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Political Corruption and Development in Brazil: Do Random Audits of Corruption Increase

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Political Corruption and Development in Brazil: Do Random Audits of Corruption Increase Economic Activity?

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Abstract: The government of Luiz Inácio Lula da Silva implemented a random audits program aimed at curbing municipal corruption in 2003. Recent literature found evidence that these audits successfully reduced corruption (Avis, Ferraz, and Finan 2018) and improved firm performance (Colonnelli and Prem 2017). However, these findings are obtained using data concerning activity occurring in the formal sector alone. Given that the focus of the audits was exclusively on federal transfers, it is possible that the true result was a reallocation of corrupt activity rather than a reduction. We utilize difference-in-difference and matching techniques in conjunction with broad measures of total economic activity to test if audited municipalities improved relative to their non-audited counterparts. We find no evidence that audited municipalities performed better. Rather, we find robust evidence of a slight decline in economic activity.

JEL Codes: D73, O1

Keywords: Corruption, Development, Informal Economy

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1. Introduction

The negative association between corruption and development is well-established (e.g. Beenstock 1979; Bardhan 1997; Olken and Pande 2012). As such, most international organizations cite corruption as a priority concern in developing countries. For example, the current president of the World Bank, Jim Yong Kim, referred to corruption as “public enemy number one” when speaking at an event hosted by the Integrity Vice Presidency in 2013 and has since reiterated these concerns through various forums. However, the inherent simultaneity between corruption and economic activity make it difficult to identify the causal nature of the established negative association. The goal of this paper is to estimate the effect of corruption on development utilizing a measure of average light intensity in conjunction with exogenous variation in corrupt activity that resulted from a random audits program in Brazil.

In 2003, the government of Luiz Inácio Lula da Silva instituted a program where auditors collected information on randomly selected municipal governments’ expenditures of federal transfers and developed reports made available to the public. The Controladoria-Geral da União (CGU), a functionally autonomous federal agency, was created to oversee these audits and act as a government watchdog.¹ The public responded to the results contained within these reports by voting corrupt mayors out of office (Ferraz and Finan 2008). Further, incumbent first-term mayors responded by reducing the level of corruption found within these reports relative to the mayors that were on their way out of office (Ferraz and Finan 2011). Avis, Ferraz, and Finan (2018) estimate that, overall, these audits reduced corruption by approximately 8 percent. In this

¹ An obvious concern is that if the CGU is corruptible, the audits are potentially biased. The audit program specifically is intentionally set up to control for this potential problem. We discuss the mechanisms in place in Section 3 of this paper.

paper, we examine whether this exogenous decrease in corruption leads to increased economic activity.

Colonnelli and Prem (2017) test the effect of these audits on firms in the formal sector. Formal firms are defined as firms that have an identification number with the Cadastro Nacional da Pessoa Jurídica (CNPJ) of the federal government. These firms are required to fill out a survey administered by the Ministry of Labor, with the information made available in the *Relação Anual de Informações Sociais* (RAIS). This results in a rich dataset of firms for researchers. By definition, firms excluded from this dataset have not registered with the federal government and are thus considered part of the informal sector. Colonnelli and Prem (2017) use this data to show that the firms involved in corrupt activities, as uncovered in the audits, improved relative to similar firms in non-audited municipalities. In particular, in the post-audit period corrupt firms increased their investment and growth relative to the counterfactual.

One interpretation of Colonnelli and Prem's (2017) findings is that the exogenous reduction in corruption caused by this audit program increased economic activity within audited municipalities. However, there are two caveats to this interpretation. First, a significant portion of economic activity in Brazil occurs in the informal sector and is consequently not captured in the RAIS dataset described above. Second, local political agents may simply reallocate corruption away from federal transfers. In both cases, it is possible that corrupt activity was altered in a way affected *different* firms.² The results of Colonnelli and Prem (2017) only apply to formal firms found to be involved in corrupt activities at the time of the audit, relative to similar firms in non-audited municipalities. It is therefore not clear how all other firms, both

² Alternatively, it may harm the same firms, but only affect the firm's business "off the books". Many registered formal firms hire informal workers, suggesting that the RAIS may be missing a significant portion of their activity (Ulyssea 2018).

formal and informal, in the audited municipality are affected. This is especially important for Brazil with approximately 90 percent of single-employee firms and 30 percent of five-employee firms operating informally (Ulyssea 2018).

We argue that the audits may have altered corruption in a negative way for two primary reasons. First, it is not immediately obvious that the form of corruption uncovered in these audits is particularly harmful. Using cross-sectional data, Bologna (2016) finds that the amount of corruption uncovered in the first year of the audit has a statistically *insignificant* effect on economic output. In other words, it does not appear to be an inhibiting factor to development. It is not clear that changing this status quo is beneficial. Second, corruption *of any form* likely became substantially more secretive in the post-audit period. The increased secrecy can also lead to greater uncertainty, which has been shown to increase the negative effects of corruption (Wei 1997; Campos et al. 1999; Lambsdorff 2003; Bologna Pavlik 2018). Thus, the potential reallocation of corruption to *different* formal firms, combined with increased secrecy and uncertainty, could be substantially more harmful and cause a reduction in total economic activity. Further, if corruption was reallocated to informal firms the effects may be even worse. Informal firms have little formal recourse for potentially harmful corrupt transactions. Bologna Pavlik and Neto (2018) find that these audits increased the existing gap in income between employees in the formal and informal sectors. Thus, if corruption is reallocated, rather than wholly reduced, because of these audits, the effects on development may be negative. Our paper addresses this gap in the literature by accounting for changes in both formal and informal economic activity due to exogenous corruption audits.

The incentive for mayors to decrease corruption outright in response to the audits is unclear. A primary channel through which political agents are punished for audit results

indicating corruption is through the electoral system (Ferraz and Finan 2008; 2011).³ In other words, voters act on the information contained within the audit reports to vote the corrupt mayor out of office and bring a plausibly non-corrupt mayor into office. The new mayor would then have an incentive to limit his or her corrupt activity after observing the electoral fate of the previous incumbent. We should consequently see a reduction in corruption. However, the mayors of these municipalities face term limits, making the job a temporary one. This lessens the impact of the electoral incentive on the new incumbent. It may even encourage these new mayors to engage in more corruption if they realize their political life cycle has the potential to be shortened. Ferraz and Finan (2011) show that first-term mayors were less corrupt relative to second-term mayors, but it is plausible that both increased their level of corruption with the latter increase being larger.

Further, it seems that politicians have discovered ways to circumvent the electoral effect of corruption in Brazil by diverting the attention of voters to other issues (Balán 2014). This is precisely how President Luiz Inácio Lula da Silva (Lula) was able to maintain power throughout numerous corruption scandals (Rennó 2011; Balán 2014). Further, political business cycle theories suggest that voters are myopic.⁴ Given that the results of the audit are made available within the same calendar year of the audit, a mayor audited early on in the election cycle is not necessarily subjected to the electoral effect. Thus, a corrupt mayor may be able to maintain power post audit, in which case they are serving their final term in office due to the two

³ Brollo (2009) argues that one of the reasons corrupt mayors fail to be reelected is because uncovered corruption tends to result in smaller discretionary federal transfers to the municipality in the future as a form of punishment. However, because 75 percent of federal transfers are constitutionally mandated (Brollo 2009), the overall effect of this form of punishment on development is likely small. Nevertheless, we address this issue in subsequent sections of our paper finding no meaningful change in our results.

⁴ See Drazen (2000) for a review of this literature.

consecutive four-year term limit where they are even more likely to be corrupt (Ferraz and Finan 2011).

The second channel through which these audits may result in the punishment of political agents is through the legal system. While the CGU auditors are tasked with conducting the audits and revealing the information to both the public and the various federal authorities, they are not in charge of dispersing punishment. The agencies in charge of the specific punishment depend on the type of corruption uncovered: administrative, civil, or criminal. The Federal Court of Accounts (Tribunal de Contas da União – TCU) handles administrative cases, while the Federal Prosecutor’s Office (Ministério Público Federal- MPF) handles criminal and civil cases (Aranha 2017). Scholars tend to agree that despite the conscious improvements in the anti-corruption arena via the CGU, formal accountability via these two agencies is lacking with a particular emphasis on the slowness of the process (Taylor and Buranelli 2007; Power and Taylor 2011; Aranha 2017).

Nevertheless, Avis et al. (2018) find that the probability of a mayor being prosecuted and convicted increases with the number of corruption irregularities found. Further, they show that the number of corruption convictions increased drastically from 2004 through 2012 in part due to the actions of the CGU.⁵ However, the only resultant punishment from these convictions they cite is banishment from running for public office for at least 5 years. Thus, it is not clear that this effect is substantially different from the electoral effect. Further, the results of Zamboni and

⁵ A portion of this increase can be explained by Federal Police crackdowns, called Operações Especiais (Special Operations), which occurred throughout this same period. These crackdowns were done in conjunction with the CGU. We utilize the list of municipalities affected by Special Operations from 2004 through 2013 from Avis et al. (2018) and information contained within the CGU website for the year 2003 (<http://www.cgu.gov.br/assuntos/auditoria-e-fiscalizacao/acoes-investigativas/operacoes-especiais/anos-antiores/2006-2003>) to exclude municipalities selected for crackdowns from our analysis (2003 through 2013). Approximately 3.4 percent of Brazil’s nearly 5,560 municipalities were selected for Special Operations from 2004 – 2013. Only certain state capitals were subjected to Special Operations in 2003; we exclude all state capitals from our analysis.

Litschig (2018) suggest that local agents react to the risk of audits by reducing corrupt activity in areas that come with a high probability of punishment (e.g. procurement), leaving their activity in other areas unchanged (e.g. health services). In addition to altering corruption behavior related to governmental activities involving federal transfers, local agents may also move their corrupt activities entirely underground affecting the informal sector, potentially exploiting their power to demand bribes from informal firms to “allow” their operation.⁶ Or, they may simply shift their corrupt activities to the allocation of their funds derived from *other* sources. Thus, even if the existing mayors consider either the electoral or non-electoral channels to be costly, they may simply change their corrupt activities such that they are undetectable in subsequent audits.

We aim to test the effect of these audits on total economic activity utilizing both GDP per-capita and a measure of nighttime light intensity. Our GDP per-capita measure is obtained from the Brazilian Institute of Geography and Statistics (Instituto Brasileiro de Geografia e Estatís – IBGE). This measure explicitly includes informal economic activity in the civil construction sector in particular (IBGE 2005). Our nighttime light measure is obtained from the Defense Meteorological Satellite Program Operational Linescan System (DMSP-OLS) Nighttime Light Times Series database. This database provides estimates of the light intensity of a 30 arc second grid after adjusting for ephemeral events (e.g. fire) and has become a widely used proxy for local economic activity (e.g. Bleakley and Lin 2012; Henderson, Storeygard, and Weil 2012; Michalopoulos and Papaioannou 2013; Campante and Yanagizawa-Drott 2018; Shenoy 2018). This measure has the benefit of capturing total economic activity, which is particularly important in a country like Brazil with a large informal sector.

⁶ Alternatively, they may demand bribes from formal firms that hire informal workers. While Almeida and Carneiro (2012) do not test this idea, they acknowledge that greater enforcement of labor market regulations is potentially related to increased corruption.

We compare both the levels and the growth of these two outcome variables in audited municipalities versus non-audited municipalities in the post-audit period using various difference-in-difference estimations.⁷ Overall, we find that audited municipalities experience a slight decline in light intensity after the audit relative to their non-audited counterparts. The audits have no robust effect on GDP per-capita. While omitted variable bias is not a concern given the random nature of the treatment selection process, we utilize the Coarsened Exact Matching algorithm developed by Iacus, King, and Porro (2012) to reduce the amount of extrapolation required in our regressions and to control for the potential of differential audit risk across states (Avis et al. 2018). Specifically, we match audited and control municipalities randomly within each state and re-estimate our results using these matched pairs. We repeat this analysis 100 times and record our results. This process has the added benefit of requiring there to be an equal number of treatment and control groups within each state and alleviates the concern that our results are limited by the extent of audit spillovers across neighboring municipalities.⁸ Our main findings remain unchanged. Our results suggest that while these audits may have achieved their intended effect of reducing corruption in federal transfers, they may have failed to reduce corruption overall.

The remainder of the paper is as follows: section 2 summarizes the research concerning corruption and development; section 3 summarizes the random audits program; section 4 presents the data; section 5 outlines our empirical methodology; section 6 discusses the results;

⁷ All regressions include municipal fixed effects. As noted in Henderson et al. (2012), while nighttime light measures are accurate representations of growth, they may be misleading when comparing levels alone. It is therefore important to examine the changes in the level light measure within municipalities and not across. We additionally utilize the growth regressions inclusive of the lagged dependent variable as this controls for the unbalanced nature of our dataset. Details on our empirical methodology are given in section 5.

⁸ Avis et al. (2018) find that the audits resulted in significant spillovers onto nearby municipalities. This bias would tend to reduce the significance of our estimates, strengthening our results. We do not expect there to be significant spillovers when randomly selecting municipal pairs across states.

section 7 concludes.

2. Corruption and Development

Despite the robust negative association found between corruption and development (Li, Xu, and Zou 2000; Mo 2001; Abed and Davoodi 2002; Treisman 2007; Aidt, 2009; Olken and Pande 2012; Bologna Pavlik 2018), the relationship is exceedingly complex. One source of complexity is definitional. Corrupt acts are not identical in nature. An all-encompassing definition of corruption common in the literature is “the misuse of public power for private or political gain” (Rose-Ackerman 2004:1). While the general sentiment of this definition is that corruption is harmful, that is not necessarily the case.

Sequeira and Djankov (2010) classify corruption into two separate categories: coercive or collusive. Coercive corruption is defined as corruption that increases the price of a good or service above the official price. Collusive corruption occurs when individuals and officials collude to share rents from an illicit transaction. Most theories relating corruption and development are focused on coercive, or cost-increasing, corruption. However, not all corruption is coercive. As an example of collusive and potentially beneficial corruption, Sequeira and Djankov (2010) discuss the additional payments given to officials to avoid high tariffs in Maputo, Mozambique. These corrupt acts represent mutually beneficial transactions. If the collusive acts of corruption increase the efficiency of the firms à la greasing the wheels (e.g. Leff 1964; Huntington 1968), it is not clear that all corruption is harmful. Alexeev and Song (2013) find that collusive corruption leads to greater competition. Similarly, Bologna (2017) finds that corruption increases competition when firms face burdensome regulations.

A second source of complexity stems from the type of corruption regime that exists in the area of interest. Shleifer and Vishny (1993) argue that there are three types of corruption regimes: (1) one in which there is a monopoly over the goods to be provided and to whom bribes should be paid is therefore clear; (2) one in which an individual requires numerous complementary goods that come from different monopolistic providers; (3) one in which competition exists among suppliers of the same goods. Bologna Pavlik (2018) finds that corruption is the most harmful when it is unpredictable and argues that corruption in the second regime is the most uncertain. The increased negative effects of corruption in the face of uncertainty is a common finding in the literature (Wei 1997; Campos et al. 1999; Lambsdorff 2003).

A third complexity stems from the numerous channels through which corruption may affect a country's productivity. For example, in most theories arguing there is a negative link between corruption and economic growth, the primary costs of corruption stem from misallocation problems, not the act of corruption itself (Svensson 2005). Thus, corruption may benefit some areas by allowing firms to operate despite poor institutional quality (Dreher and Gassebner 2013), but these firms may be inefficient. Colonnelli and Prem (2017) find that the corruption audits increase the efficiency of formal firms. This suggests that municipal corruption is not collusive and, because most uncovered corruption deals with formal firms, it is unlikely that this type of corruption has positive effects through other channels. However, if the audits simply displaced corruption such that corrupt acts are now more secretive, and consequently more uncertain, it is possible that these audits had negative consequences on development. The evidence presented in Bologna Pavlik and Neto (2018) suggest that the corruption audits may

have resulted in more corruption in the informal sector where corrupt activity is not subjected to the audits.

3. The Random Audits Program

The federal government officially created the Office of the Comptroller General (Controladoria-Geral da União (CGU)) in May of 2003 as part of a larger anticorruption initiative. The CGU essentially formalized and replaced the General Office of the Union that was established in 2001.⁹ The CGU has recently been elevated to the powers of a Ministry in May of 2016 (OECD 2017). Its current formal name is the Ministry of Transparency and Comptroller General of the Union (Ministério da Transparência e Controladoria Geral da União).

Corruption at the federal level in Brazil has been widely exposed with the recent conviction of former President Luiz Inácio Lula da Silva (Andreoni et al. 2018) and extensive investigation involving the country's state-owned oil company, Petrobras (Watts 2017). However, corruption across Brazil's nearly 5,600 municipalities is also a problem.¹⁰ Municipalities receive millions in federal transfers each year to provide various public services. The allocation of these funds, however, is largely discretionary with negligible federal oversight (Avis et al. 2018). This discretionary power has resulted in a substantial amount of local corruption.

The CGU launched the *Programa de Fiscalização por Sorteios Públicos* in 2003. We refer to this program as the Random Audits Program. It is based on the random auditing of municipal government expenditures of federal transfers. Using a *random* selection process, the

⁹ See the CGU website for further details on the history of its creation (<http://www.cgu.gov.br/sobre/institucional>).

¹⁰ The number of municipalities changes slightly through time as new municipalities are created from existing ones. For example, there were 5,507 municipalities in 2000, 5,561 in 2001, and 5,565 in 2010. We will address this by excluding changed municipalities from our analysis as a robustness check. Specifically, we drop the 58 municipalities created between 2000 and 2010, as well as the municipalities they were derived from, as a robustness check. Results available in **Table A4** of **Appendix A**. We use existing municipalities in 2010, exclusive of Fernando de Noronha as this is an archipelago island, through this section as a reference (5,664 municipalities).

CGU selects a small percentage (usually around 1 percent) of eligible municipalities to audit. The specific eligibility criteria and number of selected municipalities has varied slightly through time. Eligibility is based on population and typically excludes only the largest municipalities from audit selection. For example, the first eight lotteries excluded any municipality with a population of 300,000 or greater (approximately one percent of all municipalities).¹¹ The remaining audits excluded municipalities with a population of 500,000 or greater (less than one percent of all municipalities).¹² The excluded municipalities are mostly comprised of state capitals, subjected to alternative monitoring mechanisms. In most cases where state capitals are not excluded due to the population criterion, they are explicitly excluded from selection given the legislation. Consequently, we exclude state capitals from our analysis entirely.

Eligibility also depends on whether or not the municipality falls under a grace period. However, since each audit covers all transfers over a pre-specified number of years prior to selection, a grace period does not imply a set period of unsupervised governance. The grace period varies in length from three to twelve subsequent lotteries, depending on the lottery in question. Once this grace period expires, the municipality becomes eligible for selection once again. A municipality can fall under grace for two reasons. First, municipalities selected for audit are ineligible for selection in a specified number of subsequent lotteries. Second, lotteries selected for a “Special Inspection” (Fiscalizações Especiais) are also under grace for the

¹¹ The first three municipalities were slightly more restrictive in that moved the cutoff number was even lower. The first lottery restricted selection to only municipalities with a population of 20,000 or less. The second lottery restricted selection to only municipalities with a population of 100,000 or less. However, these first two lotteries selected only 5 (1 per region) and 26 (1 per state) municipalities, respectively. The third lottery selected 50 municipalities, excluding municipalities with a population of 250,000 or more.

¹² Details concerning the audit selection process can be found at <http://www.cgu.gov.br/assuntos/auditoria-e-fiscalizacao/programa-de-fiscalizacao-em-entes-federativos/edicoes-anteriores>.

equivalent amount of time. These Special Inspections are separate from the random audits.¹³ In our analysis, we exclude the few municipalities selected for Special Inspections from our analysis.¹⁴ After excluding these non-randomly audited municipalities, in addition to municipalities that do not fit the population criteria and state capitals, approximately 96 percent of all municipalities were eligible for audit selection at least once from 2003 through 2013.¹⁵

Once selected, the CGU collects information on all federal funds transferred to the municipal government during the three to four years prior to the municipality's audit selection. They send 10 – 15 auditors to the municipality to conduct detailed inspections of specific government projects (Avis et al. 2018). In the earlier audits, the auditors inspected all areas of federal transfers. However, they eventually resorted to randomly selecting specific areas of government activity within the selected municipalities to improve efficiency. Auditors also consult the residential population through community councils on any complaints of misconduct. The goal of the auditors is to uncover any irregularities associated with the projects. This includes the examination of accounts, the verification of the existence and quality of public construction, and the verification that certain public services were delivered as agreed. This information is collected and organized into a report and made available to the public. The reports are generally available within the same calendar year.

A concern with the effectiveness of the program is the potential bias of the auditors.

Ferraz and Finan (2011) argue that the likelihood of auditor bias is small. Auditors are selected

¹³ The Special Inspections are also different from the Special Operations held in conjunction with the Federal Police. However, it is common that the Special Operations require a Special Inspection of federal transfers, causing there to be some overlap.

¹⁴ We estimate that approximately 3 percent of municipalities were selected for Special Inspections in our sample. We identify Special Inspections as municipalities that were listed as being under grace without being previously selected for audit in the relevant period. Only one municipality, Colares, PA, was simultaneously listed under grace due to both audit selection and Special Inspection within the same grace period.

¹⁵ After additionally excluding municipalities subjected to Special Operations, this number drops only to 94 percent.

via a highly competitive process and work in teams making it difficult to engage in corrupt behavior. They test for the potential of auditor bias two ways. First, because first term mayors are up for reelection, they test if their reports are more favorable during an election year. Second, they test if audit reports are more favorable when the mayor is more closely affiliated with the federal and state governments. In both cases, they find no evidence of bias.

While the audit program is still in existence, it is no longer random. Municipalities are now selected based on a vulnerability index, endogenizing the treatment. We focus exclusively on the effect of the audits that occurred with random selection. We explain the details of our data in the following section.

4. Data

A list of variables used in our analysis, along with a brief description of each variable, can be found in **Table 1**. Summary statistics can be found in **Table 2** for all variables used in our regression analysis and in **Table 3** for all variables used in our matching analysis. We begin this section by detailing our dependent variables of interest.

We test the effect of these audits on economic activity using two alternative measures. First, we use municipal GDP per capita data constructed by the Instituto Brasileiro de Geografia e Estatística (IBGE).¹⁶ Unlike traditional international measures of economic output, this estimate explicitly includes the value added from informal workers in specific industries. Most notably, estimates include the value added in the construction industry from employees employed informally.¹⁷ However, because informal production occurs in all sectors of the

¹⁶ We put our GDP variable in constant 2000 dollars using the World Banks' GDP deflator.

¹⁷ Methodology detailing the construction of the municipal GDP estimates for each year can be found at <https://metadados.ibge.gov.br/consulta/estatisticos/operacoes-estatisticas/IO/2016/0/0>.

economy, it may still understate true economic activity. This is especially important in an economy like Brazil's where anywhere between 40 to 60 percent of employment is underground (Bologna Pavlik and Neto 2018).

To better address the issue of measurement of economic activity, we utilize nighttime light data. Nighttime light intensity estimates are constructed by the National Oceanic and Atmospheric Administration's National Geophysical Data Center using data from the Defense Meteorological Satellite Program. These data are available through the Nighttime Lights Time Series from 1992 through 2013 using a 30 arc second grid (approximately equivalent to one square kilometer at the equator). The use of nighttime light data has become increasingly popular as a measure of economic output and is particularly useful when the country's national accounts data are low quality, or the informal sector is large (Henderson et al. 2012). The nighttime light data values range from 0 (completely dark) and is top-coded at 63, with top-coding being relatively uncommon.¹⁸ These data are corrected for cloud cover and any natural sources of light present, such as fire. We combine this raster data (**Figure 1**) with municipal shapefiles (**Figure 2**) provided by the IBGE to develop a measure of municipal light intensity.¹⁹ We follow the literature and utilize the average light intensity of each municipality as our measure of economic activity.^{20,21} We calculate light values by municipality using the Zonal

¹⁸ Henderson et al. (2012) estimate top-coding to be between 0 and 2 percent in densely populated rich countries like the Netherlands. In **Appendix B, Table B2**, we re-estimate our results after excluding the top one percent and top five percent of our light intensity measure.

¹⁹ Shapefiles can be found at <https://mapas.ibge.gov.br/bases-e-referenciais/bases-cartograficas/malhas-digitais>. Because municipal borders change and new municipalities are created through time, we must use shapefiles that appropriately represent the municipal borders of the year in question. For the years where the IBGE does not provide a shapefile, we use the shapefile from the year that has identical municipal borders as the in question. For example, a shapefile for 2004 is not available. We therefore use the shapefile from 2005 for this year as there were no municipal boundary changes between the two years.

²⁰ Most years in this dataset have light data available across multiple satellites. Our main regression results utilize the average of the average light value across the two satellites within the same year. This is standard in the literature (e.g. Lowe 2014).

²¹ We additionally measure light intensity using the log of the sum of the digital value divided by the area in square kilometers and the inverse hyperbolic sine transformation on the average value. These results are available in

Statistics as Table tool in ArcGIS.²² As with GDP per-capita, this variable enters regressions in logged form.

As shown in **Table 4**, the average light value significantly correlates with both GDP per-capita and personal income measures, including informal income. The average light score negatively correlates with the percent of informal employment, an indicator of underdevelopment (Bologna 2016). The coefficient in column (1) can be interpreted as the elasticity of the average light value with respect to changes in GDP per-capita. In other words, if GDP per-capita increases by one percent, average light value increases by 0.129 percent. Similarly, the elasticity of GDP per-capita with respect to changes in our average light value is given in column (1) of **Table 5**.²³

While the audits are random, we include additional covariates that are available on annual basis and thought to affect development as a robustness check.²⁴ We also do this as a means of improving the precision of our estimates. These include, as shares of GDP, the valued added from agriculture, industry, services, and product taxes. We additionally control for population density in these same estimations. In all cases, these variables enter the regression as a lag to control for initial values.

We additionally utilize a matching algorithm, described below, to narrow our control group based on the state in which a municipality is located within and additional pre-treatment

Appendix B Table B1. While the results appear stronger with these alternative measures, we note that some skewness remains in the distribution of the transformed variables.

²² For a technical description of the zonal statistics calculations, please see <http://pro.arcgis.com/en/pro-app/tool-reference/spatial-analyst/zonal-statistics-as-table.htm> for an explanation of the output, and <http://pro.arcgis.com/en/pro-app/tool-reference/spatial-analyst/how-zonal-statistics-works.htm> for an explanation of the process.

²³ The coefficient values in **Table 5** are much smaller than the elasticity of 0.300 reported in Henderson et al. (2012). This is because their paper looks at changes in country level GDP, whereas we are only looking at changes in municipal GDP.

²⁴ See Bologna (2016) for a standard list of covariates that are likely correlated with municipal development in Brazil.

covariates. As with our control variables used in our estimations, these covariates are thought to have their own independent effects on development. These additional variables include two measures of institutional quality, numerous population shares including variables representing the population that is urban, male, black, literate, and informally employed, as well as the average age of the population.

We follow Naritomi et al. (2012) and measure institutional quality two ways.²⁵ Our first institutional quality measure comes from the Municipal Institutional Quality Indicator (IQIM) developed by the Ministry of Planning and provided by IBGE. The IQIM index uses three components of institutional quality to construct an overall measure of local government efficiency. These three components include the participation in municipal administration, municipal finances, and management capability. Each component receives equal weight. Our second institutional measure, access to justice, is constructed using information contained within a 2001 municipal survey conducted by the IBGE. From this survey, we follow, Naritomi et al. (2012) and calculate an “access to justice” index. The index ranges from 0 to 3 according to the existence of courts or justice commissions. It is calculated with data from 2001, as the sum of three binary variables indicating the existence of Small Claims Courts (“Tribunal de Pequenas Causas”), Youth Councils (“Conselho Tutelar”), and Consumer Commissions (“Comissão de Defesa do Consumidor”).

We follow Bologna (2016) and utilize Census data from 2000 to obtain estimates of demographic characteristics. Our population shares are all constructed from the 2000 Census microdata (Resultados da Amostra).²⁶ This microdata is the resulting individual level data

²⁵ Naritomi et al. (2012) additionally calculate a land GINI coefficient as a third measure of institutional quality. We focus on the more standard measures here.

²⁶ This data can be found at https://ww2.ibge.gov.br/home/estatistica/populacao/censo2000/default_microdados.shtm.

obtained from a representative sample of the population during the 2000 Census. Using this data, we calculate the average age of the population, the share of the population that resides in an urban area, share of population that is male, racial population shares (White, Black, Asian, Multiracial, Indigenous, Other), and the share of population (10 years or older) that is literate.

5. Empirical Methodology

We utilize the staggered difference-in-difference (DID) model to estimate the effect of the random audits on total economic activity. More specifically, we estimate a model such that the treatment effect is allowed to vary across each post-treatment year.²⁷ In other words, we estimate the following:

$$(4.1) \quad y_{it} = Audit_{it} + \sum_k^K \tau_k PostAudit_{it}^k + \lambda_t + m_i + u_{it}, t = 1, \dots, T$$

where *Audit* refers to the year of the actual audit and *Post Audit* refers to a series of dummy variables equal to one if a municipality was audited k years ago with $k = 1, \dots, K$.²⁸ For municipalities that were selected multiple times, we are considering the effects of their first selection only.²⁹ For our growth regressions, the dependent variable, y_{it} , is the logged difference of either a) GDP per-capita or b) average light intensity measures. In these regressions, we include the logged form of the lagged level of our dependent variable on the right-hand side of our equation. For our level regressions, our dependent variables are the logged levels in time t with no lag included.³⁰ While we expect our growth and level regressions to yield similar results,

²⁷ We additionally follow Colonnelli and Prem (2017) and estimate an average effect of audits on selected municipalities across all post-audit years for robustness. These results are available in Appendix C.

²⁸ The staggered difference-in-difference approach is a standard way to estimate treatment effects that vary in both timing and in length (e.g. Stevenson and Wolfers 2006).

²⁹ We drop the 215 municipalities that were selected for multiple audits from 2003-2013 as robustness check. These results are available in **Table A1** of **Appendix A**.

³⁰ Our growth and levels specifications are common in the growth literature in general, but also in the growth literature that uses light intensity as a measure of economic output (e.g. Campante and Yanagizawa-Drott 2018).

we rely primarily on our growth estimations, since these regressions eliminate non-annual changes in the case of unbalanced data. For example, in our level regressions with fixed effects we focus on within municipal variation. However, if the municipality is missing GDP or light intensity observation for the year prior, the within variation may not be annual. We additionally control for year effects (λ_t) and municipal fixed effects (m_i). Our error term is given by u_{it} .

The universe of our sample includes all *eligible* municipalities, excluding state capitals and municipalities selected for nonrandom inspection (see **Figure 3**).³¹ This equates to a total of 5,211 municipalities. Our treatment group includes all municipalities that were selected for an audit in any of the 38 lottery rounds that occurred between 2003 and 2013. This sample includes a total of 2,004 audits of 1,777 different municipalities.³² As mentioned above, we consider the treatment period to be the year of and the years following the *first* selection for those municipalities that were audited multiple times. Our control group includes all *eligible* municipalities that were never selected in this time-frame, a total of 3,434 municipalities. The legislation outlining the eligibility criteria of each lottery is made publicly available prior to each selection in the Diário Oficial da União (the Federal Official Gazette of Brazil). This legislation also explicitly lists all municipalities that are eligible for selection. Thus, we expect both audited and control municipalities to react similarly to the perceived risk of the audit. Any remaining differences should be attributable to the audit itself.

We use **Equation (4.1)** to analyze the treatment effects through time. Our period of analysis will be from 2001 (2000) through 2012 for GDP per-capita growth (level) and from

³¹ There were 314 municipalities selected for non-random inspections in our sample. Eligibility information is readily available on the CGU's website at <http://www.cgu.gov.br/assuntos/auditoria-e-fiscalizacao/programa-de-fiscalizacao-em-entes-federativos/edicoes-antiores/legislacao>.

³² The list of municipalities selected for random audits is available on the Ministério da Transparência, Fiscalização e Controle website at <http://www.cgu.gov.br/assuntos/auditoria-e-fiscalizacao/programa-de-fiscalizacao-em-entes-federativos/edicoes-antiores/municipios>.

2001 (2000) through 2013 for average light intensity growth (level). Starting our analysis with 2001 gives us at least two full pre-treatment periods for our growth regressions (2001; 2002) and three full pre-treatment periods for our level regressions (2000; 2001; 2002).³³ The year 2003 is the first possible year of audits, with 2004 being the first possible year of post-treatment. Thus, for our GDP per-capita measures we are able to analyze 9 potential post-audit years (2004 through 2012) and for our average light intensity measure we are able to analyze 10 potential post-audit years (2004 through 2013). We exclude the five municipalities created in 2013 from our analysis, as they did not exist in any pre-treatment period.³⁴

Though the random nature of the audit selection process suggests that omitted variables are not a concern (**Figure 4** and **Figure 5**), the diversity of municipalities is potentially problematic. Average light intensity from municipalities with a significant amount of agricultural production or situated within the rainforest region is not comparable to average light intensity from municipalities that are mostly urban. If we have a substantial number of treated or non-treated municipalities located within either of these areas, it may bias our results. This concern is amplified by the fact that the probability of selection differs across states.³⁵ We therefore resort to using the Coarsened Exact Matching algorithm developed by Iacus, King, and Porro (2012). The basic idea behind this procedure is to temporarily coarsen our data such that we create bins (strata) to match upon based on selected matching variables. We then re-estimate **Equation (4.1)**

³³ This is in line with the pretreatment window of [-3] used in Colonnelli and Prem (2017). However, we add in an additional pre-treatment year (**Table E1**) and take away a pre-treatment year (**Table E2**) finding no significant change in our results. In cases where statistical significance is lost the p-value is close to the 0.100 mark. Further, results become statistically significant after matching, as in our main regressions. These additional matched results are available upon request.

³⁴ These five municipalities are never included in the universe of our sample (5,211). It is important to note that the newly selected municipalities (2012 for GDP; 2013 for average light intensity) will have no post-period observations. As a robustness check we exclude these newly selected municipalities from our analysis and re-estimate our results. Results available in **Table A2** of **Appendix A**.

³⁵ This can result in an disproportional number of control municipalities per state, as evidenced in the gap in means between our treatment and control groups in **Figure 4**.

as a weighted regression where unmatched observations are given a weight of zero; remaining observations are weighted according to their matched stratum size.

We implement this algorithm several ways. First, we simply match on the state in which a municipality is located within, forcing the total number of treated and un-treated units to be equal within each state. More specifically, for each treated unit we randomly select a single control municipality within the same state. We consider our treated units to be any municipality selected for audit between 2003 and 2012 for our GDP data or between 2003 and 2013 for our light data. Control units include any eligible municipality not selected in the respective time period, exclusive of capitals and non-randomly selected areas. We rerun this procedure 100 times producing 100 different estimates. We calculate the percentage of times our estimates are significant at any level with the anticipated sign. Because we are selecting treatment and control pairs at random within each state, the potential for spillovers should be minimized. This is consequently our preferred matching algorithm.

Additionally, we also match on several pre-period covariates that have been shown to correlate with development, including institutional quality, geographical factors, and demographics as described in the previous section. As can be seen in **Table 6**, it is not clear that these additional covariates strengthen our match beyond random selection within states. Matching on demographic characteristics does yield a sample with differences in means that are no longer statistically significant, but at the cost of significantly reducing our sample size.

A final concern is that after corruption was uncovered in the audit, the federal government may have reduced discretionary transfers to the corrupt municipality as a form of punishment (Brollo 2009). While this form of punishment is unlikely to have significant effects, as the amount of federal transfers to a municipality is mostly constitutionally mandated, we may

find a reduction in economic activity due to this loss of transfer money in some of our treated units. We address this concern by repeating our random matching within each state algorithm with federal transfers per-capita additionally included as a matching variable. That is, we randomly match our audited municipalities with non-audited municipalities that received federal transfers per-capita within the same strata within the same state on average throughout the entire treatment period and re-estimate **Equation (4.1)**.³⁶

6. Results

Our results can be separated into two categories: baseline and matching robustness. We discuss each in turn.

6.1 Baseline

Our baseline results are given in **Table 7**. Our results for GDP per-capita are given in column (1) and (2) and our results for the average light value are given in column (3) and (4). As can be seen in the **Table**, we are able to explain more variation in both the level and growth of our light intensity measure than we are for GDP per-capita. This fact tends to hold in all subsequent specifications. The pattern we find in our coefficients given in the **Table** is also consistent throughout our analysis.

We generally find the effects of the audit to be statistically insignificant for GDP per-capita and significant for average light intensity. We also find that these post audit effects tend to be *positive* for GDP per-capita and *negative* for average light intensity. Looking at the statistically significant effects of column (3) only, we find that average light intensity growth falls by approximately seven percentage points throughout the entire post audit period (ignoring

³⁶ We get similar results if we match on the trend in federal transfers per-capita relative to 2003 instead of the average level.

compounding effects). This would explain approximately 13 percent of one standard deviation change in average light growth over ten years (53%). If we sum over all post period coefficients in column (3) we see a ten-percentage point decline in the growth of average light intensity. The contemporaneous effects of the audit are all statistically insignificant and remain so in all specifications. Thus, it seems the effects of the audits are not immediate.

Table 8 repeats the analysis after including the additional annual covariates. These additional covariates cause one year to be dropped in both our GDP per-capita and average light intensity level regressions because we include the covariates as lags to control for initial values. As can be seen in the **table**, this slightly increases both the significance and magnitude of our coefficients in our light regressions. Given the increased precision, all remaining estimates include these annual covariates. Again, summing over all post period coefficients in column (3) of the tables yields a potential decline in the growth of average light intensity by almost 11 percentage points. Thus, there is some evidence that the audits may have caused more harm than good by reducing total economic activity. Perhaps more importantly, though, is the lack of evidence that these audits have had any real benefit. We explore the robustness of our baseline results after matching below.

6.2 *Matching*

As discussed above, it seems that randomly selecting matches based off states alone provides the best overall match with a sufficient number of observations. As such, we focus on these results in this section. However, we also present results for all of our various matching procedures and note that our results are robust to all specifications.

We present the coefficients for the *first* round of our one-for-one randomly selected state-based matching specification in **Tables 9** and **10** for GDP per-capita and average light intensity,

respectively. Our significant results hold. The growth in average light intensity seems to decline in the post audit period, while measured GDP is unaffected. We repeat this random selection process an additional 99 times, finding the coefficient on our average light score growth to frequently be statistically significant and negative in the post audit period (**Table 10**). Our estimates for GDP per-capita are rarely statistically significant (**Table 9**).

Similarly, after matching on all other covariates, the estimates for the effect of audits in the post audit period on average light intensity remain negative (**Tables 11 through 13**). While we do not match one-for-one within states we do include state dummy variables in addition to our institutional variables (**Table 11**), geographical variables (**Table 12**), and demographic variables (**Table 13**). In most cases, audits have a statistically significant and negative effect on average light intensity in the post audit period. However, we do lose some statistical significance in institutionally matched regressions. As shown in **Table 6**, it seems that while matching on institutions decreased the distance between treatment and control municipalities in one category (*Institutional Quality*) it increased the distance between the two groups in another (*Access to Justice*). In fact, **Table 6** indicates that the covariate matching specifications used in **Table 11** and **Table 12** have more imbalance overall than in our random one-for-one selection procedure and are likely less reliable overall. Matching on demographics, however, gives us the most balanced dataset with the smallest number of observations. Non-coincidentally our results are the strongest for this matching procedure (**Table 13**).

Lastly, given that there is some evidence that corrupt mayors are punished with subsequently reduced federal transfers (Brollo 2009), we repeat our random one-for-one within state matching algorithm with average federal transfers per-capita throughout the entire audit period (2003 through 2013) included as an additional matching variable. The resulting estimates

are presented in **Table 14** (GDP) and **Table 15** (Light Intensity). Our main results are unchanged. It seems the audited municipalities performed worse in the post-audited period, relative to their non-audited counterparts.

7. Conclusion

In this paper, we examine the effect of randomized corruption audits on economic activity across Brazilian municipalities. The Office of the Comptroller General (Controladoria-Geral da União (CGU)), tasked with combatting local corruption, launched the *Programa de Fiscalização por Sorteios Públicos* in 2003. This program sent auditors to randomly selected municipalities to collect information on municipal governments' expenditures of federal transfers and develop reports for the public. Avis, Ferraz, and Finan (2018) find that these audits reduced corruption, while Colonnelli and Prem (2017) find that these audits improved the performance of “corrupt” formal firms suggesting that corruption “sands the wheels” of economic activity.

At first blush, these findings suggest that the exogenous reduction in corruption benefited audited municipalities. However, there are two caveats to this interpretation. First, a significant portion of economic activity in Brazil occurs outside of the formal sector and, second, corruption may have been reallocated away from federal transfers rather than reduced. In both cases, it is possible that corrupt activity was altered in a way that amplified the negative effects of corruption while harming *different* firms. Thus, an analysis of the effect of these audits on total economic activity is warranted.

We utilize a staggered difference-in-differences approach, along with Coarsened Exact Matching, to estimate the effect of randomized corruption audits on two measures of total economic activity: GDP per-capita and average nighttime light intensity. Our results suggest that

the audits slightly reduced municipal economic activity in the post-audit period, as measured by our average light intensity data. GDP per-capita is unaffected.

Our results contribute to the broad literature concerning the link between corruption and development, a relationship that is exceedingly complex. Altering this relationship must be done with care. In the case of Brazilian municipalities, we argue that while the audits of corruption may have been effective at reducing corruption involving federal transfers, they failed to address all potential sources of corruption. A random audit program that is more all-encompassing would be more likely to lead to reduced corruption, and improved economic activity, overall.

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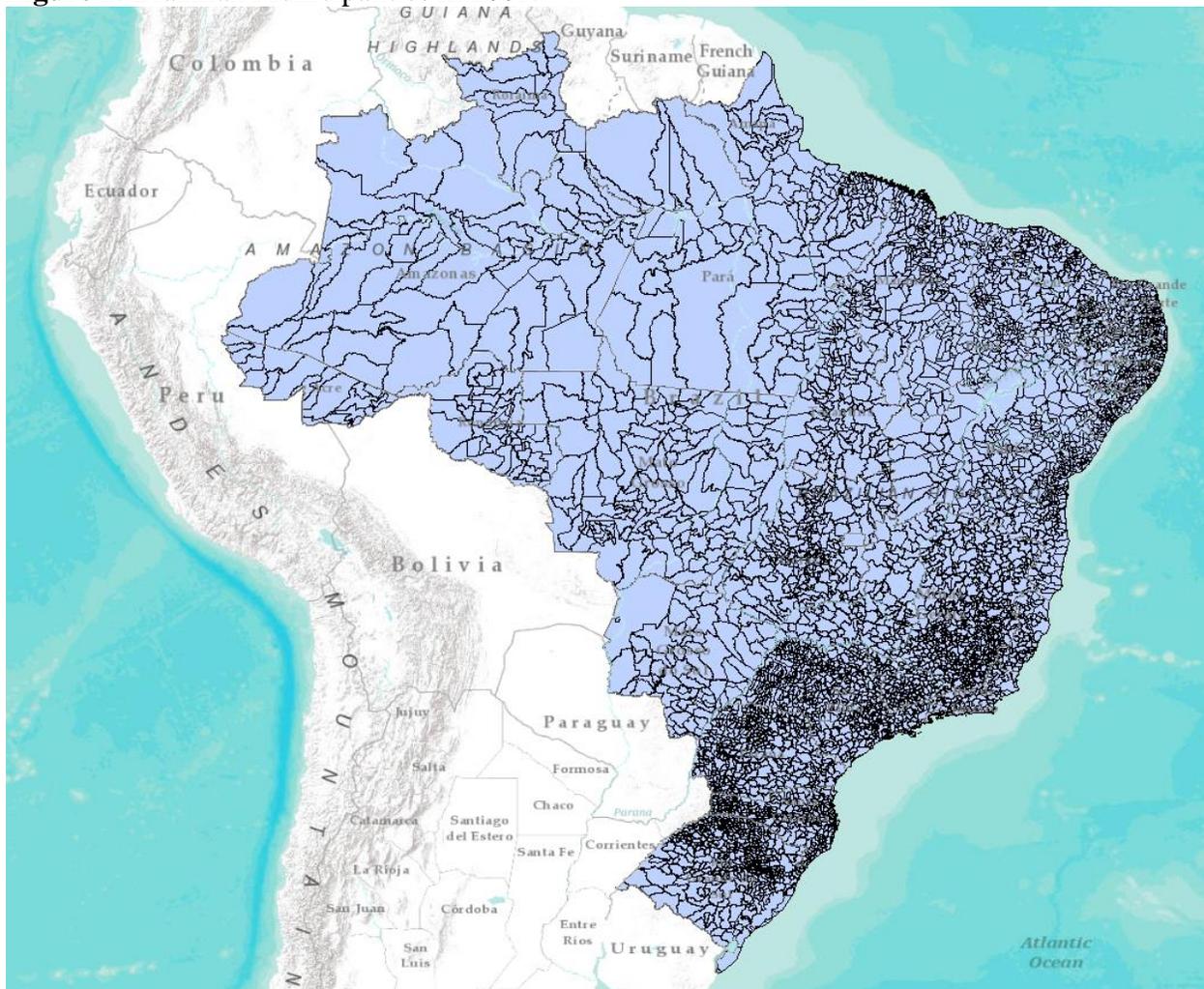
Figures

Figure 1: Nighttime Light Data for Brazil 2001.



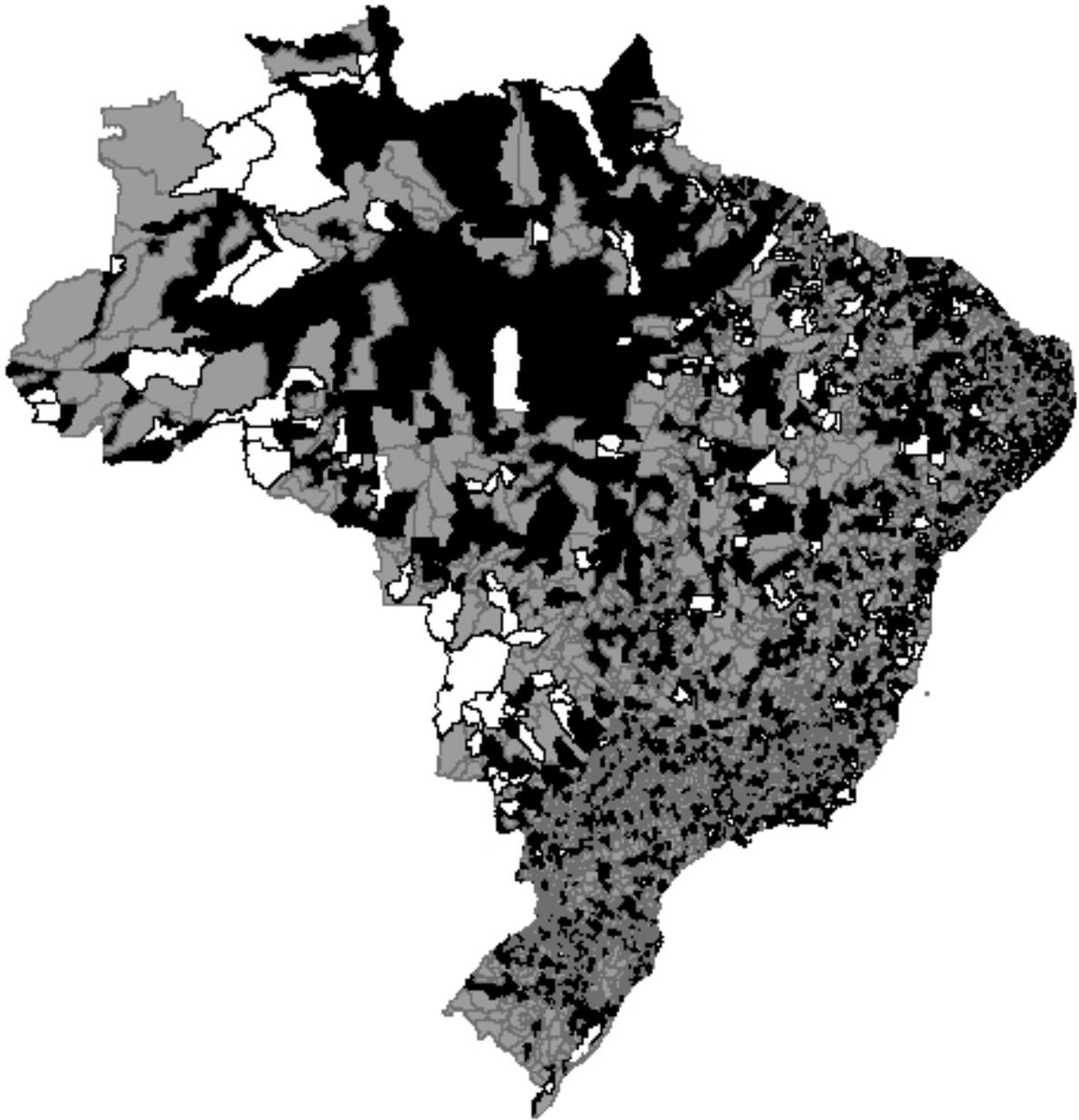
Source: Nighttime light data is provided by the DMSP-OLS Nighttime Lights Time Series, Version 4. This data can be accessed at <https://ngdc.noaa.gov/eog/dmsp/downloadV4composites.html>. We retrieved these files on August 23, 2018.

Figure 2: Brazilian Municipalities in 2001.



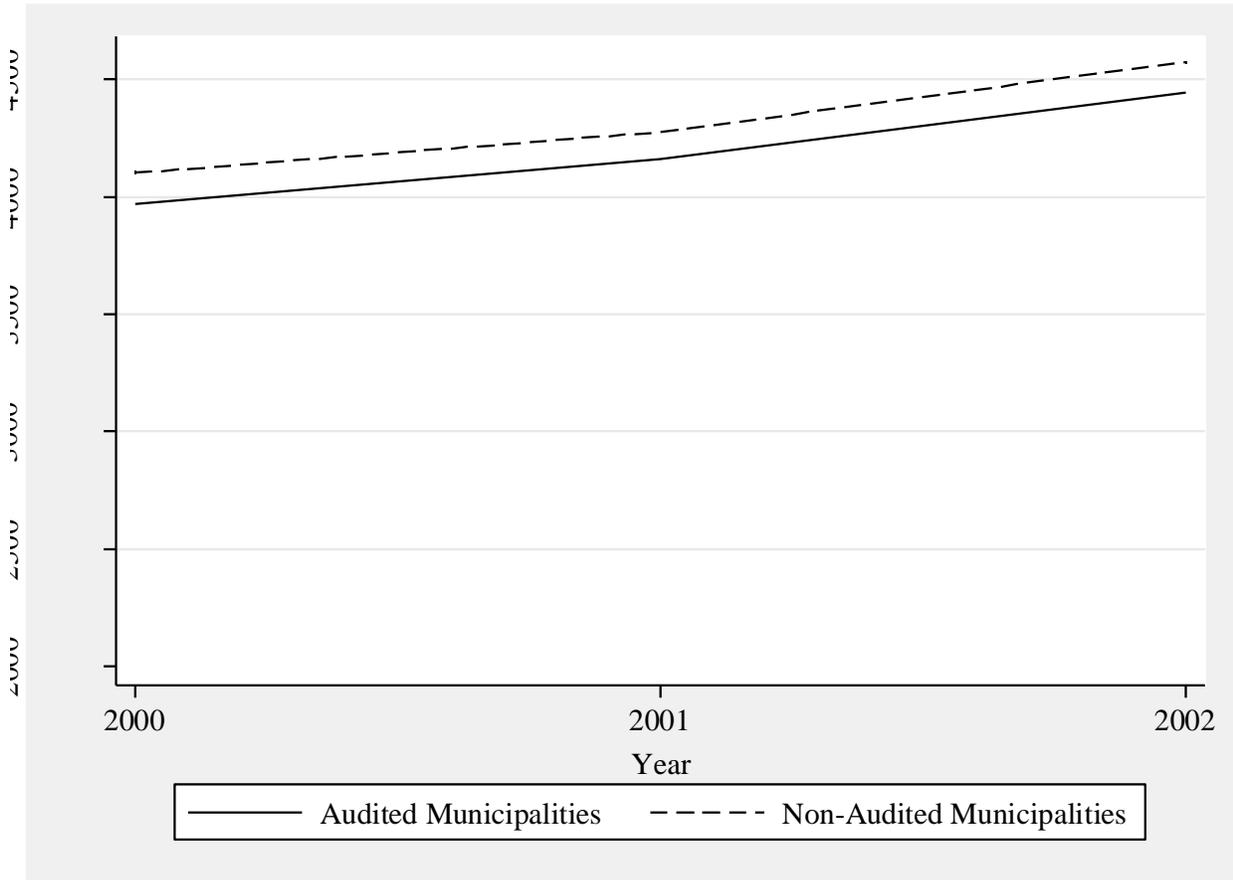
Source: Municipal shapefiles are provided by the IBGE (<https://mapas.ibge.gov.br/bases-e-referenciais/bases-cartograficas/malhas-digitais>). We retrieved these files August 23, 2018.

Figure 3: Audited (black shaded areas) and control (grey shaded areas) municipalities.



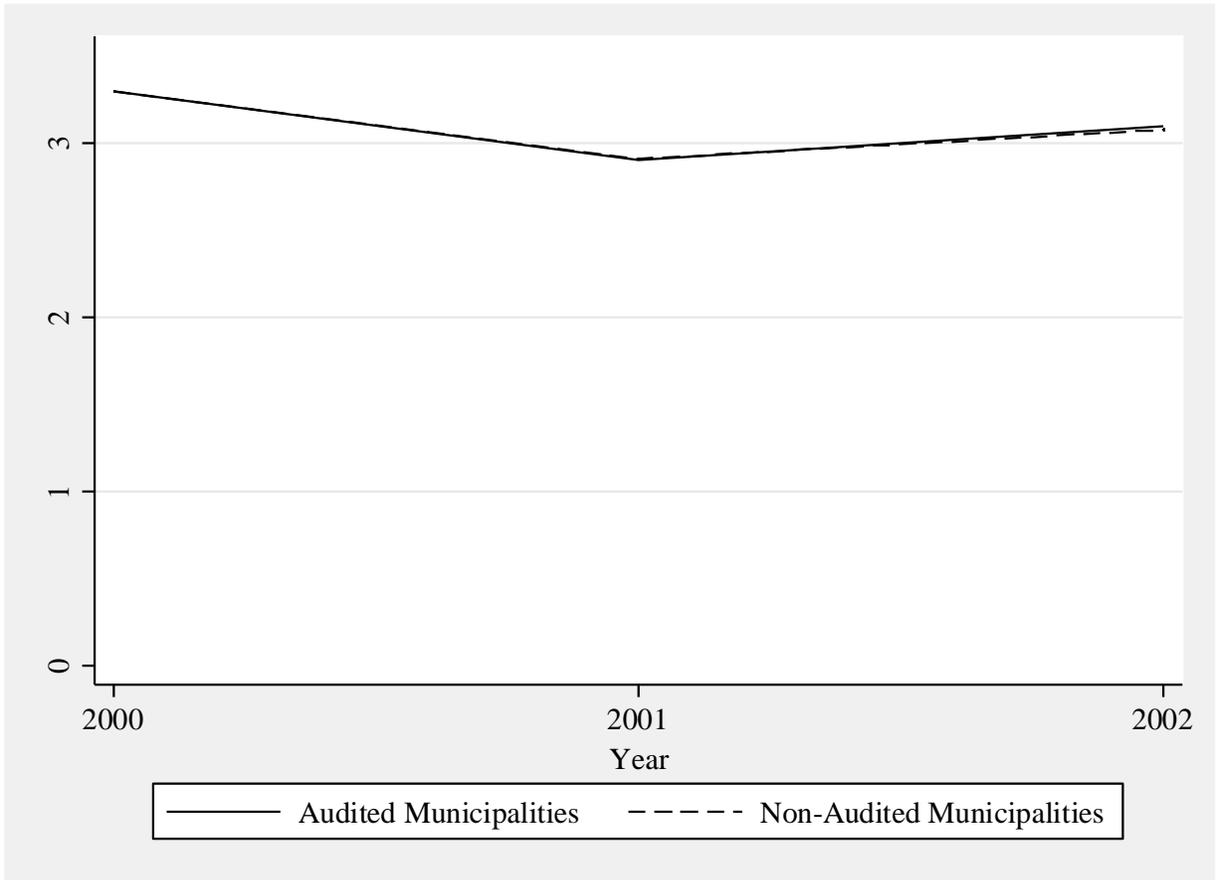
Notes: White shaded municipalities are not included in our analysis as they are either state capitals or are *non*-randomly audited municipalities.

Figure 4: Average GDP per capita for audited versus non-audited municipalities prior to the random audits program (2003).



Source: Both GDP and population data come from the IPEADATA database (<http://www.ipeadata.gov.br/Default.aspx>). A list of audited of audited municipalities is made available at <http://www.cgu.gov.br/assuntos/auditoria-e-fiscalizacao/programa-de-fiscalizacao-em-entes-federativos/edicoes-antiores/municipios>. We consider audits occurring from 2003 through 2013 only.

Figure 5: Average nighttime light score for audited versus non-audited municipalities prior to the random audits program (2003).



Source: Nighttime light data is provided by the DMSP-OLS Nighttime Lights Time Series, Version 4. This data can be accessed at <https://ngdc.noaa.gov/eog/dmsp/downloadV4composites.html>. We retrieved these files on August 23, 2018.

Tables

Table 1: Variable names, sources, and brief descriptions.

Variable	Source	Brief Description
<i>Dependent Variables</i>		
GDP per-capita Growth	IBGE	Annual growth in Gross Domestic Product per-capita.
GDP per-capita	IBGE	Level of Gross Domestic Product per-capita.
Average Light Score Growth	DMSP-OLS	Annual growth in the Average Light Score
Average Light Score	DMSP-OLS	Level of Average Light Score.
<i>Annual Covariates</i>		
Density	IBGE	People per square kilometer.
Agriculture Share of GDP	IBGE	Valued added in Agricultural Sector as a % of GDP.
Services Share of GDP	IBGE	Value added in Services Sector as a % of GDP.
Industry Share of GDP	IBGE	Value added in Industries Sector as a % of GDP.
Public Administration Share of GDP	IBGE	Value added in Government Services Sector as a % of GDP.
Product Tax Share of GDP	IBGE	Product Taxes as a % of GDP.
<i>Institutions</i>		
Institutional Quality	IBGE	Indicator of institutional quality from (1) low to (6) high.
Access to Justice	IBGE	Indicator of access to the judicial system (1) low to (3) high.
<i>Geography</i>		
Rainforest	IBGE	Indicator equal 1 if in rainforest, 0 otherwise.
Distance to Federal Capital	IBGE	Index measuring the municipality's distance to the federal capital.
Distance to State Capital	IBGE	Index measuring the municipality's distance to the state capital.
<i>IBGE Census 2000</i>		
Urban Share	IBGE	% of population residing in an urban area.
Male Share	IBGE	% of population that is male.
Age	IBGE	Average age of population.
Literate	IBGE	% of population aged 10 or more that is literate.
Black	IBGE	% of population that is of a black race.
Informally Employed	IBGE	% of employment in informal workers.

Table 2: Summary Statistics for annual regression variables, 2000- 2013.

Variable	Obs.	Mean.	Std. Dev.	Min	Max
<i>Dependent Variables</i>					
GDP per-capita Growth	67,639	0.027	0.140	-1.998	2.466
GDP per-capita	67,691	5,015	5,642	641	142,944
Average Light Score Growth	72,243	0.052	0.32	-4.016	4.598
Average Light Score	72,954	3.432	6.998	0	63
<i>Annual Covariates</i>					
Density	72,902	8.377	46.594	0.008	1,375
Agriculture Share of GDP	67,691	0.237	0.159	0.000	0.858
Services Share of GDP	67,691	0.555	0.143	0.048	1.443
Industry Share of GDP	67,691	0.151	0.126	-0.566	0.932
Public Administration Share of GDP	67,691	0.271	0.145	0.010	0.810
Product Tax Share of GDP	67,691	0.057	0.040	0.000	0.809

Notes: GDP per-capita, Average Light Intensity, and Density enter regressions as logs.

Table 3: Summary Statistics for cross-sectional pre-period covariates used for matching.

Variable	Obs.	Mean.	Std. Dev.	Min	Max
<i>Institutions</i>					
Institutional Quality	5,159	3.033	0.55	1	4.8
Access to Justice	5,210	4.065	1.806	0	6
<i>Geography</i>					
Rainforest	5,211	0.138	0.345	0	1
Distance to Federal Capital	5,159	1,067	443	32	2,868
Distance to State Capital	5,159	255	161	3.622	1,476
<i>IBGE Census 2000</i>					
Urban Share	5,159	0.612	0.216	0	1
Male Share	5,159	0.691	0.060	0.369	0.891
Age	5,159	35.488	1.753	27.879	45.154
Literate	5,159	0.844	0.118	0.360	1
Black	5,159	0.024	0.031	0	0.408

Table 4: Relationship between GDP per-capita growth, average light score growth, and the informal sector

	Average Light Value					
	(1)	(2)	(3)	(4)	(5)	(6)
GDP per-capita	0.129*** (0.016)					
GDP		0.153*** (0.017)				
Total Income Per-Worker			0.930*** (0.052)			
Formal Income Per-Worker				0.569*** (0.065)		
Informal Income Per-Worker					0.793*** (0.057)	
Informal Employees (%)						-4.711*** (0.118)
Observations	67,282	67,282	5,124	5,124	5,124	5,124
R-Squared	0.616	0.616	0.065	0.019	0.039	0.233
Fixed Effects?	Y	Y	N	N	N	N
Year Effects?	Y	Y	N	N	N	N

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. Data on total, formal, and informal income per worker and informal employees come from the 2000 Census.

Table 5: Relationship between GDP per-capita growth, average light score growth, and the informal sector

	GDP					
	Per-capita (1)	Total (2)	Per-capita (3)	Per-capita (4)	Per-capita (5)	Per-capita (6)
Average Light Value	0.043*** (0.006)	0.052*** (0.006)				
Total Income Per-Worker			1.218*** (0.014)			
Formal Income Per-Worker				1.071*** (0.020)		
Informal Income Per-Worker					1.239*** (0.017)	
Informal Employees (%)						-2.938*** (0.040)
Observations	67,282	67,282	5,159	5,159	5,159	5,159
R-Squared	0.362	0.465	0.604	0.367	0.526	0.495
Fixed Effects?	Y	Y	N	N	N	N
Year Effects?	Y	Y	N	N	N	N

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. Data on total, formal, and informal income per worker and informal employees come from the 2000 Census.

Table 6: Balance of Covariates in the Pretreatment Period (Pre-2003) using the multivariate imbalance statistic and the Coarsened Exact Matching algorithm.

	Matching Type				
	None (1)	States Only (2)	Institutions (3)	Geography (4)	Demographics (5)
<i>Institutions</i>					
Institutional Quality	0.009	-0.004	0.006	0.011	-0.005
Access to Justice	-0.057**	-0.035	-0.063**	-0.061**	-0.035
<i>Geography</i>					
Density	0.307	-0.929	1.193	1.027**	2.794
Distance to Federal Capital	-10.752**	-11.411**	-12.378***	-7.198	7.856
Distance to State Capital	-0.063	-0.082	-2.494	2.889	-10.161
<i>Demographics</i>					
Urban Share	0.012**	0.010	0.014**	0.016***	-0.009
Male Share	0.000	0.001	-0.001	-0.000	0.002
Age	-0.083*	-0.038	-0.110**	-0.102**	-0.053
Literate	-0.001	-0.000	-0.000	-0.000	-0.000
Black	-0.000	-0.001	-0.000	-0.001	0.001
Number of Matched Strata	-	26	366	240	373
Matched Treatment	-	1,726	1,653	1,711	523
Matched Control	-	1,726	3,187	3,285	788

Note: The Coarsened Exact Matching algorithm is developed by Iacus, King, and Porro (2012). We utilize the “cem” command in Stata to run this matching algorithm. Treated municipalities are any municipality selected for audit from 2003 to 2013; matched with all eligible (exclusive of capital and non-randomly selected) municipalities not audited in that time-period. Column (1) contains no matching and is our baseline. Column (2) is a one-for-one matching within states using random selection with equal bins. Column (3) matches on institutional quality and access to justice. Column (4) matches on density, a distance cost index to federal capital, and a distance cost index to state capital. Column (5) matches on the population shares (urban, male, literate, and black) and average age. All matching estimations included state dummies.

Table 7: The effect of audits on GDP per-capita and nighttime light activity; staggered difference-in-difference.

	GDP per-capita		Average Light Score	
	Growth (1)	Level (2)	Growth (3)	Level (4)
Audit Year	-0.002 (0.003)	0.001 (0.004)	-0.002 (0.006)	0.005 (0.008)
Post Audit 1	0.002 (0.003)	0.003 (0.005)	-0.009 (0.006)	-0.002 (0.008)
Post Audit 2	-0.001 (0.004)	0.001 (0.005)	0.003 (0.006)	0.006 (0.009)
Post Audit 3	0.002 (0.004)	0.003 (0.006)	-0.012* (0.006)	-0.006 (0.010)
Post Audit 4	-0.001 (0.004)	0.001 (0.006)	-0.013* (0.007)	-0.014 (0.011)
Post Audit 5	0.000 (0.005)	0.001 (0.007)	-0.000 (0.007)	-0.002 (0.012)
Post Audit 6	-0.001 (0.005)	-0.001 (0.008)	-0.018** (0.008)	-0.017 (0.013)
Post Audit 7	0.001 (0.005)	-0.000 (0.009)	-0.015* (0.008)	-0.019 (0.014)
Post Audit 8	0.012* (0.007)	0.009 (0.011)	-0.016* (0.008)	-0.027* (0.015)
Post Audit 9	0.003 (0.010)	0.003 (0.017)	-0.013 (0.010)	-0.026 (0.017)
Post Audit 10			-0.011 (0.014)	-0.012 (0.025)
Observations	62,480	67,691	67,144	72,524
R-Squared	0.248	0.357	0.572	0.633
Fixed Effects?	Y	Y	Y	Y
Year Effects?	Y	Y	Y	Y
Lagged Level?	Y	N	Y	N

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level.

Table 8: The effect of audits on GDP per-capita and nighttime light activity; staggered difference-in-difference with additional covariates.

	GDP per-capita		Average Light Score	
	Growth (1)	Level (2)	Growth (3)	Level (4)
Audit Year	-0.003 (0.003)	0.000 (0.004)	-0.002 (0.006)	0.001 (0.008)
Post Audit 1	0.002 (0.003)	0.002 (0.004)	-0.010* (0.006)	-0.008 (0.008)
Post Audit 2	-0.001 (0.004)	0.002 (0.005)	0.003 (0.006)	0.000 (0.009)
Post Audit 3	0.003 (0.004)	0.007 (0.005)	-0.012* (0.006)	-0.011 (0.010)
Post Audit 4	-0.000 (0.004)	0.006 (0.005)	-0.013* (0.007)	-0.019* (0.010)
Post Audit 5	0.000 (0.005)	0.006 (0.006)	-0.001 (0.007)	-0.009 (0.011)
Post Audit 6	-0.000 (0.005)	0.005 (0.007)	-0.018** (0.008)	-0.023* (0.013)
Post Audit 7	0.002 (0.005)	0.007 (0.007)	-0.016* (0.008)	-0.026* (0.014)
Post Audit 8	0.012* (0.007)	0.016* (0.009)	-0.016** (0.008)	-0.033** (0.015)
Post Audit 9	0.004 (0.010)	0.013 (0.013)	-0.013 (0.010)	-0.033** (0.017)
Post Audit 10			-0.012 (0.014)	-0.022 (0.025)
Observations	62,480	62,480	67,109	67,321
R-Squared	0.260	0.436	0.576	0.662
Fixed Effects?	Y	Y	Y	Y
Year Effects?	Y	Y	Y	Y
Lagged Level?	Y	N	Y	N

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. Additional controls include logged density, agriculture share of GDP, services share of GDP, tax share of GDP, and industry share of GDP.

Table 9: The effect of audits on GDP per-capita in one randomly selected sample; one-for-one matching based on the state a municipality is located within alone.

<i>Panel a: Average GDP per-capita growth.</i>										
	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7	Year 8	Year 9
Effects of the Audits	-0.005 (0.003)	-0.002 (0.004)	-0.006 (0.004)	-0.001 (0.004)	-0.005 (0.004)	-0.003 (0.005)	-0.004 (0.005)	0.000 (0.006)	0.005 (0.008)	-0.005 (0.011)
Sample 1 to 100 Significance Percentage	1.00%	0.00%	1.00%	0.00%	1.00%	0.00%	0.00%	0.00%	4.00%	0.00%
Observations	40,724									
R-Squared	0.256									
<i>Panel b: Average GDP per-capita level.</i>										
	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7	Year 8	Year 9
Effects of the Audits	-0.003 (0.004)	-0.003 (0.004)	-0.005 (0.005)	0.000 (0.005)	-0.002 (0.006)	-0.002 (0.007)	-0.004 (0.007)	0.000 (0.008)	0.005 (0.010)	0.000 (0.014)
Sample 1 to 100 Significance Percentage	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%
Observations	40,724									
R-Squared	0.449									
<i>Notes:</i> *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. All regressions include state and time fixed effects. All growth regressions include the relevant initial level. Additional controls include logged density, agriculture share of GDP, services share of GDP, tax share of GDP, and industry share of GDP. Our treatment group is all municipalities audited between 2003 and 2012 (inclusive); matched with all eligible (exclusive of capital and non-randomly selected) municipalities not audited in that time period. Thus, municipalities selected for the first time in 2013 may be included as a control.										

Table 10: The effect of audits on nighttime light intensity in one randomly selected sample; one-for-one matching based on the state a municipality is located within alone.

<i>Panel a: Average nighttime light intensity growth.</i>											
	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7	Year 8	Year 9	Year 10
Effects of the Audits	0.000 (0.006)	-0.011* (0.006)	0.005 (0.006)	-0.009 (0.007)	-0.011 (0.007)	0.001 (0.008)	-0.017** (0.008)	-0.014 (0.009)	-0.015* (0.009)	-0.011 (0.010)	-0.009 (0.016)
Sample 1 to 100 Significance Percentage	0.00%	67.00%	0.00%	17.00%	29.00%	0.00%	64.00%	35.00%	48.00%	8.00%	0.00%
Observations						44,496					
R-Squared						0.584					
<i>Panel b: Average nighttime light intensity.</i>											
	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7	Year 8	Year 9	Year 10
Effects of the Audits	0.002 (0.008)	-0.009 (0.008)	0.000 (0.009)	-0.010 (0.010)	-0.017 (0.011)	-0.006 (0.012)	-0.020 (0.014)	-0.024 (0.015)	-0.032** (0.016)	-0.029 (0.019)	-0.016 (0.027)
Sample 1 to 100 Significance Percentage	0.00%	5.00%	0.00%	3.00%	35.00%	2.00%	21.00%	30.00%	74.00%	48.00%	0.00%
Observations						44,623					
R-Squared						0.668					

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. All regressions include state and time fixed effects. All growth regressions include the relevant initial level. Additional controls include logged density, agriculture share of GDP, services share of GDP, tax share of GDP, and industry share of GDP. Our treatment group is all municipalities audited between 2003 and 2013 (inclusive); matched with all eligible (exclusive of capital and non-randomly selected) municipalities not audited in that time-period.

Table 11: The effect of audits on GDP per-capita and nighttime light activity; staggered difference-in-difference regression matched on institutional quality.

	GDP per-capita		Average Light Score	
	Growth (1)	Level (2)	Growth (3)	Level (4)
Audit Year	-0.003 (0.003)	-0.001 (0.004)	-0.003 (0.006)	-0.003 (0.008)
Post Audit 1	-0.001 (0.004)	-0.001 (0.004)	-0.011* (0.006)	-0.012 (0.009)
Post Audit 2	-0.003 (0.004)	-0.002 (0.005)	0.004 (0.007)	-0.002 (0.010)
Post Audit 3	0.002 (0.004)	0.004 (0.005)	-0.010 (0.007)	-0.012 (0.011)
Post Audit 4	-0.002 (0.004)	0.002 (0.006)	-0.013* (0.007)	-0.020* (0.012)
Post Audit 5	-0.001 (0.005)	0.002 (0.007)	0.001 (0.008)	-0.009 (0.013)
Post Audit 6	-0.003 (0.005)	-0.001 (0.007)	-0.015* (0.009)	-0.019 (0.014)
Post Audit 7	0.001 (0.005)	0.002 (0.008)	-0.012 (0.009)	-0.021 (0.015)
Post Audit 8	0.007 (0.007)	0.008 (0.010)	-0.013 (0.009)	-0.027* (0.016)
Post Audit 9	-0.003 (0.011)	0.006 (0.014)	-0.010 (0.010)	-0.029 (0.018)
Post Audit 10			-0.009 (0.016)	-0.017 (0.027)
Observations	57,957	57,957	62,315	62,523
R-Squared	0.262	0.438	0.575	0.662
Fixed Effects?	Y	Y	Y	Y
Year Effects?	Y	Y	Y	Y
Lagged Level?	Y	N	Y	N

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. Observations are weighted according to the resultant Coarsened Exact Matching weights where audited municipalities are matched with our control municipalities according to pre-treatment institutional quality and access to justice. Additional controls include logged density, agriculture share of GDP, services share of GDP, tax share of GDP, and industry share of GDP. For specifications (1) and (2): our treatment group is all municipalities audited between 2003 and 2012 (inclusive). For specifications (3) and (4): our treatment group is all municipalities audited between 2003 and 2013 (inclusive).

Table 12: The effect of audits on GDP per-capita and nighttime light activity; staggered difference-in-difference regression matched on geographical factors.

	GDP per-capita		Average Light Score	
	Growth (1)	Level (2)	Growth (3)	Level (4)
Audit Year	-0.003 (0.003)	-0.001 (0.004)	-0.002 (0.006)	0.000 (0.008)
Post Audit 1	-0.002 (0.004)	-0.003 (0.004)	-0.010 (0.006)	-0.008 (0.009)
Post Audit 2	-0.003 (0.004)	-0.004 (0.005)	0.003 (0.007)	-0.001 (0.010)
Post Audit 3	0.001 (0.004)	0.001 (0.005)	-0.010 (0.007)	-0.011 (0.011)
Post Audit 4	-0.003 (0.004)	-0.000 (0.006)	-0.014* (0.007)	-0.020* (0.012)
Post Audit 5	-0.003 (0.005)	-0.002 (0.007)	-0.000 (0.008)	-0.008 (0.013)
Post Audit 6	-0.001 (0.005)	-0.002 (0.007)	-0.016* (0.009)	-0.021 (0.015)
Post Audit 7	0.002 (0.005)	0.001 (0.008)	-0.014 (0.009)	-0.023 (0.016)
Post Audit 8	0.011 (0.008)	0.009 (0.010)	-0.015 (0.009)	-0.030* (0.017)
Post Audit 9	0.002 (0.011)	0.006 (0.014)	-0.011 (0.011)	-0.029 (0.020)
Post Audit 10			-0.007 (0.016)	-0.016 (0.028)
Observations	59,954	59,954	64,343	64,545
R-Squared	0.261	0.437	0.579	0.667
Fixed Effects?	Y	Y	Y	Y
Year Effects?	Y	Y	Y	Y
Lagged Level?	Y	N	Y	N

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. Observations are weighted according to the resultant Coarsened Exact Matching weights where audited municipalities are matched with our control municipalities according to pre-treatment density, a distance cost index to federal capital, and a distance cost index to state capital. Additional controls include logged density, agriculture share of GDP, services share of GDP, tax share of GDP, and industry share of GDP. For specifications (1) and (2): our treatment group is all municipalities audited between 2003 and 2012 (inclusive). For specifications (3) and (4): our treatment group is all municipalities audited between 2003 and 2013 (inclusive).

Table 13: The effect of audits on GDP per-capita and nighttime light activity; staggered difference-in-difference regression matched on demographic factors.

	GDP per-capita		Average Light Score	
	Growth (1)	Level (2)	Growth (3)	Level (4)
Audit Year	0.002 (0.006)	0.002 (0.007)	0.006 (0.010)	0.010 (0.014)
Post Audit 1	0.003 (0.007)	0.003 (0.008)	-0.013 (0.009)	-0.008 (0.013)
Post Audit 2	-0.004 (0.008)	-0.001 (0.010)	-0.003 (0.011)	-0.007 (0.016)
Post Audit 3	-0.008 (0.008)	-0.008 (0.011)	-0.005 (0.011)	-0.005 (0.018)
Post Audit 4	-0.006 (0.008)	-0.007 (0.012)	-0.031*** (0.012)	-0.035* (0.020)
Post Audit 5	-0.012 (0.009)	-0.015 (0.013)	-0.012 (0.013)	-0.032 (0.022)
Post Audit 6	-0.006 (0.009)	-0.011 (0.013)	-0.041*** (0.014)	-0.060** (0.024)
Post Audit 7	-0.000 (0.009)	-0.006 (0.014)	-0.030** (0.015)	-0.063** (0.027)
Post Audit 8	0.013 (0.012)	0.007 (0.016)	-0.031** (0.014)	-0.068** (0.027)
Post Audit 9	0.003 (0.022)	0.009 (0.026)	-0.032* (0.018)	-0.077** (0.032)
Post Audit 10			-0.050* (0.026)	-0.086* (0.047)
Observations	15,674	15,674	16,836	16,893
R-Squared	0.291	0.371	0.575	0.663
Fixed Effects?	Y	Y	Y	Y
Year Effects?	Y	Y	Y	Y
Lagged Level?	Y	N	Y	N

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. Observations are weighted according to the resultant Coarsened Exact Matching weights where audited municipalities are matched with our control municipalities according to pre-treatment population shares (urban, male, literate black) and average age. Additional controls include logged density, agriculture share of GDP, services share of GDP, tax share of GDP, and industry share of GDP. For specifications (1) and (2): our treatment group is all municipalities audited between 2003 and 2012 (inclusive). For specifications (3) and (4): our treatment group is all municipalities audited between 2003 and 2013 (inclusive).

Table 14: The effect of audits on GDP per-capita in one randomly selected sample; one-for-one matching based on matched on federal transfers per-capita in the post-treatment period and the state a municipality is located within.

Panel a: Average GDP per-capita growth.

	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7	Year 8	Year 9
Effects of the Audits	-0.005 (0.003)	-0.000 (0.004)	-0.004 (0.004)	-0.000 (0.004)	-0.004 (0.004)	-0.001 (0.005)	-0.002 (0.005)	0.001 (0.005)	0.010 (0.007)	-0.000 (0.011)
Sample 1 to 100 Significance Percentage	1.00%	0.00%	1.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%
Observations	39,950									
R-Squared	0.263									

Panel b: Average GDP per-capita level.

	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7	Year 8	Year 9
Effects of the Audits	-0.003 (0.004)	-0.002 (0.004)	-0.004 (0.005)	-0.000 (0.005)	-0.002 (0.006)	-0.002 (0.007)	-0.004 (0.007)	-0.001 (0.008)	0.006 (0.010)	0.003 (0.014)
Sample 1 to 100 Significance Percentage	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%
Observations	39,950									
R-Squared	0.450									

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. All regressions include state and time fixed effects. All growth regressions include the relevant initial level. Additional controls include logged density, agriculture share of GDP, services share of GDP, tax share of GDP, and industry share of GDP. Our treatment group is all municipalities audited between 2003 and 2012 (inclusive); matched with all eligible (exclusive of capital and non-randomly selected) municipalities not audited in that time-period. Thus, municipalities selected for the first time in 2013 may be included as a control.

Table 15: The effect of audits on nighttime light intensity in one randomly selected sample; one-for-one matching based on matched on federal transfers per-capita in the post-treatment period and the state a municipality is located within.

Panel a: Average nighttime light intensity growth.

	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7	Year 8	Year 9	Year 10
Effects of the Audits	0.002 (0.006)	-0.010* (0.006)	0.006 (0.006)	-0.008 (0.007)	-0.010 (0.007)	0.004 (0.008)	-0.011 (0.009)	-0.007 (0.009)	-0.012 (0.009)	-0.005 (0.011)	-0.007 (0.016)
Sample 1 to 100 Significance Percentage	0.00%	77.00%	0.00%	13.00%	37.00%	0.00%	66.00%	36.00%	57.00%	3.00%	0.00%
Observations						43,639					
R-Squared						0.586					

Panel b: Average nighttime light intensity.

	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7	Year 8	Year 9	Year 10
Effects of the Audits	0.003 (0.008)	-0.009 (0.008)	0.001 (0.009)	-0.008 (0.010)	-0.016 (0.011)	-0.003 (0.012)	-0.014 (0.014)	-0.014 (0.015)	-0.024 (0.016)	-0.021 (0.019)	-0.010 (0.028)
Sample 1 to 100 Significance Percentage	0.00%	2.00%	0.00%	0.00%	32.00%	0.00%	28.00%	36.00%	72.00%	38.00%	0.00%
Observations						43,752					
R-Squared						0.675					

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. All regressions include state and time fixed effects. All growth regressions include the relevant initial level. Additional controls include logged density, agriculture share of GDP, services share of GDP, tax share of GDP, and industry share of GDP. Our treatment group is all municipalities audited between 2003 and 2013 (inclusive); matched with all eligible (exclusive of capital and non-randomly selected) municipalities not audited in that time-period.

Appendix A

Table A1: The effect of audits on GDP per-capita and nighttime light activity; staggered difference-in-difference regression excluding audited municipalities with multiple selections.

	GDP per-capita		Average Light Score	
	Growth (1)	Level (2)	Growth (3)	Level (4)
Audit Year	-0.003 (0.004)	0.000 (0.004)	-0.002 (0.006)	0.002 (0.008)
Post Audit 1	0.002 (0.004)	0.002 (0.004)	-0.012* (0.006)	-0.009 (0.009)
Post Audit 2	0.000 (0.004)	0.003 (0.005)	0.003 (0.007)	-0.001 (0.010)
Post Audit 3	0.005 (0.004)	0.009 (0.006)	-0.009 (0.007)	-0.009 (0.010)
Post Audit 4	0.001 (0.004)	0.007 (0.006)	-0.012 (0.008)	-0.017 (0.011)
Post Audit 5	-0.001 (0.005)	0.005 (0.006)	0.002 (0.008)	-0.004 (0.012)
Post Audit 6	-0.001 (0.005)	0.003 (0.007)	-0.014 (0.009)	-0.016 (0.014)
Post Audit 7	0.003 (0.006)	0.007 (0.008)	-0.013 (0.009)	-0.018 (0.015)
Post Audit 8	0.013* (0.008)	0.016 (0.010)	-0.014 (0.009)	-0.026* (0.016)
Post Audit 9	-0.000 (0.010)	0.009 (0.013)	-0.005 (0.011)	-0.020 (0.018)
Post Audit 10			0.008 (0.018)	0.011 (0.030)
Observations	59,900	59,900	64,320	64,529
R-Squared	0.264	0.435	0.574	0.661
Fixed Effects?	Y	Y	Y	Y
Year Effects?	Y	Y	Y	Y
Lagged Level?	Y	N	Y	N

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. Additional controls include logged density, agriculture share of GDP, services share of GDP, tax share of GDP, and industry share of GDP.

Table A2: The effect of audits on GDP per-capita and nighttime light activity; staggered difference-in-difference regression excluding audited municipalities with no post-treatment observations.

	GDP per-capita		Average Light Score	
	Growth (1)	Level (2)	Growth (3)	Level (4)
Audit Year	-0.003 (0.003)	0.000 (0.004)	-0.002 (0.006)	0.000 (0.008)
Post Audit 1	0.002 (0.004)	0.002 (0.004)	-0.010* (0.006)	-0.008 (0.008)
Post Audit 2	-0.001 (0.004)	0.002 (0.005)	0.003 (0.006)	0.000 (0.009)
Post Audit 3	0.003 (0.004)	0.007 (0.005)	-0.012* (0.006)	-0.012 (0.010)
Post Audit 4	-0.001 (0.004)	0.006 (0.005)	-0.014* (0.007)	-0.020* (0.011)
Post Audit 5	0.000 (0.005)	0.006 (0.006)	-0.001 (0.007)	-0.009 (0.011)
Post Audit 6	-0.000 (0.005)	0.005 (0.007)	-0.018** (0.008)	-0.023* (0.013)
Post Audit 7	0.002 (0.005)	0.007 (0.007)	-0.016* (0.008)	-0.026* (0.014)
Post Audit 8	0.012* (0.007)	0.016* (0.009)	-0.016** (0.008)	-0.033** (0.015)
Post Audit 9	0.004 (0.010)	0.013 (0.013)	-0.013 (0.010)	-0.033** (0.017)
Post Audit 10			-0.012 (0.014)	-0.022 (0.025)
Observations	61,461	61,461	66,666	66,876
R-Squared	0.260	0.436	0.576	0.662
Fixed Effects?	Y	Y	Y	Y
Year Effects?	Y	Y	Y	Y
Lagged Level?	Y	N	Y	N

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. Additional controls include logged density, agriculture share of GDP, services share of GDP, tax share of GDP, and industry share of GDP.

Table A3: The effect of audits on GDP per-capita and nighttime light activity; staggered difference-in-difference regression excluding multiple selections and municipalities with no post-treatment observations.

	GDP per-capita		Average Light Score	
	Growth (1)	Level (2)	Growth (3)	Level (4)
Audit Year	-0.003 (0.004)	0.000 (0.004)	-0.003 (0.006)	0.002 (0.008)
Post Audit 1	0.002 (0.004)	0.002 (0.005)	-0.012* (0.006)	-0.009 (0.009)
Post Audit 2	-0.000 (0.004)	0.003 (0.005)	0.002 (0.007)	-0.001 (0.010)
Post Audit 3	0.005 (0.004)	0.009 (0.006)	-0.009 (0.007)	-0.010 (0.010)
Post Audit 4	0.001 (0.004)	0.007 (0.006)	-0.012 (0.008)	-0.017 (0.011)
Post Audit 5	-0.001 (0.005)	0.005 (0.006)	0.002 (0.008)	-0.004 (0.012)
Post Audit 6	-0.001 (0.005)	0.003 (0.007)	-0.014 (0.009)	-0.016 (0.014)
Post Audit 7	0.003 (0.006)	0.007 (0.008)	-0.013 (0.009)	-0.019 (0.015)
Post Audit 8	0.013* (0.008)	0.016 (0.010)	-0.014 (0.009)	-0.026* (0.016)
Post Audit 9	-0.000 (0.010)	0.009 (0.013)	-0.005 (0.011)	-0.020 (0.018)
Post Audit 10			0.008 (0.018)	0.011 (0.030)
Observations	58,881	58,881	63,877	64,084
R-Squared	0.263	0.434	0.574	0.661
Fixed Effects?	Y	Y	Y	Y
Year Effects?	Y	Y	Y	Y
Lagged Level?	Y	N	Y	N

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. Additional controls include logged density, agriculture share of GDP, services share of GDP, tax share of GDP, and industry share of GDP.

Table A4: The effect of audits on GDP per-capita and nighttime light activity; staggered difference-in-difference regression excluding staggered difference-in-difference regression excluding municipalities that changed borders or were created between 2000 and 2010.

	GDP per-capita		Average Light Score	
	Growth (1)	Level (2)	Growth (3)	Level (4)
Audit Year	-0.003 (0.003)	0.000 (0.004)	-0.003 (0.006)	0.001 (0.008)
Post Audit 1	0.001 (0.004)	0.001 (0.004)	-0.011* (0.006)	-0.008 (0.008)
Post Audit 2	-0.000 (0.004)	0.002 (0.005)	0.003 (0.006)	0.001 (0.009)
Post Audit 3	0.004 (0.004)	0.007 (0.005)	-0.014** (0.006)	-0.013 (0.010)
Post Audit 4	0.000 (0.004)	0.007 (0.006)	-0.013* (0.007)	-0.018* (0.010)
Post Audit 5	0.000 (0.005)	0.006 (0.006)	-0.003 (0.007)	-0.009 (0.011)
Post Audit 6	0.000 (0.005)	0.005 (0.007)	-0.018** (0.008)	-0.021* (0.013)
Post Audit 7	0.003 (0.005)	0.008 (0.007)	-0.017** (0.008)	-0.025* (0.014)
Post Audit 8	0.012* (0.007)	0.015 (0.009)	-0.017** (0.008)	-0.032** (0.014)
Post Audit 9	0.006 (0.010)	0.016 (0.013)	-0.013 (0.010)	-0.031* (0.017)
Post Audit 10			-0.013 (0.014)	-0.022 (0.025)
Observations	61,066	61,066	65,673	65,850
R-Squared	0.255	0.439	0.579	0.670
Fixed Effects?	Y	Y	Y	Y
Year Effects?	Y	Y	Y	Y
Lagged Level?	Y	N	Y	N

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. Additional controls include logged density, agriculture share of GDP, services share of GDP, tax share of GDP, and industry share of GDP.

Appendix B

Table B1: The effect of audits on nighttime light activity using alternative light measures; staggered difference-in-difference regression.

	Average Light Score		
	Sum/Area (1)	Inverse Hyperbolic Sine Growth (2)	Level (3)
Audit Year	0.001 (0.008)	0.002 (0.004)	0.001 (0.005)
Post Audit 1	-0.008 (0.008)	-0.005 (0.003)	-0.005 (0.005)
Post Audit 2	0.000 (0.009)	0.005 (0.004)	0.001 (0.006)
Post Audit 3	-0.011 (0.010)	-0.007* (0.004)	-0.007 (0.007)
Post Audit 4	-0.019* (0.010)	-0.010** (0.005)	-0.015* (0.008)
Post Audit 5	-0.009 (0.011)	-0.007 (0.005)	-0.018** (0.009)
PostAudit 6	-0.023* (0.013)	-0.015** (0.006)	-0.029*** (0.010)
Post Audit 7	-0.026* (0.014)	-0.022*** (0.006)	-0.041*** (0.011)
Post Audit 8	-0.033** (0.015)	-0.015*** (0.005)	-0.045*** (0.011)
Post Audit 9	-0.033** (0.017)	-0.014** (0.007)	-0.048*** (0.013)
Post Audit 10	-0.022 (0.025)	-0.020*** (0.008)	-0.064*** (0.018)
Observations	67,321	67,691	67,691
R-Squared	0.662	0.580	0.650
Fixed Effects?	Y	Y	Y
Year Effects?	Y	Y	Y
Lagged Level?	Y	N	Y

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. Additional controls include logged density, agriculture share of GDP, services share of GDP, tax share of GDP, and industry share of GDP.

Table B2: The effects of audits on nighttime light activity; staggered difference-in-difference regression excluding average light intensity top-coded municipalities.

	Excluding Top 1 %		Excluding Top 5 %	
	Growth (1)	Level (2)	Growth (3)	Level (4)
Audit Year	-0.002 (0.006)	-0.000 (0.008)	-0.003 (0.006)	-0.001 (0.008)
Post Audit 1	-0.010* (0.006)	-0.009 (0.008)	-0.011* (0.006)	-0.011 (0.008)
Post Audit 2	0.002 (0.006)	-0.001 (0.009)	0.002 (0.006)	-0.003 (0.009)
Post Audit 3	-0.012* (0.006)	-0.011 (0.010)	-0.013** (0.007)	-0.015 (0.010)
Post Audit 4	-0.014* (0.007)	-0.020* (0.010)	-0.015** (0.007)	-0.023** (0.011)
Post Audit 5	-0.000 (0.007)	-0.008 (0.011)	-0.001 (0.008)	-0.011 (0.012)
Post Audit 6	-0.018** (0.008)	-0.023* (0.013)	-0.018** (0.008)	-0.023* (0.013)
Post Audit 7	-0.016* (0.008)	-0.026* (0.014)	-0.017* (0.009)	-0.029** (0.014)
Post Audit 8	-0.017** (0.008)	-0.033** (0.014)	-0.017* (0.009)	-0.035** (0.015)
Post Audit 9	-0.013 (0.010)	-0.034** (0.017)	-0.014 (0.010)	-0.035** (0.017)
Post Audit 10	-0.012 (0.014)	-0.022 (0.025)	-0.015 (0.015)	-0.029 (0.026)
Observations	66,437	66,649	63,769	63,981
R-Squared	0.580	0.667	0.583	0.668
Fixed Effects?	Y	Y	Y	Y
Year Effects?	Y	Y	Y	Y
Lagged Level?	Y	N	Y	N

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. Additional controls include logged density, agriculture share of GDP, services share of GDP, tax share of GDP, and industry share of GDP. Column (1) and (2) exclude municipalities with an average light intensity score of 41.12 or higher; columns (3) and (4) exclude municipalities with an average light intensity score of 13.54 or higher.

Appendix C

Table C1: The effect of audits on GDP per-capita and nighttime light activity; average post treatment effect.

	GDP per-capita		Average Light Score	
	Growth (1)	Level (2)	Growth (3)	Level (4)
Audit Year	-0.003 (0.003)	0.000 (0.004)	-0.001 (0.006)	0.004 (0.008)
Post Audit Years	0.001 (0.003)	0.002 (0.005)	-0.008* (0.005)	-0.009 (0.009)
Observations	62,480	62,480	67,109	67,321
R-Squared	0.248	0.317	0.575	0.659
Fixed Effects?	Y	Y	Y	Y
Year Effects?	Y	Y	Y	Y
Lagged Level?	Y	N	Y	N

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level.

Table C2: The effect of audits on GDP per-capita and nighttime light activity; average post treatment effect with additional controls.

	GDP per-capita		Average Light Score	
	Growth (1)	Level (2)	Growth (3)	Level (4)
Audit Year	-0.003 (0.003)	-0.000 (0.004)	-0.001 (0.006)	0.002 (0.008)
Post Audit Years	0.001 (0.003)	0.005 (0.004)	-0.009* (0.005)	-0.012 (0.008)
Observations	62,480	62,480	67,109	67,321
R-Squared	0.260	0.436	0.576	0.662
Fixed Effects?	Y	Y	Y	Y
Year Effects?	Y	Y	Y	Y
Lagged Level?	Y	N	Y	N

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. All regressions include logged density, agriculture share of GDP, services share of GDP, tax share of GDP, and industry share of GDP as controls.

Appendix D

Table D1: Comparing the contemporaneous vs post treatment effects of audits on GDP per-capita and nighttime light activity; staggered difference-in-difference regression using data from the first satellite-year combo available.

	Average Light Score	
	Growth (1)	Level (2)
Post Audit 0	0.003 (0.006)	0.002 (0.008)
Post Audit 1	-0.006 (0.006)	-0.007 (0.008)
Post Audit 2	0.008 (0.007)	0.005 (0.009)
Post Audit 3	-0.007 (0.007)	-0.005 (0.010)
Post Audit 4	-0.020*** (0.008)	-0.022** (0.011)
Post Audit 5	0.000 (0.008)	-0.009 (0.011)
Post Audit 6	-0.017** (0.008)	-0.024* (0.013)
Post Audit 7	-0.016* (0.009)	-0.027** (0.014)
Post Audit 8	-0.018** (0.009)	-0.035** (0.014)
Post Audit 9	-0.015 (0.010)	-0.035** (0.017)
Post Audit 10	-0.013 (0.015)	-0.023 (0.025)
Observations	66,864	67,167
R-Squared	0.572	0.701
Fixed Effects?	Y	Y
Year Effects?	Y	Y
Lagged Level?	Y	N

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. Additional controls include logged density, agriculture share of GDP, services share of GDP, tax share of GDP, and industry share of GDP.

Appendix E

Table E1: Comparing the contemporaneous vs post treatment effects of audits on GDP per-capita and nighttime light activity; staggered difference-in-difference regression using 1999 as the first potential pre-treatment year.

	GDP per-capita		Average Light Intensity	
	Growth (1)	Level (2)	Growth (3)	Level (4)
Audit Year	-0.003 (0.003)	0.000 (0.004)	0.001 (0.006)	0.004 (0.008)
Post Audit 1	0.001 (0.003)	0.002 (0.004)	-0.007 (0.006)	-0.004 (0.008)
Post Audit 2	-0.002 (0.004)	0.001 (0.005)	0.006 (0.006)	0.004 (0.009)
Post Audit 3	0.003 (0.004)	0.007 (0.005)	-0.009 (0.006)	-0.008 (0.010)
Post Audit 4	-0.001 (0.004)	0.005 (0.005)	-0.010 (0.007)	-0.016 (0.010)
Post Audit 5	-0.001 (0.005)	0.005 (0.006)	0.002 (0.007)	-0.005 (0.011)
Post Audit 6	-0.001 (0.004)	0.003 (0.007)	-0.014* (0.008)	-0.019 (0.013)
Post Audit 7	0.001 (0.005)	0.006 (0.007)	-0.012 (0.008)	-0.022 (0.014)
Post Audit 8	0.010 (0.007)	0.013 (0.009)	-0.013 (0.008)	-0.029** (0.014)
Post Audit 9	0.002 (0.010)	0.011 (0.013)	-0.009 (0.009)	-0.028* (0.017)
Post Audit 10			-0.007 (0.014)	-0.017 (0.025)
Observations	67,636	67,636	72,208	72,445
R-Squared	0.255	0.475	0.561	0.641
Fixed Effects?	Y	Y	Y	Y
Year Effects?	Y	Y	Y	Y
Lagged Level?	Y	N	Y	N

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. Additional controls include logged density, agriculture share of GDP, services share of GDP, tax share of GDP, and industry share of GDP.

Table E2: Comparing the contemporaneous vs post treatment effects of audits on GDP per-capita and nighttime light activity; staggered difference-in-difference regression using 2001 as the first potential pre-treatment year.

	GDP per-capita		Average Light Intensity	
	Growth (1)	Level (2)	Growth (3)	Level (4)
Audit Year	-0.002 (0.003)	0.001 (0.004)	-0.000 (0.006)	0.000 (0.008)
Post Audit 1	0.003 (0.004)	0.003 (0.004)	-0.008 (0.006)	-0.008 (0.008)
Post Audit 2	-0.000 (0.004)	0.003 (0.005)	0.004 (0.006)	-0.000 (0.009)
Post Audit 3	0.005 (0.004)	0.009* (0.005)	-0.010 (0.007)	-0.012 (0.010)
Post Audit 4	0.001 (0.004)	0.008 (0.006)	-0.012 (0.007)	-0.020* (0.011)
Post Audit 5	0.002 (0.005)	0.008 (0.006)	0.001 (0.008)	-0.009 (0.012)
Post Audit 6	0.002 (0.005)	0.007 (0.007)	-0.015* (0.008)	-0.023* (0.013)
Post Audit 7	0.004 (0.005)	0.010 (0.007)	-0.014 (0.009)	-0.026* (0.014)
Post Audit 8	0.014** (0.007)	0.019** (0.009)	-0.014 (0.009)	-0.033** (0.015)
Post Audit 9	0.007 (0.010)	0.017 (0.013)	-0.011 (0.010)	-0.033** (0.017)
Post Audit 10			-0.009 (0.015)	-0.023 (0.025)
Observations	57,321	57,321	61,999	62,197
R-Squared	0.274	0.398	0.582	0.674
Fixed Effects?	Y	Y	Y	Y
Year Effects?	Y	Y	Y	Y
Lagged Level?	Y	N	Y	N

Notes: *, **, *** indicate statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors given in parentheses are clustered at the municipal level. Additional controls include logged density, agriculture share of GDP, services share of GDP, tax share of GDP, and industry share of GDP.