The Impact of Public Policies on Deforestation in the Brazilian Amazon

Marco Elías
Cisneros Tersitsch

Zentrum für Entwicklungsforschung

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Marco Elías Cisneros Tersitsch

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Referent: Prof. Dr. Jan Börner
1. Koreferentin: Prof. Dr. Karin Holm-Müller
2. Koreferentin: Prof. Dr. Krisztina Kis-Katos

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FÜR

MAX, LOLA UND OSCAR

"Perfect friendship is the friendship of men who are good and alike in virtue,
for these wish alike to each other qua good, and they are good in themselves.
Now those who wish well to their friends for their sake are most truly friends;
for they do this by reason of their own nature and not incidentally.
Therefore their friendship lasts as long as they are good,
and virtue is an enduring thing."

Aristotle, Nicomachean Ethics VIII, 1159 b, 2-12

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ACRONYMS

3ie International initiative for impact evaluation

AM State of Amazonas (engl.); Estado do Amazonas (port.)

APA Environmental Protection Area (engl.); Área de Proteção Ambiental (port.)

ATT Average Treatment Effect on the Treated

BCB Brazilian Central Bank (engl.); Banco Central do Brasil (port.)

BFP Bolsa Floresta Program

BIOECON Conference on Biodiversity and Economics for Conservation

BLA Brazilian Legal Amazon (engl.); Amazônia Legal do Brasil (port.)

BMZ Federal Ministry for Rconomic Cooperation and Development (engl.); Bundesministerium für Wirtschaftliche Zusammenarbeit und Entwicklung (deut.)

CAR Rural Environmental Cadaster (engl.); Cadastro Ambiental Rural (port.)

CBERS China-Brazilian Satellite for Earth Resources (engl.); Satélite Sino-Brasileiro de Recursos Terrestres (port.)

CGD Center for Global Development

CGU Office of the Comptroller General (engl.); Controladoria-Geral da União (port.)

COP Conference of the Parties

DAP Department of Protected Areas

DETER Near Real-time Deforestation Detection (engl.); Detecção do Desmatamento em Tempo Quase Real (port.)

DEval German Institute for Development Evaluation (engl.); Deutsches Evaluierungsinstitute der Entwicklungszusammenarbeit (deut.)

FAS Sustainable Amazonas Fund (engl.); Fundação Amazonas Sustentável (port.)

FD First Difference

FE Fixed Effects

FINBRA Finances of Brazil (engl.); Finanças do Brasil (port.)

FUNDEF Fund for the Maintenance and Development of Fundamental Education and Teacher Enhancement (engl.); Fundo de Manutenção e Desenvolvimento do Ensino Fundamental e de Valorização do Magistério (port.)

GDP Gross Domestic Product

GIS Geographic Information System

GIZ German Federal Enterprise for International Cooperation (engl.); Deutsche Gesellschaft für Internationale Zusammenarbeit (deut.)

GMM Generalized Method of Moments

IBAMA Brazilian Institute of the Environment and Renewable Natural Resources (engl.); Instituto Brasileiro do Meio Ambiente e dos Recursos Naturais Renováveis (port.)

IBGE Brazilian Institute of Geography and Statistics (engl.); Instituto Brasileiro de Geografia e Estatística (port.)

ICDP Integrated Conservation and Development Program

IEG Independent Evaluation Group of the World Bank

IIPF International Institute of Public Finance

IMAZON Institute of Man and Environment of Amazonia (engl.); Instituto do Homem e Meio Ambiente da Amazônia (port.)

INCRA National Institute for Colonization and Agrarian Reform (engl.); Instituto Nacional de Colonização e Reforma Agrária (port.)

INDC Intended Nationally Determined Contributions

INPE National Institute for Space Research (engl.); Instituto Nacional de Pesquisas Espaciais (port.)

IOB Operations Evaluation Department (engl.); Inspectie Ontwikkelingssamenwerking en Beleidsevaluatie (dutch)

IPAM Amazon Environmental Research Institute (engl.); Instituto de Pesquisa Ambiental da Amazônia (port.)

IPBES Intergovernmental Science-Policy Platform on Biodiversity and Ecosystem Services

IPCA National Price Index of the Broad Consumer (engl.); Indice Nacional de Preços ao Consumidor Amplo (port.)

IPCC Intergovernmental Panel on Climate Change

IPEA Institute of Applied Economic Research (engl.); Instituto de Pesquisa Econômica Aplicada (port.)

IT Indigenous Territory

IV Instrumental Variable

J-PAL Abdul Latif Jameel Poverty Action Lab

JEL Journal of Economic Literature

MATT Mechanism Average Treatment Effect on the Treated

MD Mahalanobis Distance

MMA Brazilian Environmental Ministry (engl.); Ministério do Meio Ambiente (port.)

PMV Green Municipalities Program (engl.); Programa Municípios Verdes (port.)

NATT Net Average Treatment Effect on the Treated

NGO Non-Governmental Organization

ODI Overseas Development Institute

OLS Ordinary Least Squares

PA Protected area

PAM Municipal Agricultural Production (engl.); Produção Agrícola Municipal (port.)

PES Payment for Environmental Services

PEVS Production of Vegetal Extraction and Forestry (engl.); Produção da Extração Vegetal e da Silvicultura (port.)

PLOS Public Library of Science

PPCDAm Plan for Prevention and Control of Deforestation in the Legal Amazon (engl.); Plano de Prevenção e Controle do Cesmatamento na Amazônia Legal (port.)

PPG7 Pilot Program to Conserve the Brazilian Rain Forest (engl.); Programa Piloto para a Proteção das Florestas Tropicais do Brasil (port.)

PRODES Satellite Monitoring System of the Brazilian Amazon Forest (engl.); Monitoramento Sistemático do Desflorestamento da Amazônia por Satélite (port.)

PRONAF National Program for the Strengthening of Family Farming (engl.); Programa Nacional de Fortalecimento da Agricultura Familiar (port.)

PS Propensity Score

R Brazilian *Real*

RDD Regression Discontinuety Design

RDS Sustainable Development Reserve (engl.); Reserva de Desenvolvimento Sustentável (port.)

RE Random Effects

REDD Reduced Emissions for Deforestation and Forest Degradation

RESEX Extractive Reserve (engl.); Reserva Extrativista (port.)

RQ Research Question

SD Standard Deviation

SDG Sustainable Development Goal

SEMA Special Secretariat for the Environment and Sustainablity (engl.); Secretaria de Estado de Meio Ambiente e Sustentabilidade (port.)

SIPAM Protection System of Amazonia (engl.); Sistema de Proteção da Amazônia (port.)

SLAPR Environmental Licensing System for Rural Properties (engl.); Sistema de Licenciamento Ambiental em Propriedades Rurais (port.)

SNUC National System of Conservation Units (engl.); Sistema Nacional de Unidades de Conservación (port.)

SQL Structured Query Language

STN National Treasury Secretariat (engl.); Secretaria do Tesouro Nacional (port.)

TCU Tribunal of Accounts (engl.); Tribunal de Contas da União (port.)

TEEB The Economics of Ecosystems and Biodiversity

TSE Tribunal Superior Eleitoral (engl.); High Court of Elections (port.)

UFMG Federal University of Minas Gerais (engl.); Universidade Federal de Minas Gerais (port.)

UN United Nations

UNFCC United Nations Framework Convention on Climate Change

URL Uniform Resource Locator

US United States

ABSTRACT

Between 2000 and 2012, 2.3m sq. kilometers of tree cover, equivalent to 6.4 times the size of Germany, were lost globally due to land cover change. Forest loss often comes with significant social and environmental costs. Deforestation contributes 12% - 30% of global greenhouse gas emissions, reduces biodiversity and threatens traditional livelihoods.

Forest conservation policies have shown mixed results worldwide. The success of instruments depends on both their policy design and the context to which they are applied. Even well designed policies can fail to avoid deforestation, when the bio-physical, socio-economic, and the political context are overlooked. This thesis investigates the role of the political context as a potential inhibitor or facilitator of forest conservation in Brazil.

Brazil is an ideal case to study the political economy of forest conservation. During the 2000s the country introduced a substantial forest conservation reform and deforestation rates subsequently fell by 80%. The realized policy-mix included different disincentive and incentive components. Effective measures include the expansion of protected areas, the increase in field-based environmental law enforcement, fines and credit restrictions to environmental offenders.

To understand the role of the political sector, this thesis analyzes three Brazilian forest conservation policies that address the political context and the environmental governance at the local level: An anti-corruption policy targeting districts' administrational responsibilities, though leaving environmental performance uncontrolled; a naming and shaming policy targeting districts with high deforestation rates; and an incentive-based payments for environmental services program combined with forest friendly investments to residents in protected areas.

Impacts and the mechanism through which these policies effect environmental outcomes are analyzed with a combination of spatial data processing techniques and quasi-experimental methods. High resolution satellite data is used to construct yearly outcomes on forest losses, degradation, and fires. Spatial matching and panel data estimations allow to control for selection biases and potential leakage effects.

The analysis of the anti-corruption policy reveals a robust relation between corruption and deforestation, though no effect from publishing the corruption findings. A very high reduction in deforestation rates is caused by the naming and shaming policy. This effect can be explained by an reputational risk effect that caused stakeholders to form conservation alliances. The payment for environmental services program had no sizable effects on forest cover. The missing effects can best be explained with the imperfect policy designs at hand. Whereas the high conservation impact of the naming and shaming

policy stands as an example of how to shape political contexts towards better environmental governance.

Given a reasonably well functioning institutions and enforcement system as in Brazil during the study period, complementary contextualized policies can remove potential inhibitors to conservation and motivate actors to create new conservation incentives. In addition, immediate effects are best achieved when targeting regions with high deforestation pressures, if evasive behavior of targeted actors is monitored at the same time.

ZUSAMMENFASSUNG

Im Zeitraum von 2000 bis 2012 wurde der weltweite Waldbestand um 2,3m Quadratkilometer aufgrund von Landnutzungsänderungen dezimiert, dies entspricht 6,4 Mal der Größe Deutschlands. Waldverlust geht oft mit signifikanten und Umweltkosten und sozialen Kosten einher. Abholzung trägt zu den globalen Treibhausgasemissionen mit 12% - 30% bei, reduziert Biodiversität und bedroht traditionelle Existenzgrundlagen.

Waldschutzpolitiken haben weltweit sehr unterschiedliche Wirkungen gezeigt. Ihr Erfolg hängt sowohl von ihrer Politikgestaltung, als auch von dem Kontext, in welchem sie eingesetzt werden, ab. Selbst gut gestaltete Politiken können das Ziel Abholzung zu verhindern verfehlen, wenn der bio-physische, sozio-ökonomische und politische Kontext nicht beachtet wird. Diese Dissertation untersucht die Rolle des politischen Kontextes als potenzieller Hemmer oder Verstärker von Waldschutz in Brasilien.

Brasilien ist eine ideale Fallstudie, um die politische Ökonomie des Waldschutzes zu untersuchen. In den 2000er Jahren führte Brasilien eine maßgebliche Waldschutzreform durch, woraufhin die Abholzungsraten um 80% sanken. Das neue Politikportfolio beinhaltet verschiedene Komponenten aus negativen und positiven Anreizen. Effektive Maßnahmen umfassen eine die Expansion der Naturschutzgebiete, Verstärkung der umweltrechtlichen Strafverfolgung, Geldstrafen und Kreditrestriktionen für Umweltsünder.

Um die Rolle des politischen Sektors zu verstehen, untersucht diese Dissertation drei brasilianische Waldschutzpolitiken, die den politischen Kontext und die Umwelt auf lokaler Ebene adressieren: Eine Anti-Korruptionspolitik, die die administrative Verantwortung der Distrikte anvisiert, jedoch Umweltperformance unkontrolliert lässt; eine Politik des Anprangerns, die auf Distrikte mit hohen Abholzungsraten abzielt; und ein Anreizprogramm mit Zahlungen für Umweltdienstleistungen in Kombination mit umweltfreundlichen Investitionen für Bewohner von Waldschutzgebieten.

Die Auswirkungen und Mechanismen, durch die diese Politiken Umweltergebnisse beeinflussen, werden anhand einer Kombination von räumlichen Datenverarbeitungstechniken und quasi-experimentellen Methoden analysiert. Hochauflösende Satellitendaten werden genutzt, um jährliche Daten für Waldverlust, -degradation, und -feuer zu konstruieren. Mit Hilfe von räumlichen, statistischen Zuordnungsverfahren (*matching*) und Paneldaten- Schätzungen werden Stichprobenverzerrung (*selection biases*) und potenzielle Ausweicheffekte (*leakage effects*) kontrolliert.

Die Analyse der Anti-Korruptionspolitik zeigt eine robuste Beziehung zwischen Korruption und Abholzung, jedoch keinen Effekt durch die Veröffentlichung der Korruptionsergebnisse. Eine sehr hohe Reduzierung der Abholzungsrate wird durch die Politik des Anprangerns erzielt. Diese kann, unter an-

derem, anhand eines Reputationsverlustes erklärt werden, der dazu führt, dass Interessengruppen Waldschutzallianzen gründen. Das Programm von Zahlungen für Umweltdienstleistungen hat kaum Auswirkungen auf den Waldschutz. Die fehlenden Effekte können am besten auf das zugrundeliegende, unvollkommene Politikdesign zurückgeführt werden. Wohingegen der hohe Schutzeffekt der Politik des Anprangerns als Beispiel dient für die Entwicklung einer besseren Umweltsteuerung durch den zugrundeliegenden politischen Kontext.

Bei hinreichend funktionierenden Institutionen und Strafverfolgung, wie es in Brasilien im untersuchtem Zeitraum der Fall war, können ergänzende, kontextualisierte Politiken potenzielle Hemmnisse im Waldschutz beseitigen und Akteure motivieren neue Schutzanreize zu kreieren. Des Weiteren werden unmittelbare Effekte größtmöglich erzielt, wenn auf Regionen mit hohem Abholzungsdruck fokussiert wird und gleichzeitig ausweichendes Verhalten kontrolliert wird.

ACHIEVEMENTS

The research to chapter 2 was presented on several occasions, including: the "Ausschuss für Entwicklungsländer" 2012 Conference (Bonn), the Annual Conference on Biodiversity and Economics for Conservation (Cambridge) 2013, and the Annual Congress of the International Institute of Public Finance (IIPF) 2013. The study was rewarded with the BMZ/GIZ Public Policy Award at the IIPF 2013 conference.[1] Owing to an update and revision of outcome and treatment indicators, estimation results and interpretations changed significantly: Cisneros, Hargrave, and Kis-Katos (2016b). A first version of chapter 3 was presented at the the XXIX International Conference of Agricultural Economists 2015,[2] at which it was honored with the T.W. Schultz Award for the best contributed paper presented by a young professional. The final version of this research was published after a peer-review process: Cisneros, Zhou, and Börner (2015a). Research to chapter 4 was presented at the 2016 Conference of the International Society for Ecological Economics (Washington): Cisneros, Börner, Pagiola, and Wunder (2016a).

PUBLICATION

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WORKING PAPERS

Elías Cisneros, Jorge Hargrave, and Krisztina Kis-Katos. Environmental effects of anti-corruption strategies. June 2016.

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1 See also Cisneros, Hargrave, and Kis-Katos (2013)
2 See also Cisneros, Zhou, and Börner (2015b)

Part I

CONSERVATION POLICIES AND THE AMAZON

1

INTRODUCTION AND MOTIVATION

Between 2000 and 2012, 2.3m sq. kilometers of tree cover, equivalent to 6.4 times the size of Germany, were lost globally due to land cover change (Hansen et al., 2013). Forest loss often comes with significant social and environmental costs. Worldwide forests provide livelihood to a least 250m people and the remaining 25m sq. km of tropical forests host more than half of worldwide species (Byron and Arnold, 1999; Hecht and Cockburn, 2010; Saatchi et al., 2011). Moreover, deforestation contributes 12% - 30% to global greenhouse gas emissions (IPCC, 2007, 2013; van der Werf et al., 2009). These and other negative externalities of the economic activities associated with deforestation are seldom fully taken into account by land users and political decision-makers.

The largest continuous tropical forest (2.89m sq. km or two-thirds the size of the European Union) is located in Brazil.[1] The country has undergone a major paradigm shift in the early 2000s, launching an effective coordination of forest conservation policy instruments (Maia, Hargrave, Gómez, and Röper, 2011; MMA, 2013; Miccolis et al., 2014). Consequently, deforestation dropped by 80% from 27,000 sq. km to 5,000 sq. km per year. The country's conservation policy mix is highly diverse and its effectiveness varies depending on both policy design and local economic and political contexts. Empirically disentangling the effects of multiple policy measures implemented in locally heterogeneous settings, requires a sub-national perspective and the integration of spatial analysis tools with econometrics (Anselin, 2001).

This thesis investigates the effectiveness of conservation policies in the Brazilian Legal Amazon and the mechanisms through which these policies may affect environmental governance outcomes. The introductory chapter briefly discusses the costs and benefits of deforestation and international mitigation policies and efforts. Subsequently, the underlying conceptual framework as well as the main research questions of this thesis are discussed. It follows a comprehensive description of the study area, the Brazilian Legal Amazon, and an assessment of Brazil's economic history, institutional setting and policies geared towards reducing deforestation.

1 Based on own calculation using the remaining forest cover in 2012 and the area of the 28 European Union member states (including Great Britain)

1.1 BACKGROUND

The 5th assessment report of the Intergovernmental Panel on Climate Change (IPCC) confirmed the anthropogenic influence on global warming, with a 0.85 °C increase in average temperature since the pre-industrial levels and a projected increment of 1.8 °C to 4.0 °C until the end of our century (IPCC, 2007). Whereas fossil fuel combustion is the major source of greenhouse gas emissions, deforestation holds the second largest share with 20% to 30% (IPCC, 2013; van der Werf et al., 2009). A decline in fresh water availability, accelerated species extinction, extreme weather events, and reduced crop yields already started to negatively affect the poor and most vulnerable (IPCC, 2014). Dealing with the impacts of climate change on food security, health, and political stability is considered a major societal challenge in the coming decades (UN, 2015).

From an economic perspective, the benefits from mitigating climate change outweigh the cost of action by a multitude: Stern (2006) calculates a global reduction in annual economic growth by 5%, whereas the cutting of greenhouse gas emissions to reduce climate impacts would only cost 1% of global GDP. The TEEB (2010) report estimates a total avoided damage of 3.7 Trillion US dollars of lost ecosystem services when deforestation rates are reduced by half until 2030.[2]

Furthermore, deforestation involves an array of uncertain or unknown costs. For example, because of changed evaporation patterns in deforested landscapes, regional climate could experience significant shifts in drought and rainfall intensity and timing (Malhi et al., 2008). In addition, forest conversion endangers millions of (unknown) species. Of all known plant and animal species, 50% are found in tropical forests, but an estimated 90% of species still remain unknown (Myers, 1988; Wilson, 2003). Forests thus represent an important future source for the development of new agricultural products, pharmaceuticals, biomedical treatments and bio-technology applications based on the discovery of new species and biological compounds (Balandrin, Klocke, Wurtele, and Bollinger, 1985; Cragg, Newman, and Snader, 1997; Mares, 1986; Wilson, 2003). The continuous expansion of forest frontiers also threatens the livelihoods of native populations. Displacement and cultural confrontation entails the destruction of societies from which humanity can learn different value systems, psychologies, ways of communication and forms of conflict resolutions (Everett, 2009, pp.275-279).

International commitments to environmental protection are increasingly transformed into policy incentives at international and national levels. In December 2015, at the Paris conference of the United Nations Framework Convention on Climate Change (UNFCC), 195 countries ratified to a binding agreement to take efforts to limit global warming to below 1.5 °C (Bodansky, 2016).[3]

2 Damage value calculated as net present value.

3 A complete fulfillment of the "intended nationally determined contributions" (INDC) at the Conference of the Parties in Paris (COP21) is estimated to avoid temperature increases above

Moreover, the Sustainable Development Goals (SDGs) adopted in 2015 recognize the link between economic growth, poverty alleviation and environmental conservation. Monitoring forest cover and the share of sustainably managed forests via satellite data are two concrete, easily measurable, and comparable indicators agreed upon in the SDG framework (Leadership Council of the Sustainable Development Solutions Network, 2015). Forests also stand to benefit from new global science-policy platforms, such as the Intergovernmental Science-Policy Platform on Biodiversity and Ecosystem Services (IPBES). IPBES will assess the worldwide state of biodiversity and its influences on human well-being by integrating multi-disciplinary knowledge systems across all tiers (including indigenous knowledge). Its conceptual framework emphasizes the role of spatially distributed knowledge on biodiversity and the importance of understanding the causality of interventions to promote science-based policy advice (Díaz et al., 2015). The role of conserving forests for climate change mitigation was institutionalized at the international level in 2008 with the UN Collaborative Program on Reduced Emissions for Deforestation and Forest Degradation (UN-REDD). International funding today amounts to 272m US dollars, supporting national programs and projects in 64 countries (UN-REDD, 2016a,b).

Policymakers and advisers are increasingly reacting to the scientific pledge to evidence based policy making (Sanderson, 2002; Savedoff, Levine, and Birdsall, 2006).[4] Evidence based policy making has a long tradition in medical research where experimental trials are intensively used since the 1970ies (cf. Cochrane, 1972). Experimental evidence and science based policy making is prominently promoted by organizations such as the Abdul Latif Jameel Poverty Action Lab (J-PAL), the Overseas Development Institute (ODI) the International Initiative for Impact Evaluation (3ie) or the Center for Global Development (CGD). In consequence, governments are founding own evaluation units committed to causal analysis.[5] and to make public data accessible.[6] The

2.6 - 3.1 °C only (Rogelj, den Elzen, Höhne, Fransen, Fekete, Winkler, Schaeffer, Sha, Riahi, and Meinshausen, 2016). Nonetheless, according to The Washington Post (2016), because of Donald Trump's recent (November 2016) election as President of the United States, these reductions could fall through. Under the Obama administration the US committed to a 26-28% cut of CO_2 emissions by 2025, which accords to 20% of globally intended reductions.

4 The European commission committed since 2003 to an impact assessment of all its policies (Lee and Kirkpatrick, 2006). In the United States a Commission on evidence based policy making initiated in 2016 with the goal to advise the the government and increase data accessibility to the public for evaluations (`https://www.whitehouse.gov/omb/management/commission_evidence`). Germany

5 To only mention a few: The German Ministry of Cooperation an Development created the German Institute for Development Evaluation (DEval) in 2012. In the Netherlands, the Ministry of Foreign Affairs runs the Operations Evaluation Department (IOB). The KfW Group (Reconstruction Credit Institute) founded an independent evaluation department in 2000. The World Bank leadership is advised by its Independent Evaluation Group (IEG).

6 Brazil has a long tradition in open data policy and since its launch of the National Infrastructure of Open Data (INDA) in 2011, it publishes most data online (see `http://www.portaldatransparencia.gov.br/`) (Neves, 2013). In comparison, Germany does not have an open data policy and public data are largely unavailable to the public (Boockmann, Buch, and Schnitzer, 2014). Data from governmental offices often remain closed, though remotely

scope of randomized control trials to determine causal relations in environmental research is limited (Baylis et al., 2016). Only after recent advances in empirical evaluation methods (e.g., spatial matching) environmental scientists started to command more rigorous evaluations (cf. Baylis et al., 2016; Ferraro, 2009; Ferraro and Hanauer, 2014a; Miteva et al., 2012; Pullin and Knight, 2001). The thesis uses and advances up to date empirical evaluation techniques combining spatial panel data with quasi-experimental methods. Advantages and limitations are thereby discussed for each evaluation.[7]

1.2 FRAMEWORK

To prevent the loss of environmental services, endangered environments have to be governed by society. Approaches to environmental governance have evolved over time including a paradigm change that led to the recognition of the role of local actors in shaping governance outcomes. More recently also intermediate value chain actors, such as retailers, and consumers are increasingly held responsible for negative external effects of agricultural production. Lemos and Agrawal (2006) describe environmental governance as *"the set of regulatory processes, mechanisms and organizations through which political actors influence environmental actions and outcomes."* I.e., environmental governance comprises all public policies that aim to improve the environmental behavior of any actor, be it farmers, businesses, non-governmental organizations (NGOs) or political administrations. Public policies therefore comprise traditional policy instruments like taxes, subsidies, and regulations, but also the promotion of public-private partnerships between state agencies and market actors (e.g., logging concessions with state monitoring) and private-social partnerships between market actors and communities (e.g., ecotourism) (Lemos and Agrawal, 2006).

The new forms of intervention focus on actors within a given context. Thereby they follow scientific evidence showing that the effectiveness of environmental policies depends on the bio-physical, socio-economic and political context (Lambin et al., 2014).[8] The mix of different policy instruments can increase

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sensed data from satellite images created under governmental research institutes is rapidly increasing and available to the public, boosting research (Wulder et al., 2012). New machine learning techniques combined with satellite and spatial data are now commencing to fill the gap of missing official information (cf. Blumenstock, Cadamuro, and On, 2015; Jean et al., 2016; Miteva, Pattanayak, and Ferraro, 2012).

7 Chapter 2 uses a natural experiment over time with yearly data at the district level, generated by official statistics and remotely sensed data. Chapter 3 advances the mechanism analysis presented by Ferraro and Hanauer (2014b); Flores and Flores-Lagunes (2009); Imai, Keele, Tingley, and Yamamoto (2011) for cross-section data, and applies the empirical model to a matched subset of spatial panel data. Chapter 3 applies matching with spatial data. A map with several layers of spatial information are combined and sliced by a 5 to 5 km grid. Irregular vector units are thereby left within the dataset to avoid data losses at the margin of administrational boundaries.

8 Ferraro, Hanauer, and Sims (2011) detect differential impacts of protected areas on deforestation and poverty outcomes dependent on baseline poverty levels, slope and distance to major cities.

intervention effectiveness when the created incentives are complementary and state monitoring and enforcement institutions are in place (Lambin et al., 2014; Ribot, Agrawal, and Larson, 2006). Although the scientific literature has recognized the influence of context on policy effectiveness, little attention is devoted to policies designed to incorporate context conditions and shift the context itself.[9]

Given context heterogeneity with a variety in characteristics and actors, policy interventions can have widely different outcomes. Focusing on tropical forest conservation in Brazil, the success of policies depends on characteristics of soil quality for agriculture, demography and labor scarcity, education, agricultural technologies, infrastructure development, etc. and on actors forming social, economic and political elites. Many of these potential determinants of policy outcomes are also underlying or proximate drivers of deforestation. Knowledge about their relative role in shaping policy impacts can help decision-makers to design better policy mixes, adaptive to context. Contextualization is therefore an additional policy design element alongside siting (spatial targeting), targeting, monitoring and enforcement (cf. Howlett, 2005, pp. 40). Following that line of though we can categorize public policies in terms of their "degree of contextualization", i.e., the role context plays in policy design:

- P1 Policies without apprehension of context characteristics or actors.
- P2 Policies focused on context characteristics.
- P3 Policies aimed at actors within given contexts to reduce antagonistic behavior.
- P4 Policies stimulating actors within given contexts to create own interventions.

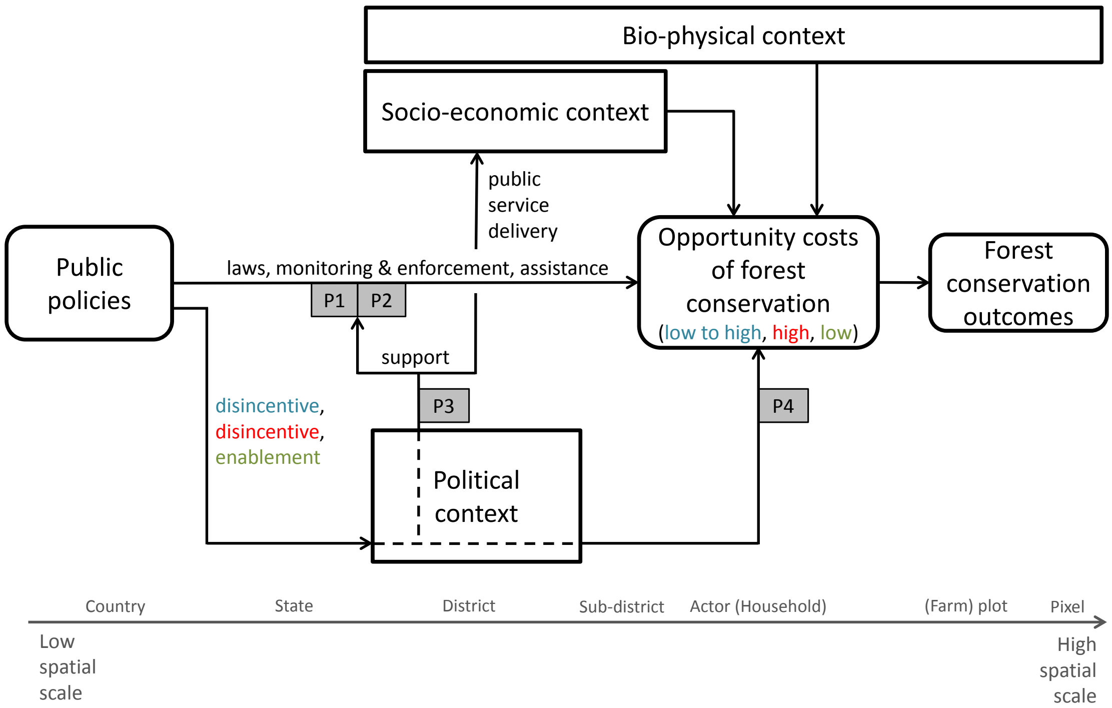
Figure 1.1 relates forest conservation outcomes to their bio-physical, socio-economic, and political context. The framework shows the option to directly target deforestation actors (P1, P2), and their opportunity costs of forest conservation. Further, it highlights the complementary approach to target political stakeholders and change the political context (P3, P4). All elements are embedded within a spatial scale, starting with the country as the highest level and decreasing through state, district, household, farm level to the smallest data level - a pixel from a satellite picture. [10]

At farm level, forest conservation is determined by the profitability of alternative land uses. Forest are converted to agricultural land, pasture for cattle, or mining sites to generate income. Investments in forest friendly land uses (e.g., non-timber forest products, agro-forestry, etc.) are often judged less attractive by farmers in forest environments. Reasons for the profitability gap of

9 Engel (2015) highlights the role of design options for Payments for Environmental Service (PES) programs to specific context conditions.

10 Analyzing federal and state policies in this thesis, we focus on the political context at the district and sub-district level, though the framework allows analyses at various other scales.

Figure 1.1: Public policies for forest conservation



forest friendly activities range from the low cost of forest conversion and cattle farming, to economics of scale, insecure property rights, risk aversion, labor availability, etc. and vary by country and region. The opportunity costs of conservation, depends on the bio-physical, socio-economic, and political context including the implemented public policy mix.

Soil quality, climate, forest resources, land inclination, etc. are primary factors determining the costs and profitability of land uses. Market structures, production technologies, population size, infrastructure, and credit access determine actors' decisions to convert forest. Household characteristics, from education and ethnicity to marital status, influence land use decisions at plot level. [11]

The political context plays a crucial role for the delivery of public services and therefore the local socio-economic context. Investments in public infrastructure like roads, public transport, education, health systems, etc. are often carried out by local administrations with varying degrees of success. Bad governance can waste resources and corrupt officials may capture federal funds, both leading to an under provision of public services (Bardhan, 2006, 1997). The lack of public services and basic infrastructure tends to hinder the de-

11 It is often difficult to predict each factors' influence on deforestation decisions. E.g., roads facilitate new clearings but at the same time decrease the probability of land abandonment and conversion to pasture for cattle ranching (Ludewigs, de Oliveira D'antona, Brondízio, and Hetrick, 2009). See also Angelsen (1999) for a detailed description of impacts of technological improvements in different contexts.

velopment and maintenance of forest friendly production systems (Ludewigs et al., 2009).

Following our framework above, governments engaging in forest protection have four design options at hand with respect to the context. The first category of policies (P1) includes all policies that are designed or carried out without differentiation to a context. Command and control instruments, laws, regulations or taxes and subsides are often unspecific to context characteristics or actor composition. E.g., actors' opportunity costs of forest conservation are directly affected through monitoring and enforcing environmental laws. Sanctions and fines decrease the profits of forest harming activities and increase the opportunity of forest friendly investments. Besides such disincentive policies, public policies do engage in incentive policies (Börner and Vosti, 2013). E.g., conditional credit support and technical assistance to forest friendly production reduce the opportunity cost of conservation.

The second category (P2) comprises policies and instruments that are targeted at a specific context or take the local context into account. Enforcement actions focused on deforestation hotspots are context specific. The transition between the first and the second category is continuous. Laws, differentiating between women and men, small and large land holders, cattle ranger and soy producer, tropical forest and savanna forest, etc. are context specific. A policy will also be region specific if areas have a predominant characteristic, e.g., areas with predominantly large cattle rangers.

The third category (P3) shifts its focus from characteristics to actors themselves. These policies perceive the context as a kind of "resistor" between instruments and the deforesting actor. Political, social and economic actors can inhibit or facilitate the working of public policies. E.g., district administrations can choose to support with less or greater effort federal programs for health care, education, environmental regulation, etc. Public policies can in consequence be designed to enable or incentivize administrations to stop inhibiting the service delivery and support (environmental) law enforcement. A tangible example is the policing of corrupt politicians that capture funds for school meals.[12]

Within the forth category (P4), policies recognize that stakeholders within given contexts, i.e., politicians, firms, land-holders, NGOs, etc. themselves can engage in environmental governance. Public policies can incentivize or enable stakeholders to be part of a forest conservation agenda, engage in policy design and secure public outcomes (health, education, environment, etc.) Type P4 policies are designed to incentivize the political actors to participate in the conservation process. In consequence local actors can create new partnerships and interventions adapted to the given context. E.g., to reward local administrations for protected area management, support private-social partnerships between environmental service providers and buyers and public-private partnerships (e.g., logging concessions, technical assistance, environmental labeling) (Lemos and Agrawal, 2006; Wunder, 2005).

12 Cf. Reinikka and Svensson (2004).

1.3 RESEARCH QUESTIONS

Policymakers are increasingly under pressure to deliver effective policies and have numerous instruments with multitude of design options at hand. A sustainable portfolio of forest conservation policies will likely have to include both actor specific interventions and policies directed at local contexts (see previous section). Relatively little empirical research has so far addressed the latter. Hence, this thesis asks the following question:

RQ How do public policies shape the outcomes of local forest governance?

Actor specific policies implemented by federal or state governments are usually intended to influence local environmental governance outcomes through incentives, disincentives or enabling measures (Börner and Vosti, 2013). This thesis analyzes two innovative disincentive-based strategies in chapters 2, 3 and a combination of enabling and incentive-based policy instruments in chapter 4. All three public policies are reflected within the theoretical framework on forest conservation instruments of the previous section and are depicted in Figure 1.1.

Corruption auditing falls under the public policy category P3. Federal governments inspect the governance quality of local administrations and publicly scrutinize the waste of public funds, the failure of public service delivery, and corrupt activities. Inspecting local administrations can improve the overall local governance. An improved public service delivery can increase the viability of forest benign economic activity especially when poor forest-dependent people gain access to such services. The disclosure of governance quality enables the electorate to punish a corrupt administration and thereby weaken ties between local political elites and illegally operating timber companies and farmers. And yet, the opposite effect can materialize, too. Increased public awareness of governance failures in the health sector, for example, may produce spillover effects to other sectors. For instance, corrupt politicians could respond to the increased scrutiny by shifting rent seeking behavior to the agricultural and forest sector. Targeting the overall governance quality rather than directly focusing on environmental governance can result in detrimental or favorable forest conservation outcomes. The corresponding research question addressed in chapter 2 is:

RQ1 Do anti-corruption measures affect forest conservation outcomes?

Public disclosure of poor local environmental governance is an alternative strategy akin to type P3 and P4 policy approaches. If only a few major actors are causing environmental damage, naming and shaming the "worst" performers can make a difference. Disclosure policies have been successfully used in different contexts to improve public service delivery, fight corruption, and control pollution (Blackman, 2010; Jacobs and Anechiarico, 1992; McGee and Gavent, 2010; Reinikka and Svensson, 2011; Tan, 2014; Tietenberg, 1998). In many of these cases, disclosure has induced government, civil society, and individuals to collaborate in selectively downscaling support or targeting punitive action to the identified key actors. Few examples and little research has evaluated public disclosure as a quality control mechanism for decentralized environmental governance. A corresponding research question of chapter 3 is:

RQ2 Does naming and shaming reduce deforestation? And, if yes, which mechanisms are at work?

Finally, many potential options exist to *enable local environmental governance*. There is broad agreement that successful environmental governance requires the involvement of local actors and an appropriate combination of rules and incentives (Agrawal and Angelsen, 2009; Agrawal and Gibson, 1999). Protected areas that allow particular local resource use strategies are one of the most common approaches to forest conservation in the tropics. Often, however, both reserve managers and the local population need additional support to effectively govern the protected territory (Brandon and Wells, 1992; Bruner, Gullison, Rice, and da Fonseca, 2001). Such support measures fall under our policy category P4. Incentive programs for residents of protected areas, for example, have been proposed as means to increase the quality of environmental governance and sustainably secure forest resources (Ferraro and Kiss, 2002).[13] This thesis evaluates the additionality of such incentive-based reserve management support, through the following research question:

RQ3 Do conservation incentives in protected areas reduce deforestation?

1.4 STUDY AREA

Brazil is an ideal laboratory to study the performance of forest conservation policies. After decades of support for infrastructure expansion in the region, leaving 20% of the Amazon deforested,[14] public policy underwent a major paradigm shift towards improved forest governance during the early 2000s (Maia et al., 2011; Miccolis et al., 2014). Allegedly as a result of this paradigm shift, deforestation rates dropped by 80% since 2004, making Brazil the country with the largest reduction in forest loss worldwide (Hansen et al., 2013; INPE, 2012).[15] In this respect the 'Brazilian approach to forest conservation' is a historical phenomenon and a highly relevant research topic. It follows a brief description of the historical context of environmental policy development in Brazil. Subsequently, the three analyzed policies are described. The section ends with an outlook on future forest conservation challenges in Brazil.

13 Wunder (2005; 2007) highlights the limit of payments for environmental services for residents of protected areas.

14 The Brazilian territory is segmented into five biomes: Amazonia, Caatinga, Cerrado, Pampas, Pantanal, Mata Atlantica. The Cerrado is commonly classified as a tropical savanna ecoregion.

15 Neighboring Latin American countries increased deforestation rates by 459 sq. km per year. Indonesia increased deforestation by 1,021 sq. km per year, reaching 20,000 sq. km of cleared forest in 2012 (Hansen et al., 2013).

1.4.1 *Brazil's conservation history*

To understand land and forest use dynamics in Brazil it helps to briefly look at its economic and political history of the second half of the 20th century. The industrialization process of the agricultural sector started to accelerate during the 1950s. Millions of laid-off rural workers migrated to the major cities during this time. In the 1960s, the increasing industrial demand for raw material and agricultural produce favored the continuous mechanization of the agricultural sector. Agricultural expansion pulled migration to the central west and north of the country, initiating an irreversible conversion of large parts of the Cerrado biome into an agricultural landscape.[16] During the military government of the 1970s and 1980s, large scale migration projects were implemented to colonize the Amazon (Miccolis et al., 2014). Hundreds of thousands of mostly poor landless farmers were offered 'uninhabited' and uncultivated land.[17] The expansion of the agricultural frontier was strongly promoted in the Amazon by full exemptions from income, import and export taxes and subsidized credits. To connect the frontier with the southern markets, public infrastructure investments expanded road and energy networks. Given a very difficult environment and the fact that property rights could best be secured by clear cutting the forest, extensive cattle ranching became the prominent form of land use. In addition, land properties agglomerated in failed settlement projects after settlers had relocated to new agricultural frontiers (Araujo et al., 2009; Fearnside, 2001; Pacheco, 2009). The agro-industry today earns 28% of the country's GDP, produces 35% more than the country consumes and exports $76 billion of its agricultural produce. Brazil produces 31% of the world's soybeans and 28% of the world's beef. It is the largest beef, sugar, coffee, tobacco, orange juice and broiler chicken producer (Miccolis et al., 2014). The economic and political processes resulted in 738,200 sq. km of cleared forest, 20% of the Brazilian Amazon - twice the size of Germany.

After the military dictatorship, the democratization process led in 1988 to the "New Citizens' Constitution", which allowed for political participation and the decentralization of power. The development of inclusive institutions resulted in a rapid economic development of the country (Acemoglu and Robinson, 2012). The new constitution acknowledges the right to a healthy environment and the rights of indigenous people.[18]

The basis of Brazil's forest protection is the Forest Code. Established in 1965, it underwent many reforms and became de facto law in 2001 (Soares-Filho

16 Today 50% of the Cerrado is deforested (Garcia, Ferreira, and Leite, 2011)

17 Pacheco (2009) reports 161,562 settled families in the BLA between 1964 and 1994 through colonization programs.

18 Constitution of the Federative Republic of Brazil of 1988, Art. 255: *port.*: "Todos têm direito ao meio ambiente ecologicamente equilibrado, bem de uso comum do povo e essencial à sadia qualidade de vida, impondo-se ao poder público e à coletividade o dever de defendê-lo e preservá-lo para as presentes e futuras gerações.", *engl.*: "Everyone has the right to an ecologically balanced environment, which is a public good for the people's use and is essential for a healthy life. The Government and the community have a duty to defend and to preserve the environment for present and future generations."

et al., 2014). This law regulates how much is allowed to be deforested on private properties in each of the Brazilian biomes. In the Amazon biome private properties are only allowed to deforest 20% of the area. Nonetheless, enforcement of the Forest Code before the mid-2000s was limited owing to the lack of appropriate monitoring tools, insufficient funding of the environmental police, and low political support (Fearnside, 2001, 2003; Miccolis et al., 2014). The paradigm shift towards forest conservation began in 2004 after the launch of an inter-ministerial coordination program, the Plan for Prevention and Control of Deforestation in the Legal Amazon (PPCDAm), by the new workers' party led government (cf. Figure 1.2). Among other measures the PPCDAm heavily relied on the continued expansion of protected areas and indigenous territories, now covering 54% of the Brazilian Amazon. The launch of a near real-time monitoring system via satellite (DETER)[19] in 2004 and the strengthened funding and power of the environmental police (IBAMA)[20] enabled environmental authorities to more effectively follow up on illegal forest uses and deforestation (Börner, Kis-Katos, Hargrave, and König, 2015a). Since 2008, environmental offenders face restricted public credit access (Assunção, Gandour, Rocha, and Rocha, 2013b; Nepstad et al., 2014). Furthermore, Brazil has supported public-private agreements between soy retailers and producers to restrict the commercialization of produce from deforested areas (Arima, Barreto, Araújo, and Soares-Filho, 2014; Nepstad et al., 2014).

1.4.2 *Researched policies*

Since the early 2000s, and in line with its decentralization process, the federal government increasingly implemented policies that target local governance at the district level. We chose an anti-corruption policy, a blacklisting policy and a payment for environmental services program (PES) to investigate the three policy options stated in the previous section: *Corruption auditing, public disclosure, enabling environmental governance*

Corruption auditing. In 2003, Brazil started a policy experiment to inspect and improve local governance, which serves to address research question RQ1. The untargeted anti-corruption program aims to reduce fraud and capture of public funds at the district level. Random audits and investigations of the districts' fiscal discipline related to federal government funds are carried out to improve the local governance. The random selection process results in a representative sample of districts, for example, in terms of governance quality, corruption, and the opportunity costs of forest conservation (cf. Figure 1.1). The publication of audit results, in principle, allows the electorate and the federal government to punish local leaders for corrupt behavior.

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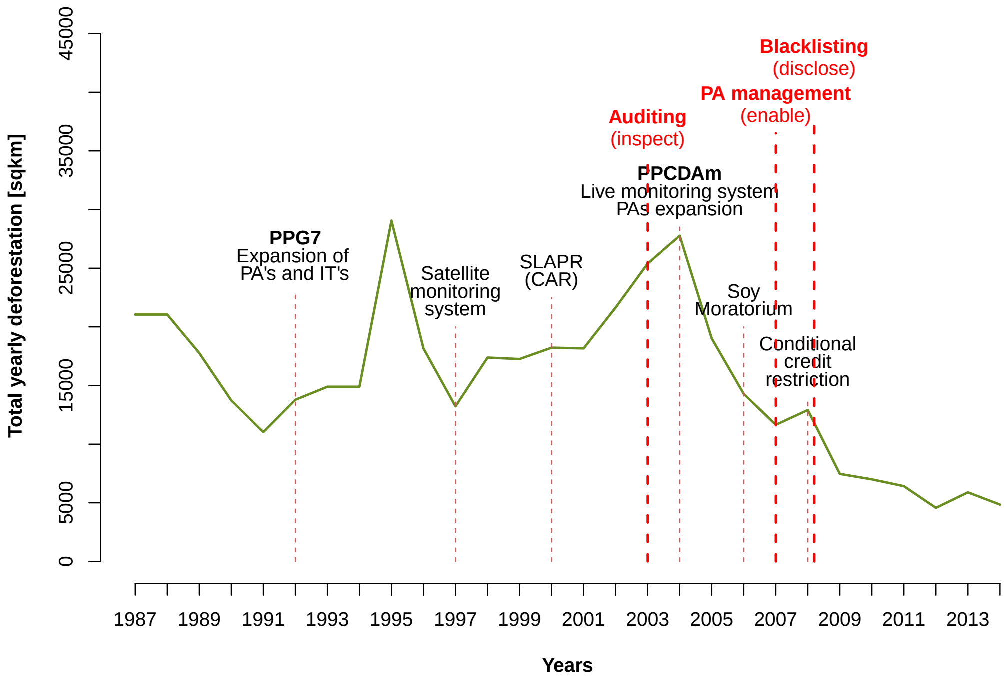
19 DETER, *engl.*: Near Real-time deforestation detection; *port.*: Detecção do Desmatamento na Amazônia Legal em Tempo Real

20 IBAMA, *engl.*: Brazilian Institute of Environment and Renewable Natural Resources; *port.*: Instituto Brasileiro do Meio Ambiente e dos Recursos Naturais Renováveis

Figure 1.2: Brazil's conservation policies



*N*ote: PPG7, Pilot Program to Conserve the Brazilian Rain Forest; SLAPR, Environmental Licensing System for Rural Properties, started in the state of Mato Grosso. In 2008, the CAR, Rural Environmental Cadaster, registration system incorporated the SLAPR system and extended to the state of Pará (although responsibilities remained at the state level). In 2012 a nation wide CAR system was launched (cf. Azevedo, 2009; Azevedo et al., 2014); PCDAM, Plan for prevention and control of deforestation in the Legal Amazon.

Public disclosure. In 2008 Brazil launched an innovative public disclosure policy, which serves as a case to address research question RQ2 in chapter 3. Out of 771 districts in the Brazilian Legal Amazon, a small sample of 50 districts with historically high deforestation rates were publicly 'blacklisted'. Hence, the policy predominantly targets districts with low environmental governance quality and high opportunity costs of forest conservation. As a result, the environmental police (IBAMA) can focus its enforcement activities on these districts. Before, environmentalist non-governmental organizations (NGOs) engaged across the whole Brazilian Amazon, now they can focus on the 'priority' districts. Actors of these districts may have concerns about economic consequences and create public-private partnerships to reduce deforestation rates and improve their environmental governance. Some state-governments, such as the state of Pará, even started programs granting support to districts that managed to be removed from the blacklist (Governo do Para).

Enabling environmental governance. In the course of Brazil's protected area expansion, the Secretariat for Sustainable Development of the Amazonas state launched a payment for environmental services program called *Bolsa Floresta* to *enable* more effective local forest governance. Today the program is led by the Sustainable Amazonas Fund (FAS), a foundation, and covers 15 multiple-use reserves. In anticipation of future agricultural frontier expansion, the program targeted primarily remote reserves with low historical deforestation rates. To offset the opportunity cost of forest conservation, sustain livelihoods in the long-run, and build environmental alliances against invasions, FAS provides conditional payments to over 8,000 households and invests in forest benign production at the community level. By law, reserve dwellers may only deforest for subsistence production, however, monitoring and law enforcement capacity is limited in Brazil's largest federal state. Therefore, the intervention qualifies as an alternative strategy to enable the environmental governance of reserve managers and communities in a context of relatively low opportunity costs.

1.4.3 *Current conservation challenges*

Since 2012, forest clearing leveled around an annual 4,900 sq. km. It is Brazil's declared ambition to curb deforestation rates even further.[21] At the current level of annual forest loss the share of small-scale, supposedly smallholder-based, deforestation is much higher (Godar, Gardner, Tizado, and Pacheco, 2014), but it is hypothesized that large holders now deforest in small patches, at a scale below the detection range of the real-time DETER monitoring system. In response, the last phase of the PPCDAm (2012-2015) focuses on an incentive rather than disincentive strategy. Brazil now aims to reform the disorganized property system, the environmental regularization system, and to promote sustainable smallholder agriculture (Ministério do Meio Ambiente, 2013). Since 2012, land holders must register their land in a geo-referenced rural environmental cadaster (CAR) as a precondition for (1) receiving environmental licenses, (2) exemption from obsolete environmental fines, and (3) access to agricultural credit after 2017. The CAR constitutes the basis for Brazil's new and future forest conservation strategy. It creates 'spatial' liability for land holders and facilitates the monitoring of environmental compliance. The reform has significant potential to improve forest governance, but only if enforcement remains effective will new investments into agricultural productivity result in 'land-sparing' rather than 'land-sharing', and curb deforestation rates (Angelsen and Kaimowitz, 2001).

Despite the above-mentioned environmental policy success, the new governance setup is continuously challenged. A first concession to agricultural interest groups was the revision of the Forest Code in 2012. The reform exempts 90% of land holders to pay environmental fines and restore areas deforested before 2008 (Soares-Filho et al., 2014). Economic interest in remote sparsely populated areas for hydropower plants and mining operations is mounting. Pledges to the National Congress to degazette and downsize reserves have reduced the protected areas by 44,100 sq. km since 2008, and are threatening an additional 21,000 sq. km (Bernard, Penna, and Araújo, 2014; Ferreira et al.,

21 The commitment made at the 15th Convention of the Parties of United Nations Framework Convention on Climate Change in 2009, requires Brazil to reduce deforestation to an annual rate below 3,925 sq. km until 2020 (Ministério do Meio Ambiente, 2013).

2014). Political pressures against forest conservation increase with increasing opportunity costs. Since the worldwide economic slowdown, lower demands for beef and soy have kept world market prices low and decreased the demand for new land. An increase in world demand for food, feed, fuel, and fibre may quickly change the picture. In this context, the recently released deforestation data of INPE shows a 47% increase in 2016 with respect to the 2012-2015 average. Nonetheless, this level is still relatively low (55% of the historical mean 1988-2003) (cf. INPE, 2016; Nature News, Jeff Tollefson, 2016).[22] If this represents a reversing development due to the political changes remains to be seen.

1.5 ORGANIZATION OF THE THESIS

This thesis is organized in five chapters to address the proposed research questions. Following this introduction (chapter 1) describing background, framework, research questions and study area, the first research question on the environmental effects of anti-corruption strategies is addressed in chapter 2. How naming and shaming policies can lead to forest conservation is addressed in chapter 3. Chapter 4 answers the third research question on how conservation incentives can reduce deforestation. Chapter 5 summarizes the main findings of the thesis and presents policy conclusions.

22 Deforested areas in years 2012-2016 were: 4,571, 5,891, 5,012, 6,207, and 7,989 sq. km, respectively.

Part II

SHIFTING LOCAL FOREST GOVERNANCE

2

ENVIRONMENTAL EFFECTS OF ANTI-CORRUPTION STRATEGIES

ABSTRACT

This chapter highlights the relationship of the recent federal anti-corruption strategy in Brazil to the environment. We rely on the unique policy experiment of fully randomized public fiscal audits to asses the overall level of governance quality at the district level, to relate corruption to deforestation and to identify causal effects of publicly revealed audit reports on deforestation trends. Public audits aimed at the implementation and usage of funds designated for governmental programs but do not target environmental responsibilities. Despite being unrelated to environmental issues our governance measures capture governance quality and corrupt behavior of local administrations. We find that districts identified as highly corrupt are associated with 25% higher forest losses between 2002 and 2012. Auditing and publicly reporting corrupt activities had no influence on subsequent deforestation, not on average neither when considering reelection incentives and learning processes from neighbors and media and judiciary presence. Environmental benefits are cannot be addressed by reducing overall corruption levels. Illegal deforestation remains to be better addressed by targeted policies.

JEL Classification: D73, O13, Q23
Keywords: Deforestation, corruption, fiscal audits, Brazil, Amazonas

2.1 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 districts undergoing public fiscal audits.[1]

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, potentially leading among others to increased deforestation. 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). Whereas it is widely documented that deforestation in Brazil is strongly affected by economic incentives (Angelsen, 1999; Pfaff, 1999; Hargrave and Kis-Katos, 2013) as well as conflicting regulatory frameworks (Alston, Libecap, and Mueller, 2000; Fearnside, 2001; Ludewigs et al., 2009), the effects of local governance on the deforestation process remain widely unexplored.[2]

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 districts. 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

1 Districts or *municípios* in Portuguese are the smallest administration unit in Brazil, comparable to counties in the US.

2 The study of Burgess et al. on the effects of decentralization on illegal logging in Indonesia is a major exception, relating deforestation to the proliferation of newly formed local governments, fighting for forest resources.

political outcomes in subsequent mayoral elections (Ferraz and Finan, 2008).[3] We use this publicly available information to construct proxies for the overall governance quality and corruption at the district 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.

Our work is closely related to several strands of empirical literature. Similarly to Burgess et al. (2012), 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, 2013). In contrast to these studies, we address an outcome, deforestation, which is not directly monitored by the public audits, and hence examine a shift in rent-extraction towards not audited sectors. 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 analysis also relates to 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.

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 2012 in 237 audited districts to the extent of corruption documented by the auditors. This descriptive analysis shows that districts with about one standard deviation higher measured corruption levels experienced up to 25% higher deforestation between 2002 and 2012, and 83% higher deforestation in years where the audit revealed the misuse of public funds.

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 no increase in deforestation. Investigating time dynamics shows no delayed impacts over time. Because of the fully randomized

3 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 and teaching supplies Ferraz, Finan, and Moreira (2012), the role of judicial presence on the overall regulatory quality (Litschig and Zamboni, 2008), the effects of exogenous shifts in budget size on local corruption (Brollo, Nannicini, Perotti, and Tabellini, 2013), or the effects of later changes in the audit risk on corruption and waste (Litschig and Zamboni, 2013).

study design, these results can be interpreted as evidence of no causal relation between the Brazilian anti-corruption measurement and deforestation as an non-targeted outcome.

In the last part of the study we investigate various mechanism that could explain the zero average effect on deforestation by disentangling it into heterogeneous auditing findings and political contexts. 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 a learning process and an increase in deforestation. If believes are updated we expect auditing to increase deforestation where auditors have detected many irregularities. Similar we expect audits on neighboring districts to easier transmit information about the auditing procedure. Results show no differential in deforestation trends after audits with worse governance findings. Audits on neighboring districts have no impact on deforestation nor could we explain the insignificant average auditing effect with opposed spatial spillovers effects to neighboring districts.

We further investigate the role of the disciplining effects of electoral accountability. Mayors, who serve their first term and hence can stand for reelection, have stronger incentives to improve detected instances of mismanagement and curb corrupt activities known to the public. Second term mayors may face lower public scrutiny and no incentives to shift rents from corrupt fiscal activities towards areas of illegal logging. Ferraz and Finan document less corrupt violations under first term mayors, nonetheless we do not find a statistically significant relationship between increases in deforestation and first term mayors. Moreover the presence of judicial seats or local radio stations have show no robust effects.

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 2.3 describes our data and section 2.4 the descriptive evidence on the correlation between corruption and deforestation. Section 2.5 investigates the causal effects of public fiscal audits on deforestation, whereas section 2.6 addresses the potential mechanisms driving these results. Section 2.7 concludes.

2.2 PUBLIC AUDITS AND CORRUPTION FINDINGS

2.2.1 *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 districts 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 districts and write an extensive audit report, which is then published online on the CGU's homepage.[4] 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, 34 lotteries were carried out in the years between 2003 and 2012, resulting in 1876 audit reports. The first 8 lotteries selected 50 districts out of all Brazilian communities with less than 300,000 inhabitants. Their scope was extended by the ninth lottery to districts with up to 500,000 inhabitants; starting with the tenth lottery 60 districts with less than 500,000 inhabitants have been selected.[5]

After a lottery has been carried out by the Office of the Comptroller General, 10 to 15 auditors are send to the districts (Ferraz and Finan, 2008).[6] 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 districts with 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 with market prices. Moreover, they interview a random sample of community households in order to reveal instances of nepotism or fraud (Ferraz and Finan, 2008). The CGU hands over the audit reports to the Tribunal of Accounts (TCU), to public prosecutors, the district 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 anal-

4 The reports are available under `http://www.cgu.gov.br/assuntos/auditoria-e-fiscalizacao/avaliacao-de-programas-de-governo/programa-de-fiscalizacao-em-entes-federativos/sorteios-publicos` (Accessed on 2016-12-02).

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

6 The Office of the Comptroller General, Controladoria-Geral da União (CGU) in Portuguese, was created by the President Fernando Henrique Cardoso in 2003 for public control, correction, prevention and fight against corruption. Formally under the responsibility of the president it was transferred in 2016 to the ministry of transparency, inspection and control.

ysis 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. Whereas some of the listed irregularities 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.g. by Ferraz and Finan, 2008, 2011, and Litschig and Zamboni, 2008, 2013), which basically counts corruption related (or total) irregularities in the reports in some form of other.[7] 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 district. A further issue of concern is whether corrupt local governments can interfere 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 district only once. They usually work in relatively large groups (about 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 improbable (Litschig and Zamboni, 2013). The auditors implement a detailed and fairly constant procedure, controlling the use of all federal funds in a district. They compare the fiscal accounts with 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 district.

The random selection of districts via lotteries results in a random subsample of all districts; in our specific case this covers 237 not yet completely deforested districts in the Brazilian Amazon, out of the total of 556.[8] Eligibility criteria of

7 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.

8 Out of the 771 districts within the Brazilian Legal Amazon, districts with less than 5% of its area covered with forest in 2002 are excluded from the analysis.

Table 2.1: Summary statistics on audit reports

	Mean	St.dev.	Min.	Max.
Government funds [m *reais*]	17.62	71.61	0.36	1170.57
No. of ministries	9.04	3.04	2.00	17.00
Pages	89.21	42.27	11.00	264.00
No. of programs	23.14	9.20	4.00	58.00
No. corrupt violations	4.94	6.78	0.00	65.00
No. irregularities	60.09	28.60	7.00	160.00
Relative No. irregularities	3.03	1.87	0.24	10.21

*N*ote: Statistics refer to N=227 audit reports for 237 districts in the Brazilian Legal Amazon, audited before 2012.

the public audits (e.g., having a population below 500,000 or state capitals) further reduces the sample to 550 districts. 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.

2.2.2 *Constructing measures for local corruption*

In an average district in our Amazonas sample, public fiscal audits control the use of about 17.8m *reais*, which is disbursed via ca. 9 different ministries (cf. Table 2.1). These are very substantial amounts; overall, federal funding accounts for about 45% of district finances.[9] Hence, records of the use of these funds can be expected to reflect very well the governance quality of a district.

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 district 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

9 Number refer to 728 out of 771 districts of the Brazilian Legal Amazon. Calculations base on the accounts of 2013 obtained from the Secretary of National Treasury (Secretaria do Tesouro Nacional - STN) and its FINBRA (Finances of Brazil) database. 16% of district finances originate from state administrations. See also: `https://siconfi.tesouro.gov.br/siconfi/pages/public/consulta_finbra/finbra_list.jsf`.

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 2.1), we normalize this measure by dividing it through the total number of programs investigated by the auditors. As Table 2.1 shows, an average audit report records about 60 administrative irregularities, which results in 3 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.[10]

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 district. It is generated by using the Global Regular Expressions Print (grep) in **R** to search for 40 different *regular expressions* within an audit report that inevitably indicate corrupt incidences (R Core Team, 2015).[11] 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 A.2.

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 illegal transfers to a provider or construction 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 expressions in the same paragraph. This could lead to an over-counting

10 Alternatively to the relative number of irregularities, we also coded the share of programs investigated that have at least one irregularity, which have been favored by Litschig and Zamboni and Ferraz and Finan. This second irregularities variable is somewhat more prone to measurement and encoding error due to 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.

11 Grep originated as 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/manpages/precise/en/man1/egrep.1.html`).

Table 2.2: Auditing procedures over time

Year	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012
No. audited districts	32	62	38	47	17	18	16	47	0	5
Median audit duration [days]	5	5	5	12	51	49.5	46	47	-	5
Median report length [pages]	69	66.5	82.5	77	83	80.5	85.5	93	-	74
Median No. listed programs	30.5	28	29	22	18	16.5	17.5	17	-	19
Median No. irregularities	1.4	1.4	2.2	2.7	3.6	5.0	4.9	4.6	-	3.7
Median No. corrupt expressions	1	1	5.5	2	4	2.5	5.5	4	-	3

*N*ote: Statistics refer to N=227 audit reports for 237 districts in the Brazilian Legal Amazon. Years refer to the August-July cycle, with no audits between August 2010 and July 2011.

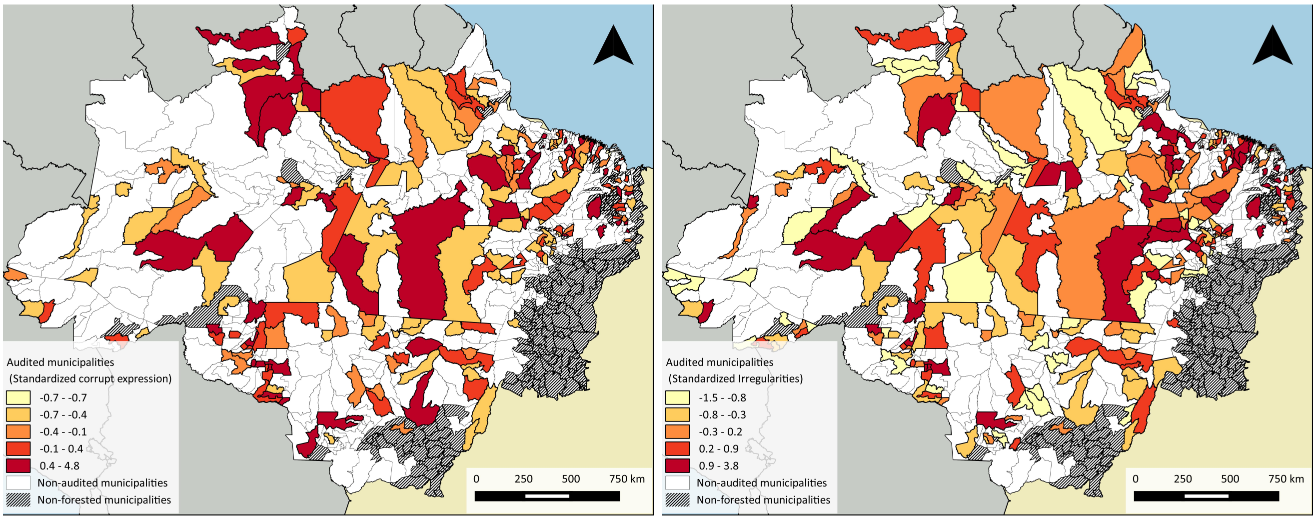
of expressions and an upward bias in the corruption measurement. We do not have any reason to suspect these stylistic differences however to be systematic, and we are convinced that the writing style of the auditors is fairly comparable within specific years. After analyzing in detail the audit reports, 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.[12]

Table 2.2 documents the changes in the number of irregularities and the text based corruption measure over time, which show 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 2005 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 districts that were audited more than once (36 out of 237), we take 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. Figure 2.1 shows the spatial distribution of our treatment units as well as the distribution of our irregularities and corruption measures.

12 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.

Figure 2.1: Audits and generated governance measures



Note: Maps are based governance measurements constructed with audit reports of the Brazilian Office of the Comptroller General (CGU). Colored categories refer to the quintiles of the standardized measurements.

2.2.3 *General hypotheses*

2.2.3.1 *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 (Pfaff et al., 2014; Pfaff, Robalino, Herrera, and Sandoval, 2015; Soares-Filho et al., 2010). 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 sizable decrease in deforestation rates during this period, most effective in areas of large-scale forest clearings (Börner et al., 2015a; Hargrave and Kis-Katos, 2013). The accompanying legal coercions (e.g., confiscations, conditional credit and market access) convert the policy into a highly cost-effective instrument, although a surprisingly small fraction of the environmental fines is actually paid (Börner et al., 2014).

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 (Cabral and Gomes, 2013; Fearnside, 2001; Godar et al., 2014). 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 (Neves, 2012; Silva, 2009). 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 district 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; Neves, 2012). 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 15 cu. meter 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 (Amacher, Ollikainen, and Koskela, 2012). 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 (Neves, 2012). 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 (Cabral and Gomes, 2013). Corruption on the district 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, whereas 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 farm abandonment, re-concentration and deforestation of land (Fearnside, 2001; da S. Martins and da S. Pereira, 2012; Schneider, 1993).

From a longer-term perspective, the failure to implement and maintain a functioning public infrastructure is also a major channel through which a corrupt district 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, that 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 (Barreto and Silva, 2010; Fearnside, 2003; Neves, 2012). 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).

2.2.3.2 *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 district 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 district 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 districts 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 concern 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 (2008) show that the physical presence of the judiciary, in form of the seat of the judiciary district being within the district, makes public prosecution of cases more probable 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 district. 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 districts 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. (2012), 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 districts 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 federal auditors scrutinize, they will adjust their behavior once learning about the audit modalities. This learning can take place when a district is audited but potentially also when neighboring districts undergo an audit process.

2.3 DATA

Our main sample includes all districts in the Brazilian Legal Amazon that had more than 5% 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, 2008a).[13] From 1988 to 2002 deforestation rates were calculated based on visual interpretation of satellite imagery. Starting with 2002, imagery interpretation has become partially automatic. PRODES measures annual rates of forest clearing with a resolution of 30 meters. Using 228 scenes of Landsat and CBERS satellites.[14] PRODES spatial data on deforestation polygons is released on the internet for public use. Annual rates for districts are publicly available. Owing to technological advances and the incorporation of new satellite data into the system the calculation methodology changed over time. We therefore opt to use the spatial data available and aggregate own rates of yearly newly deforested district area in square kilometers.[15] 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.[16] After excluding six further districts (state capitals and/or districts with population above 500,000) that were exempt from the public audit program entirely, we end up with a balanced panel of 550 districts over eleven years (from 2002 to 2012).[17]

Figure 2.2 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 237 sample districts, as well as with local governance information derived from the audit reports, which constitute our main explanatory variables of interest. Further information on local elections, the mayor's term limits, the presence of local radio stations and radio penetration come from the Institute of Applied Economic Research (IPEA). In order to contain measurement error, we use yearly information on cloud coverage (which affects the observability of deforestation) and the size of district area

13 PRODES project, satellite monitoring system of the Brazilian Amazon Forest, `http://www.obt.inpe.br/prodes/index.php`

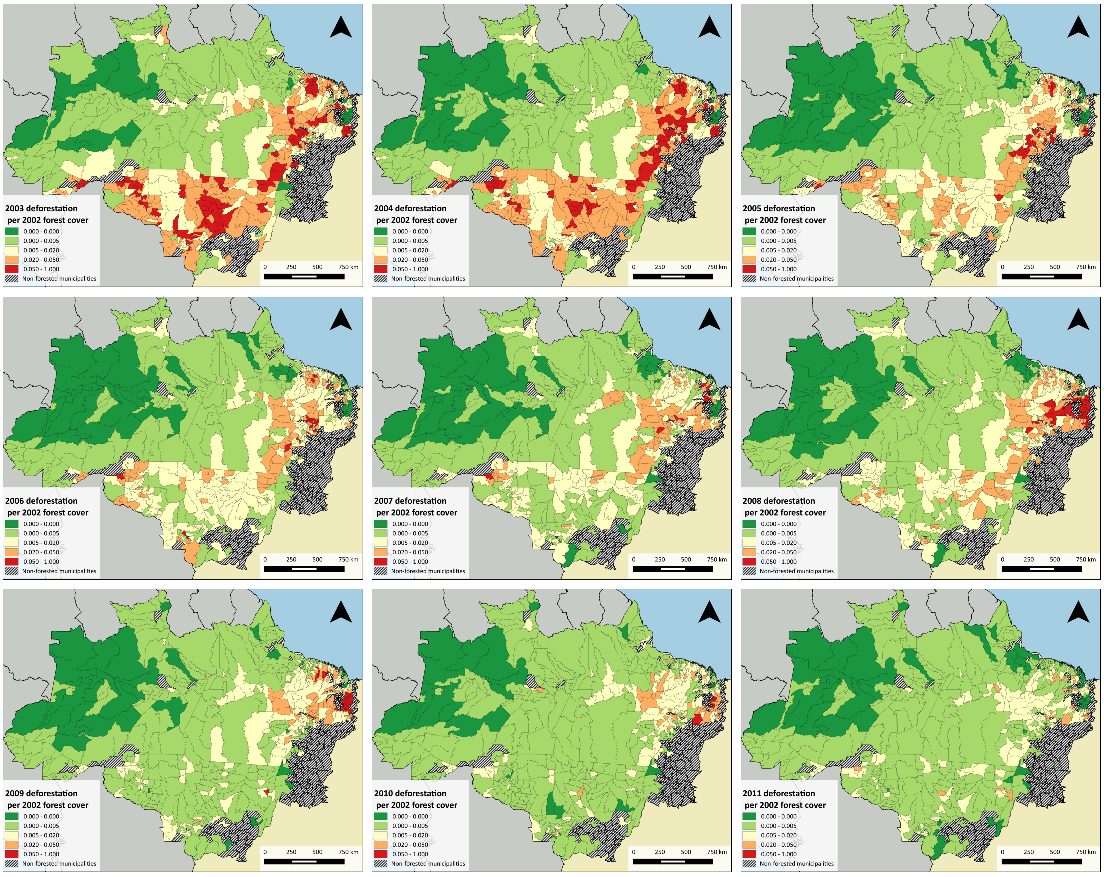
14 The number of pathrows increased over the time period reaching 228 in 2009. Yearly deforestation data are calculated using data of the latest cohort in 2012.

15 Our aggregation method only deviates little from the PRODES method. Main changes are: 1) We do not calculate hypothetical deforestation rates below clouds but choose to control for cloud cover as a measurement error in regressions. 2) We use only the latest land use classification map in 2012, which assures a consistent forest cover base for the beginning of our time frame. A detailed description of the methodology can be wound in Cisneros et al. (2015a), `http://journals.plos.org/plosone/article/asset?unique&id=info:doi/10.1371/journal.pone.0136402.s016` or in section B.1

16 We measure time according to the deforestation years, and hence also adjust panel control variables to the same August-July time window.

17 The six excluded districts are Manaus, Belém, São Luís, Proto Velho, Rio Branco Macapá. Boa Vista, Cuiabá and Palmas were excluded as non-forested districts. Our observations come from nine Brazilian states: Acre, Amapá, Amazonas, Pará, Roraima, Rondônia, Tocantins, Mato Grosso and Maranhão.

Figure 2.2: Yearly deforestation in the Brazilian Amazon districts



Note: Maps are based on yearly deforestation data from INPE/PRODES normalized by the remaining forest cover in 2002. Increasing shares are colored from green to red.

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 2012 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. The population and GDP data are acquired from the Brazilian Institute of Geography and Statistics (IBGE), the latter is measured in *real* per capita terms, deflated by the national consumer price index. The distance to Brasilia is calculated as the beeline to the district capitals.[18] 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 come 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 A.1 and A.2.

18 Calculations with spatial data are conducted on a PostgreSQL 9.2.3 server with the PostGIS 2.0.1 add-on.

2.4 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 district over the time period between 2002 and 2012 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.[19] Our corruption measures are based on audit reports published within this time frame, constructed as described in section 2.2. 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. Whereas the first one depicts general administrative quality in the district, 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 corruption environment in the district. 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 237 sample districts, explaining deforestation over the whole time period by a list of time invariant factors and initial conditions.[20] We include our two corruption proxies as main variables of interest in regressions of the following form:

$$\ln D_i = \mathbf{X}_{i0}\, \boldsymbol{\beta} + \gamma\, C_i + \kappa_s + u_i. \tag{2.1}$$

The dependent variable $\ln D_i$ is the natural logarithm of the total cumulative deforestation over the years 2002 to 2012 in district 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. Differences across states are captured with state dummies κ_s.

Table 2.3 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, whereas 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

19 At least constant during our time frame 2002-2012

20 We exclude 10 districts from the original sample of 237 audited originally forested districts due to incomplete data on some of the socio-economic control variables.

covered by clouds and hence not observable by the satellite project). Columns (2) and (5) add further controls for 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 factors (savanna coverage, distance to Brasilia). The other initial conditions are predetermined (initial forest size in year 2002, population in 2000 and pc. GDP in agriculture in 2002) as they cannot be influenced by future deforestation rates. Savanna share captures the size of district area that was never rainforest and is hence not monitored by the satellite project.[21] 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 district, 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. Protected areas are distinguished between multiple-use reserves, strictly protected reserves and indigenous territories. The designation of protected areas and settlement projects is under state or federal administration control. Nonetheless, we expect these variables to be more closely related to local governance as these policies can be promoted or impeded by local officials. These policies constitute as channels through which local corruption could 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, if effective, should inhibit deforestation.

Standardizing governance measures over all years reveals no significant relation to deforestation level (cf. panel A). Standardizing within each year increases the size of coefficients and measures of corruption as well as irregularities are positively related to deforestation on a 1% to 10% significance level (cf. panel B). Districts with a one standard deviation higher corruption levels from the average experience 25% more deforestation.[22]

All other covariates turn out significant (except indigenous territories) 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 with increasing average shares of cloud coverage over remaining forests (cloud error), but also in areas covered by savanna, which are excluded from the satellite

21 Savanna coverage is denoted not-forest area within the data of the PRODES project. The latter includes also swamp areas but these are comparably few in our region wide analysis.

22 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 districts with very high corruption levels.

Table 2.3: Relationship between total deforestation and corruption

Dependent	ln Total deforestation					
Governance var.		Corruption			Rel. irregularities	
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A	Gov. variables standardized over all reports					
Gov. failures	0.083	0.042	0.069	-0.102	-0.096*	-0.055
	(0.066)	(0.061)	(0.060)	(0.067)	(0.057)	(0.057)
Av. cloud error	-2.109**	-0.776	-0.301	-2.070**	-0.724	-0.310
	(0.833)	(0.909)	(0.788)	(0.847)	(0.905)	(0.791)
ln Ini. forest	0.556***	0.514***	0.532***	0.563***	0.516***	0.536***
	(0.044)	(0.045)	(0.047)	(0.044)	(0.045)	(0.047)
ln Dist. Brasilia		-0.994**	-0.839**		-0.992**	-0.842**
		(0.482)	(0.362)		(0.473)	(0.365)
ln Savanna area		-0.020***	-0.007		-0.020***	-0.007
		(0.007)	(0.008)		(0.007)	(0.007)
ln Ini. pop.		0.331***	0.230***		0.335***	0.241***
		(0.069)	(0.068)		(0.068)	(0.066)
ln Ini. pc. GDP in Agr.		0.244***	0.141***		0.243***	0.148***
		(0.061)	(0.054)		(0.059)	(0.054)
ln Ini. Multiple-use reserve			-0.016**			-0.015**
			(0.007)			(0.007)
ln Ini. Strictly protected reserve			-0.018**			-0.019**
			(0.008)			(0.008)
ln Ini. Indigenous territroy area			-0.003			-0.002
			(0.007)			(0.007)
ln Ini. Settlement area			0.041***			0.039***
			(0.008)			(0.008)
State effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
N	237	237	227	237	227	227
Adj. R-sq	0.577	0.630	0.683	0.579	0.634	0.683
PANEL B	Gov. variables standardized yearly					
Gov. failures	0.221**	0.155**	0.165**	0.207***	0.116*	0.150**
	(0.086)	(0.077)	(0.075)	(0.077)	(0.066)	(0.066)
Av. cloud error	-2.791***	-1.172*	-0.682	-3.082***	-1.333*	-0.884
	(0.643)	(0.680)	(0.657)	(0.642)	(0.691)	(0.659)
State effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Further controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
N	237	237	227	237	227	227
Adj. R-sq	0.312	0.477	0.529	0.309	0.472	0.526

*N*ote: The table reports OLS estimates with the dependent variable being the log of total deforested area over 2002-2012 within the district. 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. *,**,*** denote significance at 10/5/1% level.

observations. The coefficient of the distance to Brasilia, proxying for economic remoteness, shows a large difference between inner areas and the outer deforestation arc. We also also find a non-negligible correlation between the initial size of protected areas (especially multiple-use reserve) and settlement projects and subsequent deforestation, where the former tends to reduce although the latter to foster deforestation.

The above estimations assume that detected corruption across our time frame has a constant component and influences deforestation rates in all years. We test if corruption levels are also related to deforestation in years close to the auditing events. Therefore, we construct sums of deforestation within the audit year plus one or two years (j) before treatment: $S_i^j = \sum_{\tau=1}^{j} D_{t-\tau}$, where t is the audit year of district i. This excludes deforestation occurrences after treatment and tests if the relation holds for years close to treatment only. We adapt the above model (2.1) to include state-time dummies $\kappa_t s$ where t refers to the auditing year of a district, capturing structural differences across states and time. We estimate:

$$\ln S_i^j = \mathbf{X}_{i0}\,\boldsymbol{\beta} + \gamma\,C_i + \kappa_t s + u_i. \tag{2.2}$$

Table 2.4 shows results on the relationship between deforestation and corruption levels in years close to audits. Using the broader governance measure, irregularities, the relation to deforestation is insignificant, regardless of the standardization method (columns 4 to 6). Whereas using the more specific corruption measurement, all specifications show positive and significant relations between the severity findings and deforestation (columns 1 to 3). Standardized over all reports coefficients are significant on a 5% level, whereas standardized within each years coefficients sizes decrease but keep significant on a 5% to 1% level. Forest losses in years close to the audit are 61% to 83% higher in districts with one standard deviation higher corruption findings. Revealing higher coefficients when using as outcome variables only deforestation occurrences close to audits confers the idea that corruption is better related to years before audit than to the full time frame.

The above models depict a highly significant correlation between deforestation and our corruption measurement of local governance, 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 district should be a main driver of deforestation, but it is difficult to control for characteristics of a district economy like the composition or the distribution of wealth and power. We control to some extent for the level of economic activity in a district with initial GDP per person in agriculture. Infrastructure or road density and

Table 2.4: Relationship of near audit deforestation and corruption

Dependent	ln Aggregated deforestation					
Governance var.	Corruption			Rel. irregularities		
Years before audit	t	t, t-1	t, t-1, t-2	t	t, t-1	t t-1 t-2
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A	Gov. variables standardized over all reports					
Governance failures	0.608**	0.598**	0.609**	-0.145	-0.189	-0.151
	(0.308)	(0.303)	(0.310)	(0.258)	(0.247)	(0.271)
State-year effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Further controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
N	*265*	*265*	*235*	*265*	*265*	*235*
Cluster	*227*	*227*	*212*	*227*	*227*	*212*
Adj. R-sq	0.305	0.330	0.315	0.280	0.305	0.289
PANEL B	Gov. variables standardized yearly					
Governance failures	0.537**	0.480*	0.525*	-0.116	-0.142	-0.116
	(0.259)	(0.253)	(0.286)	(0.189)	(0.186)	(0.210)
State-year effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Further controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
N	*265*	*265*	*235*	*265*	*265*	*235*
Cluster	*227*	*227*	*212*	*227*	*227*	*212*
Adj. R-sq	0.302	0.324	0.309	0.281	0.306	0.289

*N*ote: The table reports OLS estimates with the dependent variable being the log of aggregated deforested area over zero to two years before an audit took place within the districts. 36 districts were audited twice or three times hence clustered 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. Additional controls include covariates of specification (3) in Table 2.3. *,**,*** denote significance at 10/5/1% level.

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 (Angelsen, 1999; Pacheco, 2009). 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 lend themselves to a causal interpretation. Nonetheless, they show some evidence for a relationship between deforestation and the auditors' corruption findings in a district.

2.5 PUBLIC AUDIT EFFECTS ON DEFORESTATION

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 local 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. Reports contain some information on the beginning and end of the auditing process, as well as a date of the publication of the report. On average, auditing starts 15 days after the lottery and takes ca. 24 days. The publishing dates of the reports often contradict with the time frame of the auditing process. Without any concrete information about the publishing procedure we rely on the official dates of the lotteries as the beginning of the auditing effect. A differential effect between the audit and the publication of the reports can not be estimated because of the small time lag between these events. The underlying panel data model can be written in the following form:

$$\ln D_{it} = \gamma_1 A_{it} + \mathbf{X}'_{it}\,\boldsymbol{\beta} + \kappa_{st} + \alpha_i + \epsilon_{it} \qquad (2.3)$$

where the dependent variable $\ln D_{it}$ stands for the natural logarithm of the newly deforested area in district i in year t, and A_{it} denotes the audit treatment.[23] The vector $\mathbf{X}_{it}$ includes the proxy for measurement error: the yearly share of forest area covered by clouds. Cloud coverage affects the yearly precision of the observations on deforestation. The implied measurement errors can be substantial — on average 15% of the forest area is unobservable because of clouds — and the inclusion of this variable can be expected to increase the precision of the estimate.

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 districts within the same state. All time

23 The auditing dummy A_{it} turns one with the beginning of the subsequent year (August to July period) in which the lottery took place.

invariant locally idiosyncratic factors affecting deforestation are captured by the district fixed effects α_i.

The centralized fiscal audit and the subsequent publishing of the audit report in a given district constitutes our treatment indicator A_{it}. In case of repeated audits (33 districts in our sample have been audited twice and four district even three times), the treatment variable measures the number of audits that have been carried out until the given year. Lotteries and audits were conducted throughout the year and we cannot expect an audit to take effect on the accumulated deforestation of the full year. We therefore set the treatment indicator between zero and one according to the share of the year it could still have taken an influence on deforestation. We specify this treatment effect in 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 district finances leads to a diversion of corrupt activities toward less observed sectors, deforestation will increase, yielding a positive $\hat{\gamma}_1$.

We estimate Equation 2.3 in a first difference form, eliminating the district fixed effects α_i through a difference specification.[24] The average effect of the audit treatment is captured by estimating the following model:

$$\Delta \ln D_{it} = \gamma_1 \, \Delta A_{it} + \Delta \mathbf{X}_{it}' \, \boldsymbol{\beta} + \kappa_{st} + \upsilon_{it}, \tag{2.4}$$

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

The first difference estimates of the average public fiscal audit effects are presented in Table 2.5. Column (1) shows the results from regressing first differences of deforestation on the treatment indicator only. The treatment coefficient is insignificant and close to zero. The measurement error proxy (clouds), included as a further control starting with column (2), turns out as highly significant on a 1% level. Its negative coefficient is close to one and rightly indicates that with an increase of 100% in the area covered by clouds detected deforestation falls to zero. Column (3) adds year fixed effects that capture common shocks, whereas 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 allowing for variations within years and states the size of the treatment coefficient increases but stays statistically insignificant. The two additional columns (5, 6) control for lottery round and district specific trends by adding $\lambda_i \cdot t$ or $\lambda_l \cdot t$ to Equation 2.3, and hence λ_i or λ_l to Equation 2.4. The first, λ_i, can account for district specific differences in the growth rates

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

Table 2.5: Effects of public fiscal audits on yearly deforestation (FD estimates)

Dependent	Δ ln Deforestation					
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Audit	-0.008	0.000	-0.074	-0.107	-0.162	-0.161
	(0.112)	(0.111)	(0.117)	(0.123)	(0.136)	(0.146)
Δ Cloud error		-0.979***	-0.932***	-1.101***	-1.109***	-1.148***
		(0.167)	(0.172)	(0.181)	(0.182)	(0.200)
Year effects	*No*	*No*	*Yes*	*Yes*	*Yes*	*Yes*
State-year effects	*No*	*No*	*No*	*Yes*	*Yes*	*Yes*
Lottery spec. trends	*No*	*No*	*No*	*No*	*Yes*	*No*
Munic. spec. trends	*No*	*No*	*No*	*No*	*No*	*Yes*
R-sq.	0.000	0.007	0.056	0.139	0.136	0.076

*N*ote: 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 district level, are reported in parentheses. The results refer to $N = 5500$ observations, for 550 originally forested districts. *,**,*** denote significance at the 10/5/1% level.

of deforestation; the second, λ_l captures all potential structural differences between the audit procedures following different lottery rounds. The inclusion of these differential trends increases further the size of audit effects though renders no significance. We also investigate the effects of the audits on deforestation dynamics more explicitly. Repeating the analysis with different outcome measures of forest conservation reveal similar results. Replacing the logarithm of yearly deforestation with logarithms of yearly forest cover, fire incidence detected by satellite images, or public rural credit show similar insignificant results.[25] Moreover using rates of deforestation over district area, over unprotected area or remaining forest area has no influence on the significance of the audit coefficient.

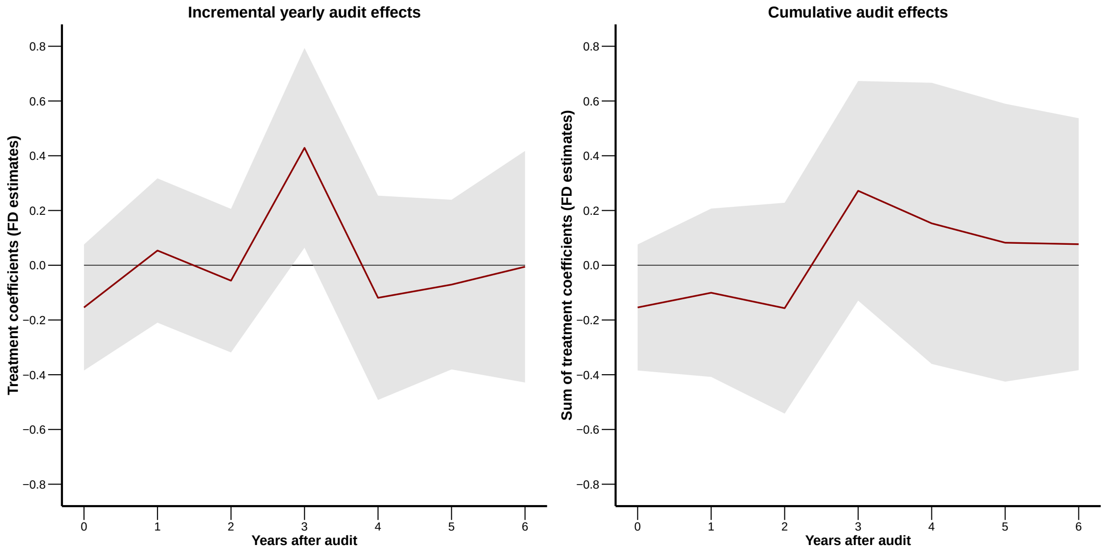
Figure 2.3 investigates the time pattern of the treatment effects more explicitly by decomposing the yearly dynamics of the public audit effects on deforestation. For this purpose, we split the treatment effect into a set of dummy variables that capture how many years (j) have past since the audit:[26]

$$\Delta \ln D_{it} = \sum_{j=0}^{6} \gamma_j \, \Delta A_{ijt} + \Delta \mathbf{X}_{it}' \, \boldsymbol{\beta} + \kappa_{st} + \upsilon_{it}. \tag{2.5}$$

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 districts with multiple audit treatments, the time dummies record the passing of time after

25 Credit access is typically associated with increases in deforestation.
26 With $A_{ijt} = A_{it-j}$ and filling up missing values with zeros before audit start.

Figure 2.3: Yearly dynamics of audit effects on deforestation



Note: The graphs report treatment coefficients and 90% confidence intervals as estimated by equation (2.5). 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.

each audit. The yearly coefficients show that the null effect of the auditing on deforestation evenly distributed over all years after the audit. The sign of the coefficients switch over the years form negative to positive and back to negative. Despite a significantly positive coefficient in third year after auditing, we cannot conclude any influence on deforestation rates. Overall, the cumulative audit effect stays insignificant over all years.[27]

Regressions on potential misspecifiactions of the treatment timing in Table 2.6 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 2.4 (including state-year fixed effects) for re-defined treatment variables that have been shifted by one to four years as compared with the timing of the actual randomized treatment. We see that from the seven reported treatment regressions 7 out of eight that shift the treatment to placebo years turn out to be insignificant.[28]

The above results of the effects of public fiscal audits fail to show a causal impact on deforestation rates. The audit treatment has been fully randomized and covers the full Brazilian Amazon over 10 years. The large heterogeneity over time and space in could diverging effects through potential impact channels of auditing in the next section.

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

28 The positively significant coefficient in specification (8) for a treatment start after years could best be attributed to a random data peculiarity.

Table 2.6: Placebo treatment regressions (FD estimates)

Dependent	Δ ln Deforestation								
Treatment year	$(t-4)$	$(t-3)$	$(t-2)$	$(t-1)$	(t)	$(t+1)$	$(t+2)$	$(t+3)$	$(t+4)$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Δ Audit	-0.002	-0.076	0.061	-0.014	0.125	0.058	0.117	0.287*	0.294
	(0.111)	(0.121)	(0.088)	(0.090)	(0.122)	(0.139)	(0.125)	(0.163)	(0.183)
State-year effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Cloud error	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*

*N*ote: 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 $t-4$ to $t+4$ for all districts. Robust standard errors, clustered at the district level, are reported in parentheses. *,**,*** denote significance at the 10/5/1% level.

2.6 POTENTIAL CHANNELS

Public audits could affect deforestation through several channels. In order to shed light on potential effects hidden within the insignificant average we investigate various interaction effects that can 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 districts. 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 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 term, 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 tests some potential mechanisms at play.

2.6.1 *The role of local corruption and mismanagement*

The effects of public audits on local administrations' behavior 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 unfavorable, local officials can face increasing pressure, both from their political adversaries (Ferraz and Finan, 2008), or from the judiciary (Litschig and Zamboni, 2008). 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 uneasiness 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 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 districts, we re-estimate Equation 2.4 by allowing treatment effects to vary with the corruption findings in the reports C_{it}:

$$\Delta \ln D_{it} = \gamma_1 \, \Delta A_{it} + \delta_1 \, \Delta A_{it} \cdot C_{it} + \Delta \mathbf{X}_{it}' \, \boldsymbol{\beta} + \kappa_{st} + \upsilon_{it}. \tag{2.6}$$

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 districts. 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 2.2), 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 districts within the Legal Amazon. Second, since findings of corruption and management irregularities increased consistently over the years (cf. Table 2.2), 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 districts, 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 districts with above and below median governance quality.

Treatment interactions with the two governance proxies (documented in Table 2.7) turn out overall insignificant. Whereas 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 districts with high and low governance quality. The first four columns show no average effect on districts deforestation. The interactions with the governance variables meant to capture an increasing audit effect with higher corruption or irregularity measures revealed to the public. The overall insignificant coefficients indicate to no effect with decreasing governance quality. Splitting the treatment effect into two in columns (5) and (6), for districts with higher/lower than median corruption findings/irregularities, likewise derive into an insignificant relation although the difference between the two treatment coefficients are sizable.

Table 2.7: 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	0.125	0.124	0.126	0.130		
	(0.122)	(0.114)	(0.122)	(0.123)		
Δ (Audit $\times$ Gov. failures)	-0.007	-0.012	0.032	-0.141		
	(0.060)	(0.105)	(0.095)	(0.088)		
Δ (Audit $\times$					-0.045	-0.031
Good governance (κ_1))					(0.126)	(0.078)
Δ (Audit $\times$					-0.220	-0.209
Bad governance (κ_2))					(0.202)	(0.187)
State-year effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Further controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
R-sq.	0.138	0.138	0.138	0.138	0.138	0.138

*N*ote: The table reports first difference estimates, with the dependent variable being the change in the log of yearly newly deforested area. Corr. abbreviates the corruption measurement, Irreg. represents the relative number of irregularities per program audited. 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 3 corrupt expressions, for irregularities, more than 2.6 irregularities per investigated program. Further controls include first differences in Cloud error. The results refer to $N = 5500$ observations, for 550 districts. Robust standard errors, clustered at the district level, are reported in parentheses. *,**,*** denote significance at the 10/5/1% level.

2.6.2 *Learning and neighborhood effects*

The unexpected anti-corruption measurement could have stronger effects in its initial phase. After audits take place, district 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.

If learning and the updating of prior beliefs play an important role we expect the auditing program to take greater influence in the initial years of the program. Further, the analysis of neighborhood effects can provide useful evidence on this mechanism. Before the district 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 districts 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 districts took place. If during the first years of the program audits lead to a shift in the deforestation trend 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 districts should also shift the the deforestation trend.

We analyze changing audit impact over time by dividing the audit effect into 33 auditing dummies for each lottery. The auditing effect A_{it} in Equation 2.4 is replaced by the interactions with lottery dummies λ_l:

$$\Delta \ln D_{it} = \Delta A_{it}\, \lambda_l\, \boldsymbol{\gamma} + \Delta \mathbf{X}'_{it}\, \boldsymbol{\beta} + \kappa_{st} + \upsilon_{it}. \tag{2.7}$$

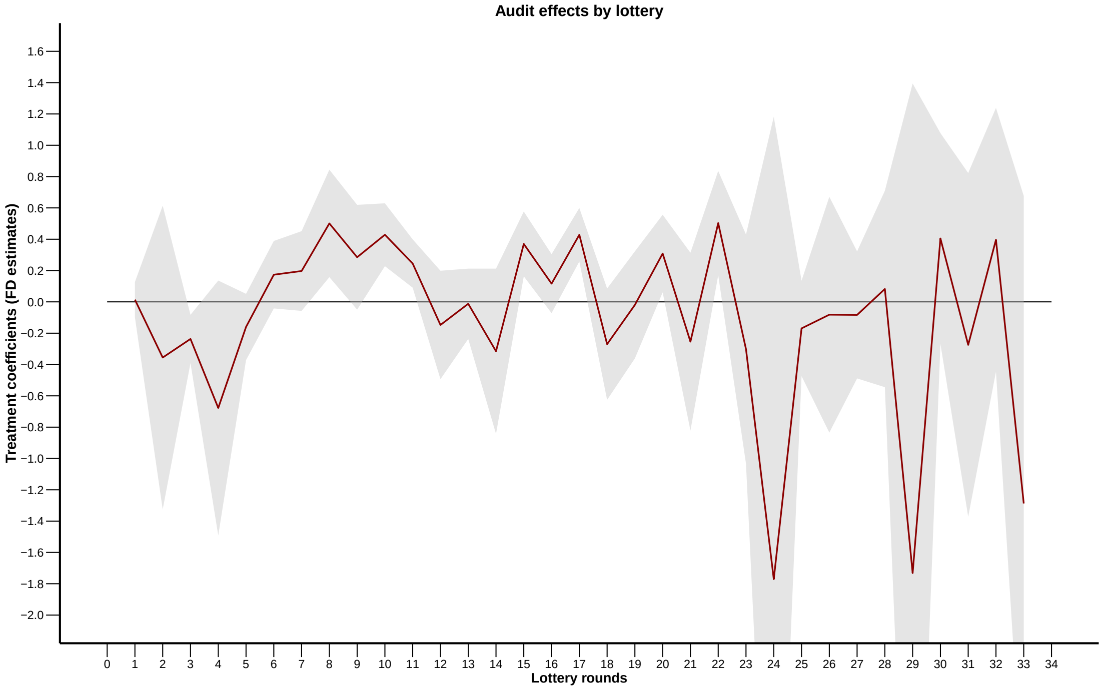
Figure 2.4 graphically depicts the the coefficient vector $\boldsymbol{\gamma}$ of auditing effects across lotteries. The graph shows an somewhat erratic path of effect sizes, switching from positive to negative coefficients. The impacts of the preliminary first two lotteries are insignificant. Coefficients are negative up to lottery 5. Between lottery 6 and 11 coefficients stay positive over a longer period, with a 10% significance in lotteries 8 and 10. Subsequently, signs in lotteries 12 to 33 switch between positive and negative and only reach significance in rounds 15, 17, and 22. Low significance levels arise from the small size of each intervention cohort. Only 7 to 13 districts are audited within the Brazilian Amazon per lottery.[29] Owing to the sporadic significance levels and unclear impact directions we cannot interpret these result as causal. If any learning effect took place it must have had a delayed start with the sixth lottery, which took place in October 2003 half year after the first lottery.[30]

We address learning from neighbors by including the change in neighboring audits, measured by the number of neighboring districts that got newly

29 The first two lotteries only selected 1 and 7 districts our sample, respectively.

30 The sixth lottery lies within the 2004 deforestation year period (August-July) and therefore takes effect from 2005.

Figure 2.4: Audit effects on deforestation by lottery



Note: The graph reports treatment coefficients by lottery round and 90% confidence intervals as estimated by Equation 2.7.

audited in a specific year $\sum A_{-it}$, into the difference Equation 2.4 as a further control:[31]

$$\Delta \ln D_{it} = \gamma_1 \Delta A_{it} + \theta_1 \Delta \sum A_{-it} + \Delta \mathbf{X}'_{it} \beta + \kappa_{st} + \upsilon_{it}. \tag{2.8}$$

Since audits are randomized at the state level, neighboring audits can also be treated as exogenous and estimated in first difference form. We expect learning from own audits as well as learning from neighboring audits to shift deforestation patterns. The coefficients γ_1 and θ_1 should point to the same direction having either both positive or negative signs. However, learning from neighbors should play a smaller role as soon as the district itself gets audited. And learning from own audits should have lower effects when neighbors were

31 For the spatial analysis, we had to exclude one control unit that had no direct neighbor in our sample of forested districts.

previously audited: we incorporate this idea by adding a further interaction between own and neighboring audits:[32]

$$\Delta \ln D_{it} = \gamma_1 \, \Delta A_{it} + \theta_1 \Delta \sum A_{-it} + \mu_1 \Delta A_{it} \cdot \Delta \sum A_{-it} + \Delta \mathbf{X}'_{it} \, \boldsymbol{\beta} + \kappa_{st} + \upsilon_{it}. \quad (2.9)$$

Moreover, neighboring audits can affect deforestation not only through learning from neighbors' experiences, but also by fostering deforestation in neighboring districts. Since deforestation is a spatially diffuse process, neighboring deforestation can have spillover effects also on its own (cf. Hargrave and Kis-Katos, 2013; Robalino and Pfaff, 2012). To additionally control for this channel, we add to Equation 2.9 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:

$$\begin{aligned} \Delta \ln D_{it} = & \gamma_1 \, \Delta A_{it} + \theta_1 \, \Delta \sum A_{-it} + \mu_1 \, \Delta A_{it} \cdot \Delta \sum A_{-it} + \lambda \, \mathbf{W}' \Delta \ln \mathbf{D}_{-it} + \\ & \Delta \mathbf{X}'_{it} \, \boldsymbol{\beta} + \kappa_{st} + \upsilon_{it}. \end{aligned} \quad (2.10)$$

The introduction of the spatially lagged dependent variable as a further explanatory factor raises however endogeneity concerns. This is the reason why we also re-estimate the spatial panel equation with a spatial panel GMM procedure, instrumenting for the endogenous spatial lag within the model.[33]

Table 2.8 presents no evidence for learning from neighboring audits. Column (2) shows that one additional audit in a neighboring district in a given year has no effect on deforestation. The interaction between own and neighborhood audits is significantly positive in column (3). Nonetheless, the combined effect of own, neighbor and interaction effect remains insignificant. The endogenous spatial lag turns out significant in specification (4) and it reduces the audit effects on deforestation in size and renders the neighbor audit effect insignificant. Addressing the endogeneity of the spatial lag in a spatial GMM procedure in column (5) increases the spatial lag coefficient and yields negatively significant audit effects and a significant neighbor interaction effect. Adjusting for potential spatial autocorrelation of the error terms (column 6) largely reduces the coefficient sizes and yields once again insignificant impacts.

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32 If corruption activities are shifted promptly by the first audits, learning might only be induced through the first auditing treatment. We test this by leaving all secondary own and neighboring audits ineffective, recoding the audit indicators as: $A^s_{it} = I(A_{it} > 0)$and $A^s_{-it} = I(\sum A_{-it} > 0)$ and estimating:

$$\Delta \ln D_{it} = \gamma_1 \, \Delta A^s_{it} + \theta_1 \Delta A^s_{-it} + \mu_1 \Delta A^s_{it} \cdot \Delta A^s_{-it} + \Delta \mathbf{X}'_{it} \, \boldsymbol{\beta} + \kappa_{st} + \upsilon_{it}.$$

Results show no qualitative differences in comparison with estimating Equation 2.9.

33 We perform the spatial panel regressions in R, whereas 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 districts that had no direct neighbors in the sample.

Table 2.8: 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	-0.125	-0.129	-0.266	-0.222	-0.258***	-0.099
	(0.122)	(0.122)	(0.169)	(0.145)	(0.098)	(0.074)
Neighb. audits		0.001	-0.026	-0.038	0.022	0.008
		(0.043)	(0.046)	(0.042)	(0.025)	(0.017)
Audit ×			0.080*	0.061	0.068**	0.078
Neighb. audits			(0.046)	(0.040)	(0.028)	(0.018)
Spatial lag				0.905***	0.963***	1.024***
				(0.069)	(0.049)	(0.129)
Spatial error						-0.887
Cloud error	-1.099***	-1.100***	-1.100***	-0.421**	0.026	-0.031
	(0.179)	(0.180)	(0.180)	(0.178)	(0.133)	(0.065)
State-year effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
No. observations	5500	5490	5490	5490	6039	6039
No. districts	550	549	549	549	549	549
R-sq.	0.218	0.221	0.222	0.348		
σ^2					1.922	1.322

*N*ote: Columns (1) to (4) report OLS estimates in first difference form, with robust standard errors clustered at the district 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 **R** with the *splm* package from Millo and Piras (2012); all other empirical results in this chapter are estimated in Stata™. *,**,*** denote significance at the 10/5/1% level.

We used lottery specific estimates and neighborhood estimates as tests for differential learning processes. Disentangling the zero average led to no consistently diverging effects of auditing on deforestation.

2.6.3 *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 district corruption levels, and in the effects of the publicly revealed audit information. In districts that got audited before the 2004 local elections, more corrupt mayors had significantly worse re-election chances than in districts that got audited after the elections (Ferraz and Finan, 2008). Moreover, because of 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 districts with mayors who serve their first term (Ferraz and Finan, 2011). Similar findings can also be replicated within our sample of Amazonas districts.

Table 2.9 shows that the probability of a mayor's reelection in the district elections of 2004 or 2008 was not affected by audits per se, but was reduced if audits resulted in high corruption findings. Highly corrupt administrations seem to experience significantly better chances of reelection. Though this advantage is reduced to zero when audits revealed corruption before elections. We restrict our attention to those Amazonas districts that were audited for the first time within a two-year window before the two district 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 188 districts. We measure local governance by corruption findings of the audit reports from just before the elections. The coefficient on corruption shows that, as long as not audited, more corrupt mayors were also more probable to be reelected, for 2008 and the pooled sample. The interaction between audit and corruption findings is negative and significant for 2004, and the pooled sample. Auditing thereby offsets the reelection advantage of corrupt administrations. This corroborates the findings of Ferraz and Finan, by showing that in the Amazon districts audit reports had similar effects as in Brazil as a whole, and mayoral chances of reelection declined significantly with negative corruption findings.[34]

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 favor 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 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.

34 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.

Table 2.9: 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	-0.040	-0.070	-0.057	-0.041	-0.064	-0.064
	(0.106)	(0.102)	(0.097)	(0.099)	(0.073)	(0.073)
Corruption		0.039		0.093***		0.070*
		(0.071)		(0.033)		(0.036)
(Audit × Corruption)		-0.252***		-0.126		-0.202**
		(0.096)		(0.140)		(0.082)
No. observations	94	94	125	125	219	219
No. districts	94	94	125	125	188	188
R-sq.	0.002	0.045	0.003	0.030	0.004	0.029

*N*ote: The table reports linear probability models estimated with OLS, explaining the probability of mayoral reelection. The sample is restricted to originally forested Amazonas districts that were audited within two years before 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. The corruption measures are standardized over all reports to have a zero mean and standard deviation of one. Clustered standard errors are reported in parentheses. *,**,*** denote significance at the 10/5/1% level. Linear combination of all coefficients in columns (2), (4) and (6) are -0.283*** (0.107), -0.074 (0.173) and -0.196* (0.101), respectively.

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 2.4 and interacting it with the public audit treatment.[35] 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:[36]

$$\begin{aligned}\Delta \ln D_{it} =& \phi_1 \, \Delta A_{it} + \phi_2 \, \Delta FT_{it} + \phi_3 \, \Delta(A_{it} \times FT_{it}) + \\ & \phi_4 \, \Delta(A_{it} \times C_{it}) + \phi_5 \, \Delta(A_{it} \times C_{it} \times FT_{it}) + \\ & \Delta \mathbf{X}'_{it} \, \boldsymbol{\beta} + \kappa_{st} + \upsilon_{it}.\end{aligned} \tag{2.11}$$

We would expect worse governance findings leading mayors to re-adjust their behavior more strongly if they stand for reelection, yielding a positive ϕ_5 if audits induce fiscal discipline and a negative ϕ_5 if audits shift corrupt activities

35 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 district was governed by a first-term mayor.

36 For the electoral incentives analysis, we had to exclude one control unit for which we could not complete information on the incumbent mayor during our time frame.

Table 2.10: Public audit effects and mayoral term limits

Dependent	Δ ln Deforestation					
Governance variable			Standard.		Standard. yearly	
			Corr.	Irreg.	Corr.	Irreg.
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Audit	-0.198	-0.269	-0.239	-0.267	-0.249	-0.278
	(0.135)	(0.175)	(0.163)	(0.175)	(0.167)	(0.169)
Δ First term mayor	-0.024	-0.027	-0.026	-0.028	-0.026	-0.027
	(0.048)	(0.049)	(0.049)	(0.049)	(0.049)	(0.049)
Δ (Audit × First term mayor)		0.106	0.086	0.100	0.091	0.124
		(0.207)	(0.192)	(0.209)	(0.199)	(0.199)
Δ (Audit × Gov. failures)			-0.123	-0.057	-0.109	-0.084
			(0.175)	(0.108)	(0.176)	(0.191)
Δ (Audit × Gov. failures			-0.039	0.141	0.069	0.289
× First term mayor)			(0.251)	(0.255)	(0.236)	(0.256)
State-year effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Further controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
No. observations	4990	4990	4990	4990	4990	4990
No. districts	499	499	499	499	499	499
R-sq	0.151	0.151	0.151	0.151	0.151	0.151

*N*ote: The table reports first difference OLS estimates, with the dependent variable being the change in the log of yearly newly deforested area. Standardized governance variables have a zero mean and standard deviation of one for all reports. Corr. abbreviates the corruption measurement, Irreg. represents the relative number of irregularities per program audited. Further controls include first differences in Clouds error Robust standard errors, clustered at the district level, are reported in parentheses. *,**,*** denote significance at the 10/5/1% level.

towards deforesting sectors. The results of these regressions are documented in Table 2.10. The coefficient on first term mayors is negative but insignificant: deforestation is not lower under first term mayors per se (column 1). Similarly, audits do not affect deforestation differently in districts with first or second term mayors (column 2).

Columns (3) to (6) present no results which would indicate electoral incentives in audited districts with high governance failures. Deforestation continues unaffected if mayors are audited with high corruption finding. Even when these mayors face reelection incentives deforestation stays unaffected with the triple interaction being insignificant.

Once again, these results cannot 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 have no play in this process: mayors who can stand for reelection are equally unaffected by audit reports than second term mayors.

2.6.4 *Public and legislative pressures*

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 districts 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 district had a radio station either in 2004 or in 2009.[37] In Table 2.11 we split the effect of audit treatment (and governance interactions) for districts with and without radio stations. We do not find a significant detrimental audit effect in districts with radio stations, and no significant effect in districts without a radio station. Nonetheless, the coefficient with radio stations is positive and the coefficient without radio stations is negative. Both are not statistically different from zero though statistically different from each other. With 5% significance, audits in districts with radio stations experience approximately 33% higher deforestation levels than audited districts without radio stations.[38] 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. Differentiating, between districts with and without radio stations and with respect to corruption findings (columns 2) show insignificant coefficients. In column (3) we interact with the standardized number of findings on mismanagement in the reports. For districts with radio stations, the increase in revealed irregularities leads to a significant increase in deforestation rates by 20.5%. However, the radio station variable might have several drawbacks. First of all, it is not randomly distributed over the audited districts 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 districts do actually have a radio station, it is possible that these variables are not the best proxies for the quality of information flows in this area.

Litschig and Zamboni (2008) show that judicial presence in a district 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 district in 1999 and

37 IBGE recorded the presence of local radio stations in 2004 and 2009; over this time period 11.3% of districts in our sample switched from no station to having a radio station or the other way around, whereas only 13.4 % have had consistently access to local radio and 24.7% had at least once a radio station.

38 The difference audit coefficients in districts with radio stations versus no radio stations is -0.350 = -0.217 - 0.133. The impact of these indicator variables on deforestation can be calculated as $[e^{-0.217} - 1] - [e^{0.133}] = -0.337$.

Table 2.11: Public audit effects and radio stations

Dependent	Δ ln Deforestation		
Governance variable		Standard.	
		Corr.	Irreg.
	(1)	(2)	(3)
(a) Δ (Audit $\times$ No radio station)	-0.217	-0.206	-0.217
	(0.158)	(0.147)	(0.157)
(b) Δ (Audit $\times$ Radio station)	0.133	0.107	0.095
	(0.096)	(0.084)	(0.085)
(c) Δ (Audit $\times$ Gov. failures $\times$ No radio station)		-0.061	-0.253
		(0.155)	(0.170)
(d) Δ (Audit $\times$ Gov. failures $\times$ Radio station)		0.091	0.187*
		(0.142)	(0.109)
p-value of test (a) = (b)	0.047	0.052	0.070
p-value of test (c) = (d)		0.466	0.032
State-year effects	*Yes*	*Yes*	*Yes*
Cloud error	*Yes*	*Yes*	*Yes*
No. districts	550	550	550
No. observations	5500	5500	5500
R-sq	0.139	0.138	0.139

*N*ote: 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 omitted category are districts and years without audit. Robust standard errors, clustered at the district level, are reported in parentheses. *,**,*** denote significance at the 10/5/1% level.

2012. While in 1999 about 51% of Amazonas districts had judiciary seats, this number increased to 61% by the end of the period.[39] Since yearly data on judiciary presence throughout the whole period are unavailable, we build the interaction terms in two ways: both based on historical (1999) and recent (2012) information. When splitting the districts into two categories, with and without direct judiciary presence in 1999, districts do not differ with respect to the deforestation after audits (cf. Table 2.12). Using the judiciary presence in 2012 (cf. Table 2.13), districts with judiciary presence experience diverging effects of public audits on deforestation. Measuring governance quality by the standardized number of irregularities found in reports, deforestation rates significantly fall after audits in districts with no judiciary seat. A counter-intuitive result, which we cannot explain by theory. Nonetheless, districts judiciary seats ex-

39 11% of districts in our sample established a judiciary seat between 1999 and 2012 and 1.8% ceased to have a judiciary seat.

Table 2.12: Public audit effects and judiciary presence (1999)

Dependent	Δ ln Deforestation		
Governance variable		Standard.	
		Corr.	Irreg.
	(1)	(2)	(3)
(a) Δ (Audit $\times$ No judiciary seat)	-0.105	-0.111	-0.090
	(0.166)	(0.174)	(0.155)
(b) Δ (Audit $\times$ Judiciary seat)	-0.152	-0.139	-0.161
	(0.175)	(0.138)	(0.170)
(c) Δ (Audit $\times$ Gov. failures $\times$ No judiciary seat)		0.042	-0.322
		(0.078)	(0.234)
(d) Δ (Audit $\times$ Gov. failures $\times$ Judiciary seat)		-0.047	0.113
		(0.192)	(0.086)
p-value of test (a) = (b)	0.840	0.897	0.753
p-value of test (c) = (d)		0.663	0.248
State-year effects	*Yes*	*Yes*	*Yes*
Cloud error	*Yes*	*Yes*	*Yes*
No. districts	535	535	535
No. observations	5350	5350	5350
R-sq	0.139	0.139	0.139

*N*ote: 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 district level, are reported in parentheses. *,**,*** denote significance at the 10/5/1% level.

perience significantly higher deforestation rates after auditing. 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 districts is not statistically significant. The difference also remains when differentiating further judicial effects by mayors who can seek reelection (cf. column 4-5) We thus find some statistically significant difference in the adverse audit effects on deforestation with respect to 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 on media and judiciary presence point into the direction which could be expected based on findings of previous literature but all differences are only statistically significant at lower thresholds.

Table 2.13: Public audit effects and judiciary presence (2012)

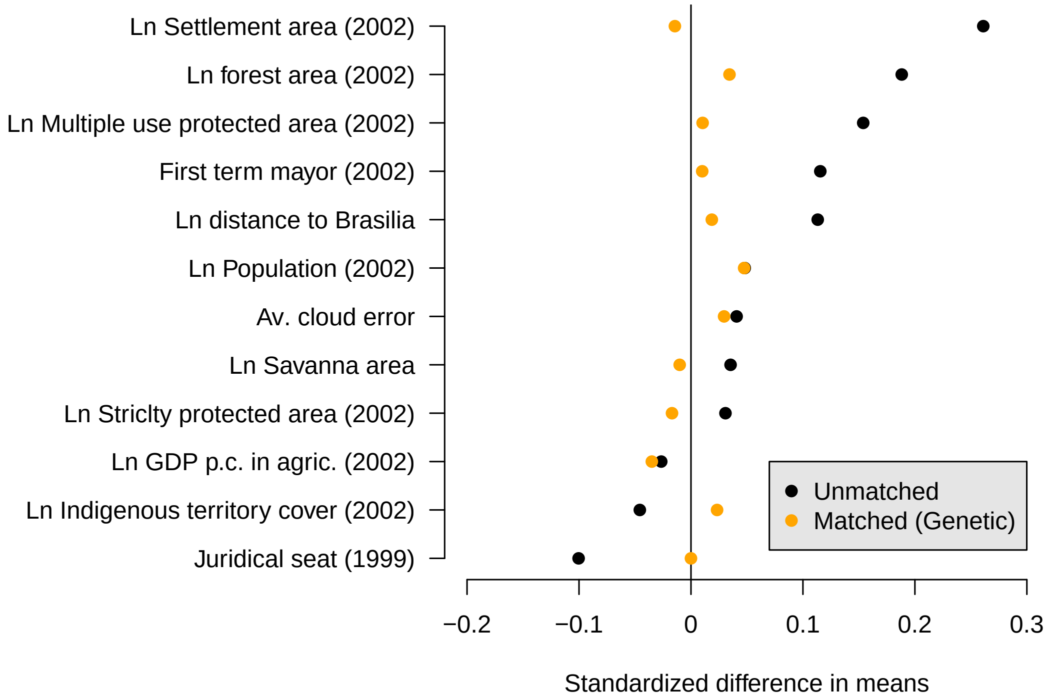
Dependent	Δ ln Deforestation				
Governance variable		Standard.		Standard.	
		Corr.	Irreg.	Corr.	Irreg.
	(1)	(2)	(3)	(4)	(5)
(a) Δ (Audit $\times$ No judiciary seat)	-0.218	-0.221	-0.241	-0.213	-0.231
	(0.208)	(0.212)	(0.209)	(0.214)	(0.216)
(b) Δ (Audit $\times$ Judiciary seat)	0.047	0.017	0.028	0.027	0.031
	(0.087)	(0.088)	(0.082)	(0.090)	(0.082)
(c) Δ (Audit $\times$ Gov. failures $\times$ No judiciary seat)		0.050	-0.719*	0.030	-0.561*
		(0.076)	(0.390)	(0.076)	(0.303)
(d) Δ (Audit $\times$ Gov. failures $\times$ Judiciary seat)		-0.094	0.146**	-0.096	0.040
		(0.080)	(0.072)	(0.140)	(0.119)
(e) Δ (Audit $\times$ Gov. failures $\times$ No judiciary seat $\times$ First term mayor)				0.032	-0.216
				(0.107)	(0.587)
(f) Δ (Audit $\times$ Gov. failures $\times$ Judiciary seat $\times$ First term mayor)				0.272*	-0.163
				(0.159)	(0.162)
p-value of test $(a) = (b)$	0.233	0.291	0.227		
p-value of test $(c) = (d)$		0.680	0.079		
p-value of test $(a) + (c) = 0$		0.283	0.093		
p-value of test $(b) + (d) = 0$		0.293	0.161		
p-value of test $(a) + (c) = (b) + (d)$		0.133	0.052		
State-year effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Cloud error	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
No. districts	534	534	534	534	534
No. observations	5340	5340	5340	5340	5340
R-sq	0.145	0.145	0.147	0.147	0.147

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 2012. Robust standard errors, clustered at the district level, are reported in parentheses. *,**,*** denote significance at the 10/5/1% level.

2.6.5 *Unbalances after random assignment*

The Brazilian random audit program was selected districts by lotteries between 2003 and 2012, which allowed us to analyze the effects of the program. Nonetheless, remaining imbalances of important covariates related to treatment could have biased our results. Above, we used political and electoral characteristics to evaluate heterogeneous auditing effects. In this section we evaluate the random characteristic of the program along these observable char-

Figure 2.5: Covariate balance after matching



Note: The graphs depicts the standardized differences in mean between audited (T) and control (C) units before and after matching. Reduced distances to zero indicate to improved covariate balance after matching. The standardized differences in mean are calculated as the difference between treatment and control divided by the standard deviation of the treated, $\frac{\mu^T - \mu^C}{\sigma^T}$.

acteristics and reduce remaining imbalances with a matching procedure. Both average and heterogeneous effects are estimated.

We use genetic matching to reduce differences of covariate distributions between treated and control districts (Sekhon, 2011).[40] Following Ho, Imai, King, and Stuart (2007) a combination of matching with post-matching parametric analyses is advantageous even where treatment was assigned randomly. Before and after balances are depict in Figure 2.5. Confirming the goodness of the randomization the unmatched sample reveals low imbalances. Almost all differences between the control and treatment group are below the commonly used threshold of 0.25 standard deviations (Ho et al., 2007). After matching differences are largely reduced towards zero. Close to perfect balance is achieved for the political indicators, first term incumbent mayor and presence of a judiciary seat.[41]

Table 2.14 shows estimates on average and heterogeneous auditing effects on deforestation. In column (1) we firstly report the estimate on the unmatched

40 The genetic matching procedure finds controls based on an evolutionary algorithm, which weights the observable covariates of the sample.

41 Genetic matching performed best in reducing imbalances with our sample confirming Diamond and Sekhon's 2013 development of the method.

Table 2.14: Audit effects after matching

Dependent	Δ ln Deforestation				
	Unmatched	Matched			
	(1)	(2)	(3)	(4)	(5)
Δ Audit	-0.162	-0.170	-0.162	-0.325	-0.058
	(0.131)	(0.135)	(0.133)	(0.189)	(0.160)
Δ (Audit × Stand. corruption findings)			-0.141		
			(0.109)		
Δ First term mayor				-0.031	
				(0.059)	
Δ (Audit × First term mayor)				0.225	
				(0.211)	
Δ (Audit × Judiciary seat in 1999)					-0.218
					(0.255)
State-year effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Further controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	4630	4060	4060	4060	4060
Cluster	463	406	334	334	334
R-sq.	0.143	0.193	0.193	0.192	0.193

*N*ote: The table reports first difference estimates, with the dependent variable being the change in the log of yearly newly deforested area. Further controls includes first differences in Cloud error. Robust standard errors, clustered at the district level, are reported in parentheses. *,**,*** denote significance at the 10/5/1% level.

reduced sample, owing the lack of complete data in all districts.[42] The auditing coefficient is consistent to previous estimates, negative and insignificant. After matching column (2) shows a similar sized insignificant auditing effect. This hints to the fact that balances where already strong to begin with, meaning the covariate distributions between controls and treatment groups were already overlapping to a large extent. Further, interactions in columns (3) to (5) with corruption findings, or our political indicators, first term mayors and judiciary presence confirm forest cover loss to have no causal relation with the auditing program.

2.7 CONCLUSION

This chapter addressed the relation between corruption and deforestation and investigated evasive effects of a federal anti-corruption program, which has

42 Owing to the incomplete information across covariates our sample reduces to 463 districts, excluding 34 audited and 53 non-audited units.

the goal of fighting corruption in district administration, on deforestation in the Brazilian Amazon. For this purpose we connected yearly panel data on deforestation dynamics from the PRODES project (for the years 2002-2012) with information derived from the local fiscal audit program for 237 originally forested Amazon districts. We used the public audit reports to construct and 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 districts with a worse corruption environment, and the relationship holds even 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 550 eligible and originally forested Amazon districts 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 district and state-year fixed effects. The results fail to show any increase in deforestation after audits. This result stands in contrast to a previously unpublished version of this analysis (Cisneros et al., 2013) presented publicly on several occasions where we had found a significant shift after audits.[43]

We test several channels through which audits can have diverging effects on deforestation. No differences occur in districts with worse corruption records and we fail to confirm the hypothesis of learning though neighboring audits where local administrations update their expectations and hence change their behavior. Auditing seems to have played an important role on reelection outcomes in the Brazilian Legal Amazon, confirming the study of Ferraz and Finan (2008) who use the sample of Brazil. Nevertheless, when mayors face reelection incentives, audit reports have likewise no influence on a subsequent increase or decrease in deforestation.

The auditing program did target outcomes of government programs in infrastructure, health, education, etc. but did not target environmental performance. 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. Nonetheless, incentives to improve governance are stronger in areas where public scrutiny exists and performance is measured. On the contrary potential unintended consequences of anti-corruption activities, which can cause rent extraction could increase in sectors less directly observed by the auditors and the public. The inability to detect a shift to the forest sector could be explained by its incapacity to

43 Presented at the Conference on Development Economics and Policy (Ausschuss für Entwicklungsländer, AEL) 2012, Congress of the International Institute of Public Finance (IIPF) 2013 and the Conference of Biodiversity and Economics for Conservation (BIOECON) 2013. Different analytical results are attributed to the update and extension of deforestation data of the years 2010 to 2012, as well as a different coding for the beginning of auditing impacts.

compensate forgone rents caused by the public auditing. Differently, corrupt and audited local governments might experience high pressures to improve fiscal procedures and mitigate the legal consequences rather than trying to find new sources of income. 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 unlikely to be successful in non-targeted areas. Targeted policies for forest conservation implemented by the federal government during the last decade have shown to be highly effective. The large expansion of the protected area network, field-based law enforcement operations based on real-time satellite information about deforestation hotspots, credit restrictions to non-compliant farmers with the Brazilian Forest Code, as well as cross-compliance incentives in form of blacklisting and shaming the highest deforesting districts have significantly contributed to the 80% overall decline in deforestation rates since 2004 (Arima et al., 2014; Assunção et al., 2013b; Cisneros et al., 2015a; Nolte, Agrawal, Silvius, and Soares-Filho, 2013; Soares-Filho et al., 2010). Anti-corruption strategies could resolve the relationship of local corruption and deforestation when embracing a multidimensional approach, targeting local environmental performance.

3

NAMING AND SHAMING FOR CONSERVATION

ABSTRACT

Deforestation in the Brazilian Amazon has dropped substantially after a peak of over 27,000 sq. km in 2004. Starting in 2008, the Brazilian Ministry of the Environment has regularly published blacklists of critical districts with high annual forest loss. Farms in blacklisted districts face additional administrative hurdles to obtain authorization for clearing forests. In this chapter we add to the existing literature on evaluating the Brazilian anti-deforestation policies by specifically quantifying the impact of blacklisting on deforestation. We first use spatial matching techniques using a set of covariates that includes official blacklisting criteria to identify control districts. We then explore the effect of blacklisting on change in deforestation in double difference regressions with panel data covering the period from 2002 to 2012. Multiple robustness checks are conducted including an analysis of potential causal mechanisms behind the success of the Blacklist. We find that the blacklist has considerably reduced deforestation in the affected districts even after controlling for the potential mechanism effects of field-based enforcement, environmental registration campaigns, and rural credit.

3.1 INTRODUCTION

Brazil stands out as one of the few countries in the world where tropical deforestation rates have dropped over the past decade (Hansen et al., 2013). Emerging evidence from quasi-experimental evaluation studies on the effectiveness of Brazil's post-2004 strategy to combat Amazon deforestation unambiguously suggests that environmental policy has come to play a major role in determining land use decisions in the region (Arima et al., 2014; Assunção, Gandour, and Rocha, 2012; Hargrave and Kis-Katos, 2013; Maia et al., 2011; Soares-Filho et al., 2010). In 2004, the Brazilian government has launched a Plan to Combat Deforestation in the Amazon (PPCDAM in its Portuguese acronym). The first two PPCDAM operated in 2004-08 and 2009-11, respectively, and the third PPCDAM ends in 2015. Clearly, the drop in Amazon deforestation from over 27,000 sq. km in 2004 to less than 10,000 sq. km since 2009 results from a myriad of factors including the effects of the 2008 global financial crisis on international commodity demand (Canova and Hickey, 2012). Arima et al.

(2014) provide a detailed account of the Brazilian environmental policy context including statistical analysis of the overall effect of the most recent policy measures on deforestation in the Amazon region.

3.2 BLACKLISTING IN BRAZIL

Here we build on the approach chosen by Arima et al. to study whether and how the list of priority municipalities (henceforth "district blacklist") issued by the Brazilian Ministry of Environment since 2008 played a measurable role in reducing Amazon forest loss. Brazil has pioneered the use of blacklisting as a forest conservation policy strategy and understanding its effect can help us to assess the potential of transparency and accountability initiatives in the conservation sector. We find that, on average, blacklisted districts have experienced distinctly larger reductions in deforestation than comparable non-listed districts and produce evidence that this difference is partially a genuine effect of blacklisting.

The chapter is structured as follows. First, we provide a brief background of the Brazilian forest policy context and describe key elements of the Brazilian blacklisting strategy. We also discuss the potential mechanisms and pathways through which blacklisting might have contributed to reducing deforestation beyond the combined effect of other policy instruments. Next we summarize our empirical strategy to estimate the effect of blacklisting on deforestation, highlighting the main differences between our approach and strategy used in Arima et al. (2014). After documenting our data sources we present main results and robustness checks. Subsequently, we discuss potential caveats of our analysis in the context of the emerging literature evaluating conservation programs and provide conclusions and implications for conservation policy design.

3.3 FOREST POLICY BACKGROUND

Apart from a substantial expansion of the region's protected area network (Soares-Filho et al., 2010), field-based law enforcement operations targeted at deforestation hot-spots by using remote sensing technologies have shown to be important short-term success factors to forest conservation (Hargrave and Kis-Katos, 2013). One of the reasons for the increased effectiveness of field-based enforcement has been an intense collaboration between the Brazilian Environmental Protection Agency (IBAMA) and the state-level public prosecutors. Public prosecutors have been shown to exert positive effects on environmental policy outcomes by enhancing legal coercion (Müller, 2010). Especially in the state of Pará, the public prosecutors have been involved in enforcing property embargoes issued by IBAMA, where for example the MP's have engaged with meat packers and supermarket chains that were previously purchasing beef from illegal sources (Arima et al., 2014).

In addition, several Amazon states pioneered by the State of Mato Grosso, introduced so called Rural Environmental Registries (CAR in its Portuguese Acronym) that were recently combined in a federal registration system. Through the CAR, landholders with and without formal property rights declare the size and spatial boundaries of their land holdings, which enhances the government's ability to monitor compliance with the Brazilian Forest Code Börner et al. (2014).

Complementary to government actions, measures to contain the effect of cropland expansion were also taken by the private sector (Lambin et al., 2014). The so called "Soy Moratorium" was an agreement among major soy bean traders to not buy soy grown on land that was cleared after July 2006. Evaluations of the Soy Moratorium have produced mixed results, with indirect land use change potentially compromising its effectiveness (Arima et al., 2014; Gibbs et al., 2015).

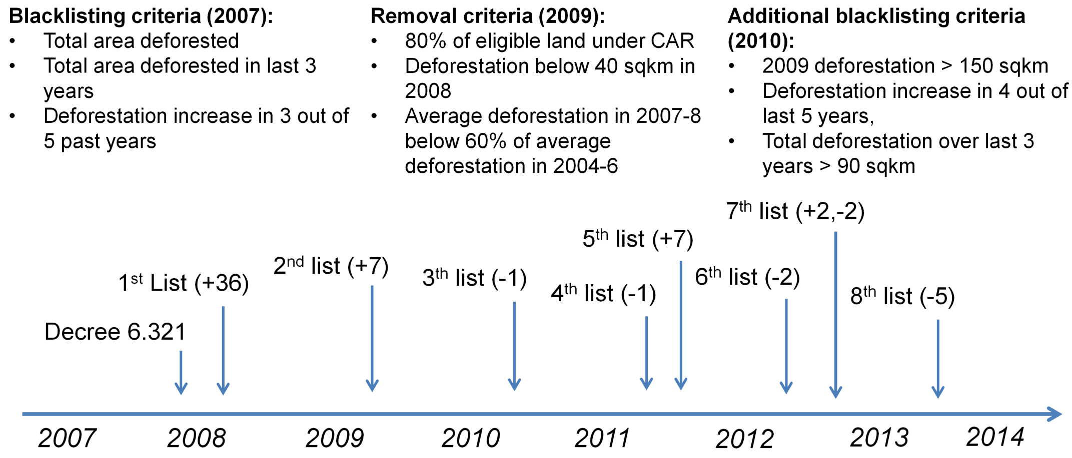
Between late 2007 and early 2008, Brazil introduced additional measures to reinforce field-based enforcement action. First, resolution 3.545 published in 2008 by the Brazilian Monetary Council (Conselho Monetário Nacional) limits credit access to farms that are non-compliant with the Brazilian Forest Code and conditions future credit access on proofs of compliance with environmental legislation. Assunção et al. (2013b) estimate that this measure has avoided 2,700 sq. km of deforestation between 2009 and 2011. Second, the Presidential Decree 6.321 (December 2007) created the legal basis for the Blacklist that contains districts with outstanding historical deforestation rates. In blacklisted districts, stricter rules with regard to the authorization of forest clearing applied and defined administrative targets had to be fulfilled in order to qualify for a removal from the list.

Both decrees essentially operate as cross-compliance measures by making access to credit (resolution 3.555) or authorization of forest clearings (Decree 6.321) conditional on compliance with forest law and registration requirements respectively. A major difference between the decrees is that the credit restriction applies to the whole Amazon biome, whereas only a subset of Amazon districts is blacklisted. This difference allows us to adopt the empirical strategy outlined in section 3.4.

History and impact logic of the Brazilian district blacklist. Decree 6.321, published in December 2007, clearly defines the objective of the Blacklist as a strategy to monitor and control illegal deforestation and prevent land degradation. It states that the list is to be updated annually based on official deforestation statistics and specifies the complementary roles of IBAMA and the National Institute for Agrarian Reform (INCRA) in monitoring and registering landholdings in the blacklisted districts. Three criteria are put forward as being used to compose the blacklist, namely:

1. The total deforested area
2. The total deforested area in the preceding three years
3. The increase of deforestation of minimum three out of the past five years

Figure 3.1: History of district blacklisting and blacklist criteria.



Note: Positive numbers in parentheses depict additions to the blacklist. Negative numbers depict removals.

Figure 3.1 schematically depicts how the Blacklist has evolved since the publication of Decree 6.321.

In January 2008, the first Blacklist was published covering 36 districts. Seven districts were added in both 2009 and 2011. The criteria for the removal from the Blacklist were introduced in 2009. Removal was conditioned on registering at least 80% of the eligible area (mostly privately claimed land) under the CAR. Moreover, annual deforestation had to be kept below 40 sq. km. Only six districts were removed as of 2012.

District blacklisting probably qualifies as the most innovative element in Brazil's multi-instrument conservation policy mix. To our knowledge no other country has yet applied a similar institutional cross-compliance mechanism in the forestry sector. The impact pathway of blacklisting is still unclear and very little research on blacklisting as a governance mechanism exists. Jacobs and Anechiarico (1992) argue that contractor blacklisting is a sensible and ethically justifiable strategy to protect government organizations from fraud. China has experimented with an environmental disclosure policy including publication of lists of environmental regulation violators. A recent study found that this blacklisting strategy has helped in engaging civil society stakeholders in environmental governance (Tan, 2014). The study, however, concluded that effects on behavioral change have been limited because of the country's authoritarian structure. In 2010, a synthesis report by the Transparency and Accountability Initiative lists several largely untested assumptions regarding the impact channels of public disclosure policies. These include greater accountability through transparency and stimulation of action among a wide range of stakeholders. The report found that public disclosure policies have considerable potential to improve governance in sectors such as public service delivery, natural resource governance and donor aid (McGee and Gavent, 2010). Similar findings on public disclosure policies are reported by Blackman (2010) and Tietenberg (1998).

Among the potential impact channels of public disclosure policies, building internal and external pressure in favor of desired action was likely to be the most important motivation behind the Brazilian Blacklist.

A multi-institutional evaluation of the Brazilian government's Plan to Combat Deforestation in the Amazon (PPCDAM) concluded that "the priority list [blacklist]. . . turned out to be a cost-effective means to stimulate co-responsibility of district-level political elites-deforestation was ultimately also a problem of mayors and the local society" (Maia et al., 2011, p. 41). Three distinct types of mechanisms have been recognized playing an important role:

1. **Administrative disincentive:** economic burden of administrative compliance measures motivate local stakeholders to take action. Specifically, landholdings in the blacklisted districts are required to obtain a georeferenced certification with INCRA as a precondition for authorized forest clearings.

2. **Reputational risk:** Public disclosure motivates action by public and private stakeholders as well as the civil society at district-level to "defend" the reputation of the district. The driving force behind this motivation could, for example, be concern about losing future business opportunities or increased environmental monitoring and enforcement action.

3. **External support/pressure:** Blacklisted districts may crowd in financial and logistic support from international NGO and public administrations creating incentives for improvements in local governance. On the other hand, blacklisted districts may also have attracted additional attention by national and subnational law enforcement agencies, such as the Brazilian Environmental Protection Agency (IBAMA).

We hypothesize that the administrative disincentive itself has played a minor role in promoting forest conservation. The Blacklist introduces additional cost to farmers that depend on legal clearing permissions because of the obligation to register with INCRA. In consequence legal clearing rates may reduce. In districts where a considerable share of the forest and agriculture-based economy relies on legal forest use and conversion, the moratorium on new licenses for legal clearings may thus have increased registrations and reduced deforestation rates. On the other hand, if most of the land users in a district rely predominantly on illegal deforestation, additional conditions attached to obtaining environmental licenses will, all else equal, only have small effects on land users' behavior.

Regarding the conditions to be removed from the blacklist, conditions such as the 80% CAR registration target are considered to be separate from the administrative disincentive channel. The reason for this is that these conditions do not exercise direct restrictions on the political administration nor on individual land users. Nonetheless, these rules generate costs if local stakeholders take action towards getting off the list. Such actions could be induced through both the reputational risk and through external support/pressure channels.

There is anecdotal evidence that reputational risk has played a significant role in bringing down deforestation in some of the blacklisted districts. In the district Paragominas (state of Pará), for example, a local stakeholder initiative to expand CAR registration and reduce deforestation rates was formed when the district appeared on the 2008 Blacklist (Viana et al., 2012). One of the motivations for the leaders of this initiative was reportedly the objective of "... reverting the negative image bestowed by being on the red-list [meaning blacklist]" (Viana et al., 2012, p. 23). Inspired by the "success" of Paragominas, the state of Pará launched a public support program (the Green Districts Program) for districts that achieved removal from the Blacklist. Similar factors played a role in the district Brasil Novo that was removed from the list in 2013 (Environmental Secretary of Brazil Novo-public event, Brasilia, 7.10.2013).

The third group of mechanisms-external support/pressure could also have played an important role. Both national and international NGOs have concentrated efforts to support CAR registration in blacklisted districts in collaboration with local and state-level government agencies. Support included both technical-scientific and local logistic measures to enable CAR registration at higher rates (The Nature Conservancy-interview, Belém, 4.10.2013). CAR registration exposes landholders to greater scrutiny by authorities and supporting NGO's and thus also a higher risk of being held responsible for illegal deforestation. A recent study on the effect of CAR on deforestation has nonetheless produced ambiguous results with respect to deforestation outcomes (Azevedo et al., 2014). In addition, blacklisted districts have been subject to more intense enforcement activities by IBAMA during (but also before) the Blacklist was published. Moreover, blacklisting could have influenced rural credit flows into blacklisted districts as suggested by article 11 of Decree 6.321.

The empirical strategy described below is designed to measure the overall effect of the blacklisting policy on deforestation and to capture the causal effect of the third (and to some extent the second) group of mechanisms. The Brazilian blacklisting policy may have had a bearing on all three mechanism categories. But, the reputational risk and external support/pressure channels most closely represent the impact theory of public disclosure policies as discussed above (Tietenberg, 1998).

3.4 EMPIRICAL STRATEGY

The methodological challenge of evaluating the effect of the Blacklist on deforestation in the blacklisted districts consists of identifying a counterfactual scenario of what would have happened in the absence of the Blacklist (Khandker, Koolwal, and Samad, 2010). From the previous section, we know that blacklisting was not random. Instead, regulators have used defined selection criteria that were linked to historical deforestation. Regression Discontinuity Design (RDD) is a commonly used evaluation technique for interventions where the selection mechanism is known (Hahn, Todd, and Van der Klaauw, 2001) however unfortunately, only the three official criteria were made public, but not the exact approach, e.g., weighting used to arrive at the published blacklists. Although past deforestation highly correlates with selection, it is not possible to reproduce the first list of 36 districts based on the three published selection criteria alone. Considering the first Blacklist criterion (see also Figure B.1), the 36 districts with the highest total forest loss as of 2007 include only 20 of the blacklisted districts. The 36 largest deforesters during the years 2005 to 2007 (second criterion) comprise only 25 of the blacklisted districts. Finally, 206 districts fulfill the criterion of three years of increments in deforestation during the past five years (2003-2007) and only 14 of them are blacklisted. Three blacklisted districts (Ulianópolis Paranaíta, Porto dos Gaúchos) did not fulfill any of the three criteria. Thus, we can only speculate which other criteria could have played a role in composing the Blacklist. Moreover, our sample of treated districts is too small for informative local linear regression analyses in a RDD.

Our approach relies on panel data with annual observations of deforestation and other covariates over eleven years that complement the relatively low number of treated observations, i.e., only 50 districts were blacklisted as of 2012. Since most of our data are available only at district level, we choose the district as our unit of analysis. We first use a first difference (FD) estimation model to eliminate unobserved time-invariant district-level effects on deforestation. Second, we use matching on pre-Blacklist characteristics to reduce model-dependence and the selection bias resulting from targeting the Blacklist to districts with high deforestation rates (Ho et al., 2007). We abstain from estimating treatment effects directly from the matched dataset, because we expect a limited degree of common support. Instead we re-estimate our FD model using the matched dataset. Third, we follow the strategy documented in Ferraro and Hanauer (2014b) to estimate the net treatment effect of blacklisting, i.e., the treatment effect after controlling for potential changes in external pressure and support measures (see previous section). This approach essentially blocks the effect that blacklist-induced changes in field-based enforcement intensity, rates of CAR registration, and rural credit flows could have had on deforestation in blacklisted districts. The remaining effect is called the net average treatment effect and captures all impact mechanisms other than the three above-mentioned causal mechanisms.

Following Jalan and Ravallion (1998), we derive the double difference estimation model for our purpose as follows. Using yearly log deforestation ($\ln D_{it}$) as the outcome variable, the panel fixed effect can be written as:

$$\ln D_{it} \;\; = \;\; + \beta\, B_{it} + \mathbf{X}'_{it}\gamma + t\mathbf{Z}'_{i}\delta + \eta_t + \alpha_i + t\kappa_s + u_{it} \qquad (3.1)$$

where B_{it} is the treatment variable indicating whether district i has been blacklisted at any time t, X_{it} is the vector for time-varying covariates, and Z_i is a vector of time-invariant covariates or the so-called "initial conditions". Initial conditions are interacted with the time variable t and thus remain in equation 3.2 below even after taking first differences. The underlying rationale is that the deforestation trend is likely to be affected by pre-treatment local conditions (Jalan and Ravallion, 1998). α_i is the district-specific fixed effect, which captures all time-invariant locally idiosyncratic influences on deforestation, year-specific effects η_t control for yearly changes to the deforestation trend, common to all districts, notably macroeconomic or environmental shocks and changes in the Brazilian environmental policies. State-specific effects κ_s capture differences in the implementation of federal laws on the state level. u_{it} denotes the error term. Both fixed effect and first difference estimators can be used, but we proceed with the first difference estimator that is less prone to serial correlation (Verbeek, 2008, p. 349). Taking first differences, equation 3.1 becomes:

$$\Delta \ln D_{it} \;=\; + \,\beta\, \Delta B_{it} + \,\Delta \mathbf{X}_{it}'\,\gamma + \,\mathbf{Z}_i'\,\delta + \,\Delta\eta_t + \,\kappa_s + \,\Delta u_{it} \tag{3.2}$$

here the district-specific fixed effect is canceled out and the initial conditions stay in the equation as time-invariant covariates ($\Delta\, t = 1$). In the first difference form, the year specific effects to the trend are transformed to standard year dummies $\Delta\,\eta_t$.

The treatment coefficient β measures the average treatment effect, i.e., the average change in deforestation due to blacklisting for all years after treatment (shift in deforestation trend). Deforestation is measured over the period from August to July (see S1 Text). Treatment indicators have to account for the fact that blacklists were released at different points in the year. The first list of 36 districts was published at the end of January 2008 (see Figure 3.1). Hence, we set treatment B_{it} to 0.5 to represent the six months during which blacklisting could have affected deforestation in 2012 (see equation 3.3). The second and fifth blacklists were published at the end of March 2009 and end of April 2011 and the respective treatments are set to 0.25 and 0.17. The 7th Blacklist was published in October 2012 and thus is outside our analytical timeframe. Six districts were released from the Blacklist during our time period. We do not expect the blacklisting effect on deforestation to vanish immediately after a district has been released from the list. Especially the second and third of the three potential impact channels discussed above are likely to result in longer term effects on deforestation dynamics. Moreover, off-listing is conditioned on having at least 80% of the eligible land registered under the CAR-a measure that effectively improves the government's ability to monitor land cover change in the long run, also after a district was released from the list. Our treatment variable is thus coded as follows:

$$B_{it} \;=\; \begin{cases} [0,1] & \text{in 1st year of blacklisting} \\ 1 & \text{in 2nd and all subsequent years after blacklisting} \\ 0 & \text{otherwise} \end{cases} \tag{3.3}$$

i.e., treatment is coded between 0 and 1 when blacklisting occurs after the start date of the period over which deforestation is measured (August 1st-July 31st).

Confounding factors that could affect deforestation are considered in the covariates vectors X_{it} and Z_i of equation 3.2. Our choice of covariates is based on previous empirical work on tropical deforestation in the Amazon region and beyond (Aguiar, Câmara, and Escada, 2007; Andersen, 1996; Araujo et al., 2009; Arima, Simmons, Walker, and Cochrane, 2007; Hargrave and Kis-Katos, 2013; Pfaff, 1999; Kaimowitz and Angelsen, 1998).

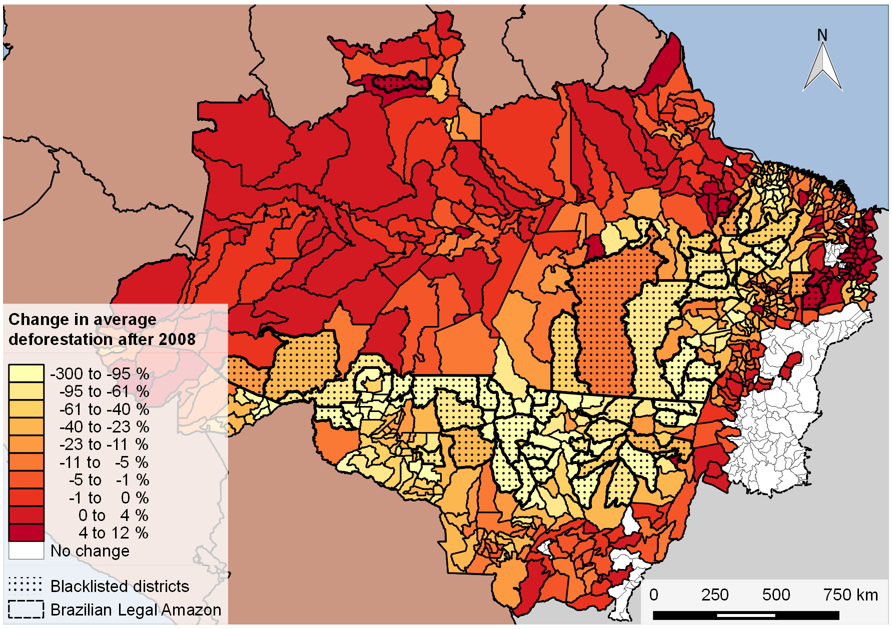
Among time-invariant covariates, we consider various measures of deforestation and forest cover up until before the first Blacklist in 2008 and control for district size and population density. Moreover, we control for farm characteristics, indicators of agricultural intensification and average land values, which have shown to be important predictors of deforestation in previous studies (Angelsen and Kaimowitz, 2001; Pfaff, 1999). Initial forest cover and average travel distance are only used in matching but omitted in regression analyses to avoid multi-collinearity. Since clouds represent a significant source of measurement error in remotely sensed deforestation data, we include cloud cover over remaining forests in all regression analysis.

Among time varying predictors, we consider GDP per capita, timber and soy prices (zero in districts without soy production) (cf. Hargrave and Kis-Katos, 2013), and the area of settlements, protected areas, and indigenous territories in each district. All these tenure categories have been found to affect deforestation rates in previous studies (Ezzine-de Blas et al., 2011; Soares-Filho et al., 2010). In addition, we control for political factors by introducing dummy variables indicating whether districts are governed by the same political party as the president of Brazil (Brazilian Social Democracy Party in 2002 and Brazilian Workers Party from 2003 to 2012). In causal mechanism analyses, we consider yearly data on the number of field-based inspections registered by the environmental protection agency, the percentage of land coverage of CAR registrations and the annual rural credit issued by the Brazilian Central Bank (BCB).

Since the group of potential control districts is likely to exhibit lower pretreatment levels in deforestation than the treated districts, we rely on the double difference method to estimate the treatment effect of blacklisting [3, 20]. A critical assumption of the double difference method is that treated and control observations exhibit parallel time trends in the outcome variable (time-invariant heterogeneity). In other words, in the absence of blacklisting, we assume that treated and control districts would have had the same change in deforestation over time even though they exhibit different absolute levels in forest loss.

To ensure pre-treatment parallel time trends between control districts and blacklisted districts and to also cope with selection bias of the policy we rely on matching to filter out inappropriate controls. Matching is a frequently used quasi-experimental evaluation technique in the presence of unknown selection mechanisms (Andam et al., 2008; Gaveau et al., 2009; Ho et al., 2007; Honey-Rosés, Baylis, and Ramírez, 2011; Rosenbaum and Rubin, 1983). Matching relies on propensity scores or other distance measures that are derived from observed characteristics of treated and untreated observations (here districts). Treated observations are paired with "similar" non-treated (or control) observations to reduce the bias in treatment effect estimations. A strong assumption

Figure 3.2: Average change in deforestation after 2008



Note: Deforestation is measured in percentage deforestation over the district area. The change refers to the average difference between the time periods 2003 to 2007 and 2008 to 2012.

of the matching estimator is unconfoundedness, i.e., one assumes that no other than the observed criteria were relevant in selecting districts into the Blacklist. Moreover, matching requires that there is a considerable region of overlap in the distance measures or propensity scores of treated and untreated observations of the sample. Whereas we are able to control for a large number of potential selection criteria (see below), our sample of non-blacklisted districts is unlikely to be a satisfactory pool of potential controls because most blacklisted districts have indeed been amongst the highest deforesting districts in the Brazilian Amazon region before the Blacklist was enacted (see Figure 3.2). Matching can help us to identify similar control observations and thus represents a sensible preprocessing step in our evaluation strategy (Ho et al., 2007). We use 1 to 1 matching on the covariate distance between districts weighted by the inverse-variance with replacement. As described above we cannot explicitly know how the selection of blacklisted districts took place. In addition to the three official selection criteria we thus also rely on pre-treatment district characteristics as matching covariates.

The official blacklisting criteria are defined as accumulated deforested area in 2007, deforested area in 2005, 2006 and 2007 and the number of times deforestation increased over the past five years. We further use district size, the remaining forest cover as a percentage of a district area in 2007, and the average distance to the district capital estimated by Nelson (2008) to account for deforestation potential and accessibility. To control for socio-economic factors we include population density in 2007 and construct indices for farm density, the share of small farms, percentage of land holders with legal land titles, the share of farm area within a district, and cattle stocking rates from the 2006 Agricultural Census. From the same source we calculated the average land value per hectare and the number of tractors per farm to control for conservation opportunity costs and capitalization levels. Further we control for GDP per capita in 2005, 2006 and 2007. To capture potential political selection determinants we also use the dummy on district mayors' political affiliations in 2007 as explained above.

Details on the approaches used to analyze, dynamic treatment effects, spatial spillovers and causal mechanism effects are provided in the respective subsections.

3.5 STUDY AREA AND DATA

Our study area is located in the Legal Brazilian Amazon, an area of approximately 5m sq. km that extends into nine Brazilian states. Figure 3.2 depicts the study area highlighting changes in average deforestation in blacklisted and non-blacklisted districts after the cutoff point in 2008, when the Decree 6.321 was enacted.

From Figure 3.2 it becomes clear that the blacklisted districts have experienced the largest reductions in annual deforestation from the period 2003-2007 to the period 2008-2012. Large increases in average deforestation almost exclusively occurred in non-blacklisted districts, but many also experienced reductions in forest loss.

The Brazilian Legal Amazon district database from the Brazilian Institute for Geography and Statistics (IBGE) covers 771 districts. All variables are defined according to official 2007 administrative boundaries as put forward by IBGE. To avoid bias, we exclude from our sample 273 districts (none of which was blacklisted) with less than 10% forest cover in 2002. Most of these districts are located in the Amazon/Cerrado ecotone. We further exclude six districts because of missing data. This leaves us with a database of 492 districts (see Figure B.2).

To ensure consistency with 2007 administrative boundaries and control for cloud-related measurement errors we construct our dependent variable, annual deforestation, from INPE's (Brazilian Space Research Center) publicly available vector dataset for 2012. Details on how deforestation, forest, and cloud cover are defined are provided in the supplementary file S1 Text.

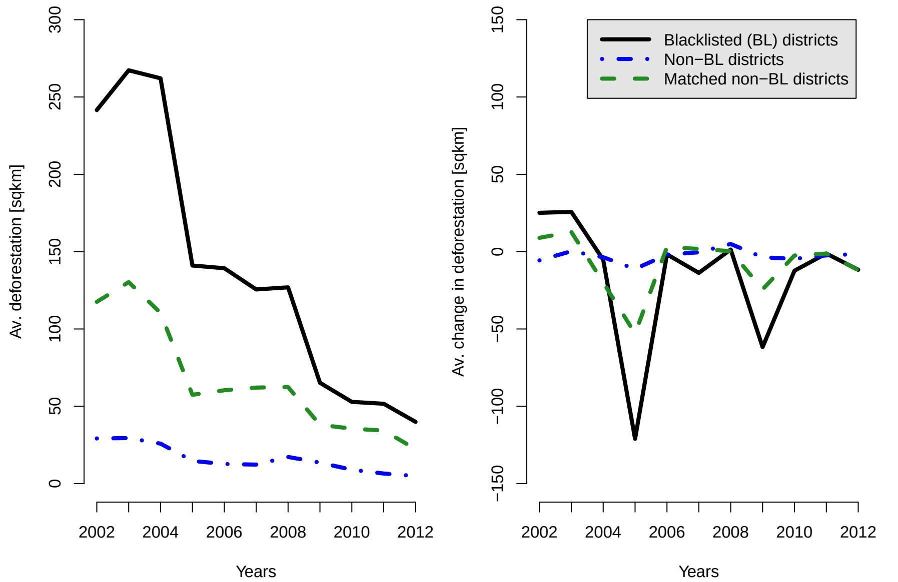
Table B.1 summarizes the data sources used in this study. Table B.2 presents descriptive statistics for the variables used in the panel data analysis and Table B.3 presents means and differences in means of matching variables.

3.6 RESULTS

3.6.1 *Descriptive analysis and baseline regressions*

Figure 3.3 depicts average deforestation (left panel) and average yearly changes in forest loss for blacklisted and non-blacklisted districts during our study period. Average deforestation in blacklisted districts exhibits a much faster decrease than deforestation in untreated districts, but substantial decreases already occurred before the Blacklist was enacted in 2008, for example between 2004 and 2005. The right panel of Figure 3.3 shows that average year-to-year decreases in deforestation were constantly larger in blacklisted than in control districts after 2005.

Figure 3.3: Deforestation in treatment and control districts



Note: Average yearly deforestation levels on the left panel and average change in deforestation on the right panel. Solid lines depict averages of the blacklisted districts (50). The dashed lines show averages of all non-blacklisted districts (442). The dotted lines show averages of the matched control sample (50).

We start our analysis with all observations in a series of three baseline models using the specification in equation 3.2 and gradually adding covariate groups (summary in Table 3.1, complete results in Table B.4). Complementary estimation results are summarized in S2 Text.

All three models yield similar results with large and highly significant average treatment effects. The first model only includes cloud cover, year dummies and state dummies. Cloud cover is highly significant and with a coefficient close to -1. An increase in one percentage point of cloud cover is therefore associated with an almost 1% decrease in detected deforest area. The second model includes time-invariant effects of initial conditions that determine the deforestation trend in each district. Our third and preferred model includes time varying effects. Among the time-invariant covariates the share of small farmers and the cattle stocking rate, are negatively associated with increases in deforestation. Among time varying covariates, the timber price is negative and the settlement area is positively associated with deforestation. Hargrave and Kis-Katos (2013) report similar results with regard to timber prices and argue that high value timber could boost long-term investment in forest and therefore contribute to lower deforestation. However, the models in Table 3.1 are bound to overestimate the effect of blacklisting on deforestation, because the control group contains many districts with virtually no deforestation during the observation period. We thus proceed to pre-process our dataset using matching on pre-treatment characteristics as outlined earlier.

Table 3.1: Effect of blacklisting on deforestation (full sample)

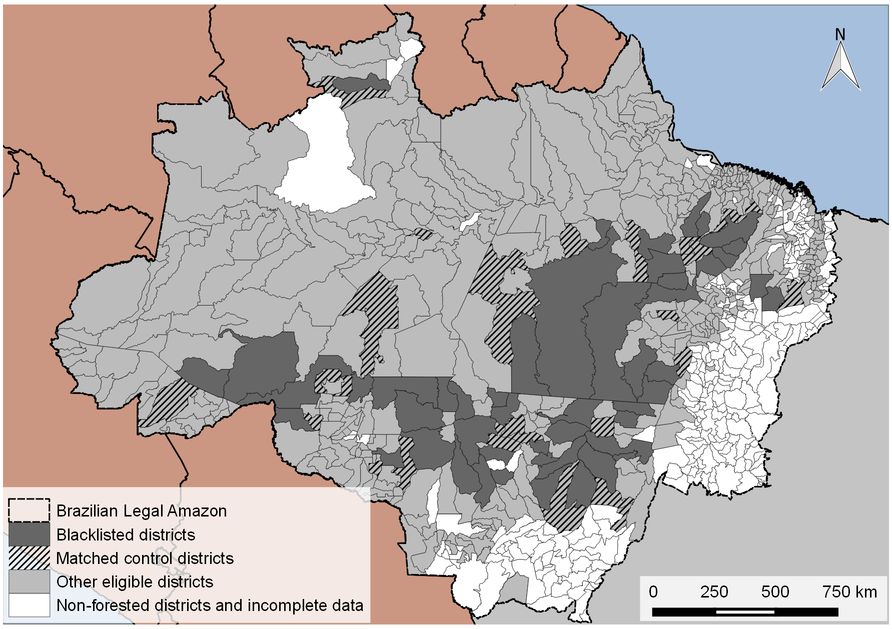
Dependent	Δ ln Deforestation		
	(1)	(2)	(3)
Δ Blacklisted_{it}	-0.803***	-0.992	-0.998***
	(0.192)	(0.205)	(0.204)
Year and state effects	*Yes*	*Yes*	*Yes*
Time-invariant controls		*Yes*	*Yes*
Time-variant controls			*Yes*
Observations	4920	4920	4920
Cluster	492	492	492
Adj. R-squared	0.064	0.065	0.064

*N*ote: The table reports first difference estimates with the dependent variable being the change in the log of yearly newly deforested area. Standard errors, clustered at district level, are reported in parentheses. Time-invariant and variant controls include first differences of the variables reported in S2 Table. *** denotes significance at the 1% level.

3.6.2 *Post-matching regressions*

Matching is implemented in R using the "Matching" package (Sekhon, 2011) and the inverse-variance weights of the covariates. We find control districts by matching each blacklisted district with one non-blacklisted district using replacement. This results in 50 pairs with 50 treated districts and a set of 50 paired control observations that consist of 26 unique districts (see Figure 3.4). Most pairs turn out to be direct neighbors. A comparison of the covariate balance before and after matching is provided in Table B.3. For all variables, the standard mean difference has greatly improved after matching. However, significant imbalances still exist and thus a simple comparison between average deforestation in blacklisted with matched non-blacklisted groups would probably be biased. Figure 3.3 further compares average year-to-year changes in deforestation and deforestation trends separately for blacklisted, non-blacklisted and matched non-blacklisted districts. After matching, treated and control districts exhibit similar pre-Blacklist deforestation trends (see test documentation and results in S3 Text). This gives us confidence that the critical assumption for our subsequent double difference regression is likely to hold.

Figure 3.4: Blacklisted and matched control districts



Note: Paired control districts are found with 1 to 1 matching with replacement using inverse-variance weights.

We use the matched dataset to re-estimate baseline models (1-3) in Table 3.1. Results are presented in Table 3.2 (see Table B.5 for complete results). Post-matching, the magnitude of the blacklisting impact on deforestation drastically decreases by more than 70% to values below 0.3. The coefficient of blacklisting in model 1 is insignificant after matching. In model 2 and 3 we include again the official selection criteria, and all matching variables. Our preferred model (3) includes all yearly information that could affect year to year changes in deforestation. After controlling for the remaining differences in the covariates the blacklisting indicator is significant. The coefficient of 0.294 suggests an average treatment effect of a 25% decrease in deforestation in blacklisted district as a result of blacklisting in each subsequent year after treatment (Halvorsen and Palmquist, 1980).

3.6.3 *Dynamic treatment effects*

As discussed previously, several blacklists were published over time and some districts were removed from the lists in the process. Delayed response to treatment can lead to substantial differences in treatment effects in the post-treatment periods (Laporte and Windmeijer, 2005). Whereas such differences do not change our conclusion with regard to the overall effect of blacklisting, knowledge about how the effect evolves over time (dynamic treatment effect) can be helpful for the design of a blacklisting policy. In this section we test

Table 3.2: Effect of blacklisting on deforestation (matched sample)

Dependent	Δ ln Deforestation		
	(1)	(2)	(3)
Δ Blacklisted$_{it}$	-0.249	-0..276*	-0.297*
	(0.150)	(0.153)	(0.155)
Year and state effects	*Yes*	*Yes*	*Yes*
Time-invariant controls		*Yes*	*Yes*
Time-variant controls			*Yes*
Observations	1000	1000	1000
Cluster	76	76	76
Adj. R-squared	0.251	0.245	0.258

*N*ote: The table reports first difference estimates with the dependent variable being the change in the log of yearly newly deforested area. Standard errors, clustered at district level, are reported in parentheses. Time invariant and time-variant controls include first differences of the variables reported in S2 Table. Observations are selected by a 1:1 closest neighbor matching using inverse-variance variance weights, with replacement. * denotes significance at the 10% level.

whether the size of treatment effects varies over time. We split the original blacklisting indicator into multiple treatment indicators as follows:

$$\Delta \ln D_{it} \;=\; + \sum_{k=0}^{3} \Delta B'^{k}_{it}\,\beta_k + \Delta \mathbf{X}'_{it}\,\gamma + \mathbf{Z}'_i\,\delta + \Delta\eta_t + \kappa_s + \Delta u_{it} \qquad (3.4)$$

B^k_{it} it is based on the original Blacklist dummy of equation 3.2, with the difference that B^0_{it} it overtakes only the value in year k after the treatment year and stays 0 otherwise. Thereby we split the original effect into 4 components. B^0_{it} it is between 0 and 1 as for the year of blacklisting and zero for all subsequent years. The treatment variables B^1_{it}, B^2_{it} and B^3_{it} it are set to one only in the first, second and third year after blacklisting respectively, for each blacklisted district. We thereby capture the effect of blacklisting over the years. The treatment coefficients β_0 to β_3 can be interpreted as the average effect of blacklisting on deforestation for the respective year after blacklisting. Results are shown in Table 3.3. All models (1-3) produce negative and insignificant estimates for blacklisting in its initial year, but significant effects in the first and second year after treatment. For the initial year, the treatment variable is set to have only a partial effect on the overall deforestation within a district. The blacklisting effect of the initial year is thus insignificant as the change in the treatment variable is small in the beginning. Further, when blacklisting started after the dry-season, we would expect to find no effect in the first year if deforestation mainly occurs during the dry-season. The second year effects show twice as large coefficients as the first year effects. This indicates that blacklist-

Table 3.3: Dynamic effects of blacklisting

Dependent	Δ ln Deforestation		
	(1)	(2)	(3)
Δ Blacklist effect in t	-0.399	-0..399	-0.372
	(0.314)	(0.316)	(0.155)
Δ Blacklist effect in $t+1$	-0.249	-0..212*	-0.230*
	(0.212*)	(0.124)	(0.126)
Δ Blacklist effect in $t+2$	-0.123***	-0..482***	-0.461***
	(0.156)	(0.158)	(0.156)
Δ Blacklist effect in $t+3$	-0.291*	-0..291*	-0.264
	(0.159)	(0.161)	(0.160)
Year and state effects	*Yes*	*Yes*	*Yes*
Time-invariant controls		*Yes*	*Yes*
Time-variant controls			*Yes*
Observations	1000	1000	1000
Cluster	76	76	76
Adj. R-squared	0.258	0.253	0.264

*N*ote: The table reports first difference estimates with the dependent variable being the change in the log of yearly newly deforested area. Standard errors, clustered at district level, are reported in parentheses. Time-invariant and time-variant controls include first differences of the variables reported in S2 Table. Observations are selected by a 1:1 closest neighbor matching using inverse-variance variance weights, with replacement. *,*** denote significance at the 10/1% level.

ing effects have materialized only slowly over time, which may be attributed to the gradual roll out of external support measures in the field.

3.6.4 *Spatial spillover effects*

Spatial spillover effects, such as leakage or deterrence, could bias our treatment effect estimation. In our sample, 132 out of the 442 non-blacklisted districts share at least one point on their border with another blacklisted district, i.e., they are direct neighbors. Leakage could take place if the Blacklist encouraged deforestation agents to move to neighboring non-blacklisted districts. Yet, it is also possible that having a blacklisted neighboring district deters land users in non-blacklisted districts from deforesting. In the case of leakage from blacklisted to neighboring non-blacklisted districts we would overestimate the effect of blacklisting on deforestation as 46 out of the 50 matched control districts are direct neighbors. If deterrence effects of blacklisting were leading to

Table 3.4: Spatial spillover effects of blacklisting

Dependent	Δ ln Deforestation		
	(1)	(2)	(3)
Δ Neighbor of Blacklisted$_{it}$	-0.159	-0..158	-0.148
	(0.106)	(0.106)	(0.108)
Year and state effects	*Yes*	*Yes*	*Yes*
Time-invariant controls		*Yes*	*Yes*
Time-variant controls			*Yes*
Observations	1000	1000	1000
Cluster	76	76	76
Adj. R-squared	0.080	0.081	0.081

*N*ote: The table reports first difference estimates with the dependent variable being the change in the log of yearly newly deforested area. Standard errors, clustered at district level, are reported in parentheses. Time invariant and time-variant controls include first differences of the variables reported in S2 Table. Observations are selected by a 1:1 closest neighbor matching using inverse-variance variance weights, with replacement. Estimated coefficients have p-values larger than 0.1.

more conservation in neighboring districts, we would underestimate the effect of blacklisting both in blacklisted districts and at the regional scale.

To test for spillover effects one approach is to include an additional dummy in equation 3.2 which indicates whether a district has a neighboring district that is treated at a given point in time. However, neighbors of blacklisted districts are subject to the same selection bias as blacklisted districts. Moreover, such an approach would rely on merely four control districts in our matched dataset, which do not have a blacklisted neighbor.

Instead, we analyze spillover effects by excluding all blacklisted districts from the sample and interpreting non-blacklisted neighbors (NB_i) of blacklisted districts as if treated. We rerun our matching analysis and conduct the post-matching regression by estimating:

$$\Delta \ln D_{it|B=0} \;=\; +\,\phi\,\Delta NB_{it} + \Delta \mathbf{X}'_{it}\,\gamma + \mathbf{Z}'_i\,\delta + \Delta\eta_t + \kappa_s + \Delta u_{it} \qquad (3.5)$$

Our interest lies in the effect of blacklisting on neighboring districts that have not been blacklisted, ϕ. The neighbor effect NB_{it} is set equal to one when it has at least one blacklisted direct neighbor in a given year, otherwise it equals zero. Table 3.4 reports the results for model specifications (1-3) as in the previous section.

In all 3 specifications the coefficient of the treatment indicator is negative but insignificant. The treatment effects estimated in Tables 3.2 and 3.3 are thus unlikely to be biased by spatial spillover effects from blacklisted to non-blacklisted neighboring districts.

3.6.5 *Robustness*

In the previous sections we have estimated the impact of the blacklisting policy on deforestation. A comparison of the results in Tables 3.1 and 3.2 suggests that we would have overestimated the blacklisting effect without matching as a preprocessing step. Here we evaluate the robustness of our findings vis-à-vis alternative matching techniques and run a series of placebo tests to gain confidence in our estimated treatment effects.

We first test whether our results are robust to the use of alternative matching techniques. We compare the results from our preferred matching approach to (1), a one-to-one matching on the Mahalanobis distance, (2) a one-to-one matching on propensity scores, (3) a one-to-two matching using the inverse-variance weights, and (4) a one-to-one matching with the inverse variance weights, but using only the three official blacklisting criteria provided in Decree 6.321. Results are presented in Table B.6. The blacklisting effect is robust in all specifications. All impact estimates are highly significant and larger than in our preferred estimation method. Our preferred method, the one-to-one matching with replacement on the covariate distance, weighted by the inverse-variance, based on an extended set of controls turns out to be the most conservative version to estimate the effect of blacklisting.

Secondly, we run a placebo analysis where we assume that blacklisting started prior to the actual start date. We shift the start date of the blacklisting policy successively to one, two, and three years before actual treatment. Results are shown in Table B.7. Column 4 repeats the main results of column 3 in Table 3.2. In column 3 we estimate the effect of blacklisting had it occurred in the previous year. Columns 2 and 1 show results of shifting treatment two and three years back, respectively. As expected, none of the placebo treatments are significant; indicating that the treatment effect identified earlier (Table 3.2) is not merely a result of preexisting differences between treated and control districts.

The results above make us confident in interpreting the observed estimates of Table 3.2 as a causal effect of the blacklisting policy on deforestation.

3.6.6 *The net treatment effect of blacklisting*

Above we have produced evidence that blacklisting has led to a significant reduction in deforestation after 2007 on top of the existing decreasing trend. However, the analysis so far does not allow for conclusions with respect to the causal channels involved. In section 3.1 we have discussed potential impact channels that could have played a role in reinforcing the effectiveness of blacklisting: (1) Administrative disincentives, (2) reputational risk, and (3) external support/ pressure. Given district level information about changes in indicators of these channels, we can empirically assess their role as causal mechanisms behind the conservation effect of the Blacklist (Ferraro and Hanauer, 2014b).

Here we follow the approach used by Ferraro and Hanauer (2014b) to identify the net treatment effect of blacklisting, i.e., the effect of blacklisting in the absence of effects via selected mechanisms. We consider three causal mechanisms measured annually and at district level, (1) the amount of documented environmental fines issued by the Federal Environmental Protection Agency (IBAMA), (2) the percentage of eligible area under CAR registration, and (3) the amount of official rural credit flows. We thus hypothesize that blacklisting has affected deforestation by boosting field-based enforcement action, motivating farmers to register under the CAR system, and restricting access to public rural credit. We have measured the joint effect of all potential mechanism by estimating the average treatment effect (ATT) in Table 3.2. The effect that is caused by blacklist-induced changes in the three above-mentioned mechanisms is called the Mechanism Average Treatment effect on the Treated (MATT). The sum of all remaining mechanism effects is the Net Average Treatment effect on the Treated (NATT). The ATT is thus composed of MATT and NATT. Our main interest in this analysis is to find the NATT that remains after controlling for the MATT. The conventional approach to control for mechanism effects is to include the variables into equation 3.2 as explanatory factors. We follow this procedure in a first step and estimate the following model where M_{it} represents the mechanism values of environmental fines, CAR registrations, and public rural credit for district i in year t.[1]

$$\Delta \ln D_{it} \quad = \quad + \, \beta \, \Delta B_{it} + \, \Delta \mathbf{X}_{it}' \gamma + \, \mathbf{Z}_i' \delta + \Delta \mathbf{M}_{it}' \lambda + \, \Delta \eta_t + \, \kappa_s + \, \varepsilon_{it} \qquad (3.6)$$

Whereas we control for annual changes in the mechanisms in equation 3.6, our estimator may still be biased if these mechanisms were actually affected by blacklisting (Rosenbaum, 1984). To avoid this bias and separate the NATT from the ATT we need an empirical approach that allows us to determine (a) what the level of our mechanism indicators would have been in the absence of blacklisting, and (b) what the effect of blacklisting would have been, had the Blacklist not affected the mechanisms. Based on a method proposed by Flores and Flores-Lagunes (2009), Ferraro and Hanauer (2014b) have recently addressed similar questions in the context of protected areas.

Beyond the assumptions made up to this point, two additional assumptions are necessary to estimate the MATT and NATT (Ferraro and Hanauer, 2014b): (1) Selection has not been influenced by expectations that blacklisting will shift the mechanisms. In our setting, this means that conditional on observed blacklisting criteria, the possibility of increased density of field inspections, higher efforts in registering landholdings into CAR, and tighter restrictions on public credit does not affect the selection into the Blacklist. Note that this assumption is is different from the expectation that the blacklisting policy as such may alter these mechanisms. (2) Changes in the mechanisms have the same

1 Cisneros et al. (2015a) falsely use η_t instead of $\Delta\eta_t$ in equation 3.6.

effect on deforestation in districts where blacklisting has affected the mechanisms and in districts where it has not affected the mechanisms. The second assumption could theoretically be violated, for example, if field inspections in blacklisted districts would somehow have been of a different nature than inspections in other districts. To gauge the potential mechanism effects, we estimate the NATT with the mechanism effect blocked, i.e., setting field inspections, CAR registrations, and public rural credit of blacklisted districts to their counterfactual levels. The difference between the overall average treatment effect measured in Table 3.2 and the NATT is the joint effect of the three mechanisms.

The implementation involves three steps (note that we use a hat to indicate estimated coefficients and a tilde to indicate predicted values):

1. **Estimating counterfactual mechanism values.** Closely following Ferraro and Hanauer (2014b) we use two alternative approaches to estimate counterfactual mechanism values. (a) replacing the real values of the mechanisms after treatment with the values of the paired matched control districts (paired values). (b) using the matched control sample and estimate the influence of our covariate set on each mechanism (estimated values, see equation 3.7 below).[2]

$$\Delta M_{it|B_i=0} \;=\; +\,\Delta \mathbf{X}'_{it}\,\gamma^1 + \,\mathbf{Z}'_i\,\delta^1 + \,\Delta\eta^1_t + \,\kappa^1_s + \,\Delta u_{it} \qquad (3.7)$$

   We predict counterfactual mechanism values ($\widetilde{M}_{it}$) with the point estimates ($\hat{\gamma}$,$\hat{\delta}$) for blacklisted districts, but only for the years after blacklisting had started. Therefore the new predicted mechanism vector has the properties: ($\widetilde{M}_{it}|B_{it} = 0) = (M_i t|B_{it} = 0)$. This step creates mechanism values that would have been realized in blacklisted districts if blacklisting had not influenced the mechanism policies.

2. **Counterfactual deforestation values.** Under the second assumption, we estimate how much deforestation would have occurred in blacklisted districts if blacklisting had not influenced mechanisms. We first estimate the influence of the real mechanism values on the subsample of the blacklisted districts after blacklisting as follows[3]:

$$\Delta \ln D_{it|B_{it}=1} \;=\; +\,\Delta \mathbf{X}'_{it}\,\gamma^2 + \,\mathbf{Z}'_i\,\delta^2 + \,\Delta \mathbf{M}'_{it}\,\lambda + \,\Delta\eta^2_t + \,\kappa^2_s + \,\Delta u_i (3.8)$$

   With the point estimates ($\hat{\gamma}^2$,$\hat{\gamma}^2$,$\hat{\lambda}$) and the counterfactual mechanism values ($\widetilde{M}_{it}$) we predict the counterfactual deforestation levels ($\widetilde{D}_{it}$) for the blacklisted districts after blacklisting. Under the assumptions made

2 For convenience we do not change the letters of the residual terms. Correct would be the use of different letters for all equations.

3 Superscript numbers above coefficients indicate different estimates for the equations 3.7, 3.8, 3.9

above, the counterfactual deforestation represents the level of deforestation had there been no change in field inspections, CAR registrations and public rural credits as a result of blacklisting.

3. **Estimating the NATT involves** re-estimating model (3) from Table 3.2 using the counterfactual deforestation levels estimated in the previous step as follows:

$$\Delta \ln \widetilde{D}_{it} \;=\; + \beta\, \Delta B_{it} + \Delta \mathbf{X}'_{it}\, \gamma^3 + \mathbf{Z}'_i\, \delta^3 + \Delta \eta^3_t + \kappa^3_s + \Delta u_{it} \quad (3.9)$$

After blocking the influence of blacklisting on the mechanisms, we expect to find counterfactual levels with decreased fines, lower CAR registrations and higher amounts of public credit, relative to the observed (real) values. Using the counterfactual mechanism values, we expect to predict higher counterfactual deforestation rates than the observed rates of deforestation. Assuming a high share of the mechanism effects (MATT) within the overall effect of blacklisting (ATT), we expect to find lower and even insignificant estimates of the remaining NATT.

Results from estimating equation 3.7 are presented in Table B.8. Statistics on the real and counterfactual values of the blacklisted districts after 2007 are presented in Table 3.5. The mean and standard deviation of the real mechanism values are reported in column (1). The two approaches used to determine counterfactual mechanism values do not yield fully consistent results. With paired values, we unexpectedly find counterfactual rural credit levels to be lower than with blacklisting. With estimated values, counterfactual environmental fine levels are higher than with blacklisting, contrary to our expectation. Differences vis-à-vis observed values with blacklisting are small, however, and the estimated counterfactual deforestation levels (equation 3.8) are higher than observed deforestation for the blacklisted districts independent of the approach used to estimate counterfactual mechanism values.

The results of the mechanism analysis are shown in Table 3.6. In column 1 we repeat the result of our ATT estimation in Table 3.2 (column 3). In columns 2-5 we add mechanism effects as explanatory variables to the regression. In order to avoid reverse causality between deforestation and environmental fines we use lagged time values for environmental fines. Differences in the mechanism values between blacklisted and non-blacklisted matched district are small (Table 3.5). The mechanism coefficients thus tend to have small and insignificant coefficients. This exercise does not alter the estimate of the blacklisting effect vis-à-vis the results in Table 3.2. The coefficient remains at a 10% significance level with a negative estimate close to -0.3.

Columns 6 and 7 show results after using the paired and estimated mechanism counterfactuals, respectively, to estimate counterfactual deforestation. The new estimates represent the NATT of blacklisting, which is still negative and significant, but somewhat smaller than the ATT estimated in Table 3.2. The joint effect of the three potential causal mechanisms we studied thus seems to

Table 3.5: Statistics on counterfactual mechanism values

Variable	Statistic	Observed values	Counterfactual paired values	Counterfactual estimated values
Lagged No. of	Mean	58.15	47.73	71.16
environmental fines	St.dev.	(70.58)	(72.85)	(117.94)
Car area coverage [%]	Mean	19.40	14.94	17.15
	St.dev.	(18.81)	(16.52)	(14.69)
Lagged No. of	Mean	26.61	20..21	653.20
environmental fines	St.dev.	(29.24)	(25.92)	(6528.55)

*N*ote: Statistics show observed (real) and counterfactual values estimated as described in section 5 on the blacklisted districts between the years 2008 to 2012. Paired values are adopted form the corresponding paired matched controls district of each blacklisted districts. Estimated values are based on estimations of mechanisms on the covariates.

be relatively small compared to the other two potential impact channels (administrative disincentive and reputational risk) discussed above.

3.7 DISCUSSION

We have found a robust and significant negative effect of district blacklisting on deforestation. As outlined in the introduction, there are several theoretical pathways to explain this result, including administrative disincentives, reputational risks, and external support/pressure from both government and non-governmental organizations. We have implemented an innovative econometric method to block three potential causal mechanisms related mainly to the theoretical impact channel "external pressure/ support" that have shown to be relevant factors in Brazil's overall strategy to reduce Amazon deforestation [5, 12]. Namely, environmental fines, geo-referenced land use registrations, and public credits. As causal mechanisms, however, these factors turned out to be of marginal importance in explaining deforestation reductions in the blacklisted districts. Clearly, this does not undermine the important role that any of these policy instruments plays in Brazil's national strategy to reduce tropical deforestation. In fact, fine-based law enforcement, land registry (CAR), and rural credit policies, including the credit restriction imposed by the Brazilian Monetary Council in 2008 (Assunção et al., 2013b), apply to all districts in the Brazilian Amazon. They may only have played a more important role in blacklisted districts if blacklisting caused quantitative or qualitative shifts in their implementation. We emphasize that our mechanism analysis only captures quantitative changes in these policy components. Administrative disincentives and reputational risk in addition to qualitative changes in external support/-pressure channels thus remain potentially effective drivers behind the effect of the Brazilian Blacklist that could be explored in further research. Moreover, ad-

Table 3.6: Net average treatment effect of blacklisting

Dependent	Δ ln Deforestation					Δ ln counterf. Deforestation	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ Blacklisted$_{it}$	-0.297*	-0..299*	-0.298*	-0.297*	-0.299*	-0.224**	-0.211**
	(0.155)	(0.156)	(0.154)	(0.155)	(0.156)	(0.100)	(0.103)
Δ ln No. of fines$_{it-1}$		0.003			0.003		
		(0.028)			(0.028)		
Δ CAR area cover$_{it}$			-0.042		-0.043		
			(0.491)		(0.155)		
Δ ln Total rural credit$_{it}$				0.010	0.010		
				0.060	(0.060)		
Year and state effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Time-invariant controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Time-variant controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1000	1000	1000	1000	1000	1000	1000
Cluster	76	76	76	76	76	76	76
Adj. R-squared	0.258	0.258	0.258	0.258	0.256	0.308	0.313

*N*ote: The table reports first difference estimates. Columns 1-5 use the change in log of yearly newly deforested area as the dependent variable. Columns 6 and 7 use the change in log of counterfactual deforestation in the blacklisted observations. Counterfactual deforestation in column 6 is constructed form paired control matches. Counterfactual deforestation in column 7 is estimated based on the set paired control matches. Standard errors, clustered at district level, are reported in parentheses. Time-invariant and time-variant controls include first differences of the variables reported in S2 Table. Observations are selected by a 1:1 closest neighbor matching using inverse-variance weights, with replacement. * ,** denote significance at the 10/5% level.

ditional causal mechanisms may exist that we were not able to capture because of data limitations. These include, for example, increases in external pressure on embargoed producers through so called compliance commitments issued by public prosecutors.

Like any quasi-experimental evaluation, our analysis remains prone to unobservable bias. We control for a wide range of factors comprising physical, socio-economic and political indicators. Owing to limited common support, this bias could not be fully corrected for by matching, which is why we only rely on matching as a pre-processing technique (Ho et al., 2007). We rely on the 'weaker' assumption of parallel time trends and control for all unobserved fixed effects.

The selection of districts to the Blacklist is endogenously determined by deforestation which could violate the parallel time trend assumption. Without treatment, the decrease in deforestation rates of blacklisted districts could have materialized at a much slower pace than of control districts (see Figure

B.5 panel a). The probable bias from using inadequate controls with lower deforestation and more rapid drops deforestation rates would lead us to under rather than overestimate the conservation effect of blacklisting.

A common evaluation pitfall that would also violate the parallel time trend assumption and lead us to overestimate the Blacklist effect is the "Ashenfelter's or pre-program dip". It can occur if selection is affected by unusual pre-program changes in the outcome variable (Heckman and Smith, 1999). In our case, a pre-Blacklist peak in deforestation could hypothetically have resulted in a selection of districts that would have exhibited much faster decreases in deforestation - even in the absence of blacklisting - than any potential control district (see Figure B.5, panel b). Whereas we cannot completely rule out such a phenomenon, we argue that it is unlikely to play a major role in explaining our findings. First, because we control for past increases in deforestation rates in our matching exercise and formally tested the hypothesis of equal pre-program deforestation trends in treated and control districts. Secondly, because the Blacklist was enacted five years after average deforestation had peaked in the blacklisted districts (see Figure 3.3). In the two years prior to the publication of the blacklist, deforestation trends had instead been rather stable. And thirdly, the blacklisted districts have been leading deforestation rankings even prior to our observation period. Hence, and as supported by our placebo treatment analysis (Table B.7), the substantial drop in average forest loss in these districts after 2008 can hardly be attributed solely to normalization after an unusual peak.

We are thus confident that our analysis correctly identifies the Blacklist as an environmental governance measure that made a substantial complementary contribution to bringing deforestation down in the Brazilian Amazon region.

3.8 CONCLUSIONS

In this study we have used a quasi-experimental evaluation design to gauge the potential contribution of district blacklisting to the drop in deforestation rates in the Brazilian Amazon. Blacklisting has previously been used and studied in other environmental governance contexts, such as pollution control (McGee and Gavent, 2010).

We find that the average effect of blacklisting on deforestation in blacklisted districts ranges between roughly 13-36% considering standard errors (see Table 3.2, model 3). This corresponds to an absolute reduction in deforestation of 600-6,750 sq. km (4,022 sq. km on average) from 2008 to 2012. This is far less than the 59,511 sq. km cumulative conservation effect of improved field-based enforcement calculated by Assunção, Gandour, and Rocha (2013a) for the period from 2007 to 2011. However, it is more than the amount of avoided deforestation (2,700 sq. km) that Assunção et al. (2013b) attribute to the credit restrictions that were enacted in 2008. Compared with Arima et al. (2014) who report a range from 2,304 to 11,653 sq. km, our ATT and NATT estimates lie at the lower end and are thus also much more conservative than the 11,396 sq. km estimated by Assunção and Rocha (2014).

In other words, between 2008 and 2012, the decision to bolster the Brazilian anti-deforestation campaign by district blacklisting has conserved an amount of forest cover that is almost equivalent to the current average annual forest loss, i.e., 4,848 sq. km in 2014 according to official INPE statistics.

At the federal level, the incremental administrative costs of maintaining the Blacklist have probably been low given that no significant additional governance and implementation structure had to be put in place. However, the Blacklist has reportedly induced a substantial amount of local level transaction costs and operational expenses by supporting NGOs and state-level government organizations. Putting a price tag on the Brazilian blacklisting experience therefore is not a straightforward exercise.

Relating to the effectiveness of blacklisting, this also depends on factors outside the control of the federal government. Here, one must also concede that the policy has shortcomings. For example, some states have not yet fully developed capacities to implement CAR registries at relevant scales. In such cases it seems unrealistic that districts can achieve the 80% CAR target to be removed from the list, which could undermine incentives to engage in alternative district level efforts to reduce deforestation. Conversely, on the positive side, the Blacklist has also inspired state-level initiatives, such as the Green Municipalities Program (PMV) in the state of Pará, to reward good forest stewardship in districts that voluntarily comply with the criteria for removal from the Blacklist.

Given the scarce evidence on the effectiveness of transparency and accountability measures in conservation, our results should encourage experimentation with blacklisting as a complementary forest conservation measure. Clearly, a country's administrative structure is likely to affect outcomes in significant ways. For example, Brazilian districts (i.e. municipalities) have much less legal autonomy in environmental policy than in the more decentralized governance structure of other tropical forest countries, such as Indonesia (Luttrell, Resosudarmo, Muharrom, Brockhaus, and Seymour, 2014; Burgess et al., 2012). The effectiveness of the diverse potential impact channels of blacklisting may thus differ substantially depending on the ability of local stakeholders to organize themselves towards the goal of being removed from a Blacklist.

From the government's point of view, as well as in the context of an international mechanism to Reducing Emissions from Deforestation and Degradation (REDD+), blacklisting appears to be a low-cost and no-regret option to increase compliance with existing forest law. Overall costs to land users clearly depend on the kind of action that blacklisting evokes at the local and district level.

4

CONSERVATION INCENTIVES FOR PROTECTED AREA MANAGEMENT

ABSTRACT

Incentive-based conservation programs are a promising approach to preserve remaining tropical rain forest cover and guarantee the supply of important environmental services. Pilot projects, often in the context of REDD+ schemes, focus on protection areas with low economic potential and low demographic densities. The success of such projects is judged by their additional conservation effects beyond the effect of the protected area status. The ability of additional conservation effects depends on the economic context of protected areas. We explain context dependent outcomes with a conceptual framework of protected area instruments. When evaluating the impact the methodological challenge arises, of how to separate the effect of the incentive component from the impact of the protected area status. Our analysis focuses on the Bolsa Floresta Program (BFP) which offers community support for generating sustainable income activities and provides payments for environmental services. The program includes over 9,000 inhabitants in 15 out of 61 multiple-use reserves in the Brazilian state of Amazonas. In order to identify the effect of the reserve status and the effect of the incentive scheme, we use yearly information on a grid-based data structure. Potential control grid cells lie in extractive reserves not covered by the BFP. The selection bias of reserves' and BFP's siting is minimized by matching treated and control grid cells on characteristics of temporally and spatially lagged deforestation and other covariates. Though, no program wide additional conservation effects can be detected, low heterogeneous effects are prevalent. For protected areas, under high economic pressures from outside but traditionally low deforestation rates, the BFP had significant positive conservation effects beyond the protection status. In contexts of high economic pressures and traditionally higher deforestation rates the effects opposed and the BFP lead to very low but significantly negative conservation outcomes. Our results confirm that incentive projects can increase the conservation capacity of protected areas where design elements are adapted to the context of deforestation pressures.

4.1 INTRODUCTION

Protected areas (PAs) play a key role in the preservation of biodiversity-rich landscapes and forest carbon. Using counterfactual evaluation methods,[1] strong evidence underscores the effectiveness of PAs in conserving forests worldwide (Joppa and Pfaff, 2010; Nelson and Chomitz, 2011), and particularly in Brazil (Soares-Filho et al., 2010). Brazil's foundation of forest conservation is its PA system. 2.6m sq. km, or 52% of the Brazilian Legal Amazon, are covered by PAs and indigenous territories - an increase from 34% in 2004 (see Figure 4.1).[2]

Annual deforestation in Brazil fell dramatically by some 80% during 2004-12 (from 27,000 to 5,000 sq. km), while leveling off hereafter (Hansen et al., 2013). This is attributed to both economic and political factors (Canova and Hickey, 2012; Hargrave and Kis-Katos, 2013). In 2004, the Plan to Combat Deforestation in the Amazon launched and was key to curbing deforestation in subsequent years. Quasi-experimental evidence underlines the important contribution of Brazilian policies (Arima et al., 2014; Assunção et al., 2012; Maia et al., 2011). Furthermore, the 2012 new forest law, and investments in infrastructure, mining and hydropower remain imminent threats (Ferreira et al., 2014; Miccolis et al., 2014; Soares-Filho et al., 2014).

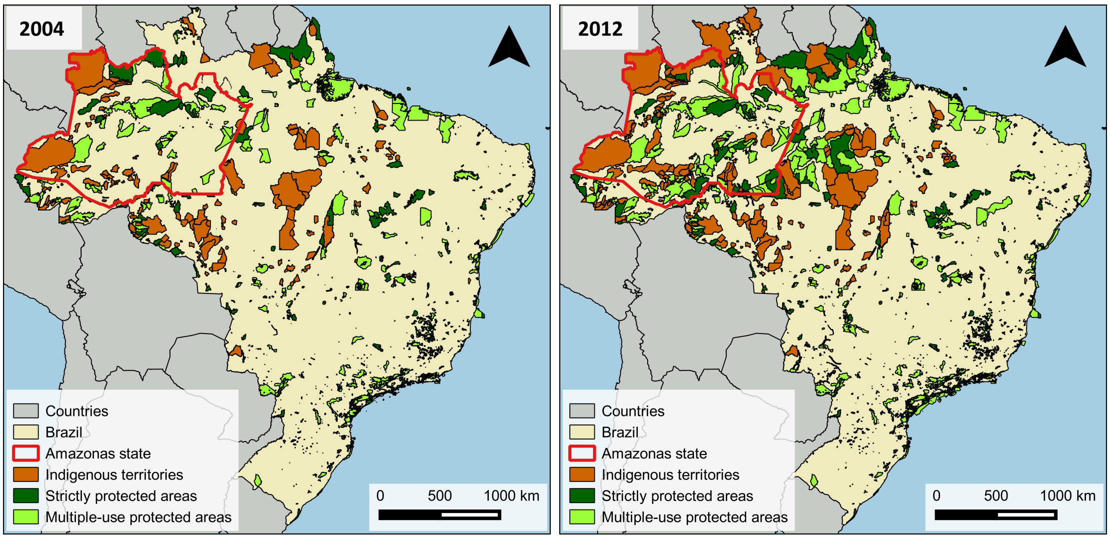
It has also been argued that the sole focus on command-and-control measures (PA, forest-law enforcement) in Brazilian forest conservation policies will not be sustainable: some carrots (i.e., incentives) would have to be added to pre-existing sticks (i.e., disincentives) to avoid excessive welfare losses for land users, which could lead to a political backlash for conservation (Nepstad et al., 2014). The question then becomes how to design a policy mix of carrots and sticks so as to adequately balance environmental impacts with equity objectives (Börner, Marinho, and Wunder, 2015b).

In that sense, payments for environmental services (PES) have become an increasingly important environmental management instrument, including occasionally in PAs (Honey-Rosés et al., 2011; Wunder et al., 2008). In Latin America, Brazil has been a latecomer in PES development, but has since picked up markedly, led by watershed schemes in the Atlantic Forest biome and carbon initiatives in the Amazon (Pagiola, Carrascosa von Glehn, and Taffarello, 2013). A systematic review of rigorous forest-based PES evaluation studies worldwide showed relatively low additionality (Samii et al., 2014). But because of its demanding methodological selection filter, this study ended up with a very small and arguably biased sample: barely a dozen of studies on Costa Rica and Mexico made the threshold. A new collection of conservation impact

1 Ferraro (2009) describe counterfactual thinking: "Impact evaluations assess the degree to which changes in outcomes can be attributed to a program, policy, or intervention [...]. An answer requires knowing what outcomes would have looked like in the absence of the intervention".

2 The Brazilian National System of Protected Areas (Sistema Nacional de Unidades de Conservación, SNUC) classifies two types of protected areas, namely sustainable use reserves (*áreas do uso sustentavel*) and integral protected reserves (*área de proteção integral*), which are also denoted as multiple-use reserves and strictly protected areas.

Figure 4.1: Protected areas in Brazil



*N*ote: Note: The Figure depicts the protected areas in Brazil founded until August 2004 (left) and August 2012 (right). Multiple-use reserves are depicted light green. Strictly protected areas are depicted in dark green. Indigenous territories are depicted in orange. See also video (`ttps://www.youtube.com/watch?v=Tdgth77U5Ig`) on the evolution of protected areas in Brazil created by Elías Cisneros and Johannes Schielein in 2014.

studies adds rigorous studies with a somewhat more optimistic outlook on the effectiveness of PES (Börner et al., 2016).

With respect to PAs and PES combined, Honey-Rosés et al. (2011) showed for a case in Mexico that this mix had high conservation effects vis-à-vis unprotected areas, but the authors could not distinguish the effect of the PES introduction from that of the pre-existing PA. To our knowledge, only Clements and Milner-Gulland (2014) empirically analyzed the additionality of conditional payments within PAs: Conditional payments in Cambodia reduced deforestation rates significantly within the treated communities, as well as increasing the wellbeing of participating households, compared with a control group of PA residents that had not received payments.

The Bolsa Floresta Program (BFP) aims to foster forest-friendly development in 16 multiple-use reserves (in Port.: *áreas de uso sustentável*), of the Amazonas state. The multiple-use reserves are a subcategory of the PA system in Brazil that allows traditional resident populations to pursue livelihood strategies with benign environmental impacts inside the PA. BFP includes a PES program for families within PAs, the payment of which is conditional upon compliance with rules that are slightly more restrictive than multiple-use reserve rules (Börner et al., 2013). Incentives also include more traditional integrated conservation and development (ICDP) components, such as social services (health, education), alternative forest-friendly productive investments, and organizational support. The BFP is thus an example *par excellence* of the aforementioned policy mixes, adding carrots to preexisting sticks.

The main environmental effectiveness question is thus to what extent the incentives provided through BFP have conserved additional forest, beyond what would have been saved solely from having these areas under multiple-use reserve protection. We investigate the conservation effects of the BFP across 15 PAs covered by the program up to 2012.[3] We use quasi-experimental empirical methods and time series data. We consider the program's effects on remotely sensed deforested areas, forest degradation, and fire incidence, respectively. Our analysis considers the full program over 10m hectares, but can only measure the initial effects of the program, as the program started in 2008 and data for this analysis is available from 2003 to 2012.[4] Hence by the nature of our analysis, we cannot capture impacts on behavioral or attitudinal changes of participants, nor the development targets set out by the program. Nonetheless, we extend the analysis from the usual average changes at the reserve level and investigate forest changes within different economic contexts both within around the reserves.

Section 4.2 gives an overview of the various program components of the BFP. Section 4.3 describes a range of specific policy instruments and their mechanisms available to reserve managers and relates these to the BFP. Section 4.4 deduces context depended hypotheses of program's impact. The empirical strategy for the evaluation of the program is described in section 4.5. Data sources and their processing are depicted in section 4.6. Section 4.7 presents the results, whereas 4.9 discusses caveats and section 4.10 concludes.

4.2 THE BOLSA FLORESTA PROGRAM

The Bolsa Floresta Program (BFP) is one of Latin America's largest PES programs, with over 9,000 participating families and an area of more than 10m ha. The program started in 2007 as an initiative of the Secretary for Sustainable Development of Amazonas. The program is under the patronage of the Amazonas Sustainable Foundation (*Fundação Amazonas Sustentável*, FAS). The long term goal of the BFP is to protect its reserves against external and internal economic pressures and to increase residents' welfare. The program is implemented within 16 out of 61 multiple-use reserves within the state of Amazonas (see Figure 4.2).[5]

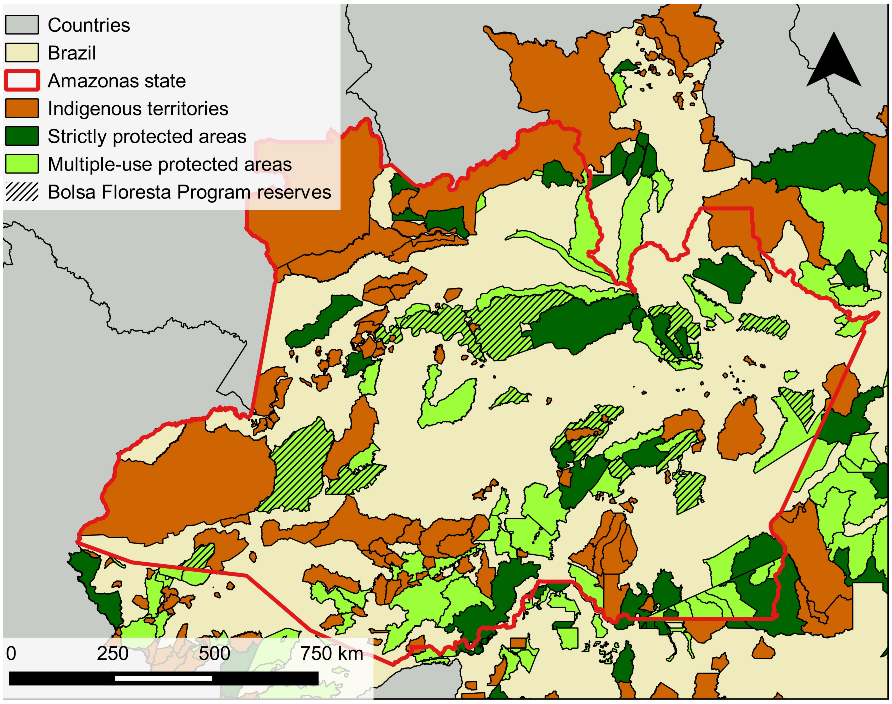
The BFP aims to increase environmental awareness and to build conservation alliances with the inhabitants of the PAs. In order to achieve its goals, the BFP includes payments to participating households, community support for sustainable economic income activities as well as investment in social infrastructure and organizational support. The program is organized into four com-

3 On reserve, the Environmental Protection Area (APA) Rio Negro was divided into two reserves, resulting in the 16 reserves under the BFP today.

4 The National institute of Spatial Research (*Instituto Nacional de Pesquisas Espaciais*, INPE) provides full access to its deforestation database through 2015 as of the date of this study (April 2016), but it was not feasible to process the high resolution data (30 by 30 meter) beyond 2012.

5 For the empirical analysis 15 reserves are considered as covered by the BFP at the end of our timeframe in 2012.

Figure 4.2: Protected areas and the Bolsa Floresta program



Note: Figure shows the extent of protected areas until 2012. Few reserves were created after 2012, mainly by splitting already existing reserves, including one reserve covered by the BFP.

ponents. The PES component (Bolsa Floresta Familia), a social development component (Bolsa Floresta Social), a community governance capacity building component (Bolsa Floresta Associação), and a sustainable income component (Bolsa Floresta Renda).

The first component to be rolled out to all participating families is the Bolsa Floresta Familia component *(family component)* - a conditional payment to individual participating families for environmental services. All families living within the targeted reserves for longer than two years can participate. [6] Each family receives a monthly payment of 50 *reais*,[7] paid to the female household head or wife. The disbursement of the payments starts after signing a commitment to comply with the rules of the BFP. The restrictions of the BFP advance beyond the reserve rules, and include the prohibition of new clearing in primary forests and the attendance of all children in schools. Participation rates range from 70 to 100%, today covering over 9,400 families (Newton, Nichols, Endo, and Peres, 2012).

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6 Further stipulations require that beneficiaries be older than 18 years of age, to discourage early childbirth. Widowers or fathers who care for school age children are also permitted to participate.

7 1 *real* converts to €0.51 or $0.66. 50 *reais* equals €25.61 or $32.90 based on average exchange rates of 2012. (`https://www.imf.org/external/np/fin/ert/GUI/Pages/CountryDataBase.aspx`)

Investments to the local infrastructure are realized through the Bolsa Floresta Social component *(social component)*. Through this channel the BFP conducts basic service infrastructure investments within the communities of a reserve. Yearly investments, amounting up to 350 *reais* per family, flow into the establishment of electricity, water supply, sanitation and communications systems (Börner et al., 2013).

The Bolsa Floresta Associação *(association component)* component aims to support local associations and collaborations among communities and partnerships with other organizations and local governments. The program promotes meetings within communities and reserves in order to build leadership capacity and promote participation, secure social justice and the interests of all inhabitants. The annual grants amount to 10% of all Bolsa Floresta Familia Payments, and can be used autonomously by the communities (Börner et al., 2013; FAS, 2013; Newton et al., 2012).

The BFP expanded after the first year with its Bolsa Floresta Renda *(income component)*. This component aims to foster sustainable forest friendly production systems. Each community within the reserve decides independently which investments to conduct. Technical assistance on new production systems is provided by FAS staff. The most frequent investments include poultry, nuts, natural oil production, agroforestry, fruit production, and tourism. Annual investments of approximately 350 *reais* per family aim to increase the productivity of supported activities while introducing new income generating opportunities (Newton et al., 2012). The idea is that the increased productivity shifts families' income sources towards more forest-friendly activities. The master thesis of Swartz (2015) analyzes the income component with data on over 200 households living on both banks of the *Rio Negro*.[8] On both sides of the river, households participate in the BFP, though by the time of the survey the income component had only started on the southern side of the river. For the short time period households benefited from the income component, the study could not find a statistically robust difference in income or asset levels between both groups.

The BFP is therefore composed of a PES to the individual families of the reserves, complemented by community development investments. The social, association and income components are added on top of the voluntary participation of inhabitants in the family component. We define this type of conditional payments as a PES+ program where additional investment components are added to a PES scheme. The total support provided by the BFP in the Rio Negro Sustainable Development Reserve was calculated at 1,413 *reais* per household per year (FAS, 2013). Newton et al. (2012) measured an average annual BFP support to families of 1,300 *reais* in the multiple-use reserve of Uacari.

8 The RDS Rio Negro reserve on the southern river bank and the APA Rio Negro reserve on the northern river bank.

4.3 POLICY INSTRUMENTS OF RESERVE MANAGERS AND THE BFP

Before turning to the hypotheses on BFP's impacts on forest conservation, it is important to first compare the program with the policy instruments commonly available to PA managers. Available instruments to inhabited multiple-use reserves can be grouped into monitoring and enforcement, integrated conservation and development programs (ICDPs), conditional payments or payments for environmental services (PES), and the establishment of residential monitoring and enforcement.

The instruments aim to reduce forest-harming outcomes such as deforestation, logging, game hunting, and over-exploitation of other natural resources (such as fish stocks). Although these activities produce economic gains, they can also cause economic and social risks. From the perspective of individual households, activities that harm the forest are often more profitable than those that conserve it. If they were to implement forest-friendly activities, they would thus face an opportunity cost.

As both PA residents and outsiders can obtain economic gains from forest-harming activities, PA managers and conservation non-governmental organizations (NGOs) often try to reduce the opportunity cost of conservation. Residents have direct interests in the forest resources available to them. Individuals living outside of reserves have interests on the forest resources as they also represent possible economic gains, though outsiders have to compete or collude with residents either by migration or invasion. Any instrument has its advantages and disadvantages in reducing the opportunity costs for residents and for outsiders to conserve the forest.

A *PA monitoring and enforcement infrastructure* is the first instrument available to their managers. The employment of rangers is typically used in strictly protected wildlife and natural reserves such as parks and biological refuges. Both enforce the rule of law by detecting violations and issuing fines to perpetrators. These activities increase the risk of detection and, coerced through fines, increase the costs of forest harming activities and therefore reduce their benefits. This instrument serves to reduce internal and external opportunity costs for conservation.

The BFP does not execute direct monitoring and enforcement activities. Nonetheless, monitoring is conducted indirectly by the staff members of the FAS. Staff members frequently visit communities for the subscription of new families to the program or for technical assistance to the supported production lines (income component). Local violations against the BFP rules can easily be detected and reported to the FAS headquarters. The BFP issued warnings to 4.6% of participants through 2013 (Börner et al., 2013). Although we do not have any information on the suspension of payments as a direct enforcement instrument, it is credible to assume that the expectation of punishment exists.

Integrated conservation and development programs (ICDPs) implemented by the reserve management and its partners target internal opportunity costs within multiple-use protected areas. They aim to shift inhabitant's income sources towards sustainable forest friendly activities and to increase welfare. The policy assumes that inhabitants lack technical knowledge or investment capacities to shift into sustainable income sources. The idea of ICDP investments is that once forest friendly production systems are installed, they crowd out forest-harming activities. Nonetheless, following Weber, Sills, Bauch, and Pattanayak (2011) and Bauch, Sills, and Pattanayak (2014) gains in production efficiency can have two diverging effects: (1) they can lead to increased production and divert labor away from forest-harming activities and into forest-friendly activities, whose rate of return has increased; (2) they can result in reduced labor needs for production in forest-friendly activity, freeing labor for forest-harming activities. Which of these effects predominates depends on a multitude of factors among which are: access to labor markets, markets to sell products, leisure preferences, etc. The outcomes on the opportunity costs of conservation are ambiguous and will depend on the specific context.

The BFP incorporates a variety of investments with its social, association and income components. Investments focus on infrastructure (water, sanitation), education, healthcare and forest-friendly production systems. The latter is most related with the idea of shifting residents' income base toward forest-friendly production. The program components fit the definition and goals of ICDPs. The impact of the BFP on the opportunity costs of conservation will first depend on the labor productivity of the supported production practices. But more importantly it will depend on each community's and reserve's context: How high the gains from forest harming activities are and if outside labor and goods markets exist.

Conditional payments or payments for environmental services (PES) are the third conservation instrument available to protected area managers. In the case of protected areas, the environmental service provided can be defined as the compliance with the reserve rules or inclusion of additional conservation rules (Wunder, 2005). Irrespective of both cases the success of PES generally depends on whether the payments offset the opportunity costs of conservation and on the existence of a credible monitoring and enforcement system.

The BFP's focus lies in the participation of families within their conditional payments component (family component). Families receive monthly 50 *reais* per month conditioned on their compliance with the BFP rules. As described in the previous section, these rules are somewhat stricter than the rules of the PAs. Börner et al. (2013) and Newton et al. (2012) show that payments are most probably sufficient to offset the opportunity costs of conservation. As described above, the conditionality condition of the BFP is fulfilled with an indirect monitoring and enforcement via warnings of suspension from the program. In consequence, the PES component of the BFP reduces the opportunity costs of conservation and moves residential activities towards forest conservation.

Monitoring & enforcement efforts by residents themselves can be induced informally by conditional payment schemes or ICDPs (Robinson, Albers, Ngeleza, and Lokina, 2014). Violations against protected area rules (e.g., logging, clear cut forest patches, etc.) are very difficult to attribute to specific actors. Violations are mostly never observed as they occur, but with a time delay. This makes it often impossible to attribute illegal outcomes to specific residents or non-residents, especially in PAs where clearly defined property rights and liability rules are missing. However, violations are attributable to communities and reserves where they are sighted. PA management could choose to hold residents collectively responsible, and suspend single communities or reserves from the PES program. In consequence, participants complying with the rules of the PES will unjustly face a risk of losing their payments due to others' non-compliance. The risk of losing future payments leads participants to monitor and enforce compliance (1) among their fellow residents and (2) against outsider invasions and illegal settlers.[9] Both forms of control, will take increasing effect with an increasing number of participants holding positive expected benefits from compliance. Accordingly, internal opportunity costs of conservation of residents decrease due to the additional social costs of non-compliance and the external opportunity costs of outsiders increase as the risk of detection and denunciation by residents increases. It is important to notice that residential monitoring can only develop if collusion with outsiders does not create higher benefits than the PES and associated benefits.

The BFP effort unintentionally induces monitoring and enforcement in its targeted reserves. The regular visits of staff members to the benefiting communities allow the FAS to observe changes in compliance with the BFP rules. The FAS cannot attribute violations to individual families and has to be careful to maintain the trust built with the residents. Nonetheless, the BFP could decide to cease or to fadeout their support to communities with continuous violations. The high participation rates suggest that communities have a strong interest to maintain their status within the BFP. Non-participating families must subordinate to the dominant compliance strategy of participants. The BFP's explicit long-term goal is to build what they call 'conservation alliances' with the reserve dwellers. The FAS installed radio telephones in all reserve centers which facilitate communication and can be used to assure timely report of invasions from outsiders. Thereby, the conditions for an induced monitoring and enforcement activity by residents are present within BFP reserves.

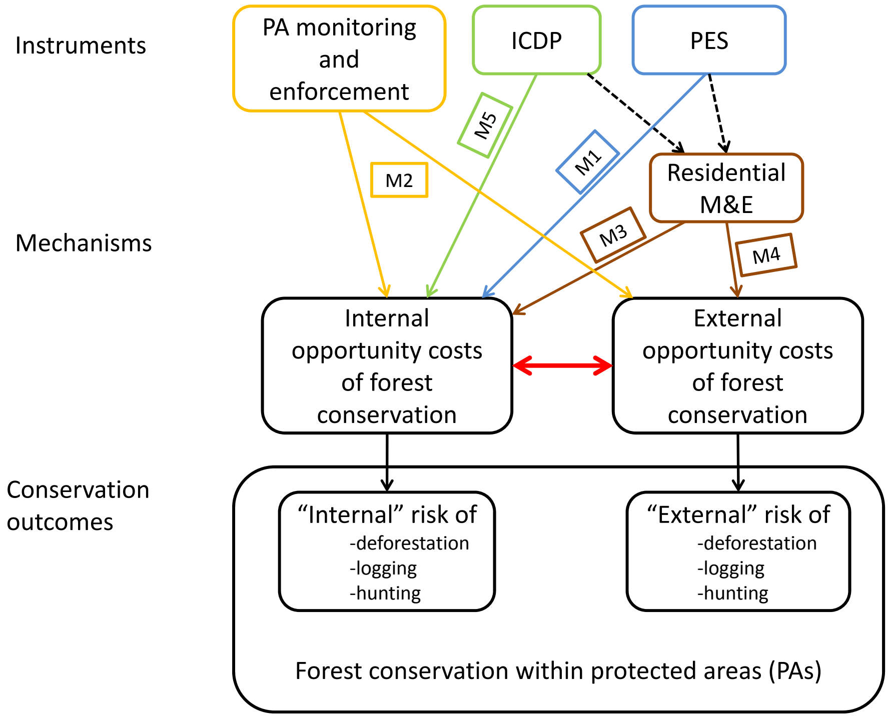
The link between instruments and conservation outcomes are depicted in Figure 4.3. These mechanisms are:

M1 **Compensations:** The conditional payments to families at least compensate the opportunity costs of additional forest conservation. In consequence forest-harming activities by residents decrease, which reduces deforestation and forest degradation.

M2 **Indirect monitoring:** PA monitoring activities are introduced through the execution of the BFP. The increased probability of punishment de-

9 Enforcement could be exerted by social sanctions. However, if there are gainers from non-compliance and losers from compliance, a Coasean bargaining solution among residents might exist.

Figure 4.3: Instruments and mechanisms for protected area management



creases the profitability of forest harming activities, thereby reducing the incentives for deforestation by residents.

M3 **Mutual monitoring:** Compliant residents' interest in maintaining the benefit flow of the BFP translates into a mutual monitoring and social sanctioning of non-compliant residents. This increases the costs of forest harming activities resulting in decreased incentives for deforestation by residents.

M4 **Conservation alliance:** Compliant residents' interest in maintaining the benefit flow of the BFP translates into an increased monitoring and reporting of non-residents' invasions - under the condition that participants receive higher gains from the program than from collusion with outsiders.

M5 **Forest friendly production:** Investments into forest friendly production lines divert production factors away from forest harming activities - under the conditions that saved resources are used for increased production rather than increased leisure time and a sufficient market demand for the goods produced exists.

4.4 INTERVENTION HYPOTHESES

The mechanisms of decreased opportunity costs (M1), increased expected punishment (M2) and increased mutual monitoring (M3) lead us to our first hypothesis:

H1 The Bolsa Floresta Program has a positive impact on forest conservation.

Compensations (M1), indirect monitoring (M2) by the FAS and mutual monitoring (M3) lower the opportunity costs of conservation to BFP participants in the treated reserves. In consequence, forest conservation is increasingly rewarding to participants and forest conservation outcomes improve.

The average effect of the BFP program (H1) varies across the targeted 15 reserves. Some protected areas are under higher deforestation pressures than others. All mechanisms are dependent on the context, specifically on the opportunity costs of forest conservation residents are facing and on the opportunity costs non-residents have of staying out of reserves. We operationalize the mechanisms by considering the internal and external deforestation pressures of each reserve. The internal deforestation pressure relates to the opportunity cost of conservation residence face. The external deforestation pressure relates to the opportunity costs outsiders face. Distinguishing between high and low external or internal pressures we relate to the following four hypotheses:

H2.1 In a context of **low internal and low external pressures** on multiple-use reserve forests the BFP has no effect on forest conservation: Such contexts have traditionally few forest-harming activities inside and outside of reserves, compliance levels of residents and outsiders are high. The mechanisms compensation (M1), indirect monitoring (M2), and mutual monitoring (M3) are unable to increase forest conservation because deforestation is too low to be lowered further. Similarly, conservation alliances (M4) are unable to reduce the threat from outside invasions as there are no such invasions. Forest friendly production (M5) is unable to crowd out forest harming activities as activities are already forest friendly.

H2.2 In a context of **low internal and high external pressures** on the reserves' forests the BFP increases forest conservation. Where residents have low economic opportunity costs of conservation and compliance levels are high the mechanisms of indirect monitoring (M2) and mutual residential monitoring (M3) are unable to further increase compliance behavior. But outsiders face high benefits of invasion, and a combination of compensations (M1), benign forest production (M5) and environmental alliances (M4) can be highly effective in increasing residents' incentives to defend the natural resources against outsiders.

H2.3 In a context of **high internal and low external pressures** on the reserves' forests the BFP has a positive effect on forest conservation. In such contexts residents traditionally engage in forest harming activities, and outsiders face low economic interests on the forest resources. Forest alliances

(M4) have little effect as invasions are few. Mutual monitoring (M3) is unlikely to emerge as in these contexts a majority of residents seem to gain from forest harming activities. Investments in forest friendly production (M5) might increase labor productivity but labor may shift to the more profitable forest harming activities. Nonetheless, external economic pressures are low and the BFP should be able to shift behaviors towards forest friendly activities with a combination of compensations (M1) and a strong indirect monitoring (M2).

H2.4 In a context of **high internal and high external pressures** on the reserves' forests the BFP has a negative impact effect on forest conservation. In such contexts residents and outsiders gain high benefits from forest harming activities. Compensations (M1) with indirect monitoring (M2) are unable to offset the opportunity costs of conservation and mutual residential monitoring (M3) is unlikely to emerge as everyone gains from forest harming activities. Furthermore, environmental alliances (M4) are improbable as collusion with outsiders brings higher benefits. These mechanisms are ineffective and the BFP has no positive effect through them. Nonetheless, the investments taken with compensations (M1) and the forest friendly production (M5) help residents to capitalize. Households divert the saved resources and labor input from the development aid towards higher production of forest harming activities. In consequence, the BFP unintentionally decreases forest conservation outcomes.

4.5 EVALUATION METHODOLOGY AND DATA

The impact of the BFP on forest conservation depends on its ability to provide additional conservation outcomes - an additional conservation effect beyond that derived from having placed the landscape under a PA category (multiple-use reserve). This analysis focuses on the comparison of reserves with the BFP against reserves without the BFP on observed biophysical outcomes (deforestation, forest degradation and fire incidence). The conservation effect of the BFP is defined as the difference between the observed outcomes and the unobserved outcomes that would have occurred if the policy had not been implemented. This unobserved outcome is defined as the counterfactual to the treated group. The BFP has actively selected 15 out of 61 multiple-use reserves within Amazonas. The unselected reserves serve as a base to construct the counterfactual scenario of what would have been the outcome without the intervention. As the BFP has not chosen randomly among the reserves it is best to assume that the treated 15 reserves systematically differ from the non-selected reserves. E.g., it might be that the treated reserves experience higher deforestation rates even without the intervention, or vice versa. Comparing the BFP reserves with the full set of non-participating reserves would produce biased results. In consequence, we would fail to measure potentially positive conservation effects.

Quasi-experimental evaluation designs aim to construct a credible counterfactual scenario to the treated group. 'Matching' is a prominent quasi-experimental method in spatial environmental applications to overcome selection biases (Andam et al., 2008; Gaveau et al., 2009; Honey-Rosés et al., 2011; Pfaff et al., 2015). Matching reduces any potential selection bias by finding for each treated unit the most similar untreated unit considering observable characteristics before the intervention started. As Pfaff et al. (2015) figuratively describe this strategy as the comparison of "apples-to-apples" by leaving out the peaches.

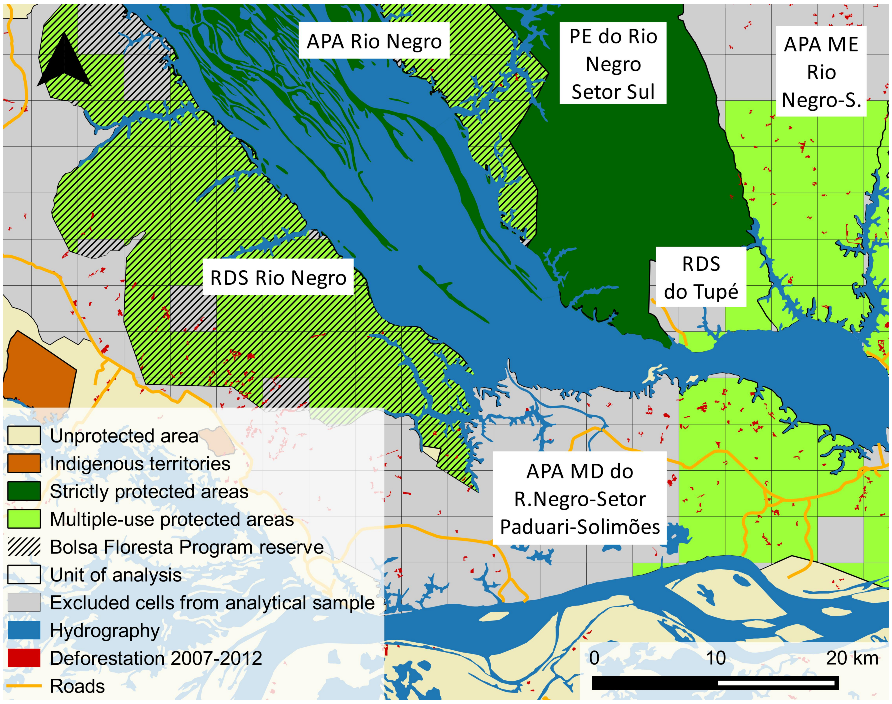
This analysis deals with the selection bias of the BFP in three steps. *First*, we "slice" all reserves into smaller spatial units of 5 to 5 km.[10] This procedure helps to better capture the large variety of environmental and economic characteristics across and within each of the reserves. In the *second* step the pool of control cells is reduced to only include cells that are similar to the treated BFP cells. With nearest neighbor 'matching' techniques we construct a sample of control cells most akin to the BFP cells in terms of pre-treatment deforestation trends, socio-economic indices and geo-environmental characteristics. This reduces potential biases from observable characteristics. The *third* step exploits the panel data available and the fact that BFP started in different years in different reserves. Fixed Effects (FE) estimations serve to reduce the risk of potential biases surging form unobservable time-invariant factors. The combination of all 3 methods is rarely found in environmental impact evaluations due to either a lack of information or data processing capacity. The combination of matching with panel data estimations promises to find accurate causal relations in comparison to the 'gold standard' of experimental evaluation Ferraro and Miranda (2014).

4.5.1 *Slicing the reserves*

To capture the spatial diversity and deforestation pressures within reserves we create a higher spatial resolution for the analysis. We intersect a grid with the administrative boundaries of all multiple-use reserves, which gives us a multitude of cells in each reserve (see Figure 4.4). The size chosen for the grid is 0.045 by 0.045 degrees which correspond to 5 by 5 km rectangles at the equator. Owing to our vectored data structure the resulting spatial units range between 0 and 25 sq. km. Especially at the border of reserves, cells are only a fraction of the original grid. We keep all units of the slicing process, fully covered and partially covered cells. This technique avoids a potential bias from loss of information or misattribution at the border. Often grid cells at bordering regions are excluded as they overlay several administrational units. Reserve borders are often drawn at natural boundaries like rivers or roads at which human activities and deforestation accumulate. Keeping all units avoids los-

10 The 5 to 5 km grid cells are artificial data containers and do not affect the resolution of the spatial data. E.g., the 30meters resolution of the deforestation data from PRODES remains but each pixel is assigned to a cell. The spatial data aggregation to a lower resolution of 5 to 5 km grid cells serves to avoid biases from e.g., spatial autocorrelation addressed in section 4.9.

Figure 4.4: The spatial slicing of protected areas



Note: The Figure depicts the slicing of reserves into spatial units of 5 to 5 km. The dashed reserves are the RDS do Rio Negro (south) and APA Rio Negro (north). RDS (Sustainable Development Reserve) and APA (Environmental Protection Area) are two subtypes of the multiple-use reserves category in the Brazilian protected area system.

ing important information. Simultaneously the irregular cell structure allows to rightly attributing deforestation patches to their according administrational unit. Furthermore the size of our cells is a compromise between spatial precision and spatial autocorrelation. Using a lower spatial resolution would fail to capture the spatial diversity, and a higher spatial resolution risks creating redundant observations and spatial autocorrelation within the data. We only exclude very small units that are covered by only 5% of the slicing grid units. The 61 reserves are thereby divided into 14,397 grid cells. Spatial information on outcomes and controls are intersected and attributed to these smaller units, making the analysis spatially explicit (see section 4.6 below).

4.5.2 *Constructing counterfactual observations*

The second step of our estimation analysis consists of finding counterfactual cells that are the most similar to BFP cells before the intervention started. Guided by our theoretical framework we want to find control cells that experienced equal internal and external deforestation pressures. Matching techniques are implemented to find for each treated unit ('apples') the most similar control unit ('peaches that are almost apples') out of the full set of controls. Similarity of deforestation pressures is approximated with observable pre-treatment environmental and socio-economic characteristics. Pressures from within and from outside of reserves are considered using variables on tree data levels, cell characteristics, reserve characteristics and district characteristics (see Figure C.1) Tables C.1 and C.2 list the data class, level and sources of each covariate.. The selected control cells from non-BFP reserves represent the counterfactual behavior of the treated cells that would have occurred without the intervention.

Matching is implemented with Sekhon's 2011 'Matching' package, using a 1to1 nearest neighbor matching technique with replacement on the Mahalanobis distance. We implement two additional non-standard restrictions to the matching algorithm. First, each cell can cover an area of 5 to 100% of its slicing grid cell (5 to 5 km). In order to avoid matches between observations with different sizes, we restrict the algorithm to find only pairs within a margin around 5 percentage points on this variable. Second, the age of treated and control reserves varies significantly and some reserves were founded at the very outset of the BFP while others are much older. To avoid control reserves that were founded later than the BFP started, we restrict the procedure to only find control matches from reserves founded before a BFP started. A detailed summary of all covariates used in the matching procedure are shown in Table C.3. The Table describes the attribute class of each factor (natural, economic or political), reports the data level (cell, reserve or district) and classifies the drivers of deforestation (internal or external).

Characteristics of the *natural environment* are considered at the cell level with pre-existing deforestation trends, pre-existing number of fire incidents, initial forest cover, secondary vegetation, non-forest area (swamps and bush land areas) and water bodies (lakes and rivers). The *economic environment* is considered on a cell level with infrastructural indices (distances to roads, rivers, and district capitals). Economic activities are controlled for on a cell level with remotely sensed land use classes (agricultural land, mixed occupation, secondary vegetation, pasture and urban land). Economic interests form outside of reserves are approximated with official statistics at the district level (population density, GDP per capita, GDP per capita from agriculture, the percentage area under farms, the share of small farms, the average tractors per farm and an average timber price). Furthermore, we include an index of land speculation at a cell level based on Bowman et al.'s (2012) spatial model of extensive cattle profitability.

The *political conservation* environment is measured with data on settlement projects and data on further protected areas surrounding each cell. Settlement projects are a major influence, as they often take on characteristics in opposition to conservation although more recent efforts to establish "sustainable settlements" in the Amazon may change this situation (Ludewigs et al., 2009). We use an binary indicator to determine whether or not a cell is covered by a settlement project. A favorable conservation environment is considered with distances to the next strictly protected reserve and the next indigenous area. The conservation quality of each reserve is partly controlled for by the size and years of existence of the respective reserve. Distances of each cell to its own reserve border capture the relative internal position and reflect the degree of exposure to external deforestation pressures. Finally, we include indices on the narrower spatial context using neighborhood characteristics of adjacent cells.

Matching estimators are based on two strong assumptions: unconfoundedness and common support. Unconfoundedness can be described by the fact that the selection of the reserves was solely dependent on the observable characteristics. Common support is a requirement of overlapping distributions of the distance measure or propensity scores between the control group and the treatment group. After personal discussions with the head of FAS (Virgílio Viana) and implementing agents we are confident that we have not missed out any relevant factor of the selection process.[11] The importance of the matching assumptions for causal interpretations is alleviated, as our analysis relies on panel data estimations to find the average treatment effect on the treated (ATT), described below.

4.5.3 *Regression analysis*

The third step aims to reduce the risk of potential biases surging from unobservable characteristics. After selecting an adequate control group with the previous matching procedure we exploit our panel database. We implement Fixed Effects (FE) regressions of the yearly outcome variables at the start of the BFP. The BFP started in 9 reserves in 2008, and added 5 more in 2009 and one in 2010. The different timing of treatment start allows us to filter out potential confounding factors that are unobserved and invariant over time. Our panel model is:

$$\ln \mathsf{DEF}_{irdt} = \mathbf{C}'_{it}\,\beta + \gamma\,\mathsf{PES}_{rt} + \mathbf{D}'_{dt}\,\delta + \mu_i + \kappa_r + \sigma_d + \eta_t + \varepsilon_{irdt} \quad (4.1)$$

The outcome variable ln DEF_{irdt} represents the annual logarithmic deforestation within each cell i in year t that resides in reserve r and is located within district d. Further analyses replace the deforestation outcome newly degraded forest areas or fire incidences. Our dataset runs from 2003 to 2012. Owing to data limitations, the analysis of forest degradation only includes years from 2007 to 2012. Yearly deforestation depends on individual cell characteristics, reserve characteristics, district characteristics, and the policy intervention of the BFP. The treatment dummy is denoted as PES_{rt}, turning one from year t onwards in which a reserve r starts to enroll in the BFP. Varying observable factors on the cell level and district level are denoted by C_{it} and D_{dt}, respectively. Under individual cell characteristics we consider yearly detected cloud coverage, which acts as a measurement error of the remotely sensed deforestation. Varying external pressures on forested areas are considered with yearly lagged district characteristics on GDP per capita, GDP per capita in the agricultural sector and average timber prices.

11 Visits to the FAS headquarters of FAS in Manaus was visited in July and November 2013.

Our panel model considers observable and unobservable fixed effects, which determine the yearly deforestation rates. Fixed factors can be classified into individual level characteristics μ_i, reserve level characteristics κ_r and district level characteristics σ_d. Year fixed effects common to all regions and cells of the study area are captured with dummies for each year, η_t. The year fixed effects control for variations in the overall macroeconomic environment and changes in the implementation of environmental laws from the state or federal government. The idiosyncratic error term is denoted as ε_{irdt}.

Regressing equation 4.1 would be prone to serial correlation and would fail to control for unobserved fixed effects, which simultaneously shift deforestation and treatment. We use a fixed effects (FE) estimation approach to assess the impact of the BFP on our outcome variables. Other studies on forest conservation use first-difference (FD) estimation approaches. FE and FD approaches both exclude the unobservable fixed effects on the cell, reserve and district levels (part of μ_i, κ_r, σ_d). In comparison, the FE estimator is more efficient if the idiosyncratic error term ε_{irdt} is not serially correlated, and the FD estimator is more efficient if the error term follows a random walk (Wooldridge, 2010). Most observations reveal only peaks of logging at some point within our timeframe. With no clear trend in the dependent variables, this confirms our preference for FE specifications, as follows:

$$\widetilde{\ln \mathrm{DEF}}_{irdt} = \widetilde{\mathbf{C}}'_{it}\,\beta + \gamma\,\widetilde{\mathrm{PES}}_{rt} + \widetilde{\mathbf{D}}'_{dt}\,\delta + \widetilde{\eta}_t + \widetilde{\varepsilon}_{irdt} \tag{4.2}$$

Equation 4.2 results from subtracting the means over time within each cell from equation 4.1.[12] This procedure excludes both observable and unobservable fixed effects. The loss of information from the excluded fixed observable factors is limited, as we have controlled for these with matching as a pre-estimation technique.

Our interest lies in the parameter γ measuring the ATT on deforestation within each grid cell. For the interpretation of the ATT as a causal relationship empirical methods rely on its standard error. The standard error is the estimate of the dispersion from the mean given by the sample. Therefore its calculation relies on the assumptions of the data structure at hand. Dividing the reserves into smaller spatial units creates a non-random selection process within our data generation. Meaning, the cells within each reserve are not independent of each other, they are subordinate to the same administration (e.g., management quality) and are treated or not treated simultaneously. The intra-class correlation within reserves violates the standard assumption of independent observations ($E[\varepsilon_{irdt}, \varepsilon_{jrdt}] = 0$). Standard errors relying on the assumption of independence assumption are largely underestimated and lead to high but untrue significance levels (Cameron and Miller, 2015). To correct for the group structure of our data we cluster the standard errors on the reserve level, following the suggestion from Angrist and Pischke (2009).

12 Procedure is clarified in Appendix C.3.

4.6 DATA

4.6.1 *Outcome variables*

Our outcome variables are yearly deforestation, forest degradation, and number of fires per cell. Spatial data processing is conducted on a PostgresSQL 9.2.3 data server with a PostGIS 2.0.1 spatial extension. Deforestation, forest degradation, and fire incidence data are downloaded in shape file format at its fullest resolution from the web-site of the National Institute of Spatial Research (Instituto Nacional de Pesquisas Espaciais, INPE).[13] INPE's deforestation measurement is based on Landsat imagery, and deforestation patches are defined as clear-cut deforestation - the complete loss of tree cover on a 30 m resolution. Reliable deforestation data are available from 2003 on and our time-frame ends in 2012.

INPE uses the August to July cycle for its yearly measurements - exploiting the relatively cloud-free dry period of the year.[14] INPE started to record forest degradation data in 2007, therefore few observations are available from before the Program started in 2008. As consequence, this analysis relies more strongly on matching assumptions rather than FE assumptions.

The number of yearly fires is based on a simple count measurement of fire incidence detected by several satellites on a daily basis. It therefore captures the frequency and intensity of forest burning activities within a predefined area.

4.6.2 *Covariates*

Covariates used for the matching procedure and the time series analysis are summarized in Tables C.3 and C.4, respectively. Data are either constructed through spatial calculations or obtained from official secondary data sources (district, reserve). Outcome variables (deforestation, degradation, and fires), data on forest coverage, cloud coverage, non-forest coverage and hydrography coverage (rivers, lakes) are provided by INPE's yearly deforestation database. Coverage is measured as a ratio of land use over the total area of a cell.

Land use classes are obtained from the 2008 revision of INPE's TerraClass project.[15] Areas deforested before 2008 are classified into new land use types of agricultural land, mixed land occupation, secondary vegetation, pasture land, and urban areas. We use these classes to construct coverages for each cell. INPE classified these lands in the same year in which the BFP rolled out. It is reasonable to assume that the BFP has not shaped in the first months' land use decisions and we can treat these variables as unaffected by the program.

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13 INPE-PRODES Instituto Nacional de Pesquisa Espaciais / Projeto PRODES - Monitoramento da floresta amazônica brasileira por satélite, `http://www.obt.inpe.br/prodes/index.php`.

14 For a detailed description of the methodology used to construct yearly deforestation data see section B.1 in the Appendix of chapter 3

15 `http://www.inpe.br/cra/projetos_pesquisas/terraclass2010.php`

We include coverage data of federal agrarian settlement projects, using the shape file provided by the National Institute of Colonization and Agrarian Reform (Instituto Nacional de Colonização e Reforma Agrária, INCRA).[16] A land speculation map is provided by Bowman et al. (2012), indicating whether or not a particular plot of forest land can be considered as being potentially profitable if converted for cattle ranching. We intersect Bowman's layer with the cells to construct an index of land profitability that can capture the pre-existing deforestation pressures on the reserves.

Distance variables are constructed with PostGIS by connecting the center of each cell with the nearest object in question (see an example in C.4). Distances in meters are logged to reduce skewed distributions stemming from outliers. The data on road networks from the Protection System of Amazonia (Sistema de Proteção da Amazônia, SIPAM)[17] serve to construct connecting beelines. The connectivity to water bodies is measured using the hydrography map of the Brazilian Institute of Environment and Renewable Natural Resources (Instituto Brasileiro do Meio Ambiente e dos Recursos Naturais Renováveis, IBAMA).[18] The distances to the boundaries of indigenous areas, strictly protected areas and multiple-use reserves are obtained with spatial layers from IBAMA (2013).

Non-spatial attributes to our cells database are cross-linked via the spatial location of the cell centroids within administrative entities. Figure C.1 depicts on a map excerpt the cell, reserve, and district data levels. Reserve characteristics on size and the foundation year are obtained from the spatial database on reserves. The district boundaries from 2007 are used for our analysis and are publicly available from Brazilian Institute of Geography and Statistics (INPE, 2012).District characteristics include population densities in 2007 from the IBGE Demographic Census (IBGE, 2000),GDP per capita and agricultural GDP per capita in 2006 from IBGE Agricultural Census (IBGE, 2006).Information on the farm coverage, the share of small farms and tractors per farm also come from the Agricultural census. Timber prices between 2003 and 2006 are constructed as the ratio between quantity and total value of timber produced and obtainable from the IBGE-PEVS report (IBGE, 2014).

Neighboring variables are used as covariates to control for dependencies of deforestation on close contexts. Neighboring covariates are constructed by "queen style", defining neighbors as such when two cells share a point on the boundary line. A weighted average of all neighboring values by the number of total neighbors serves in consequence as the neighboring covariate.

16 http://acervofundiario.incra.gov.br/i3geo/datadownload.htm
17 http://www.sipam.gov.br/
18 Original download is no longer available. Updated data are downloadable via the geoserver of the Center of Remote Sensing (CSR): http://maps.csr.ufmg.br/

4.6.3 *Sample*

The sample used in this research differs from the full database. We drop observations owing to data characteristics or to analytical reasoning before the analysis. The cell structure is constructed with 71,354 rectangular grid cells of 5 to 5 km covering the full Brazilian Legal Amazon. The resulting 86,483 cells are the data shell in which we fill the data described above.

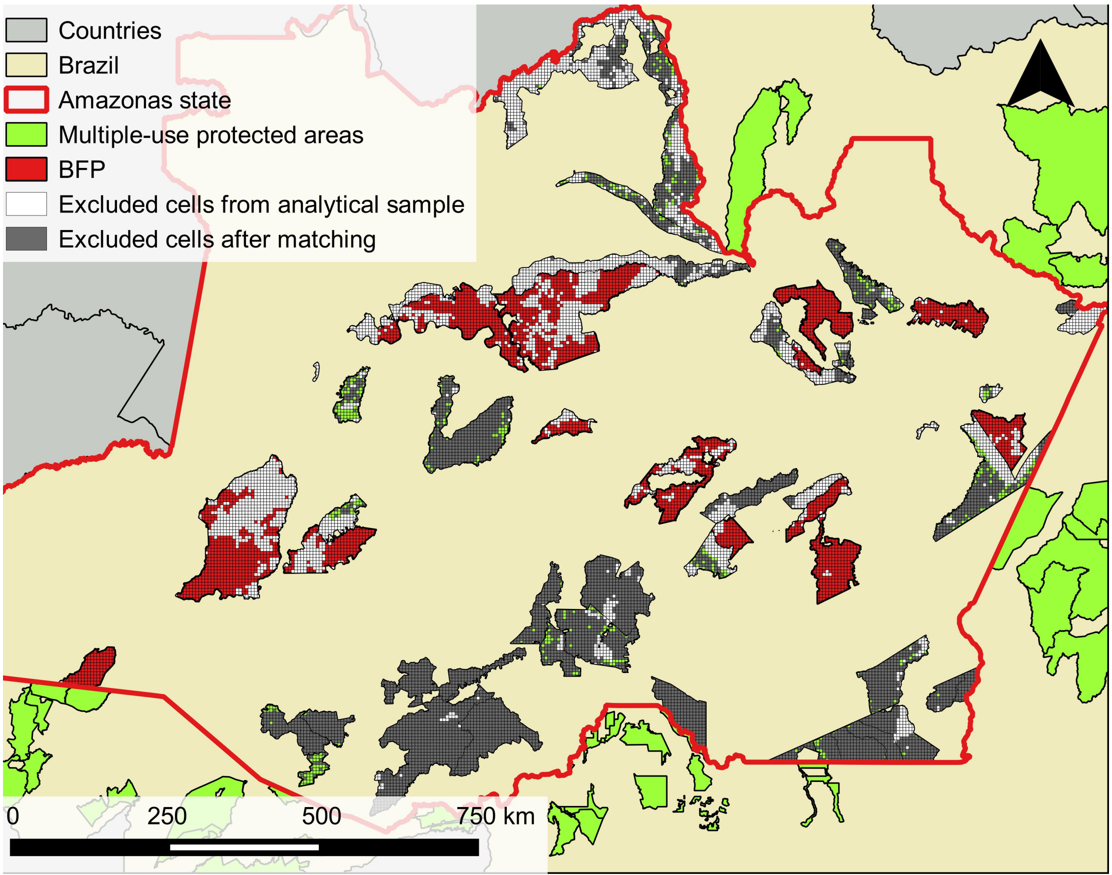
As we are focusing only on multiple-use reserves our sample reduces to 38,886 observations. Owing to data irregularities of the satellite monitoring system at the edges of images we further exclude 89 observations. No cells have to be excluded because of missing neighbors (e.g., singular cells on islands). Our analytical focus resides within Amazonas, with 61 multiple-use reserves, resulting in 9,143 control cells without the BFP and 5,254 treated cells of the BFP.

For analytical reasons, we drop 928 units that cover less than 5% of the original grid cell, i.e., dropping each cell that is roughly smaller than 1.25 sq. km We suspect that these small spatial units are prone to measurement errors and create problems of spatial interdependencies in the unobservables. More importantly, we exclude all observations that have no forest cover (152). These can result from extensive logging in previous years, but more probably are fully covered by swamps and water bodies. To avoid an analytical bias from spatial leakage we exclude all untreated cells within a distance of 20 km to treated cells (899). This results in 12,418 observations. Clouds introduce a bias from systematic measurement errors of yearly deforestation for small spatial units. We therefore exclude all cells that had experienced more than 90% cloud coverage in any year of our timeframe, dropping 2,632 observations. Finally, we identify 11 outliers in our control group that have experienced exaggerated deforestation levels with more than 200 ha cleared in some year during our timeframe. For this analysis we assume that these occurrences are driven by local unobservable peculiarities which could be related to treatment status and bias our results. Our final sample consists of 6301 control and 3474 treatment cells covering over 56 reserves including the 15 reserves of the BFP. Figure 4.5 maps the analytical sample used in the matching process.

4.7 RESULTS

The results of the counterfactual analysis on BFP impacts are presented in 5 parts: First, we evaluate the quality of our matching procedure. Second, we assess the ability to measure impacts in the context of low deforestation occurrences. Third, the average impacts on deforestation are discussed in detail following Hypothesis (H1) (see section 4.4). Fourth, context dependent heterogeneous impacts are analyzed following hypotheses H2.1 - H2.4. Fifth, we test the impacts on alternative measures of forest conservation (forest degradation and forest fires).

Figure 4.5: Matched sample of spatial cells



Note: The Figure shows the sliced sample of the multiple-used reserves in the state of Amazonas. The BFP cells in red 3474 are matched to the unique non-BFP cells (722) in green with a 1:1 nearest neighbor matching based on the Mahalanobis distance measurement.

4.7.1 *The quality of matching*

Matching is conducted on the Mahalanobis distance on pre-program characteristics covering environmental, social, and economic conditions. Our preselected sample included 6,301 control cells and 3,474 treated cells across 15 treated and 56 control reserves (see also Figure 4.5). The matched 1 to 1 sample resulted in 3474 paired controls created out of 722 unique controls spread over 30 reserves (see Figure 4.5). The aim of the matching procedure is to reduce imbalances among the covariates that indicate the probability of program selection (e.g., FAS having preselected those reserves that had exhibited lowest prior deforestation rates). Figure 4.6 depicts the imbalances between control and treated group before and after matching. The dots indicate the (standardized) mean distance between control cells and treatment cells. The differences before matching - including all cells of non-participating reserves

- are on average higher than after matching.[19] The main focus lies on the percentage deforestation of each cell before the BFP start. The standardized mean difference between 2003 to 2006 declines on average from 0.18 to 0.02 standard deviations, a decline of 90% to almost zero difference.

4.7.2 *Ability to measure impacts*

Deforestation rates are traditionally very low within multiple-use reserves in Amazonas. At the beginning of 2002, 850 sq. km had already been deforested within the BFP reserves. Deforestation activities during the years from 2003 to 2007 amounted to only 40.8 sq. km, and after the start of the program until 2012 they totaled 32.8 sq. km (cf. Figure 4.7). The detection of forest harming activities in a context of low deforestation rates is limited by the nature of the satellite monitoring system. INPE reports only newly deforested areas. Once an area is deforested, it is no longer monitored for return deforestation, which is a common process among shifting cultivators. Deforested and abandoned areas regenerate after a few years and our deforestation measurement does not account for re-deforestation activities. Given that a large part of the area was already deforested before 2003 and given that newly deforested areas are small within all reserves, we test our ability to measure any impact on forest cover of the BFP.

The maximum forest conservation impact of the BFP would consist of a complete cessation of deforestation beginning with the start of the program. The overall deforestation level is very low within all reserves in Amazonas, averaging between 0.54 ha to 0.06 ha of yearly deforestation in the matched sample (see Table C.4). These low variations cast doubt on our ability to detect any impacts with remotely sensed data. Before continuing, we test the ability to empirically measure impacts by setting the few treated observations with positive deforestation values to zero in the years the BFP starts in each of the reserves (see Table C.5). Estimates of the BFP using our FE model indicate to the hypothetically maximum impacts the intervention could have achieved.

Results of the FE regressions using the hypothetical deforestation levels are presented in Table 4.1 below. Column (1) shows results using the full unmatched sample. The coefficient is negative (-0.286) and significant at a 1 percent level. The second column presents the FE impact estimate after matching - our preferred estimation method. The coefficient is also negative (-0.254) and significant on a 10% level. This result suggests that the BFP treatment could only be measured up to an average reduction of deforestation of 22%.[20] [21]

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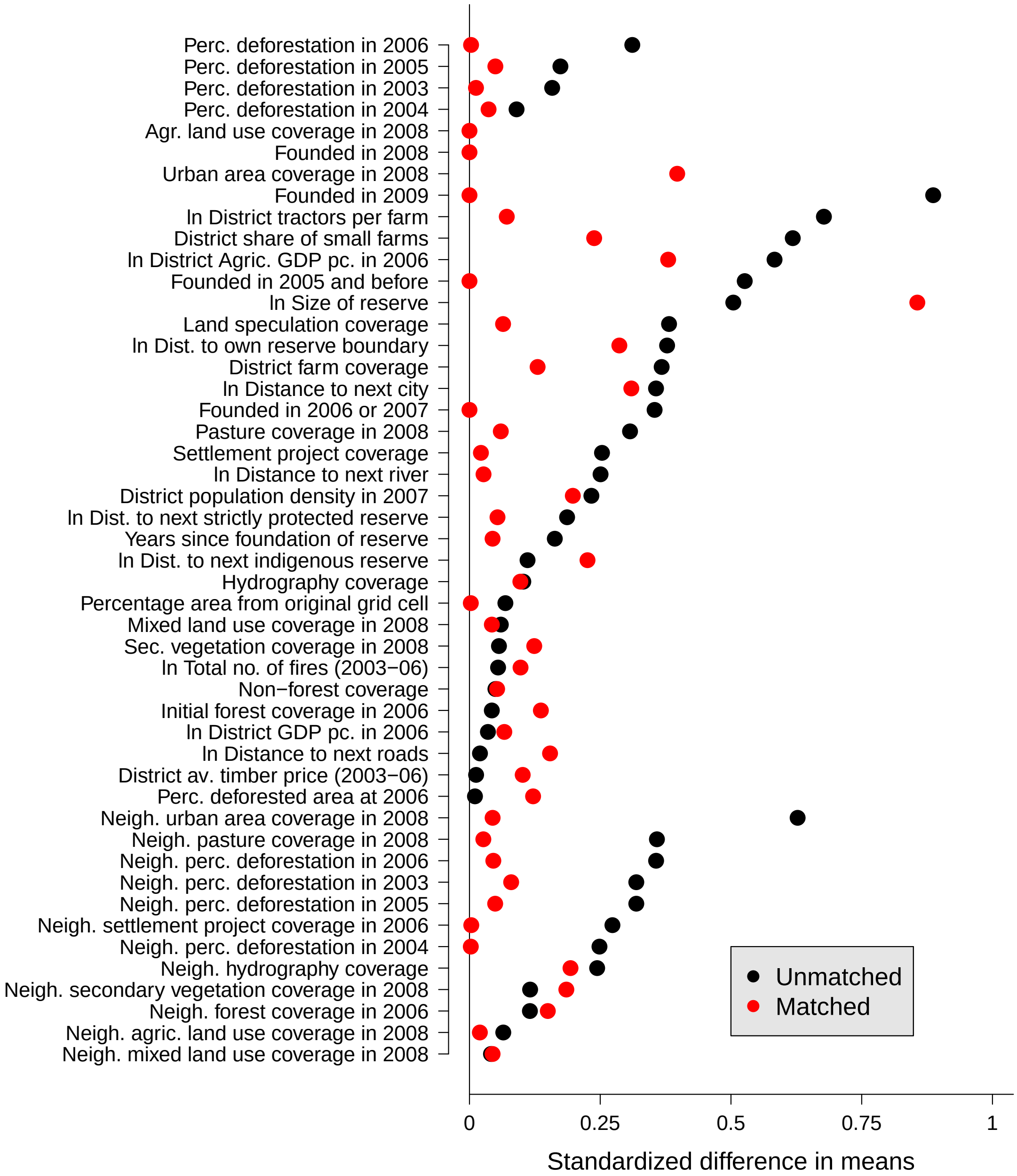
19 The median difference across all variables before matching is 0.057. After matching mean standard deviations fall sharply by 59% to 0.023, indicating a significant improvement of sample balances.

20 In the unmatched case the coefficient has a higher value than in the matched case and is significant indicating that all unmatched regressions tend to upward bias our results.

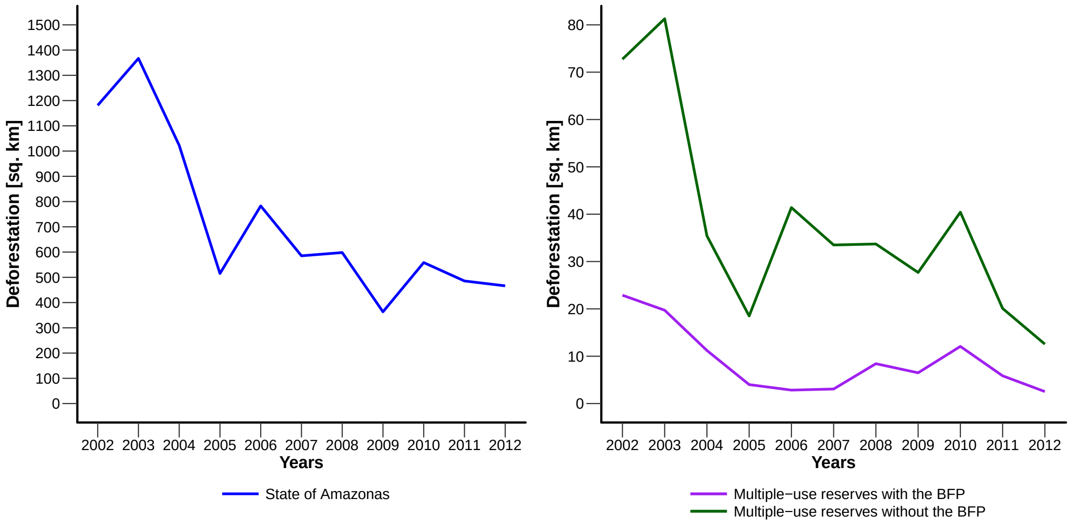
21 $0.22 = \exp(-0.254) - 1$; See Halvorsen and Palmquist (1980) for the calculation of impact of explanatory dummy coefficients in regressions.

Figure 4.6: Covariate balances before and after matching



*N*ote: The Figure depicts the mean standardized differences between control observations and treated observations, before (black) and after (yellow) matching. The distances decrease after matching significantly towards zero. At zero difference between matched controls and treated units, the selection bias converges to zero.

Figure 4.7: Deforestation in the state of Amazonas and its multiple-use reserves



*N*ote: Left panel shows yearly deforestation levels within the state of Amazonas. Right panel shows trends for multiple-use reserve of the state of Amazonas receiving the Bolsa Floresta program (BFP; purple, lower line) and not receiving the BFP (upper, green line).

The distribution of our dependent variable is highly skewed because of the low deforestation rate in forest reserves in Amazonas. In the matched sample, 95% of all observations, across cells and years, report zero deforestation. Only 12.7% of all cells experience some deforestation during our timeframe. This circumstance could downward bias our treatment estimates as variations in the explanatory variables lack a response in large parts of the dependent variable. We deal with this in 3 ways. First, we estimate a weighted FE estimation in column (3). Weights are constructed by the inverse probability of the cell experiencing some deforestation before treatment (2003-2007). Weighting the sample by probabilities gives less influence to observations that would not have deforested in any case. Second, we weight by the size of our cell units in column (4), giving lower importance to very small observational areas. This method controls for the probability of observations experiencing zero deforestation simply because small areas are less probable to be affected by deforestation. Third, we test our results in column (4) with the estimation of a Random Effects Panel Tobit model, which incorporates the skewed distribution. Coefficients in columns (2) to (4) are still negative but insignificant. These results demonstrate that despite the low variation of our outcome, it would technically be possible to measure significant impacts of the BFP.

4.7.3 *The average effects on deforestation*

The average effects of the BFP on deforestation are reported in Table 4.2. All coefficients of the BFP dummy are insignificant, regardless of the estimation method. Comparing the unmatched with the matched sample, the matched

Table 4.1: The hypothetical maximum Bolsa Floresta effect

Dependent	ln hypothetical deforestation				
	Unmatched	Matched			
	FE	FE	weighted FE by P.def.	weighted FE by cell size	RE Tobit
	(1)	(2)	(3)	(4)	(5)
Bolsa Floresta	-0.286***	-0.254*	-0.089*	-0.191	-0.417
	(0.104)	(0.143)	(0.047)	(0.141)	(0.053)
Year effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Further controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	97750	69480	69480	69480	69480
Groups	56	45	45	45	45
R-sq. / Chi-sq	0.032	0.045	0.012	0.046	1773.0

*N*ote: The Table reports fixed effects estimates in columns (1)-(4). Column (3) uses weights by estimates of a pre-treatment probability of deforestation. Column (4) uses weights by the cell size. Column (5) reports Random Effects Tobit estimates. The dependent variable is the change in the log of yearly newly deforested area. Further controls include cloud coverage over remaining forest area, the log of yearly district GDP per capita, the log of yearly agricultural GDP per capita and the log of yearly average timber prices. Clustered standard errors on a reserve level are reported in parentheses. *,**,*** denote significance at the 10/5/1% level.

coefficient is approximately 50% higher, indicating a downward bias of the treatment effect in the unmatched regression. The last estimate in column 5, the Random Effects Tobit estimate, shows a higher coefficient, though insignificant and therefore unreliable for a causal interpretation. These results indicate no causal change of deforestation rates due to the BFP intervention.

The previous section verified that a impact on deforestation would be measurable if deforestation had ceased with the start of the BFP. However, at the aggregate level we cannot find any impact of the BFP. Following the impact hypothesis H2.1 - H2.4 of section 4.4, we investigate for heterogeneous effects in the next section.

4.7.4 *Context dependent heterogeneous impacts*

The analysis of the average effect of the BFP failed to detect any positive or negative impacts on deforestation. A first explanation is the incipient phase of the program. Starting to introduce conservation incentives in 2008, the program might not have had the chance to significantly protect forests by 2012 against a counterfactual scenario.

Table 4.2: The Bolsa Floresta effect on deforestation

Dependent	ln Deforestation				
	Unmatched	Matched			
	FE	FE	weighted FE by P.def.	weighted FE by cell size	RE Tobit
	(1)	(2)	(3)	(4)	(5)
Bolsa Floresta	0.032	0.049	-0.000	0.067	0.375
	(0.057)	(0.114)	(0.037)	(0.123)	(0.065)
Year effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Further controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	97750	69480	69480	69480	69480
Groups	56	45	45	45	45
R-sq. / Chi-sq	0.022	0.028	0.006	0.030	1116.6

*N*ote: The table reports fixed effects estimates in columns (1)-(4). Column (3) uses weights by estimates of a pre-treatment probability of deforestation. Column (4) uses weights by the cell size. Column (5) reports Random Effects Tobit estimates. The dependent variable is the change in the log of yearly newly deforested area. Further controls include cloud coverage over remaining forest area, the log of yearly district GDP per capita, the log of yearly agricultural GDP per capita and the log of yearly average timber prices. Clustered standard errors on a reserve level are reported in parentheses. *,**,*** denote significance at the 10/5/1% level.

The second explanation for the zero average impacts refers to the hypotheses H2.1 to H2.4 described in section 4.4. Diverging positive and negative effects of the BFP can sum up to zero. We expect no change in deforestation due to the program where deforestation pressures are low internally and externally (H2.1). Differently, we expect a strong influence of the environmental alliances mechanism to reduce deforestation in a context of low pressures internally and high pressures externally (H2.2). In a context of high internal pressures but low external pressures we expect declining deforestation rates, arguing that the newly introduced monitoring and sanctioning mechanisms are able to offset the benefits from forest harming activities (H2.3). Finally, we expect increasing deforestation rates to be observable where internal and external pressures on reserve forests are high, arguing that the development aid facilitates already existing forest harming activities (H2.4).

External and internal pressures on forest resources are measured with respect to the pre-treatment deforestation within each reserve and in a 20 km buffer around each reserve. We measure the risk of the BFP reserves with the ratio of total deforestation between 2003 and 2006 over the remaining forested area in 2002. A reserve is defined to experience a high internal risk if it is among the group with the seven highest deforestation ratios (see Table 4.3,

Table 4.3: Classification or relative internal & external deforestation pressures of the BFP reserves

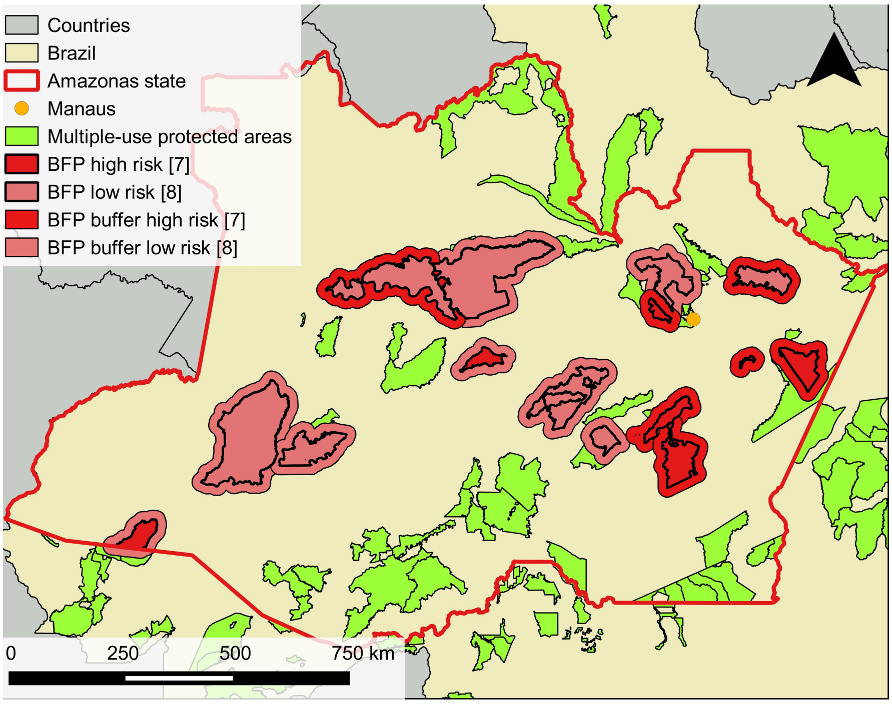
Reserves	internal Def. ratio	external Def. ratio	high internal risk (HiR)	high external risk (HeR)	HiR & HeR	HiR & LeR	LiR & HeR	LiR & LeR
RDS Canumã	0.458	0.454	1	1	1			
RDS do Rio Negro	0.367	0.367	1	1	1			
RESEX Catuá-Ipixuna	0.131	0.041	1			1		
RESEX do Rio Gregório	0.130	0.016	1			1		
RDS do Juma	0.124	0.365	1	1	1			
FE de Maués	0.105	0.284	1	1	1			
RDS Rio Madeira	0.080	0.128	1	1	1			
RDS do Uatumã	0.064	0.176		1			1	
APA do Rio Negro	0.048	0.095						1
RDS Piagaçu-Purus	0.019	0.032						1
RDS Amanã	0.018	0.014						1
RDS Uacari	0.014	0.010						1
RDS Cujubim	0.002	0.017						1
RDS Mamirauá	0.001	0.213		1			1	
RDS Rio Amapá	0.000	0.033						1

*N*ote: Column 2 reports the internal (within reserves) deforestation ratio measured as the percentage share of deforested area between 2003 and 2006 of the remaining forested area in 2002. Column 3 reports the external (in a 20 km buffer of reserves) deforestation ratio measured as the percentage share of deforested area between 2003 and 2006 of the remaining forested area in 2002. Column (4) indicates if a reserve is among the group with the 7 highest values of internal deforestation ratio. Column (5) indicates if a reserve is among the group with the 7 highest values of external deforestation ratio. Columns 5-8 indicate the grouping of reserves into high internal risk (HiR) or low internal risk (LiR) combined with high external risk (HeR) or low external risk (LeR). APA (Environmental Protection Area), RESEX (Extractive Reserve), RDS (Sustainable Development Reserve), and FE (State Forest) are subtypes of the multiple-use reserve category.

column 4 below). The remaining eight reserves experience relatively low internal pressures. A reserve is classified to experience a high external risk if it is among the group with the seven highest deforestation ratios within its buffer zone. Low external risks are attributed to the remaining eight reserves. This results in reserves with high internal & high external risk (HiR & HeR), high internal and low external risk (HiR & HeR), low internal risk and high external risk (LiR & HeR), low internal risk and low external risk (LiR & LeR). See the classification of reserves on a map in Figure 4.8 below.

Table 4.4 shows the context-dependent heterogeneous impacts of the BFP. Estimations follow our preferred FE model (Table 15, Column 2). Each risk group is estimated individually. We conduct this sub-estimation by using only cells of treated reserves within a given risk group and their corresponding paired control cells. Column (1) shows the BFP effect for the group of reserves that experienced a high internal risk and high external risk of deforestation.

Figure 4.8: Classification of internal and external risk of the BFP reserves



*N*ote: Note: The Figure shows the 15 BFP reserves and their buffer zones, classified in high (dark red) and low (light red) relative risks of deforestation. Risks are defined by the share of deforested area between 2003 and 2007 over the remaining forested area. The highest 7 values classify a BFP reserve as experiencing high deforestation pressures within. High external risks are classified for the reserves with the highest 7 values within their buffer zones.

For this group the treatment effect is positive with a coefficient of 0.271 and significant at 5% level. In accordance to our hypothesis, the result indicates that in these particular reserves deforestation increased by 31% owing to the BFP.[22] The estimate for the reserves with high internal but low internal risk (in column 2) is negative and insignificant. Nonetheless the direction of the sign is in line with our expectations. Put differently, reserves that experience relatively high pressures from outside but have traditionally low deforestation inside (column 3) the coefficient of the BFP is -0.097 and significant on a 5% level. Again our hypothesis is confirmed, indicating a reduction of deforestation by 9% in these reserves due to the program. Lastly, the group of reserves with low external and internal risk (column 4) shows a low and insignificant BFP impact coefficient of 0.010, close to zero.

Impacts of the BFP on deforestation rates are low in absolute terms. The deforested area within the BFP reserves during the years of the intervention total 3038 ha (see also Table C.5). The group of reserves with a high internal and external risk contributes with 1402 ha. Following the estimate of column 3 in Table 4.4, deforestation would have been 333 ha lower (31%) without the intervention. This increase is partly offset in reserves with low internal and high external risk (column 3 of Table 4.4) with a decrease of 59 ha of

22 $0.31 = \exp(0.271) - 1$

Table 4.4: The Bolsa Floresta effect and its risk context

Dependent	ln Deforestation			
	high internal & high external risk	high internal & low external risk	low internal & high external risk	low internal & low external risk
	(1)	(2)	(3)	(4)
Bolsa Floresta treatment	0.271**	-0.200	-0.097**	0.010
	(0.113)	(0.163)	(0.044)	(0.069)
Year effects	*Yes*	*Yes*	*Yes*	*Yes*
Further controls	*Yes*	*Yes*	*Yes*	*Yes*
Observations	12400	4360	12300	40420
Groups	22	14	13	28
R-sq. / Chi-sq	0.071	0.105	0.014	0.014

*N*ote: The table reports fixed effects estimates after matching, the dependent variable being the change in the log of yearly deforested area. Further controls include cloud coverage over remaining forest area, the log of yearly district GDP per capita, the log of yearly agricultural GDP per capita and the log of yearly average timber prices. Clustered standard errors on a reserve level are reported in parentheses. *,**,*** denote significance at the 10/5/1% level.

deforestation from 643 to 584 ha. As discussed above the diverging trends in deforestation after the beginning of the program are in sum too low to attribute a positive or negative effect of the BFP across all reserves.

4.7.5 *Alternative measures of conservation outcomes*

The BFP has different conservation outcomes in line with the reserve rules, in addition to forest conservation. We examine two other conservation outcomes: reducing forest degradation and fires.

Yearly information of degraded forest areas is available since 2007 (INPE, 2008b). This reduces the timeframe on our estimates to 2007 - 2012, with observations for only one year before treatment started. Our estimations thus rely more strongly on the matching assumptions and less on the panel data assumptions. As a measurement of fire incidence we use the aggregate count of heat foci within a year from different satellite sources provided by INPE.

Results are schematically summarized in Table 4.5 for all reserves and our risk groups using the 4 post-matching estimation techniques presented in section 4.7.2.[23] Blank cells indicate non-significant results, positively and negatively significant results are indicated with plus and minus signs. The Table

23 We repeat the exercise for each reserve separately. Results are presented in the Appendix in Table C.6.

Table 4.5: The Bolsa Floresta effects by risk context

Dependent	ln Deforestation				ln Forest degradation				ln No. of fires			
	FE	w.FE P.def.	w.FE c.s.	RE Tobit	FE	w.FE P.def.	w.FE c.s.	RE Tobit	FE	w.FE P.def.	w.FE c.s.	RE Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
All matched reserves											+	
High internal & high external risk	+		+	+				+				+
High internal & low external risk		-	-		-		-					
Low internal & high external risk	-			-	+	+	+	+		+		
Low internal & low external risk									+	+	+	

*N*ote: The table reports the significant results of fixed effects estimates using different dependent variables. The first row reports estimates on the full matched sample of cells across all Bolsa Floresta reserves. Rows 2 to 5 report estimates on sub-groups. Internal and external risks refer to the ratio of deforestation between 2003 and 2006 over forest cover in 2002 within reserves and in 20 km buffer zones around reserves (excluding water areas and other reserve areas). High and low groups are defined as the upper and lower part of an ordered sample. The 7 reserves with the highest internal ratio of deforestation classify as 'high internal risk'. The 7 reserves with the highest external ration of deforestation classify as 'high external risk'. Positive (negative) signs indicate significant positive (negative) coefficients at a minimum 10% level. Missing signs indicate to insignificant estimates.

repeats estimates on logarithm yearly deforestation in columns (1) to (4).[24] Columns (5-8) use the logarithm of yearly degraded forest areas as the outcome and columns (9) to (12) use the yearly no. of fire incidences. It is apparent from a first look on the table that significance levels of coefficients vary across estimation techniques and therefore switch between statistical significance and insignificance. These fluctuations are presumably explained by the large share of observations with zero values in the dependent outcome. We therefore tend to interpret significant results as a causal direction of impacts and do not interpret the size of the effects.

First we revisit the analysis on yearly deforestation by risk group in columns (1) to (4). Presenting results from different estimation techniques gives additional insights. For reserves with high internal and high external risks, estimates are positive and significant in 3 out of 4 estimations. Reserves with high internal but low internal risks (column 2) show a significant negative coefficient in 2 out of 4 specifications. Similarly, we find 2 significant negative coefficients for reserves with low internal and high external risks. Independent of the estimation technique, no significant impact on deforestation is detected in

24 Full estimation results by risk groups and on different outcomes for each individual reserve are provided in Table 10.

reserves with low internal and external risks. Although not all specifications are significant, the results confirm our hypotheses on heterogeneous impacts in relation to the context of deforestation pressures.

Regressions on forest degradation are shown in columns (5) to (8) of Table 4.5. Average impacts using the full sample of reserves (first row) are not significant, as on deforestation. For reserves with high internal and external risks, we find little support for BFP impacts on forest degradation, with only 1 out of 4 significant specifications (hypothesis H2.4). Forest conservation measured by degraded areas is increased in reserves with high internal pressures but low external pressures (hypothesis H2.3). Reserves with low internal and high external risk reveal a different impact direction, contrary to our expectations (hypothesis H2.2). In all specifications, the BFP appears to cause an increase in degraded areas. A possible explanation to this unexpected effect is that the remotely sensed satellite data are interpreting areas under BFP-supported sustainable timber production as degraded. There are no significant effects on forest degradation for reserves with low internal and external pressures.

Regressions on yearly fire incidences are shown in columns (9) to (12) of Table 4.5. Average impacts remain insignificant, except for a positive impact in the FE specification, which we judge to be insufficient to confirm hypothesis H2.4 using fires as a forest conservation outcome. Similarly, we cannot confirm hypotheses H2.3 for reserves with high internal and low external pressures with no significant impact estimates. Also for reserves with low internal and high external pressures we find only one significant specification (hypothesis H2.2). Hypothesis H2.1 stated our expectation that the BFP is unable to further increase already high compliance rates. The last row indicates otherwise with 3 out of 4 specifications with significantly positive impact coefficients. We speculate that because of the low economic opportunities inhabitants of these reserves remain dependent on slash and burn agriculture. An increase in fire activity without an increase in deforestation rates can be explained if the BFP invested in extensification of existing agricultural production systems within secondary forests not surveyed by INPEs monitoring system.

Summarizing, we find no overall, measurable effect of the BFP on forest conservation, measured in terms of deforestation, forest degradation or fires. Detrimental effects can be found in contexts of high internal and high external pressures on forest resources in terms of deforestation. Nevertheless, in a context of low internal pressure and low external pressure, fire incidence increased in BFP supported reserves. In a context of low internal pressures and high external pressures program impacts are mixed with decreasing deforestation but increasing forest degradation. Only in a context of high internal pressures but low external pressures the BFP shows consistently favorable impacts on both deforestation and forest degradation.

4.8 ROBUSTNESS

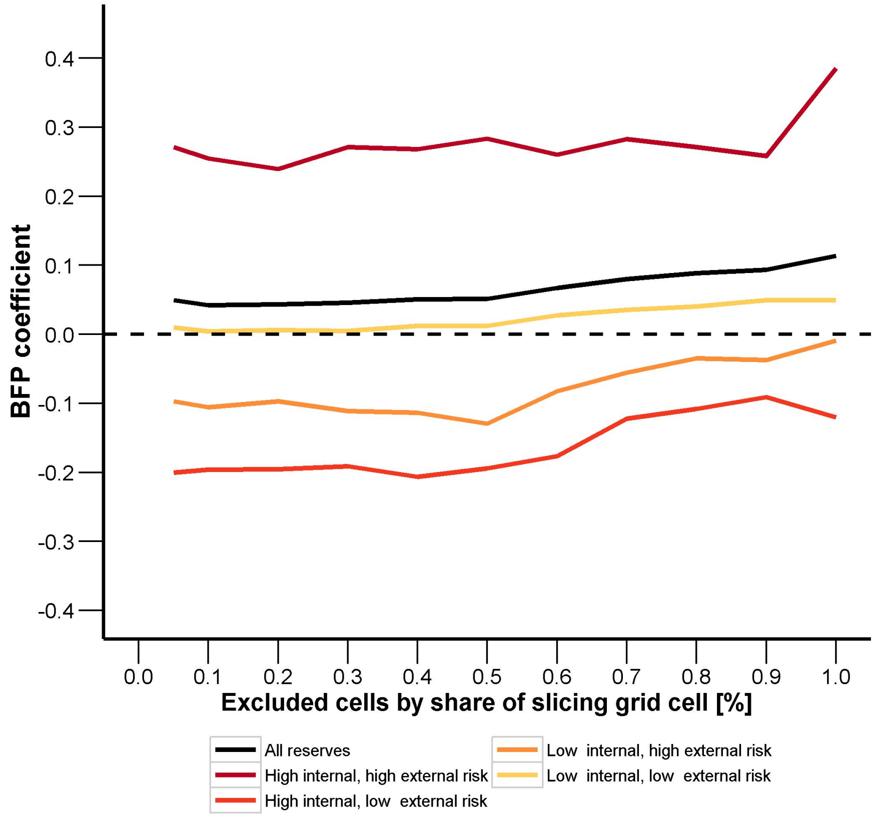
The overall effect of the BFP on deforestation rates is low and insignificant using our preferred estimations in section 4.7.3. Although matching achieved a significant improvement of the covariate balance (cf. Figure 4.6), remaining unbalances could bias our results. We conduct a variety of alternative matching procedures to test for misspecifications (not reported). First, we increase the stringency of 'similarity' between the matched pairs. We restrict paired matches to 2.0, 1.5, 1.0 or 0.5 caliper of standard deviation difference in their covariate values. Impact coefficients remain low and insignificant. Furthermore, we test if the BFP effects cumulate within transport corridors and restrict the sample to distances of 30, 20, and 10 km from cities, rivers, or roads. After rematching, the FE estimations coefficients turn negative - ranging from -0.067 to -0.026 - but remain insignificant.

Multiple-use reserves in Brazil are managed under federal, state, or municipal administration. The FAS specifically implemented the BFP program within state-administered reserves. In our preferred matching procedure we use all administration types, to maximize the pool of potentially matched control cells. The matching procedure allows including federal administered reserves to the control sample because after the procedure observations aimed to be 'equal' along the dimensions of covariates. To exclude federal reserves beforehand would necessarily lead to a decreased similarity of the matched pairs. In addition, this analysis relies on post-matching panel data estimations where fixed factors over time, (e.g., reserve administration and management) are canceled out. A bias will only occur if federal reserves have sharply changed their management quality after the BFP start in 2007. For example if federal reserves improved their protection capabilities simultaneously as the state reserves they would not serve as good controls and lead to an under-estimation of the BFP's effects.

To rule out the possible bias from inadequate control selection, we test our estimation strategy using only state administered reserves. Cells of the 15 BFP reserves are matched to cells of remaining 20 state reserves. Post-matching FE estimates of the average BFP's impact stay insignificant. The random effects (RE) Tobit estimate increases to 0.513 and becomes significant at a 1% level. This result indicates that deforestation levels in federal reserves tend to be higher than in untreated state reserves. This exercise suggests that using only state reserves positively biases our impact estimates.

Our database includes smaller and larger cells due to the slicing process of reserves by a 5 to 5 km grid (see section 4.5.1 and 4.6.3). We choose to keep all irregular cells that are not fully covered by the original grid cells to avoid biases from the loss of information or misattribution (see next section). Convinced that controls should be similar in size we restrict the matching process to find only pairs where the cell size is equal (to a margin of 5%). However, the concern remains that this data structure could drive our results. We test therefore, if estimates change when successively excluding cells smaller

Figure 4.9: Bolsa Floresta impacts by cell size exclusions



*N*ote: Lines represent the impact the FE estimates of the BFP intervention after matching. The Abscissa indicates the threshold of cells excluded from the sample. E.g., the value 0.2 indicates a sample containing only cells larger or equal to 20% of the original 5 to 5 km grid cell used for slicing the reserves. Sample sizes using all reserves (without differentiation on risk type) vary between 6,948 (0.05) to 4,500 (1.0) units. A detailed depiction of estimates with confidence intervals is shown in Figure C.2.

than 10, 20, . . . 100% of the slicing grid cell. The results are presented in Figure 4.9 (Figure C.2 in detail) and show that the size of impact coefficients increase with the exclusion of smaller cells. For example, excluding successively cells below 10 to 100% increases the average BFP impact coefficient increases from 0.049 to 0.113. Nonetheless, reducing the sample size does not change the order of risk groups, and significance levels generally stay the same.

4.9 CAVEATS

4.9.1 *Potential biases from artificially small spatial units*

Ideally, statistical analysis is based on actual decision units. By construction, PAs lack clearly defined property rights or information on defined responsibilities (Avelino, Baylis, and Honey-Rosés, 2015). In our case, decisions are undertaken at the reserve, community, and household levels. In order to balance all aspects, we choose a grid size of 0.045 degrees in height and width to slice the reserves into smaller cells. We believe this 5 to 5 km grid balances five prevalent data-driven problems that result from slicing spatially demarcated administrational entities for regression analyses, namely: The problem of computing power, and the biases of spatial autocorrelation, information loss, and misattribution.

Analysts are often constrained to the computing power and the format of information in which the spatial information is provided. Remotely sensed data are mostly provided in high resolution raster formats. Analysts often choose the given resolution as unit of analysis and refrain from spatial aggregation to a lower resolutions.[25] In consequence, the high resolution data increase rapidly the requirement on computational power. To limit the number of observations, researches sometimes repeatedly select random subsamples, estimate and average the results. This method also aims to resolve the issue of spatial autocorrelation but deliberately leaves a bulk of available information aside.[26] Our choice of 5 to 5 km grid cells allows us to feasibly process all spatial information and use regression analysis keeping the full spatial area available.

The bias from spatial autocorrelation from omitted variables can emerge from raster data structures as used in this study. Factors that determine the risk of deforestation tend to be increasingly equal with decreasing distances between neighboring observations. Omitted explanatory variables that do not vary across neighboring cells create a correlation among the individual residuals of regression models (Anselin, 2002). For example, in this study an area with high valued trees prone to attract logging and be deforested could cover several cells. The resulting correlation would bias the estimates. Reducing the size of the artificial spatial units increases the underlying 'similarity' of omitted factors among neighbors and increases the risk of spatial autocorrelation. We believe to have limited the possibility of spatial autocorrelation with a fairly large grid of 5 to 5 km, which allows potential omitted variables to be spatially uncorrelated.

A more severe bias could emerge from the spatial autocorrelation through a redundancy of data. Decreasing the size of grid cells results in an increasing 'similarity' of neighboring grid cells in the observed characteristics. This effect can be described as an artificial multiplication of data. This would not invalidate the estimates but decrease their standard errors drastically. A statistical inference of the estimates is not possible with such uncorrected standard errors. Although we did not test for spatial autocorrelation per se, we avoid a too small grid cell size and cluster our standard errors on the reserve level.

Dividing spatial administration units into cells creates smaller units that are either covered completely by the larger entity or are only partially covered (see

25 See (Andam et al., 2008; Joppa and Pfaff, 2010; Kirby et al., 2006; Pfaff et al., 2014, 2015; Robalino and Pfaff, 2012; Robalino, Sandoval, Barton, Chacon, and Pfaff, 2015)

26 Ibid.

Figure 4.4). Analysts commonly deal with partially covered units in two ways: (a) Excluding all cells from the database which are not covered by 100% (or close to 100%); or (b) considering a partially covered cell as fully covered upon a subjective threshold (e.g., over 50% covered, or centroid allocation). Option (a) produces a bias from loss of information. In the case of PAs, which often have physical borders (rivers, roads, etc.), excluding bordering cells leads to a bias where we specifically expect at these edges the majority of deforestation activities. We do not face this bias as we keep all grid cells in our sample.

Option (b) leads to a bias from misattribution. Grouping all deforestation patches at a border into one cell that *de facto* crosses a reserve border makes it impossible to distinguish if deforestation happened at the inner border or at the outer border region (see Figure 4.4). If spatial leakage effects exist the estimated treatment effect will be biased. With positive leakage effects leading to overestimations and negative leakage effects to underestimations of the treatment effect due to the false attributions of deforestation patches. The bias from loss of information and the bias from misattribution, both biases decline with decreasing grid cell sizes. With declining unit sizes the precision of representing the factual borders increases and the biases from information loss and misattribution fall towards zero. We averted the bias of misattribution by using our irregular cell structure, which allows us to unambiguously attribute any spatial information to the right administrational unit.

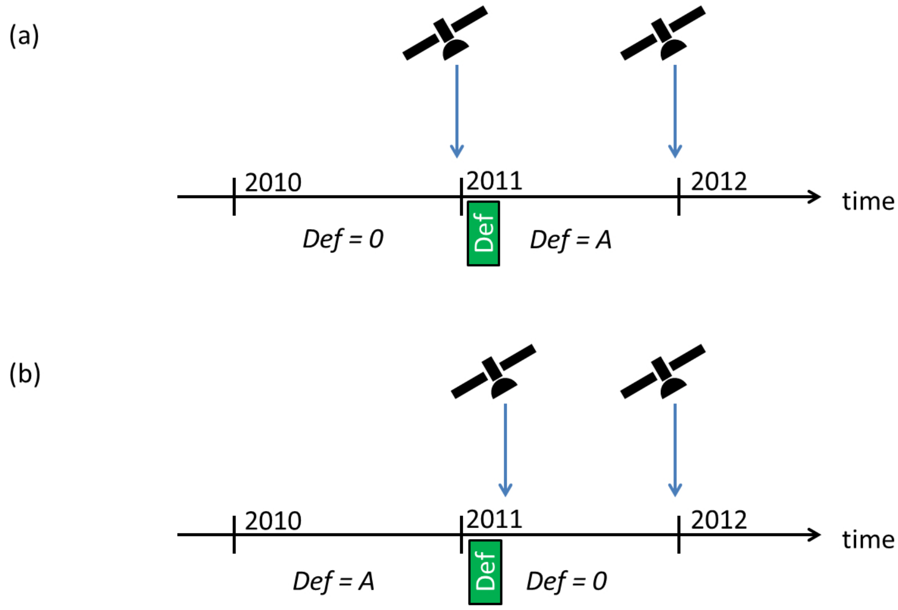
4.9.2 *Caveats from remotely sensed deforestation of small spatial units*

For our context, where the incidence of deforestation is low and the spatial units are small, several limitations of our outcome measurements should be considered: The role of regrowth in areas of low deforestation, the role of detection time lags, and the role of clouds for small spatial units.

The role of forest regrowth is a general limitation to INPE's deforestation data, which omit deforestation of secondary forests that have regrown on already deforested patches. This could introduce an analytical error in our context as we have large areas that have already been classified as deforested within the protected reserves in 2003. 4.1% of the area within reserves was on average already deforested in 2003. This analytical bias tends to be higher in our research context where deforestation rates are traditionally low and the need to extend logging into virgin forests is low as long as reusable areas of secondary forest is available.

The role of detection time lags for small spatial units emerges from the technicalities of INPE's remote sensing strategy. The date of satellite pictures can vary between May and October, and thus a given deforestation event may be attributed to one year or the next, depending on the timing of the picture (see Figure 4.10). Small spatial units are particularly prone to this problem, as the smaller the spatial unit, the higher the probability that deforestation occurs only during one or few days. We tackle this limitation by testing for the robustness of our results shifting the start of the policy to a one and two years

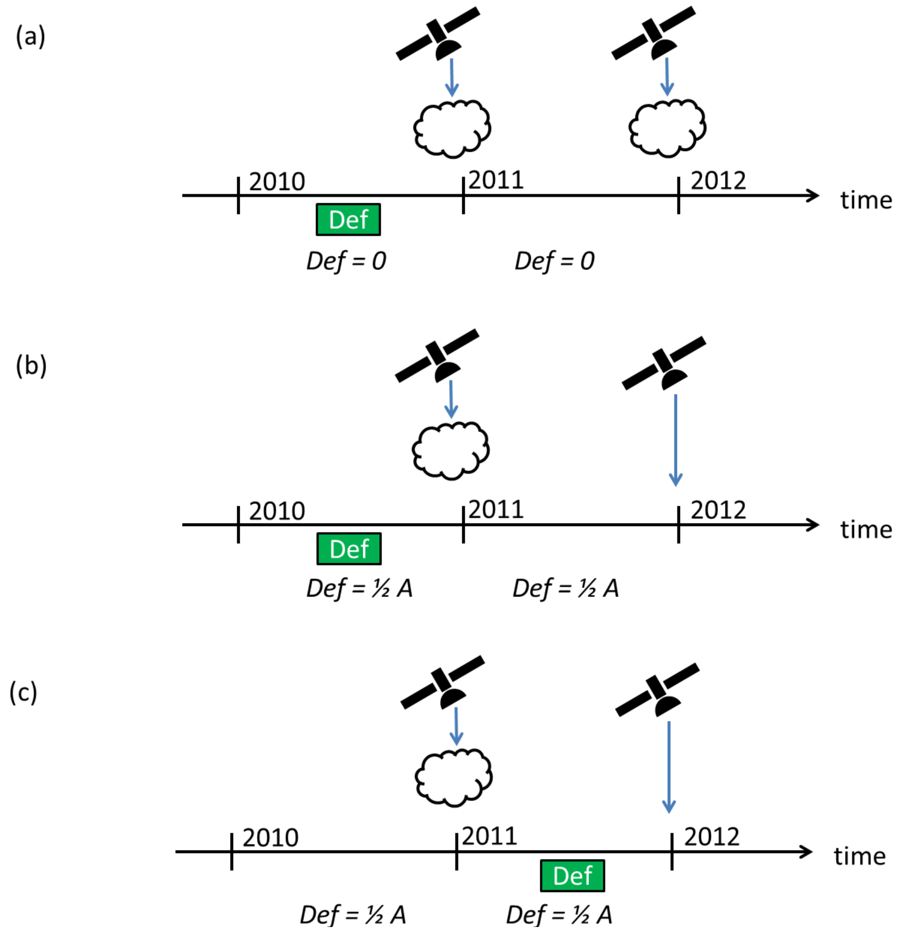
Figure 4.10: Satellite timing and deforestation measurement errors



before and after program start. Average impacts remain insignificant, for both directional shifts.

The role of clouds also deserves higher attention when using smaller spatial units. Small spatial units are highly sensitive to measurement errors from cloud coverage, as they can easily be fully obscured by clouds in a given satellite image. If a small spatial unit is fully covered by clouds till the end of our timeframe, it will receive a zero deforestation rate in all years, irrespective of whether deforestation has occurred (see Figure 4.11a). Alternatively, deforestation in a given year may remain undetected because of clouds that year but be detected the following year, and hence attributed to the wrong year (see Figure 4.11b). Conversely, if deforestation is detected at the end of a year but the area was clouded during the previous year, deforestation would be falsely attributed to both years, appearing go have occurred earlier (see Figure 4.11c). When observational units are relatively large in comparison with clouds, false attributions to future and previous years tend to cancel out, but this is less probable when units are small. Neighboring small units tend to be covered by the same cloud, therefore the measurement errors have the same direction and spatial-autocorrelation biases are created. We control for yearly cloud coverage in our estimation procedures and exclude all observations with more than 90% cloud coverage during our timeframe. Although, this does not solve the problem of misattribution entirely we are confident that the fairly large cell size avoids spatial-autocorrelation due to clouds.

Figure 4.11: Cloudy satellite images and deforestation measurement errors



4.10 CONCLUSIONS

In this section, we present results from the first regional counterfactual evaluation of the BFP's initial effects on outcome variables related to forest cover and forest quality (degradation and fire incidence). We emphasize that it was not the objective of this study to evaluate the intended development outcomes of the program.

Given the extremely low pre-program levels of deforestation in the intervention area and the relatively short time since the program began (2008-2012), a priori we can not expect to measure large conservation effects. Using a high spatial resolution and a large number of covariates reflecting potential confounding variables, we were indeed not able to demonstrate a robust overall conservation effect of the program. We show, nonetheless, that it would have been possible to measure an average positive conservation effect had deforestation entirely ceased in participating reserves. We further explore theoretically motivated hypotheses about heterogeneous treatment effects and find relatively robust conservation effects in local contexts characterized by high external and low internal anthropogenic pressure on natural resources. However, we also find largely consistent evidence for small increases in forest and forest quality loss in reserves subject to high internal and external pressure.

The usual caveats of quasi-experimental evaluation techniques, including the potential influence of unobserved confounding variables (Rosenbaum, 2002), apply also to this study. From PA research it is well known, however, that the usual source of (remote location) bias typically leads to an overestimation of conservation impacts. This in combination with our systematic and extensive robustness analyses makes us relatively confident that our analysis reflects the actual impacts of the program on the measured outcome variables up to the end of the observations in 2012. Continuing observation may or may not confirm the conditions described here.

Against this backdrop, we can only speculate why the BFP has not produced more robust measurable impacts on the observed outcome variables. Potential reasons include the overall slowdown in deforestation throughout the Brazilian Amazon that could have delayed the advance of the deforestation frontier towards the BFP reserves vis-à-vis the expectations of implementers in the program planning stage. In this case, only time will show to what extent program induced conservation alliances with reserve inhabitants are strong enough to achieve conservation levels beyond those that would have been achieved by protection alone.

If future analyses confirm the conjectures that emerge from our analysis of effects in the heterogeneous local resource pressure contexts of the BFP, the following lessons can be drawn: (1) PES-cum-ICDP programs aimed at forming alliances between reserve management and local forest users can be effective in conserving forests if external pressure on resources threatens local forest dependent livelihoods that are characterized by low conservation opportunity costs. (2) In situations of comparatively high internal and external anthropogenic pressures, and thus opportunity costs, conditional transfers require stronger elements of conditionality than currently imposed by the program to generate conservation effects.

Finally, what are the strategic policy implications from our findings? If no (or only negligible) deforestation and forest degradation happened both before and after the BFP was implemented in a reserve, and the same holds true for the logically designated low-threat control areas, then this can in a project evaluation still be seen as a success for the PA and the BFP in question, although it will mean that the incremental conservation impact of BFP in that case was nil. Such a zero-additionality scenario does thus not imply that the program was ill-designed or -implemented; however, it can raise legitimate questions about the pre-selection of predominantly low-threat areas: why invest considerable resources in additional 'treatment', when in environmental terms there was no sign of a 'disease' in the first place?

Certainly, a rationale for such a strategy could still for a number of reasons make sense. First, the construction of longer-term conservation alliances (i.e., acting in advance of future land-use and resource-extractive pressures) could be one reason for 'preventive treatment' or 'vaccination'. Second, the prize function of rewarding good stewardship with a grant could demonstrate to other communities the potential for conservation incentive strategies, even if the program had zero effects of improving that stewardship further – and it simultaneously brings welfare gains to the recipients. Finally, BFP was a pilot in Brazil both as REDD and PES initiative, and BFP as an early mover allows us to learn some lessons. Yet, the strategic question for prospective replications of the BFP approach is if these longer-term concerns are sufficiently good reason for the allocation of scarce conservation funding to remote low-threat areas.

Part III

CONCLUSION

5

CONCLUSION

5.1 SUMMARY

This thesis addressed the role of the political context and the effectiveness of mixed incentive strategies for tropical forest conservation in Brazil.

Conserving the remaining global reserves of natural forests and their ecosystem services hinges on the design of effective policy instruments. Most countries have environmental legislation and protection schemes in place (Kanowski, McDermott, and Cashore, 2011). Nonetheless, developing countries often lack an adequate forest cover monitoring system and/or an effective law enforcement infrastructure (Börner et al., 2015a). In Brazil, both these necessary conditions for effective forest conservation are in place and annual forest loss has fallen significantly since the early 2000s (Soares-Filho et al., 2014). This thesis shows the contribution of complementary interventions targeting the political context of conservation policy interventions.

To assess the relationships between political players, agricultural and economic elites, chapter 2 investigated a federal anti-corruption policy. Targeting Brazilian districts via a lottery, auditors controlled and reported the governance quality in health care, education, and public infrastructure related sectors, but excluded environmental policy mandates of local governments. Audit reports revealed a robust and positive correlation between the level of corruption and deforestation. In addition, the natural experiment of the policy allowed for a causal investigation of corruption disclosure. Results showed no effect of the public auditing on deforestation dynamics. However, other studies did find an auditing impact on the targeted outcomes. Hence, the agricultural and forestry sectors that drive Amazon deforestation were less prone to rent seeking spillover effects than anecdotal evidence on collusion between these sectors and local political elites might have suggested.

To understand the interaction of local governments and land users, chapter 3 analysed the blacklisting policy, a regularly published list of districts with the highest deforestation rates. Blacklisting demonstrated to be an effective measure to curb forest clearing. To disentangle the underlying mechanisms of the effect, an innovative combination of three empirical methods was used, namely: Matching and mechanism analysis using panel data estimation techniques. Net conservation effects after controlling for external incentive mechanisms, suggested in line with anecdotal evidence that *reputational risk* might have been an important driver of collective conservation action at the local level. Local administrations, farmers and NGOs had often organized to reduce deforestation and to exit the blacklist.

Successful environmental governance can be enabled through public policies. One incentive-based option to improve natural resource management in protected areas was investigated in Chapter 4. The study analyzed a Payments for Environmental Services (PES) program for households in combination with investments in forest-friendly production and infrastructure at community level. Located predominantly in a remote parts of the Amazonas state of Brazil, the program targeted reserves in areas with traditionally low deforestation pressures. On the aggregate, forest losses and forest degradation outcomes did not differ in comparison to non-participating reserves. Nonetheless, diverging effects between reserves are consistently significant. Forest losses are related to targeted reserves with high deforestation pressures, whereas additional forest conservation is detected in targeted reserves with low pressures inside boundaries, but high pressures in close distance.

5.2 POLICY DESIGN AND POLICY RECOMMENDATIONS

The evaluation results are summarized in Table 5.1. The forest conservation impacts of the anti-corruption, Blacklisting, and BFP interventions are on average: None, positive, and none, respectively. And yet, scrutinizing average effects for effect heterogeneity and underlying mechanisms, however, reveals that the underlying stories are more complex. For the development of better forest conservation policies, it is thus not sufficient to find out "what worked" and "what did not work". Decision makers need to know "why" interventions work (or not) and under which conditions. In theory, well designed policies should be effective. In practice, however, conservation policy design often deviates from scientific recommendations. Trade-offs between efficiency and equity as well as political motivations influence the design of conservation policies (Börner et al., 2010; Engel, Pagiola, and Wunder, 2008; Rosa da Conceição, Börner, and Wunder, 2015). In the context of tropical forest conservation, common design shortcomings include: sub-optimal spatial targeting (siting) and socio-economic targeting, insufficient monitoring and enforcement, and limited adaptation to peculiarities of local context (contextualization), including local governance structures. The potential gains in policy cost-effectiveness from addressing such shortcomings have been widely discussed (Engel et al., 2008; Minang and van Noordwijk, 2013; Lambin et al., 2014; Lemos and Agrawal, 2006). Nonetheless, further empirical work is needed to identify promising entry points for interventions towards enhancing the existing conservation policy mixes in the world's tropical forest regions. Quantitative comparative analyses and meta-studies could produce new insights and hypotheses about the relative importance of specific design elements (Bohm and Russell, 1985; Börner et al., 2016; Ezzine-de Blas, Wunder, Ruiz-Pérez, and Moreno-Sanchez, 2016). The following draws upon the framework as introduced in section 1.2 and sets into

Table 5.1: Design elements and impacts

	ANTI-CORRUPTION PROGRAM Ch. 2	BLACKLIST Ch. 3	BOLSA FLORESTA PROGRAM Ch. 4
Research question	Do anti-corruption measures affect forest conservation outcomes?	Does naming and shaming reduce deforestation? And, if yes, which mechanisms are at work?	Do conservation incentives in protected areas reduce deforestation?
Siting	From low to high opportunity costs	High opportunity costs	Low opportunity costs
Targeting	Indirectly political stakeholders	Directly political stakeholders and land-owners	Directly land-users
Monitoring	None	Satellite and CAR	informal and community-based
Enforcement	None	Inclusion to and holding within the blacklist	Community-based
Contextualization (policy type)	high (P3)	very high (P3, P4)	high (P4)
Approximate cost efficiency	Low (costly selection, auditing, prosecution)	High (inexpensive selection, costs burden out-shifted)	Very low (inexpensive selection, tough high costs from payments, administration, and targeting)
Conservation impact	None	Positive	None (Though, positive [negative] in reserves with low [high] internal pressures and high external pressures)

relation the three policy designs, their context, design elements and respective conservation impacts.

The siting of an environmental policy is defined by its spatial targeting criteria. Given scarce resources, conservation policies are best applied where high effects can be expected. The opportunity costs of forest conservation are often proposed as an important targeting criterion. Low opportunity costs tend to be associated with low deforestation levels and then any intervention will bring about little additional conservation. The potential of impacts increases as forest conversion becomes more profitable, but in the presence of exuberant conservation opportunity costs few intervention options will be effective either. The three analyzed policy interventions are sited across the Brazilian Legal Amazon (BLA) at different levels of opportunity costs of forest conservation (see Figure 5.1).The anti-corruption policy randomly selected districts and is spread across regions of the BLA with *low to high* levels of opportunity costs. For comparability, 49.3% of all forest clearings occurred within the 307 audited districts during the years 2002-2007.[1] By its randomized design, siting and the level of opportunity costs can not serve to explain the zero effect. The blacklisting policy targeted 50 high deforesting districts with *high* opportunity costs of forest conservation (50.3% of all forest clearings in the years 2002-2007). In consequence, blacklisting avoided approximately 4,022 sq. km of deforestation. In comparison, the BFP initiated its support in reserves of the Amazonas state, a region with *low* deforestation pressures. Its share on total deforestation amounts to 0.26% (2002-2007). The absence of significant deforestation pressures can be regarded as the main reason for the lack of measurable effects. Without playing down the multiple (including social) objectives and the long term strategy of the BFP, the comparison with the Blacklist example leads to a straightforward policy recommendation:

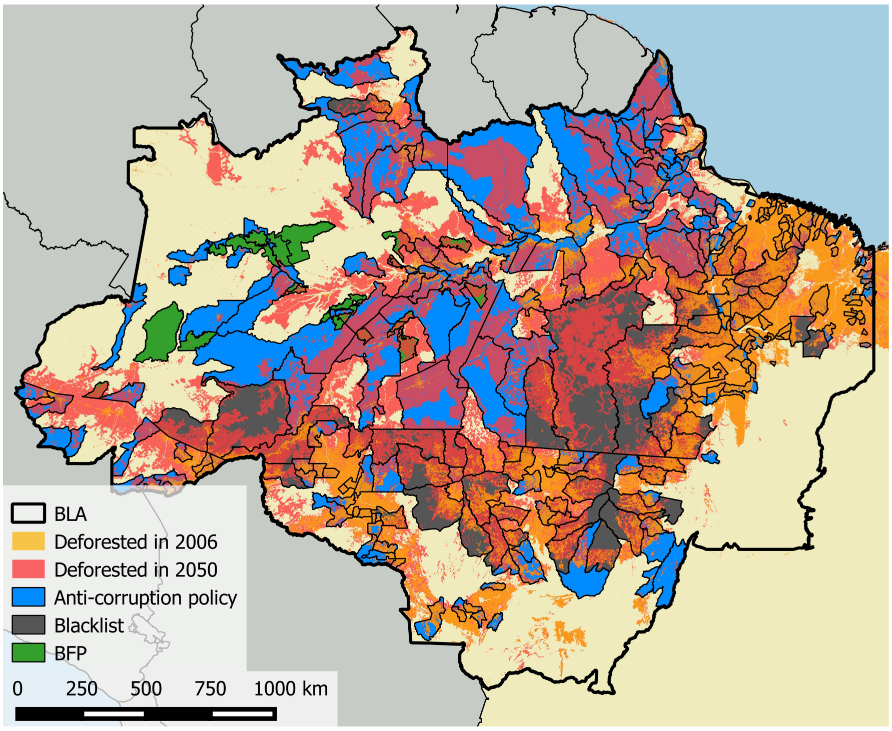
R1 For immediate conservation impacts, policies should target areas where deforestation pressures are high not only tomorrow, but also today.[2]

Development economics for a long time recognized the trade-offs between targeting and poverty alleviation effects (cf. Ravallion and Chao, 1989; Ravallion, 2003). Forest conservation research addressed the role of targeting firstly in relation with the protection of biodiversity rich regions (Margoluis, Stem, Salafsky, and Brown, 2009; Myers et al., 2000) before turning towards socio-economic indicators (Naidoo and Ricketts, 2006; Pagiola, Arcenas, and Platais, 2005; Wünscher, Engel, and Wunder, 2008) and deforestation risk indicators (Alix-Garcia, De Janvry, and Sadoulet, 2005; Wunder, 2009; Wünscher and Engel, 2012) . Targeting can also be understood as the ability of a policy to reach key stakeholders in the land and forest use system. The anti-corruption policy targeted districts' governance quality and thereby addressed a major stakeholder to forest conservation. Nonetheless, the program did not target environmental action of local governments and their outcomes. Although good governance and good public infrastructures are related to forest conservation, this *indirect* approach did not induce measurable additional conservation action. In contrast, the Blacklist directly targeted district administrations and land-based economic sectors. Thereby, the policy managed to target local politicians and land-owners, many of which then reportedly self-organized to engage in forest conservation. The BFP *directly* targeted land-users of the reserves, though participation and payments were provided irrespective of their opportunity cost. Newton et al. (2012) argue that variable BFP payments could induce higher conservation effects. Further, the analysis showed only positive conservation

1 The time-frame 2002-2007 refers to the pre-treatment periods of the Blacklist and the BFP. In chapter 2 only 227 forested districts are used for the analysis.

2 More precisely, a combination of high forest stock and high immediate pressure is needed for conservation policies to exhibit measurable effects in the time frames commonly used for impact evaluation. Policymakers may have different time frames, but should then adequately adjust their reference scenarios, which often serve as a basis for program funding.

Figure 5.1: Policy siting



Note: Data on predicted deforested area is obtained from Soares-Filho, Nepstad, Curran, Cerqueira, Garcia, Ramos, Voll, McDonald, Lefebvre, and Schlesinger (2006)

effects in contexts where residents have moderate opportunity costs but face higher pressures from outside of the reserve boundaries. Hence, whereas the BFP directly targeted residents it may have missed out on key stakeholders involved in forest harming activities.It can thus be concluded that:

R2 Programs involving conservation incentives should target *all* relevant stakeholders.

Most policies need monitoring and enforcement to induce behavioral change (Lambin et al., 2014; Ribot et al., 2006). Ideally, a monitoring system is capable of observing the intended and unintended outcomes of policies, whereas enforcement involves effectively following up on illegal behavior through liability establishment and coercion (e.g., by suspending PES or issuing fines). Although active enforcement requires some sort of monitoring, some policies can be effective without active enforcement. For example, in the case of the anti-corruption policy, federal prosecutors did the monitoring and public disclosure was enough to induce behavioral change in the monitored sectors as evidenced by previous research. Contrary to anecdotal evidence on the close links between local political elites and the land and forest-based economic sectors in the Amazon region, this thesis showed that such behavioral change seems to have left (non-targeted and unenforced) environmental outcomes unaffected. This finding, however, cannot be attributed to either of the missing design elements: targeting, monitoring, or enforcement. It remains to be shown whether corruption auditing in the local environmental policy administration, i.e., a hypothetical policy approach specifically targeted to the outcome of interest in our analysis, would produce measurable change. The blacklisting policy has clearly defined its monitoring element. Deforestation rates and land registrations in the rural cadaster (CAR) are traced. Enforcement is conducted by keeping districts in the Blacklist, which are non-compliant with the environmental goals (reduced deforestation and increased land registrations). These well formalized design elements are probabe to have contributed to the large impact of the policy. The BFP had not defined a monitoring system for its PES contracts. Undefined land rights within the targeted reserves made an individual monitoring unfeasible. Informal monitoring did exist at the community level by the staff members of the BFP. In addition, community-based monitoring and enforcement was probalby the case because compliant participants would suffer from non-compliant behavior of fellow residents or outsiders. The weak or non-existent formal monitoring and enforcement design of the BFP are indispensable to understand the observed small but negative conservation effects in areas with higher deforestation pressures. Residents of reserves with comparatively high historical deforestation rates within as well as outside reserve boundaries tend to be exposed to economic opportunities in agriculture and forestry. In these cases an incentive program with stricter monitoring and enforcement measures could have helped to avoid a capitalization of forest-harming practices.

R3 In cash-constrained local economies, monetary conservation incentives can increase the opportunity costs of conservation and need to be accompanied by stricter monitoring and enforcement action.

Contextualized environmental policies are interventions adapted to conditions of the targeted area. Context characteristics have a major influence on the effectiveness of policies (cf. Lambin et al., 2014). Hence policies adapted to the context can achieve higher impacts (Engel, 2015) Contextualization as a design element was introduced in chapter 1 by presenting how policies handle different characteristics (bio-physical, socio-economic and political) and actors of a given context. Policies of type P1 were defined as interventions without apprehension of the diversity in characteristics and actors determining environmental outcomes. Policies of type P2 are designed to target specific context characteristics. Policies of type P3 aim at actors within given contexts and provide incentives and disincentive to steer allegiance with existing policies. Lastly, policies of type P4 are designed to incentivize, disincentivize or enable actors to create own interventions within their context. The degree of contextualization, therefore, depends on the policy goal and its other design elements (siting, targeting, monitoring, enforcement). The anti-corruption policy can be described as a type P3 policy. Although, it did not target a specific region , it discouraged specific (corrupt) actors in specific local administrative sectors. Hence, the policy exhibits a high degree of contextualization. As discussed above, this thesis looked at an hypothesized spillover effect of the policy's targeting strategy that could not be confirmed. The Blacklisting policy targeted districts with high deforestation rates. It recognized political actors as inhibitors to environmental policy application (type P3). In parallel, the disclosure of the blacklist motivated conservation partnerships between politicians, farmers and NGOs (type P4). The combination of both approaches resulted in large deforestation reductions. The bundle of PES and infrastructural investments of the BFP can be interpreted as a type P4 policy. Highly contextualized, the program increased the management quality of state reserves and incentivized residents to develop forest friendly production systems. Nonetheless, the program created ambiguous impacts that can best be explained by the insufficient monitoring mechanism. In synthesis, it can be recommended:

R4 To increase the potential success of conservation efforts, contextualized policies can remove potential inhibitors to conservation and motivate actors to create new conservation incentives.

A complete policy evaluation must finally include a cost estimation. This thesis did not focus on cost-efficiency, but a brief inquiry on the ratio between impacts and investments can provide helpful insights. Furthermore, we consider only implementation costs of each policy and leave out transaction costs. The 'cost efficiency winner" is clearly the Blacklist, which is the only policy with a significant impact (25.6% or 4,022 sq. km of avoided forest loss).[3] Nonetheless, this thesis and the discussion above argues that some effects could have been generated by the other two policies, had they been designed differently. An alternative design of the anti-corruption program could have included environmental outcomes and scrutinized the performance of local environmental governance. For the sake of the argument, lets assume the estimation results in chapter 2 were significant. This would result in deforestation reductions by 10.1% or 3,932 sq. km.[4] The BFP showed a reduced forest loss of 9.2% in a subset of reserves.[5] With stricter enforcement mechanisms within the other reserves, the same effect could be assumed in the remaining reserves with an avoided forest loss of 2.8 sq. km.Given these back of the envelope calculations, the implementation costs of avoided deforestation were: 22,920 *reais* per sq. km for the anti-corruption program, 60 *reais* per sq. km for the Blacklisting policy, and 5.1m *reais* per sq. km for the BFP.[6] As previously discussed, the oppor-

3 See Table 3.2, model 3.
4 See Table 2.5, model 4.
5 See Table 4.4, model 3.
6 The anti-corruption policy sent a maximum of 15 auditors to each of the 225 selected districts (Ferraz et al., 2012). In addition, it can be assumed that auditors spend a full month for each selected district. The maximum wage of public officials in Brazil is the salary of supreme-court judges, with 26,700 *reais* (although fraud exceptions do exist) (The Economist, 2012). Summing up, the salary costs probably did not exceed 90.1m *reais*. The three published Blacklists base on public data and are assumed to have occupied a maximum of 3 public officials for one month each. This gives salary costs of 240,300 *reais*. The BFP gave 50 *reais* to each of the 8,000 families (standing in 2008) for at least 3 years, summing to 14.4m *reais* of implementation costs (without cost for the other parts of the program or staff salaries). Cost values refer to the

tunity costs of forest conservation were at the average for the regions of the anti-corruption program, high for the Blacklist and low for the BFP.[7].

In sum, the Blacklist stands out as a low-cost and high-impact strategy - implemented in an area of high opportunity costs it spent little funds and managed to significantly reduce deforestation. The anti-corruption policy, in our hypothetical scenario, exhibits a median level of cost efficiency. Nonetheless, real transaction costs and opportunity costs are difficult to assess with certainty. Lastly, the BFP, situated in areas with historically low deforestation, invested large amounts of resources to offset the low opportunity costs of protected area residents. Even in a conservative scenario, the program thus exhibits a relatively low level of environmental cost-effectiveness - its potential development benefits notwithstanding. However, achieving forest conservation by disclosing information to the public, blaming key stakeholders, scrutinizing environmental compliance and motivating forest-friendly partnerships between politicians and farmers is only cost-effective from an implementation cost perspective. An important share of the total economic costs of the policy is borne by the land users.

R5 Public disclosure as a cross-compliance mechanism can be a cost-effective instrument to increase compliance with environmental regulations.

The policy recommendations outlined above largely assume a reasonably well functioning institutional framework and enforcement system. Adequate siting, careful targeting, thorough monitoring and enforcement or wise contextualization are only sufficient conditions for the success of policies. Good design without siting interventions in relevant areas, cannot create additional forest conservation. Satisfactory siting without targeting key stakeholders that would not comply in absence of the intervention will fail to produce additional conservation. Without monitoring and enforcement, perfect siting and targeting has little chance to induce behavioral changes. Lastly, contextualization can only be a complementary strategy to improve actors' environmental governance.

This thesis has demonstrated that context specific environmental policies, especially those targeting political actors, can be both ineffective or effective. Success depends on the design features of the policy and the broader policy context, in particular environmental law and enforcement action. Conservation policy instruments aimed at political actors can then exercise tangible pressure towards encouraging local (including collective) conservation action.

year 2012 with an exchange rate of 1.95 *reais* per dollars (`http://www.federalreserve.gov/Releases/H10/20120130/`).

7 For comparison, Börner et al. (2010) analyze the potential of REDD payments in the BLA and calculate opportunity costs between 14.33 *reais* per sq. km and 17.80 *reais* per sq. km across the Brazilian Amazon

Part IV

APPENDIX

A

APPENDIX TO CHAPTER 2

A.1 TABLES

Table A.1: Summary statistics on total deforestation and initial conditions

	Mean	St.dev.	Min.	Max.
Total deforestation	275.47	564.54	0.50	8146.25
Corruption (standardized)	-0.01	0.91	-0.73	5.42
Corruption (standardized yearly)	0.00	0.94	-1.26	4.33
Irregularities (standardized)	-0.02	0.99	-1.86	3.49
Irregularities (standardized yearly)	0.01	0.99	-2.25	3.18
Irregularities p. program (standardized)	-0.02	0.98	-1.49	3.85
Irregularities p. program (standardized yearly)	0.03	0.98	-1.90	3.45
Initial forest	5955.53	14597.76	12.81	150502.20
Cloud error	0.16	0.25	0.00	1.00
Distance to Brasilia [km]	1477.85	499.54	369.94	2872.22
Savanna area	987.32	2363.35	0.00	19759.08
Initial population (2000)	27325.61	36976.44	1365.00	471980.00
Ini. real GDP p.c.	6881.20	6877.06	1466.24	73753.38
Ini. real GDP p.c. in agriculture	1596.96	5912.70	1.37	73168.49
Ini. Multiple-use protected area	554.99	2118.80	0.00	39093.36
Ini. Strictly protected area	351.27	1851.57	0.00	30239.70
Ini. Indigenous territory	1168.00	5666.61	0.00	88371.95
Ini. Settlement project size	407.3584	844.3924	0	10684.7

Note: Statistics refer to $N = 550$ originally forested districts. Governance variables are standardized over the sample of $N = 237$ audited districts in the Legal Amazon. Ini. real GDP p.c. and Ini. real GDP p.c. in agriculture respectively to 548 and 516 districts. Area values are in units of sq. kilometer. Monetary units are deflated values of Brazilian Reais.

Table A.2: Summary statistics of the panel data (2002-2012)

	Mean	St.dev.	Min.	Max.
Deforestation [sq. km]	25.04	63.82	0.00	1307.89
Audit	0.27	0.50	0.00	3.00
Standardized corruption	0.00	0.64	-0.73	8.85
Yearly stand. Corruption	0.00	0.64	-1.26	4.85
Standardized irregularities	-0.01	0.67	-1.86	3.49
Yearly stand. Irregularities	0.00	0.66	-2.25	3.18
Standardized relative irregularities	-0.01	0.66	-1.49	3.85
Yearly stand. relative irregularities	0.01	0.66	-1.90	3.45
Neighb. deforestation rates	29.99	52.55	0.00	684.00
Neighb. Audits	0.27	0.30	0.00	1.67
Neighb. corruption (yearly st.)	0.01	0.32	-0.74	2.57
Neighb. relative irregularities (yearly st.)	0.02	0.36	-1.09	1.60
Clouds error [%]	0.10	0.20	0.00	1.00
First term mayor	0.70	0.46	0.00	1.00

Note: For *First term mayor* statistics refer to N=5489 observations (on 499 districts). Values of neighboring covariates refer to N= 6039 (on 549 districts) one observation less than for all other variables with N=6050 (on 550 districts). This results because of one district having no neighbor in the subsample.

A.2 LIST OF SEMANTIC EXPRESSIONS

Listing A.1 provides a coding example for the construction of a corruption measurement. The process searches for 40 expressions indicating to a corrupt event, grouped into seven categories. We use **R**'s grep function (originally developed for Ubuntu) to scan each text line for a match with the mass of regular expressions (R Core Team, 2015). Thereby, the constructed corruption measurement represent the number of lines having at least one corrupt expression.

Listing A.1: Counting corrupt expressions (R code)

```
###read text file
textfile <- readLines("_usr_include_php_projetos_scas_arquivos_26—AM—
    Tapaua.txt")

### Corrupt expressions
## 1 Diversion of public funds
expr_d1 <- "(valor*.*indevido*|indevido*.*valor*)"
expr_d2 <- "(pag[a*|o*]*.*indevido*|indevido*.*pag[a*|o*]*)"
expr_d3 <- "(utilizado*.*indevido*|indevido*.*utilizado*)"
expr_d4 <- "(não.*utilizaç*.*objeto)"
expr_d5 <- "(não.*comprovad*.*utilização.*recursos*)"
expr_d6 <- "(não.*atesto*.*recebimento*|falta.*atesto*.*recebimento*|
    nenhuma*.*atesto*.*recebimento*|sem.*atesto*.*recebimento*|ausência.*
    atesto*.*recebimento*)"
expr_d7 <- "((não|nenhuma|falta|ausência).*comprov*.*recebimento*)"
expr_d8 <- "((não|ausência).*comprov*.*depósito*)"
expr_d9 <- "(despesa*.*não.*previsa*|despesa*.*prevista*.*não|prevista*.*n
    ão.*despesa*|prevista*.*despesa*.*não|não.*despesa*.*prevista*|não.*
    prevista*.*despesa*)"
expr_d10<- "(ausência.*entrega.*medi*|ausência.*entrega.*equi*|ausência.*
    entrega.*mate*|ausência.*reci*.*entrega)"
expr_d11<- "(Lei n° 9.424|Lei n° 9.394)"
expr_d12<- "(incompatí*.*FUNDEF)"
## 2 Irregular procurement
expr_i1 <- "(sem.*prévio.*empenho*)"
expr_i2 <- "(Lei n° 4.320)"
expr_i3 <- "(sem.*devido*.*licitató*|não.*devido*.*licitató)"
expr_i4 <- "(Lei n° 8.666)"
## 3 Over-invoicing
expr_o1 <- "(sobrepreço*)"
expr_o2 <- "(acima.*mercado*|mercado*.*acima)"
expr_o3 <- "(elevado*.*preço*|preço*.elevado*)"
## 4 Fraud
expr_f1 <- "(pago*.*pessoa*.*não)"
expr_f2 <- "(servidor*.*com recursos)"
expr_f3 <- "(secretária.*com recursos)"
expr_f4 <- "(empresas*.*agente*.*público*|agente*.*público*.*empresas*)"
expr_f5 <- "(firma.*agente*.*público*|firmas.*agente*.*público*)"
expr_f6 <- "(firma .*prefeito|firmas .*prefeito*)"
```

```
expr_f7 <- "(sem.*empenho*.*notas*|não.*empenho*.*notas*|falta.*empenho*.*notas*)"
expr_f8 <- "(não.*notas*.*fiscais|falta*.*notas*.*fiscais)"
expr_f9 <- "(ausência.*notas*.*fiscais.*originais*)"
expr_f10<- "(cláusulas*.*restritiva*)"
expr_f11<- "((não|sem|falta|nenhuma|nem|ausência).*comprova*.*compra)"
## 5 Incomplete construction
epxr_c1 <- "(não.*construída*)"
epxr_c2 <- "(não.*realizad*.*(construçoe s| obras |construção| obra )|(construçoe s| obras |construção| obra ).*não.*realizad*)"
epxr_c3 <- "(não.*executad*.*(construçoe s| obras |construção| obra )|(construçoe s| obras |construção| obra ).*não.*executad*)"
epxr_c4 <- "(não.*construíd*.*(construçoe s| obras |construção| obra )|(construçoe s| obras |construção| obra ).*não.*construíd*)"
epxr_c5 <- "(não.*concluíd*.*(construçoe s| obras |construção| obra )|(construçoe s| obras |construção| obra ).*não.*concluíd*)"
epxr_c6 <- "(execu.ão.*parcialm*)"
epxr_c7 <- "((recursos*| obra).*equivale.*%|equivale.*%.*( obra| obras|recursos*))"
## 6 Inexistence of documentation
epxr_t1 <- "((informaçõ*|documentaçã*).*não.*disponibi*|não.*disponibi*.*(informaçõ*|documentaçã*))"
epxr_t2 <- "((informaçõ*|documentaçã*).* omissã*| omissã*.*(informaçõ*|documentaçã*))"
## 7 Advanced payment
epxr_a1 <- "(antecipad*.*pagam*|pagam*.*antecipad*)"

### Collection of expressions
expr_all<- paste(
  expr_d1,expr_d2,expr_d3,expr_d4,expr_d5,expr_d6,expr_d7,expr_d8,expr_d9,expr_d10,expr_d11,expr_d12,
  expr_i1,expr_i2,expr_i3,expr_i4,
  expr_o1,expr_o2,expr_o3,
  expr_f1,expr_f2,expr_f3,expr_f4,expr_f5,expr_f6,expr_f7,expr_f8,expr_f9,expr_f10,expr_f11,
  epxr_c1,epxr_c2,epxr_c3,epxr_c4,epxr_c5,epxr_c6,epxr_c7,
  epxr_t1,epxr_t2,
  epxr_a1,
  sep="|")

### Matching lines
matches<-grep(pattern=expr_all,x=textfile)

### Count matching lines
length(matches)
```

Note: FUNDEF, engl.: Fund for the Maintenance and Development of Fundamental Education and Teacher Enhancement, port.: Fundo de Manutenção e Desenvolvimento do Ensino Fundamental e de Valorização do Magistério

B

APPENDIX TO CHAPTER 3

B.1 CALCULATION OF YEARLY DEFORESTATION RATES PER DISTRICTS

Calculation of annual deforestation. In addition to district-level annual gross deforestation tables, the Brazilian Space Research Center (INPE) provides the spatial data files used to estimate gross deforestation rates for public download on an annual basis. The data are provided in shape file format for each Landsat path row. In the 228 downloadable layers that cover the Brazilian Legal Amazon (BLA) in 2012, polygons are classified into remaining forest, hydrography, non-forest (swamps, savanna), clouds, residual, deforested and detected in 1997, deforested and detected in 1999, deforested and detected in each year between 2000 and 2012. INPES classification methodology of deforestation detects only clear cut deforestation on areas that have never been deforested. Forest regrowth on abandoned plots is not accounted for. For each deforested polygon a detection date is provided which we call the end_date. To construct annual deforestation levels from the spatial data we need to know when the deforested polygon had last been classified as forest, we call this the start_-date. For each deforestation polygon INPE also reports the number of years it was covered by clouds during previous years. To define the start date for each polygon we thus rely on the shape files provided for previous years. In Figure B.3 polygons with different start and end dates are shown schematically. Each of the three polygons could have been deforested at any time or gradually between its start and end date. Polygon 3, for example, spreads over 2 years, as it has been covered by clouds in the image of 2009. We assume that the polygon could have been deforested with the same probability at each point in the relevant time frame and then aggregate the expected deforestation for each day to estimate how much deforestation has most likely occurred between the 1st of August and the 31st of July of each year in each district. This procedure differs slightly from INPE's methodology described in Câmara, de Morisson Valeriano, and Vianei Soares (2006). Here it is assumed that deforestation could only have occurred in a specific dry season for each pathrow.

The main difference between our and INPE's approach is that we do not calculate expected deforestation below clouds. INPE uses the cloud coverage of each year to estimate deforestation below clouds relative to the ratio of deforestation to forest within a specific area. The published deforestation rates therefore refer to detected deforestation plus expected deforestation below clouds. For the purpose of estimating the impact of blacklisting at a specific date on deforestation after this date, we prefer to use raw deforestation calculated as described above. We then use cloud cover (see below) as an indicator of measurement error in our regression. By using the official 2007 administrative boundary shape file for Brazil as published by the Brazilian Institute for Geography and Statistics (IBGE) we ensure that our deforestation estimate is consistent across all data sources.

Figure B.4 depicts the total aggregated yearly deforestation rates per district published by INPE and our estimation of deforestation rates. The data processing and area calculations are conducted with PostgreSQL 9.2.3 and the PostGIS 2.0.1 add-on. The difference between our calculated total deforestation in the BLA and the official values are the result of large cloud cover in the early years of the observation period.

Calculation of yearly forest area. Our forest area calculations for each year are anchored to the year 2012 based on the shape files published in the same year. We calculate the remaining forest area in 2012. INPE only classifies cloud coverage above the remaining forest areas. We therefore add to the calculated forest area in 2012 area of clouds. Thereafter we subtract from our calculated yearly deforestation rates (see above) to arrive at forest area for each year prior to 2012. As a result forest, non-forested and deforested areas always sum up to 100% of the district area.

Calculation of measurement (cloud) errors. Since clouds are not randomly distributed over space and time we have to control for the fact that some deforestation polygons were detected only after the respective area had been under clouds for several years. Especially in the most recent year of our time period the cloud cover will veil some deforestation. But also between 2000 and 2005 PRODES estimates suffered from large cloud cover. We use the percentage share of yearly cloud areas over the remaining forest area as a yearly indicator to the measurement error of deforestation within districts.

Area calculation of protected areas. Spatial information on the protected areas of Brazil from IBAMA as reported in Table B.1 below. We calculate protected area coverage within the districts of the BLA separately for multiple-use reserves, strictly protected areas and indigenous territories. The information on each protected unit comes with its respective decree number and date of establishments. This allows us to calculate the yearly cover of protected areas within each district for each year. We use the same time frame that applies to our deforestation data from August to July. For example a protected area that was established before the 31st of July 2010 will be used to calculate total protected area per district in 2010.

Area calculation of landholdings registered within the Cadastro Ambiental Rural (CAR). The spatial information on each registered landholding is divided into definite and provisional CAR. The former comprises CAR properties that have already gone through the verification process of the States' Special Secretariat of Environment (SEMA). Because the relative share of definite CARs is rather small we rely on the total registered area. Every CAR registration comes with a date and/or a date of submission. We rely on this information to calculate the total annual CAR area within districts using July 31st as cut-off date. The spatial database on CAR registrations has a large amount of overlaying polygons. We deal with this fact by merging all CAR registrations within each year to one single layer without overlays. The resulting 6 layers for the years 2007 to 2012 are then intersected with the district layer to calculate annual CAR coverage.

B.2 PARALLEL TIME TRENDS OF DEFORESTATION BEFORE TREATMENT

Testing for pre-treatment parallel trends in deforestation. A key assumption in our analysis of blacklisting effects on deforestation is that forest loss would have followed the same trend in the blacklisted districts and the non-blacklisted districts in the absence of blacklisting. Since our database covered the years between 2002 and 2007, we can test whether this has been the case before the blacklisting policy started in 2008. As per equation 3.2 we rely on a first difference (FD) specification with log of deforestation as a dependent variable. Specifically, we thus assume equal growth trends in deforestation conditional on all covariates (see right panel of Figure 3.3):

$$E[\Delta \ln D_{it} | \Delta \mathbf{X}_{it}, \Delta B_i = 1] = E[\Delta \ln D_{it} | \Delta \mathbf{X}_{it}, \Delta B_i = 0] \qquad \text{for } t = \{2002, \ldots, 2007\} \tag{B.1}$$

We use two approaches to testing for pre-treatment equality of trends. The first test uses the full matched sample and the second (Chow's test) compares trends of the blacklisted with non-blacklisted subsamples (Chow, 1960).

The full sample test relies on the following model using the notation from equations 3.1 and 3.2:

$$\begin{aligned} \Delta \ln D_{it} \quad = \quad & + \beta\, B_i + \lambda_1 \cdot t + \lambda_2 \cdot t \cdot B_i + \Delta \mathbf{X}'_{it} \gamma + \mathbf{Z}'_i \delta + \kappa_s + \Delta \varepsilon_{it} \\ & \text{for } t = \{2002, \ldots, 2007\} \end{aligned} \tag{B.2}$$

The deforestation growth trend for non-blacklisted districts is given by λ_1. The trend of the blacklisted districts is the linear combination of $\lambda_1 + \lambda_2$. We use the Wald test for linear combinations to test the null-hypotheses that or $\lambda_1 = \lambda_1 + \lambda_2$ or $0 = \lambda_2$. The alternative hypothesis is , which we expect to reject if pre-treatment trends were equal.

For Chow's test we first estimate the two models:

$$\Delta \ln D_{it|B=1} = + \beta\, B_i + \lambda^1 \cdot t + \Delta \mathbf{X}'_{it} \gamma + \mathbf{Z}'_i \delta + \kappa_s + \Delta \varepsilon_{it} \tag{B.3}$$

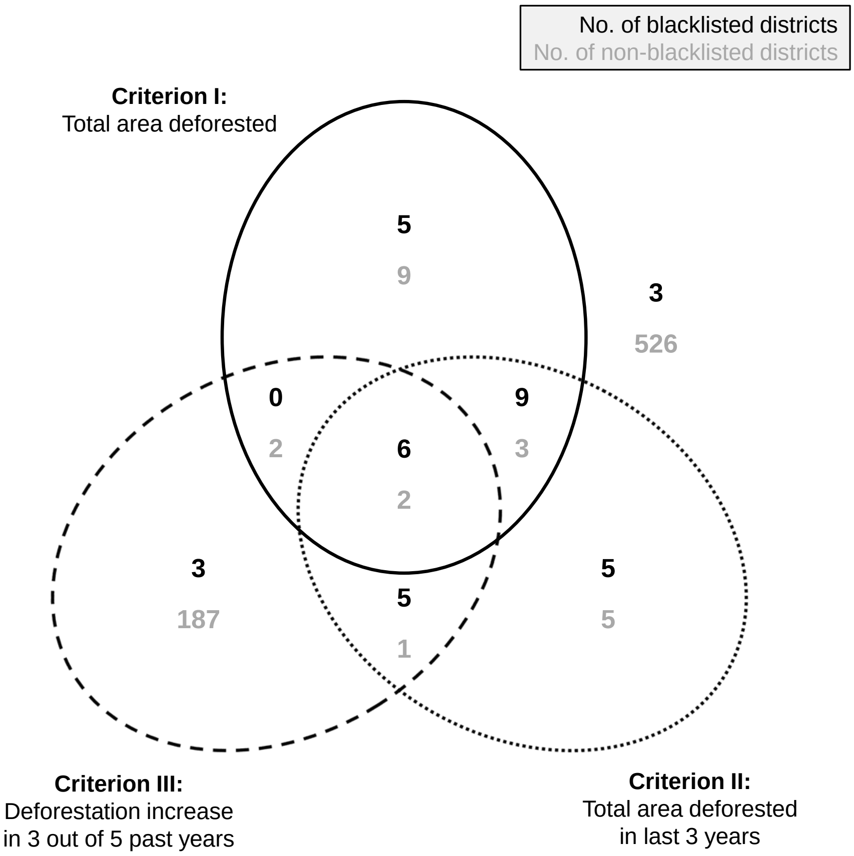
$$\Delta \ln D_{it|B=0} = + \beta\, B_i + \lambda^0 \cdot t + \Delta \mathbf{X}'_{it} \gamma + \mathbf{Z}'_i \delta + \kappa_s + \Delta \varepsilon_{it} \tag{B.4}$$

i.e., we run the same regression for blacklisted and matched non-blacklisted subsamples. To test if the time trends of both groups are equal we implement a Chow test to compare the two coefficients λ^1 and λ^0 (Chow, 1960). In the case of equal pre-treatment deforestation growth trends we would fail to reject the Null-hypothesis that $\lambda^1 = \lambda^1$.

Results on the test of parallel time trends. The estimation results of equations B.2, B.3, B.4 are presented in columns 1-3 of Table B.9, respectively. Our first test produces an F-statistic of 2.63 and we thus cannot reject the null hypothesis of equal time trends with a p-value of 0.109. This means that the two coefficients a not significantly different from each other. Our second test shows the same result with a Chi-squared statistic of 1.30 and a p-value of 0.257. We also restricted the samples to years between 2003 to 2007, 2004 to 2007 and 2005 to 2007, to see if our assumption also holds for a fewer years before the blacklisting policy starts. Results are not reported as all tests produce equivalent results.

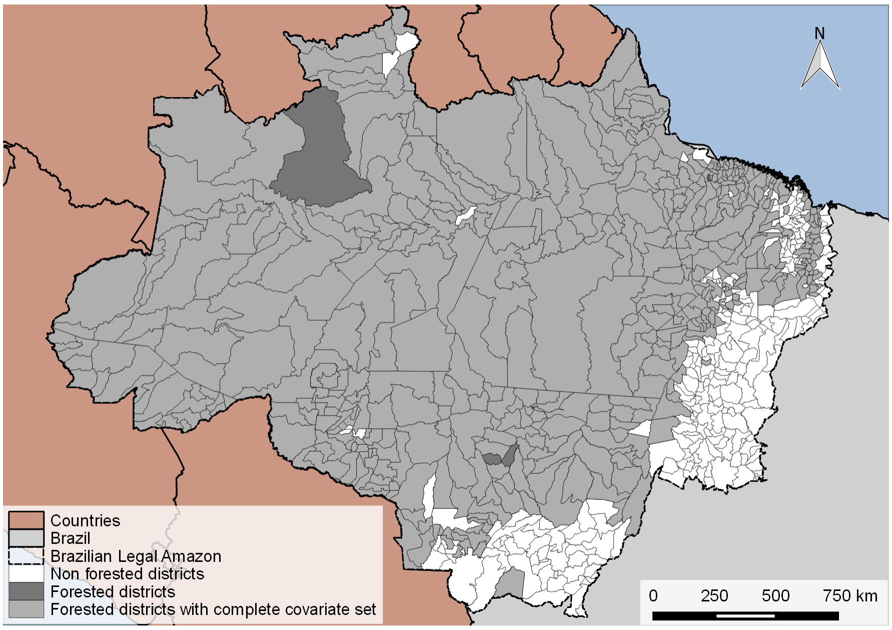
B.3 FIGURES

Figure B.1: Blacklisted districts and the blacklist criteria



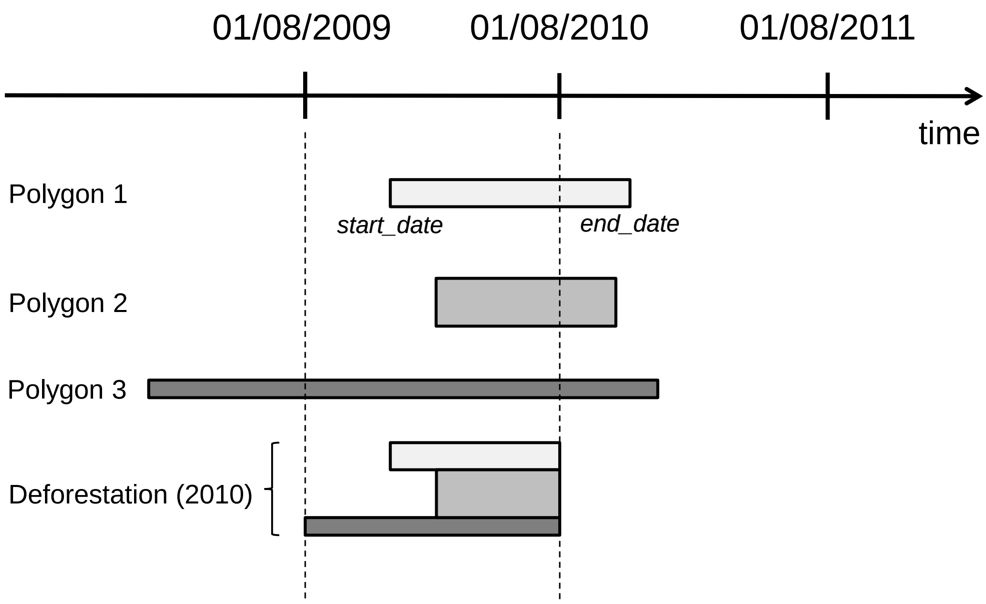
Note: The Venn diagram depicts the number of districts blacklisted and non-blacklisted from the first published list in 2008. Counts are based on PRODES official deforestation data. The blacklist was composed during the year 2008, therefore we consider for the first criterion the total deforested area until 2007. The first 36 districts with the highest deforested area fulfill criterion I. The first 36 districts with the highest deforested area between 2005 and 2007 fulfill the second criteria. All districts that at least show 3 years with increasing deforestation rates between 2003 and 2007 fulfill criterion III.

Figure B.2: Forested districts of the Brazilian Legal Amazon



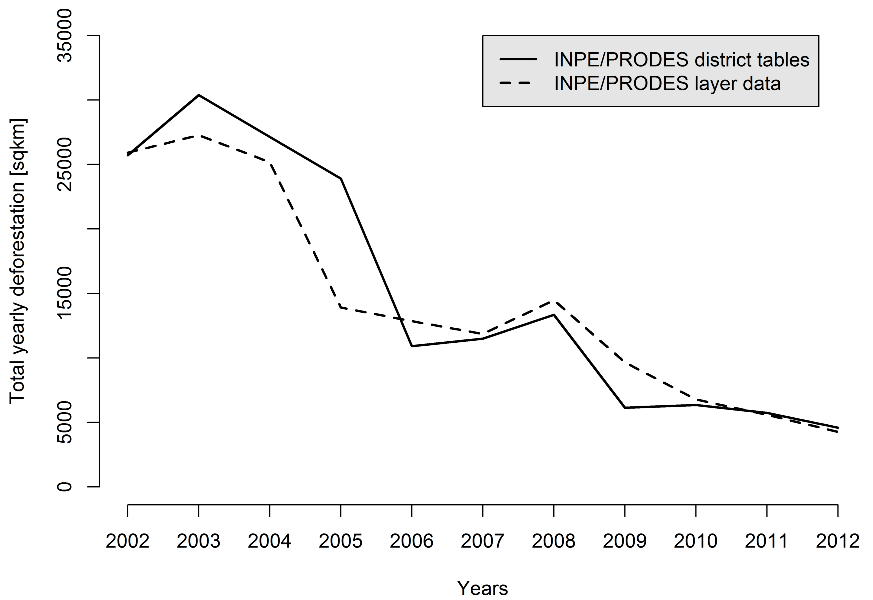
Note: The map shows all districts of the Brazilian Legal Amazon, defined by INPE (771). In light grey are all districts with more than 10% forest cover in 2002 and complete information on all covariates used for the analysis (492). In dark grey are forested districts with incomplete data on the covariates (6).

Figure B.3: Deforestation polygons to aggregate deforestation rates



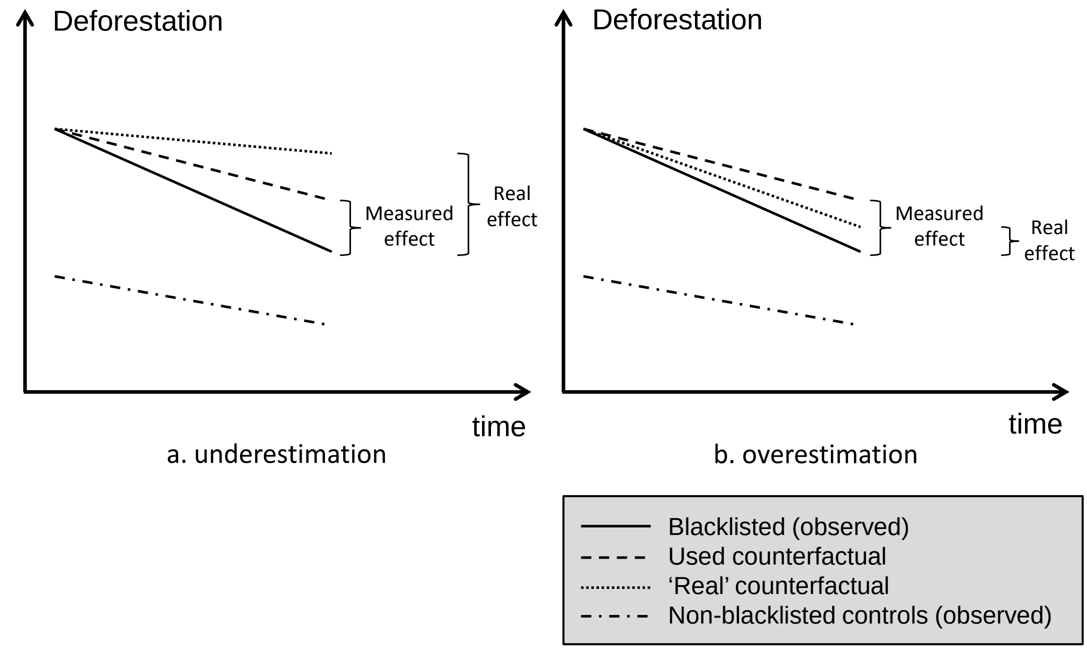
Note: Three detected deforestation polygons by satellite imagery are represented by the closed lines. The detection date of each polygon (end_date) represents the last date it could have been deforested. The first date an area could be deforested (start_date) is determined by the last satellite image that determined the polygon as forested. Annual deforestation rates are constructed by the sum of all polygons weighted by the share of the polygons' time frame within a given year.

Figure B.4: Yearly total deforestation in the Brazilian Legal Amazon



*N*ote: The solid line shows yearly deforestation rates calculated by the INPE/PRODES project for the districts of the Brazilian Legal Amazon (771). The dashed line shows deforestation rates calculated from INPE's shapefiles.

Figure B.5: Parallel time trend assumption and potential biases conceptually



*N*ote: Panel a. shows the case of underestimating the impact due to selection bias where the real counterfactual of the blacklisted (had they not been treated) exhibits slower deforestation decreases than the used counterfactual, constructed from the control districts. Panel b. depicts the case of overestimating the impact where the real counterfactual of the treated districts would have had faster deforestation decreases than the used counterfactual (e.g., Ashenfelter's dip).

B.4 TABLES

Table B.1: Data sources

Variable	Year(s)	Source
Blacklist additions and removals	2008-2012	Decree 6.321/2007 and Provision 28/2008, Provision 102, 203/2009, Provision 66,67,68/2010 , Provision 138, 139, 175/2011, Provision 187,322,323,324/2012 (Imprensa Nacional do Brazil, 2007)
Deforestation and clouds	2002-2012	INPE (2012) - PRODES
District list and borders	2007	IBGE (2013a)
Protected areas	2002-2012	IBAMA (2013)
Indigenous areas	2002-2012	IBAMA (2013)
Settlement areas	2002-2012	INCRA (2010)
Mayors' party affiliation	2002-2012	TSE (2015)
IPCA price deflator	2002-2012	IBGE (2013b)
Soy prices	2002-2012	IBGE (2010) - PAM
Timber prices	2002-2012	IBGE (2014) - PEVS
GDP	2002-2011	IBGE (2013)
Number of farms	2006	IBGE (2006) - Agricultural Census
Share of land owners	2006	IBGE (2006) - Agricultural Census
Land value per ha	2006	IBGE (2006) - Agricultural Census
Number of tractors	2006	IBGE (2006) - Agricultural Census
Cattle stocking rate	2006	IBGE (2006) - Agricultural Census
Population	2007	IBGE (2000)
Average distance to district center		Nelson (2008)
Field-based law enforcement inspections	2001-2012	IBAMA (2015)
Landholdings registered within the Cadastro Ambiental Rural (CAR)	2002-2012	Database provided by the Amazon Environmental Research Institute (IPAM) in October 2013
Rural credit	2002-2012	BCB (2015)

Table B.2: Summary statistics on regression variables

	N	Mean	SD	Min.	Max.
TIME VARIANT VARIABLES					
Blacklisted	5412	0.04	0.18	0	1
Cloud error [share]	5412	0.1	0.19	0	1
Deforestation [sq. km]	5412	28.3	68.82	0	1307.89
GDP per capita [R]	5412	9317.82	10439.9	1313.55	158973
Soy price [R/kg]	5412	0.13	0.28	0	1.99
Timber price [R/cu. m]	5412	91.14	90.07	0	959.98
Indigenous territory area cover [share]	5412	0.08	0.17	0	1
Multiple use protected area cover [share]	5412	0.11	0.23	0	1
Strictly protected area cover [share]	5412	0.03	0.1	0	0.72
Settlement area cover [share]	5412	0.14	0.2	0	1
Federal party affiliation	5412	0.11	0.3	0	1
TIME INVARIANT VARIABLES					
Initial total deforested area [km^2]	492	1087.84	1206.9	0	10253.2
District area [sq. km]	492	8667.78	15648.1	103.25	159523
Farm area [sq. km]	492	1641.25	2038.26	7.56	14576
Population density [No./sq. km]	492	21.3	97.32	0.09	1321.93
Farms density [No./sq. km]	492	0.59	0.93	0	12.3
Share of small farms	492	0.71	0.19	0.02	0.99
No. of tractors per farm	492	0.15	0.49	0	7.85
Cattle rate [No./ha]	492	1.5	2.13	0	31.94
Share of land owners [%]	492	73.34	23.17	4.49	100
Land value [R/ha]	492	1220.66	1007.49	80	7502.08

*N*ote: Monetary figures are Million Brazilian Reais (R) deflated to 2012 prices, 1 *real* corresponded to $0.56 on average in 2012 (`www.oanda.com`).

Table B.3: Covariate balance before and after treatment

Covariate	Status	Mean blacklist	Mean non-blacklist	Difference in means	Normalized difference
Total deforested area in 2007	Unmatched	3937.86	966.21	2971.65	1.21
	Matched	3937.86	2351.69	1586.17	0.65
Deforestation in 2005	Unmatched	141.02	14.68	126.35	1
	Matched	141.02	57.41	83.62	0.66
Deforestation in 2006	Unmatched	139.28	12.69	126.59	0.91
	Matched	139.28	60.36	78.92	0.56
Deforestation in 2007	Unmatched	125.55	12.28	113.27	0.75
	Matched	125.55	62.11	63.44	0.42
Deforestation increases	Unmatched	2.38	1.7	0.68	1.07
	Matched	2.38	2.44	-0.06	-0.09
District area	Unmatched	18106.38	7600.14	10506.2	0.42
	Matched	18106.38	12569.2	5537.14	0.22
Forest cover in 2007	Unmatched	0.58	0.46	0.12	0.63
	Matched	0.58	0.54	0.04	0.2
Population density in 2007	Unmatched	3.13	23.36	-20.23	-6.35
	Matched	3.13	3.03	0.1	0.03
Farms per km^2 in 2006	Unmatched	0.16	0.64	-0.49	-3.7
	Matched	0.16	0.2	-0.04	-0.32
Share of small farms in 2006	Unmatched	0.64	0.72	-0.08	-0.48
	Matched	0.64	0.66	-0.02	-0.13
Farm area cover in 2006	Unmatched	0.39	0.41	-0.02	-0.12
	Matched	0.39	0.4	-0.02	-0.09
No. of tractors per farm in 2006	Unmatched	0.31	0.14	0.17	0.37

Table B.4: Deforestation and blacklisted districts, full sample FD regressions

Dependent	Δ ln Deforestation		
	(1)	(2)	(3)
Δ Blacklisted$_{it}$	-0.803***	-0..992	-0.998***
	(0.192)	(0.205)	(0.204)
Δ Cloud error$_{it}$	-0.978***	-0.971***	-0.984***
	(-0.171	-0.171	-0.171
ln Initial total deforested area$_i$		0.000**	0.000**
		(0.000)	(0.000)
ln District area$_i$		0.000***	0.000***
		(0.000)	(0.000)
ln Farm area$_i$		0.000	0.000
		(0.000)	(0.000)
ln Population density$_i$		0.000	0.000
		(0.000)	(0.000)
ln Farms per km$^2{}_i$		0.022	0.022
		(0.017)	(0.017)
ln Share of small farms$_i$		-0.180*	-0.180*
		(0.092)	(0.094)
ln No. of tractors per farm$_i$		-0.085	-0.086
		(0.055)	(0.056)
ln Cattle rate$_i$		-0.026***	-0.026***
		(0.009)	(0.009)
ln Share of land owners$_i$		0.000	0.000
		(0.001)	(0.001)
ln Land value$_i$		0.000	0.000
		(0.000)	(0.000)
Δ ln GDP per capita$_{it-1}$			-0.024
			(0.133)
Δ ln Soy price$_{it-1}$			0.041
			(0.116)
Δ ln Timber price$_{it-1}$			-0.059**
			(0.026)
Δ Indigenous territory area cover$_{it}$			0.344
			(0.397)
Δ Multiple use protected area cover$_{it}$			-0.045
			(0.392)
Δ Strictly protected area cover$_{it}$			-0.061
			(0.28)
Δ Settlement cover$_{it}$			0.731**
			(0.342)
Δ Federal party affiliation$_{it}$			0.007
			(0.075)
Constant	-0.056*	0.093	0.092
	(0.034)	(0.099)	(0.104)
Year and state effects	*Yes*	*Yes*	*Yes*
Time-invariant controls		*Yes*	*Yes*
Time-variant controls			*Yes*
Observations	4920	4920	4920
Cluster	492	492	492
Adj. R-squared	0.064	0.065	0.064

Note: The table reports first difference estimates with the dependent variable being the change in the log of yearly newly deforested area. Standard errors, clustered at district level, are reported in parentheses. Observations are selected by a 1:1 closest neighbor matching using inverse-variance weights, with replacement. *,**,*** denote significance at the 10/5/1% level

Table B.5: The effect of blacklisting after matching

Dependent	Δ ln Deforestation		
	(1)	(2)	(3)
Δ Blacklisted$_{it}$	-0.249	-0.276*	-0.297*
	(0.150)	(0.153)	(0.155)
Δ Cloud error$_{it}$	-0.524***	-0.526***	-0.586***
	(0.187)	(0.192)	(0.173)
ln Initial total deforested area$_i$		0.000	0.000
		(0.000)	(0.000)
ln District area$_i$		0.000***	0.000**
		(0.000)	(0.000)
ln Farm area$_i$		-0.000	-0.000*
		(0.000)	(0.000)
ln Population density $_i$		0.002	0.003
		(0.003)	(0.003)
ln Farms per km$^2{}_i$		-0.098	-0.141*
		(0.071)	(0.073)
ln Share of small farms$_i$		0.019	0.001
		(0.058)	(0.066)
ln No. of tractors per farm$_i$		0.012	0.006
		(0.026)	(0.028)
ln Cattle rate$_i$		-0.011	-0.023
		(0.023)	(0.021)
ln Share of land owners$_i$		0.000	0.000
		(0.001)	(0.001)
ln Land value$_i$		-0.000***	-0.000***
		(0.000)	(0.000)
Δ ln GDP per capita$_{it-1}$			-0.011
			(0.129)
Δ ln Soy price$_{it-1}$			-0.078
			(0.188)
Δ ln Timber price$_{it-1}$			-0.081**
			(0.039)
Δ Indigenous territory area cover$_{it}$			2.207***
			(0.437)
Δ Multiple use protected area cover$_{it}$			0.130
			(0.586)
Δ Strictly protected area cover$_{it}$			-0.721
			(0.732)
Δ Settlement cover$_{it}$			1.112
			(0.884)
Δ Federal party affiliation$_{it}$			0.149
			(0.164)
Constant	0.129**	0.228**	0.324***
	(0.061)	(0.101)	(0.103)
Year and state effects	*Yes*	*Yes*	*Yes*
Time-invariant controls		*Yes*	*Yes*
Time-variant controls			*Yes*
Observations	1000	1000	1000
Cluster	76	76	76
Adj. R-squared	0.251	0.245	0.258

*N*ote: The table reports first difference estimates with the dependent variable being the change in the log of yearly newly deforested area. Standard errors, clustered at district level, are reported in parentheses. Observations are selected by a 1:1 closest neighbor matching using inverse-variance weights, with replacement. *,**,*** denote significance at the 10/5/1% level.

Table B.6: The effect of blacklisting after different matching techniques

Dependent	Δ ln Deforestation			
	1:1 MD	1:1 PS	1:2 IV	1:1 IV restricted
	(1)	(2)	(3)	(4)
Δ Blacklisted$_{it}$	-0.346**	-0.444***	-0.323**	-0.437***
	(0.148)	(0.147)	(0.142)	(0.161)
Year and state effects	*Yes*	*Yes*	*Yes*	*Yes*
Time-invariant controls	*Yes*	*Yes*	*Yes*	*Yes*
Time-variant controls	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1000	1200	2000	1000
Cluster	88	70	95	71
Adj. R-squared	0.251	0.251	0.245	0.258

Note: The table reports first difference estimates with the dependent variable being the change in the log of yearly newly deforested area. Standard errors, clustered at district level, are reported in parentheses. Observations of column (1) are selected by a 1:1 matching on the Mahalonobis distance. Observations of column (2) are selected by a 1:1 matching on the propensity scores. Observations of column (3) are selected by a 1:2 matching using inverse-variance weights. Observations of column (4) are selected by a 1:1 matching using inverse-variance weights based on a reduced sample of covariates (official criteria, see section 3.1). Time invariant and variant controls include first differences of the variables reported in Table B.2. **,*** denote significance at the 5/1% level.

Table B.7: Placebo regressions on the timing of blacklisting

Dependent	Δ ln Deforestation			
	t-3	t-2	t-1	t-0
	(1)	(2)	(3)	(4)
Δ Blacklisted$_{it-k}$	0.178	-0.035	-0.083	-0.297*
	(0.116)	(0.145)	(0.153)	(0.155)
Year and state effects	*Yes*	*Yes*	*Yes*	*Yes*
Time-invariant controls	*Yes*	*Yes*	*Yes*	*Yes*
Time-variant controls	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1000	1000	1000	1000
Cluster	76	76	76	76
Adj. R-squared	0.258	0.255	0.256	0.258

Note: The table reports first difference estimates with the dependent variable being the change in the log of yearly newly deforested area. Standard errors, clustered at district level, are reported in parentheses. Observations are selected by a 1:1 closest neighbor matching using inverse-variance weights, with replacement. * denotes significance at the 10% level.

Table B.8: The influence of covariates on mechanisms

Dependent	Δ ln No of env. fines	Δ Car area coverage	Δ ln Rural credit
Dependent			
	(1)	(2)	(3)
Δ Cloud error$_{it}$	-0.730	0.006	0.425**
	(0.623)	(0.011)	(0.162)
Δ ln Initial total deforested area$_{i}$	-0.000	0.000**	0.000
	(0.000)	(0.000)	(0.000)
ln District area$_{i}$	-0.000	-0.000***	-0.000**
	(0.000)	(0.000)	(0.000)
ln Farm area$_{i}$	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
ln Population density$_{i}$	0.019	-0.000	0.026
	(0.020)	(0.002)	(0.021)
ln Farms per km²$_{i}$	-0.503*	-0.044*	-0.324
	(0.285)	(0.023)	(0.269)
ln Share of small farms$_{i}$	-0.335	0.110***	-0.321
	(0.359)	(0.016)	(0.220)
ln No. of tractors per farm$_{i}$	-0.387	0.033**	-0.408**
	(0.351)	(0.014)	(0.189)
ln Cattle rate$_{i}$	-0.030	0.005	0.037
	(0.082)	(0.004)	(0.035)
ln Share of land owners$_{i}$	0.001	0.000***	-0.004***
	(0.002)	(0.000)	(0.001)
ln Land value$_{i}$	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Δ ln GDP per capita$_{it-1}$	-0.271	-0.036***	-0.250*
	(0.340)	(0.011)	(0.140)
Δ ln Soy price$_{it-1}$	-0.012	-0.006	0.260*
	(0.594)	(0.010)	(0.145)
Δ ln Timber price$_{it-1}$	-0.079	-0.006*	0.008
	(0.131)	(0.003)	(0.018)
Δ Indigenous territory area cover$_{it}$	1.822*	-0.084***	-2.316***
	(0.908)	(0.025)	(0.405)
Δ Multiple use protected area cover$_{it}$	-0.158	0.033	0.285
	(1.708)	(0.044)	(0.395)
Δ Strictly protected area cover$_{it}$	-2.824	0.136	3.527***
	(3.284)	(0.117)	(0.862)
Δ Settlement cover$_{it}$	0.630	-0.020	-0.778***
	(0.563)	(0.029)	(0.197)
Δ Federal party affiliation$_{it}$	0.316	0.033*	-0.140
	(0.329)	(0.017)	(0.224)
Constant	1.282***	-0.122***	1.067***
	(0.449)	(0.023)	(0.292)
Year and state effects	*Yes*	*Yes*	*Yes*
Time invariant controls	*Yes*	*Yes*	*Yes*
Observations	*500*	*500*	*500*
Clusters	76	76	76
Adj. R-squared	0.102	0.523	0.145

*N*ote: The table reports first difference. Car area coverage is measured between 0 and 1. Standard errors, clustered at district level, are reported in parentheses. Observations are selected by a 1:1 closest neighbor matching using inverse-variance weights, with replacement. *,**,*** denote significance at the 10/5/1% level.

Table B.9: Test for pre-treatment parallel time trends

Dependent	Δ ln Deforestation		
	(1)	(2)	(3)
Blacklisted$_i$	127.197		
	(78.503)		
Year	-0.012	-0.065***	-0.016
	(0.033)	(0.025)	(0.026)
Blacklisted$_i$ × Year	-0.063		
	(0.039)		
State effects	*Yes*	*Yes*	*Yes*
Time-invariant controls		*Yes*	*Yes*
Time-variant controls			*Yes*
Observations	500	250	250
Cluster	76	50	26
Adj. R-squared	0.156	0.154	0.149

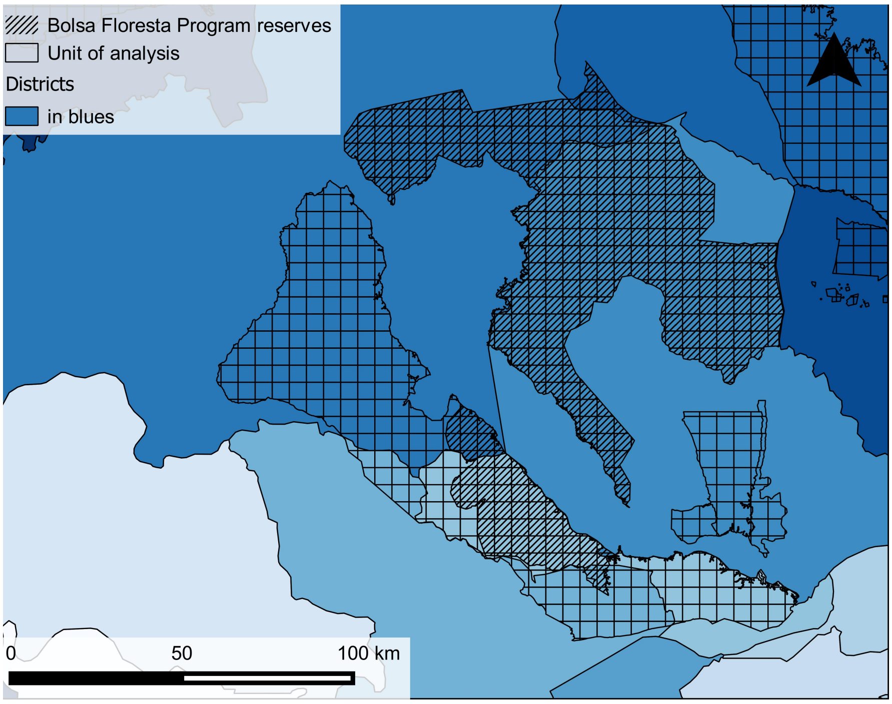
*N*ote: The table reports first difference estimates with the dependent variable being the change in the log of yearly newly deforested area. Standard errors, clustered at district level, are reported in parentheses. Time invariant and variant controls include first differences of the variables reported in Table B.2. Observations are selected by a 1:1 closest neighbor matching using inverse-variance weights, with replacement. *** denotes significance at the 1% level.

C

APPENDIX TO CHAPTER 4

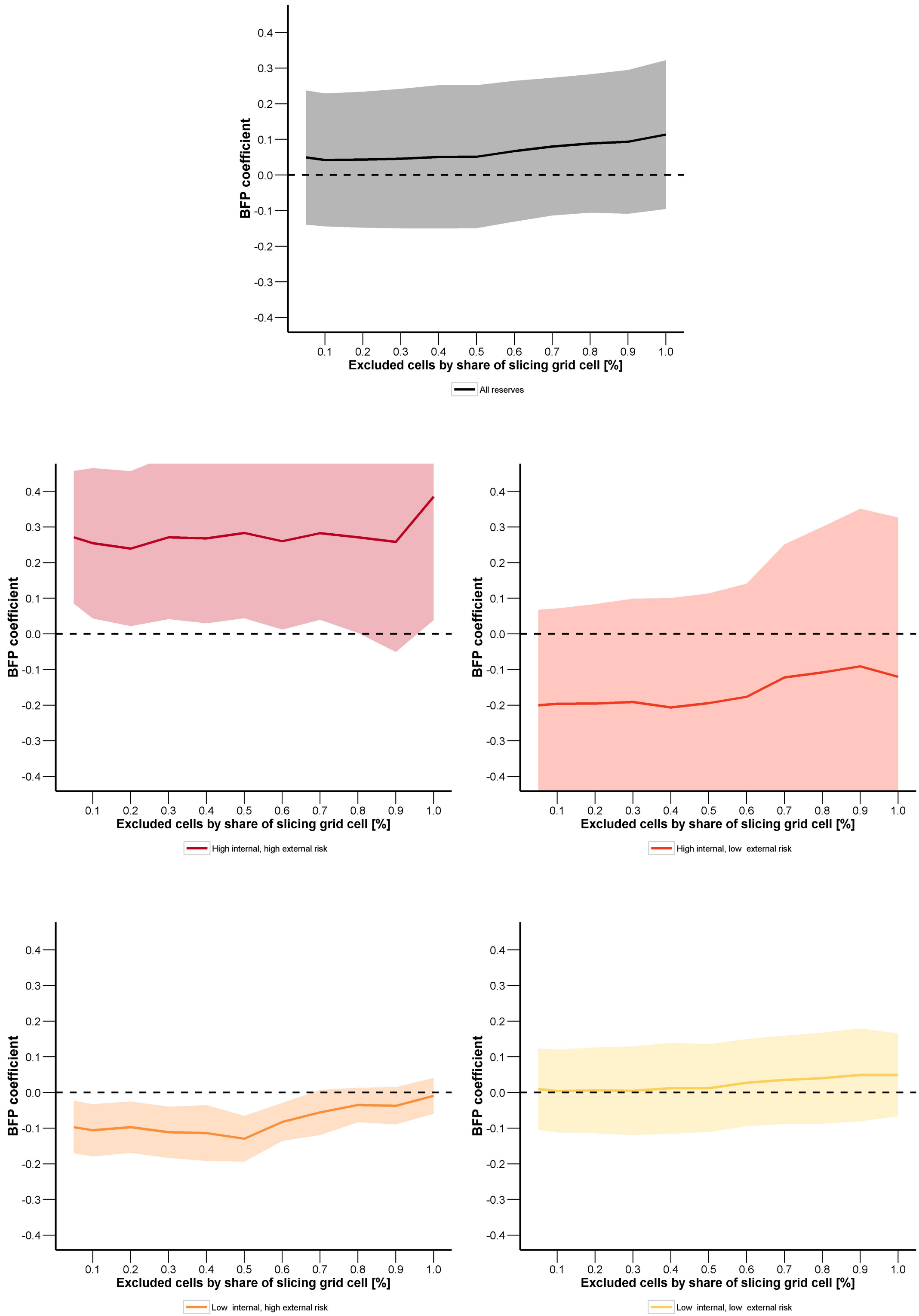
C.1 FIGURES

Figure C.1: Data levels



Note: The Figure depicts different data levels. The unit of analysis - the irregular cells - comprise data specific to its own environment. Forest conservation at a cell level depends on its own context and on the surrounding context in which it belongs to, namely the reserve and district. Cell, reserve and district characteristics serve to describe the internal and external pressures each cell experiences.

Figure C.2: Bolsa Floresta impacts by cell size exclusions and risk type



Note: Lines represent the impact the FE estimates of the BFP intervention after matching. The Abscissa indicates the threshold of cells excluded from the sample. E.g., the value 0.2 indicates a sample containing only cells larger or equal to 20% of the original 5 to 5 km^2 grid cell used for slicing the reserves. Sample sizes using all reserves (without differentiation on risk type) vary between 6948 (0.05) to 4500 (1.0) units. Shaded areas correspond to a confidence interval at 10% significance level.

C.2 TABLES

Table C.1: Matching covariates of pre-treatment deforestation pressures (1)

Covariates	Class	Data level	Driving source of deforestation pressure
ln Size of reserve	Conservation policy	Reserve	Internal
Years since foundation of reserve	Conservation policy	Reserve	Internal
Founded in 2009	Conservation policy	Reserve	Internal
Founded in 2008	Conservation policy	Reserve	Internal
Founded in 2006 or 2007	Conservation policy	Reserve	Internal
Founded in 2005 and before	Conservation policy	Reserve	Internal
Percentage area from original grid Cell	Natural, conservation policy	Cell	Internal, external
Perc. deforestation in 2003	Natural, economic	Cell	Internal, external
Perc. deforestation in 2004	Natural, economic	Cell	Internal, external
Perc. deforestation in 2005	Natural, economic	Cell	Internal, external
Perc. deforestation in 2006	Natural, economic	Cell	Internal, external
Perc. deforested area in 2006	Natural, economic	Cell	Internal, external
Inital forest coverage in 2006	Natural, economic	Cell	Internal, external
Non-forest coverage	Natural	Cell	Internal, external
Hydrography coverage	Natural	Cell	Internal, external
ln Distance to next roads	Natural, economic	Cell	Internal, external
ln Distance to next city	Natural, economic	Cell	Internal, external
ln Distance to next river	Natural, economic	Cell	Internal, external
Agricultural land use coverage in 2008	Economic	Cell	Internal
Mixed land use coverage in 2008	Economic	Cell	Internal
Secondary vegetation coverage in 2008	Economic	Cell	Internal
Pasture coverage in 2008	Economic	Cell	Internal
Urban area coverage in 2008	Economic	Cell	Internal

*N*ote: Together with table C.2 below, this table reports the covariates used for the matching procedure described in section 4.5.2. The covariates are used to find similar control units for the treated grid cells of the Bolsa Floresta Program.

Table C.2: Matching covariates of pre-treatment deforestation pressures (2)

Covariates	Class	Data level	Driving source of deforestation pressure
ln Total no. of fires (2003-06)	Economic	Cell	Internal, external
Land speculation coverage	Economic	Cell	External
ln Distance to multiple-use reserve boundary	Conservation policy	Cell	External
ln Distance to next indigenous reserve	Conservation policy	Cell	External
ln Distance to next strictly protected reserve	Conservation policy	Cell	External
Settlement project coverage	Conservation policy	Cell	Internal
District population density in 2007	Economic	District	External
ln District GDP per capita in 2006	Economic	District	External
ln District Agric. GDP per capita in 2006	Economic	District	External
District farm coverage	Economic	District	External
District share of small farms	Economic	District	External
ln District tractors per farm	Economic	District	External
District av. timber price (2003-06)	Economic	District	External
Neigh. perc. deforestation in 2003	Spatial setting	Cell	Internal
Neigh. perc. deforestation in 2004	Spatial setting	Cell	Internal
Neigh. perc. deforestation in 2005	Spatial setting	Cell	Internal
Neigh. perc. deforestation in 2006	Spatial setting	Cell	Internal
Neigh. forest coverage in 2006	Spatial setting	Cell	Internal
Neigh. hydrography coverage	Spatial setting	Cell	Internal
Neigh. settlement project coverage in 2006	Spatial setting	Cell	Internal
Neigh. mixed land use coverage in 2008	Spatial setting	Cell	Internal
Neigh. agric. land use coverage in 2008	Spatial setting	Cell	Internal
Neigh. secondary vegetation coverage in 2008	Spatial setting	Cell	Internal
Neigh. pasture coverage in 2008	Spatial setting	Cell	Internal
Neigh. urban area coverage in 2008	Spatial setting	Cell	Internal

*N*ote: Together with table C.1 above, this table reports the covariates used for the matching procedure described in section 4.5.2. The covariates are used to find similar control units for the treated grid Cells of the Bolsa Floresta Program.

Table C.3: Summary statistics of matching covariates

		Mean	St.dev	Median	Min	Max
ln Size of reserve		22.73	1.10	22.77	15.08	24.60
Years since foundation of reserve		4.81	7.11	1.00	-3.00	44.00
Founded in 2006 or 2007		0.30	0.46	0.00	0.00	1.00
Founded in 2005 and before		0.60	0.49	1.00	0.00	1.00
Founded in 2008		0.06	0.24	0.00	0.00	1.00
Founded in 2009		0.04	0.19	0.00	0.00	1.00
Percentage area from original grid cell	[%]	83.61	28.33	100.00	5.00	100.00
Perc. deforestation in 2003	[%]	0.17	0.90	0.00	0.00	39.33
Perc. deforestation in 2004	[%]	0.17	0.91	0.00	0.00	27.73
Perc. deforestation in 2005	[%]	0.09	0.54	0.00	0.00	14.98
Perc. deforestation in 2006	[%]	0.10	0.65	0.00	0.00	19.22
Perc. deforested area in 2006	[%]	3.55	11.79	0.00	0.00	101.01
Initial forest coverage in 2006	[%]	89.76	22.60	100.00	0.00	100.00
Non-forest coverage	[%]	4.03	15.66	0.00	0.00	100.00
Hydrography coverage	[%]	2.54	10.00	0.00	0.00	99.92
ln Distance to next roads		10.41	1.25	10.71	1.65	12.47
ln Distance to next city		11.18	0.71	11.31	5.13	12.62
ln Distance to next river		9.54	1.51	9.95	-0.62	12.21
Agricultural land use coverage in 2008	[%]	0.00	0.05	0.00	0.00	5.73
Mixed land use coverage in 2008	[%]	0.26	2.02	0.00	0.00	67.34
Secondary vegetation coverage in 2008	[%]	1.45	4.89	0.00	0.00	72.64
Pasture coverage in 2008	[%]	1.58	7.16	0.00	0.00	99.96
Urban area coverage in 2008	[%]	0.05	1.28	0.00	0.00	70.63
ln Total no. of fires (2003-06)		0.43	0.91	0.00	0.00	5.02
Land speculation coverage	[%]	18.91	35.27	0.00	0.00	100.00
ln Distance to multiple-use reserve boundary		8.94	1.76	9.04	0.09	12.48
ln Distance to next indigenous reserve		10.94	1.16	11.24	0.42	12.61
ln Distance to next strictly protected reserve		10.78	1.35	11.08	0.01	12.77
Settlement project coverage	[%]	22.00	41.45	0.00	0.00	200.00
District population density in 2007	[per ha]	4.93	30.35	0.95	0.09	1156.36
ln District GDP per capita in 2006	[Reais]	8.76	0.59	8.61	7.49	10.73
ln District Agric. GDP per capita in 2006	[Reais]	-1.36	1.43	-0.98	-6.97	3.28
District farm coverage	[%]	9.77	16.07	5.07	0.06	170.06
District share of small farms		0.63	0.21	0.64	0.10	0.99
ln District tractors per farm		0.05	0.10	0.01	0.00	1.09
District av. timber price (2003-06)	[Reais]	291.11	247.69	219.62	0.00	1554.57
Neigh. perc. deforestation in 2003	[%]	0.23	0.72	0.00	0.00	12.15
Neigh. perc. deforestation in 2004	[%]	0.23	0.75	0.00	0.00	12.29
Neigh. perc. deforestation in 2005	[%]	0.12	0.41	0.00	0.00	8.20
Neigh. perc. deforestation in 2006	[%]	0.14	0.53	0.00	0.00	9.30
Neigh. forest coverage in 2006	[%]	88.26	21.54	98.41	0.00	100.00
Neigh. hydrography coverage	[%]	2.91	8.55	0.00	0.00	92.39
Neigh. settlement project coverage in 2006	[%]	17.57	34.61	0.00	0.00	200.00
Neigh. Mixed land use coverage in 2008	[%]	0.28	1.71	0.00	0.00	55.29
Neigh. agric land use coverage in 2008	[%]	0.01	0.22	0.00	0.00	8.74
Neigh. secondary vegetation coverage in 2008	[%]	1.50	3.65	0.12	0.00	45.61
Neigh. pasture coverage in 2008	[%]	2.17	7.25	0.00	0.00	72.24
Neigh. urban area coverage in 2008	[%]	0.07	1.34	0.00	0.00	68.79

*N*ote: Statistics refer to 3,474 treated BFP cells and 3,474 matched control cells.

Table C.4: Panel summary statistics of matched covariates

Years		2003	2004	2005	2006	2007	2008	2009	2010	2011	2012
Deforestation	Mean	0.54	0.25	0.08	0.06	0.13	0.13	0.19	0.36	0.10	0.07
[ha]	St.dev	3.62	2.11	0.76	0.83	1.60	1.38	1.75	2.51	0.98	0.81
	Min	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	Max	167.25	121.72	26.82	36.80	31.23	40.44	64.84	55.14	27.89	23.48
Cloud error	Mean	6.03	7.34	3.89	2.75	7.64	2.69	2.49	7.74	1.33	9.60
[%]	St.dev	15.13	16.87	11.57	9.26	18.63	10.31	10.84	16.42	7.60	18.12
	Min	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	Max	89.95	89.83	89.89	89.00	89.56	88.47	89.76	89.95	89.84	89.97
GDP p. capita	Mean	4475.98	6204.31	5942.53	5904.91	5913.75	6628.93	6490.78	6851.22	6704.91	8048.02
[Reais]	St.dev	3101.65	3469.25	3750.45	4238.84	4714.58	5234.29	5088.91	5624.79	4945.00	5853.84
	Min	2170.94	2656.09	2460.57	2434.31	2222.81	3247.32	3436.96	3888.64	3722.18	4718.39
	Max	18654.93	20268.91	20868.54	23606.85	23706.24	25921.85	27173.99	29384.24	28923.68	33176.04
GDP p. capita	Mean	0.51	0.87	0.75	0.62	0.42	0.46	0.43	0.35	0.35	0.52
in agric.	St.dev	0.52	0.76	0.69	0.62	0.80	0.79	0.75	0.47	0.54	0.72
[Reais]	Min	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01
	Max	3.05	3.70	3.34	3.03	4.98	4.71	4.56	2.75	3.34	3.55
Timber price	Mean	48.27	15.19	19.92	18.22	20.71	20.53	22.23	22.19	19.87	28.06
[Reais]	St.dev	47.80	16.29	17.90	16.37	18.34	18.19	19.42	19.02	18.24	11.97
	Min	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	Max	98.13	64.67	58.51	53.53	56.69	56.29	71.62	69.18	64.68	50.75

*N*ote: Statistics refer to 3474 treated BFP cells and 3474 matched control cells.

Table C.5: Bolsa Florsta program start by reserves

Reserve	Year of BFP start (Aug-Jul)	Yearly average deforestation after BFP start [ha]	Total deforestation after BFP start [ha]
APA Rio Negro	2010	81.75	245.26
RDS do Rio Negro	2009	132.04	528.17
FE de Maués	2008	65.63	328.15
RDS Canumã	2009	7.86	31.44
RDS Uacari	2008	8.70	43.50
RDS Rio Amapá	2009	0.00	0.00
RDS do Uatumã	2008	100.16	500.80
RDS Amanã	2009	77.56	310.23
RDS Rio Madeira	2008	54.08	270.38
RDS do Juma	2008	48.92	244.60
RDS Mamirauá	2008	16.64	83.22
RDS Cujubim	2008	22.87	114.35
RDS Piagaçu-Purus	2008	30.21	151.05
RESEX do Rio Gregório	2009	30.10	120.41
RESEX Catuá-Ipixuna	2008	13.29	66.45
Total			3038.02

*N*ote: Years are based on the August to July time-frame according to INPEs deforestation monitoring system. Information is based on FAS data. APA (Environmental Protection Area), RESEX (Extractive Reserve), RDS (Sustainable Development Reserve), and FE (State Forest) are subtypes of the multiple-use reserve category.

Table C.6: Bolsa Floresta effects by reserve

Risk group	Dependent / Reserve	ln Deforestation FE (1)	ln Deforestation w.FE P.def. (2)	ln Deforestation w.FE c.s. (3)	ln Deforestation RE Tobit (4)	ln Forest degradation FE (5)	ln Forest degradation w.FE P.def. (6)	ln Forest degradation w.FE c.s. (7)	ln Forest degradation RE Tobit (8)	ln No. of fires FE (9)	ln No. of fires w.FE P.def. (10)	ln No. of fires w.FE c.s. (11)	ln No. of fires RE Tobit (12)
HiR & HeR	RDS Canumã												
	RDS do Rio Negro	+	+			+	+						
	RDS do Juma	-	-	+	-	-	+	-		+	+	+	
	FE de Maués						+	+	+				
	RDS Rio Madeira	+		+	+					+		+	
HiR & LeR	RESEX Catuá-Ipixuna	-	-	-	-			-	-				
	RESEX do Rio Gregório												+
LiR & HeR	RDS do Uatumã												
	RDS Mamirauá					+	+						
LiR & LeR	APA do Rio Negro	+	+	+	+	-		-	-				
	RDS Piagaçu-Purus	-	-							+	+	+	+
	RDS Amanã	-		-	-	-		-	-	+	+	+	+
	RDS Uacari	-	-	-	-								
	RDS Cujubim	+	+				+	+		+		+	
	RDS Rio Amapá					+	+	+		+	+		

*N*ote: The Table reports the significant results of fixed effects estimates using different dependent variables. The first row reports estimates on the full matched sample of cells across all BFP reserves. Rows 2 to 5 report estimates on sub-groups. Internal and external risks refer to the ratio of deforestation between 2003 and 2006 over forest cover in 2002 within reserves and in 20 km buffer zones around reserves (excluding water areas and other reserve areas). High and low groups are defined as the upper and lower part of an ordered sample. The 7 reserves with the highest internal ratio of deforestation classify as 'high internal risk'. The 7 reserves with the highest external ration of deforestation classify as 'high external risk'. Positive (negative) signs indicate significant positive (negative) coefficients at a minimum 10% level. Missing signs indicate to insignificant estimates. APA (Environmental Protection Area), RESEX (Extractive Reserve), RDS (Sustainable Development Reserve), and FE (State Forest) are subtypes of the multiple-use reserve category.

C.3 FIXED EFFECTS TRANSFORMATION

The fixed effects estimator:

$$\widetilde{\ln \mathrm{Def}}_{irdt} \;=\; \widetilde{\mathbf{C}}'_{it}\,\beta + \gamma\,\widetilde{\mathrm{PES}}_{rt} + \widetilde{\mathbf{D}}'_{dt}\,\delta + \widetilde{\eta}_t + \widetilde{\varepsilon}_{irdt}$$

is constructed as the subtraction of the full data model:

$$\ln \mathrm{Def}_{irdt} \;=\; \mathbf{C}'_{it}\,\beta + \gamma\,\mathrm{PES}_{rt} + \mathbf{D}'_{dt}\,\delta + \mu_i + \kappa_r + \sigma_d + \eta_t + \varepsilon_{irdt}$$

from its time demeaned data:

$$\overline{\ln \mathrm{Def}}_{ird} \;=\; \overline{\mathbf{C}}'_{i}\,\beta + \gamma\,\overline{\mathrm{PES}}_{r} + \overline{\mathbf{D}}'_{d}\,\delta + \overline{\mu}_i + \overline{\kappa}_r + \overline{\sigma}_d + \overline{\eta} + \overline{\varepsilon}_{ird}$$

where:

$$\begin{aligned}
\overline{\ln \mathrm{Def}}_{ird} &= T^{-1}\sum_{t=1}^{T} \ln \mathrm{Def}_{irdt} \\
\overline{\mathbf{C}}'_{i} &= T^{-1}\sum_{t=1}^{T} \mathbf{C}'_{it} \\
\overline{\mathrm{PES}}'_{i} &= T^{-1}\sum_{t=1}^{T} \mathrm{PES}'_{it} \\
\overline{\mathbf{D}}'_{i} &= T^{-1}\sum_{t=1}^{T} \mathbf{D}'_{it} \\
\overline{\mu}_i &= T^{-1}\sum_{t=1}^{T} \mu_i = \mu_i \\
\overline{\kappa}_r &= T^{-1}\sum_{t=1}^{T} \kappa_r = \kappa_r \\
\overline{\sigma}_d &= T^{-1}\sum_{t=1}^{T} \sigma_d = \sigma_d
\end{aligned}$$

for all groups i.

C.4 SPATIAL DATA PROCESSING WITH POSTGIS

All spatial data measurements are constructed with PostgreSQL 9.2.3 and the PostGIS 2.0. Examples include layers of polygons, lines and points. Distances between individual cells and line objects such as rivers, roads, reserve boundaries, etc. are constructed as direct lines from the center point of a cell to the nearest line-fragment of the respective object (see Listing C.1). Distances between individual cells and point objects (e.g., district capitals) are constructed as direct lines from the center point of a cell to the nearest point (see Listing C.2). To calculate distances to polygons, the object has to be converted into lines first (see Listing C.3). Area calculations, like deforestation within cells are based on the intersection of the two layers. An efficient computation of areas with an intersection is exemplified in Listing C.4.

Listing C.1: Distance calculation from small polygon to line object

```sql
drop table if exists new_object ;
with
dist_to as
(
select
 gid, --identifier of line_object
 (
 st_dump( --dump line_object into its single elements
  st_simplify(geom,0.04) --simplify lines to reduce computation
  )
 ).geom as geom from line_object
)
select
 l.aid,
 min(
  st_length(
   geography(
    ST_ShortestLine(p.centroid,l.geom) --constructs shortest line (
        centroid point already exist)
   )
  )
 ) as distance
into
 new_object
from
 polygon_object p,
 dist_to l
group by
 p.aid --line identifier
;
```

Listing C.2: Distance calculation from small polygon to point object

```
drop table if exists new_object ;
create table new_object as
select
 p.aid, --polygon identifier
 min(
  st_length(
         geography( --converstion to meter with WGS84 projection
          ST_ShortestLine(p.centroid,c.geom) --beeline
         )
        )
 ) as distance
from
 polygon_object p,
 (select st_snapToGrid(geom,0.0005) as geom from cit001) c --simplify
    coordinates of point layer
where
 st_dwithin(c.geom,p.centroid,4.1) --with 4.1 degrees being the highest
    distance
group by
 p.aid ;
```

Listing C.3: Distance calculation from small polygon to polygon set

```
drop table if exists new_object ;
with
dist_to as
(
select
 gid,
 st_exteriorRing( --converting polygons to boundary lines ignoring holes/
     interior rings
  (st_dump(
          st_simplify(geom,0.04)
         )
        ).geom
 ) as geom from target_polygon
)
select
 g.aid,
 min(st_length(geography(ST_ShortestLine(g.centroid,r.geom)))) as distance
into
 new_object
from
 polygon g,
 dist_to r
group by  g.aid ;
```

Listing C.4: Instersection of deforestation layer with cells

```
drop table if exists new_intersect ;
create table new_intersect as
select
 g.aid, --cell identifier
 p.def, --land class dummy on deforestation
 p.floresta, --land class dummy for forest
 p.hidrografia, --land class dummy for hydrography
 p.nao_floresta, --land class dummy for not-forest area (savanna, swamps)
 p.residuo, --land class dummy for residual areas
 p.nuvem, --land class dummy for clouds
 p.nid, --PRODES identifier
 p.year3112,
 p.lag_p, --passe years since the polygon was observed (without cloud
     cover) as forest
 p.tstart_p, --constructed date where polygon was last detected as forest
 p.tend_p, --date of satellite image that classified polygon as deforested
 case --do not intersect if the PRODES polygon is fully contained within a
      cell
  when st_coveredby(p.geom,g.geom) then
   st_area(geography(p.geom)) --calculate area
  else
   st_area(geography(st_intersection(p.geom,g.geom))) --intersect and
      calculate area
 end as prodes2012_area
from
 prodes2012 p --polygon layer constructed from the downloadable PRODES
     layer database
inner join
 cells g
ON (
 p.geom && g.geom and not --intersect bounding boxes first to reduce
     computation time
 st_touches(p.geom,g.geom) --do not intersect polygons that would result
     in a line only.
 ) ;
```

Note: The PRODES vector data on all land classes (forest, deforested, hydrography, residual, clouds, not forest) can be downloaded by satellite pathrow (INPE, 2012).

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