature XI as supportive evidence that media coverage and exposure were important in the shift against corrupt incumbents by the Italian electorate. When this evidence is added to that presented in Figure 7, as well to as the growing cross-national and country-specific literature that documents systematic relationships between press freedom, information dissemination, and corruption reduction, we believe a reasonable explanation of the Italian electorate's turn against corrupt incumbent deputies was the increased press coverage of political corruption in the early 1990s.[65]

There is also considerable auxiliary evidence attesting to the importance of the barrage of media attention to political corruption in the

[65] Besley and Burgess 2002; Reinikka and Svensson 2005; Gentzkow, Glaeser, and Goldin 2004; Cole, Healy, and Weker 2008; Ferraz and Finan 2008.

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years beginning in 1993. The Clean Hands investigations, public exposure, and resulting collapse of the postwar party system seem to have undone 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 break with old associations but even so won only 5 percent of lower house seats in 1994. The small traditional parties of the center, which had helped govern the country for four decades, were wiped out.[66]

Nor were these changes merely nominal. More than 70 percent of those elected in 1994 had never served before, compared with the previous parliament in which one-third of the deputies had already served three or more terms.[67] The 1994 elections mark a clear shift to an almost entirely inexperienced and new political elite.[68] 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, which we have already touched on, 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.[69] The data presented in Figure 2 show a gradual reversal in the partisan affiliation of those charged over the course of the postwar era until the final legislature, as the incriminated shift from being overwhelmingly affiliated with the Communist and Socialist parties to those 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 criminal behavior. Only when the distribution of RAP are consistent with what voters suspect to be true about the distribution of actual malfeasance across the partisan spectrum are the RAP incorporated into the voter's electoral calculus. Only in the Eleventh Legislature were RAP overwhelmingly concentrated on members of the ruling parties; perhaps this finally made them credible to the electorate.

[66] Sani 1995.
[67] Katz and Ignazi 1996, 22.
[68] Lanza 1995, 220ff.
[69] We are indebted to one of our reviewers for suggesting this line of argument.

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We see two major weaknesses with this line of thought. First, evidence from the U.S. tells us that voters are distinctly partisan in how they evaluate and react to information about possible criminal malfeasance on the part of elected officials.[70] Although we lack any direct evidence for Italy in this regard, the country's deep partisan divide and well-known ideological polarization suggest that voters would have filtered their evaluations of RAP through a strongly partisan lens, probably to a much 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 representatives, 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 by no means equally gradual. Whereas accusation distributions move gradually toward ruling-party members until the onslaught of accusations in the Eleventh Legislature, we see a significant response by voters only after the wave of press attention to corruption surrounding the Clean Hands investigations. Nor do we not find any evidence that judicial effectiveness (measured at the district level) is related to reelection rates of district deputies.

The variable that seems 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, we therefore 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.[71] 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 the massive increase in press coverage of corrupt elected officials. We conclude that the media were crucial.

[70] Peters and Welch 1980.

[71] See also Zaller 1992; Besley and Burgess 2002; Adserà, Boix, and Payne 2003; Brunetti and Weder 2003; Chowdhury 2004; Reinikka and Svensson 2005; Gentzkow, Glaeser, and Goldin 2004; Freille, Haque, and Kneller 2007; Cole, Healy, and Weker 2008; Ferraz and Finan 2008.

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Concluding Thoughts

A number of issues remain open. First, our study considers the behavior of political elites in the legislature, judiciary, and press, but we are ultimately interested in the electorate. We assume that political accountability rests in the hands of the voters. 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 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 and with varying amounts of public information. Unfortunately, to our knowledge no such individual-level data exist. Instead, we examine the aggregate voter response to each incumbent deputy measured by the electoral 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 theoretically important 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 and province-level newspaper circulation for only the final few legislatures, providing little variation to attribute press coverage of corruption and media exposure to the outcomes for individual deputies. Despite these data limitations, we find evidence consistent with other research that reports 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 efficiency decreased and then increased in the 1980s and 1990s, it was only in the early 1990s that press coverage of corruption rose dramatically—just one year before 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.

Our findings arguably reflect a broader and "widely observed paradox: unpopular corruption and popular corrupt politicians."[72] Our data

[72] Kurer 2001, 63.

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from before 1994 show that Italian voters tolerated allegations of criminality at rates that were not much different than those tolerated by voters in other wealthy democratic societies. In Japan, Reed finds that legislators lose only a few percentage points over their previous vote shares when they are indicted for corruption, whereas those who are convicted actually see increases in their vote shares.[73] 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.[74] 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.[75] Dimock and Jacobson estimate that the ultimate impact of the 1992 House bank scandal, when legislators were exposed as having written bad checks and which resulted in the greatest turnover in the U.S. House of Representatives in forty 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.[76] The survival rate of those implicated in bad check writing was 80 percent compared with 98 percent for those with no overdrafts. Finally, preliminary research on a poor democracy shows that national legislators with criminal records are elected at twice the rate as those without such criminal histories, suggesting that voters in poor countries may be even more susceptible to the appeals of corrupt politicians than voters in their wealthy counterparts.[77]

During the first ten legislative periods we study, our data reveal patterns that appear typical of wealthy democracies. Italian deputies for whom the judiciary requested 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 received a share of preference votes no different from that received by those who were 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

[73] Reed 2005.
[74] Peters and Welch 1980.
[75] Welch and Hibbing 1997.
[76] Dimock and Jacobson 1995; Groseclose and Krehbiel 1994.
[77] Golden and Tiwari 2009.

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by our finding that legislators who looked as though they would have been likely to be charged had the legislature not been dissolved early lost their seats even without having actually been charged. After decades of chronic high-level political malfeasance, and abetted by serious judicial action and, crucially, a strident press corps, Italian voters finally threw the rascals out.

We have presented evidence internal to the specifics of our case but corroborated by other case studies conducted in other countries that the calculus of voters is sensitive to the informational environment. Where the press generates a barrage of new information alerting voters to political corruption, voters respond. We suspect that they respond for two interrelated but distinct reasons that we cannot untangle analytically. First, corruption becomes politically more salient, moving up in the hierarchy of reasons that a voter may prefer one candidate over another. But second, the intensity of the informational shift alerts each voter that she is not alone. The barrage of information on political corruption that the Clean Hands operation engendered also included a barrage of information about the depth of anticorruption sentiment among voters. Interspersed with embarrassing admissions of chronic wrongdoing by some of Italy's most senior politicians was footage of hundreds of thousands of ordinary citizens weeping and cheering in the streets and piazzas of Milan as the public prosecutors strode through the crowds showered by roses. These striking images documented for voters a shared public distaste for political malfeasance and a new willingness to respond electorally. The media attention allowed voters to coordinate across parties and electoral districts, thereby removing enough corrupt politicians to induce an overall change in the national political environment.

What is the external validity of our study? As far as we are aware, there are few other instances in which widespread political corruption has undergone as substantial and permanent a reduction as Italy experienced after the Clean Hands investigations. But that in itself does not undermine our argument and may even strengthen it. It is rare for the informational environment to shift as dramatically as it did in Italy in the early 1990s. All of the cases of major changes in the informational environment of which we are aware document subsequent reductions in corruption and improved political accountability.[78] Indeed, our results help explain why it is so difficult to alter situations of widespread and

[78] Besley and Burgess 2002; Chowdhury 2004; Reinikka and Svensson 2005; Gentzkow, Glaeser, and Goldin 2004; Freille, Haque, and Kneller 2007; Cole, Healy, and Weker 2008; Ferraz and Finan 2008.

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entrenched political corruption. Doing so requires a massive and thorough change in the political information available to the public. Even if the public does not approve of illegal behavior by elected officials and even if the judiciary is zealous in investigating and indicting, it requires an aggressive free press for corruption to become sufficiently salient to enough voters for them to achieve electoral retribution. Anticorruption efforts must therefore be rapid and thorough to succeed, and they must convey the possibility of success to voters. This perfect storm of interacting processes is unusual. Most of the time corrupt incumbents retain public office. Our study sheds light on what would have to occur for them to lose their grip on political power.

If our interpretation is correct, it does not bode well for political accountability in established democracies. The informational environment is currently undergoing a rapid and dramatic change as the ability and willingness of the companies that own much of the television and print media to pay for investigative reporting declines with the shift of information to the Internet. Investigative reporting lacks a business model to support it in the current trajectory toward online content. A free and aggressive press is likely to be critical to protecting the public from a creeping degeneration of the political elite toward greater corruption. Our study suggests that anything that compromises the press potentially compromises democratic accountability.

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