Unintended consequences of anti-corruption strategies: Public fiscal audits and deforestation in the Brazilian Amazon*

Elías Cisneros[†], Jorge Hargrave[‡] and Krisztina Kis-Katos[§]

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Abstract

This paper documents an increase in deforestation in Amazon municipalities as a consequence of the otherwise successful federal anti-corruption strategy in Brazil. We rely on the unique policy experiment of fully randomized public fiscal audits to identify the causal effects of the audits on deforestation and to assess governance quality at the municipality level. Deforestation increased on average by at least 11% in the aftermath of public fiscal audits, with larger increases in more corrupt municipalities. Municipalities seem also to have learned from neighboring audits, which affected deforestation outcomes in a similar way to their own audits. Deforestation increased especially under mayors who were facing reelection constraints and received unfavourable audit reports. All these findings can be reconciled with a shift in illicit/corrupt activities towards spheres less easily observable by federal auditors.

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[†]ZEF (Center for Development Research), Bonn

[‡]IPEA (Institute for Applied Economic Research), Brazil

[§]University of Freiburg and IZA, Bonn. Address: Institute for Economic Research, University of Freiburg, Platz der Alten Synagoge 1, 79085 Freiburg, Germany. Contact: krisztina.kis-katos@vwl.uni-freiburg.de.

1 Introduction

Strict monitoring by the central government and providing corruption information to the public offer promising ways to address corruption within the lower tiers of the government administration (Svensson 2005, Olken and Pande 2011). The recent government initiative in Brazil has proven successful in this context: public information from local fiscal audits has significantly reduced reelection chances of corrupt local politicians (Ferraz and Finan 2008), and electoral accountability in turn has significantly reduced local corruption levels (Ferraz and Finan 2011).

However, if local agents perform multiple tasks, increased incentives to perform well in a certain sphere can lead to a deterioration of performance in another (Holmstrom and Milgrom 1991). More specifically, increased attention to fiscal discipline might shift local corruption to other, less directly observed activities. This can mainly happen through two channels. Local governments focusing on administrative reforms might simply lack the capacity to monitor illegal land use. But the increasing need to observe fiscal discipline might also lead local administrators to refocus their personal and political interests on spheres less easily observable by federal fiscal auditors. In order to preserve their political power, they might start to cater more strongly to the interests of local landowners and sawmill operators, for instance by tolerating illegal land grabs, enabling thus the conversion of forested land to cattle pasture or soybean plantations (Fearnside 2001). Thus, anti-corruption policies focusing on fiscal discipline could have the undesired side effect of increasing deforestation in the municipalities undergoing public fiscal audits. The question whether this mechanism has played an important role in explaining deforestation of the Brazilian Amazon over the last decade lies at the heart of this study.

Preservation of the existing rainforests is one of the major global environmental priorities. The Brazilian Amazon contains 40% of the world's remaining tropical forests and plays a crucial role in biodiversity preservation as well as for the global climate system (Kirby et al. 2006). While it is widely documented that deforestation in Brazil is strongly affected by economic incentives (Angelsen 1999, Pfaff 1999, Hargrave and Kis-Katos 2012) as well as conflicting regulatory frameworks (Alston, Libecap and Mueller 2000, Fearnside 2001, Ludewigs, de Oliveira D'Antona, Brondízio and Hetrick 2009), the effects of local governance on the deforestation process remain widely unexplored.[1]

[1]The study of Burgess, Hansen, Olken, Potapov and Sieber (2011) on the effects of decen-

We use the unique policy experiment of the Brazilian local fiscal audits to investigate the effects of this anti-corruption strategy on deforestation. Starting with 2003, the Brazilian government implemented a lottery system that resulted in strict fiscal audits in randomly selected municipalities. The results of these audits were subsequently published on the internet and made widely available to the public. This newly revealed information on local governance quality has significantly shaped the local political environment and affected political outcomes in subsequent mayoral elections (Ferraz and Finan 2008).[2] We use this publicly available information to construct proxies for the overall governance quality and corruption at the municipality level. We combine this information with yearly satellite data on the deforestation process from the Brazilian PRODES project in order to investigate the relationship between audits, local governance and deforestation dynamics.

We address the relationship between deforestation and local corruption in three steps. First, we assess the correlation between local governance quality and average deforestation levels, after having controlled for other fundamental determinants of deforestation. We do so by relating total deforestation levels over the time period of 2002 to 2009 in 209 audited municipalities to the extent of corruption documented by the auditors. This descriptive analysis shows that municipalities with about one standard deviation higher measured corruption levels experienced up to 20% higher deforestation between 2002 and 2009, although a large part of this effect vanishes once other initial socio-economic determinants are controlled for.

In a second step, we exploit the fully randomized allocation of public audits in order to investigate deforestation outcomes after the fiscal audits have taken place and the reports have been published. Our results show that public fiscal audits have lead to an average increase in deforestation by about 11 to 17%, which is significant both in economic and statistical terms. Investigating the time dynamics shows us that most of this increase in deforestation rates was realized in the first years right after the audit, while it took six years after the audits for the first significant decreases in deforestation to happen. There is also evidence

tralization on illegal logging in Indonesia is a major exception, relating deforestation to the proliferation of newly formed local governments, fighting for forest resources.

[2]The same natural experiment of public fiscal audits in Brazil has also been exploited to address the effects of corruption on schooling outcomes and teacher quality (Ferraz, Finan and Moreira 2012), the role of judicial presence on the overall regulatory quality (Litschig and Zamboni 2010), the effects of exogenous shifts in budget size on local corruption (Brollo, Nannicini, Perotti and Tabellini 2010), or the effects of later changes in the audit risk on corruption and waste (Litschig and Zamboni 2011).

for spatial spillovers as audits of neighboring municipalities led also to increases in deforestation. Due to the fully randomized study design, these results can be interpreted in strictly causal terms.

In the last part of the study we discuss various mechanisms that could explain these substantial increases in deforestation. The external auditors scrutinize the use of federal funds in all sectors, and focus on irregularities in public procurement as well as unrealized investments (reflecting potentially outright theft of federal funds), but the operation of local land markets, the presence of illegal settlements, or deforestation outcomes are not among the issues investigated by the auditors. The increased public scrutiny can lead to a substitution of attention from or corruption and political support seeking activities towards sectors that are less directly observable within this public audit system, leading among others to increased deforestation.

We document that deforestation increased after the audits especially in municipalities with worse governance findings and this increase cannot be explained by differences in pre-trends of deforestation in municipalities with worse governance environment. One likely explanation for this effect is that local administrations are imperfectly informed about the scope and severity of the public audit program, and once audited, update their beliefs about what types of behaviors are scrutinized by the federal auditors. The realization that deforestation related issues are not subject to federal audits can thus lead to an increase in deforestation. This updating of beliefs is also in line with the evidence on neighborhood audits, which tend to increase deforestation as well, especially if the auditors have detected many irregularities in neighboring municipalities. This neighborhood effect cannot be entirely explained by spatial spillovers from neighboring deforestation, hence learning also plays and important role.

In a further step, we investigate the role of the disciplining effects of electoral accountability. We find that electoral considerations play a considerable role in explaining the increase in deforestation. Mayors, who serve their first term and hence can stand for reelection, react to bad audits differently from second term mayors, resulting in a sizeable increase in deforestation. This result once again is in line with a shift in corruption towards unobserved spheres and qualifies the positive findings of Ferraz and Finan (2011) who document that less corrupt violations happen under first term mayors. In contrast, we do not find a statistically significant relationship between increases in deforestation and the presence of judicial seats or local radio stations in the municipality.

All of our results can be reconciled with members of the local executive shifting their rent-seeking, corruption and shirking activities towards less directly monitored spheres, yielding in this case an increase in illegal deforestation. An observationally equivalent explanation involves local administrators trying hard to improve their governance record, especially when facing reelection incentives, and neglecting other governance spheres. Whichever of these two explanations is right, the empirical results offer first-hand evidence on unintended spill-over effects of anti-corruption strategies can have to non-audited spheres.

Our paper is closely related to several strands of empirical literature. Similarly to Burgess et al. (2011), who link deforestation dynamics in Indonesia to the proliferation of new districts and the local election cycle, we also address tropical deforestation from a political economic perspective and offer further support for local elections affecting deforestation. Our study differs from their analysis considerably by assessing the effects of one specific anti-corruption intervention and linking deforestation dynamics with direct measures of governance quality. Our study also contributes to the literature on the effects of the Brazilian public fiscal audits on political and governance outcomes, like mayoral reelection chances (Ferraz and Finan 2008), and corruption and local public service delivery (Litschig and Zamboni 2011). In contrast to these studies, we address an outcome, deforestation, which is not directly monitored by the public audits, and hence document a shift in rent-extraction towards not audited spheres. Since we measure our main outcome of interest on a yearly basis, we are also able to describe the time dynamics of the audit effects at a much finer scale than before-after analyses do. Our findings also resonate with some of the results of Olken (2007), which indicate a potential shift in corruption (towards nepotism) in the face of fiscal audits in a road building program in Indonesia.

In what follows, we explain the policy experiment of public audits in more detail, describe how our corruption proxies were generated and present first hypotheses on the effects of corruption and public audits on deforestation. Section 3 describes our data and the descriptive evidence on the correlation between corruption and deforestation. Section 4 investigates the causal effects of public fiscal audits on deforestation, and addresses the potential mechanisms driving these results. Section 5 concludes.

2 Public audits and corruption findings

2.1 Public fiscal audits in Brazil

In 2003, as part of its new anti-corruption strategy, the Federal Government of Brazil introduced the Random Audit Program (Programa de Fiscalização a partir de Sorteios Públicos) to control the local use of federal funds and the realization of federal programs. The audited municipalities are selected by public lottery, and are subsequently visited by auditors from the Office of the Comptroller General (CGU), which is the federal agency for internal control, public audits, and corruption prevention. The CGU officers make a detailed assessment of the expenses and procedures of the selected municipalities and write an extensive audit report, which is then published online on the CGU's homepage.[3] Thus, revealed instances of mismanagement of public funds are fully disclosed to the public.

The public lotteries started in April 2003 and have been carried out since about 3 to 7 times a year by the Federal Savings Bank, at the same time with the regular national money lottery. Overall, 30 lotteries were carried out in the years between 2003 and 2009, resulting in 1636 audit reports. The first 8 lotteries selected 50 municipalities out of all Brazilian communities with less than 300,000 inhabitants. Their scope was extended by the ninth lottery to municipalities with up to 500,000 inhabitants; starting with the tenth lottery 60 municipalities with less than 500,000 inhabitants have been selected.[4]

After a lottery has been carried out, the Office of the Comptroller General sends 10 to 15 auditors to the municipalities (Ferraz and Finan 2008). The auditors control all local accounts and documents to check the usage and right implementation of federal funds. They conduct a Public Expenditure Tracking Survey by comparing the governmental funds sent to the municipalities and the funds that have reached the entitled entities (health centers, schools, etc.). Simultaneously they check the presence and condition of services and constructions, estimate the quantities and value of public goods that have not reached their intended users, and compare billing prices to market prices. Moreover, they interview a random sample of community households in order to reveal instances of nepotism or fraud

[3]The reports are available under `http://www.cgu.gov.br/english/default.asp`.

[4]Procedures of the first lotteries varied somewhat. The first two lotteries were smaller and had a preliminary character, including only 5 and 26 municipalities respectively. The second and third lottery involved only municipalities of less than 250,000 inhabitants. Lotteries 9, 11 and 13 excluded municipalities under 10,000 inhabitants. However, these minor differences do not affect strongly the random character of the lotteries.

(Ferraz and Finan 2008). The CGU hands over the audit reports to the Tribunal of accounts (TCU), to public prosecutors, the municipal legislative branch and to the media (Ferraz and Finan 2008).

Generally, reports start with information on the total federal fund use, and include a listing of all federal programs by their originating ministry as well as a description of the general objectives of each program, and a detailed assessment of its implementation. Each problem listed by the auditors is related to non-compliance with a specific governmental law or directive and is outlined in detail in the report. The explicit finding is called an *irregularity*, and it is accompanied by a description of the facts found and the evidence used, potentially followed by a statement of the mayor and closed by a final analysis of the audit team. The irregularities describe various incidences, from non-competitive public procurement processes, improperly implemented programs, and dysfunctional local administrative processes, to illicit expenditures, excessive spending and overpricing of items, lack of documentation, expenditures to family enterprises and other forms of nepotism, the use of federal funds for private gains or outright disappearance of funds. While some of the listed irregularities thus refer to management failures and imply passive waste (Bandiera, Prat and Valletti 2009), others are more clearly identifiable as corruption.

By constructing our measures of local governance quality, we follow largely the approach used in the existing literature (e.q., by Ferraz and Finan 2008, 2011, and Litschig and Zamboni 2010, 2011), which basically counts corruption related (or total) irregularities in the reports in some form of other.[5] This approach has been criticized by Olken and Pande (2011) as prone to measurement error, since it might be hard for auditors to discover the actual levels of corruption in any municipality. A further issue of concern is whether corrupt local governments can tamper with the audit reports either by misrepresenting information or by bribing the auditors. However, given the implementation of the central auditing procedure, these concerns do not seem to be warranted. Federal auditors are well-trained and earn highly competitive wages (Ferraz and Finan 2008), they are thus likely to be able and willing to detect and report obvious forms of corruption. They are also less prone to collusion with local governments as they come unexpectedly and stay at the municipality only once. They usually work in relatively large groups (about

[5]Quantifying the share of federal resources affected by corruption (as in Ferraz and Finan 2011) is less viable in our case because of structural changes in whether and how affected funds are reported over the relatively long time period that we use in our study.

10 auditors or even more), so that the whole group would have to be bribed in order to make them conceal unfavourable findings, which is very unlikely (Litschig and Zamboni 2011). The auditors implement a detailed and fairly constant procedure, controlling the use of all federal funds in a municipality. They compare the fiscal accounts to actual realizations of the investments, documenting precisely (and often with photographs) the completion and usage of various federally funded facilities and the presence and use of specific investment goods. They also estimate the actual value of the realized investments, and assess whether disbursements were made at market prices. Moreover, a wide range of procedural irregularities gets recorded as well as any further public complaints. Although this procedure will not be free of measurement error, the arising measurement error is unlikely to be systematic. We believe that these very detailed public audit reports give a good first assessment of the overall quality of governance in any municipality.

The random selection of municipalities via lotteries results in a random subsample of all municipalities; in our specific case this covers 212 not yet completely deforested municipalities in the Brazilian Amazon, out of the total of 605 forested municipalities. Eligibility criteria of the public audits (having a population below 500,000) further reduces the sample to 602 municipalities. Since audits can be considered as completely random within states and across time, this enables us to use the information on the corruption findings collected by the auditors and address the effects of the random audits on subsequent deforestation outcomes.

2.2 Constructing measures for local corruption

In an average municipality in our Amazonas sample, public fiscal audits control the use of about 9.6 million R$, which is disbursed via ca. 9 different ministries (cf. table 1). These are very substantial amounts; overall, federal funding accounts for about 70% of municipality finances (SOURCE). Hence, records of the use of these funds can be expected to reflect very well the governance quality of a municipality.

We design two main measures of local governance quality based on the audit reports. Both measures exploit the fact that audit reports broadly retained their structure over time and auditors follow the same general reporting style, use similar phrases, and when identifying breaches of law always refer to the specific laws and directives.

The first one, *irregularities*, is a rather crude measure which results from adding up the numbers of reported irregularities in any municipality and can be seen as

a very broad measure of administrative quality. The sum of all irregularities of course does not exclusively reflect corruption and fraud but also includes measures of waste, inefficiencies and administrative failures. The higher the number of such irregularities, the more public resources will be wasted, either by loss or capture of rents. Thus, this measure is useful for assessing the overall quality of local government administration. Since local governments differ strongly in their fiscal size and capacity (the number of federal programs ranging from 4 to 58, cf. table 1), we normalize this measure by dividing it through the total number of programs investigated by the auditors. As table 1 shows, an average audit report records about 58 administrative irregularities, which results in 2.9 irregularities per investigated program. In order to ease interpretation, the subsequent empirical analysis uses a standardized version of this relative irregularity measure, with mean zero and a standard deviation of one over all reports.[6]

By contrast, our second and main measure, *corruption*, is based on a text mining procedure that counts the number of corruption related expressions within any report. It thus more specifically reflects the overall extent of corrupt violations in a municipality. It is generated by using the Extended Global Regular Expressions Print (Egrep) to search for 40 different *regular expressions* within an audit report that inevitably indicate corrupt incidences.[7] We classified the 40 regular expressions used to identify corruption under the categories of diversion of public funds, over-invoicing, irregular procurement, advanced payment, fraud, incomplete construction and non-existence of documentation. All regular expressions as well as the detailed procedures used are presented in the Appendix (Table A.1).

Diversion of public funds counts expressions describing instances when funds are used for other purposes, if they have disappeared or if expenditures are done without any proof of provision or purchase. *Irregular procurement* refers to expressions indicating a procurement process without a call for bids or no minimum number of bids. *Over-invoicing* is identified whenever payments use higher than market prices. *Advanced payments* are transfers to a provider or construction

[6]Alternatively to the relative number of irregularities, we also coded the share of programs investigated that have at least one irregularity, which have been favoured by Litschig and Zamboni (2011) and Ferraz and Finan (2011). This second irregularities variable is somewhat more prone to measurement and encoding error because of structural changes in the presentation of the reports over the eight years, but overall, it yields qualitatively similar results to our normalized irregularities measure and hence we do report it separately.

[7]Egrep is a text search command of the Unix operating system that searches for matches of a string within a text. A regular expression specifies a set of small strings or characters which can be interconnected by arithmetic functions (see also `http://manpages.ubuntu.com/`).

company before goods are delivered or constructions have been completed, and are nearly always accompanied by abandoned or sloppy construction sites. *Fraud* includes illicit expenditures to staff or family members, non-existence of invoices, contraction of inexistent firms, inclusion of illicit regulations in the bidding process and exaggerated expenses for oil and gasoline. *Incomplete constructions* arise if the examined buildings do not meet the funds invested or buildings do not exist. *Non-existing documentation* inhibits auditors to analyze what the funds were used for or to find any evidence of fraud.

An irregularity in an audit report could be identified as corrupt with multiple matching expressions in the same paragraph. This could lead to an over-counting of expressions and an upward bias in the corruption measurement. We do not have any reason to suspect these stylistic differences however to be systematic, and we are convinced that the writing style of the auditors is fairly comparable within specific years. After analyzing in detail the audit reports, we come to the conclusion that reporting style has changed somewhat in the course of the time but is structurally highly comparable for reports written within the same year.[8]

Table 2 documents the changes in the number of irregularities and the text based corruption measure over time, which show an increasing trend in the intensity of corruption findings. This is unlikely to reflect an overall worsening of the corruption environment; if anything, existing literature on the effects of public audits argues that electoral accountability tended to reduce corruption over time (Ferraz and Finan 2008). Rather, it shows a gradual tightening of the auditing procedures: Whereas the number of investigated federal programs even decreased over the time period of our analysis, both the number of total irregularities and that of corruption related expressions increased considerably, with major structural breaks occurring in 2004 and 2009. Our subsequent panel data analysis takes these changes into account by including state specific time fixed effects in the main specifications, and hence identifying the audit effects based on within state variation in any given year only. A second way to deal with the time variation in reporting styles, which we will also pursue, is to re-normalize the corruption measures on a yearly basis, and thus treat the worse corruption findings within any year's distribution as similar to each other across the years.

For municipalities that were audited more than once (21 out of 212), we take

[8]Constancy in the writing style is further supported as highly competitive wages earned by the auditors (Ferraz and Finan 2008) provide an incentive to stay on the job and write many audit reports.

the average of the corruption measures in the cross sectional analysis, and base the corruption measure on the most recent audit report in the panel data models.

2.3 General hypotheses

2.3.1 Potential links between local governance and deforestation

The legal framework of forest conservation in Brazil is established mainly at the federal and state level. Currently, considerable parts of the Amazon rainforest are either under federal or state protection, which takes the form of integral protection (National Parks, Ecological or Biological Reserves) and sustainable use (National Forests and Extractive Reserves), or under indigenous management (Fearnside 2001). In the remaining areas, private landowners are required to maintain 80% of their land under forest cover. The federal environmental agency (IBAMA, the Brazilian Institute of Environment and Renewable Natural Resources) is responsible for the enforcement of environmental laws, and has increased its law enforcement activities considerably in the course of the last decade. From 2002 to 2009, the size of environmental fines administered per deforested area increased by about 18-fold, which has contributed to the sizeable decrease in deforestation rates during this period (Hargrave and Kis-Katos 2012). However, a surprisingly small fraction of the environmental fines is actually paid.

The economic interests of local ruling elites are often aligned towards unsustainable uses of forest, both through capturing economic rents from logging and through increasing the land available for cattle ranching or soybean plantations. Since farming and logging are typically of central importance for local economies, economic groups linked to these activities tend to have important political power at the local level. Local politicians and mayors are themselves often either loggers (owners of sawmills) or cattle ranchers. But even where politicians themselves are not directly involved in these activities, they can benefit from close ties to large farmers and sawmill owners, who play an important role in financing mayoral election campaigns.

Although forest management and the enforcement of environmental laws are both centralized, and hence not under direct local control, the governance quality in a municipality can affect deforestation dynamics in many ways. A corrupt administration can contribute directly to unsustainable land use by tolerating the illicit selling of untitled land and supporting large landlords in their violent expulsion of small farmers (Fearnside 2001, Ludewigs et al. 2009). Large farms,

supported by a corrupt administration, have it easier to convert the land to pasture and start ranching cattle (Pacheco 2009). A corrupt administration can also collude with local sawmills fostering illegal logging. Legal log selling is allowed to an amount of $15m^3$ per hectare per year, but falsified documents for further wood can be easily obtained and presented to the sawmills (Fearnside 2001). Corrupt administrations are prone to rent extraction in this industry and contribute to deforestation. The presence of illegal settlements within the forest is another driver of deforestation. Once again, local administrations might decide to be more accommodating towards illegal settlers. Indebted landholders have also the incentive to invite squatters to invade their unused forested land and to incorporate it into a settlement project, claiming compensation from the Ministry of Agriculture afterwards (Fearnside 2001). Moreover, local governments can decide whether to support or even try to inhibit federal raids aiming at the enforcement of environmental laws. Depending on whether local officials cooperate with federal agencies, law enforcement will be more or less credible to agents and therefore will reach varying levels of effectiveness. Corruption on the municipality level can also interact with the rural subsidized credit program, PRONAF (National Program for Family Agriculture), which is designed to help small farmers and settlers to implement sustainable agriculture. The selection of program beneficiaries ought to be controlled by the local government. However, monitoring incentives are low, while the incentives to defect on the subsidized loans are great, since the PRONAF credit is tied to the lot rather than to the owner. This leads to lot abandonment, re-concentration and deforestation of land (Fearnside 2001).

From a longer-term perspective, the failure to implement and maintain a functioning public infrastructure is also a major channel through which a corrupt municipality can foster deforestation (Ludewigs et al. 2009). Corrupt administrations might fail to produce crucial infrastructural services by capturing government funds as well as rents from the logging industry. The resulting poor physical as well as health and education infrastructure hinders considerably the economic and social viability of small settlements. As a result of poor public services, small-scale farmers tend to abandon their properties or sell them to neighboring farms, which once again leads to concentration of land and an acceleration of deforestation. The process of forming protected forest areas is another channel through which a corrupt administration can contribute to higher deforestation rates. The central government program "Protected Areas of Brazil" of the Ministry of Environment orders local governments to contribute to the planning, implementation

and management of protected areas (Fearnside 2003). A corrupt administration might yield to lobbying from landholders, settlement projects, or squatter associations, and show no interest in forest conservation. Corrupt local governments can also lobby the state legislatures against the enlargement of protected areas. Some states welcome the involvement of local governments in the management of protected areas, and also leave them some discretionary power in their establishment (Fearnside 2001).

2.3.2 Potential effects of public audits on deforestation

The increased public scrutiny resulting from centralized and published fiscal audits can be expected to lead to improvements in administrative procedures and governance capacity. After audit information on the levels of corruption and irregularities in a municipality gets publicly revealed, a corrupt administration could be pressured both from its own citizens and the central government or state judiciaries to improve its governance record. Electoral accountability and legal prosecution are two powerful institutions that can lead to subsequent improvement in the observed local governance quality.

Electoral accountability induces improvements in local governance if there is a fair chance that local constituencies will vote for a different mayor in the subsequent elections. Ferraz and Finan (2008) document that the findings of the federal auditors were used by political adversaries before the municipal elections. They show that the publication of the audit reports has had a negative impact on the performance of the incumbent in the subsequent elections, once he has been revealed as highly corrupt. Ferraz and Finan (2011) find lower corruption records in municipalities that had first term mayors, since first term mayors face reelection incentives and hence are more interested to keep the quality of public service delivery higher.

The fear of legal prosecution can also induce improvements in local governance. Audit findings that discover large-scale corruption cases are more likely to be followed up by the state judiciary, and can even lead to large fines and incarceration of the public officials. The majority of the findings however concerns management irregularities or less clear-cut cases of potentially corrupt activities where state legislatures have ample discretionary power to decide what cases should be followed up in more detail. Litschig and Zamboni (2010) show that the physical presence of the judiciary, in form of the seat of the judiciary district being within

the municipality, makes public prosecution of cases more likely and leads to less corruption findings.

Improvements in governance quality can at the same time interact with the ongoing deforestation dynamics. The increased public awareness and the increased control by the central government and judiciary could affect the behavior of local administration in two different ways. The increased pressure might improve overall government performance and reduce corruption in subsequent time periods, improve among others the management of local agriculture and lead to a better management of the forest resources in the municipality. However, unintended consequences are also equally if not even more likely. Increased monitoring efforts in a specific sphere of local public finance can decrease monitoring capacities in other tasks (Holmstrom and Milgrom 1991). Local governments focusing on administrative reform might simply lack the capacity to address governance issues affecting deforestation. At the same time they might be tempted to shift corrupt activities from one area to another area with less public scrutiny. The municipalities could shift their capture of funds for example from educational grants or health-care subsidies to more collusion with players of the agricultural sector, or towards collecting rents from illegal sawmill operations. This latter mechanism would also be in line with the findings of Burgess et al. (2011), who find a shift towards more deforestation in Indonesia before elections, especially in areas with less possibilities to extract rents from other natural resources.

The presence of audit effects on deforestation presupposes that local administrations were imperfectly informed about the audits or had imperfect foresight and failed to adjust their behavior perfectly when the new auditing program was announced. Were precise information about all the modalities of the audit programs common knowledge, and were local officials perfectly foresighted about the chances of any specific activity being detected by the auditors, they should have adjusted their behavior even before they were randomly selected to be audited. Under perfect information and foresight, we should see deforestation only be affected by the introduction of the program in 2003 in both audited and non-audited municipalities alike, and no further audit treatment effects should be expected. If however audits induce local administrations to update their beliefs about what types of activities do federal auditors scrutinize, they will adjust their behavior once learning about the audit modalities. This learning can take place when a municipality is audited but potentially also when neighboring municipalities undergo an audit process.

3 Data and overall correlations

3.1 Data

Our main sample includes all municipalities in the Brazilian Legal Amazon that had more than zero forest cover in 2002 and were observed by the satellite-based monitoring system of the National Institute for Space Research (INPE). Since 1988, INPE monitors deforestation in the region annually using image interpretation of the Landsat satellite, within the so called PRODES project (INPE 2011). From 1988 until 2002 deforestation rates were calculated based on visual interpretation of satellite imagery. Starting with 2003, imagery interpretation has become partially automatic. PRODES measures annual rates of forest clearing for increments greater than 6.25 hectares by using about 220 scenes of Landsat and CBERS satellites with a resolution of 30m. PRODES final data is released on the internet for public use. Annual rates are computed from August of one year through July of the following year, since these are the months with the least cloud cover in the region. For our empirical analysis, we aggregate the measured deforestation patches to a municipality scale and record the size of the yearly newly deforested municipality area in square kilometers.[9] After excluding three further municipalities (with population above 500,000) that were exempt from the public audit program entirely, we end up with a balanced panel of 602 municipalities over eight years (from 2002 to 2009).[10]

Figure 1 visualizes the spatial distribution of yearly deforestation rates (normalized by forest size in 2002). The maps demonstrate large differences in the deforestation pressure between the peripheral forest (the so-called "deforestation arc") and the less affected central areas. At the same time they show clearly an overall reduction in deforestation in the second part of the decade, which has been attributed to falling product prices, and the increasing effectiveness of the environmental police (IBAMA).

We combine this information on deforestation with information on the timing of the randomized audits in 212 sample municipalities, as well as with local governance information derived from the audit reports, which constitute our main ex-

[9]We measure time according to the deforestation years, and hence also adjust all control variables to the same August-July time window.

[10]The three excluded municipalities are Manaus, São Luís and Cuiabá. Our observations come from nine Brazilian states: Acre, Amapá, Amazonas, Pará, Roraima, Rondônia, Tocantins, Mato Grosso and Maranhão.

planatory variables of interest. Further information on local elections, the mayor's term limits, the presence of local radio stations and radio penetration come from IPEA. In order to contain measurement error, we use yearly information on cloud coverage (which affects the observability of deforestation) and the size of municipality area not observed by the PRODES project as controls, both of which come from INPE.

For the descriptive analysis of the long-term relationship between local governance and deforestation, we take total deforestation between the years 2002 to 2009 as dependent variable, and add further initial and geographic conditions as controls. Data on initial forest size and savanna coverage come from the PRODES project of INPE. Average rainfall data is obtained from the National Aeronautics and Space Administration (NASA) through its Goddard Earth Sciences Data and Information Services Center (GES-DISC). The population and GDP data is acquired from the Brazilian Institute of Geography and Statistics (IBGE), the latter is measured in real per capita terms, deflated by the national CPI. The distance to Brasilia comes from IPEA. The data on settlement projects are from the Brazilian Agency of Agrarian Reform (INCRA) and the Institute of Man and Environment in the Amazon (IMAZON). The data on protected areas comes from the Department of Protected Areas of the Brazilian Environmental Ministry (DAP/MMA). Summary statistics of the cross-sectional and panel data are presented in table 3 and 4.

3.2 Local governance and deforestation

A first way to assess the relationship between average local governance quality and deforestation is to regress total deforestation in a municipality over the time period between 2002 and 2009 on the average corruption findings from the audit reports. This approach assumes that the corruption measures based on punctual audits reflect well the underlying more or less constant corruption environment. Our corruption measures are based on audit reports published within this time frame, constructed as described in section 2. We use both the number of administrative irregularities listed in a report, normalized by the total number of investigated programs, and our text based corruption intensity measure. While the first one depicts general administrative quality in the municipality, it is much broader in scope than our direct corruption measure. We believe, our second measure captures closer the extent of clearly corrupt violations and hence the cor-

ruption environment in the municipality. To ease interpretation, we standardize both governance measures to have zero mean and a standard deviation of one. In order to correct for structural shifts in our governance measures over time, we also test governance variables that have been standardized yearly and hence reflect the variation in governance measures within any given audit year.

We estimate linear regression models for a cross section of 209 sample municipalities, explaining deforestation over the whole time period by a list of time invariant factors and initial conditions.[11] We include our two corruption proxies as main variables of interest in regressions of the following form:

$$\ln D_i = \mathbf{X}_{i0}\,\boldsymbol{\beta} + \gamma\, C_i + u_i. \tag{1}$$

The dependent variable $\ln D_i$ is the natural logarithm of the total cumulative deforestation over the years 2002 to 2009 in municipality i. We proxy for governance failures C_i by both the relative number of irregularities and our text-based corruption measure. The vector of initial conditions $\mathbf{X}_{i0}$ includes three sets of variables: baseline controls for scale and measurement errors, and two further sets of geo-climatic factors and initial socio-economic conditions.

Table 5 shows the results, where the first three specifications use our preferred text-based corruption measure, and the second three columns the relative number of irregularities as the main explanatory variable. Panel A presents the estimated coefficients of the standardized governance variables and all other controls, while panel B shows only the coefficients on the yearly standardized governance variables from otherwise identical regressions. Specifications (1) and (4) control only for scale factors (the natural logarithm of initial forest size), which capture differences in the size of land that can be potentially deforested, and measurement error (the log of the average yearly area covered by clouds and hence not observable by the satellite project). Columns (2) and (5) add further controls for average yearly rainfall, savanna coverage (in %), the log distance to Brasilia, the log of initial population, and the log of initial pc. GDP in agriculture. Some of these variables are truly exogenous capturing geographic (savanna coverage, distance to Brasilia) or climatic factors (average yearly rainfall). The other initial conditions are predetermined (initial forest size, population and pc. GDP in agriculture) as they cannot be influenced by future deforestation rates. Savanna share captures

[11] We exclude three municipalities from the original sample of 212 audited originally forested municipalities due to incomplete data on some of the socio-economic control variables.

the size of municipality area that was never rainforest and is hence not observed by the satellite project. The distance to Brasilia serves as an overall proxy for remoteness, also capturing the distance to the major markets. Socio-economic initial conditions control for deforestation pressure. Population pressure is accounted for by initial population size, which can both increase the demand for agricultural land (Fearnside 2001), and lead to a fragmentation of agricultural lots, potentially inducing deforestation (Ludewigs et al. 2009). Initial per capita GDP in agriculture controls for the scale of agricultural production in the municipality, which is also related to the demand for land and can induce lot consolidation (Ludewigs et al. 2009). Models (3) and (6) additionally include initial policy conditions (the size of protected and officially designated settlement areas) that are potentially interdependent with the corruption environment. We expect these variables to be more closely related to local governance and also constitute channels through which local corruption might affect the overall deforestation dynamics. The initial size of settlement projects can be expected to increase deforestation pressure both through land clearing and by inducing further migration (Fearnside 2001). By contrast, protected areas (under federal or state protection or indigenous use), if effective, should inhibit deforestation.

In all of these specifications, our governance measures are positively related to deforestation, and in several they are statistically significant or near to the 10% significance threshold. The strength of the relationship decreases as further initial conditions are added to the regression. While our corruption measures stay more or less the same irrespectively of whether normalized by year or over the whole time period, yearly normalization increases the size and significance of the irregularities coefficients. This suggests that structural breaks because of changing reporting style might be more of an issue in case of the plain irregularities count measure.[12]

All other controls turn out significant in explaining deforestation and exhibit the expected signs. The positive signs of initial forest size, population and agricultural GDP per capita indicate the presence of scale effects. As expected, less deforestation is measured if the average cloud coverage is higher, but also in areas covered by savanna, which are excluded from the satellite observations. The coefficient of the distance to Brasilia, proxying for economic remoteness, shows a large difference between inner areas and the outer deforestation arc. Rainfall tends

[12]Semi-parametric specifications including a number of quantiles of the governance measures show that especially the correlation between deforestation and corruption is driven by significantly larger deforestation in municipalities with very high corruption levels.

to inhibit deforestation by rendering logging as well as agricultural activities less profitable. We also also find a non-negligible correlation between the initial size of protected areas and settlement projects and subsequent deforestation, where the former tends to reduce while the latter to foster deforestation.

The above models depict a weakly significant or nearly significant correlation between deforestation and local governance within the local governments, even after controlling for major determinants of deforestation. However, these regressions cannot be considered causal. For instance, the measured coefficient could be affected by reverse causality if the demand for favors from the administration increases with increasing forest clearing, in which case estimated coefficients capture the total strength of the interrelationship. A potentially more serious concern in terms of the endogeneity of the local governance variables is the possibility of omitted variable bias. The economic structure of a municipality should be a main driver of deforestation, but it is difficult to control for characteristics of a municipal economy like the composition or the distribution of wealth and power. We control to some extent for the level of economic activity in a municipality with initial per capita GDP in agriculture. Infrastructure or road density and road quality could also be important omitted variables, although the sign of the bias is unclear. Roads are often identified as a major determinant of illegal occupation of land and deforestation (Pacheco 2009, Angelsen 1999). However, lack of proper infrastructure can also lead to abandonment of land and re-concentration of plots (Ludewigs et al. 2009).

Given these limitations, regressions of total deforestation on governance measures do not yield themselves to a strictly causal interpretation. Nonetheless, they show some evidence for an average relationship between deforestation and the auditors' corruption findings in a municipality.

4 Public audit effects on deforestation

4.1 Audit effects

The publication of the random audit reports allows us to study the effects of publicly revealed central audits on deforestation. As explained before, increased scrutiny of the fiscal governance procedures by auditors can induce a shift of corrupt activities to other, less observed spheres. Land use decisions, which are not directly observed by the auditors but can directly affect deforestation, offer lo-

cal administrations ample opportunities for generating illegal revenues or political support.

We investigate both the average effect of the audits on deforestation in the following years and the adjustment of deforestation dynamics following the report. We assume that the treatment effect of the audits starts as soon as the audits have taken place.[13] The underlying panel data model can be written in the following form:

$$\ln D_{it} = \gamma_1 A_{it} + \mathbf{X}'_{it}\boldsymbol{\beta} + \kappa_{st} + \alpha_i + \epsilon_{it} \tag{2}$$

where the dependent variable $\ln D_{it}$ stands for the natural logarithm of the newly deforested area in municipality i in year t, and A_{it} denotes the audit treatment. The vector $\mathbf{X}_{it}$ includes two variables proxying for measurement error: the log of yearly area covered by clouds and the log of yearly area not directly observed by the satellites. Both cloud coverage and the size of area excluded from the observation affect the yearly precision of the observations on deforestation. Although the implied measurement errors are not very substantial—on average 2.6% of municipality area is unobservable yearly because of clouds and other 1.3% are not covered by the project—the inclusion of these two variables can be expected to increase the precision of the estimates.

In these specifications we do not include any further policy variables or economic controls as they are most likely jointly determined with deforestation. State-time fixed effects, denoted by κ_{st}, control for average changes in environmental and other economic policies (notably, changes in rural credit policies and the increasing stringency of the environmental police), as well as macroeconomic shocks and average fluctuations in agricultural product prices, all of which can affect deforestation decisions. In our preferred specifications, we allow the time effects to be state-specific and hence identify the audit effects based on variations in deforestation of municipalities within the same state. All time invariant locally idiosyncratic factors affecting deforestation are captured by the municipality fixed effects α_i.

The centralized fiscal audit and the subsequent publishing of the audit report in a given municipality constitutes our treatment indicator A_{it}. In case of repeated audits (20 municipalities in our sample have been audited twice and one municipality even three times), the treatment variable measures the number of audits that have been carried out until the given year. We specify this treatment effect in

[13] To match the August-July deforestation window, we consider audits that have taken place until July 15 of the given year to potentially affect deforestation of that year.

various forms, capturing either the average effect for all the years after the audits or yearly treatment effects. Were the governance environment to improve in all respects after increased scrutiny, this would be reflected in negative $\hat{\gamma}_1$ coefficients. However, if increased public attention on the management of local municipality finances leads to a diversion of corrupt activities toward less observed sectors, deforestation will increase, yielding a positive $\hat{\gamma}_1$.

We estimate equation (2) in a first difference form, eliminating the municipality fixed effects α_i through a difference specification.[14] The average effect of the audit treatment is captured by estimating the following model:

$$\Delta \ln D_{it} = \gamma_1 \, \Delta A_{it} + \Delta \mathbf{X}'_{it} \, \boldsymbol{\beta} + \kappa_{st} + \upsilon_{it}, \tag{3}$$

where γ_1 measures the average audit effect on deforestation in all years following the audit. In all models, we cluster standard errors at municipality level, allowing thus for any form of autocorrelation within a municipality.

The first difference estimates of the average public fiscal audit effects are presented in table 6. Column (1) shows the results from regressing first differences of deforestation on the treatment indicator only. These simple treatment effects are sizeable: after an audit takes place, deforestation in a municipality increases on average by about 17%. The two measurement error proxies (clouds and not observed areas), included as further controls starting with column (2), turn out as highly significant (with an F-statistic of 40). Their inclusion tends to slightly increase the point estimates as well as their precision. Column (3) adds year fixed effects that capture common shocks, while column (4), our preferred specification, allows the year effects to vary across the nine states, capturing thus state-wise differences not only in the overall economic and policy environment, but also potential differences in the implementation of the audits or the strictness of the prosecution. When relying on within-state variation only, the treatment coefficient decreases, but deforestation still rises by about 11% in the aftermath of the audits. The two additional columns check for the robustness of these estimates by adding municipality or lottery round specific trends, $\lambda_m \, t$ or $\lambda_l \, t$ to equation (2), and hence $\lambda_{m,l}$ to equation (3). The first, λ_m, can account for municipality specific differences in the growth rates of deforestation; the second, λ_l captures all potential structural differences between the audit procedures following different lottery rounds. The

[14]The Harris-Tzavalis unit root test for short panels confirms very strongly the stationarity of our deforestation data (Harris and Tzavalis 1999).

inclusion of these differential trends leaves our audit effects broadly unaffected.

We also investigate the effects of the audits on deforestation dynamics more explicitly. The two last columns of table 6 include the audit treatment indicator in levels instead of first differences, and hence measure changes in the trend of deforestation growth. Column (7) shows that the trend in deforestation growth does not change for all the years after the audits. However, when we additionally control for the years since the audit in column (8), we see increases in the trend of deforestation immediately after the audit, with the effect turning zero within the next 2 years. Figure 2 investigates the time pattern of the treatment effects more explicitly by decomposing the yearly dynamics of the public audit effects on deforestation. For this purpose, we split the treatment effect into a set of dummy variables that capture how many years j have past since the audit:

$$\Delta \ln D_{it} = \sum_{j=1}^{6} \gamma_j \, A_{ijt} + \Delta \mathbf{X}'_{it} \, \boldsymbol{\beta} + \kappa_{st} + \upsilon_{it}. \tag{4}$$

We follow the effects for up to six years after the audit year, which is the longest time-period that we can observe after the first 2003 audits. For municipalities with multiple audit treatments, the time dummies record the passing of time after each audit. The yearly coefficients show that deforestation increases significantly in the audit year and keeps increasing in the year after the audit; in the next few years, deforestation stays constant at this increased level. Only in the sixth year after the audit appears a significant decrease in deforestation rates, which by construction reflects larger decreases in deforestation in 2009 in those municipalities that were audited in the first lottery rounds. This last result might point towards genuine longer term beneficial effects of the public audits, arising from long-term improvements in local governance. Overall, the cumulative audit effect is statistically significantly detrimental up to two years after the audit, and is indistinguishable from zero for all the later years.[15]

Finally, placebo regressions in Table 7 show that specifying incorrectly the timing of the audit treatment does not yield significant treatment effects. For this purpose, we rerun the regression specified in equation 3 (including state-year fixed effects) for re-defined treatment variables that have been shifted by one to three years as compared to the timing of the actual randomized treatment. We see that

[15]We compute the cumulative audit effect as a linear combination of the estimated yearly effects over the analyzed time period.

from the seven reported treatment regressions all six that shift the treatment to placebo years turn out to be insignificant.

The above results on the effects of public fiscal audits yield to a strictly causal interpretation. Since the audit treatments have been fully randomized, the timing of each audit as well as whether it occurs at all are truly random. After filtering out the time invariant municipality fixed effects and the effects of state-wise common yearly shocks, the public audit coefficients capture the causal effect of public fiscal audits on deforestation, and show a sizable average increase of deforestation in the aftermath of the public audits by at least 11%. The local forest resources seem to suffer especially in the years immediately after the audits, while the detrimental effects tend to vanish after a longer time period.

4.2 Potential channels

Public audits could affect deforestation through several channels. In order to shed light on the relative importance of different explanations we investigate various interaction effects that highlight differential effects of these public audits, depending on the governance quality, neighborhood spillovers and learning, political structure and electoral environment, or information dissemination in the audited municipalities. This approach has the limitation that lottery draws were not randomized along some of the dimensions that we are examining, most importantly, governance quality, but also media presence or other socio-economic factors. In these cases, we cannot claim the same strong causal link for the differential audit effects as with the overall audit effect. Other variables are less affected by this issue. Neighboring audits are subject to the same randomization process and can be considered as fully exogenous, especially once the common state-year variation, capturing also the progress of the auditing process, is controlled for. Whether mayors serve their first or second terms, and hence are subject to reelection incentives, is also mainly path-dependent and can be considered as good as random in our context. Overall, though less strongly causal, the further evidence clearly highlights some potential mechanisms at play.

4.2.1 The role of local corruption and mismanagement

The effects of public audits on local administrations' behaviour can be expected to depend on the local governance environment as well as the actual findings of the auditors. If auditors are effective in discovering mismanagement and corrupt

practices, which is very likely given the very thorough auditing procedures, the audit reports should reflect quite well the quality of local governance. If the audit report turns out to be very unfavourable, local officials can face increasing pressure, both from their political adversaries (Ferraz and Finan 2008), or from the judiciary (Litschig and Zamboni 2010). As a consequence, local governance quality can be expected to increase in the long term. As described before, this can happen when very corrupt mayors lose elections (electoral mechanism) or if local officials adjust their behavior in order to comply more strongly with federal legislation in fear of future retributions (expected punishment mechanism). At the same time, however, freshly audited local administrations might shift their rent seeking activities towards spheres that are not monitored by this audit program. Local administrators who are more involved in corrupt activities could be more strongly tempted to resort to other means of generating income when facing an increase in fiscal scrutiny, for instance through promoting illegal land use. Moreover, if the public pressure to reduce local corruption and mismanagement increases with the severity of the corruption findings, this will also reinforce the incentive to search for political support among local landlords, leading potentially to more deforestation.

In order to see whether deforestation increases in more corrupt municipalities by more, we re-estimate equation (3) by allowing treatment effects to vary with the corruption findings in the reports C_{it}:

$$\Delta \ln D_{it} = \gamma_1 \Delta A_{it} + \gamma_2 \Delta A_{it} \times C_{it} + \Delta \mathbf{X}'_{it} \boldsymbol{\beta} + \kappa_{st} + \upsilon_{it}. \tag{5}$$

To the extent that the audit based governance measures reflect the local corruption environment, we expect to see a larger audit response in more corrupt municipalities. As before, we use both a measure of overall management related irregularities and of more specifically corruption related findings to assess local governance quality. The audit reports give us a measure of the quality of governance in the years preceding a given audit year. In case of repeated audits, we substitute the new reported corruption level once revealed. Since auditing and reporting procedures changed over the time period of seven years somewhat (cf. section 2), we apply two different procedures to scale governance measures. First, we standardize corruption measures, which are normalized to have zero mean and a standard deviation of one over all reports on municipalities within the Legal Amazon. Second, since findings of corruption and management irregularities increased consistently over the years (cf. table 2), we also experiment with governance measures that

have been standardized year-by-year. This second normalization only exploits the within-year variation across the audited municipalities, and hence purges the governance proxies of all influences that might come from structural changes in the reporting procedures over time. Third, we split the treatment effect into two, for municipalities with above and below median governance quality.

Treatment interactions with the two governance proxies (documented in Table 8) turn out overall insignificant. While models (1) and (2) standardize corruption measures over the whole time period, columns (3) and (4) report yearly standardized corruption measures. Columns (5) and (6) split the audit effects into two, for municipalities with high and low governance quality. The first four columns show that in a municipality with average governance environment, deforestation increases by about 11%. The interactions with the governance variables, although somewhat higher with the yearly normalization, remain overall insignificant. Splitting the treatment effect into two in columns (5) and (6), for municipalities with higher/lower than median corruption findings/irregularities, results in a significant detrimental treatment effect only in high corruption municipalities. The difference between the two treatment coefficients is sizeable in case of our corruption variable, but still not statistically significant.

The level of governance failures is however significantly related with increases in deforestation if we restrict our attention to audited municipalities. In order to see this, we re-estimate the first difference equation for the 212 audited Amazon municipalities, for the whole time period as well as separately for the time before and after the audits. Here we include our governance proxies in levels and thus test for whether governance affects the trend in deforestation. For the time before the audit, we impute the governance proxies from the closest following audit, thus assuming a constant governance environment. Table 9 reports the results for these audited municipalities. While the first two columns show once again a statistically insignificant effect on the trend in deforestation growth, for the time period after the treatment both governance proxies lead to statistically significantly larger trend growth in deforestation. None of these differential effects arise though in the years prior to the audits: in columns (5) and (6), coefficients on governance proxies are negative and insignificant.[16] These effects point towards differential response to the public audits in more and less corrupt municipalities. These differential governance findings are not driven by pre-trends but arise after audits

[16]The panel for the time period before the audits can only be estimated for 193 municipalities, since 20 municipalities were already audited in summer 2003.

have taken place. These findings yield some, although not overwhelming, support to the potential explanation that officials in high corruption municipalities adjust their behavior after the first-hand experience of public audits, and shift revenue generating activities to less observed fields.

4.2.2 Neighborhood effects and learning

We observe that deforestation rates increase significantly in the first two years after the audit. This leads us to believe that after audits take place, municipality officials update their beliefs about the likelihood that specific forms of corruption or mismanagement are going to be discovered by the federal auditors as well as about the potential costs of these activities. Among others, they might realize that deforestation related activities are not among the audited outcomes. Even if auditors were not perfectly able to discover all cases of corruption, more corruption related findings in the report should lead to a larger re-adjustment of expectations and a larger behavioral response.

If learning and the updating of prior beliefs play an important role in explaining the audit effects, the analysis of neighborhood effects can provide useful further evidence on this mechanism. Before the municipality gets audited, local governments might be only imperfectly informed about the exact auditing procedures, the scope of the investigations, the thoroughness of the auditors and hence their likelihood to discover specific forms of mismanagement and corruption. However, since information flows relatively easily between direct neighbors, local governments should also be able to learn from the audit experiences of neighboring municipalities and update their beliefs about the modalities of public audits. We would thus expect that local administrations will adjust their behavior not only after a public audit of their own books and procedures but also after audits of neighboring municipalities took place. If audits lead to more deforestation at the local level either by shifting attention to fiscal management or shifting corrupt activities to non-audited fields, learning about the audits of other municipalities should also lead to an increase in deforestation.

We address learning from the neighbors by including the change in neighboring audits, measured by the number of neighboring municipalities that got newly audited in a specific year $\sum A_{-it}$, into the difference equation (3) as a further control:

$$\Delta \ln D_{it} = \gamma_1 \, \Delta A_{it} + \theta_1 \Delta \sum A_{-it} + \Delta \mathbf{X}'_{it} \, \boldsymbol{\beta} + \kappa_{st} + \upsilon_{it}. \tag{6}$$

Since audits are randomized at the state level, neighboring audits can also be treated as exogenous and estimated in first difference form. We expect both learning from own audits as well as learning from neighboring audits to lead to increases in deforestation, and hence yield positive γ_1 and θ_1 coefficients. However, learning from neighbours should play a smaller role as soon as the municipality itself gets audited: we incorporate this idea by adding a further interaction between own and neighboring audits. Moreover, neighboring audits can affect deforestation not only through learning from neighbors' experiences, but also by fostering deforestation in neighboring municipalities. Since deforestation is a spatially diffuse process, neighboring deforestation can have spillover effects also on its own (cf. Robalino and Pfaff 2012, Hargrave and Kis-Katos 2012). To additionally control for this channel, we add to the above equation a spatial lag in deforestation, weighting the changes in the neighboring deforestation vector $\Delta \ln \mathbf{D}_{-it}$ with a vector of spatial contiguity $\mathbf{W}$, which is normalized so that spatial weights sum up to one:

$$\Delta \ln D_{it} = \gamma_1 \Delta A_{it} + \theta_1 \Delta \sum A_{-it} + \theta_2 \mathbf{W}' \Delta \ln \mathbf{D}_{-it} + \Delta \mathbf{X}'_{it} \boldsymbol{\beta} + \kappa_{st} + \upsilon_{it}. \qquad (7)$$

The introduction of the spatially lagged dependent variable as a further explanatory factor raises however endogeneity concerns. This is the reason why we also reestimate the spatial panel equation with a spatial panel GMM procedure, instrumenting for the endogenous spatial lag within the model.[17]

Table 10 presents evidence for learning from neighboring audits. Column (2) shows that one additional audit in a neighboring municipality in a given year increases deforestation by almost the same extent as own audits do. The interaction between own and neighborhood audits is highly significantly negative in column (3): the own audit effects are smaller if neighbors get audited at the same time and vice versa. These neighborhood effects cannot be fully explained by spatial contagion. The endogenous spatial lag turns out significant in specification (4) and it reduces the audit effects on deforestation in size and renders the own audit effect even insignificant. Addressing the endogeneity of the spatial lag in a spatial GMM procedure in columns (5) and (6) halves the spatial lag coefficient and yields once again significant neighboring audit effects, although of about half of

[17] We perform the spatial panel regressions in R, while all other models are estimated in the statistical package Stata™. In the spatial panel GMM we apply a fixed effect transformation, instead of the first difference form. For reasons of convergence, in the spatial GMM only time fixed effects but no state-time fixed effects are included. For the spatial analysis, we also had to exclude two further municipalities that had no direct neighbors in the sample.

the original magnitude. For the neighborhood results it does not matter whether we only estimate a spatial lag model (column 5) or also adjust for potential spatial autocorrelation of the error terms (column 6), but the latter reduces somewhat the standard errors of the own audit effect (to a significance level of 12%).

Table 11 once again differentiates between audit effects by audit findings, this time both for own and neighboring audits. Columns (1) and (2) show that deforestation increases after neighboring audits by even more if these find many irregularities or document high levels of corruption. A new audit in one of the neighboring municipalities, yielding governance findings one standard deviation worse than the yearly average, leads to an increase in deforestation of about 20-22%, which is about the double of the simple neighboring audit effect. Once again, controlling for the (endogenous) spatial lags in columns (3) and (4) reduces these differential neighboring effects considerably in magnitude, but they stay still significant. Columns (5) to (8) show estimation results from the spatial GMM procedure including a spatial lag, which is instrumented within the model. As before, once the endogeneity of the spatial lag is accounted for, we see that deforestation increases by more when neighboring municipalities receive less favourable audits. Municipalities that observe an abundance of unfavourable findings within the neighboring audit reports might feel especially threatened by the fiscal audits, which leads to a stronger deforestation response.

These neighborhood results are once again perfectly in line with an explanation that emphasizes learning. Local administrations that learn from own and neighboring audit experiences update their beliefs with respect to what types of corrupt behavior get detected and potentially sanctioned and adjust their future behavior accordingly. This interpretation is strengthened by the decreasing impact of neighboring audits once a municipality has got audited itself as well as by the impact increasing with the rise of bad governance findings within the neighboring reports.

4.2.3 Electoral considerations

Reelection considerations could offer us an important explanation for the observed reaction of deforestation to public audits. Ferraz and Finan (2008, 2011) document that for Brazil as a whole, electoral accountability has played an important role both in explaining differences in municipal corruption levels, and in the effects of the publicly revealed audit information. In municipalities that got audited

before the 2004 local elections, more corrupt mayors had significantly worse re-election chances than in municipalities that got audited after the elections (Ferraz and Finan 2008). Moreover, due to a two-term limitation for mayors, first term mayors face larger incentives to perform well in order to be reelected. As a result, corruption levels measured by the audits turn out generally lower in municipalities with mayors who serve their first term (Ferraz and Finan 2011). Similar findings can also be replicated within our sample of Amazonas municipalities.

Table 12 shows that the probability that a mayor was reelected in the municipal elections of 2004 or 2008 was not affected by audits per se, but was reduced if audits resulted in high corruption findings. We restrict our attention to those Amazonas municipalities that were audited for the first time within a two-year window before or after the two municipal election rounds, and where the incumbent was standing for reelection. We estimate the probability of reelection of the incumbent mayor separately for the two election rounds and for the pooled sample of 92 municipalities. We measure local governance by corruption findings of the audit reports from just before or after the elections. The coefficient on corruption shows that, as long as not audited, more corrupt mayors were also more likely to be reelected. The interaction between audit and corruption findings is negative but insignificant for 2004, and significant in the pooled sample and for 2008. This corroborates the findings of Ferraz and Finan (2008), by showing that in the Amazon municipalities audit reports had similar effects as in Brazil as a whole, and mayoral chances of reelection declined significantly with negative corruption findings.[18]

The above results, together with the more detailed evidence on Brazil as a whole by Ferraz and Finan (2008, 2011), indicate that electoral accountability restricts rent extraction by elected officials. However, electoral considerations can also have the opposite effect, especially in the case of rents that do not worsen and potentially even improve the reelection chances of mayors in office. If rent extraction through illegal deforestation benefits many local players, the median voter might be in favour of more leniency towards deforestation. Once audited, mayors facing reelection incentives might be in even larger need of the support of local loggers or squatters, and hence might decide to foster or tolerate more deforestation. We would thus expect that electoral considerations should affect

[18]The same results cannot be replicated when using our irregularities measure instead of corruption findings. We also do not see statistically significant differences in corruption findings between first- and second-term mayors.

the local administrations' response to the audits. Mayors who face reelection incentives, should react more strongly to negative reports than mayors who cannot be reelected. In Brazil, mayors face a term limit regulation that restricts their mayoral activities to two consecutive terms. Although they could still return to local politics after a break, this is a rather rare event: only 12% of second term mayors in the 2001-2004 term were reelected in 2008, and only further 9% of them were running for a higher office (Ferraz and Finan 2011, p. 1281). The term limit rule seems thus to effectively shape the political horizon of the mayors.

We address the effects of mayoral reelection incentives on deforestation by introducing an indicator variable for the mayor serving his or her first term FT_{it} into equation (3) and interacting it with the public audit treatment.[19] In further specifications, we differentiate between the mayoral reelection incentives depending on the governance findings of the audit reports C_{it}, by introducing a triple interaction between audits, first term mayors, and governance findings:

$$\begin{aligned}\Delta \ln D_{it} =& \phi_1 \Delta A_{it} + \phi_2 \Delta(A_{it} \times C_{it}) + \phi_3 \Delta(A_{it} \times C_{it} \times FT_{it}) + \\ & \phi_4 \Delta FT_{it} + \phi_5 \Delta(A_{it} \times FT_{it}) + \Delta \mathbf{X}'_{it} \boldsymbol{\beta} + \kappa_{st} + \upsilon_{it}.\end{aligned} \tag{8}$$

We would expect worse governance findings leading mayors to re-adjust their behavior more strongly if they stand for reelection, yielding a positive ϕ_3 coefficient. The results of these regressions are documented in table 13. The coefficient on first term mayors is positive but insignificant: deforestation is not higher under first term mayors per se. Similarly, audits do not affect deforestation differently in municipalities with first or second term mayors. Columns (3) and (4) present however some very suggestive results. Worse corruption findings lead to considerably more deforestation if the mayors face reelection constraints. The baseline audit effect (for second term mayors with average corruption findings) turns slightly insignificant, and it is clearly zero for second term mayors with very high corruption findings. However, the triple interaction effects are very sizeable: A one standard deviation increase in corruption findings from the mean of that year increases deforestation by further 22-24% if the mayor serves only his first term.[20] The overall audit effect for a municipality with a first term mayor and corruption or irregularity levels one

[19] In years when a change in mayors or mayoral terms takes place, our first term mayor variable records the share of the August-July deforestation year for which the municipality was governed by a first-term mayor.

[20] These differential effects are still highly significant even if governance variables are standardized over the whole time period (and not yearly).

standard deviation above the mean of that year lies around 36%; these effects are highly significant (with standard errors of about 10%). These effects are considerably higher than the about 12% and slightly insignificant increase in deforestation in a municipality with average corruption findings and a first term mayor, and become quickly statistically insignificant as corruption findings improve beyond the average.

Once again, these results support the notion that in the face of increased public scrutiny, there is a substitution of illegal activities towards less closely observed areas. Reelection incentives play a detrimental role in this process: mayors who can stand for reelection react to negative audit reports much more strongly than second term mayors.

4.2.4 Further issues

Ferraz and Finan (2008, 2011) argue that information dissemination plays an important role in explaining the audit effects on local governance, and find especially larger audit effects in municipalities with a local radio station that can disseminate news. We cannot confirm these effects strongly in our sample. In order to test for this effect, we control for the presence of local radio stations by an indicator variable which takes one if the municipality had a radio station either in 2004 or in 2009.[21] In table 14 we split the effect of audit treatment (and governance interactions) for municipalities with and without radio stations. We find a significant detrimental audit effect in municipalities with radio stations, but no significant effect in municipalities without a radio station. This finding is in line with the literature that would predict that audits are only relevant if audit information gets disseminated by the local media. However, neither the audit coefficients nor their interactions with governance quality are significantly different for the two groups of municipalities in statistical terms. Moreover, the radio station variable might have several drawbacks. First of all, it is not randomly distributed over the audited municipalities and might pick up also the effects of other confounding economic factors. Secondly, information is only available for one or two years over the whole time period and thus cannot capture well changes in the media environment. Since only a smaller share of urbanized Amazonas municipalities do actually have a radio station, it is possible that these variables are not the best proxies for the quality

[21] IBGE recorded the presence of local radio stations in 2004 and 2009; over this time period 11.5% of municipalities switched from no station to having a radio station or the other way around, while only 13% have had consistently access to local radio.

of information flows in this area.

Litschig and Zamboni show that judicial presence in a municipality reduces the likelihood that audits will discover administrative irregularities by about 0.3 standard deviations, although it does not affect corruption on the intensive margin. In order to test for the importance of this channel, we assembled information on the presence of a judicial seat within the municipality in 1999 and 2012. While in 1999 about 48% of Amazonas municipalities had judiciary seats, this number increased to 60% by the end of the period. Since yearly data on judiciary presence throughout the whole period is unavailable, we build the interaction terms in two ways: both based on historical (1999) and recent (2002) information. Table 15 presents the results using the 1999 seat measure; results using the other measure are virtually the same. When splitting the municipalities into two categories, with and without direct judiciary presence, only municipalities with judiciary presence experience significant detrimental effects of public audits on deforestation. Once again, this is in line with the expectations from the literature, but just as in the case of radio stations, the difference between the two groups of municipalities is not statistically significant. The difference also remains insignificant when differentiating judicial effects by the extent of corruption findings, or even interacting them with mayors who can seek reelection (not shown here). We thus find no statistically significant difference in the adverse audit effects on deforestation irrespectively of the potential strength of the threat of judiciary prosecution. A limitation of this approach remains that since the placement of judiciary seats cannot be considered as random, this selection bias might affect our estimates.

Overall, the results point into the direction which could be expected based on findings of previous literature but all difference remain statistically insignificant.

5 Conclusion

This paper addressed the unintended effects of a federal anti-corruption program, which has the goal of fighting corruption in municipality administrations, on deforestation in the Brazilian Amazon. For this purpose, we connected yearly panel data on deforestation dynamics from the PRODES project (for the years 2002-2009) with information derived from the local fiscal audit program for 212 originally forested Amazon municipalities. We used the public audit reports to construct an overall measure of administrative quality by counting irregularities reported

by the auditors, as well as a more specific text-search based semantic measure of corruption intensity. On average, the descriptive evidence shows that over the observed eight years, deforestation was higher in municipalities with a worse corruption environment, although the relationship tends to become weaker when more socio-economic controls are included.

The random fiscal audits, implemented by a national lottery, offer a unique opportunity to assess the effects of anti-corruption initiatives on sectors not directly audited/monitored within the program. We exploit the random distribution of the audits across 602 eligible and originally forested Amazon municipalities as well as the random timing of the audits. We regress the size of yearly newly deforested area on the public fiscal audit treatment in a first difference framework that controls for municipality and state-year fixed effects. The results show a considerable average increase in deforestation (by at least 11%) after the audits, with increases in deforestation rates concentrated in the first two years after the audits.

We identify several channels through which audits affect deforestation. The adverse deforestation effects are stronger in municipalities with worse corruption records, and arise only after these municipalities have been audited. Deforestation also increases after neighboring audits take place, especially if these latter result in worse governance findings, which cannot be entirely explained by the spatial process of deforestation. One potential explanation for these findings can be that local administrations learn from the audit experiences and update their expectations and hence change their behavior, which results in increased deforestation. Electoral considerations seem also to play an important role: when mayors face reelection incentives, audit reports with worse governance findings lead to a larger subsequent increase in deforestation.

These findings might seem unexpected and counterintuitive as top-down monitoring and increased public scrutiny coupled with electoral accountability mechanisms should ideally lead to overall improvements in the governance performance of local governments. However, the results call attention to the potential of unintended consequences of anti-corruption activities, which can cause rent extraction to increase in sectors less directly observed by the auditors and the public. Such results do not question the benefits of central and public monitoring in the fight against corruption, they show however that anti-corruption strategies are more likely to be overall successful if they embrace multidimensional approaches.

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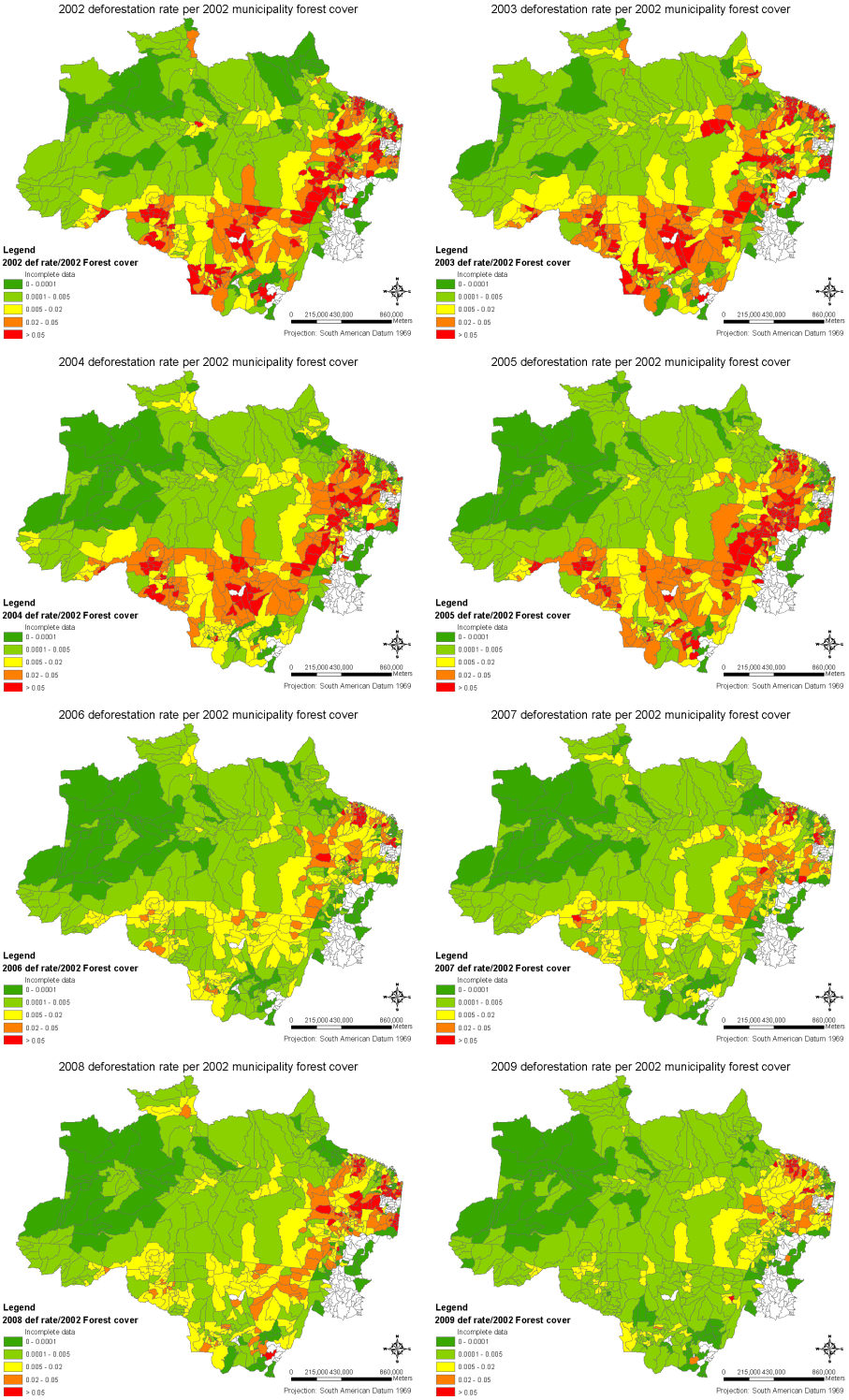
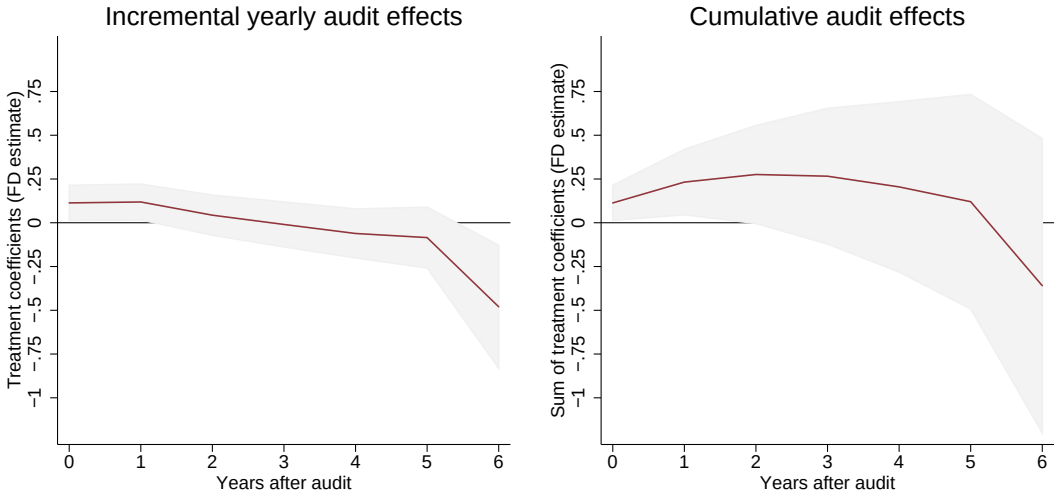
Figures

Figure 1: Yearly deforestation in the Brazilian Amazon municipalities



Note: Maps are based on yearly deforestation data from INPE; they are generated by ArcGIS.

Figure 2: Yearly dynamics of audit effects on deforestation



Note: The graphs report treatment coefficients and 90% confidence intervals as estimated by equation (4). The left panel shows the yearly increment of the audit effect, the right panel shows the total effect of an audit for each year after it took place, computed as a linear combination of the yearly incremental effects.

Tables

Table 1: Summary statistics on audit reports

	Mean	St.dev.	Min.	Max.
Government funds [mn R$]	9.6	26.4	0.2	436.6
No. of ministries	9.0	2.9	2	17
Pages	87.7	44.4	11	264
No. of programs	23.3	8.8	4	58
No. corrupt violations	8.9	9.3	0	61
No. irregularities	58.0	29.2	7	160
Relative No. irregularities	2.9	1.9	0.2	10.2

Note: Statistics refer to N=319 audit reports for 285 municipalities in the Brazilian Legal Amazon, audited before 2010.

Table 2: Auditing procedures over time

Year	2003	2004	2005	2006	2007	2008	2009
No. audited municipalities	73	63	58	36	33	23	33
Median audit duration	5	5	5	47	52	54	60
Median report length	65	74	67	77	101	84	143
Median No. listed programs	29	28	24	21	17	16	15
Median No. irregularities	41	49	52	52	65	71	81
Median No. corrupt expressions	3	8	7	7	7	6	12

Note: Statistics refer to N=319 audit reports for 285 municipalities in the Brazilian Legal Amazon.

Table 3: Summary statistics on total deforestation and initial conditions

	Mean	St.dev.	Min.	Max.
ln Total deforestation	4.43	1.82	0	9.01
Corruption (standardized)	-0.03	1.01	-0.96	5.59
Irregularities (standardized)	-0.16	0.88	-1.40	3.90
Corruption (standardized yearly)	0.01	0.97	-1.18	3.70
Irregularities (standardized yearly)	-0.07	0.98	-1.98	3.00
ln Initial forest	6.91	2.47	-2.30	11.93
ln Cloud coverage	2.68	2.69	0	8.73
ln Average rainfall	5.12	0.18	4.50	5.62
ln Distance to Brasilia	7.24	0.39	5.93	7.96
Savanna percent	8.98	21.42	0	100
ln Initial population	9.60	1.00	7.12	12.67
ln Ini. real GDP p.c in agriculture	0.10	0.99	-3.14	2.99
ln Ini. protected area	4.33	3.59	0	10.97
ln Ini. settlement project size	4.04	2.92	0	9.56

Note: Statistics refer to $N = 209$ originally forested municipalities. Governance variables are standardized over the sample of $N = 285$ audited municipalities in the Legal Amazon.

Table 4: Summary statistics of the panel data (2002-2009)

	Mean	St.dev.	Min.	Max.
ln Deforestation	2.00	1.65	0	7.25
Audit	0.22	0.45	0	3
Standardized corruption	-0.03	0.41	-0.96	5.59
Standardized irregularities	-0.07	0.37	-1.40	3.90
Yearly stand. corruption	0.00	0.42	-1.18	4.35
Yearly stand. irregularities	-0.01	0.45	-1.98	3.00
Neighb. audits	1.19	1.44	0	8
Neighb. deforestation rates	1.58	2.52	0	29.08
Neighb. corruption (yearly st.)	0.03	0.63	-1.18	3.70
Neighb. irregularities (yearly st.)	0.00	0.65	-1.98	3.00
First term mayor	0.73	0.41	0	1
ln Clouds	1.30	2.30	0	10.36
ln Not observed	1.29	1.98	0	9.18
Second term mayor	0.59	0.50	0	1

Note: For *First term mayor* statistics refer to N=4598 observations (on 600 municipalities), for *Second term mayor* statistics are on N=92 observations. For all other variables N=4816 (on 602 municipalities).

Table 5: Relationship between total deforestation and corruption

Dependent	ln Total deforestation					
Governance var.		Corruption			Irregularities	
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A	Gov. variables standardized over all reports					
Gov. failures	0.196**	0.111	0.096	0.137	0.037	0.042
	(0.077)	(0.068)	(0.062)	(0.122)	(0.111)	(0.088)
ln Ini. forest	0.760***	0.681***	0.653***	0.762***	0.675***	0.647***
	(0.041)	(0.040)	(0.041)	(0.040)	(0.039)	(0.040)
ln Cloud coverage	-0.318***	-0.189***	-0.157***	-0.343***	-0.191***	-0.159***
	(0.037)	(0.042)	(0.040)	(0.036)	(0.042)	(0.040)
ln Av. rainfall		-1.495***	-1.416***		-1.552***	-1.453***
		(0.516)	(0.441)		(0.518)	(0.442)
ln dist. Brasilia		-0.887***	-0.741***		-0.871***	-0.720***
		(0.233)	(0.208)		(0.238)	(0.213)
Savanna %		-0.017***	-0.016***		-0.017***	-0.015***
		(0.004)	(0.003)		(0.004)	(0.003)
ln Ini. pop.		0.480***	0.404***		0.518***	0.434***
		(0.090)	(0.086)		(0.085)	(0.080)
ln Ini. pc. GDP in Agr.		0.395***	0.334***		0.393***	0.334***
		(0.085)	(0.083)		(0.086)	(0.083)
ln Ini. protected			-0.075***			-0.074***
			(0.021)			(0.021)
ln Ini. settlement			0.133***			0.135***
			(0.024)			(0.024)
Adj. R-sq	0.617	0.701	0.751	0.609	0.698	0.749
PANEL B	Gov. variables standardized yearly					
Gov. failures	0.232***	0.123	0.100	0.213**	0.139*	0.119*
	(0.077)	(0.076)	(0.069)	(0.086)	(0.078)	(0.070)
Further controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Adj. R-sq	0.621	0.701	0.751	0.571	0.698	0.749

Note: The table reports OLS estimates with the dependent variable being the log of total deforested area over 2002-2009 within the municipality. Robust standard errors are reported in parentheses. Governance variables are standardized over all reports in the Legal Amazon in panel A and standardized yearly in panel B. Columns in Panel B include the same additional controls as the corresponding columns in Panel A. The observations are restricted to $N = 209$ municipalities with a full set of controls. *,**,*** denote significance at 10/5/1% level.

Table 6: Effects of public fiscal audits on yearly deforestation (FD estimates)

Dependent	Δ ln Deforestation							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ Audit	0.169**	0.177***	0.124*	0.113*	0.163**	0.143*		
	(0.070)	(0.068)	(0.067)	(0.062)	(0.075)	(0.074)		
Audit							-0.010	0.059*
							(0.017)	(0.031)
Audit × Months since audit								-0.036***
								(0.011)
Further controls	*No*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Year effects	*No*	*No*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
State-year effects	*No*	*No*	*No*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Lottery spec. trends	*No*	*No*	*No*	*No*	*Yes*	*No*	*No*	*No*
Munic. spec. trends	*No*	*No*	*No*	*No*	*No*	*Yes*	*No*	*No*
R-sq.	0.002	0.026	0.107	0.218	0.220	0.248	0.217	0.218

Note: The table reports first difference estimates, with the dependent variable being the change in the log of yearly newly deforested area. Robust standard errors, clustered at the municipality level, are reported in parentheses. Further controls include the first difference in ln Clouds and ln Not observed area. The results refer to $N = 4214$ observations, for 602 originally forested municipalities. *,**,*** denote significance at the 10/5/1% level.

Table 7: Placebo treatment regressions (FD estimates)

Dependent	Δ ln Deforestation						
Treatment year	$(t-3)$	$(t-2)$	$(t-1)$	(t)	$(t+1)$	$(t+2)$	$(t+3)$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ Audit	-0.084	0.069	-0.035	0.113*	0.004	-0.075	-0.045
	(0.089)	(0.076)	(0.064)	(0.062)	(0.051)	(0.055)	(0.060)
State-year effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Further controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*

Note: The table reports first difference estimates, with the dependent variable being the change in the log of yearly newly deforested area. The different columns present treatment effect estimates where the actual treatment in year t has been shifted to $t-3$ to $t+3$ for all municipalities. Further controls include first differences of ln Clouds and ln Not observed. The results refer to $N = 4214$ observations, for 602 municipalities. Robust standard errors, clustered at the municipality level, are reported in parentheses. *,**,*** denote significance at the 10/5/1% level.

Table 8: Differential audit effects by governance quality (FD estimates)

Dependent	Δ ln Deforestation					
Governance var.	Standardized		Yearly standard.		Categories of	
	Corr.	Irreg.	Corr.	Irreg.	Corr.	Irreg.
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Audit	0.113*	0.113*	0.113*	0.115*		
	(0.062)	(0.061)	(0.062)	(0.062)		
Δ (Audit × Gov. failures)	0.014	-0.005	0.055	0.044		
	(0.036)	(0.050)	(0.044)	(0.054)		
Δ (Audit × Good governance (κ_1))					0.027	0.096
					(0.088)	(0.094)
Δ (Audit × Bad governance (κ_2))					0.169**	0.103
					(0.078)	(0.073)
State-year effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Further controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
R-sq.	0.218	0.218	0.218	0.218	0.218	0.218

Note: The table reports first difference estimates, with the dependent variable being the change in the log of yearly newly deforested area. Standardized governance variables are standardized over all reports to have a zero mean and standard deviation of one. Yearly standardized variables have a zero mean and standard deviation of one for all reports from the same year. Good/bad governance are indicator variables for governance findings above/below the median. For corruption, this indicates more than 5 corrupt expressions, for irregularities, more than 1.97 irregularities per investigated program. Further controls include first differences in ln Clouds and ln Not observed. The results refer to $N = 4214$ observations, for 602 municipalities. Robust standard errors, clustered at the municipality level, are reported in parentheses. *,**,*** denote significance at the 10/5/1% level.

Table 9: Deforestation and governance in audited municipalities (FD estimates)

Dependent	Δ ln Deforestation					
Time frame	2003–2009		After first audit		Before first audit	
Governance var.	Corr.	Irreg.	Corr.	Irreg.	Corr.	Irreg.
	(1)	(2)	(3)	(4)	(5)	(6)
Gov. failures	0.015	0.020	0.042**	0.071***	-0.006	-0.021
	(0.012)	(0.013)	(0.017)	(0.021)	(0.027)	(0.035)
State-year effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Further controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
No. observations	1484	1484	975	975	509	509
No. municipalities	212	212	212	212	192	192
R-sq.	0.275	0.275	0.367	0.368	0.184	0.185

Note: The table reports first difference estimates for balanced and unbalanced panels of ever audited municipalities. The dependent variable measures the change in the log of yearly newly deforested area. Governance variables are standardized over all reports in the legal Amazon to have a zero mean and standard deviation of one and are included in levels. Further controls include first differences in ln Clouds and ln Not observed. Columns (1) and (2) report results for the whole period of observation; (3) and (4) show results for the years after the first audit; (5) and (6) show results for the years before the first audit took place, imputing the governance findings from the first subsequent report. Robust standard errors, clustered at the municipality level, are reported in parentheses. *,**,*** denote significance at the 10/5/1% level.

Table 10: Neighboring spillovers from public fiscal audits

Dependent	ln Deforestation					
Model	FD	FD	FD	FD	FE GMM	FE GMM
	(1)	(2)	(3)	(4)	(5)	(6)
Audit	0.113*	0.115*	0.228***	0.051	0.044	0.051
	(0.062)	(0.062)	(0.085)	(0.055)	(0.033)	(0.033)
Neighb. audits		0.097***	0.117***	0.069***	0.041***	0.045***
		(0.025)	(0.027)	(0.022)	(0.012)	(0.015)
Audit ×			-0.078***			
Neighb. audits			(0.027)			
Spatial lag				0.147***	0.077***	0.085***
				(0.010)	(0.009)	(0.010)
Spatial error						0.075
State-year effects	*Yes*	*Yes*	*Yes*	*Yes*	*No*	*No*
Year effects	*No*	*No*	*No*	*No*	*Yes*	*Yes*
Further controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
No. observations	4214	4214	4214	4214	4800	4800
No. municipalities	602	602	602	602	600	600
R-sq.	0.218	0.221	0.222	0.348		

Note: Columns (1) to (4) report OLS estimates in first difference form, with robust standard errors clustered at the municipality level. Columns (5) and (6) report spatial panel GMM estimates of a fixed effect transformation, correcting for spatial dependence. These latter results are estimated in the statistical package R; all other empirical results in this paper are estimated in Stata™. Further controls include ln Clouds and ln Not observed. *,**,*** denote significance at the 10/5/1% level.

Table 11: Neighboring spillovers from public fiscal audits

Dependent	ln Deforestation							
Governance var.	Corr.	Irreg.	Corr.	Irreg.	Corr.	Irreg.	Corr.	Irreg.
Model	FD	FD	FD	FD	FE GMM	FE GMM	FE GMM	FE GMM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Audit	0.116*	0.116*	0.051	0.051	0.044	0.045	0.052	0.054*
	(0.061)	(0.062)	(0.055)	(0.055)	(0.032)	(0.032)	(0.032)	(0.032)
Neighboring audits	0.092***	0.096***	0.066***	0.069***	0.044***	0.044**	0.047***	0.050***
	(0.024)	(0.024)	(0.022)	(0.022)	(0.012)	(0.012)	(0.014)	(0.014)
Audits × gov. failures	0.058	0.036	0.031	0.011	0.040	0.077**	0.045	0.073**
	(0.044)	(0.053)	(0.039)	(0.046)	(0.030)	(0.030)	(0.003)	(0.030)
Neighb. audits × neighb. gov. failures	0.090***	0.116***	0.052**	0.068**	0.065***	0.071***	0.063***	0.053**
	(0.029)	(0.038)	(0.026)	(0.033)	(0.020)	(0.021)	(0.022)	(0.023)
Spatial lag			0.147***	0.147***	0.086***	0.089***	0.093***	0.099***
			(0.010)	(0.010)	(0.008)	(0.008)	(0.009)	(0.009)
Spatial error							0.065	0.061
State-year effects	*Yes*	*Yes*	*Yes*	*Yes*	*No*	*No*	*No*	*No*
Year effects	*No*	*No*	*No*	*No*	*Yes*	*Yes*	*Yes*	*Yes*
Further controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
No. observations	4214	4214	4214	4214	4800	4800	4800	4800
No. municipalities	602	602	602	602	600	600	600	600
R-sq.	0.223	0.223	0.348	0.348				

Note: Columns (1) to (4) report OLS estimates in first difference form, with robust standard errors clustered at the municipality level. Columns (5) to (8) report spatial panel GMM estimates of a fixed effect transformation, correcting for spatial dependence. These latter results are estimated in the statistical package R, all other empirical results in this paper are estimated in Stata™. Further controls include ln Clouds and ln Not observed. *,**,*** denote significance at the 10/5/1% level.

Table 12: Audit reports and the probability of reelection

Dependent	Second-term mayor					
Election period	2004		2008		2004 and 2008	
	(1)	(2)	(3)	(4)	(5)	(6)
Audit	-0.087	-0.088	-0.056	0.079	-0.015	0.007
	(0.123)	(0.132)	(0.189)	(0.251)	(0.106)	(0.110)
Corruption		0.081		0.072		0.093**
		(0.292)		(0.259)		(0.012)
(Audit × Corruption)		-0.122		-0.237***		-0.159**
		(0.166)		(0.080)		(0.079)
No. observations	68	68	24	24	92	92
R-sq.	0.008	0.020	0.004	0.198	0.000	0.034

Note: The table reports linear probability models estimated with OLS, explaining the probability of mayoral reelection. The sample is restricted to originally forested Amazonas municipalities that were audited +/-2 years around the mayoral elections and where the mayor was running for reelection. The dependent variable is an indicator variable for the incumbent mayor winning the second term. Robust standard errors are reported in parentheses. *,**,*** denote significance at the 10/5/1% level.

Table 13: Public audit effects and mayoral term limits

Dependent	Δ ln Deforestation			
Governance variable			Yearly Standard. Corr.	Irreg.
	(1)	(2)	(3)	(4)
Δ Audit	0.112*	0.127	0.123	0.130
	(0.062)	(0.082)	(0.080)	(0.083)
Δ First term mayor	0.047	0.053	0.051	0.055
	(0.035)	(0.040)	(0.039)	(0.040)
Δ (Audit × First term mayor)		-0.022	-0.008	-0.014
		(0.080)	(0.077)	(0.079)
Δ (Audit × Gov. failures)			-0.129**	-0.096
			(0.063)	(0.067)
Δ (Audit × Gov. failures			0.236***	0.216***
× First term mayor)			(0.064)	(0.068)
State-year effects	*Yes*	*Yes*	*Yes*	*Yes*
Further controls	*Yes*	*Yes*	*Yes*	*Yes*
No. municipalities	600	600	600	600
No. observations	3998	3998	3998	3998
R-sq	0.245	0.245	0.246	0.246

Note: The table reports first difference OLS estimates, with the dependent variable being the change in the log of yearly newly deforested area. Yearly standardized governance variables have a zero mean and standard deviation of one for all reports from the same year. Further controls include first differences in ln Clouds and ln Not observed. Robust standard errors, clustered at the municipality level, are reported in parentheses. *,**,*** denote significance at the 10/5/1% level.

Table 14: Public audit effects and radio stations

Dependent	Δ ln Deforestation		
Governance variable		Yearly Standard.	
		Corr.	Irreg.
	(1)	(2)	(3)
(*a*) Δ (Audit × Radio station)	0.225***	0.213**	0.233***
	(0.083)	(0.090)	(0.084)
(*b*) Δ (Audit × No radio station)	0.076	0.081	0.076
	(0.075)	(0.075)	(0.076)
(*c*) Δ (Audit × Gov. failures × Radio station)		0.023	0.053
		(0.052)	(0.069)
(*d*) Δ (Audit × Gov. failures × No radio station)		0.028	0.017
		(0.052)	(0.058)
p-value of test (*a*) = (*b*)	0.167	0.244	0.683
p-value of test (*c*) = (*d*)		0.939	0.151
State-year effects	*Yes*	*Yes*	*Yes*
Further controls	*Yes*	*Yes*	*Yes*
No. municipalities	602	602	602
No. observations	4214	4214	4214
R-sq	0.218	0.218	0.218

Note: The table reports first difference OLS estimates, with the dependent variable being the change in the log of yearly newly deforested area. Yearly standardized governance variables have a zero mean and standard deviation of one for all reports from the same year. Further controls include first differences in ln Clouds and ln Not observed. Robust standard errors, clustered at the municipality level, are reported in parentheses. *,**,*** denote significance at the 10/5/1% level.

Table 15: Public audit effects and judiciary presence

Dependent	Δ ln Deforestation		
Governance variable		Yearly Standard.	
		Corr.	Irreg.
	(1)	(2)	(3)
(*a*) Δ (Audit × Judiciary seat)	0.165**	0.161**	0.164**
	(0.077)	(0.078)	(0.077)
(*b*) Δ (Audit × No judiciary seat)	0.063	0.078	0.074
	(0.091)	(0.090)	(0.091)
(*c*) Δ (Audit × Gov. failures × Judiciary seat)		0.021	0.002
		(0.045)	(0.078)
(*d*) Δ (Audit × Gov. failures × No judiciary seat)		0.110	0.090
		(0.101)	(0.064)
p-value of test (*a*) = (*b*)	0.379	0.469	0.438
p-value of test (*c*) = (*d*)		0.421	0.381
State-year effects	*Yes*	*Yes*	*Yes*
Further controls	*Yes*	*Yes*	*Yes*
No. municipalities	601	601	601
No. observations	4207	4207	4207
R-sq	0.218	0.218	0.218

Note: The table reports first difference OLS estimates, with the dependent variable being the change in the log of yearly newly deforested area. Yearly standardized governance variables have a zero mean and standard deviation of one for all reports from the same year. The presence of judiciary seat is measured in 1999. Further controls include first differences in ln Clouds and ln Not observed. Robust standard errors, clustered at the municipality level, are reported in parentheses. *,**,*** denote significance at the 10/5/1% level.

Data generation

A.1 List of semantic expressions

Diversion of public funds
egrep -ic "(valor*.*indevido*|indevido*.*valor*)"
egrep -ic "(pag[a*|o*]*.*indevido*|indevido*.*pag[a*|o*]*)"
egrep -ic "(utilizado*.*indevido*|indevido*.*utilizado*)"
egrep -ic "(não.*utilizaç*.*objeto)"
egrep -ic "(não.*comprovad*.*utilização.*recursos*)"
egrep -ic "(não.*atesto*.*recebimento*|falta.*atesto*.*recebimento*
|nenhuma*.*atesto*.*recebimento*|sem.*atesto*.*recebimento*
|ausência.*atesto*.*recebimento*)"
egrep -ic "((não|nenhuma|falta|ausência).*comprov*.*recebimento*)"
egrep -ic "((não|ausência).*comprov*.*depósito*)"
egrep -ic "(despesa*.*não.*previsa*|despesa*.*prevista*.*não
|prevista*.*não.*despesa*|prevista*.*despesa*.*não
|não.*despesa*.*prevista*|não.*prevista*.*despesa*)"
egrep -ic "(ausência.*entrega.*medi*|ausência.*entrega.*equi*
|ausência.*entrega.*mate*|ausência.*reci*.*entrega)"
egrep -ic "(Lei nř 9.424|Lei nř 9.394)"
egrep -ic "(incompatí*.*FUNDEF)"

Irregular procurement
egrep -ic "(sem.*prévio.*empenho*)"
egrep -ic "(Lei nř 4.320)"
egrep -ic "(sem.*devido*.*licitató*|não.*devido*.*licitató)"
egrep -ic "(Lei nř 8.666)"

Over-invoicing
egrep -ic "(sobrepreço*)"
egrep -ic "(acima.*mercado*|mercado*.*acima)"
egrep -ic "(elevado*.*preço*|preço*.elevado*)"

Fraud
egrep -ic "(pago*.*pessoa*.*não)"
egrep -ic "(servidor*.*com recursos)"
egrep -ic "(secretária.*com recursos)"
egrep -ic "(empresas*.*agente*.*público*|agente*.*público*.*empresas*)"
egrep -ic "(firma.*agente*.*público*|firmas.*agente*.*público*)"
egrep -ic -w "(firma .*prefeito|firmas .*prefeito*)"
egrep -ic "(sem.*empenho*.*notas*|não.*empenho*.*notas*
|falta.*empenho*.*notas*)"
egrep -ic "(não.*notas*.*fiscais|falta*.*notas*.*fiscais)"
egrep -ic "(ausência.*notas*.*fiscais.*originais*)"
egrep -ic "(cláusulas*.*restritiva*)"
egrep -ic "((não|sem|falta|nenhuma|nem|ausência).*comprova*.*compra)"

Incomplete construction
egrep -ic "(não.*construída*)"
egrep -ic "(não.*realizad*.*(construçoe s| obras |construção| obra)
|(construçoe s| obras |construção| obra).*não.*realizad*)"
egrep -ic "(não.*executad*.*(construçoe s| obras |construção| obra)
|(construçoe s| obras |construção| obra).*não.*executad*)"
egrep -ic "(não.*construíd*.*(construçoe s| obras |construção| obra)
|(construçoe s| obras |construção| obra).*não.*construíd*)"
egrep -ic "(não.*concluíd*.*(construçoe s| obras |construção| obra)
|(construçoe s| obras |construção| obra).*não.*concluíd*)"
egrep -ic "(execu.ão.*parcialm*)"
egrep -ic "((recursos*| obra).*equivale.*%
|equivale.*%.*(obra| obras|recursos*))"

Inexistence of documentation
egrep -ic "((informaçõ*|documentaçã*).*não.*disponibi*
|não.*disponibi*.*(informaçõ*|documentaçã*))"
egrep -ic "((informaçõ*|documentaçã*).* omissã*
| omissã*.*(informaçõ*|documentaçã*))"

Advanced payment
egrep -ic "(antecipad*.*pagam*|pagam*.*antecipad*)"

The egrep command "egrep -ic" counts the lines containing all elements of the regular expression in the audit report. This leaves out all regular expressions spread over 2 or more lines of text. Thus, the count-mechanism at hand slightly underestimates corruption level by missing some corrupt expressions. This underestimation is however not systematic and it restricts counting to words that are definitely connected. The first egrep command searches for instance all expressions that include the strings "valor" and "indevido". E.g. it finds matches for lines containing "valor...indevido", "valores...indevidos", "valor...indevidamente" etc. or the way around: "indevido...valor" etc. The first regular expression therefore captures all reported transfers that have been made with undue values. The second command captures all illicit payments mentioned in the audit report. All regular expressions can be aggregated infinitely into one single egrep command. This avoids a multiple count of a certain line containing several expressions.