Convicting Corrupt Officials∗
Evidence from Randomly Assigned Cases

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Abstract

Can the judiciary help root out government corruption? This paper exploits the random assignment of court cases to justices who exhibit varying degrees of strictness to examine how convicting corrupt officials affects local government outcomes in the Philippines. I document that convictions improve the management of local public finances and reduce associated corruption. An exploration of mechanisms suggests that legal deterrence effects contribute to these findings. The results further indicate that convictions are effective at stifling corruption among bureaucrats, but not politicians. Consistent with this heterogeneity, convictions reduce electoral competition and consequently weaken accountability for incumbent politicians.

Keywords: Corruption, Judicial Accountability

JEL: K42, D72, H41, O17

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1 Introduction

Government corruption is widely considered a major impedient to development. Yet few studies have examined how it can be effectively combated.¹ Recent empirical research has documented that anti-corruption policies that expose corrupt practices, such as audit interventions or bottom-up monitoring, can have mixed results on local government performance (Olken, 2007; Ferraz and Finan, 2008; Bobonis et al., 2016; Avis et al., 2018; Zamboni and Litschig, 2018). One potential explanation for these mixed results is the extent to which the judiciary responds to the exposure of corruption – i.e., whether corrupt officials are held accountable for their actions and convicted of their crimes. While judicial checks on the executive have long been argued to prevent the abuse of power (Montesquieu, 1748), little is known about how judicial decisions affect local government outcomes.²

This paper sheds light on this question by estimating the causal effect of convicting corrupt government officials on local government units (LGUs) in the Philippines. The Philippines is a particularly well-suited setting in which to investigate this issue. Not only does it suffer from widespread government corruption (Transparency International, 2015), but its judicial system has some unique features that make it possible to identify the causal effect of judicial decisions. A national corruption court – the Sandiganbayan – hears local corruption cases, which are assigned by a lottery to justices (ponentes) who differ in how likely they are to convict. This process generates exogenous variation in the probability of conviction that can be used to estimate the causal effect of convicting local officials on government outcomes.³ Using data from local corruption cases filed with the Sandiganbayan from 1979 to 2015, I examine how the conviction of politicians and bureaucrats affects local revenue, spending allocation and future corruption.

I document three sets of main results that are consistent with reductions in rent seeking by local officials. First, I demonstrate that convictions lead to changes in both revenue and expenditure patterns. The share of locally generated revenue increases by 15 percentage points, which reduces reliance on central government transfers by 18%. The share of spending allocated to the local government administration (e.g. the treasurer’s and mayor’s offices) is reduced by about 6 percentage points, which corresponds to a 10% reduction. This impact on revenue and spending is driven by legal actions against bu-

¹See Olken and Pande (2012) for an overview of the literature on corruption in developing countries.
²There is, however, a large cross-country literature that has studied the relationship between the features of national judicial systems, government corruption, and economic performance (La Porta et al., 1999; Glaeser and Shleifer, 2002; Djankov et al., 2003; La Porta et al., 2004; Leaderman et al., 2005).
³Similar identification strategies that exploit quasi-random assignment to justices or examiners have been used in other contexts to study individual (rather than local government) outcomes, such as the labour supply effects of receiving disability insurance (Maestas et al., 2013; French and Song, 2014), the importance of family welfare cultures (Dahl et al., 2014), the impacts of incarceration (Kling, 2006; Aizer and Doyle, 2015), the consequences of foster care (Doyle, 2007), and the effects of consumer bankruptcy protection (Dobbie and Song, 2015).
revaucrats and persists for up to 15 years after the initial judicial decision. These findings indicate improved management of local public finances and closely align with the national policy objectives of reducing reliance on transfers, decreasing personnel expenses, and increasing expenditure on economic and social services (Bureau of Local Government Finance, 2015). Second, I establish that convictions reduce corruption that is likely associated with the management of local public finances. Convictions reduce the probability of any future corruption case related to the embezzlement of government funds (so called malversation cases) by roughly 50%. The results further indicate that convictions specifically reduce the probability of any future corruption case involving bureaucrats. I find no evidence that convictions reduce corruption involving politicians or corruption unrelated to government funds. Third, I provide suggestive evidence that convictions improve collaboration with the Commission on Audit (COA) and make the COA more likely to issue favourable audit reports. However, this result is potentially also consistent with targeted audits.

Convictions may influence local government outcomes through various mechanisms. The law and economics literatures distinguish between two key channels: incapacitation and deterrence. *Incapacitation* would prevent officials from continuing to engage in corrupt activities by removing them from office (see, e.g. discussion in Polinsky and Shavell, 2000), while *deterrence* would discipline incumbent officials by altering their beliefs about the legal costs of engaging in corruption (in line with Becker, 1968; Becker and Stigler, 1974). In addition to the direct legal effects, convictions could affect the type of individuals in office. This may occur if convictions lower the expected private returns of holding a government position, thereby influencing who pursues a government career. In other words, convictions may change the type of individuals who *self-select* into office (see e.g. Caselli and Morelli, 2004). Alternatively, convictions could aid the *screening and selection* of both bureaucrats and candidates for political office (Fearon, 1999). Finally, convictions may affect incumbent politicians’ incentives to engage in corruption by influencing *electoral accountability* (see e.g. Ferejohn, 1986). Such effects are theoretically ambiguous. On the one hand, convictions may be seen as levelling the playing field and lead to the entry of new (non-corrupt) political candidates, which could increase the competition for office. On the other hand, in the absence of political entry, convictions may weaken key political groups (e.g. via the reputational consequences of corruption convictions) and thus decrease the competition for office.

To determine which of these channels are driving the main results, I perform several additional empirical tests. First, I examine whether convictions affect the likelihood that defendants will be involved in future corruption cases to assess *incapacitation effects*. I find no evidence that convictions make defendants less likely to engage in future corruption, which suggests incapacitation effects are not the key driver of the main results. This is consistent with the fact that judicial processes are lengthy and that defendants may
no longer be in office when judicial decisions are made. Second, I investigate outcomes in neighbouring local governments to test for deterrence effects. Since officials in these LGUs are not implicated in the case, any change in outcomes in these neighbouring governments cannot be due to incapacitation effects. Yet, the results indicate that both budget and corruption outcomes are affected in neighbouring local governments. While these results are consistent with deterrence effects, they do not necessarily rule out the possibility that other forces are simultaneously at play. Therefore, in a third test I explore one such alternative channel – changes in the selection of local officials. To determine whether the main results are consistent with selection effects, I investigate the temporal pattern of the response and document immediate changes in audit and budget outcomes following case decisions. This finding suggests that the results are not driven by lengthy processes, which would be necessary for the selection of both politicians, due to 3-year electoral terms, and bureaucrats, due to their security of tenure. For politicians, I can go further in a sub-sample of the data and document suggestive evidence that the characteristics of local leaders do not change in response to convictions. Finally, I assess how convictions affect electoral accountability by leveraging the central role of families in Philippine politics and study the impact of convictions on electoral outcomes for defendants and their relatives. Convictions diminish the electoral support for (and participation of) convicted families and do not induce new candidates to run for office. As a consequence, political competition is reduced in the local government and incumbent officials therefore face weaker electoral accountability.

The evidence on mechanisms offers a potential explanation for why convictions may not reduce rent seeking among politicians: they also contribute to lowering electoral accountability. To explore whether the impact on political competition can explain the lack of response among politicians, I exploit heterogeneity within the group of political defendants that I posit would generate a differential response in political competition – and therefore also in rent-seeking behaviour among incumbents. First, I establish that convicting political opponents of the incumbent leads to a greater reduction in political competition and find suggestive evidence that point estimates on future corruption are closer to zero. Second, I find that convicting term-limited politicians (which has no impact on political competition) leads to a reduction in future corruption. These results suggest that convicting politicians can reduce corrupt behaviour, provided there are no detrimental effects on political competition.

This study most closely relates to research on the effect of policies designed to curb corruption (Di Tella and Schargrodsky, 2003; Olken, 2007; Ferraz and Finan, 2008, 2011; Litschig and Zamboni, 2015; Bobonis et al., 2016; Avis et al., 2018; Zamboni and Litschig, 2018). A key part of this literature focuses on monitoring or auditing interventions, which

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4The paper is also related to an empirical micro literature studying the causal effect of judicial reforms on firm performance and economic activity (Chemin, 2009; Lichand and Soares, 2014; Chemin, 2018).
have produced mixed results. For example, Olken (2007) finds that increasing government audits reduced corruption in village road projects in Indonesia, while bottom-up monitoring by citizens had no effect. Bobonis et al. (2016) find that audit reports that were released just before an election, rather than after, affected short-term municipal corruption in Puerto Rico but had no long-term effects. The three papers most closely related to the current article are Litschig and Zamboni (2015), Zamboni and Litschig (2018) and Avis et al. (2018), all which investigate corruption in Brazil. Litschig and Zamboni (2015) determine that rent extraction is lower in Brazilian municipalities where state judiciaries are located (i.e. in localities with higher de jure judicial accountability). Zamboni and Litschig (2018) find that an increased risk of audit in Brazil reduced corruption differently depending on the probability of being sanctioned, but had no impact on public service provision. Avis et al. (2018) document that being subject to random audits reduced the amount of corruption in future audits and increased the likelihood of legal action in a municipality; they highlight the need to better understand how to enhance judicial accountability where corruption is endemic.

This paper contributes to the literature by identifying the causal effect of judicial decisions. The evidence generated by previous studies relies on random variation in audits and thus assesses the impact of an increased risk of detection of corruption, but not the causal effect of judicial sanctions. This is because the legal sanctions imposed are potentially endogenous; for example, they depend on the severity of the corruption or local judicial quality. Understanding the role of judicial sanctions is important for designing effective anti-corruption policies, since it can help shed light on whether policy makers can combat corruption simply by adjusting sanctions without modifying the likelihood of detection. Such insights may be particularly useful for fighting corruption given the difficulty of detecting and exposing corrupt behaviour. Unlike earlier work, I study decisions in specific corruption cases, which allows me to investigate how decisions in different types of cases (such as cases against bureaucrats versus politicians) shape government performance. The outcome data also permits me to estimate the long-run effects of an anti-corruption intervention. This is particularly important given prior arguments about the difficulty of bringing about persistent change in settings where corruption permeates the government (see e.g. Myrdal, 1968; Tirole, 1996; Bardhan, 1997; Mishra, 2006).

The paper is organized as follows. Section 2 provides an overview of the working procedures of the national corruption court, describes the structure of the local government and discusses the role of families in politics in the Philippines. It also briefly summarizes the data used in this study. Section 3 explains the empirical framework employed to estimate the causal effect of convictions and discusses how to interpret the instrumental

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5 Although it concentrates on a low-corruption, developed country, Alt and Lassen (2008)’s work on the US is also related. They establish that corruption is lower in states where judges are elected rather than appointed.
variables (IV) estimates and potential threats to identification. Section 4 reports the main results on budget outcomes, future corruption, and financial audits and discusses alternative treatment parameters. I then explore potential mechanisms in Section 5 and discuss why the results may differ for politicians and bureaucrats. Section 6 concludes.

2 Background and Data

2.1 Sandiganbayan’s Judicial Procedure

The Sandiganbayan is the national anti-corruption court of the Philippines. It has exclusive original jurisdiction over corruption cases committed by officials with a salary grade 27 or higher (62,670 pesos = 1,400 U.S. dollars), which includes local politicians and bureaucrats such as mayors, vice-mayors, councillors, budget officers and treasurers.

Figure 1 outlines its typical judicial procedure, from complaint to verdict. First, the Office of the Ombudsman initiates an investigation against a government official – either on its own or based on a complaint from the general public. If the investigation establishes probable cause, the ombudsman files charges with the Sandiganbayan. The case is then consolidated with any cases filed by other investigative authorities (such as the COA) pertaining to the same incident and is allocated through a public raffle to a division of the Sandiganbayan. The three justices in the assigned division handle all matters related to the case, and it cannot be reassigned to another division. Once a case has been assigned, the accused must be arraigned within 30 days. Following the arraignment, the defendant is typically suspended from holding any public office for 90 days.

Once a trial has been held, the chairman of the division assigns a member of the division in a second raffle (the ponente) to study the case and propose a verdict within 90 days. Since the penal code determines the sentence ranges for each offence, the ponente is mainly charged with deciding whether to suggest a conviction or acquittal. All three justices in the division deliberate before the ponente’s proposed decision becomes final (Republic of the Philippines Supreme Court, 2002). After the deliberation, the decision is signed and immediately promulgated to the accused and the public.

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6 Under the Philippine constitution, the Office of the Ombudsman is the main body responsible for monitoring the government on issues related to corruption, and is obliged to investigate all cases filed with it.

7 This follows from the 1982 amendment of the Anti-Graft and Corrupt Practices Act (Congress of the Philippines, 1960; National Assembly of the Philippines, 1982) and applies to all government officials who have been prosecuted under this act or for any offence involving fraud upon government or public funds/property.
Figure 1: Assignment of Corruption Cases

Notes: This figure illustrates the procedure for hearing corruption cases in the Philippines. Complaints filed with the ombudsman are investigated. If probable cause can be established, the case is filed with the Sandiganbayan. Cases are assigned to divisions in public weekly raffles. Within the division, a second raffle assigns cases to a ponente (one of three justices in the division), who is responsible for drafting the verdict.

Source: Figure constructed by author.

2.2 Local Government Funding and Responsibilities

The analysis in this paper focuses on the LGUs that are responsible for providing key public services: cities and municipalities. The Local Government Code of 1991 granted them autonomy. Most (80%) of their revenues comprise central government transfers, which are determined by a fixed formula based on population and land area. However, LGUs can also collect local taxes and fees. Increasing the share of revenue that is generated locally is a priority for the central government: it is one of the criteria used in the Bureau of Local Government Finance’s fiscal sustainability scorecard.

The chief executive of the LGU is the mayor, who enjoys substantial discretionary power over how to allocate the local budget (see e.g. Labonne et al., 2015). However, given their high dependence on transfers and a requirement to balance their budget, local officials have less influence over the LGU’s total revenue and expenditures. The vice-mayor is the second in command; he or she signs all draws on the local treasury and takes over if the mayor’s position is vacated. Since the fall of the Marcos regime in 1986, these positions have been elected separately in local first-past-the-post elections.

Bureaucrats are also key figures in the operation of the local government. Each LGU has several offices headed by officials with responsibilities including tax collection, accounting, budgeting and providing public services. These are permanent positions that are typically appointed by the mayor subject to civil service law and regulations. The exception is the local treasurer, who is appointed at the national level by the Secretary of...
Rebucrats cannot be removed or suspended without just cause and due process (Congress of the Philippines, 1988). Statistics from 2008 suggest that the number of new hires in the local bureaucracy roughly corresponds to the number of separated officials, and that most public officials leave due to exogenous factors, such as retirement or resignation (Civil Service Commission of the Philippines, 2008). Appendix Section A.3 lists the key officials, their main responsibilities, and outlines the process for appointing and removing local bureaucrats.

2.3 Politics and the Role of the Family

In the Philippines, family ties play an important role in politics. At both the national and local levels, relatives take turns holding office or hold several different positions in the same government. Previous studies have identified multiple implications of this family involvement. For instance, families in political office provide benefits to their relatives (Querubin, 2016; Fafchamps and Labonne, 2014). Partly to curb the influence of political dynasties, binding term limits were introduced in 1987 to prevent individuals from serving more than three consecutive terms. However, the intervention seems to have been largely unsuccessful in restricting the power of families (Querubin, 2011). Labonne et al. (2015) show that female dynastic candidates related to the incumbent were more likely to be elected after the introduction of term limits, and there was no change in policy or economic outcomes.

2.4 Data

To investigate the impact of convictions on individual as well as local government outcomes, I construct two main samples. The case sample includes the first corruption case against an individual and the outcomes of the defendants in those cases, while the LGU sample focuses on the first case disposed in a LGU and the corresponding LGU outcomes. In this section, I first describe the data on corruption cases, which forms the foundation of both samples. I then briefly discuss the main data sources and outcomes for the respective samples in Sections 2.4.1 and 2.4.2. Details on data construction and descriptive statistics are reported in Appendix B.

The corruption data contains all Sandiganbayan decisions against officials at the municipality/city level from 1979 to 2015. It includes 1,073 consolidated cases against 1,968 defendants occurring in 669 LGUs. Each case contains information about the date of filing and disposal, the assigned division/ponente, the type of offence, the decision taken, and the names and positions of the accused. Figure 2 illustrates that these cases are spread

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out across the country, and Appendix Figure B1 demonstrates that they are evenly distributed over time. In most LGUs, only a small number of corruption cases were brought to a final decision during the sample period: 65% had one, 21% had two and 10% had three cases. While a few cases involved multiple defendants, 70% were against a single defendant. Politicians and bureaucrats are both well represented in these cases; about half involved at least one politician. Table B1 provides additional details about the defendants’ positions in the government. The two most common political positions were mayors (57%) and councillors (37%); for bureaucrats, the most common were treasurers (35%) and revenue collectors (9%). Cases brought to a final decision take an average of 4.6 years to process from the year they were first filed, and 46% result in at least one individual being convicted. In 80% of these convictions only one individual was convicted and in less than 5% of cases more than three were convicted. Almost two-thirds of cases (63%) involved some non-zero monetary amount: I refer to these cases as financial. Nearly three-quarters of all cases are classified as either malversation of public funds (38%) or anti-graft and corrupt practices (34%). The former refers to cases in which public officials have failed to account for government funds they are responsible for managing, and the latter is a broader category that includes, for example, giving a private party unwarranted benefits, persuading other officials to violate regulations, or requesting gifts or payment for providing a government license or contract. Table B2 lists additional offence categories.

2.4.1 Case Sample

The case sample is based on all 1,073 consolidated cases. Table B5 reports summary statistics for this sample. To investigate the impact on the defendants in these cases, I link them to electoral records and other corruption cases filed with the Sandiganbayan. Given the importance of families in Philippine politics, I construct outcomes for both the individuals involved in the case and their relatives using their location and a 2-gram vectorial decomposition algorithm to match names. This procedure helps account for misspelled names, which are common in this setting. Individuals are matched based on first and last names within an LGU, while relatives are matched on last names within an LGU. This strategy enables me to identify relatives and individuals due to the Spanish naming conventions used in the Philippines since the colonial period. Previous work on family ties in the Philippines has employed similar strategies (see e.g. Querubin, 2011; Fafchamps and Labonne, 2014).

Electoral Records: To capture participation and performance in elections, I rely on data from the Commission on Election (COMELEC) on the 10 local elections held in
between 1988 and 2016. The data contain the names of the winners of all 10 elections, and those of all candidates running for office for all but the first election. I use this data to construct indicators for whether defendants and their relatives ran for (or held) a mayor or vice-mayor position in the LGU following the case disposal. As shown in Panel B of Table B5, defendants and their relatives participate successfully in local elections following case disposals. In 44% of cases, at least one family member ran for office afterwards, and in 24% of cases they became a mayor or vice-mayor.

**Future Corruption Cases:** To capture whether defendants and their family members continue to engage in corruption following a disposal, I construct indicators for whether defendants are involved in a future corruption case using data on all local corruption cases filed with the Sandiganbayan after the disposal, including cases that have
not reached a decision. Panel C of Table B5 shows that in 6% of cases, family members are involved in a future corruption case in the LGU. For completeness, I also consider all cases filed against individuals with the same family name, irrespective of locality, which generates a substantially higher share of individuals implicated in a future corruption case. However, I place less weight on these figures since they may be partly driven by individuals from other families with the same name.

2.4.2 LGU Sample

The LGU sample is based on the first consolidated case in each of the 669 LGUs that ever experienced a disposal. Table B6 reports summary statistics for this sample. As shown in Panel A, the characteristics of these cases are very similar to the full set of consolidated cases. The cases in the LGU sample are matched to the following local government outcomes.

**Local Budgets:** Data on local revenue and spending from 1992 to 2016 were collected from the Bureau of Local Government Finance (BLGF). Two main outcomes of interest, which can be consistently observed during the entire study period, are constructed for each LGU-year: (1) the local revenue share, which is the portion of revenue not stemming from the central government’s Internal Revenue Allotment (IRA) and (2) the share of current expenditure allocated to general government administrative expenses. These two outcomes closely follow what the BLGF has identified as key priorities for improving local fiscal performance in the Philippines: reducing reliance on central government transfers and spending less on personnel and more on economic and social services (Bureau of Local Government Finance, 2015). Panel B of Table B6 shows that the local revenue share averages 21%, while 55% of spending is dedicated to administrative expenses. I also construct yearly per capita measures for local revenue and for total revenue and expenditure. For the years 2001–2016, the budget data contain more detailed information on how spending is allocated. For each LGU-year, outcomes are created to capture the share of expenditure allocated to housing, health, social services, education, economic services, and labour and employment. Table B3 reports the summary statistics for these variables.

To directly test the impact on fiscal sustainability, I also collected data from the BLGF’s 2012 Fiscal Sustainability Scorecard. The scorecard is based on a wide range of indicators related to revenue generation, expenditure management and the timely

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11 Appendix Table B4 presents the sub-components for both general government expenditure and local revenue using data from 2010–2014, which contains more detailed information. This table illustrates that local revenue is mainly from local business tax (36%), local property tax (27%), local income from economic enterprises (10%), and local regulatory fees from permits and licenses (6%). Over one-third of general government expenditure (36%) is allocated to the office of the mayor. Large portions of funding were also provided to local legislatures (11%), the public order and safety department (11%), and government offices with responsibilities related to public finances (the treasurer, accountant, assessor and budget officer obtained a combined 13%).
submission of relevant financial reports. Based on these criteria, each LGU is given an overall grade ranging from poor to excellent. On average, 27% of LGUs are rated good or above. Appendix Section B.4 reports further information about this data.

Future Corruption Cases: To investigate the impact on future corrupt practices within the LGU, I follow the same approach as above and construct measures of future corruption cases linked to the local government. I determine whether any corruption cases were filed after the first disposal and construct an indicator that is coded 0 before this case was filed and 1 afterwards. I construct similar indicators for the type of corruption and offence category as well as the defendant’s position in the government. These outcomes cover the years 1980–2015 – i.e. from the year after the first disposal until the year of the final disposal. Panel C of Table B6 presents summary statistics for these variables.

Audit Reports: I collected all available audit reports from the COA’s central archives in Manila to assess local governments’ audit performance. The COA is mandated to conduct annual audits that assess the fairness of all LGUs’ financial records as well as their compliance with prescribed rules and regulations. However, this is not always possible if local governments do not cooperate and provide the required documentation. A high-ranking COA official interviewed for this study suggested that withholding financial documentation is a likely indication of corruption.\(^\text{12}\) I reviewed all COA records to construct outcomes that capture both the likelihood that an audit report was compiled and the audit opinion expressed in that report. Using the archives from 1987 to 2018, I constructed four outcome variables: an indicator for whether any report was issued for the LGU in a given year, an indicator for whether financial statements were presented fairly (i.e. an unqualified or qualified opinion was issued), an indicator for whether financial statements were not presented fairly (i.e. an adverse or disclaimer opinion was issued) and an indicator for whether the LGU was mentioned in a summary of the audit report but no opinion was given due to missing data. An audit report was issued in 38% of the LGU-year observations, and most of these reports assessed that the financial statements were presented fairly. Appendix Section B.2 describes the data and construction of the variables in detail.

Electoral Records: Using data from COMELEC, I construct two categories of outcomes to investigate the impact on the political environment.

The first category consists of three separate measures of political competition in the mayoral election: whether there was any competition for the mayoral position (i.e. if more than one candidate ran for office), the Herfindahl index of political competition and the margin of victory. Since the 1988 data only includes information on the winning candidate, these outcomes have been constructed for the 1992–2016 elections. Panel E of Table B6 reports summary statistics, which indicate low levels of political competition: only one candidate ran in 6% of the elections, and the average margin of victory was 27

\(^{12}\)Interview with the Director of the Government Accountancy Office, 6 November 2019, COA, Manila.
The second category of outcomes comprises two types of measures of candidate quality to capture selection effects. The first type includes indicators for whether the mayor or their family had a corruption case filed against them before the election. These outcomes have been constructed for the 1992-2016 mayoral elections. The summary statistics suggest that 13% of the elected mayors had a corruption case filed against their family before the election. The second type is the candidate’s predicted vote share in the 2016 election. Data on candidate characteristics have been compiled from candidates’ handwritten certificates of candidacy, which are only available for the 2016 election. For each candidate, COMELEC collected information on their occupation, place of birth, age, gender, and marital status. Appendix Section B.3 provides additional information about the processing of the electoral data and the construction of the outcome variables.

3 Empirical Framework

3.1 Identification

The paper’s main goal is to estimate the causal effect of convictions in corruption cases on both defendant and LGU outcomes. The key empirical challenge is that there are likely to be systematic differences between cases that result in convictions and those that result in acquittals. For example, if the judiciary is able to identify the truly corrupt individuals, then the cases that result in a conviction will involve individuals who are, on average, more corrupt. Hence, a simple comparison of cases will not yield an unbiased causal estimate of the effect of conviction.

Instead, this study exploits the fact that cases are effectively randomly assigned to ponentes. To the extent that ponentes vary in their strictness, this creates potential exogenous variation in the probability of conviction. Panel (a) of Figure 3 illustrates that this is the case: there is substantial variation in strictness across the 41 ponentes in the sample. Panel (b) depicts one source of the variation in strictness – the ponente’s judicial experience. Those who have more years of experience at the time of the case disposal tend to be stricter.\textsuperscript{13}

3.2 Instrumental Variable Construction

To construct a measure of ponente strictness ($z_{pl}$) that is exogenously determined for a given case, I follow the approach used in previous studies that exploit random assignment to justices or examiners (see e.g. Kling, 2006; Doyle, 2007; Maestas et al., 2013; French \textsuperscript{13}Since experience is not exogenously determined, it is unclear whether ponentes learn to become stricter, or if there is endogenous selection into acquiring experience for stricter ponentes.
Figure 3: Ponente Strictness

(a) Conviction Rate by Ponente

(b) Evolution of Conviction Rate

Notes: This figure displays the conviction rate by ponente as well as the correlation between the conviction rate and the ponente’s experience at the time of disposal.

Source: Constructed by the author.

and Song, 2014; Dobbie and Song, 2015; Aizer and Doyle, 2015).

I use the following equation:

\[ z_{pl} = \frac{CON_p - CON_{pl}}{CASES_p - CASES_{pl}}, \tag{1} \]

where \( CON_p \) is the total number of convictions handed down by ponente \( p \) and \( CASES_p \) the total number of cases assigned to the same justice. \( CON_{pl} \) and \( CASES_{pl} \) indicate the number of convictions and cases in LGU \( l \) assigned to the ponente. Hence, the instrument, \( z_{pl} \), for a case in LGU \( l \) assigned to ponente \( p \) is the share of all cases dealt with by that ponente concerning other LGUs that resulted in a conviction (i.e. the leave-out mean); it takes a value between 0 and 1. To obtain valid estimates of the strictness measure I only use ponentes who have been assigned more than 11 cases to construct this leave-out mean (see Greene, 2001, for a discussion of the appropriate sample size to estimate fixed effects).

3.3 Instrument Validity

This section reports on several tests to check the validity of the identification strategy used in this study.

14Using ponente fixed effects as instruments would not satisfy this requirement in a finite sample, since it would induce a mechanical correlation between a case’s outcome and the estimate of the fixed effect. If the number of cases for each ponente approached infinity, the estimate would be consistent. However, since the sample size in this study is relatively modest, the bias could potentially be large if the suggested correction is not applied.

15I exclude other cases in an LGU that were assigned to the same ponente to rule out dependence across cases within an LGU. This is particularly important for the LGU-level analysis. Including these cases results in a stronger instrument and consistent results.
First Stage: The first requirement is that *ponente* strictness, as defined above, significantly affects whether a case results in a conviction. The following first-stage equation is estimated to test whether this is the case:

\[ c_i = \alpha_1 z_{pl} + \gamma_j + \alpha_2 X_i + e_i, \]  

(2)

where \( c_i \) is a variable indicating whether the consolidated case \( i \) resulted in any convictions, \( z_{pl} \) is the conviction rate of the *ponente* assigned to the case, defined according to Equation (1), \( \gamma_j \) represents disposal year fixed effects and \( X_i \) is a vector of case-specific controls. Disposal year fixed effects are key for identification, since the *ponente* distribution changes over time. Case controls are included for precision, and contain fixed effects for the offence category, the number of individuals involved in the case, whether the case was financial, whether it was in a city or municipality, as well as controls for the number of politicians/bureaucrats, a quadratic polynomial of the number of neighbouring LGUs and the processing time.\(^\text{16}\) Following the literature exploiting random assignment to justices and examiners (see e.g. Maestas et al., 2013; French and Song, 2014), I cluster standard errors at the *ponente* level to address potential dependence across cases assigned to the same *ponente*.

Table 1 presents the results from running this specification: a *ponente*’s conviction rate is highly predictive of whether a case results in any convictions. Column (1) reports the baseline estimate, which controls for disposal year fixed effects; it shows that a case that is assigned to a *ponente* who, on average, is 10% more likely to convict in other cases is about 5% more likely to result in a conviction. The point estimate is very similar when adding the case-specific controls, which strengthens confidence that the randomization was successful. The third column reports the results on the number of convicted defendants instead of a dummy indicating any conviction, and shows that the same increase in strictness leads to 0.76 additional individuals being convicted. The corresponding results for the restricted sample, which only include the first case disposed in an LGU, are reported in columns (4)–(5) and show slightly larger estimates. Column (6) presents the effect of being assigned to a strict *ponente* in the first case on there ever being a conviction in the LGU (including all future corruption cases). The point estimate for this outcome suggests that being assigned to a 10% stricter *ponente* in the first case significantly increases the probability that the LGU will ever experience a conviction by 5.1%. This result suggests that subsequent decisions in the LGU did not counteract the decision taken in the first case. In other words, LGUs are not experiencing enough corruption cases to make the decision in the first corruption case irrelevant. All first-stage

---

\(^{16}\)I control for a quadratic polynomial of the number of neighbours since decisions in neighbouring LGUs affect outcomes – see the discussion in Section 5.1. I include the control for processing time to capture potential differences in processing time between *ponentes*, which may influence outcomes. Appendix D.1 discusses the exclusion restriction.
estimates are highly statistically significant: the F-statistic is 22–55, depending on the specification.

Table 1: First Stage

<table>
<thead>
<tr>
<th></th>
<th>Case Sample</th>
<th></th>
<th></th>
<th>LGU Sample</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Conviction</td>
<td># Conv.</td>
<td>Conviction</td>
<td># Conv.</td>
<td>Ever Conv.</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td>Ponente Strictness</td>
<td>0.48***</td>
<td>0.49***</td>
<td>0.76***</td>
<td>0.63***</td>
<td>1.09***</td>
<td>0.51***</td>
</tr>
<tr>
<td></td>
<td>(0.067)</td>
<td>(0.073)</td>
<td>(0.16)</td>
<td>(0.085)</td>
<td>(0.16)</td>
<td>(0.091)</td>
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<tr>
<td>F Statistic</td>
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<td>45.8</td>
<td>22.3</td>
<td>54.5</td>
<td>47.8</td>
<td>31.0</td>
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<td>Observations</td>
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<td>669</td>
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<tr>
<td>Disposal Year FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Case Controls</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Notes: This table reports the first-stage results for the main samples. Case controls include fixed effects for offence category and the number of individuals in the case as well as controls for whether the case was financial, whether it was in a city, the number of politicians/bureaucrats/neighbours and the processing time. Robust standard errors clustered at the ponente level in parentheses. Statistical significance is indicated by *** at 1%, ** at 5%, and * at 10%.

Random Assignment: The core identification assumption is that the randomized allocation procedure outlined in the Sandiganbayan rulebook is properly followed. If it is, the instrument should be balanced across predetermined case characteristics. Table 2 reports the results from such a balance test for the full sample of consolidated cases, conditioning on disposal year fixed effects. For comparison, the results are reported for both the instrument ($z_{pl}$) and for a regression on actual convictions ($c_i$).

The results reported in Table 2 illustrate that convictions are strongly correlated with case characteristics. Cases against politicians are less likely to result in a conviction than those against bureaucrats. Financial cases have a substantially higher conviction rate than those not involving money, as do those against local officials who are appointed centrally by the Department of Finance (rather than by the local government). If the instrument is used instead, these differences lose statistical significance and the point estimates are substantially smaller. As a final check, the last two rows of Table 2 report the results for the predicted probability of conviction based on the case characteristics for both the full sample and for the first case in the LGU. The outcome in this specification is the predicted value of regressing the conviction dummy on the listed characteristics in the table as well as 16 region fixed effects and 43 offence category fixed effects. For both samples, the predicted conviction rate is strongly related to the actual conviction rate, but not to the instrument. These results underline the importance of using the IV approach to obtain unbiased estimates of the effect of convictions.

Exclusion Restriction and Monotonicity: The other two assumptions required for identification are the exclusion restriction and monotonicity assumptions. The former
### Table 2: Covariate Balance

<table>
<thead>
<tr>
<th></th>
<th>Association with Conviction</th>
<th>Association with Instrument</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Coefficient</td>
<td>T-stat</td>
</tr>
<tr>
<td>Bureaucrat</td>
<td>0.143</td>
<td>4.491</td>
</tr>
<tr>
<td>Politician</td>
<td>-0.208</td>
<td>-5.238</td>
</tr>
<tr>
<td>Mayor</td>
<td>-0.170</td>
<td>-4.125</td>
</tr>
<tr>
<td>In-Office</td>
<td>-0.039</td>
<td>-1.855</td>
</tr>
<tr>
<td>Financial</td>
<td>0.174</td>
<td>4.800</td>
</tr>
<tr>
<td>City</td>
<td>-0.058</td>
<td>-2.311</td>
</tr>
<tr>
<td>DOF</td>
<td>0.267</td>
<td>6.049</td>
</tr>
<tr>
<td>Individuals</td>
<td>-0.295</td>
<td>-2.009</td>
</tr>
<tr>
<td>P(Convicted</td>
<td>X)</td>
<td>0.177</td>
</tr>
<tr>
<td>P(Convicted</td>
<td>X)</td>
<td>0.157</td>
</tr>
</tbody>
</table>

Notes: This table reports results on predetermined case characteristics. Each row in each panel is a separate regression. Outcomes are variables indicating whether the case involved a bureaucrat, a politician, a mayor, any individual currently in office, any monetary amount (i.e. a financial case); whether it occurred in a city (rather than a municipality); whether it involved officials appointed by the Department of Finance, DOF (rather than locally by the Department of the Interior and Local Government); as well as the number of individuals involved. All regressions control for disposal year fixed effects. The first three columns report the results of regressing these characteristics on a dummy that indicates whether a case resulted in a conviction or not, whereas the following three columns use the instrument instead. P(Convicted| X) is a predicted propensity for being convicted using the case characteristics listed above as well as 16 region and 43 offence category dummies. Standard errors are clustered at the ponente level.

requires that ponente strictness does not affect outcomes through other channels than convictions, while the latter requires that cases that result in conviction by a lenient ponente would also result in conviction by a strict ponente. I investigate the support for these assumptions in Appendix D.1 and find evidence suggesting that they hold in this setting.

### 3.4 Empirical Specifications

To estimate the causal effect of convictions, Equation (2) is used as the first stage in a set of two-stage least squares regressions. The exact specification differs depending on the nature of the outcome and the available data. All of the IV specifications seek to estimate the impact of any conviction in the consolidated case considered, relying on the first-stage estimates in columns (2) and (4) of Table 1. Hence, the estimates reflect the extensive margin effect of any conviction for the compliers. Section 3.5 elaborates on the interpretation of the IV estimates and discusses alternative treatment parameters.

Case Specification: To study the impact of convictions in the case-level sample, I combine the first stage described above with the following second stage:

\[\text{An alternative way to scale the coefficient is by the number of convicted individuals (30% of the cases involve more than one defendant). This would generate smaller IV estimates, reflecting the larger first-stage coefficients in Table 1.}\]
\[ y_i = \beta_1 c_i + \gamma_j + \beta_2 X_i + e_i, \]  

(3)

where \( y_i \) is the outcome of interest for defendants in consolidated case \( i \). All other variables are specified as above. The coefficient \( \beta_1 \) captures the effect of conviction (instrumented by *ponente* strictness) for compliers.

**LGU Specifications:** At the local government level, outcomes are typically available for several years before and after the conclusion of the first case. Therefore, I primarily use the following first and second stages:

\[ d_{lt} \times c_i = \alpha_1 d_{lt} \times z_{pl} + \lambda_{jt} + \alpha_2 X_i + e_{lt}, \]  

(4)

\[ y_{lt} = \beta_1 d_{lt} \times c_i + \lambda_{jt} + \beta_2 X_i + e_{lt}, \]  

(5)

where \( y_{lt} \) is the outcome of interest for LGU \( l \) in year \( t \), and \( d_{lt} \) is an indicator for all time periods after the disposal of the first corruption case in LGU \( l \). The variable \( c_i \) is an indicator for whether the first corruption case resulted in a conviction or not, which is instrumented by the *ponente* strictness \( (z_{pl}) \) for that case. Fixed effects for the disposal year \( (j) \) of the first case in the LGU interacted by time are indicated by \( \lambda_{jt} \). These fixed effects ensure that the only variation exploited for identification is that induced by the assignment to *ponentes* with different degrees of strictness across cases disposed in the same year (i.e. the variation stemming from the timing of disposals is absorbed). All other variables are specified as above. The coefficient \( \beta_1 \) captures the effect of the first corruption case in an LGU resulting in a conviction versus an acquittal.

Finally, to determine the temporal response to convictions, I also estimate the following flexible reduced-form specification:

\[ y_{lt} = \sum_{k=-b}^{+a} \theta_k d_{lt}^k \times z_{pl} + \lambda_{jt} + \delta X_i + e_{lt}, \]  

(6)

where the \( d_{lt}^k \)s are event-time dummies equal to 1 for time periods before and after the disposal of the first case in an LGU \( l \). The number of time periods before \( (b) \) and after \( (a) \) the event depends on data availability for the outcome studied. The \( \theta_k \)s capture the reduced-form impact on the outcome at time \( k \) of the first case being assigned to a stricter *ponente*.

\(^{18}\)Note that 65% of LGUs had only one corruption case concluded during the sample period. If all corruption cases were considered, LGUs with many cases would be oversampled. Later corruption cases might also be influenced by the outcomes of previous cases. Indeed, the analysis of future corruption cases in Section 4 suggests that this is the case.
3.5 Interpreting the IV: LATE and Other Parameters

The IV estimates in this study represent the local average treatment effect (LATE) for cases that would have resulted in another decision if they were assigned to a different ponente. To shed light on what this LATE captures and how it relates to the average treatment effect, I perform two additional analyses.

First, I characterize the share of compliers and their characteristics following the approach developed by Abadie (2003) and extended by Dahl et al. (2014). Appendix C provides a detailed description of the procedure and calculations. I find that 50–58 percent of cases are “compliers”, i.e., the decision would have been different if they had been assigned to the strictest instead of the most lenient ponente. A further 20–28 percent of cases are “never takers”, meaning they would be acquitted by all ponente, and 22 percent are “always takers”, i.e., they would be convicted by all ponente. The characteristics of compliers are comparable to those in the main sample on most observables (e.g. similar shares of politicians, mayors and bureaucrats, similar likelihood of being financial and having the baseline characteristics that predict conviction). However, complier cases are less likely to be from a city or to involve more than one defendant, and officials in complier cases are less likely to be in office when the decision is handed down. To benchmark IV estimates against the relevant population I use the same approach to calculate the expected value of the outcome for the non-treated complier population and report this for all main results.

Second, I use the marginal treatment effects (MTE) framework to uncover three additional treatment parameters: the average treatment effect (ATE), the average treatment effect on those who were convicted (ATT) and the average treatment effect on those who were acquitted (ATUT). Appendix C provides a detailed description of the procedure and calculations. In short, I follow prior work (see e.g. Carneiro et al., 2011; Cornelissen et al., 2016; Andresen, 2019; Bhuller et al., 2020) to estimate marginal treatment effects for different levels of unobserved resistance to treatment. These can be identified under the IV assumptions above and the additional assumption of separability between observed and unobserved heterogeneity in the treatment effects. By aggregating the distribution of marginal treatment effects, I can then uncover the ATE, ATT and ATUT (Heckman and Vytlacil, 1999).

4 Main Results

4.1 IV Estimates

Local Public Finances: I start by investigating how convictions affect local government revenue and spending. Figure 4 reports the results from estimating the reduced-form Equation (6) for 5-year bins before and after the disposal for the two main outcomes,
while Table 3 reports IV estimates from Equation (5).

Figure 4 (a) illustrates that convictions cause an increase in the locally generated revenue share that persists for at least 15 years after the disposal and that there is no difference in revenue generation before the disposal for LGUs assigned to differently strict ponente. Figure D1 concentrates on the time of disposal and shows that the strongest response is in the year after the decision. Table 3 reports LATE estimates and documents that convictions increase the local revenue share by 15 percentage points following a conviction, and generate a 23-percentage-point increase in the first year after disposal. These results suggest a substantial increase in the local revenue share and an 18% lower reliance on central government transfers in the post period on average, since transfers constitute 83% of revenue among non-convicted compliers. Appendix Table D3 sheds further light on the drivers and consequences of this shift. It documents that the change in the local revenue share is driven by an increase in local revenue per capita rather than changes in central government transfers, which is in line with the fact that the IRA is determined by a fixed formula. In addition, these estimates demonstrate that the increase in local revenue is not strong enough to generate a statistically significant increase in overall income or expenditure per capita – likely because local revenue constitutes a limited share of LGUs’ overall budgets and due to the balanced budget requirement.

Figure 4 (b) displays the impact of corruption case decisions on the allocation of spending. It illustrates that the share of spending allocated to administrative expenses substantially declines in the 5 years immediately after a conviction. Estimates remain similar for subsequent years, but are imprecisely estimated. Point estimates for the period before the disposal indicate no differences in levels or trends in spending allocation. Figure D1 displays yearly estimates around the disposal: the strongest response is in the year immediately following the decision. In line with the reduced-form temporal results, the estimates in Table 3 suggest a large impact in the year immediately following the disposal (a 10-percentage-point reduction reported in column (5)) and an average reduction of 6.2 percentage points in the full post-period reported in column (6). To investigate how these reduced administrative expenditures are allocated, I exploit data from 2001 to 2016, for which more detailed information on spending is available. Appendix Table D4 presents the results. Column (1) confirms that there is a similar and even slightly larger reduction in general expenditure during this period. These funds are reallocated to education and economic services (such as agricultural extension services and local public works); there are no statistically significant changes in the share of expenditures in other categories. The increase in spending on economic and education services aligns with the BLGF’s priorities; it argues that these categories are related to “poverty alleviation and improvement in the human development index” (Bureau of Local Government Finance, 2015).

To understand who is driving the change in local public finances documented above,
I compare corruption cases involving politicians vs. bureaucrats. Columns (3) and (7) of Table 3 establish that the response is significantly weaker for cases involving politicians. Hence, the effects are predominantly driven by cases against bureaucrats, which increase the local revenue share by an estimated 20 percentage points and decrease the administrative expenditure share by 9 percentage points; both are statistically significant at the 1% level. While I cannot fully alleviate the concern that cases against politicians may differ in important ways from those against bureaucrats, I address this issue in columns (4) and (8) by adding the instrumented interaction of conviction with key case characteristics: whether it belongs to the main offence category (malversation), the number of individuals involved in the case, whether the case is financial and whether it is in a city. The differential effect for cases involving politicians is even stronger after accounting for these interactions, which suggests that it is indeed driven by the government position of the defendant.

Figure 4: LGU Revenue and Spending: Temporal Response

Notes: This figure displays estimates from the reduced-form Equation (6) on the local share of government revenue and the share of expenditure allocated to general administrative expenses. Estimates are reported for 5-year bins before and after the disposal, which is indicated by the dotted vertical line. All regressions include fixed effects for an interaction between the disposal year and time as well as case controls (fixed effects for the offence category and the number of individuals in the case, as well as controls for whether the case was financial, whether it was in a city, the number of politicians/bureaucrats/neighbours and the processing time). Error spikes represent 95 percent confidence intervals.
Source: Constructed by the author.

Finally, to investigate the overall impact of the change in revenue and spending on fiscal sustainability, I analyse data from the BLGF’s Fiscal Sustainability Scorecard. Since these data are only available for 2012, I estimate Equation (3) for all corruption cases decided before 2012. Appendix Table D6 reports the results for overall fiscal sustainability. While the estimates are imprecise and are only significant at the 10% level, they suggest that LGUs that experienced a conviction in their first corruption case are more likely to have been graded as good, or above, on their fiscal performance in 2012. This effect is driven by a shift in the middle of the distribution: there is an increase in the share of LGUs that are graded as good and a decrease in the share of those considered average,
Table 3: Budget: Baseline Estimates

<table>
<thead>
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<th>Outcome:</th>
<th>Local/Revenue</th>
<th>General/Expenditure</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Conviction</td>
<td>0.23**</td>
<td>0.15**</td>
</tr>
<tr>
<td></td>
<td>(0.10)</td>
<td>(0.058)</td>
</tr>
<tr>
<td>Conviction × Politician</td>
<td>-0.11**</td>
<td>-0.14**</td>
</tr>
<tr>
<td></td>
<td>(0.046)</td>
<td>(0.056)</td>
</tr>
<tr>
<td>F Statistic</td>
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<td>57.8</td>
</tr>
<tr>
<td>E[Y</td>
<td>D=0, Complier]</td>
<td>0.17</td>
</tr>
<tr>
<td>Observations</td>
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<td>16511</td>
</tr>
</tbody>
</table>

Notes: This table reports the results from estimating Equation (5). The outcome in columns (1) to (4) is the local share of government revenue, and columns (5) to (8) present the share of spending allocated to general administrative expenses. The data cover 669 LGUs and the period 1992–2016, except for columns (1) and (5), which only include 1 year after the disposal. All regressions include fixed effects for an interaction between disposal year and time as well as case controls (fixed effects for the offence category and the number of individuals in the case, as well as controls for whether the case is financial, whether it is in a city, the number of politicians/bureaucrats/neighbours and the processing time). Columns (3), (4), (7) and (8) controls and adds an interaction for cases involving politicians. Columns (4) and (8) add the instrumented interaction of conviction with key case characteristics: whether it belongs to the main offence category (malversation), the number of individuals involved in the case, whether the case is financial and whether it is in a city. The omitted reference category for these specifications is financial malversation cases in municipalities with the average number of defendants. Robust standard errors clustered at the ponente level in parentheses. Statistical significance is indicated by *** at 1%, ** at 5%, and * at 10%.

while there are no statistically significant shifts at the extremes.

Financial Audits: Next, I assess how convictions influence financial audits. Figure 5 reports estimates from Equation 6 on the probability of any audit report being issued and on the probability of an unqualified/qualified report being issued (i.e. an assessment that suggests the financial statements are presented fairly).

Figure 5 (a) indicates that the central audit agency is more likely to issue an audit report following a conviction. This effect persists for 15 years following a disposal. Estimates presented in Appendix Table D7 show that convictions lead to an average 6.5-percentage-point increase in the probability that an audit report will be issued (which corresponds to a 14% increase compared to non-convicted compliers). One possible interpretation of these findings is that convictions increase collaboration with the auditing agency in providing financial documents. This is consistent with the reduction in missing data documented in Table D7. However, another potential interpretation is that convictions encourage the auditing agency to increase its efforts. I explore this alternative explanation further when investigating mechanisms in Section 5.4.

Figure 5 (b) suggests that the increase in the probability of an audit report is driven by reports with a positive assessment. However, these estimates are imprecise. Nor do they necessarily suggest that convictions improve local performance in audits, since convictions may affect which local governments are audited. Indeed, Table D7 establishes
that convictions do not uniformly influence the probability that an audit report will be issued across different local governments.¹⁹ This may suggest selection into observing audit outcomes. I account for this potential selection effect by controlling for local auditing behaviour in two ways. First, I include fixed effects for percentile categories of the local auditing rate (i.e., the share of years during the sample period in which an audit report was issued for the LGU) interacted with the disposal year fixed effect. This control ensures that I compare the effect of convictions on audit outcomes for LGUs with similar probabilities of being audited. However, since local audit rates are endogenous, I also implement an alternative approach that controls for fixed effects for the audit rate of the regional audit office (leaving out the LGU considered) interacted with disposal year fixed effects. Panels B–D of Table D7 report the results when adding these additional controls and the combination of the two. Across all specifications, the results indicate that convictions no longer affect the probability of an audit report, while the effects on audit outcomes are of a comparable magnitude. These results are consistent with convictions improving audit outcomes beyond selection effects. However, I caution against placing too much weight on these findings given that they are imprecise and that I cannot rule out the possibility of selection on unobserved margins.

Figure 5: LGU Audit Report: Temporal Response

![Figure 5: LGU Audit Report: Temporal Response](image)

(a) Any Audit Report  
(b) Any Unqualified or Qualified Report

Notes: This figure shows estimates from the reduced-form Equation (6) on the probability of a COA audit report being issued for the local government (a) and on the probability of an unqualified/qualified report being issued (b). Estimates are reported for 5-year bins before and after the disposal, which is indicated by the dotted vertical line. All regressions include fixed effects for an interaction between disposal year and time as well as case controls (fixed effects for the offence category and the number of individuals in the case, as well as controls for whether the case was financial, whether it was in a city, the number of politicians/bureaucrats/neighbours and the processing time). Error spikes represent 95 percent confidence intervals.

Source: Constructed by the author.

Future Corruption Cases: I now turn to whether convictions affect future cor-

¹⁹Estimates are larger for local governments that have a lower baseline probability of receiving a conviction (using the measure from Table 2). There are at least two potential interpretations of this finding. The first is that LGUs that are visibly less corrupt respond more strongly to a conviction (by either increased targeting or cooperation). Second, LGUs that are better able to hide corruption may respond more strongly to a conviction.
ruption in the LGU. Table 4 reports estimates from Equation 5, while Figure D3 in the online appendix reports temporal estimates from Equation 6.

Column (1) of Table 4 shows a negative, but insignificant, estimate on any future corruption case in the local government. However, columns (2)–(10) indicate that this average effect masks substantial heterogeneity – by both the type of offence and whether the defendant is a politician or bureaucrat. First, in line with the above estimates on budget outcomes, column (2) documents that convictions in cases against bureaucrats significantly reduce the probability of any future corruption case by roughly 30 percent (28 percentage points compared to a mean of 88 among non-convicted compliers). The interaction term suggests weaker effects for cases against politicians, also when controlling for interactions with other case characteristics (column (3)), but these estimates are not significantly different at conventional levels. Columns (8)–(10) focus on the position of the officials involved in future cases to identify who responds to the conviction. The results establish that a conviction reduces the probability of any case being filed against a bureaucrat in the future by 24 percentage points (compared to a mean of 42 among non-convicted compliers), while there is no change in cases involving politicians. Columns (4)–(7) present estimates by offence category and the type of corruption. There is a significant 34-percentage-point reduction for future financial cases and a 22-percentage-point decrease in malversation cases; cases that do not involve any monetary amount are unaffected.

To shed light on the temporal response, Figure D3 plots estimates from Equation 6 for the main outcomes. Since the treatment is set at the time of the disposal of the first corruption case, the number of future cases is by definition zero in the pre-period, so these estimates are not particularly informative. In addition, there are typically long lags between corrupt actions taking place and when cases are filed with the Sandiganbayan (often 5–10 years). The estimates in Figure D3 suggest that there is limited evidence of a reduction in the probability of a new case during the first 5 years after a conviction. During this time, there is only a weak indication of a reduction in the probability of a malversation case – the most common corruption offence. However, beyond 5 years there are significant reductions for financial, malversation as well as bureaucratic corruption.

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20 Corruption needs to be detected, investigated, and sufficient evidence collected, before a case is filed with the Sandiganbayan. See, e.g., Sandiganbayan criminal cases 24718 and 24139.
Table 4: Convictions and Future Corruption in the LGU

<table>
<thead>
<tr>
<th>Outcome:</th>
<th>Future Corruption Case (LGU)</th>
<th></th>
<th>Financial</th>
<th>Malversion</th>
<th>Position</th>
</tr>
</thead>
<tbody>
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<td></td>
<td>Any</td>
<td></td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
</tr>
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<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td>Conviction</td>
<td>-0.22</td>
<td>-0.28**</td>
<td>-0.30**</td>
<td>-0.34***</td>
<td>0.083</td>
</tr>
<tr>
<td></td>
<td>(0.17)</td>
<td>(0.14)</td>
<td>(0.13)</td>
<td>(0.11)</td>
<td>(0.19)</td>
</tr>
<tr>
<td>Conviction × Politician</td>
<td>0.13</td>
<td>0.15</td>
<td></td>
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</tr>
<tr>
<td></td>
<td></td>
<td>(0.14)</td>
<td>(0.12)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>F Statistic</td>
<td>50.7</td>
<td>27.2</td>
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<td>24084</td>
<td>24084</td>
<td>24084</td>
<td>24084</td>
</tr>
<tr>
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<td>Yes</td>
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<td>Yes</td>
</tr>
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<td>Case Controls</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Conviction × X</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
</tr>
</tbody>
</table>

Notes: This table reports the results from estimating Equation (5). The outcome is an indicator set to 1 for all time periods following the filing of the first subsequent corruption case with the Sandiganbayan in an LGU. The first three columns consider all future cases, while columns (4)–(10) define indicators depending on the type of offence and whether the defendant was a politician or bureaucrat. All regressions include fixed effects for an interaction between the disposal year and time as well as case controls (fixed effects for the offence category and the number of individuals involved in the case, as well as controls for whether the case was financial, whether it was in a city, the number of politicians/bureaucrats/neighbours and the processing time). Column (3) also includes the instrumented interaction of conviction with key case characteristics: whether it belongs to the main offence category (malversation), the number of individuals in the case, whether the case is financial and whether it is in a city. The omitted reference category for this specification is financial malversation cases in municipalities with the average number of defendants. Robust standard errors clustered at the _ponente_ level are in parentheses. Statistical significance is indicated by *** at 1%, ** at 5%, and * at 10%.
4.2 Alternative Treatment Parameters

The LATE estimates presented in the previous section capture the effect of convictions for *compliers*, and therefore may not generalize to the entire population. To investigate the broader impact of convictions and how changes in judicial strictness may affect outcomes, I discuss ATE, ATT and ATUT estimates in this section. Appendix C.2 reports the distribution of common support of the propensity scores and thus the sample used for this estimation. Tables C3 and C4 report parameter estimates under various modelling assumptions. Following prior work, I focus on the cubic polynomial as the preferred specification (Bhuller et al., 2020).

Estimates for budget outcomes, reported in Table C3, indicate that the above LATE estimates are very similar to the ATEs (they differ by about 1 percentage point).\(^{21}\) This conclusion is robust irrespective of the parametric model used. Estimates further suggest that ATTs are substantially larger than ATUTs across specifications. For the general share of expenditure, ATT estimates suggest a 12-percentage-point reduction, while ATUT estimates are not statistically different from zero.

Table C4 presents estimates for future corruption cases. The preferred specification suggests ATE estimates range from 18–22 percentage-point reductions in the probability of a future financial corruption case or case involving malversation, while estimates for cases involving bureaucrats suggest a reduction of 15 percentage points. Hence, the ATE estimates are smaller than the baseline LATE estimates discussed in the previous section. This disparity is largely due to the restricted sample with common support used to estimate the marginal treatment effects. Taking future financial corruption cases as an example, the LATE estimate for the restricted sample is a 22-percentage-point reduction compared to a 34-percentage-point reduction for the full sample. Across most specifications, the estimates follow the same pattern as the budget results: ATT estimates tend to be larger than ATUT estimates. However, the differences between ATT, ATE and ATUT estimates are small.

Overall, the results in this section suggest that the cases that the Sandiganbayan convicts (possibly because of stronger evidence) are those for which convictions tend to have the largest impact on the outcomes studied. However, convictions tend to reduce corruption across the distribution of cases filed with the Sandiganbayan. This is consistent with an explanation that all cases filed with the court, including those that did not result in a conviction, successfully identified corrupt practices.

\(^{21}\)The estimation method and sample both differ from the baseline empirical strategy. The MTE analysis relies on the trimmed sample with common support and uses probit to estimate the propensity score. See Appendix C.2 for additional details.
5 Mechanisms

The results thus far suggest that convictions improve the management of local public finances and reduce the probability of future corruption related to malversation and financial corruption cases in an LGU. In this section, I explore potential mechanisms that may be driving these results. I start by considering direct legal channels: incapacitation and deterrence. Then, building on the political agency literature, I consider drivers that change either the characteristics of government officials or the incentives for incumbent officials to engage in corruption. Section 5.4 explores alternative explanations of the findings related to targeted audit efforts and the displacement of corrupt activities, and Section 5.5 discusses potential reasons why effects may differ between politicians and bureaucrats.

5.1 Incapacitation vs. Deterrence

Incapacitation and deterrence are two key channels through which judicial decisions may directly influence local outcomes. In the former, convictions prevent officials from continuing to engage in corrupt activities by removing them from office (see, e.g. discussion in Polinsky and Shavell, 2000). In the latter, convictions affect the incentives of incumbent government officials to take part in corruption by altering their beliefs about the probability or severity of future legal actions and therefore increasing the expected costs of engaging in the practice (in line with Becker, 1968; Becker and Stigler, 1974).

To assess the potential for the incapacitation mechanism, I start by considering the length and features of the judicial process. Corruption cases are often filed years after the misconduct occurs, and once filed, take more than 4 years to resolve. Thus, many accused individuals will no longer be in office when the decision is taken. For example, only 10% of consolidated cases involve individuals who hold a leading position (mayor or vice-mayor) in the LGU at the time of disposal, while they constituted 50% of cases at the time of filing. In addition, as discussed in Section 2, all officials will typically be removed from office for a limited period before a decision is taken. These contextual features limit the potential for the incapacitation mechanism to be driving the results. However, it does not rule it out, since convictions may still prevent defendants from returning to office. I therefore formally test this mechanism by using Equation (3) to estimate how a conviction affects the probability that individuals involved in a corruption case will be implicated in a future corruption case. This allows me to investigate whether convictions directly prevent politicians or bureaucrats from engaging in future corruption. Appendix Table D8 reports estimates on a range of outcomes for both local and non-local corruption cases, including the defendant in the case, their relatives, and the combination of these two. Estimates

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22 Due to data limitations I cannot calculate corresponding numbers for bureaucrats.
for defendants are positive and statistically insignificant, implying that convictions do not prevent the defendant from engaging in future corruption. On the contrary, there is suggestive evidence that relatives are less likely to figure in a future corruption case, which is instead consistent with a deterrent effect. However, this estimate must be interpreted with caution as it is based on a limited sample of 14 future cases. When considering defendants as well as their relatives, convictions do not affect the likelihood that a family member will feature in a future case.

To isolate the *deterrence* mechanism, I study how the main outcomes are affected in neighbouring local governments, since convictions may cause officials in nearby LGUs to update their beliefs about the probability of being convicted if they engage in corruption. Table 5 reports the results. Columns (1) and (2) report first-stage estimates on the probability of conviction within the LGU and in the neighbouring LGU. These results reveal two important findings. First, the strictness of the *ponente* assigned to the LGU significantly shapes the likelihood of a conviction in the LGU for this sub-sample, but not the probability of a conviction in neighbouring LGUs, where the outcome is observed. Columns (3) to (5) of Table 5 report the impact on local public finances and future corruption cases. Point estimates for future corruption cases and local revenue are very similar to the baseline estimates, but they are somewhat smaller and only significant at the 10% level for general expenditure. These results indicate that neighbouring officials, who do not face incapacitation, still adjust their behaviour in a similar manner to those in the LGU where the conviction occurs.

Overall, the findings in this subsection are most consistent with the result being driven by legal deterrence rather than incapacitation effects. However, they do not necessarily rule out the possibility that incapacitation effects play some role, since tracking future corruption cases may not capture all rent-seeking behaviour among defendants. Other explanations may also contribute to the observed patterns. For instance, convictions may alter the characteristics of government officials, or the incentives they face, through other channels. Such mechanisms could potentially also affect outcomes in neighbouring governments. In the following sections, I explore the empirical support for these explanations.

### 5.2 Changing Characteristics: Entry and Selection

Another way in which convictions may improve local outcomes is by changing the type of officials in government. Convictions may change who is willing to work as a government official by altering the expected returns – affecting both who runs in elections and who applies for a government job (see e.g. Caselli and Morelli, 2004). In addition, convictions may help the selection of non-corrupt officials through both elections and appointments
Table 5: Conviction and Deterrence: Impact on Neighbours

<table>
<thead>
<tr>
<th>Outcome:</th>
<th>Conviction</th>
<th>Local/Revenue</th>
<th>General/Expenditure</th>
<th>Future Corruption</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Strictness</td>
<td>0.62***</td>
<td>0.042</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.088)</td>
<td>(0.11)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Conviction</td>
<td>0.13**</td>
<td>-0.034*</td>
<td>-0.21*</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.050)</td>
<td>(0.020)</td>
<td>(0.11)</td>
<td></td>
</tr>
<tr>
<td>F Statistic</td>
<td>59.4</td>
<td>59.4</td>
<td>49.5</td>
<td></td>
</tr>
<tr>
<td>E[Y</td>
<td>D=0, Complier]</td>
<td>0.13</td>
<td>0.58</td>
<td>0.54</td>
</tr>
<tr>
<td>Observations</td>
<td>23508</td>
<td>23508</td>
<td>16281</td>
<td>16281</td>
</tr>
</tbody>
</table>

Notes: This table reports the results of estimating Equation (5) on the sample of LGUs with at least one neighbour. The first two columns report first-stage estimates: column (1) for the local government where the corruption case occurred and column (2) for the neighbours. Columns (3) to (5) report the main outcomes for neighbouring local governments. All regressions include fixed effects for an interaction between disposal year and time as well as case controls (fixed effects for the offence category and the number of individuals involved in the case, as well as controls for whether the case is financial, whether it is in a city, the number of politicians/bureaucrats/neighbours and the processing time). Robust standard errors clustered at the component level are in parentheses. Statistical significance is indicated by *** at 1%, ** at 5%, and * at 10%.

To assess whether changes in characteristics may be driving the results, I first consider how long it takes for the main effects to materialise because (absent incapacitation) changes in who holds local government positions take time. Politicians are elected every 3 years and career bureaucrats are not easily replaced due to civil service regulations and security of tenure. As discussed in Section 2 and Appendix A.3, hiring and separation of local bureaucrats are roughly comparable and separations tend to be driven by exogenous factors such as retirement and resignation. Local leaders therefore have limited opportunities to quickly make substantial changes to the composition of the local bureaucracy. Hence, to the extent that convictions affect local government outcomes by changing the characteristics of local officials, effects should take time to materialize. Yet Figures D1 and D2 indicate that convictions have an immediate effect on outcomes that local officials can directly influence (budget and audit outcomes), which makes selection effects less likely.

To further evaluate this mechanism, I use two different approaches to investigate whether convictions affect the characteristics of elected officials (I lack the necessary data to perform this analysis for bureaucrats). First, I estimate whether a candidate with a previous corruption case filed against them, or their family, is less likely to be

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23See Dal Bó and Finan (2018) for a review of the literature on political selection. In this setting, selection may occur, for example, if convictions carry reputational consequences and convicted officials and their relatives are inherently different from other candidates. I discuss this in more detail in the next section.
elected mayor using Equation 5. In a second approach, I use detailed data on mayor characteristics from 2016 (the only year for which this information is available) and estimate Equation 3 for the first corruption case in each LGU (results in Appendix Table D9). The first two columns establish that I find no evidence that convictions affect the probability that an individual with a prior case filed against them is elected. The remaining columns show that convictions did not affect mayor characteristics, or the predicted vote share based on these characteristics, in 2016. An important limitation of this analysis is that data on officials’ characteristics is only available for one election. However, given the persistence of the main results, it is reasonable to expect selection patterns to persist as well if they are driving the main results. To validate the 2016 outcomes, I estimate the effect of convictions on other political outcomes, which can be observed in both the short and long run, and find similar results (Table 7 presents short-run estimates and Appendix Table D9 presents estimates only using data for 2016). The next section discusses these political outcomes in more detail.

5.3 Changing Incentives: Political Environment

Even if convictions do not alter the type of officials in government, they could still influence electoral accountability and the incentives to engage in corruption by affecting the political environment for incumbent politicians. For instance, theoretical models with identical agents and moral hazard (e.g. Ferejohn, 1986) illustrate that incumbents have an incentive to refrain from corruption in response to greater political competition. To determine if the results are consistent with this type of mechanism, I start by investigating whether convictions have electoral costs for defendants and then discuss the implications for political competition.

**Electoral Costs of Convictions:** To investigate electoral costs, I use Equation (3) to estimate the impact of convictions on the probability that defendants ever run for or hold office after case disposal.\textsuperscript{24} Columns (1) and (2) of Table 6 show that convictions do not significantly affect the probability of running for local office (mayor or vice-mayor), but do make individuals less likely to hold such an office in the future. IV estimates suggest that convictions reduce an individual’s likelihood of holding office by 20 percentage points compared to 30% among non-convicted compliers (i.e. corresponding to a 67% reduction). In addition to reputational consequences affecting the electoral support of convicted officials (see e.g. Tirole, 1996), these results are also potentially consistent with incapacitation. To separate these two explanations, I investigate the impact on relatives of those involved in the case. While individuals directly involved in the case may suffer from both effects, relatives are only affected by reputational consequences. The results,\textsuperscript{24}These two estimates are not directly comparable, since the running variable does not include candidates from the 1988 election, for which no information is available.

30
presented in columns (3)–(4), indicate that convictions reduce the probability of relatives holding office by a similar magnitude. Hence, under the assumption that the reputational impact is similar for defendants and their relatives, these results suggest that convicted individuals’ reduced office holding is due to a reduction in electoral support rather than incapacitation. Columns (5) and (6) report joint estimates for individuals and their relatives and show that convictions reduce the probability of running for and holding office by 32 and 47 percentage points, respectively. The reduction in running is consistent both with families internalising reduced electoral support and with a lower perceived benefit of holding office.

**Political Competition:** The results presented in the previous section suggest that convictions generate electoral costs for defendants’ families and lower their participation in the political process. As documented in Section 5.2, this change does not seem to alter the characteristics of elected officials. In this section, I explore how the demise of the families of convicted officials affects the political environment and thus the incentives for incumbent politicians to engage in corruption.

Theoretically, it is unclear how the weakening of these families affects the political environment. On the one hand, if convictions are seen as levelling the playing field, and the barriers to political entry are low, they may strengthen political competition by bringing in new, less corrupt, candidates for office. On the other hand, convictions may weaken electoral competition if a few dominant families control local politics and the exit of one family cements the power of those that remain. The latter may be more relevant in the Philippines: 53% of all local elections between 1992 and 2016 were contested by at most two families; in over 90% of elections, a maximum of four families competed for the position.

To determine how convictions change the political environment, I estimate Equation 5 for the three different measures of political competition defined above. The results are presented in Table 6. Columns (7)–(9) show that convictions reduce political competition: the probability of a competitor in the mayoral race decreases by 13 percentage points, the margin of victory increases by 18 percentage points and the Herfindahl index of political competition declines by 11 points. To further investigate the temporal response to convictions, Appendix Figure D4 presents the results from estimating Equation (6). These graphs establish that the strongest response in electoral competition is in the election immediately after the decision, but the effects persist for more than three subsequent elections.

The above results indicate that convictions lead to political exit rather than entry. However, it is still possible that they affect the political environment by changing the type of officials running for office. To explore whether this is the case, I follow the same strategy as in Section 5.2 but instead estimate Equation 3 for all mayoral candidates in the 2016 election. Appendix Table D10 reports the results. As discussed above, I validate
### Table 6: Conviction and Electoral Outcomes

<table>
<thead>
<tr>
<th></th>
<th>Defendant Outcomes</th>
<th>LGU Outcomes</th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
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</thead>
<tbody>
<tr>
<td></td>
<td>Individual</td>
<td>Relative</td>
<td>Indiv. + Rel.</td>
<td>Candidates</td>
<td>Win</td>
<td>Herfindahl</td>
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<td></td>
<td>Run In Office</td>
<td>Run In Office</td>
<td>Run In Office</td>
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<td>Margin</td>
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<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(5)</td>
<td>(6)</td>
<td>(7)</td>
<td>(8)</td>
<td>(9)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Conviction</td>
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<td>-0.25</td>
<td>-0.32**</td>
<td>-0.47***</td>
<td>-0.13***</td>
<td>0.18***</td>
<td>-0.11***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.11)</td>
<td>(0.095)</td>
<td>(0.16)</td>
<td>(0.16)</td>
<td>(0.15)</td>
<td>(0.046)</td>
<td>(0.047)</td>
<td>(0.034)</td>
<td></td>
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</tr>
<tr>
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<td>44.8</td>
<td>44.8</td>
<td>44.8</td>
<td>44.8</td>
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<tr>
<td>Disposal × Time FE</td>
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<td>No</td>
<td>No</td>
<td>No</td>
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<td>Yes</td>
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</tr>
<tr>
<td>Case Controls</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**Notes:** This table reports the results on how corruption convictions affect electoral outcomes. Columns (1)–(6) report estimates from Equation (3) on defendant outcomes in the case sample, while columns (7)–(8) report estimates from Equation 5 on local elections using the LGU sample. The outcome in the first six columns is an indicator for either running for office (uneven columns) or holding office (even columns) after the case has been decided. Holding office is based on data from all 10 elections, while running for office excludes the 1988 election, for which no information is available on candidates running. Columns (1)–(2) report the outcome for the individuals involved in the case, columns (3)–(4) for relatives and columns (5)–(6) for any individual sharing the same last name in the LGU. Columns (7)–(9) report the effect on three measures of electoral competition: whether at least two candidates ran, the margin of victory and the Herfindahl index of political competition. The first six specifications control for fixed effects for disposal year while the last six control for disposal year by time fixed effects. All regressions include case controls (fixed effects for the offence category and the number of individuals involved in the case, as well as controls for whether the case was financial, whether it was in a city, the number of politicians/bureaucrats/neighbours and the processing time). Robust standard errors clustered at the *ponente* level are in parentheses. Statistical significance is indicated by *** at 1%, ** at 5%, and * at 10%.

This approach by estimating the long-term effects of convictions on political competition outcomes, restricting the sample to data from 2016, and document effects that are even larger than those reported in Table 6. Keeping the limitations of the data in mind, the results in Table D10 indicate that convictions did not alter the type of individuals running for office in 2016. The results in this section suggest that convictions do not improve local government outcomes by strengthening political accountability. On the contrary, results show that convictions weaken political competition.

### 5.4 Alternative Mechanisms

**Targeted Audits:** Another plausible mechanism is that convictions attract greater scrutiny of the LGU’s operation from other government agencies. Indeed, the results presented above suggest that an LGU is more likely to be audited following a conviction. As discussed above, this may be because LGUs increase their cooperation in response to the conviction. However, an alternative explanation is that the central audit agency targets LGUs with convicted officials. Since previous work suggests that audits can reduce corruption (Avis et al., 2017), the increased audit probability in LGUs with a convicted
official may contribute to the main results. To test for this alternative mechanism, I control for local audit probabilities as above, but I instead estimate the effect of conviction on the main outcomes (see Appendix Table D11). As previously documented, the estimates on the probability of an audit report being issued are much smaller and no longer statistically significant when these controls are included. However, the baseline effects of convictions on local revenue, the allocation of spending and future corruption remain largely consistent across specifications. These results provide some suggestive evidence that increased scrutiny by the COA is not the main mechanism driving the results.

**Displacement:** An alternative interpretation of the results on future corruption is that officials who have been convicted of corruption choose to engage in types of future corruption that are harder to detect. If this is the case, then the results may not reflect true reductions in corruption. There are at least three reasons why this is unlikely to be the main explanation for these results. First, the differential effect for bureaucrats and politicians reported in Table 4 is difficult to reconcile with the displacement channel. While bureaucrats typically work in a specific department, politicians have wider influence over many local government decisions (ranging from budgets to hiring and policy design) and would therefore arguably have more opportunities to displace corruption. Hence, the fact that I find no effect for politicians seems less consistent with displacement effects. Second, the results on public finances follow a pattern that is in line with a reduction in corruption. Corruption is perceived to be prevalent in both government spending and revenue collection in the Philippines (World Bank, 2001) and previous work has argued that corruption can both reduce government revenue (Tanzi and Davoodi, 1998, 2002; Friedman et al., 2000) and distort the allocation of spending toward areas in which embezzlement is easier (Mauro, 1998; Gupta et al., 2001; Tanzi and Davoodi, 2002; Liu and Mikesell, 2014). The reduction in general administrative expenses would arguably move resources further away from key local government officials – both politicians and top bureaucrats – and thus make corruption more difficult. Finally, if convictions displace corruption, it would be natural to expect that corruption would be more likely to decrease in areas where convictions have occurred, while it may increase elsewhere. To test whether this is the case, I re-estimate the effect of convictions on the main corruption offence categories reported in columns (3)–(6) of Table 4, by the offence category in the original case. Appendix Table D12 reports the results. These estimates suggest that the reduction in financial and malversation cases documented in Table 4 is not driven by convictions in these offence categories. The interaction terms are insignificant and even of the opposite sign. In addition, convictions in these offence categories do not seem to boost other forms of corruption, for which point estimates are again of the opposite sign. Together, these results are consistent with an interpretation that the observed reduction in future cases reflects a true reduction in corruption.
5.5 Politicians vs. Bureaucrats

There are several potential explanations for why convicting politicians of corruption may be less effective at improving local outcomes than convicting bureaucrats. However, the above analysis of mechanisms points toward one key candidate: the adverse effect of convictions on political competition. The weakening of electoral accountability would arguably reduce politicians’ incentives not to engage in corruption and thus counteract the legal deterrence effect.

In Table 7, I examine whether the effect on political competition can help explain why convicting politicians has weaker effects on local government outcomes. Columns (1) and (2) start by reporting the effect on the Herfindahl index separately for defendants who are politicians and bureaucrats, with and without controls for other case characteristics interacted with instrumented conviction. Convictions in cases only involving bureaucrats has a significant effect on political competition – possibly due to spillovers on politicians associated with the convicted bureaucrats, but the effects are roughly 50% larger for politicians. However, these estimates are imprecise and only significant at the 10% level when adding the additional controls.

To further investigate the role of political competition versus other potential explanations, I explore heterogeneous responses within the group of politicians. I expect the effect on political competition to vary along two key dimensions: whether the accused politician is a rival to the incumbent mayor and whether s/he is term limited. In the former case, I expect the impact on political competition to be greater, and the effect on corruption therefore to be less pronounced, if political competition is the driving force. In the latter case, I expect the impact on political competition to be weaker, since term limits prevent both convicted and non-convicted politicians from running for office, and the effects on corruption therefore to be stronger if political competition is driving the results.

Columns (3)–(4) of Table 7 investigate heterogeneity in whether the defendants are politicians from a rival political family (defined as a different family than the incumbent mayor at the time of disposal). The estimates establish that the effect on political competition is more than twice as large for cases involving a political rival compared to all other cases. Columns (7)–(8) indicate that cases involving rivals have a weaker impact on the probability of future corruption cases than politicians overall (cf. columns (2) and (3) of Table 4). However, these estimates are imprecise and therefore need to be interpreted with caution.

Columns (5)–(6) report heterogeneous results for term-limited politicians (defendants who are prevented from running for office at some point in time after the disposal due to binding term limits). Term limits became binding for the first time in 1998 – the fourth

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25 The term limit variable is defined at the case level and does not vary over time: it separates cases
electoral period after they were introduced in 1987 – and therefore capture cases disposed later in the sample period. The estimates show that convicting term-limited politicians has no effect on political competition. The last two columns of Table 7 present the differential effect on future corruption cases for these politicians and document, consistent with the null effect on political accountability, that convicting these politicians leads to a strong reduction of future corruption. This finding implies that convicting politicians can indeed improve local outcomes if the detrimental effect on political competition is mitigated.

One caveat with this analysis is that politicians who face term limits may be different from those who do not. To make sure that the results are not driven by such differences, I conduct a placebo analysis with an indicator for a politician facing a term limit before, rather than after, the case disposal. I find no differential results on political competition or future corruption for these politicians – suggesting that the results are not driven by other characteristics associated with term-limited politicians (see Table D13). Another potential concern is that convictions affect the probability that a politician will face a term limit. However, the results in Table D13 suggest that this is not the case.

To further substantiate that political defendants are driving the impact on political competition, I separately study how their conviction affects the probability that their families will run for or hold a local government office. Columns (3)–(6) of Table D14 report the results, which demonstrate that the impact on these outcomes is purely driven by convicting politicians. This is consistent with the weakening of the defendants’ political families driving the differential impact on political competition.

The above results suggest that the impact of convictions on political competition at least partly contributed to the heterogeneous government performance documented for politicians and bureaucrats. To investigate other potential drivers of this heterogeneity, Table D14 also reports the other mechanism results separately for politicians. It presents estimates of the differential impact on future corruption cases related to the defendant (capturing incapacitation), on any future audit report being issued (capturing targeted audits) and on local revenue, the allocation of spending and future corruption in neighbouring local governments (capturing deterrence). The results establish that there is no differential effect for politicians for these other mechanisms. Hence, among the channels that I can investigate, it is only for electoral competition that the effects differ by the position of the defendant.

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in which at least one of the defendants faces a binding term limit after the disposal from those in which none of the defendants face such a restriction.
Table 7: Conviction and Political Competition for Different Politicians

<table>
<thead>
<tr>
<th>Outcome:</th>
<th>Political Competition (Herfindahl Index)</th>
<th>Future Corruption (LGU)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) (2) (3) (4) (5) (6)</td>
<td>(7) (8) (9) (10)</td>
</tr>
<tr>
<td>Conviction</td>
<td>-0.087** -0.093** -0.079** -0.089** -0.11*** -0.11***</td>
<td>-0.29** -0.31** -0.20 -0.26*</td>
</tr>
<tr>
<td></td>
<td>(0.033) (0.040) (0.031) (0.037) (0.035) (0.037)</td>
<td>(0.14) (0.13) (0.17) (0.14)</td>
</tr>
<tr>
<td>Conviction * Politician</td>
<td>-0.045 -0.051*</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.030) (0.026)</td>
<td></td>
</tr>
<tr>
<td>Conviction * Politician (Rival)</td>
<td>-0.092** -0.11**</td>
<td>0.20 0.26*</td>
</tr>
<tr>
<td></td>
<td>(0.044) (0.042)</td>
<td>(0.16) (0.14)</td>
</tr>
<tr>
<td>Conviction * Politician (Term)</td>
<td>0.12*** 0.12***</td>
<td>-0.28** -0.28**</td>
</tr>
<tr>
<td></td>
<td>(0.035) (0.044)</td>
<td>(0.11) (0.11)</td>
</tr>
<tr>
<td>F Statistic</td>
<td>29.1 9.01 35.8 10.6 27.9 7.93</td>
<td>34.4 13.9 25.4 10.2</td>
</tr>
<tr>
<td>E[Y</td>
<td>D=0, Complier]</td>
<td>0.49 0.49 0.49 0.49 0.49 0.49</td>
</tr>
<tr>
<td>Observations</td>
<td>6010 6010 6010 6010 6010 6010</td>
<td>24084 24084 24084 24084</td>
</tr>
<tr>
<td>Disposal × Time FE</td>
<td>Yes Yes Yes Yes Yes Yes</td>
<td>Yes Yes Yes Yes</td>
</tr>
<tr>
<td>Case Controls</td>
<td>Yes Yes Yes Yes Yes Yes</td>
<td>Yes Yes Yes Yes</td>
</tr>
<tr>
<td>Conviction × X</td>
<td>No Yes No Yes No Yes</td>
<td>No Yes No Yes</td>
</tr>
</tbody>
</table>

Notes: This table reports the results from estimating heterogeneity using Equation (5). Columns (1)–(6) present results on the Herfindahl index of political competition, while columns (7)–(10) report estimates for any future corruption case. All regressions include fixed effects for an interaction between disposal year and time and case controls (fixed effects for the offence category and the number of individuals involved in the case, as well as controls for whether the case was financial, whether it was in a city, the number of politicians/bureaucrats/neighbours, the processing time). Even columns also include the instrumented interaction of conviction with key case characteristics: whether it belongs to the main offence category (malversation), the number of individuals involved in the case, whether the case is financial and whether it is in a city. The omitted reference category for this specification is financial malversation cases in municipalities with the average number of defendants. Robust standard errors clustered at the ponente level are in parentheses. Statistical significance is indicated by *** at 1%, ** at 5%, and * at 10%.
6 Discussion and Concluding Remarks

This study leverages the random assignment of corruption cases in the Philippines to study how judicial decisions affect rent-seeking behaviour among local government officials. The results show that convictions improve the management of local public finances and reduce future corruption associated with local government funds, such as malversation. Further investigation of mechanisms explores potential drivers of these results, including legal incapacitation, deterrence, selection and changes to the political environment. While data limitations prevent sharp tests of all mechanisms, the pattern of the results suggests that legal deterrence effects at least partly discipline incumbent officials.

In addition, this paper emphasizes that convictions affect bureaucrats and politicians differently. The effects on public finances are driven by the conviction of bureaucrats. It is also among bureaucrats that convictions reduce future corruption, while no such effects are documented for politicians. A potential explanation for this heterogeneity is the detrimental impact that convictions have on political competition, which weakens electoral accountability for incumbent politicians.

The findings in this paper provide some lessons for policy. They suggest that the judiciary can play a pivotal role in combating corruption in the bureaucracy. This is potentially crucial given that we know less about how to effectively tackle this type of corruption. Local bureaucrats are partly shielded from the electoral accountability mechanism that earlier studies have shown to be effective for politicians (e.g, Ferraz and Finan, 2011; Bobonis et al., 2016).

However, the findings caution against an overly simplistic view of judicial accountability. In a setting where political power is concentrated in the hands of a few, convicting politicians can jeopardise electoral competition. This highlights that interactions between different accountability mechanisms are not necessarily mutually reinforcing. Therefore, such interactions must be taken into account when designing anti-corruption policy. Effective policies should ensure that efforts to strengthen one source of accountability do not unintentionally undermine other efforts. More work is needed to understand where and when such interactions are at play and how they can be effectively addressed.

Data Availability Statement

The data and code underlying this research is available on Zenodo at https://doi.org/10.5281/zenodo.11035383.
References


Republic of the Philippines Supreme Court (2002). Revised Internal Rules of the Sandiganbayan.


