

# Winning and Losing the Resource Lottery: Governance after Uncertain Oil Discoveries\*

Erik Katovich<sup>†</sup>

Current Draft: May 15th, 2022

[Click here for most recent version](#)

## Abstract

Resource discoveries are often followed by long delays and heterogeneous production outcomes. Post-discovery uncertainty creates challenges for governance: policymakers may alter present behavior in anticipation of future revenues or struggle to adapt to disappointed expectations. I exploit quasi-experimental subnational variation in oil discoveries and subsequent production realizations in Brazil to identify dynamic effects of news and revenue shocks on municipal public finances, public goods provision, and political competition, selection, and patronage. Relative to never-treated controls, places that experience discovery announcements but never receive anticipated windfalls suffer significant declines in per capita investment and public goods spending after ten years. Places where discoveries are realized enjoy significant growth in per capita revenues and spending, but do not invest in economic diversification and fail to improve public goods provision. Findings reveal inefficient windfall spending and adjustment costs after disappointment as two faces of the resource curse, and highlight the importance of accounting for heterogeneity in production outcomes after discovery announcements.

**JEL Codes:** D72, H72, H75, Q32, Q33, Q38

**Keywords:** Resource Curse, Oil Discoveries, Public Finance, Elections, Brazil

---

\*This paper has benefited from helpful discussions with Ian Coxhead, José Gutman, Sarah Johnston, Alexey Kalinin, Matthew Klein, Joana Monteiro, Dominic Parker, Steven Poelhekke, Fernando Postali, Sergio Prado, Helder Queiroz, Frederico Rocha, Natalia Serna, Rodrigo Serra, Justin Winikoff, and seminar participants at AAEA, AERE, LACEA, NEUDC, SEA, the Sovereign Wealth Fund of Niterói, Universidad de San Andrés-Economics, Universidade de São Paulo-FEA, Universidade Federal Fluminense-PPGE, University of Geneva-IEE, and UW-Madison's Development Seminar and Development and Political Economy Lab. I thank Fabio Freitas and the Instituto de Economia at the Universidade Federal do Rio de Janeiro for access to the RAIS dataset. This project received generous financial support from IRIS and Tinker Nave field research grants. UW-Madison's Institutional Review Board approved the use of human subjects data in this project (Project ID 2019-1122).

<sup>†</sup>University of Wisconsin-Madison Email: [ekatovich@wisc.edu](mailto:ekatovich@wisc.edu)

# 1 Introduction

Since 2010, sixteen developing countries representing over half the world’s population have experienced giant offshore oil or natural gas discoveries (Zhang et al., 2019).<sup>1</sup> Discoveries constitute large shocks to expected long-term wealth, and can cause policymakers to make irreversible investments or borrow against future resource revenues. Discoveries increase expected returns to holding political office, and may encourage both political competition and rent-seeking. Nevertheless, discoveries are notoriously noisy signals, and are likely to become increasingly uncertain in coming decades.<sup>2</sup>

Heterogeneity in discovery realizations causes some affected countries or regions to receive vast revenue windfalls, while others receive nothing. In places with successful discoveries, natural resource extraction and revenues create opportunities for economic development (Toews and Vézina, 2020; Venables, 2016), but also bring challenges associated with the “Resource Curse.”<sup>3</sup> Independent of extraction or revenues, anticipation after discovery announcements can provoke increases in rent-seeking and corruption (Armand et al., 2020; Vicente, 2010). Places where discoveries fail to produce must grapple with disappointed expectations leading to revenue shortfalls and public finance dysfunction (Mihalyi and Scurfield, 2020).

I measure how local governments, politicians, and voters respond to major offshore oil and natural gas discoveries in Brazil during the 2000s and 2010s. Specifically, I assess whether discovery announcements cause anticipatory changes in municipal public finances, public goods provision, and political competition, selection, and patronage before resource revenues begin to flow. Further, I evaluate the frequency with which municipalities’ discovery expectations are fulfilled or disappointed, and how governance outcomes evolve in places where discoveries lead to revenue windfalls versus places where discoveries fail to produce.

Methodologically, I exploit quasi-experimental variation in news and revenue shocks created by the interaction of exogenous offshore discoveries and subsequent production realizations with Brazil’s

---

<sup>1</sup>These countries include Angola, Brazil, China, Egypt, French Guiana, Ghana, Guyana, India, Indonesia, Malaysia, Mozambique, Myanmar, Nigeria, Philippines, Senegal, and Tanzania (Zhang et al., 2019). In total, there have been 236 giant oil or gas discoveries (of more than 500 million barrels of oil equivalent) across 46 countries since 1988 (Cust and Mihalyi, 2017).

<sup>2</sup>Since 1950, oil discoveries have taken an average of seven years to begin production, with a standard deviation of nine years (Mihalyi, 2020). A fall in global prices can make a promising field commercially unviable; reserves can turn out to be smaller, lower quality, or more difficult to extract than initially estimated. As exploration moves into deeper waters and more remote locations, production delays are likely to grow, increasing the scope for anticipation and uncertainty (Geiger, 2019). Furthermore, pressures to leave fossil fuels in the ground to combat climate change may cause discoveries to remain undeveloped in the future (McGlade and Ekins, 2015; Welsby et al., 2021).

<sup>3</sup>Development challenges associated with natural resources include Dutch Disease and deindustrialization (Corden and Neary, 1982; Pelzl and Poelhekke, 2021), corruption and rent-seeking (Baragwanath, 2020; Brollo et al., 2013), conflict (Berman et al., 2017; Nillesen and Bulte, 2014), erosion of human capital investment (Agüero et al., 2021; Gylfason, 2001), and exposure to volatility (van der Ploeg and Poelhekke, 2009).

formulaic oil and gas revenue sharing rules.<sup>4</sup> Based on geographical alignment between coastal municipalities and offshore fields, these rules allow municipal governments to predict whether they will be future beneficiaries of announced discoveries—thus introducing subnational variation amongst comparable local governments. To link discoveries to coastal municipalities, I construct an original geolocated dataset of 179 major offshore discovery announcements made by oil companies to the *Comissão de Valores Mobiliários* (CVM), Brazil’s Securities and Exchange Commission, between 2000 and 2017. I reconstruct Brazil’s geodesic offshore projection maps to tie discoveries to affected municipalities.

I identify dynamic causal effects of discovery announcements and subsequent production outcomes by comparing municipalities affected by discovery announcements with never-treated municipalities where exploratory offshore wells were drilled after 1999 but no discoveries occurred, under the assumption that, conditional on drilling, the success of a well is as-if-random ([Cavalcanti et al., 2016](#); [Cust et al., 2019](#)). To quantify heterogeneity in discovery realizations, I develop a forecasting model based on standard offshore production assumptions, announced reserve volumes, average production delays, and Brazil’s royalty distribution rules. Comparing forecast and realized revenues in each discovery-treated municipality reveals that some places eventually enjoy revenue windfalls in line with expectations, while others are disappointed. I leverage this exogenous variation in forecast error to categorize municipalities into two treatment arms (“Satisfied” and “Disappointed”). Leveraging a rich panel dataset, I estimate event study specifications around the first major discovery announcement separately for each of these groups relative to never-treated controls. I implement [Callaway and Sant’Anna \(2020\)](#)’s group-time treatment effect estimator to address biases introduced by staggered treatment timing and heterogeneous treatment effects ([de Chaisemartin and D’Haultfœuille, 2020](#); [Goodman-Bacon, 2018](#)).

Forty-eight Brazilian municipalities were affected by offshore discovery announcements between 2000-2017. Of these, only 18 ultimately receive 50% or more of the revenues they could have expected, suggesting disappointment was widespread, though not universal. Municipalities did not exhibit rapid anticipatory fiscal responses to discovery announcements, possibly due to constraints imposed by a fiscal responsibility law that limits municipalities’ ability to engage in deficit spending. In both disappointed and satisfied municipalities, levels of public spending, hiring, and debt remain mostly indistinguishable from controls for up to five years after the first discovery announcement. This result contrasts with findings in [Mihalyi and Scurfield \(2020\)](#), who report rapid worsening of fiscal measures such as debt

---

<sup>4</sup>Offshore discoveries are exogenous to municipalities, as they are made by multinational corporations operating hundreds of kilometers offshore, servicing installations from distant ports, and responding to international prices and technologies. Subsequent production outcomes are also driven by extra-municipal factors: a fall in global prices can make a promising field commercially unviable; reserves can turn out smaller or more difficult to extract than initially estimated. Companies may leave discoveries undeveloped due to policy changes, financial difficulties, or shifts in strategy.

sustainability in 9 out of 12 African countries recently affected by major oil discoveries. This contrast highlights the important role of institutions such as Brazil's fiscal responsibility law in tempering fiscal excesses after discoveries. Alternatively, it may illustrate emergent properties of discovery dynamics at the subnational level, where policy options (e.g., issuing debt) are fundamentally different than those available to national governments.

As production ramps up between five and ten years after the discovery announcement, municipalities' "type" is realized (i.e., disappointed or satisfied) and outcomes for the two groups diverge sharply. In satisfied municipalities, per capita revenues increase by 75% ten years after the first major discovery announcement (from a baseline control mean of R\$1,084 to R\$1,898 ten years on) relative to counterfactual municipalities that had exploratory wells but no major discoveries during this period. Municipal per capita tax revenues in this group decline by 30% (though these estimates are not statistically significant) and per capita oil revenues increase by a striking 5,441% (from a baseline control mean of R\$129 to R\$7,121 ten years on, or from 2% to 109% of baseline average annual income), highlighting the radical effects discoveries can exert on public finances. Per capita spending in satisfied municipalities increases by 20% (from a baseline control mean of R\$874 to R\$1,055 ten years on), and per capita spending on education and health increase by 28% and 26%, respectively.

Despite these dramatic changes in revenues and spending in satisfied municipalities, measures of real public goods provision, quality, and outcomes decline significantly relative to controls in the decade following a major announcement. This finding corroborates the result in [Caselli and Michaels \(2013\)](#) that oil revenues increase public goods spending, but not real public goods provision, in Brazilian municipalities. This may be the result of limited municipal capacity to spend oil windfalls effectively, leakage of oil rents into corruption, or lags in improving hard-to-change education and health outcomes. Furthermore, satisfied municipalities do not increase public investment or spending on promotion of non-extractive sectors (i.e., agriculture, industry, and services).

In disappointed municipalities (i.e., those that experience discovery announcements but never receive expected windfalls), per capita oil revenues remain unchanged ten years after the first major discovery announcement, yet total per capita revenues decline by 27% relative to controls (from a baseline control mean of R\$1,084 to R\$795 ten years on), largely as a result of falling tax revenues (-37%) and other transfers from federal and state governments (-9%). Consequently, per capita spending declines 24% (from a baseline control mean of R\$874 to R\$664 ten years on), per capita investment by 57% (from a baseline control mean of R\$101 to R\$44 ten years on), and per capita education and health spending by 26% (from a baseline control mean of R\$536 to R\$397 ten years on). Indicators of public goods provision, quality, and outcomes decline significantly relative to never-treated controls.

To explore mechanisms underlying disappointed places' negative outcomes and satisfied places inability to translate windfalls into improved public goods provision, I measure the effects of discovery announcements on political competition, selection, and patronage in municipal elections between 2000-2016. Mayors and municipal council members control oil and gas revenues and extract personal and political rents. Thus, the expected value of holding future office increases when a discovery is announced, potentially attracting rent-seeking candidates with lower private-sector opportunity costs ([Caselli and Morelli, 2004](#)). Individuals may also increase campaign donations to buy influence with politicians whom they expect to reciprocate in the form of discretionary public employment when anticipated revenue windfalls arrive ([Colonnelli et al., 2019](#)).

I estimate difference-in-differences specifications where treatment is defined as occurrence of a discovery announcement prior to municipal elections. Results show that discoveries increase the number of competitive candidates as well as the value and number of campaign donations made in local elections. Discoveries decrease schooling levels of candidates and winners, which could erode local governing capacity and public service delivery. I find no effects on public employment patronage. Furthermore, I find that incumbent politicians are significantly less likely to be reelected when a municipality's oil revenues are below expectations at the time of the election (e.g., when the municipality is disappointed). These results suggest voters, unable to observe politicians' true quality and honesty, may interpret a lack of revenue windfall after promising discoveries as an indicator of corruption and punish incumbents accordingly. In turn, increased political turnover in disappointed places may disrupt public service delivery and ability to adjust to revenue shortfalls ([Akhtari et al., 2021](#); [Toral, 2021](#)).

To test robustness of my results to the choice of control group, I re-estimate my main specifications using pre-matched never-treated control groups constructed through coarsened exact matching ([Iacus et al., 2012](#)). I also re-estimate event studies using the standard two-way fixed effects (TWFE) estimator as a benchmark comparison. Results are stable in sign, magnitude, and significance across alternative samples and estimators. I estimate conditional random assignment tests to document that neither baseline (year 2000) municipality characteristics nor political alignment between municipal mayors and state/federal leaders predict where offshore exploratory wells are drilled, where discovery announcements occur, or what type of discovery outcome is realized. Finally, I explore robustness to alternative forecasting parameters and matching specifications, and re-estimate event studies using flexible specifications that allow for multiple events per treated unit ([Sandler and Sandler, 2014](#)).

I contribute causal, subnational evidence of short and long-term impacts of resource discoveries and subsequent production realizations on governance. A growing literature on the "Presource Curse" has documented long delays, fiscal problems, arms purchases, and corruption after major oil and natural gas

discoveries in Africa (Cust and Mihalyi, 2017; Mihalyi and Scurfield, 2020; Vezina, 2020; Vicente, 2010; Wright et al., 2016). I explore these dynamics in a novel context that presents institutional contrasts to earlier research. I extend previous findings, which have faced cross-country data limitations, by constructing uniquely detailed municipality-level panel datasets measuring a wide range of governance outcomes. More broadly, I contribute to literature on the Resource Curse, which has increasingly moved from studies at the cross-country level (Alexeev and Conrad, 2009; Mehlum et al., 2006; Sachs and Warner, 2001) to the subnational level (Cust and Poelhekke, 2015). By taking the timing and heterogeneity of discoveries and production into account, I add nuance to existing evidence on the effects of resource revenues on local public finances (Ardanaz and Tolsa Caballero, 2016; James, 2015).

My findings add to existing research on the economic and political effects of Brazil's royalty transfers (Bhavnani and Lupu, 2012; Monteiro and Ferraz, 2010; Serra, 2005). In line with my results, Postali (2015) finds royalty recipient municipalities in Brazil exert less tax collection effort, creating risk of dependency on oil revenues and vulnerability to oil shocks. Baragwanath (2020) finds that royalties increase corruption and entry of more corrupt candidates, supporting my hypothesis that discovery announcements may encourage rent-seekers to run for office. Lastly, Cavalcanti et al. (2016) compare economic outcomes in Brazilian municipalities where successful versus unsuccessful wells were drilled between 1940-2000. They find that *onshore* discoveries had positive economic effects, but no detectable effects from *offshore* discoveries. My work complements this study by exploring a key determinant of economic development: local governance. I focus on effects of major offshore discoveries announced publicly since 2000, which were much larger and more salient than pre-2000 discoveries. A key contribution I make to this body of research is identification of municipalities left disappointed after discovery announcements, which experience negative long-term fiscal outcomes.

I make a methodological contribution to the analysis of resource discoveries and the Resource Curse by quantifying heterogeneity in discovery realizations. My forecasting model reveals the scale of windfalls and the extent of disappointment. Failure to account for heterogeneous discovery realizations could lead studies of resource revenues to inadvertently include disappointed places as controls (since they never receive resource windfalls), despite significant resource impacts on this group. Likewise, studies focused on effects of discoveries may offer biased estimates insofar as they do not account for long-term divergence in outcomes between disappointed and satisfied places. Finally, my findings offer actionable policy implications for the design of resource revenue sharing rules, regulation of discovery announcements and forecasts, and post-discovery management and planning.

The remainder of this paper is organized as follows: In section 2, I describe the institutional context of oil and governance in Brazil. In Section 3, I develop an analytical framework for understanding how

a resource discovery with production delay affects local governance. In section 4, I develop a model of offshore oil production and royalty distribution to forecast municipalities' revenue expectations. In section 5, I present data sources for outcomes of interest. In section 6, I introduce my empirical strategies and discuss causal identification. In section 7, I present results and robustness checks, and in section 8 I conclude with discussion and policy implications.

## 2 Context: Oil and Local Governance in Brazil

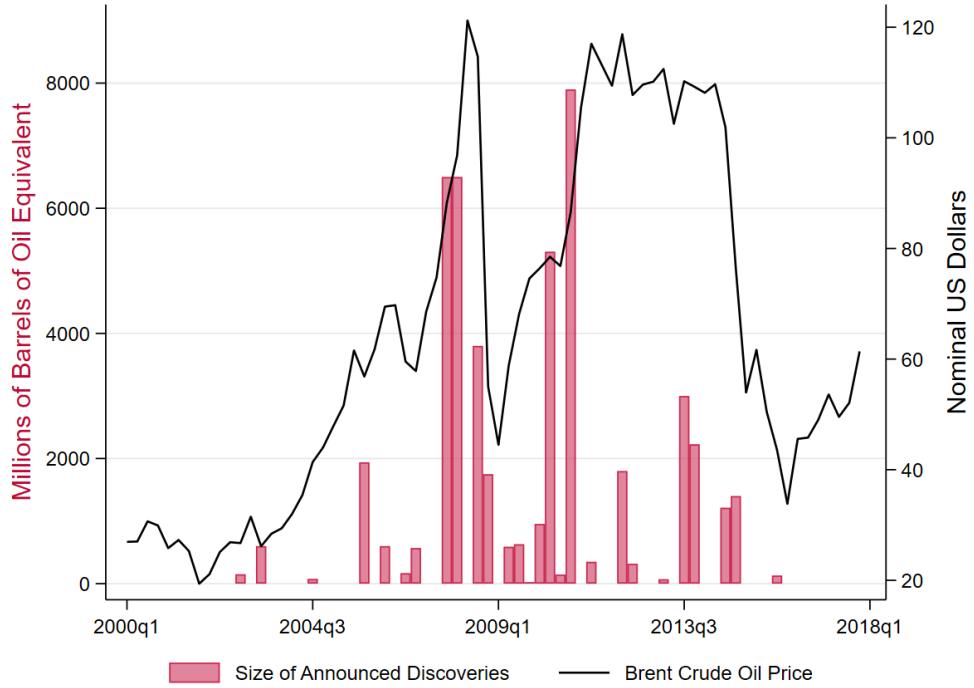
Brazil experienced major offshore oil and gas discoveries during the 2000s and 2010s. The largest occurred in the ultra-deepwater Pre-Salt layer of the Santos and Campos sedimentary basins off the coast of São Paulo, Rio de Janeiro, and Espírito Santo, though large discoveries were made off the coasts of Sergipe, Rio Grande do Norte, and Ceará as well. Major Pre-Salt discoveries included the announcement in November, 2007 of the 5-8 billion barrel Tupi field (production name Lula), the announcement in May, 2010 of the 4.5 billion barrel Franco field (production name Búzios), and the announcement in October, 2010 of the 7.9 billion barrel Mero field (production name Libra). In total, 179 major discoveries averaging 429 million barrels each were announced between 2000 and 2017. Figure 1 illustrates annual announced discovery volumes and world oil prices over this period.

Contemporaneously with the Pre-Salt discoveries, a period of high world oil prices increased the expected value of the finds and provoked a wave of optimism. In 2009, Brazil's president at the time, Luiz Inácio Lula da Silva, said that "the Pre-Salt is a gift from God, a passport to the future, it's a winning lottery ticket, but could become a curse if we don't invest the money well ([Batista, 2008](#))."  
Lula's then chief of staff and later president Dilma Rouseff remarked that "there will be money left over [from the Pre-Salt] for pensions, for improving the living conditions of the population, for investment, for everything ([Batista, 2008](#))."  
Despite this optimism, the crash in world oil prices in 2014 and the outbreak of a major corruption scandal centered on Petrobras (Brazil's national oil company) in 2014 combined to slow Pre-Salt developments.<sup>5</sup>

---

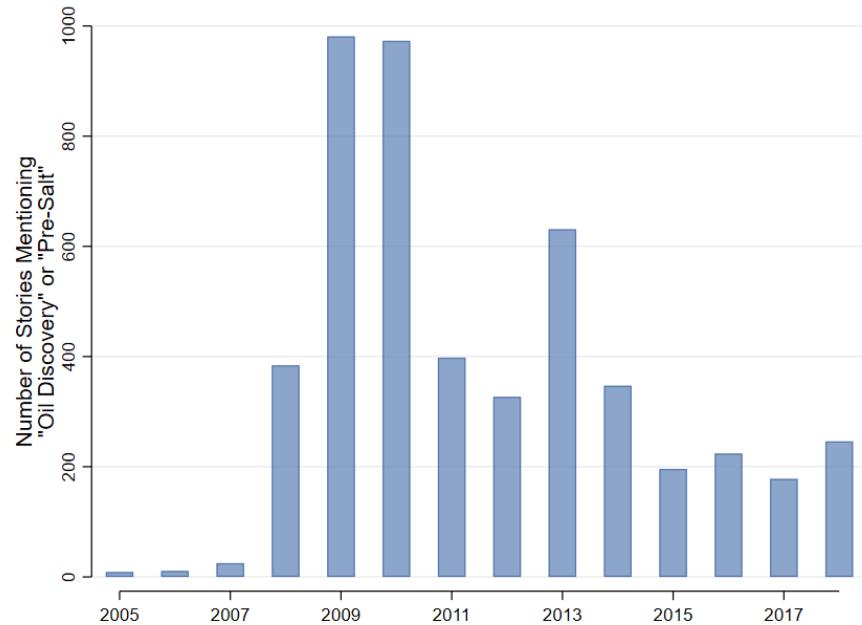
<sup>5</sup>Further delays were introduced by a reform of the Brazilian oil sector begun in response to the Pre-Salt discoveries. In 2006 the Brazilian government shut down auctions of exploratory blocks in the Pre-Salt region until it could develop a new regulatory regime for these areas. Development in these fields was largely paused until this reform passed in 2010, substituting a concession regime for a production sharing regime and requiring a minimum 30% participation by Petrobras on Pre-Salt exploration and production operations ([Florêncio, 2016](#)).

Figure 1: World Oil Prices and Major Offshore Discoveries in Brazil



The Pre-Salt discoveries became a major topic in news media, with 981 stories reporting on them in Rio de Janeiro's *O Globo* newspaper in 2009 alone (Figure 2). This public visibility likely filtered down to municipalities in affected regions, where elections in 2008, 2012, and 2016 could have been influenced by discovery announcements.

Figure 2: News Coverage of Oil Discoveries in *O Globo*



## 2.1 Discovery Announcements

Oil companies during this period announced major discoveries in “communications to the market” filed with the *Comissão de Valores Mobiliários*, Brazil’s Securities and Exchange Commission.<sup>6</sup> I compile all communications pertaining to preliminary exploratory drilling results, new oil discoveries, confirmatory discoveries, and declarations of commerciality for 26 major and minor oil companies operating in Brazil between 2000 and 2017 (see Appendix B1 for additional information on companies and discoveries in the CVM dataset). Collectively, these companies were responsible for nearly 100% of oil drilling, and all major discovery announcements during the period, with the exception of three major discoveries announced by Brazil’s *Agência Nacional do Petróleo* (ANP), or National Oil Agency (also included in the dataset). Declarations typically specify the well, exploratory block, and exploratory field where the discovery occurred, and often include a map of the discovery to illustrate its position relative to coastal municipalities. Figure 3 maps all major offshore discoveries announced between 2000 and 2017.

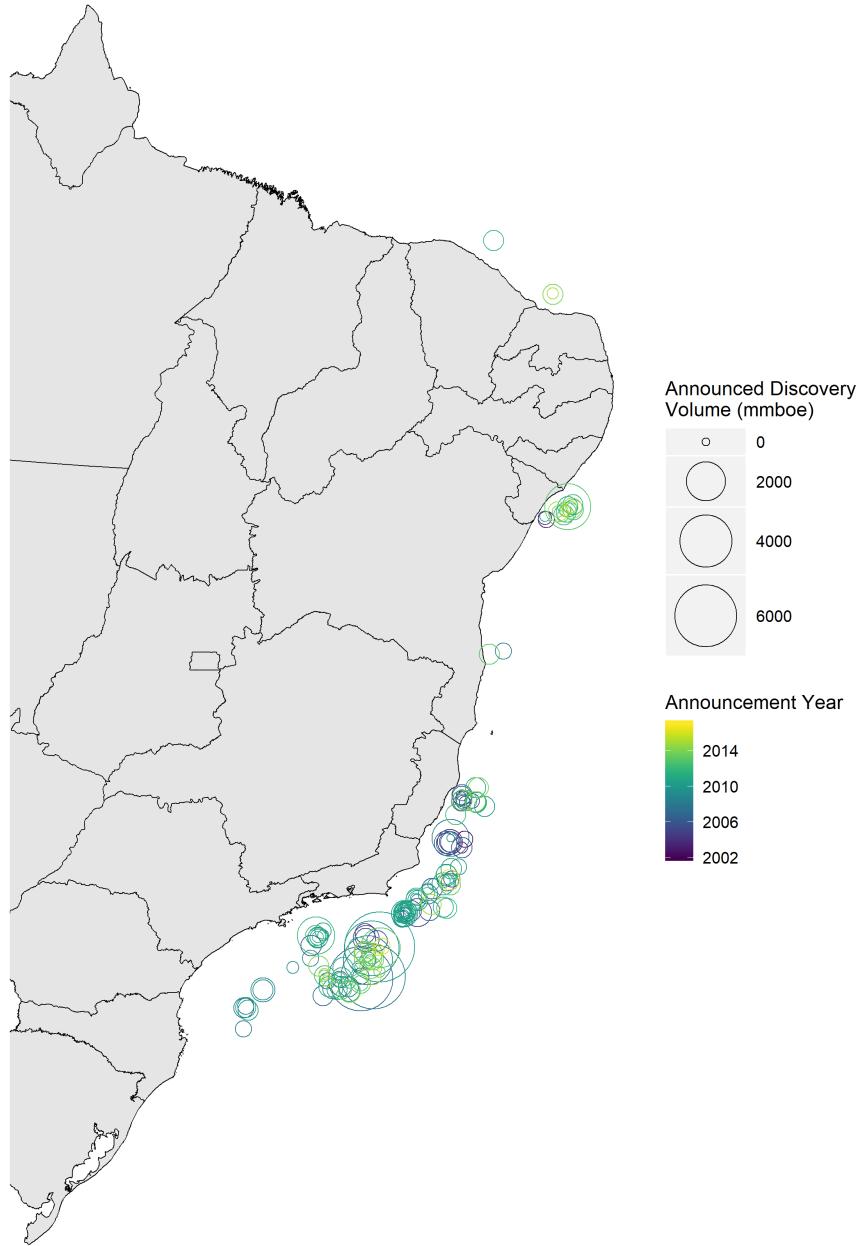
Information in CVM declarations frequently appears promptly in news coverage, transmitting discovery information to the broader population. Interested parties, such as municipal governments, can also access the National Oil Agency website directly to ascertain offshore developments. I document news coverage of discoveries by compiling news stories mentioning “oil discovery” (and variations) in *O Globo*, Rio de Janeiro’s newspaper of record, dating back to 2005. I am able to identify contemporaneous news coverage of nearly every CVM announcement published during this period.<sup>7</sup>

---

<sup>6</sup>An alternative definition of discovery is provided by “declarations of hydrocarbon detections,” which are reports filed by oil companies with the ANP whenever an exploratory well encounters signs of oil or gas. These declarations are much more numerous than CVM discovery announcements, and are likely much less salient. In Appendix A1, I plot histograms of well initiation, conclusion, and declaration of hydrocarbon detection around the date of CVM discovery announcements to document that the date of hydrocarbon detection is closely related to the date of discovery announcement. Hydrocarbon detections are an administrative filing with little public transparency, in contrast to the well-publicized CVM announcements. Furthermore, while hydrocarbon detections give no measure of the scale of the discovery, and often include very small finds, CVM announcements are typically reserved for major discoveries, further increasing the salience of these events.

<sup>7</sup>In Appendix A2, I present a CVM announcement of a major discovery by Petrobras, and the news report on this announcement that was published the same day and which reported all key information contained in the announcement.

Figure 3: Offshore Oil or Gas Discovery Announcements Filed by Oil Companies with Brazil's *Comissão de Valores Mobiliários* (2000-2017)



## 2.2 Disappointment at Country and Field Levels

Mihalyi and Scurfield (2020) document near-universal disappointment after major oil discoveries in 12 African countries. Was Brazil also disappointed by its wave of offshore oil and gas discoveries? In Figure 4, I compile country-level production forecasts from a variety of sources and plot them

against realized production levels. Evidently, forecasts were systematically overoptimistic during this period. This disconnect between forecasts and realized production was likely the result of a number of factors, including the technical difficulties of ultra-deep water drilling and extraction, the Lava Jato corruption scandal that impacted Petrobras in 2014, and the sharp decline in world oil prices that same year. Differently from many of the African countries studied by [Mihalyi and Scurfield \(2020\)](#), Brazil's economy is large and diversified, reducing the potentially deleterious impacts of forecast error at the national level. Nevertheless, the Brazilian oil industry is geographically concentrated, and subnational regions may have faced the brunt of any disappointment.

Moving to the field level, I compile every instance in which a CVM discovery announcement or official ANP statement offered a prediction of field-level start dates. In Figure 5, I plot the relationship between forecast and realized years to production. In the African context, [Mihalyi and Scurfield \(2020\)](#) find that all but one major field lies on or above the 45 degree line, suggesting that field-level delays were nearly universal. In Brazil, field-level time-to-production forecasts were more heterogeneous. Many fields (including major fields such as Tupi/Lula and Mero/Libra) lie on or below the 45 degree line, suggesting they began production on or ahead of schedule. Nonetheless, a number of fields that were forecast to begin production within the sample period never produced as of 2018.

Figure 4: Brazil: Country-Level Production Forecasts vs Realized Production

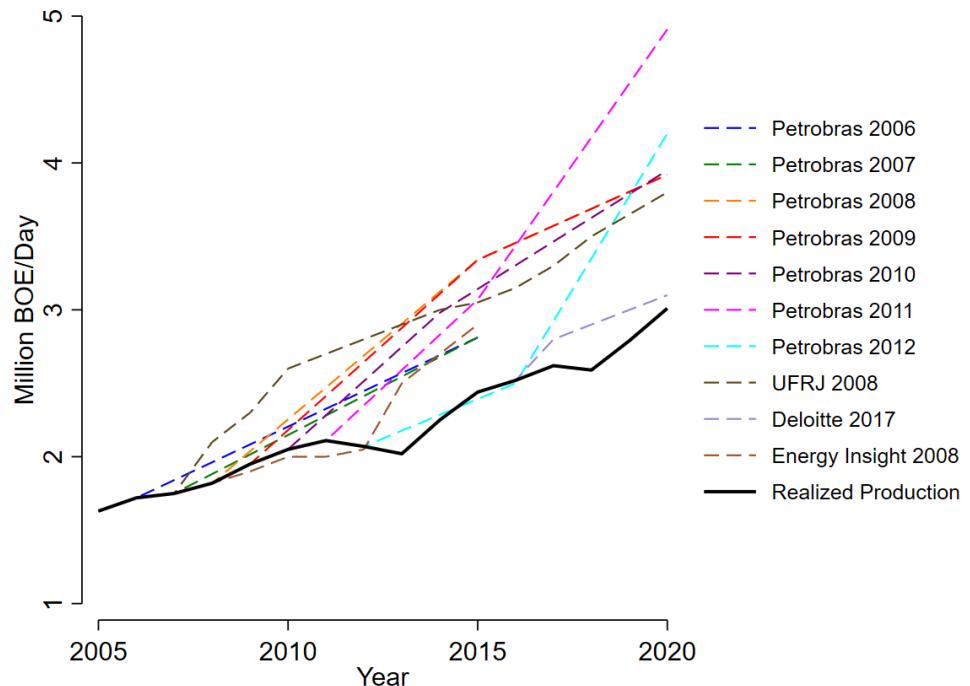
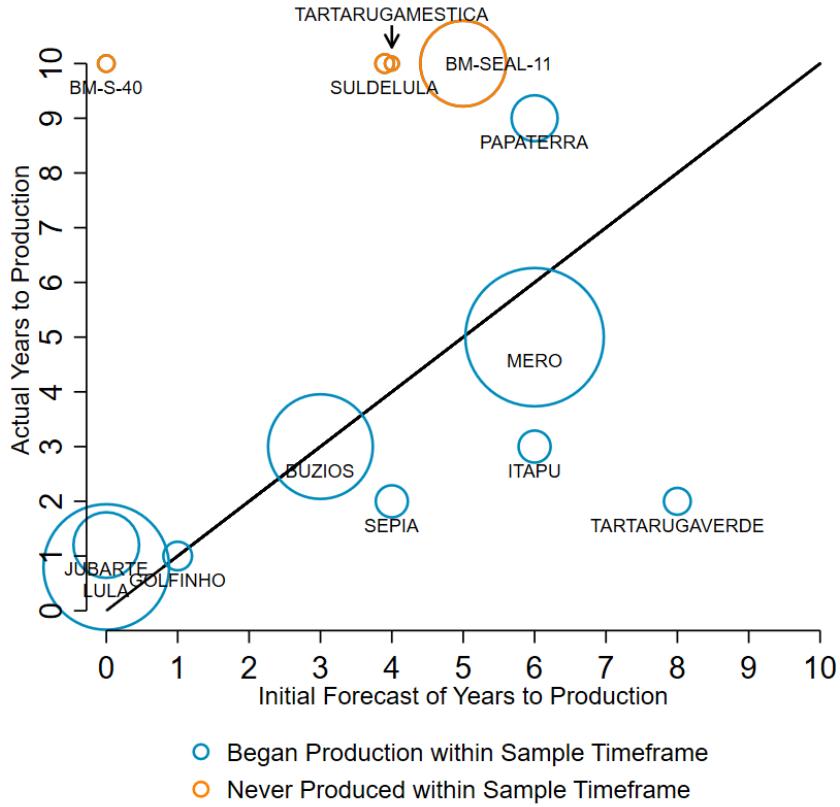


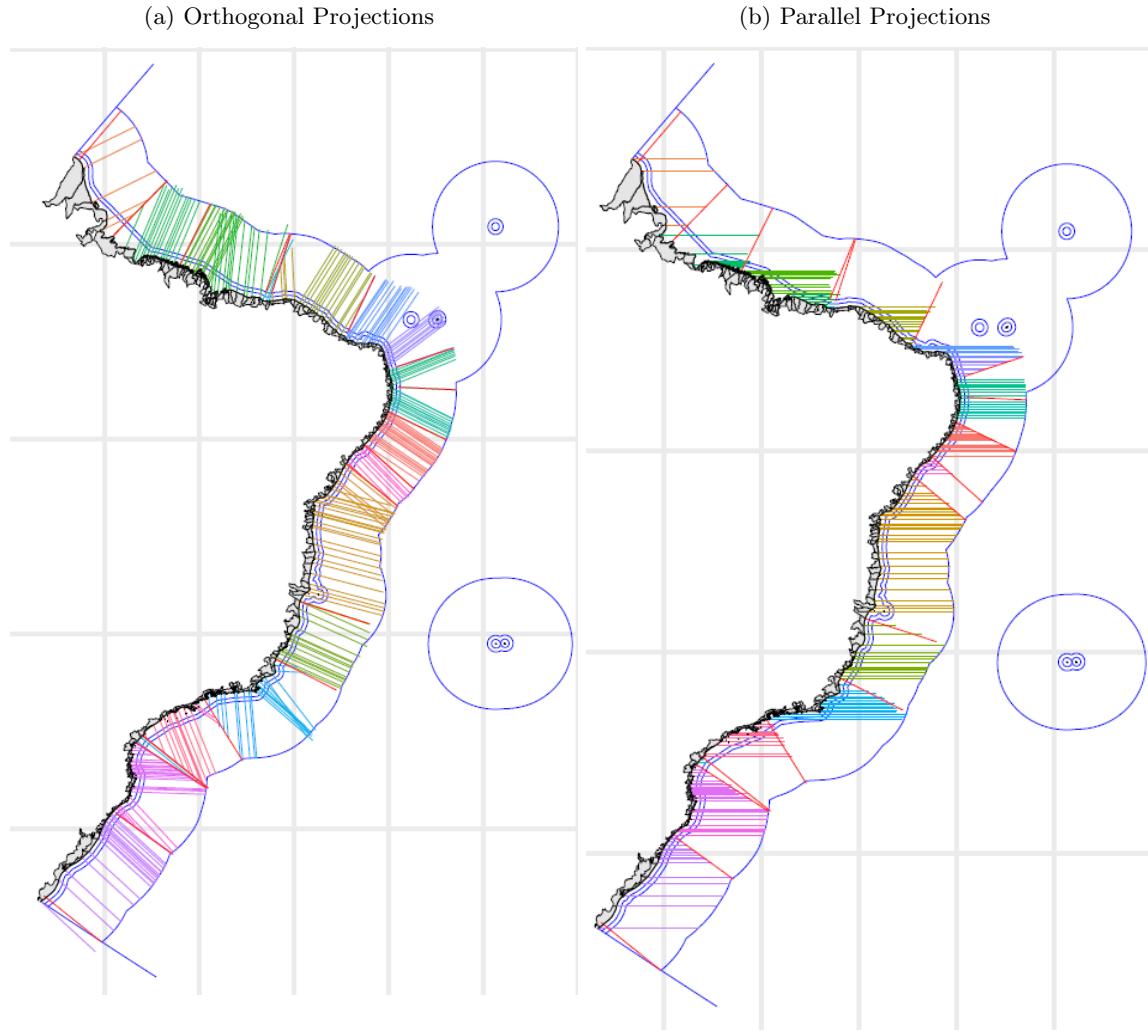
Figure 5: Brazil: Field-Level Time-to-Production Forecasts



### 2.3 Royalty Distribution

In 1985-1986, Laws 7.453/85 and 7.525/86 established royalty requirements for Brazilian offshore oil production and created a system of orthogonal and parallel geodesic projections of coastal municipal boundaries to determine royalty distribution to coastal municipalities (Piquet and Serra, 2007). Distribution is determined by a formula that takes into account geographical alignment with offshore oil and natural gas fields, population, the presence of oil and gas infrastructure within municipal boundaries, specific tax rates applied to each field, and the current volume and value of production. Municipalities directly aligned with offshore fields are called “producer municipalities,” and receive large shares of royalties and additional revenues from especially productive fields, called “special participations” (Gutman, 2007). I describe royalty distribution rules in more detail in Appendix D.2.

Figure 6: Geodesic Projections to Maritime Boundary for Oil & Gas Revenue Distribution



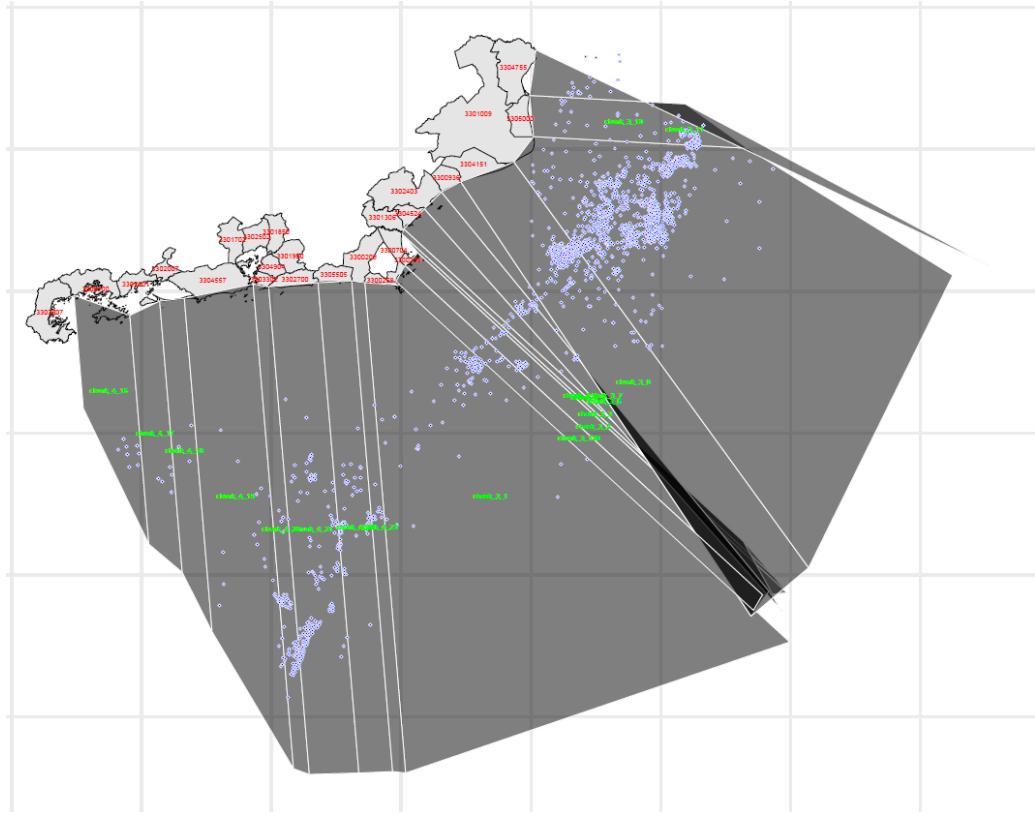
Note: Colors correspond to states. Orthogonal and parallel projections of municipal boundaries are drawn separately for each state, and cut off at state boundary-projections. Projections extend 200 nautical miles (370km.) to Brazil's maritime limit, designated by the solid blue line.

Brazil's use of geodesic boundary projections to determine offshore royalty allocation creates a quasi-experiment in which exogenous offshore discoveries and production outcomes are transparently tied to specific coastal municipalities for reasons outside of municipalities' control. Coastal municipalities are likely to have at least a basic understanding of the projections and the extent of their individual catchment zones, since these determine their royalty receipts and thus significant fractions of their budget. To tie each major discovery announcement back to geographically aligned municipalities, I merge wells cited in discovery announcements with the ANP's comprehensive well database, allowing me to geolocate discovery wells. I next reconstruct the orthogonal and parallel projections of municipal coastal boundaries used by the ANP to determine municipal royalty distributions. I present further

details on this reconstruction in Appendix D1. Figure 6 presents orthogonal and parallel projections of municipal boundaries.

Finally, I overlay all wells in the ANP registry on top of catchment zones created by the geodesic projections, and link wells discovery wells back to their aligned municipality, as illustrated in Figure 7, which presents orthogonal projections for the state of Rio de Janeiro. This well-municipality crosswalk creates the municipality-year level treatment variable I use in event studies.

Figure 7: Rio de Janeiro: Offshore Wells Overlaid on Orthogonal Projections



## 2.4 Municipal Public Finances

Brazil has a federal governing system with significant authority devolved to the municipal level. Municipal governments receive the majority of their budgets from formulaic federal and state transfers based on variables such as population. Municipalities also collect taxes, specifically on real estate transactions (ITBI), service providers (ISS), and property (IPTU) ([Egestor, 2020](#)). These taxes typically account for 5-25% of municipal budgets, with the rest coming from transfers ([Abrucio and Franzese, 2010](#)). Using these funds, municipal governments are responsible for a large proportion of health, education,

public safety, infrastructure, environmental, and cultural services. For instance, the vast majority of schools and hospitals in Brazil are run by municipalities. Municipal governments therefore have significant responsibility and autonomy in financial administration and public goods provision.

There are, however, important limitations on municipal government financial autonomy. The primary constraint is the Fiscal Responsibility Law (LRF) introduced in 2000 ([Giuberti, 2017](#)). The LRF puts limits on allowable levels of spending and debt for municipal governments. While these limits are quite generous and do not bind for most municipalities, they nonetheless restrain extreme fiscal behaviors and may temper municipal reactions to discovery announcements ([Fioravante et al., 2006](#)). Specifically to oil and gas royalties, rules stipulate that the funds cannot be spent to service debt or to pay for public employment. Rather, they must be spent on public goods and services such as infrastructure, health, and education. Nonetheless, money is fungible and royalty transfers can be used to substitute funds in other areas, making their use quite flexible ([Pacheco, 2003](#)).

## 2.5 Municipal Elections

Municipal elections occur every four years in Brazil, offset by two years from state and national elections. Municipal elections occurred in 2000, 2004, 2008, 2012, and 2016. Municipal elections elect mayors and municipal council members (i.e., legislators), whose number is determined by the population of the municipality. In municipalities with populations less than 200,000, mayors are elected in a first-past-the-post system. For municipalities with more than 200,000 people, mayoral elections go to a second round if no candidate wins a majority in the first round. Councilors are elected using an open list proportional representation system. Voting is obligatory. Campaign donations were allowed from individuals, parties, campaign committees, and businesses through the 2012 election; donations from businesses were banned in the 2016 election. Mayors are eligible to serve only two consecutive terms ([Lavareda and Telles, 2016](#)).

## 3 Analytical Framework

How might local policymakers react to major discovery announcements and the expectation of higher future revenues they create? In this section, I develop two alternative analytical lenses to address this question: the political agent framework ([Brollo et al., 2013](#); [Caselli and Cunningham, 2009](#); [Robinson et al., 2006](#)), and the benevolent government framework ([James, 2015](#)).

### 3.1 The Political Agent Framework

Suppose a local government has elected leaders. Leaders are utility maximizers who seek to appropriate personal rents and win re-election against free entry of challengers. Leaders allocate government revenues (including exogenous resource revenues) to personal appropriation, public employment and goods provision, or patronage, and set local taxes. Challengers do not yet control the levers of power, and so cannot appropriate personal rents or provide public goods. They can however make commitments to patronage, such as promising supporters public jobs conditional on winning office.<sup>8</sup>

After announcement of an oil discovery, political agents update their expectations of future resource revenues. The positive shock to expected revenues increases incentives for incumbents to stay in office and for challengers to enter office. If leaders substitute from productive activities (e.g., governing) to reelection activities (e.g., fundraising), public goods provision could suffer. Likewise, if leaders shift revenues from public goods provision to patronage, this could reduce welfare directly (fewer public goods) and indirectly (by giving public jobs to unqualified political supporters) ([Caselli and Cunningham, 2009](#)). Leaders can also cut taxes to curry popular support under the assumption that future resource revenues will fill the gap. Tax cuts could undermine governing capacity and public goods provision in the present and may be difficult to undo if expectations of resource revenues are disappointed. Alternatively, increased interest in holding office could prompt leaders to exert *more* governing effort or provide *more* public goods. The relative efficacy of public goods provision versus patronage in winning elections is thus an important determinant of whether resource revenues are a curse or blessing for the community. This depends on prevailing levels of institutional quality and governing capacity. In communities with weak institutions and low capacity, public goods provision may be inefficient and patronage may be easier, shifting the balance in favor of a low-public goods, high-patronage equilibrium.

An anticipated increase in the value of holding office after a discovery may increase political competition and selection. Quality of challengers may rise if the prospect of increased rents attracts individuals with higher opportunity costs ([Galasso and Nannicini, 2011](#)). On the other hand, quality of challengers may fall since rents are more valuable for lower-ability individuals who can earn less in alternative occupations ([Caselli and Morelli, 2004](#)). Competition can shorten time horizons for leaders, increasing personal appropriation of rents if they believe their days are numbered ([Laurent, 2021](#)).

After resource revenues begin to flow, political agents observe whether their expectations of revenues

---

<sup>8</sup>[Robinson et al. \(2006\)](#) show that, in their modeling setup, challengers cannot make credible commitments to hiring workers after winning the election. This conveys an electoral advantage on incumbents, particularly in communities where weak institutions make patronage easier.

were accurate or not. In the case of a high revenue realization, further rounds of increased political competition and selection may unfold. In this case, leaders may find it easier to appropriate personal rents without voters noticing, leading to an increase in corruption and patronage ([Baragwanath, 2020](#)). In the case of a disappointing revenue realization, incumbent leaders may be forced to cut spending or hiring, or struggle to explain the absence of promised windfalls, reducing their reelection rates. Increased political turnover may disrupt and reduce the quality of administration and public service delivery ([Akhtari et al., 2021](#); [Toral, 2021](#))

Based on the political agent framework, I make the follow predictions about the effects of an oil discovery and subsequent revenue realizations on local governance: (i) after a discovery announcement, spending on public goods and personnel will rise and taxes will fall as leaders appeal to voters (this could be curtailed by fiscal constraints); (ii) after a discovery announcement, political competition and patronage will increase as expected returns to holding office rise; (iii) after realization of high revenues, spending on public goods and personnel will increase, taxation will fall, and corruption will increase; (iv) after realization of low revenues, spending and incumbent reelection rates will fall.

### 3.2 The Benevolent Government Framework

In this alternative setup, a benevolent government earns revenues from local taxes and an exogenous resource sector, and maximizes welfare over two periods ( $t$  and  $t+1$ ) by choosing public goods spending and setting local tax rates subject to a balanced-budget constraint ([James, 2015](#)). In the first period, the government forms expectations of future  $t+1$  resource revenues and seeks to smooth public and private consumption across time. If the local government is constrained only by a balanced-budget requirement at the end of period  $t+1$ , it will borrow in period  $t$  and increase present public goods spending and/or cut taxes in anticipation of future resource revenues. If a period-specific balanced-budget constraint is imposed, the local government will not be able to borrow in  $t$ , and will only increase public goods spending and cut taxes upon receipt of its revenue windfall in  $t+1$ .

In the case of disappointed resource revenue expectations in  $t+1$ , the benevolent government will cancel plans to increase public goods spending or cut taxes and will continue along its baseline equilibrium. Evidently, a single-period balanced budget constraint (i.e., Brazil's fiscal responsibility law) limits the scope for negative impacts of disappointment. Any observed effects of disappointment may therefore be the result of the political agent dynamics described above. Alternatively, negative effects of low revenue realizations may arise from spatial spillovers between disappointed and satisfied places, wherein firms or individuals shift to booming places after discovery realizations, eroding the

local tax and transfer base.

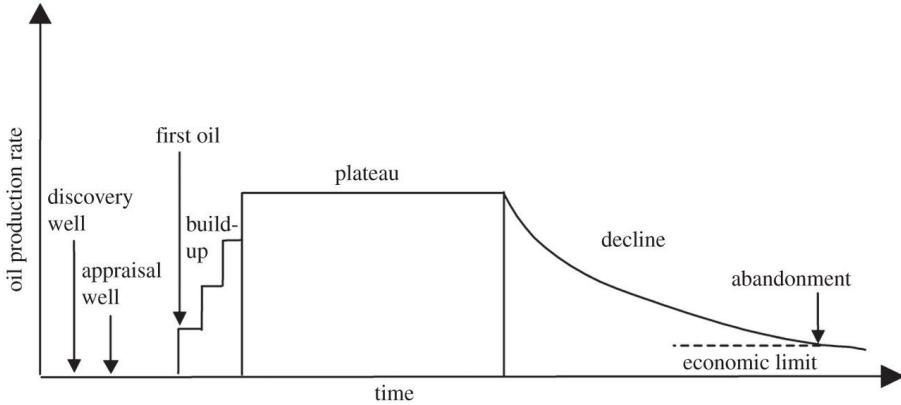
The benevolent government framework generates the following predictions: (i) after a discovery announcement, spending on public goods and personnel will rise and taxes will fall (if not constrained by a single-period balanced budget constraint); (ii) after realization of high revenues, spending on public goods and personnel will increase and taxation will fall; (iii) after realization of low revenues, public goods and personnel spending will remain unchanged from baseline levels. This framework does not rule out increased political competition and selection effects after discovery announcements, but does preclude increased patronage or corruption after discovery announcements and realizations.

## 4 Modeling Discovery Expectations

How often are individual municipalities disappointed or satisfied with the discoveries in their offshore catchment zones? In this section, I develop a model of offshore oil production and royalty distribution to forecast each discovery-affected municipality’s expected oil and gas revenues following a discovery announcement. I then use these forecasts to categorize municipalities as “satisfied” or “disappointed” based on the gap between their expected and realized revenues. The intention of this exercise is to build a heuristic model that approximates reasonable expectations municipal leaders or informed citizens could have formed upon observing a discovery announcement.

After a discovery well is drilled, there is a buildup period of several years before peak production is reached. Figure 8 depicts a standard production trajectory for offshore oil and gas ([Höök et al., 2014](#)). I estimate this production curve for each discovery-affected municipality. I then input values from this curve into the ANP royalty distribution formula to calculate the expected revenue stream from a discovery. Where multiple discoveries occur in the same municipality, I treat them additively.

Figure 8: Offshore Oil Production Curve



Source: Höök et al. (2014)

For each discovery announcement  $d$ , let  $t_0$  be discovery year,  $\theta_{st}$  be average discovery-to-production delay in sedimentary basin  $s$  up to year  $t$ , and  $V_d$  be the announced volume of new estimated reserves associated with discovery  $d$ .<sup>9</sup> Then  $\delta V$  is the peak rate of production, where  $\delta$  is a proportion of the total reserve volume extracted each year. In my preferred specification I use  $\delta = 0.02$ , which would result in approximately 46% of recoverable reserves being extracted over 30 years, a conservative but plausible expectation (US Energy Administration, 2015). I then calculate the expected production stream of  $d$  in year  $t$  for each municipality  $m$  that is aligned with  $d$  ( $\mathbb{1}(\text{alignment}_{md} = 1)$ ) according to the geodesic projection maps described above:

$$E(\text{Production}_{mdt}) = \begin{cases} \mathbb{1}(\text{alignment}_{md} = 1) \times \delta V_d \times \frac{(t-t_0)}{\theta_{st}} & \text{if } t - t_0 \leq \theta_{st} \\ \mathbb{1}(\text{alignment}_{md} = 1) \times \delta V_d & \text{if } t - t_0 > \theta_{st} \end{cases} \quad (1)$$

For simplicity, I do not forecast the production stream out to the exponential decline period, since the longest post-discovery period I observe in the data is 15 years. Expected production stream  $E(\text{Production}_{mdt})$  thus varies according to the prevailing basin-level delay period up to the year of discovery, allowing for geological variation in delay times across basins.

To compute expected royalty revenues from a specific discovery, I apply the official ANP royalty formula (ANP, 2001), where  $P_t$  is the Brent Crude reference price in year  $t$ ,  $X_t$  is the BRL/USD exchange rate in year  $t$ ,  $R_f$  is the tax rate applied to field  $f$ , and  $A_{mf}$  is the alignment share between

<sup>9</sup>For oil companies, estimating the size of newly discovered reserves based on a small number of exploratory wells is challenging, and companies often hold back on giving an estimate of a reserve's size until multiple successful wells have been drilled. Thus, CVM declarations sometimes announce a discovery without announcing an estimated reserve volume. For announcements that do not declare volume, I impute volume based on the median volume declared for other announcements of the same type (Preliminary, Discovery, Confirmatory, and Commerciality). Due in part to the imprecision introduced by this imputation, I check the robustness of my results to low, medium, and high forecasts.

municipality  $m$  and field  $f$ :

$$Royalties_{mdt} = \underbrace{\left( \mathbb{1}(alignment_{md} = 1) \times E(Prod_{mdt}) \times (P_{t0} \times X_{t0}) \times 0.30 \times 0.05 \right)}_{\text{First 5\% of Royalty Tax to Municipalities Aligned with Well}} + \\ \underbrace{\left( E(Prod_{mdt}) \times (P_{t0} \times X_{t0}) \times 0.225 \times (R_f - 0.05) \times A_{mf} \right)}_{\text{Tax in Excess of 5\% to Municipalities Aligned with Field}} \quad (2)$$

See Appendix D.2 for a more complete exposition of the royalty distribution formula. In Equation 2, I fix world oil price  $P_t$  and exchange rate  $X_t$  to levels at the time of discovery in order to focus on expectations as they would have been formed in  $t_0$ . I simplify by converting oil and gas discoveries into oil equivalent units and by ignoring special participations, which are additional government takes applied to high productivity fields.

Finally, I compute a normalized measure of forecast error, which I refer to as  $Disappointment_{mt}$ , by taking the ratio of realized growth in per capita revenue between the year of the event and year  $t$  over expected revenue growth over this period:

$$Disappointment_{mt} = \frac{\frac{Royalties_{mt}}{Royalties_{m,t0}}}{\frac{E(Royalties_{mt})}{Royalties_{m,t0}}} \quad (3)$$

Equation 3 generates a municipality-time varying measure of forecast error that is less than 1 when realized revenue growth between years  $t_0$  and  $t$  is less than forecast revenue growth over that period, and greater than 1 when realized growth is greater than forecast growth over that period. In the main event study analysis, I explore heterogeneity across forecast error by classifying municipalities into two groups: (i) "disappointed" municipalities are those where  $Disappointment_{m,2017} \leq 0.4$ , indicating that post-discovery realized oil revenues grew by less than 40% of what these places could have expected by 2017; (ii) "satisfied" municipalities are those where  $Disappointment_{m,2017} > 0.4$ .<sup>10</sup>

To account for assumptions in the model, I check for robustness to low, medium, and high combinations of parameter assumptions. I vary  $\delta$  (annual extraction rate) within bounds suggested in US Energy Information Administration forecasts (0.01 to 0.03), and the alignment between municipalities and newly-forming offshore fields between 0.1 and 0.3 (accounting for the fact that municipalities are typically aligned with only fractions of offshore fields, and thus receive only fractions of the revenues). Figure 9 shows selected examples of municipalities affected by discovery announcements. In each graph,

---

<sup>10</sup>I opt for the 0.4 cutoff value as it approximates the 50th percentile of the distribution of  $Disappointment_{m,2017}$  across alternative forecasting specifications while preserving the intuition behind the disappointed/satisfied classification. In Appendix A4, I report kernel density plots of  $Disappointment_{m,2017}$  derived from low, medium, and high variations of forecast parameters (described in next paragraph).

red lines depict the range of expected revenue forecasts generated by the offshore production model, black lines depict realized oil revenues, and vertical lines mark the first major discovery announcement. In the figure, the top row of municipalities are “disappointed,” that is, they experience large negative forecast error between expected and realized revenues. The bottom row of figures are “satisfied.” In my preferred specification, 30 Brazilian municipalities are left disappointed by major discovery announcements, while 18 are satisfied. In Appendix B2, I report disappointed/satisfied classifications for all discovery-affected municipalities under alternative modeling parameters.

Figure 9: Municipality-Level Per Capita Revenue Forecasts vs Realized Revenues (Selected Examples)

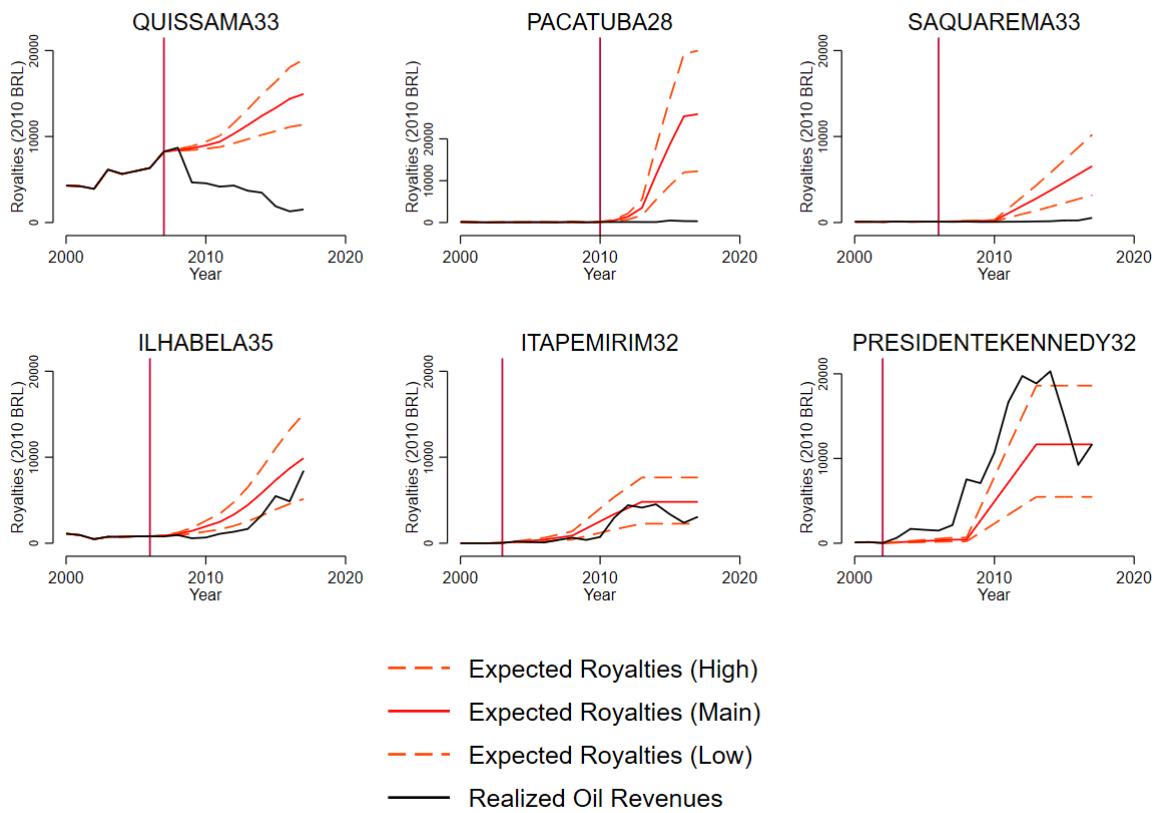
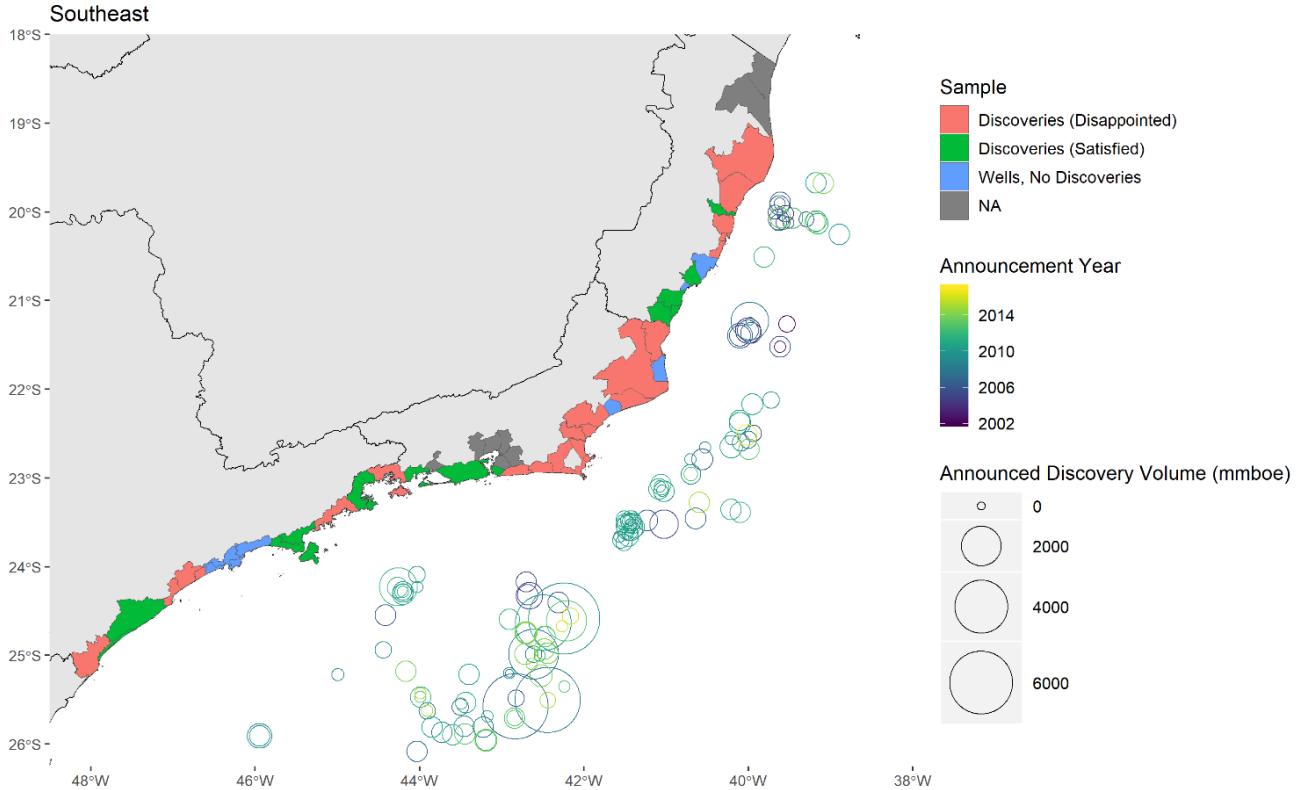


Figure 10 plots all major offshore oil and gas discoveries reported between 2000 and 2017 in Brazil’s Southeast region (where most major Pre-Salt discoveries occurred), as well as the two treatment arms (disappointed and satisfied municipalities) identified by my revenue forecasting model. The figure also maps municipalities that had offshore exploratory wells drilled in their catchment zones during this period, but no major discovery announcements. This group constitutes my preferred control group under the assumption that, conditional on drilling, discoveries and realized outcomes are as-if random

(Cavalcanti et al., 2016; Speight, 2014). In Appendix A5, I reproduce Figure 10 for the entire Brazilian coastline.

Figure 10: Southeast Brazil: Major Offshore Discoveries and Affected Municipalities



## 5 Data

I draw on a wide array of administrative data sources to build an exceptionally rich municipality-year panel dataset to explore the effects of discovery announcements on governance outcomes between 2000-2017. Outcomes include municipal public finance variables such as disaggregated revenues, spending, and investment (realized rather than budgeted), federal and state transfers to municipal governments, municipal public hiring, municipal public goods provision and quality, municipal GDP, and population. I also construct a municipality-election period panel for 2000-2016 that includes demographic, vote, and donations data for all municipal candidates during this period. Monetary values are deflated to constant 2010 Brazilian Reals using the INPC deflator from IPEA. I provide details on data sources and preparation in Appendix D.3. Table 1 summarizes my data sources.

Table 1: Data Sources

Data	Source	Years	Raw Level	Analysis Level
Discovery Announcements	CVM	2000-2017	Well	Municipality
Oil Royalties & Special Part.	ANP	1999-2017	Municipality	Municipality
Offshore Well Shapefiles	ANP	2000-2017	Well	Municipality
Oil and Gas Production	ANP	2005-2017	Well	Municipality
Municipality Shapefiles	IBGE	2010	Municipality	Municipality
Public Finances	FINBRA & IPEA	2000-2017	Municipality	Municipality
Employment & Firm Entry	RAIS	2000-2017	Individual	Municipality
Federal and State Transfers	Tesouro Nacional	2000-2017	Municipality	Municipality
Elections (Candidates)	TSE	2000-2016	Individual	Municipality
Elections (Donations)	TSE	2004-2016	Individual	Municipality
Health Indicators	SUS	2000-2017	Municipality	Municipality
Education Indicators	Basic Ed Census	2000-2017	School	Municipality
Education Outcomes	IDEB	2005-2017	School	Municipality
Municipal Development Index	FIRJAN	2000, 2005-16	Municipality	Municipality
Municipality Characteristics	Census	2000, 2010	Individual	Municipality
Brent Crude Oil Prices	FRED	2000-2017	World	World
Currency Deflator	IPEA (INPC)	2000-2017	Brazil	Brazil
Interest Rate	IPEA (Selic)	2000-2017	Brazil	Brazil

In Table 2, I present baseline (year 2000) descriptive statistics for treated subsamples (“Disappointed” and “Satisfied”) and alternative control groups. My preferred control group (referred to throughout as "Wells") consists of municipalities that received exploratory offshore wells after 1999 but did not receive major discovery announcements. This group differs along a number of dimensions from treated municipalities. Municipalities in the Wells group are located further north (average latitude of -13.04 for Wells versus -19.5 for Disappointed and -21.8 for Satisfied), have smaller populations (averaging 55.4 thousand people versus 91.8 thousand for Disappointed and 398 thousand for Satisfied) and lower average incomes (averaging 1,985 2010 BRL versus 3,135 for Disappointed and 4,065 for Satisfied). They also have lower Municipal Development Indices, revenues, and expenditures. Significant differences between treated and control groups may raise concerns that heterogeneous time-varying shocks could confound estimation of treatment effects. To reduce these concerns, I construct pre-matched control samples for each treated subsample using coarsened exact matching (see details in next section). I include baseline descriptives for all never-treated municipalities in coastal states for comparison.

Table 2: Pre-Treatment (Year 2000) Balance Between Samples

	Treated Samples		Control Samples			
	D	S	Wells	Match (D)	Match (S)	Coastal
<i>Latitude</i>	-19.50 (6.25)	-21.82 (3.13)	-13.04 (9.59)	-20.21 (7.91)	-20.00 (8.13)	-16.40 (9.24)
<i>Dist. from State Capital</i>	116.62 (85.35)	88.59 (57.12)	150.15 (120.02)	192.14 (143.64)	92.79 (38.81)	248.87 (159.90)
<i>Population (Thousands)</i>	91.88 (122.23)	398.53 (1,367.51)	55.42 (81.82)	38.11 (77.30)	56.82 (471.41)	32.26 (192.54)
<i>GDP per capita</i>	17,769 (26,418)	13,779 (12,003)	6,552 (6,735)	6,814 (7,261)	7,840 (9,641)	5,443 (5,978)
<i>Annual Income p.c.</i>	3,135 (131)	4,065 (183)	1,985 (129)	2,474 (92)	2,688 (123)	2,019 (102)
<i>Income Gini Coefficient</i>	0.57 (0.05)	0.57 (0.04)	0.56 (0.07)	0.55 (0.06)	0.53 (0.06)	0.54 (0.07)
<i>Municipal Dev.Index</i>	0.60 (0.07)	0.64 (0.09)	0.50 (0.10)	0.57 (0.09)	0.57 (0.13)	0.53 (0.13)
<i>Urban Share of Pop.</i>	0.83 (0.21)	0.80 (0.22)	0.66 (0.24)	0.68 (0.20)	0.66 (0.25)	0.57 (0.24)
<i>% HHs w. Water/Sewer</i>	7.76 (8.01)	3.63 (3.95)	20.56 (19.57)	10.03 (12.19)	10.67 (15.81)	13.64 (16.19)
<i>% Empl. in Extractive</i>	1.07 (2.01)	0.96 (1.98)	1.03 (3.57)	0.44 (1.01)	0.45 (0.96)	0.44 (1.50)
<i>% Formally Employed</i>	46.14 (12.45)	47.39 (12.46)	34.39 (16.47)	46.19 (15.70)	45.58 (19.09)	36.00 (19.44)
<i>Municipal Revenue p.c.</i>	1,628 (1,478)	1,729 (1,047)	1,011 (809)	969 (2,993)	1,220 (3,840)	1,000 (1,496)
<i>Municipal Tax Rev. p.c.</i>	209.3 (224.4)	395.5 (438.5)	123.3 (276.0)	71.4 (459.8)	114.7 (596.1)	41.8 (225.5)
<i>Municipal Oil Rev. p.c.</i>	420.6 (999.4)	161.8 (334.7)	129.7 (412.9)	15.1 (100.4)	10.2 (43.4)	6.1 (60.0)
<i>Municipal Spending p.c.</i>	1,222 (973)	1,435 (812)	807 (554)	857 (2,913)	1,062 (3,745)	865 (1,442)
<i>Municipal Invest. p.c.</i>	161.0 (223.9)	123.1 (110.3)	98.2 (172.1)	55.0 (116.9)	69.7 (143.8)	63.3 (83.2)
n	30	18	53	836	500	3,902

Note: Sample means with standard deviations in parentheses are reported for treated samples (D = Disappointed and S = Satisfied), as well as alternative control groups: Wells (never-treated municipalities with exploratory off-shore wells completed after 1999), Match (D) (never-treated municipalities matched to Disappointed municipalities on geographic and pre-treatment characteristics using coarsened exact matching), Match (S) (never-treated municipalities matched on Satisfied municipalities in the same manner), and Coastal (all never-treated municipalities in coastal states). Monetary values are deflated to constant 2010 Brazilian Reals. Reported values are from baseline year 2000.

## 6 Empirical Strategy and Identification

I estimate dynamic effects of a discovery announcement on municipal public finances and other outcomes of interest using an event study framework (Sun and Abraham, 2021; Borusyak and Jaravel, 2017). This approach allows me to detect both rapid reactions to discovery announcements that occur in anticipation of future royalties, and longer-term trends driven by the gradual realization of discovery type. To accommodate staggered treatment timing and heterogeneous treatment effects, I implement Callaway and Sant'Anna (2020)'s (CS) group-time average treatment effect estimator.

For municipality  $m$  in year  $t$ , let  $E_m$  be the period when  $m$  is first treated by a discovery announcement.<sup>11</sup> Then let  $K_{mt} = t - E_m$  be the number of years before or after the event. I regress municipality-level outcome  $Y_{mt}$  on  $\mathbb{1}(K_{mt} = k)$  relative year indicators for the fully-saturated set of indicators going from the beginning to end of my sample. I control for municipality and year fixed effects,  $\delta m$  and  $\lambda_t$ , and cluster standard errors at the level of treatment (municipality):

$$Y_{mt} = \delta m + \lambda_t + \sum_{k \neq -1} [\mathbb{1}(K_{mt} = k)]\beta_k + \epsilon_{mt} \quad (4)$$

In this expression,  $\beta_k$  is the average treatment effect on the treated at length of exposure  $k$  from the first discovery announcement. One common challenge with event studies is to find a valid control group that is similar enough to treated units to satisfy the parallel pre-trends assumption, yet is not itself treated. Using already-treated units as controls introduces significant problems for causal inference (de Chaisemartin and D'Haultfœuille, 2020). In my preferred specifications, I use municipalities that received exploratory offshore wells between 2000-2017, but never received a major discovery announcement, as controls. The intuition underlying this choice of control group is that all municipalities that received exploratory offshore wells were comparably attractive in terms of oil prospects and exploratory conditions. Furthermore, previous studies have argued that, conditional on drilling, discovery outcomes are as good as random, introducing further quasi-experimental variation (Cavalcanti et al., 2016; Cust et al., 2019; Speight, 2014).

Since Table 2 documented imbalances in baseline characteristics between subsamples treated with major discoveries and never-treated municipalities that got exploratory wells, I construct pre-matched control groups as a robustness check. Specifically, I use coarsened exact matching (Iacus et al., 2012) to construct never-treated control groups that are balanced with treated groups along the dimensions

---

<sup>11</sup> Discovery treatment turns on at the time of the announcement and never turns off. Relative time indicators are set to 0 for never-treated controls. I assume here that each municipality is treated only once by a discovery announcement. In reality, some municipalities are treated multiple times. Following the methodology proposed by Sandler and Sandler (2014), I estimate an event study specification with multiple events per unit as a robustness check in Appendix C12.

of (pre-treatment, year 2000) quintiles of GDP, population, distance from state capital, latitude, and municipal development index.<sup>12</sup>

I estimate Equation 4 separately for disappointed and satisfied municipalities, each relative to Wells and Matched control groups. Since both discoveries and subsequent production realizations are exogenous to municipal characteristics and outcomes (see discussion of identification below), I conceptualize disappointed and satisfied groups as two independent treatment arms, whose type is known to the econometrician but only gradually revealed to municipalities as discovery outcomes are realized. For all continuous outcome variables, I apply the inverse hyperbolic sine transformation. To interpret semi-elasticities, I follow [Bellemare and Wichman \(2020\)](#) and use the small sample bias correction proposed by [Kennedy \(1981\)](#) to account for the small number of treated units in my sample:

$$\hat{P} = (e^{(\beta - \frac{\widehat{Var}(\beta)}{2})} - 1) \times 100 \quad (5)$$

## 6.1 Political Agent Mechanisms: Discovery Effects on Elections

Political agents' and voters' responses to discovery announcements may be underlie observed governance outcomes. I implement a difference-in-differences specification to study discovery effects on political competition, selection, and patronage. To measure political competition, I compute number of candidates and competitive candidates (total and per seat) ([Niemi and Hsieh, 2002](#)). I compute the number and value of campaign donations to measure intensity of fundraising and influence-buying. As a measure of political selection (and winner characteristics), I use candidates' and winners' sex, age, and education-level. To measure intensity of public employment patronage, I follow [Colonnelly et al. \(2019\)](#) in computing the number and share of campaign donors who are hired to discretionary municipal public jobs (*cargos comissionados*) after the candidate they donated to wins an election.

Let  $Y_{me}$  be an outcome in municipality  $m$  in election period  $e$ . I regress this outcome on unit and time fixed effects ( $\delta_m$  and  $\lambda_e$ ) and  $T_{me}$ , a time varying measure of discovery-treatment.<sup>13</sup> For continuous outcome variables, I apply the inverse hyperbolic sine transformation. I cluster standard errors at the municipality level.

$$Y_{me} = \delta_m + \lambda_e + \beta T_{me} + \epsilon_{me} \quad (6)$$

Finally, I test whether disappointment in offshore revenue expectations at the time of the election

<sup>12</sup>As further robustness checks, I also match on baseline levels of revenue and expenditure.

<sup>13</sup>I estimate this specification using both [Callaway and Sant'Anna \(2020\)](#)'s (CS) estimator and a two-way fixed effects (TWFE) OLS estimator. For the CS estimator, treatment turns on in the period a discovery occurs and remains on until the end of sample. For the TWFE estimator, treatment turns on in the period where discoveries occurred, and turns off in subsequent periods.

leads to lower reelection rates for incumbent politicians. To assess this, I calculate the ratio of realized revenue growth over the previous electoral mandate over expected revenue growth over the same period. I use this time-varying measure of forecast error to create a  $Disappointed_{me}$  indicator that takes a value of 1 when  $Disappointment_{me} < 0.4$  and a  $Satisfied_{me}$  indicator that takes a value of 1 when  $Disappointment_{me} > 0.4$  at the time of the election. I then estimate logit and linear probability models of reelection likelihood for candidate  $c$  in municipality  $m$  in election period  $e$ , where  $X$  is a vector of candidates' age, sex, and schooling level. Standard errors are clustered at the municipality level, and  $\delta_s$  and  $\lambda_e$  are state and election-period fixed effects, respectively:

$$P(Reelection_{cme} = 1) = \delta_s + \lambda_e + \beta Disappointed_{me} + X'\mu + \epsilon_{cme} \quad (7)$$

Here,  $\beta$  is the average treatment effect of disappointment at the time of the election on reelection rates for incumbents. I hypothesize that reelection rates will fall in municipalities experiencing disappointment after a major discovery.

## 6.2 Identification Challenges

An ideal experiment to evaluate the effects of discovery announcements and subsequent revenue realizations on municipal outcomes would randomly allocate discoveries to municipalities. Within the group of municipalities that received discovery announcements, the experiment would then randomly assign some municipalities to the disappointed treatment arm and others to the satisfied arm some years later. In considering identification challenges presented by the Brazilian context, it is useful to focus on ways in which the reality diverges from this experimental ideal.

### *Conditional Random Assignment*

First, are discoveries and subsequent production realizations as-if-randomly allocated to municipalities? The location of offshore exploratory drilling is determined by geological features of the seabed, technologies internal to major oil companies, and exogenous global prices. Thus, geographical features are predictive of offshore oil and gas outcomes. Conditional on fixed geographical features, do pre-discovery municipality characteristics predict where future discoveries occur, or whether discoveries end up successful or disappointed? Since exploratory drilling is extremely expensive, and drilling in the right versus wrong place can mean huge differences in production outcomes, oil companies' profit motives to get the geology right make it very unlikely that they could be influenced by municipal lobbying of any kind. Furthermore, since offshore fields are serviced by ship and helicopter from major

ports, local infrastructure or local economic or governance conditions are unlikely to shape an oil company's decision of where to drill. Once exploratory drilling is undertaken, finding oil or natural gas is as good as random. If it were otherwise, the oil company would have used this information to avoid costly drilling in unsuccessful places ([Speight, 2014](#)).

Among discovery-treated municipalities, are subsequent production realizations as good as random? Development of an offshore field depends on a succession of operations that gradually reveal information about that field, including geological features of the reserve and its surroundings that could make it more difficult than expected to exploit. Further variation in development of fields is caused by idiosyncratic events affecting specific oil companies.<sup>14</sup> Since discoveries occur at different times, global oil price fluctuations introduce additional exogenous variation into revenue realizations: a discovery in 2004 may have begun production in 2009 at the peak of world oil prices, while an identical discovery in 2010 may have begun production after the price crash of 2014, leading to far lower royalties.

To test these arguments empirically, I estimate conditional random assignment tests, where  $Y_m^{2000}$  are municipality characteristics such as GDP, population, etc. in 2000 (pre-discovery),  $Treatment_m$  is an indicator of (i) whether wells are drilled in coastal municipalities; (ii) whether a major discovery is announced in municipalities where wells are drilled; and (iii) whether expectations are satisfied in municipalities that received discovery announcements. I include a vector of time-invariant geographical controls (latitude, distance to state and federal capitals) and state fixed effects:

$$Y_m^{2000} = \alpha + \beta_1 Treatment_m + X'\lambda + \delta_s + \epsilon_m \quad (8)$$

In Table 3, I report results of conditional assignment tests. I estimate Equation 9 separately for each outcome reported in the table, always including geographical controls and state fixed effects. For each test, I report the p-value for the outcome in question, which, if significant, suggests the value of that variable in 2000 was significantly predictive of future wells being drilled (column 1), discoveries being made (column 2), or discovery expectations being satisfied (column 3). In parentheses, I report Romano-Wolf p-values, which adjust for the family-wise error rate after multiple hypothesis testing. As shown in the table, initial municipality characteristics are in some cases predictive of where wells are drilled, but not of where discoveries are made or whether expectations are satisfied. This supports the argument that offshore discoveries and realizations were exogenous to municipality characteristics.

---

<sup>14</sup>For instance, a major Brazilian oil company, OGX, made many large discoveries during the late 2000s and early 2010s, but later encountered financial difficulties and went bankrupt, leaving its fields undeveloped ([Moreno, 2013](#)). The financial health of this company was unknowable to municipalities at the time of discovery announcements, and they had no reason to suspect that the company's discoveries would have different production realizations than discoveries made by Petrobras.

Table 3: Conditional Random Assignment: Pre-Treatment Municipality Characteristics (2000)

Outcome	$\mathbb{1}(Wells = 1)$	$\mathbb{1}(Discovery = 1)$	$\mathbb{1}(Satisfied = 1)$
	p-value (FWER-adjusted)	p-value (FWER-adjusted)	p-value (FWER-adjusted)
<i>Population</i>	0.261 (0.817)	0.661 (0.994)	0.206 (0.804)
<i>GDP</i>	0.016 (0.135)	0.902 (0.995)	0.235 (0.804)
<i>Municipal Develop. Index</i>	0.192 (0.777)	0.163 (0.684)	0.183 (0.804)
<i>Urban Share of Population</i>	0.484 (0.974)	0.600 (0.993)	0.123 (0.725)
<i>Income per capita</i>	0.022 (0.135)	0.673 (0.994)	0.404 (0.804)
<i>Income Gini Coefficient</i>	0.858 (0.992)	0.017 (0.119)	0.192 (0.804)
<i>% Employed in Extractive</i>	0.046 (0.135)	0.802 (0.995)	0.226 (0.804)
<i>% Formally Employed</i>	0.667 (0.92)	0.496 (0.988)	0.450 (0.804)
<i>% Homes w. Water &amp; Sewer</i>	0.755 (0.992)	0.823 (0.995)	0.958 (0.961)
Sample	Municipalities on Coast 277	Municipalities w. Wells 101	Municipalities w. Discoveries 48
Observations			

All regressions are estimated separately using OLS on cross-sectional municipality-level datasets and controlling for the following geographical controls: distance to federal and state capitals, latitude, and state fixed effects. All distances and monetary values use the inverse hyperbolic sine transformation. Outcomes are measured in 2000 (prior to discovery treatment) with the exception of GDP, which is reported in 2002. Model p-values associated with parameter  $\beta_1$  from Equation 9 are reported, with family-wise error rate corrected Romano-Wolf p-values in parentheses. Estimation used rtwolf package in Stata, with adjusted p-values estimated using 1000 bootstrap iterations (seed = 100). Insignificant p-values indicate that the outcome variable measured at baseline was not significantly predictive of that municipality getting wells, offshore discoveries, or a successful discovery realization in the post-2000 period.

Perhaps political favoritism influenced where oil companies focused their exploration or efforts to develop fields? To test for this possibility, I estimate conditional random assignment tests equivalent to those reported in Table 3, but with outcomes registering alignment between the political party of municipal mayors and state governors or the president. I include a state capital dummy and the standard geographical controls. As illustrated in Table 4, political alignment is not significantly predictive of future wells being drilled (column 1), discoveries being made (column 2), or discovery expectations being satisfied (column 3). The state capital dummy is predictive of where wells are drilled, but not discoveries or realizations. Table 4 again supports the claim that offshore outcomes were exogenous to municipality conditions.

Table 4: Conditional Random Assignment: Political Alignment

Outcome	$\mathbb{1}(Wells = 1)$	$\mathbb{1}(Discovery = 1)$	$\mathbb{1}(Satisfied = 1)$
	p-value (FWER-adj.)	p-value (FWER-adj.)	p-value (FWER-adj.)
<i>Cumulative Party Align. w. Governor</i>	0.417 (0.668)	0.604 (0.879)	0.926 (0.937)
<i>Cumulative Party Align. w. President</i>	0.953 (0.963)	0.680 (0.879)	0.160 (0.521)
<i>State Capital Dummy</i>	0.091 (0.283)	0.745 (0.879)	0.198 (0.521)
<i>Contemp. Party Align. w. Governor</i>	0.745	0.387	NA
<i>Contemp. Party Align. w. President</i>	0.558	0.550	NA
<i>State Capital Dummy</i>	0.000	0.973	NA
Sample	Municipalities on Coast	Municipalities w. Wells	Municipalities w. Discoveries
Observations	277	101	48

Regressions in the first panel are estimated separately using OLS on cross-sectional municipality-level datasets and controlling for the following geographical controls: distance to federal and state capitals, latitude, and state fixed effects. All distances use the inverse hyperbolic sine transformation. Cumulative party alignment with governor is the number of years since 2000 in which the municipal mayor's political party was the same as the state governor's party. Likewise, cumulative party alignment with president is the number of years in which the mayor's party was the same as the federal president's party. Regressions in the second panel are estimated separately using logit models on municipality-year panel datasets and controlling for the same geographical controls. Contemporaneous party alignment with governor (likewise for president) is an indicator variable that takes a value of 1 in years when the municipal mayor's political party is the same as the state governor's party (or federal president's party). Model p-values associated with parameter  $\beta_1$  from Equation 9 are reported, with family-wise error rate corrected Romano-Wolf p-values in parentheses where applicable. Estimation used rwoolf package in Stata, with adjusted p-values estimated using 1000 bootstrap iterations (seed = 100).

### *Other Threats to Causal Inference*

Identification of causal effects requires parallel pre-trends between treated and control units, limited spillovers onto neighboring municipalities (the Stable Unit Treatment Value Assumption, or SUTVA), and limited anticipation of discovery announcements ([Callaway and Sant'Anna, 2020](#)). While pre-trends may be verified visually in event studies ( $\beta_k = 0$  for  $t < -1$ ), I also graph sample means of key outcomes for treated subsamples and their control groups in Appendix C14, allowing the reader to evaluate differences in levels and "wiggles" as well as trends ([McKenzie, 2021](#)).

Offshore oil and gas revenues generate small fiscal spillovers as a feature of Brazil's revenue sharing rules, which stipulate that 20% of the municipal share of royalties from a field be shared amongst municipalities sharing a mesoregion (a geographical unit containing an average of forty municipalities) with the producer municipality. Such widespread sharing dilutes fiscal spillovers onto untreated municipalities. Furthermore, municipalities are rooted in place—municipal revenues are mostly spent within municipal boundaries and public services are mostly restricted to municipal residents. Participation in local elections requires (de facto) municipal residency. These factors limit the scope of fiscal and political spillovers from offshore revenue windfalls. Disappointed places never receive revenue windfalls and thus should not exert fiscal spillovers on neighbors. Potential remaining channels for spillovers are migration and firm relocation. Since offshore oil fields are serviced remotely from a few major hubs, most treated municipalities only feel the effects of offshore production through royalty transfers, limiting likely firm-level effects to sectors that contract with municipal governments (e.g., construction). In Appendix C18, I implement the spillover-robust difference-in-differences strategy proposed in [Clarke \(2017\)](#) to explicitly analyze spatial spillovers from discovery announcements.

My preferred choice of control group ("Wells") reduces concerns over anticipatory effects since both treated municipalities and control municipalities in this group are experiencing relatively constant offshore oil exploration activity. The unpredictable timing of major discoveries means additional anticipation is unlikely. Moreover, I do not observe rapid changes in outcomes after discovery announcements.<sup>15</sup>

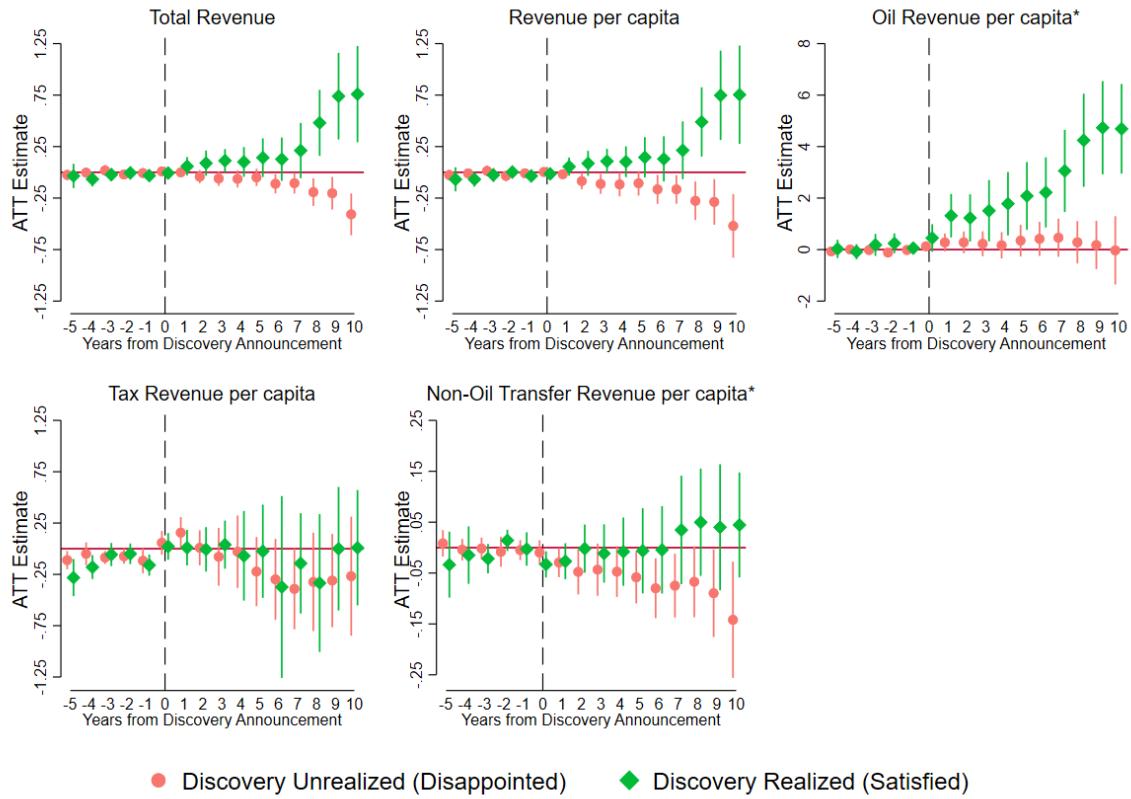
---

<sup>15</sup>To preserve reasonable sample sizes, I do not impose a balance requirement on treated units across relative time periods. As a result, panel composition changes slightly, with all treated municipalities present in the panel at time  $t=0$  and some dropping out in more extreme years. I focus my analysis from  $t=-5$  to  $t=10$  to allow verification of pre-trends and account for the typical delay between discovery announcement and peak production (which ranges from 5-10 years). In Appendix C17, I plot histograms of number of treated units over relative year indicators for both treated groups (Disappointed and Satisfied). I also plot treated sample means for key baseline characteristics over relative years to assess whether the composition of treated groups changes substantively across the panel. While the number of treated units declines slightly toward the extremes of the panel, mean baseline characteristics remain relatively stable.

## 7 Results

I focus first on standard municipal public finance outcomes. Figure 11 displays event study results for discovery effects on total and disaggregated municipal revenues. I plot estimates for Satisfied and Disappointed treated groups on the same graph, but each is estimated separately relative to the Wells control group. For all outcomes, I fully saturate relative time indicators in estimation, but plot periods  $t-5$  to  $t+10$  from the first discovery event. In Appendix ??, I present tables of coefficient estimates and standard errors, sample sizes, and sample characteristics.

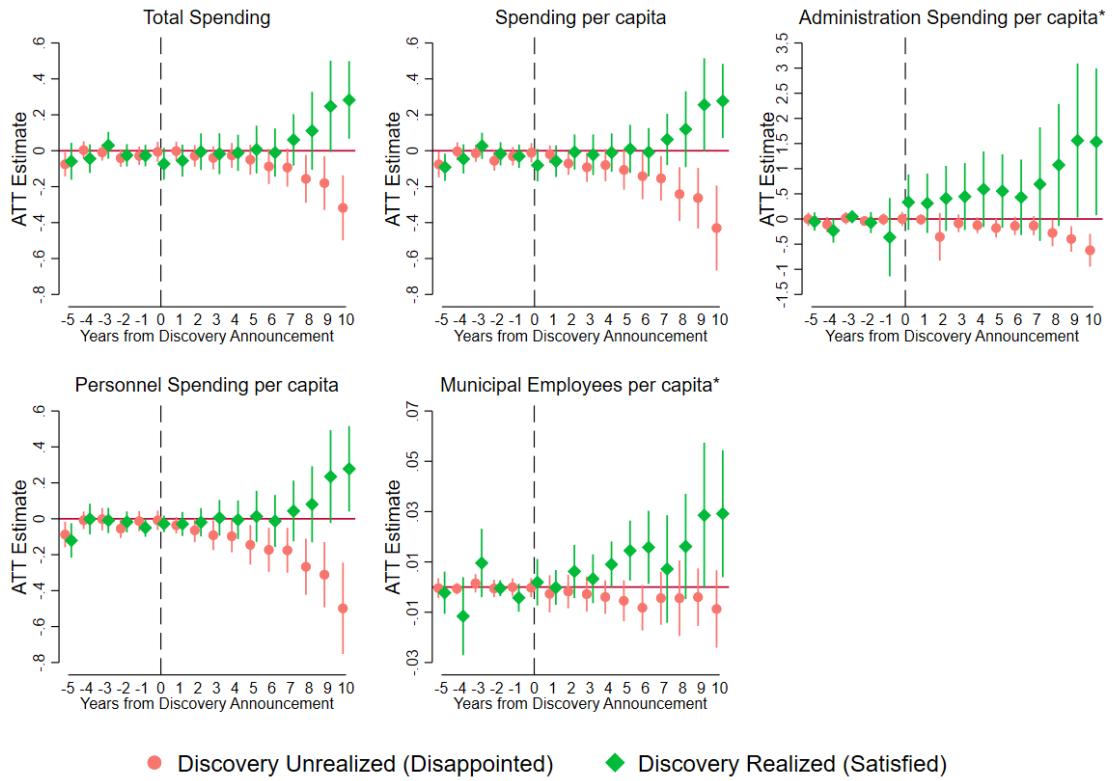
Figure 11: Revenues



Note: Event studies are estimated separately for Disappointed and Satisfied municipalities relative to never-treated controls (municipalities with exploratory offshore wells between 2000-2017 but no discovery announcements), and superimposed on the same graph for visual comparison. Event study specifications include municipal and year fixed effects and are estimated using Callaway and Sant'Anna (2020) *csdid* estimator. Continuous outcomes are transformed using inverse hyperbolic sine transformation. Monetary values are deflated to constant 2010 BRL. Standard errors are clustered at the municipality level and 95% confidence intervals are reported. Revenue variables refer to current (realized) rather than budgeted values. Disappointed municipalities are those that experienced a major offshore oil or gas discovery announcement between 2000-2017, but received less than 40% of forecast revenues from that discovery by 2017. Satisfied municipalities experienced a major discovery announcement and received more than 40% of forecast revenues by 2017. \* Asterisks indicate that a different y-axis scale is used from the rest of the sub-figures, in order to accommodate large differences in scale of effects.

As evidenced in Figure 11, oil revenues increase within one year of a discovery announcement in municipalities that will ultimately be classified as “Satisfied” (e.g., have their discovery realized). After ten years, discoveries in satisfied municipalities increase per capita oil revenues by 5,442% relative to never-treated controls. “Disappointed” municipalities never experience an increase in oil revenues, suggesting that indications of a place’s ultimate discovery realization begin to emerge relatively soon after a discovery announcement. Disappointing discoveries lead to 26.7% lower per capita revenues in affected communities after 10 years. Non-oil transfer revenues from state and federal governments decline by 9% in disappointed municipalities and remain unchanged in satisfied municipalities.<sup>16</sup> Tax revenues are noisily estimated, but trend downward in both satisfied and disappointed places after discoveries.

Figure 12: Expenditures and Employment



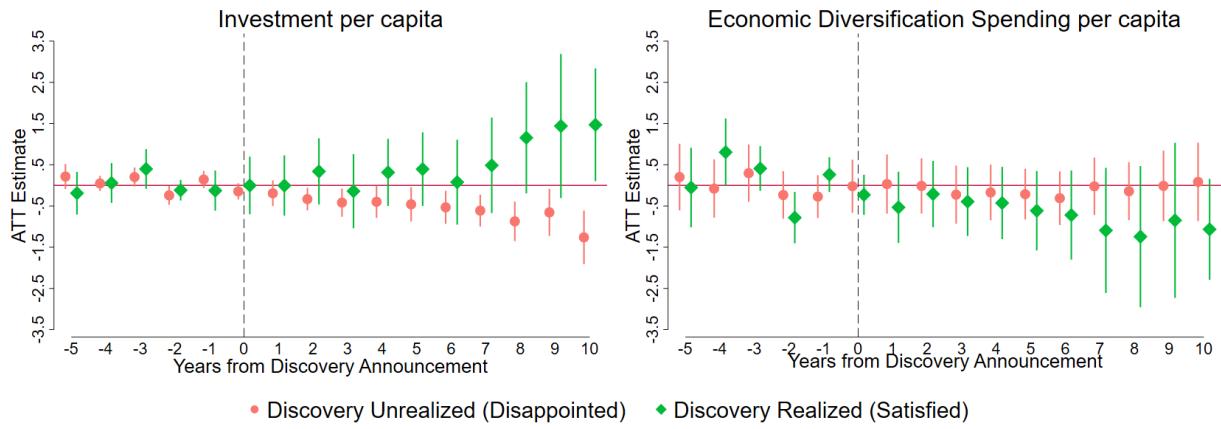
Note: Event study specifications include municipal and year fixed effects and are estimated using [Callaway and Sant'Anna \(2020\)](#) *csdid* estimator. Continuous outcomes are transformed using inverse hyperbolic sine transformation. Monetary values are deflated to constant 2010 BRL. Standard errors are clustered at the municipality level and 95% confidence intervals are reported. Spending variables refer to current (realized) spending. Disappointed municipalities experienced a major offshore oil or gas discovery announcement between 2000-2017, but received less than 40% of forecast revenues by 2017. Satisfied municipalities experienced a major discovery announcement and received more than 40% of forecast revenues by 2017. \* Asterisks indicate that a different y-axis scale is used from the rest of the sub-figures, in order to accommodate large differences in scale of effects.

<sup>16</sup>Transfers follow formulaic rules that do not change during the study period. I report estimates of discovery effects on each type of state/federal transfer in Appendix A7. In disappointed municipalities, transfers pegged to population (FPM), students (FUNDEF/FUNDEB), and exports (Lei Kandir) decline significantly beginning two years after discoveries.

Changes in revenue translate closely into changes in spending (Figure 12). In satisfied municipalities, per capita spending increases significantly beginning nine years after the first major discovery announcement (+20.8% ten years on), aligning with the typical delay period between discovery and peak offshore oil and natural gas production. In contrast, per capita spending declines significantly in disappointed municipalities beginning three years after a discovery (-24% ten years on). Spending on personnel falls by 26.4% in disappointed places after ten years, and increases by 14.3% in satisfied places. Finally, the number of municipal government employees per capita remains unchanged in disappointed places, and increases significantly in satisfied places beginning five years after a discovery announcement.

Public investment (e.g., in infrastructure) is a primary function of municipal government and a long-term determinant of growth potential. In satisfied municipalities, public investment trends upwards after discoveries (+jjj% ten years on). Investment in disappointed municipalities is reduced by 43.1% relative to controls after 5 years, and by 56.9% after 10 years. Significant reductions in investment begin just two years after a discovery announcement, and may compromise long-term growth prospects for disappointed municipalities.

Figure 13: Public Investment and Economic Diversification



Note: Investment refers to public municipal investment (e.g., infrastructure). Economic development spending is the sum of spending to promote industry, services, and agriculture. Event study specifications include municipal and year fixed effects and are estimated using Callaway and Sant'Anna (2020) *csdid* estimator. Continuous outcomes are transformed using inverse hyperbolic sine transformation. Monetary values are deflated to constant 2010 BRL. Standard errors are clustered at the municipality level and 95% confidence intervals are reported. Disappointed municipalities experienced a major offshore oil or gas discovery announcement between 2000-2017, but received less than 40% of forecast revenues by 2017. Satisfied municipalities experienced a major discovery announcement and received more than 40% of forecast revenues by 2017.

To explore municipal efforts to promote non-extractive economic sectors, I sum expenditures on promotion of industry, agriculture, and services (i.e., “economic diversification spending”). While estimates

of this outcome are noisy due to incomplete reporting in administrative records, Figure 13 suggests that satisfied municipalities never increase spending on economic development of non-extractive sectors, despite increasing spending in nearly every other category.

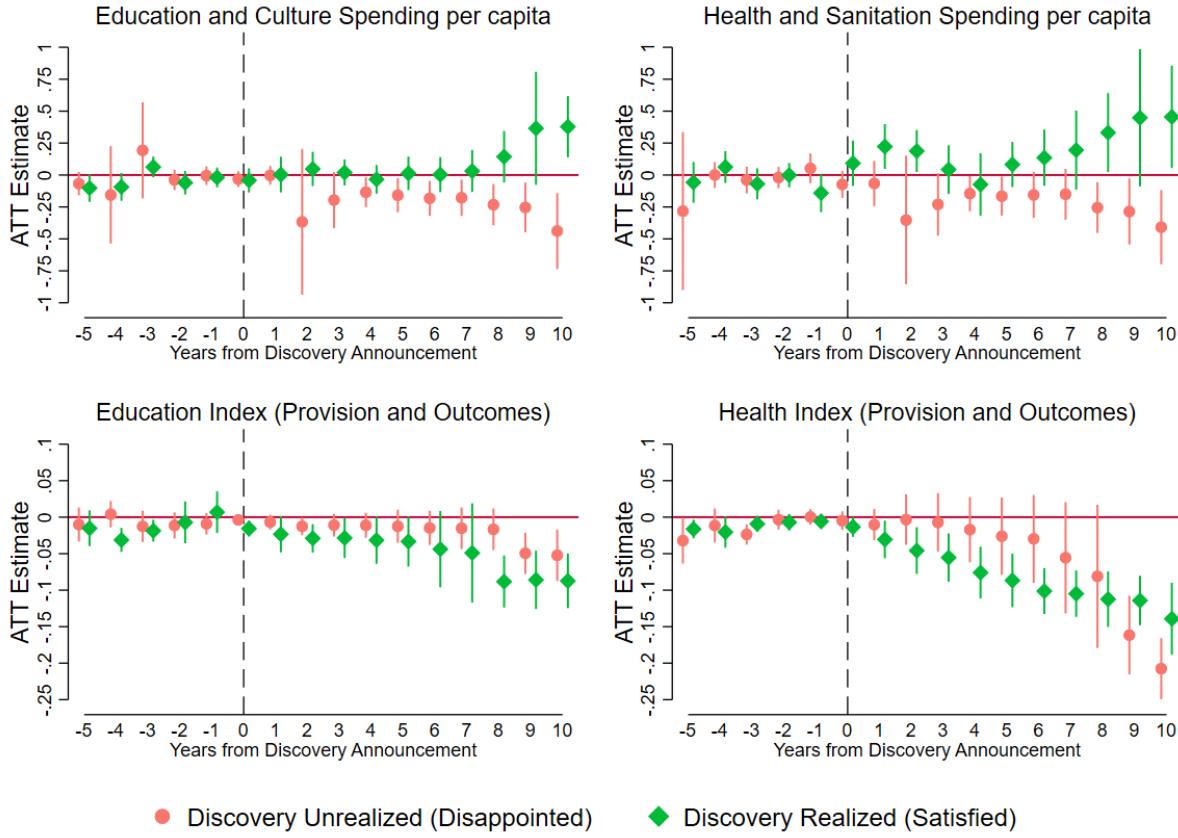
To assess the state of “fiscal health” in discovery-affected municipalities, I compute measures proposed by [FIRJAN \(2019\)](#), a Brazilian industry association, and report estimation results in Appendix A8.<sup>17</sup> In satisfied municipalities, budget share going to investment rises while the share going to personnel falls sharply approximately 8 years after the discovery announcement. For disappointed communities, investment shares fall and personnel shares remain stable, suggesting governments in these places pare back longer-term investments and focus on covering fixed costs, such as public salaries and benefits. The tax share of revenues rises for disappointed municipalities, in part mechanically (other revenues are falling), but also, potentially, as governments raise taxes in response to a fiscal crunch caused by disappointed expectations. There are no significant effects of discovery announcements on the share of municipal budgets going to debt management. In Appendix A9, I present event study results for additional measures of debt and again find no significant effects. This result is unsurprising, since municipal governments in Brazil have limited capacity to issue debt.

Provision of public goods is an integral part of municipalities’ role in Brazil’s federal system. Municipalities are responsible for significant provision of education, health, infrastructure, public safety, and other goods and services. Figure 14 illustrates effects of discovery announcements on public goods spending (top row) and indices of real public goods provision and outcomes taken from the FIRJAN Municipal Development Index ([FIRJAN, 2019](#)). In satisfied municipalities, per capita spending on education and culture increases by 28% after 10 years, and spending on health and sanitation increases by 25.4%. Despite these significant increases, index measures of public goods provision and outcomes show significant declines for these communities. These results corroborate previous findings by [Caselli and Michaels \(2013\)](#), who document increased public goods spending and declining or stagnant public goods provision after royalty windfalls in Brazil. This disconnect between increased public goods spending and worsening public goods outcomes suggests that municipalities dealing with major oil booms may lack the capacity to spend windfalls efficiently, or may suffer leakage of revenue windfalls into corruption. Municipalities may also allocate health and education spending toward areas that are not directly measured in index outcomes, such as salaries.<sup>18</sup> Disappointed municipalities experience

<sup>17</sup>[FIRJAN \(2019\)](#) assess municipalities’ fiscal health along four dimensions: share of municipal revenues going to investment, personnel, and debt, and share of municipal revenues derived from local taxes. Higher investment shares and lower personnel shares are considered healthy. Low tax shares are suggestive of low fiscal “autonomy,” (i.e., a municipality’s ability to sustain its own budget without state or federal transfers).

<sup>18</sup>Brazilian municipalities are required by law to dedicate set percentages of their revenues to health and education spending. Thus, when major revenue windfalls arrive, municipalities may rush to increase spending without having a

Figure 14: Public Goods Spending & Performance Indices



Note: Education and Health Indices are drawn from the FIRJAN Municipal Development Index (FIRJAN, 2020), a comprehensive measure of municipal development published annually by FIRJAN, a nonprofit. The Education Index is an aggregate score ranging from 0-1, composed of the following indicators: early childhood enrollment rates, graduation rates, grade-age distortion, hours spent in class, share of teachers with college degrees, and IDEB test scores. The Health Index is an aggregate score ranging from 0-1, composed of the following indicators: proportion of pregnant women receiving  $>7$  pre-natal visits, deaths of undefined causes, and avoidable infant mortality. Event study specifications include municipal and year fixed effects and are estimated using Callaway and Sant'Anna (2020) *csdid* estimator. Continuous monetary outcomes are transformed using inverse hyperbolic sine transformation. Index outcomes are untransformed. Monetary values are deflated to constant 2010 BRL. Standard errors are clustered at the municipality level and 95% confidence intervals are reported. Disappointed municipalities experienced a major offshore oil or gas discovery announcement between 2000-2017, but received less than 40% of forecast revenues by 2017. Satisfied municipalities experienced a major discovery announcement and received more than 40% of forecast revenues by 2017.

declining per capita education (-25.6%) and health (-26.2%) spending after 10 years, and also show significantly declining indices of provision and outcomes. In Appendix A10, I present dynamic estimates of discovery effects on disaggregated measures of public goods provision, quality, and outcomes.

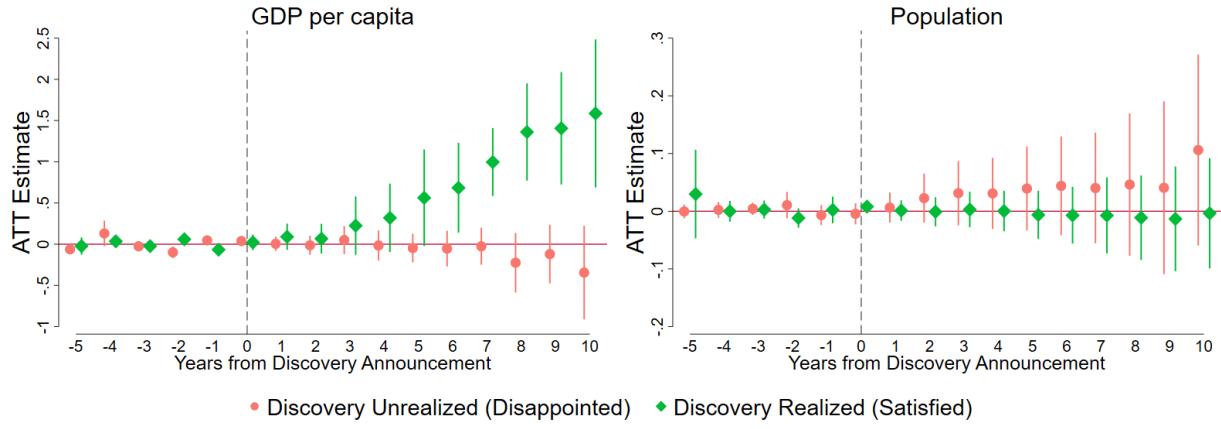
Realized discoveries have large positive effects on overall economic activity as measured by GDP per capita in satisfied municipalities (+253.1% ten years on). To note, oil and gas revenue transfers enter GDP in a direct accounting sense as part of the government budget. GDP trends downward but remains statistically indistinguishable from zero in disappointed municipalities. Population

---

well-developed investment or development plan in place, leading to waste and inefficiency.

trends upward after discoveries in disappointed places, but is not statistically distinguishable from zero. To test for the possibility of in-migration more explicitly, I draw on retrospective migration questions from the 2010 Demographic Census to measure annual rates of in-migration over the 2000-2010 period. I do not find significant increases in in-migration in either group (Appendix A11). However, as most major discoveries occurred in 2007 or later, I may miss important migration effects due to temporal limitations of this dataset.

Figure 15: GDP & Population



Note: Event study specifications include municipal and year fixed effects and are estimated using [Callaway and Sant'Anna \(2020\)](#) *csdid* estimator. GDP per capita is transformed using the inverse hyperbolic sine transformation. Index outcomes are untransformed. Monetary values are deflated to constant 2010 BRL. Standard errors are clustered at the municipality level and 95% confidence intervals are reported. Disappointed municipalities experienced a major offshore oil or gas discovery announcement between 2000-2017, but received less than 40% of forecast revenues by 2017. Satisfied municipalities experienced a major discovery announcement and received more than 40% of forecast revenues by 2017.

## 7.1 Robustness to Alternative Estimators, Controls, and Forecasts

To assess sensitivity of event study estimates to choice of estimator and control group, I re-estimate specifications using control groups matched on baseline characteristics and the two-way fixed effects estimator. Results are largely stable in sign, magnitude, and significance across different combinations of control group (Wells and Matched) and estimator (CS and TWFE), as reported in Appendix ??.

The revenue forecasting model used to group discovery-treated municipalities into disappointed and satisfied treatment arms involves several assumptions of model parameters and functional forms. Altering these parameters leads to slightly different groupings of municipalities into disappointed and satisfied. Likewise, coarsened exact matching is subject to researcher decisions regarding which variables to include in the matching algorithm, and which cutoffs to define. To test the robustness of event studies to model selection, I re-estimate specifications for key outcomes (per capita revenues, expenditure, investment, and GDP, and index scores of education and health provision and quality) using a variety of alternative forecasting and matching parameters. These variations include medium

and high revenue expectation forecasts (shifting marginal municipalities from satisfied to disappointed categories), and a non-parametric definition of satisfied/disappointment that simply measures whether oil equivalent production increased between the discovery announcement and 2017 by more than a factor of 2. I also construct alternative matched control samples by matching on baseline per capita revenues and spending, rather than municipality characteristics. Finally, I re-estimate event studies using the full sample of municipalities in coastal states. I report results from these robustness exercises in Appendix C1. Results are stable across alternative specifications.

## 7.2 Political Selection Mechanism: Discovery Effects on Local Elections

Did changes in local governance drive municipal responses to oil discoveries? What explains disappointed places' negative outcomes and satisfied places' inability to convert windfalls into improved public goods provision? In the following section I explore a number of mechanisms, beginning with electoral responses to discovery announcements.

In Table 7, I present results from difference-in-difference specifications that regress outcomes on municipality and year fixed effects and a treatment indicator that (i) turns on after the first discovery announcement (in the case of the [Callaway and Sant'Anna \(2020\)](#) (CS) estimator), or (ii) turns on when a discovery announcement occurred in the four years leading up to a local election (in the case of the TWFE estimator). Each column of Table 7 presents results from a different combination of control sample (Wells and Pre-Matched) and estimator (CS and TWFE) to assess robustness.

Results indicate that major discovery announcements significantly increase the number of competitive candidates running for council, and significantly decrease the number of competitive candidates running for mayor<sup>19</sup>. Discoveries significantly increase the number and value of donations made to municipal candidates. Finally, discoveries appear to induce less educated candidates to run for election. Given that [Baragwanath \(2020\)](#) documents strategic entry of corrupt candidates in response to oil royalty windfalls in this context, this evidence may suggest that individuals with lower private sector opportunity costs may run for office in expectation of future oil rents. In Appendix B9, I report results from difference-in-difference estimates of the effects of a discovery announcement on winning candidate characteristics and measures of public employment patronage, defined as the number and share of campaign donors hired to discretionary municipal public jobs (*cargos comissionados*) after their candi-

---

<sup>19</sup>To compute the number of competitive candidates, I adopt a methodology from [Niemi and Hsieh \(2002\)](#). For candidate  $i$  in election  $e$ , let  $v_{ie}$  be the number of votes received. Then let  $\sum_i v_{ie}$  be total votes cast for municipal council, and  $\theta_{ie}$  be the share of total council votes received by  $i$ . Let  $S_m$  be the number of council seats in municipality  $m$ . Consider a candidate to be competitive if  $\theta_{ie} > (1/(1+S_m))/8$ . For example, in a municipality with 10 council seats, a candidate must receive over 1.14% of total votes to be competitive. For mayors, I simply consider candidates to be competitive if they receive more than 10% of total votes.

date wins a local election. Results indicate that discovery announcements reduce the average schooling levels of elected candidates, which in turn could reduce governing capacity and the quality of public service delivery. This may help explain the difficulties of satisfied municipalities in translating revenue windfalls into improved public goods provision, and the difficulties of disappointed municipalities in adjusting to revenue shortfalls. Announcements have no detectable effects on the intensity of patronage.

Table 5: Discovery Effects on Electoral Competition, Fundraising, and Candidate Characteristics

	CS Wells	CS Matched	TWFE Wells	TWFE Matched
<b>Electoral Competition</b>				
<i>Council Candidates (Total)</i>	0.172 (0.235)	0.070* (0.037)	0.131 (0.122)	0.046 (0.032)
<i>Council Candidates (Competitive)</i>	0.098* (0.105)	0.066* (0.037)	0.070 (0.061)	0.061 (0.034)
<i>Mayoral Candidates (Total)</i>	0.065 (0.068)	0.054 (0.050)	0.041 (0.052)	0.035 (0.048)
<i>Mayoral Candidates (Competitive)</i>	-0.129*** (0.045)	-0.087* (0.046)	0.001 (0.046)	0.008 (0.047)
<i>Competitive Council Cand. Per Seat</i>	0.068*** (0.025)	0.033 (0.022)	0.047** (0.019)	0.038** (0.018)
<b>Campaign Fundraising</b>				
<i>Total Number of Donations</i>	0.157* (0.092)	0.164** (0.069)	0.169* (0.087)	0.149 (0.091)
<i>Total Value of Donations</i>	0.238** (0.120)	0.114* (0.113)	0.131* (0.078)	0.119 (0.083)
<b>Candidate Characteristics</b>				
<i>Share of Candidates Female</i>	-0.010 (0.010)	-0.006 (0.120)	-0.008*** (0.007)	-0.016 (0.005)
<i>Avg. Candidate Age</i>	-0.031** (0.014)	0.000 (0.011)	0.001 (0.005)	-0.002 (0.004)
<i>Avg. Candidate Schooling</i>	-0.031** (0.014)	-0.009 (0.010)	-0.030*** (0.009)	-0.024*** (0.006)
Municipality FEs	Y	Y	Y	Y
Election Period FEs	Y	Y	Y	Y
n (municipality-election periods)	404	3,745	404	3,745

Table reports results from estimation of the following difference-in-differences specification:  $Y_{me} = \delta_m + \lambda_e + \beta T_{me} + \epsilon_{me}$ , where  $Y_{me}$  are outcomes measuring dimensions of municipal electoral competition, fundraising, or candidate characteristics,  $\delta_m$  and  $\lambda_e$  are municipality and election period FEs, and  $T_{me}$  is a binary treatment dummy that takes a value of 1 after a major offshore oil or gas discovery was announced during the previous four-year election period in a municipality  $m$ 's offshore catchment zone. To compute the number of competitive candidates, let  $v_{ie}$  be the number of votes received by candidate  $i$  in election period  $e$ . Then let  $\sum_i v_{ie}$  be total votes cast for municipal council, and  $\theta_{ie}$  be the share of total council votes received by  $i$ . Let  $S_m$  be the number of council seats in municipality  $m$ . Consider a candidate to be competitive if  $\theta_{ie} > (1/(1+S_m))/8$ . Standard errors are clustered at the municipality level in all specifications. Each column reports coefficient estimates and standard errors for a specific control group-estimator pair. Column 1 reports the preferred specification, which uses the Callaway and Sant'Anna (2020) (CS) *cslid* estimator with municipalities that had offshore exploratory wells drilled since 2000, but no discoveries, as a control group. Column 2 reports results using CS estimator and a control group matched with discovery-treated municipalities on baseline characteristics using Coarsened Exact Matching. Columns 3 and 4 report results from wells and matched control groups using the two-way fixed effects (TWFE) OLS estimator. Monetary values are deflated to constant 2010 BRL. Continuous variables are transformed using the inverse hyperbolic sine transformation. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

### 7.3 Political Turnover Mechanism: Discovery Effects on Incumbent Reelection

When there is a shortfall between discovery expectations and realized royalties, are incumbent politicians punished with reduced reelection rates? [Monteiro and Ferraz \(2010\)](#) find that municipal politicians in Brazil are rewarded electorally for early revenue windfalls. Since voters cannot perfectly observe politicians' quality or honesty, they may vote according to observable performance, such as public goods provision or hiring. In this case, disappointment could result in a fiscal crunch, requiring local leaders to cut spending or raise taxes. Voters may also interpret the lack of revenue windfall after promising discovery announcements as an indicator of corruption and punish incumbent politicians accordingly. Incumbent politicians may exacerbate these dynamics by stoking euphoria or making promises after discovery announcements, which prove impossible to keep when discovery expectations are disappointed.

Table 8 reports results from regressions of probability of reelection on a time-varying measure of disappointment over the four years leading up to an election. I estimate logit (reporting marginal effects) and linear probability models, controlling for candidates' age, sex, and schooling and state and year fixed effects. Findings suggest council incumbents are less likely to win reelection when their municipality was disappointed by discovery expectations over the last four years. Mayors are weakly likely to win reelection. Satisfaction appears to have insignificant effects on reelection rates. [Akhtari et al. \(2021\)](#) and [Toral \(2021\)](#) show that political turnover at the municipal level in Brazil leads to administrative disruptions and reduced provision and quality of public goods and services. Disappointment, by decreasing reelection rates for incumbents, may increase turnover and associated disruptions, making it more difficult for municipal governments to adjust to the challenges created by oil revenue shortfalls.

### 7.4 Why Are Disappointed Municipalities Left Worse Off?

Results from event studies indicate that disappointed municipalities did not significantly increase spending or borrowing immediately following discovery announcements. Further, their oil revenues remain largely unchanged over time. Nonetheless, total and per capita revenues fall in disappointed places, leading to cuts in spending and investment and worsening measures of fiscal health and public goods provision ten years after discoveries. What explains these negative outcomes?

Previous sections revealed political reactions to discovery announcements that may contribute to negative outcomes in disappointed municipalities. Discoveries cause lower-education individuals to run for and win municipal office. These individuals may have lower private-sector opportunity costs and

Table 6: Effects of Disappointment or Satisfaction with Oil Discoveries on Incumbent Reelection Rates

	Disappointed		Satisfied	
	LPM	Logit	LPM	Logit
<i>Mayor</i>	-0.119*	(0.070)	-0.136	(0.089)
Controls	Y	Y	Y	Y
State FE <sub>s</sub>	Y	Y	Y	Y
Election Period FE <sub>s</sub>	Y	Y	Y	Y
n (candidate-election periods)	10,815	10,815	10,850	10,850
<i>Council</i>	-0.052***	(0.017)	-0.042***	(0.016)
Controls	Y	Y	Y	Y
State FE <sub>s</sub>	Y	Y	Y	Y
Election Period FE <sub>s</sub>	Y	Y	Y	Y
n (candidate-election periods)	160,169	160,169	160,945	160,945

Table reports coefficient estimates (marginal effects for logit models) with standard errors in parentheses for specification:  $P(\text{Reelection}_{cme} = 1) = \delta_s + \lambda_e + \beta \text{Disappointed}_{me} + X'\mu + \epsilon_{cme}$ . Standard errors are clustered at the municipality level in all specifications.  $\text{Disappointed}_{me}$  is a binary indicator that takes a value of 1 when the ratio of realized oil revenue growth over the previous election period over expected oil revenue growth over that period < 0.4. Alternatively,  $\text{Satisfied}_{me}$  is a binary indicator that takes a value of 1 when the ratio of realized to expected revenue growth > 0.8. These specifications compare municipalities with substantive levels of discovery disappointment/satisfaction with control municipalities consisting of all untreated municipalities in coastal states. State and election period fixed effects are included, and controls include candidate age, sex, and education level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

potentially greater interest in rent-seeking ([Caselli and Morelli, 2004](#)). [Baragwanath \(2020\)](#) shows oil revenues increase entry of corrupt candidates. Increased political turnover in disappointed places may disrupt public service delivery ([Akhtari et al., 2021](#)).

Private sector activities may also relocate away from disappointed places. In particular, sectors that are dependent on public spending and contracts, such as construction, may be especially likely to relocate as disappointment becomes evident. Indeed, formal employment in construction falls by 25% in disappointed places ten years after a discovery. Brazilian municipalities collect taxes on real estate transactions (ITBI), service providers (ISS), and urban properties (IPTU) ([Egestor, 2020](#)). A declining construction sector—closely tied to real estate and property markets—may thus weaken the local tax base. Tax revenues trend downward in disappointed places after discovery announcements, falling by 37% ten years.

Formulaic transfer revenues from federal and state governments also decline significantly in disappointed municipalities (-9% ten years on). These transfers make up 75-95% of most municipal budgets. Transfer formulas did not change during the study period. I disaggregate transfer revenues in Appendix A7 to show that disappointed municipalities suffer significant cuts in the following transfers: FUNDEF/FUNDEB (which funds primary and secondary schools and is calculated on a per-student basis), FPM (calculated based on municipal population), and Lei Kandir (calculated based on value added

in goods and services for export). Collectively, these transfers account for 92.5% of total transfers to municipalities. Speculatively, declines in education transfers (FUNDEF/FUNDEB) could be part of a vicious cycle in which budget cuts to municipal education lead to poorer outcomes and lower student enrollment, which in turn results in lower education transfers (calculated on a per student basis), leading to further cuts, worsening conditions, and dropout. Per capita declines in population-based transfers may occur as population trends upwards in disappointed places after discoveries (though never statistically significantly), pushing municipalities into higher population brackets with lower per capita transfer coefficients ([Abrucio and Franzese, 2010](#)). A better understanding of what drives changes in transfers to disappointed municipalities is an area for future inquiry.

## 8 Discussion and Policy Implications

Existing literature on resource discoveries and the “Presource Curse” has documented widespread disappointment, fiscal dysfunction, and corruption after major discoveries in a number of African countries ([Mihalyi and Scurfield, 2020](#)). I contribute to this literature with evidence of dynamic discovery effects in Brazil, a resource-rich, middle-income country. Moving to the subnational level and exploiting a quasi-experiment created by Brazil’s formulaic royalty rules and exogenous offshore discoveries allows me to conduct credible causal inference. Building a rich 18-year municipality-level panel enables me to dig into the details of local governance, including public finances, elections, and public goods provision.

I compile an original geolocated dataset of 179 major offshore oil and gas discoveries announced in Brazil between 2000-2017. I next reconstruct Brazil’s offshore catchment zone projections, map each discovery back to aligned municipalities, and then build a forecasting model of offshore production and royalty distribution to measure the gap between each municipality’s expected and realized revenues after discovery announcements. I find that 18 of the 48 municipalities affected by offshore discovery announcements during this period ultimately received more than 50% of the revenues they could have expected. Municipalities do not exhibit rapid anticipatory fiscal responses to discovery announcements, likely due to limits imposed by a fiscal responsibility law and credit constraints. This finding contrasts with [Mihalyi and Scurfield \(2020\)](#), who document the worsening of debt sustainability in 9 out of 12 African countries after major oil discoveries. Institutions such as Brazil’s Fiscal Responsibility Law may play an important role in controlling anticipatory excesses after major discoveries.

Municipalities where discovery expectations were satisfied enjoy large increases in revenue, spending, and GDP per capita 10 years on from the first discovery announcement, but experience worsening or stagnant public goods provision and fail to invest in economic diversification. Disappointed munic-

ipalities experience lower revenues, spending, and investment, as well as worsened indicators of fiscal health and public goods provision. Event study results are robust to alternative control groups, forecasting specifications, and estimators. I implement [Callaway and Sant'Anna \(2020\)](#)'s *csdid* estimator to deal with staggered treatment timing and heterogeneous treatment effects across time and groups, avoiding well-known problems with standard two-way fixed effects approaches ([Goodman-Bacon, 2018](#)). I conclude that outcomes in disappointed municipalities provide circumscribed evidence of a Pre-resource Curse, in the sense that these places are left worse off by a resource discovery despite never receiving resource windfalls or direct extractive activities.

Political competition increases weakly after discovery announcements and the average schooling of candidates and winners declines. While increased political competition may bring out better candidates ([Galasso and Nannicini, 2011](#)), the lower schooling of candidates suggests discoveries may attract rent-seekers to office, as [Baragwanath \(2020\)](#) finds that royalty receipts do. Reduced education levels of elected politicians may reduce governing capacity in discovery-affected places, which could prove particularly problematic in disappointed places facing shortfalls. Incumbent politicians' reelection rates are reduced when municipalities are disappointed at the time of an election, suggesting voters punish incumbents for negative outcomes. Increased political turnover in disappointed places may disrupt administration and public service delivery ([Akhtari et al., 2021](#)).

By focusing on subnational units, my study highlights emergent properties that may not be apparent at the national level. First, disappointed municipalities in Brazil may experience in-migration after discovery announcements, though migration data from the 2020 Demographic Census is not yet available to explore these dynamics in detail. Major resource discoveries affecting only certain parts of a country could provoke migration in anticipation of future windfalls. Resource revenue sharing rules that further concentrate the effects of discoveries in certain places could contribute to these effects. Second, local governments have fundamentally different policy options than do national governments ([Agrawal et al., 2020](#)). Local governments often face borrowing constraints, potentially reducing issues related to debt relative to the national level, where governments may seek to borrow against future resource wealth. On the other hand, local governments may be more susceptible to elite capture and rent-seeking ([Bardhan and Mookherjee, 2000](#)). Third, local economies may not vary along macroeconomic dimensions that affect the entire country, including real exchange rate effects that are a primary mechanism underlying Dutch Disease. Looking across countries, [Harding et al. \(2016\)](#) document significant increases in real exchange rates following giant oil discoveries, driven almost entirely by increases in the price of nontradable goods.

Moving to the country level, interpreting causal effects of the Pre-Salt oil discoveries becomes much

more challenging. Brazil has a large, diversified economy, and oil rents made up only 2.07% of Brazil's GDP in 2018 ([World Bank, 2020](#)). Corroborating findings by [Toews and Vézina \(2020\)](#), I find that Brazil experienced a boom in FDI inflows following major Pre-Salt discoveries (see Appendix A3). Foreign direct investments were likely concentrated in the oil sector. [Magalhães and Domingues \(2014\)](#) compute a dynamic global generalized equilibrium model to analyze impacts of Pre-Salt discoveries on Brazil's economic structure. Their simulations indicate that discoveries increased investment, exports, and growth in industrial sectors tied to oil and gas (e.g. construction, refining, petroleum products) and reduced investment, exports, and growth in unrelated industrial sectors, thus leading to increased dependence on commodities.

National and international factors contributed to disappointment of optimistic oil production forecasts in Brazil. Within the country, a regulatory overhaul undertaken after the initial Pre-Salt discoveries in 2007 may have delayed and constrained production ([Florêncio, 2016](#)). The outbreak of a corruption scandal linked to Petrobras in 2014 led to slashed investment by the oil company. The crash in world oil prices in the same year reduced the commercial viability of ultra-deep fields, and the rise of US fracking from 2009 onwards drew international capital away from enormous fixed investments in Brazilian offshore. This disappointment may have contributed to Brazil's lost decade following the Pre-Salt boom of the late 2000s and early 2010s. Per capita GDP growth fell from an annual average of 2.92% between 2005-2012 to -0.75% between 2013-2019 ([World Bank, 2020](#)). Leaders' claims after major discoveries that these would be a "passport to the future" and a "winning lottery ticket" that would pay for everything, have undoubtedly been disappointed in the medium term.

My study highlights a number of policy implications. First, revenue allocation rules that concentrate the positive and negative effects of resource discoveries into specific regions may amplify uncertainty and volatility after discoveries. For discovery-affected municipalities, anticipation of future revenue windfalls may lead to political rent-seeking and efforts to subvert fiscal responsibility rules. In places where discovery expectations are realized, booming revenues strain local government capacity, as evidenced by the disconnect between increased public goods spending and stagnating public goods outcomes. Along these lines, [Borge et al. \(2015\)](#) find that exogenous resource windfalls derived from hydropower in Norway reduce the efficiency of public goods provision. Likewise, negative effects of disappointed discoveries may have been avoided if revenues were more evenly distributed throughout the country. In a large country such as Brazil, which experienced many discoveries, spreading impacts across geographical units would smooth over heterogeneous outcomes in individual fields, dilute disappointment, and avoid overloading local governments with limited administrative capacity. Smaller or less-diversified countries may be less able to smooth outcomes across many discoveries and locations.

Second, my study highlights the importance of institutions and governance, particularly in commodity sectors. Brazil's fiscal responsibility law may have helped to avoid municipal fiscal excesses following discovery announcements. Oil sector regulators should outline rules for discovery announcements, ensuring they are accurate and reflect realistic development prospects. Finally, leaders should manage expectations after oil discoveries at both national and local levels. There are often political incentives to generate euphoria and claim credit after major discovery announcements. Yet leaders should treat discoveries with caution, given that exogenous negative realizations may reduce reelection rates for incumbents. National leaders should actively communicate with local leaders in discovery-affected regions to transmit good practices and support capacity-building in preparation for coming booms or busts.

My findings reveal inefficient windfall spending in satisfied places and adjustment costs in disappointed places as two faces of the resource curse. Dramatic divergence in outcomes between these two groups highlights the importance of accounting for heterogeneity in discovery outcomes when analyzing the effects of natural resource sectors on governance and economic development.

## References

- Abrucio, F. L. and Franzese, C. (2010). Federalismo e Politicas Publicas: o Impacto das Relacoes Intergovernamentais no Brasil. *Fundacao Getulio Vargas*.
- Agrawal, D., Hoyt, W., and Wilson, J. (2020). Local Policy Choice: Theory and Empirics. *Journal of Economic Literature*, Forthcomin.
- Agüero, J. M., Balcázar, C. F., Maldonado, S., and Ñopo, H. (2021). The value of redistribution: Natural resources and the formation of human capital under weak institutions. *Journal of Development Economics*, 148(October 2020).
- Akhtari, M., Trucco, L., Abadie, A., Bitler, M., Cagé, J., Caramp, N., Chetty, R., Costin, C., Bo, E. D., Fabregas, R., Ferraz, C., Finan, F., Fryer, R., Ganimian, A., Ganong, P., and Glaeser, E. (2021). Political Turnover , Bureaucratic Turnover , and the Quality of Public Services \*. *American Economic Review*, Forthcomin.
- Alexeev, M. and Conrad, R. (2009). The Elusive Curse of Oil,. *Review of Economics and Statistics*, 91(3):586–598.
- ANP (2001). Guia dos Royalties do Petroleo.
- Ardanaz, M. and Tolsa Caballero, N. (2016). A subnational resource curse ? Revenue Windfalls and the Quality of Public Spending in Colombian municipalities. *V Jornadas Iberoamericanas de Financiación Local*, pages 0–30.
- Armand, A., Coutts, A., Vicente, P. C., and Vilela, I. (2020). Does information break the political resource curse? Experimental evidence from mozambique. *American Economic Review*, 110(11):3431–3453.
- Baragwanath, K. (2020). The Effect of Oil Windfalls on Corruption : Evidence from Brazil. *Unpublished Job Market Paper*, pages 1–79.
- Bardhan, P. and Mookherjee, D. (2000). Capture and governance at local and national levels. *American Economic Review*, 90(2):135–139.
- Batista, H. G. (2008). Lula defende que país tenha responsabilidade no uso dos recursos do pré-sal.
- Bellemare, M. F. and Wichman, C. J. (2020). Elasticities and the Inverse Hyperbolic Sine Transformation. *Oxford Bulletin of Economics and Statistics*, 82(1):50–61.

- Berman, N., Couttenier, M., Rohner, D., and Thoenig, M. (2017). This mine is mine! How minerals fuel conflicts in Africa. *American Economic Review*, 107(6):1564–1610.
- Bhavnani, R. and Lupu, N. (2012). Oil Windfalls and the Political Resource Curse: Evidence from a Natural Experiment in Brazil. *Fiscal Studies*, 31(3):373–404.
- Borge, L. E., Parmer, P., and Torvik, R. (2015). Local natural resource curse? *Journal of Public Economics*, 131:101–114.
- Borusyak, K. and Jaravel, X. (2017). Revisiting Event Study Designs , with an Application to the Estimation of the Marginal Propensity to Consume . pages 1–25.
- Brollo, F., Nannicini, T., Perotti, R., and Tabellini, G. (2013). The political resource curse. *American Economic Review*, 103(5):1759–1796.
- Callaway, B. and Sant'Anna, P. H. C. (2020). Difference-in-Differences with Multiple Time Periods. *Journal of Econometrics*, In Press.
- Caselli, F. and Cunningham, T. (2009). Leader behaviour and the natural resource curse. *Oxford Economic Papers*, 61(4):628–650.
- Caselli, F. and Michaels, G. (2013). Do oil windfalls improve living standards? Evidence from brazil. *American Economic Journal: Applied Economics*, 5(1):208–238.
- Caselli, F. and Morelli, M. (2004). Bad politicians. *Journal of Public Economics*, 88(3-4):759–782.
- Cavalcanti, T., Da Mata, D., and Toscani, F. (2016). Winning the Oil Lottery: The Impact of Natural Resource Extraction on Growth; IMF Working Paper.
- Clarke, D. (2017). Munich Personal RePEc Archive Estimating Difference-in-Differences in the Presence of Spillovers Estimating Difference-in-Differences in the Presence of. (81604).
- Colonnelly, E., Prem, M., and Teso, E. (2019). Patronage and Selection in Public Sector Organizations. *American Economic Review forthcoming*, (March 2017).
- Corden, W. M. and Neary, J. P. (1982). Booming Sector and De-Industrialisation in a Small Open Economy. *The Economic Journal*, 92(368):825–848.
- Cust, J., Harding, T., and Vézina, P.-L. (2019). Dutch Disease Resistance: Evidence from Indonesian Firms. *Journal of the Association of Environmental and Resource Economists*, 6(6):1205–1237.

- Cust, J. and Mihalyi, D. (2017). Evidence for a Presource Curse? *Policy Research Working Paper*, 8140(July):1–32.
- Cust, J. and Poelhekke, S. (2015). The Local Economic Impacts of Natural Resource Extraction. *Annual Review of Resource Economics*, 7(1):251–268.
- Dahis, R. (2020). Cleaning the Relação Anual de Informações Sociais (RAIS) dataset, 1985-2018.
- de Chaisemartin, C. and D'Haultfoeuille, X. (2020). Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. *American Economic Review*, 110(9):2964–2996.
- Egestor (2020). Quais são os Impostos federais, estaduais e municipais?
- Fioravante, D. G., Pinheiro, M. M. S., and Vieira, R. d. S. (2006). Lei de Responsabilidade Fiscal e Finanças Públicas Municipais: Impactos sobre Despesas com Pessoal e Endividamento. *Instituto de Pesquisa Econômica Aplicada*, pages 1–31.
- FIRJAN (2019). Indice Firjan de Gestao Fiscal. Technical report.
- Florêncio, P. (2016). The Brazilian 2010 oil regulatory framework and its crowding-out investment effects. *Energy Policy*, 98:378–389.
- Galasso, V. and Nannicini, T. (2011). Competing on good politicians. *American Political Science Review*, 105(1):79–99.
- Geiger, J. (2019). The Biggest Oil and Gas Discoveries of 2019.
- Giuberti, A. C. (2017). Lei De Responsabilidade Fiscal : Efeitos Sobre O Gasto Com Pessoal Dos Municípios Brasileiros.
- Goodman-Bacon, A. (2018). Difference-in-Differences with Variation in Treatment Timing. *National Bureau of Economic Research*.
- Gutman, J. (2007). *Tributação e Outras Obrigações na Indústria do Petróleo*. Freitas Bastos Editora.
- Gylfason, T. (2001). Natural resources, education, and economic development. *European Economic Review*, 45(4-6):847–859.
- Harding, T., Stefanski, R., and Toews, G. (2016). Boom Goes the Price: Giant resource discoveries and real exchange rate appreciation. *Oxford Center for the Analysis of Resource Rich Economies*.

- Höök, M., Davidsson, S., Johansson, S., and Tang, X. (2014). Decline and depletion rates of oil production: A comprehensive investigation. *Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences*, 372(2006).
- Iacus, S. M., King, G., and Porro, G. (2012). Causal inference without balance checking: Coarsened exact matching. *Political Analysis*, 20(1):1–24.
- James, A. (2015). US state fiscal policy and natural resources. *American Economic Journal: Economic Policy*, 7(3):238–257.
- Kennedy, P. (1981). Estimation with Correctly Interpreted Dummy Variables in Semilogarithmic Equations. *American Economic Review*, 71(4):4–5.
- Laurent, H. (2021). Corruption and politicians' horizon. *Economics of Governance*, 22(1):65–91.
- Lavareda, A. and Telles, H. (2016). *A lógica das eleições municipais*. Editora FGV.
- Magalhães, A. S. and Domingues, E. P. (2014). Blessing or curse: Impacts of the Brazilian Pre-Salt oil exploration. *EconomiaA*, 15(3):343–362.
- McGlade, C. and Ekins, P. (2015). The geographical distribution of fossil fuels unused when limiting global warming to 2°C. *Nature*, 517(7533):187–190.
- McKenzie, D. (2021). An Adversarial or “Long and Squiggly” Test of the Plausibility of Parallel Trends in Difference-in-Differences Analysis.
- Mehlum, H., Moene, K., and Torvik, R. (2006). Institutions and the resource curse. *Economic Journal*, 116(508):1–20.
- Mihalyi, D. (2020). The Long Road To First Oil. *SSRN Electronic Journal*, pages 1–44.
- Mihalyi, D. and Scurfield, T. (2020). How Africa’s prospective petroleum producers fell victim to the presource curse. *Extractive Industries and Society*.
- Monteiro, J. and Ferraz, C. (2010). Does Oil Make Leaders Unaccountable? Evidence from Brazil’s offshore oil boom. *Unpublished Working Paper - PUC-Rio*, pages 1–67.
- Moreno, F. (2013). Relembre a trajetória da OGX, da fundação à recuperação judicial.
- Niemi, R. G. and Hsieh, J. F. S. (2002). Counting candidates: An alternative to the effective N (with an application to the M + 1 rule in Japan). *Party Politics*, 8(1):75–99.

Nillesen, E. and Bulte, E. (2014). Natural resources and violent conflict. *Annual Review of Resource Economics*, 6(1):69–83.

Orair, R. O., Hamilton Matos dos Santos, C., de Jesus Silva, W., Mauricio de Mello Brito, J., Leal Silva, H., Silva Rocha, W., and dos Santos Ferreira, A. (2010). Uma Metodologia de Construcao de Serieis Temporais de Alta Frequencia das Financas Publicas Municipais no Brasil com Aplicacao para o IPTU e o ISS: 2004-2010. *IPEA Texto para Discussao*, 1632.

Pacheco, C. (2003). A Aplicacao e o Impacto dos Royalties do Petroleo no Desenvolvimento Economico dos Municipios Confrontantes da Bacia de Campos. *Undergraduate Thesis*.

Pelzl, P. and Poelhekke, S. (2021). Good mine, bad mine: Natural resource heterogeneity and Dutch disease in Indonesia. *Journal of International Economics*, 131:103457.

Piquet, R. and Serra, R. V. (2007). *Petróleo e região no Brasil: o desafio da abundância*.

Postali, F. A. S. (2015). Tax effort and oil royalties in the Brazilian municipalities. *Economia*, 16(3):395–405.

Robinson, J. A., Torvik, R., and Verdier, T. (2006). Political foundations of the resource curse. *Journal of Development Economics*, 79(2):447–468.

Sachs, J. D. and Warner, A. M. (2001). The curse of natural resources. *European Economic Review*, 45(4-6):827–838.

Sandler, D. H. and Sandler, R. (2014). Multiple event studies in public finance and labor economics: A simulation study with applications. *Journal of Economic and Social Measurement*, 39(1-2):31–57.

Serra, R. V. (2005). Contribuicoes para o debate acerca da reparticao dos royalties petroliferos no Brasil. *Doctoral Dissertation*, page 368.

Speight, J. (2014). *Handbook of Offshore Oil and Gas Operations*. Gulf Professional Publishing, 1st edition.

Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199.

Toews, G. and Vézina, P.-L. (2020). Resource Discoveries, FDI Bonanzas, and Local Multipliers: Evidence from Mozambique. *The Review of Economics and Statistics*, pages 1–36.

- Toral, G. (2021). Turnover: How electoral accountability disrupts the bureaucracy and service delivery.
- US Energy Administration (2015). Assumptions to the Annual Energy Outlook 2015 - Oil and Gas Supply Module. *U.S. Energy Information Administration*, (January):128–146.
- van der Ploeg, F. and Poelhekke, S. (2009). Volatility and the natural resource curse. *Oxford Economic Papers*, 61(4):727–760.
- Venables, A. J. (2016). Using natural resources for development: Why has it proven so difficult? *Journal of Economic Perspectives*, 30(1):161–184.
- Vezina, P.-L. (2020). The oil nouveau-riche and arms imports. *Working Paper*.
- Vicente, P. C. (2010). Does oil corrupt ? Evidence from a natural experiment in West Africa . *Journal of Development Economics*, 92(1):28–38.
- Welsby, D., Price, J., Pye, S., and Ekins, P. (2021). Unextractable fossil fuels in a 1.5 °C world. *Nature*, 597(7875):230–234.
- World Bank (2020). World Development Indicators.
- Wright, A., Fjelstad, O.-H., Jahari, C., Mmari, D., Hoem Sjursen, I., and Tungodden, B. (2016). Not so great expectations: Gas revenue, corruption and willingness to pay tax in Tanzania. *CMI Brief*, 15(4).
- Zhang, G., Qu, H., Chen, G., Zhao, C., Zhang, F., Yang, H., Zhao, Z., and Ma, M. (2019). Giant discoveries of oil and gas fields in global deepwaters in the past 40 years and the prospect of exploration. *Journal of Natural Gas Geoscience*, 4(1):1–28.

# Appendices

## Part

### Table of Contents

---

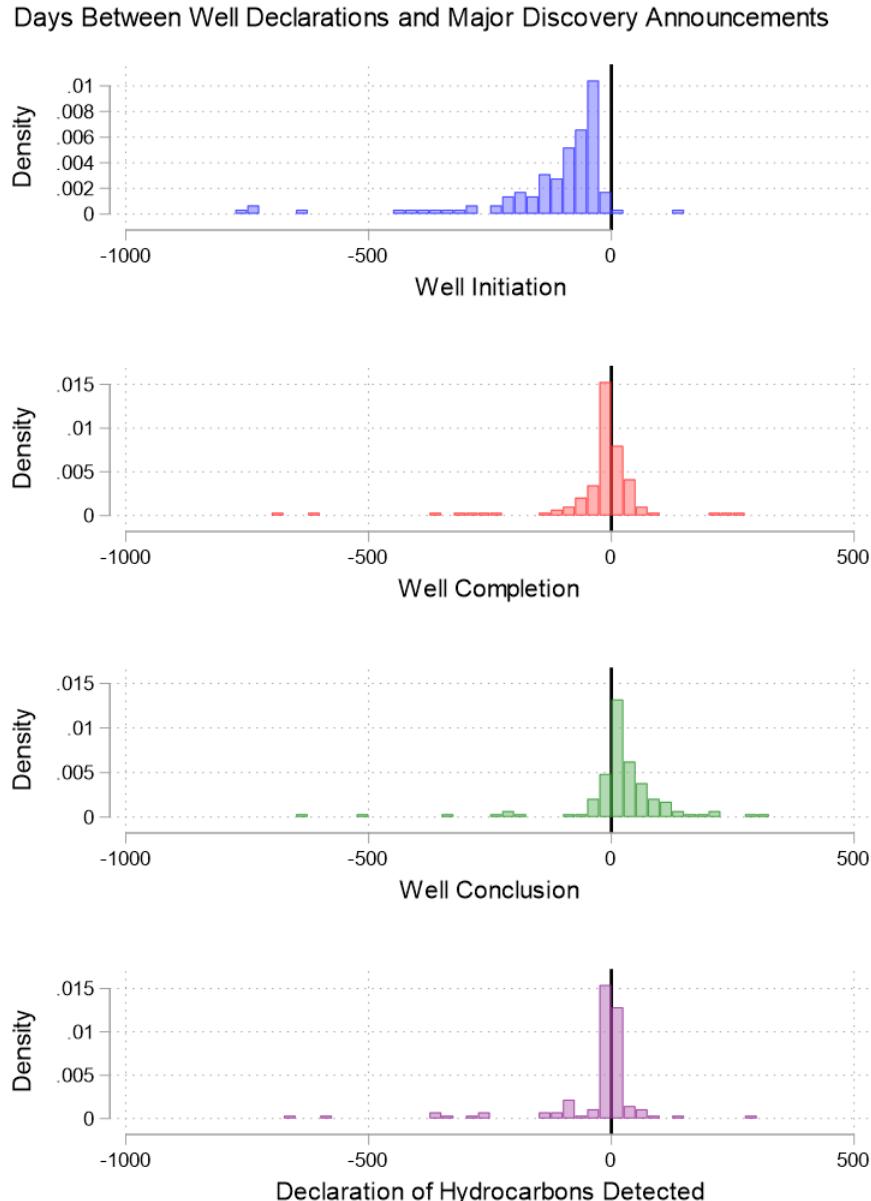
<b>A Supplementary Figures</b>	<b>52</b>
A.1 Descriptive Figures . . . . .	52
A.2 Additional Results Figures . . . . .	56
<b>B Supplementary Tables</b>	<b>61</b>
B.1 Descriptive Tables . . . . .	61
B.2 Results Tables . . . . .	63
<b>C Robustness Checks and Extensions</b>	<b>70</b>
C.1 Main Results: Robustness Across Samples and Estimators . . . . .	70
C.2 Robustness to Alternative Forecasting and Matching Parameters . . . . .	72
C.3 Event Studies with Matched Controls . . . . .	78
C.4 Event Studies with Multiple Events . . . . .	83
C.5 Sample Means for Treated and Control Groups Over Time . . . . .	85
C.6 Sample Balance Across Relative Time Indicators . . . . .	87
C.7 Spatial Spillovers from Discoveries . . . . .	88
<b>D Explanatory Notes</b>	<b>91</b>
D.1 Reconstructing Geodesic Projection Maps . . . . .	91
D.2 Municipal Royalty Distribution Formula . . . . .	93
D.3 Data Sources and Cleaning Procedures . . . . .	94

---

## A Supplementary Figures

### A.1 Descriptive Figures

Figure A1: Timing of Well Stages Relative to CVM Announcements



Note: Vertical black line indicates date of CVM announcement. As expected, well initiation precedes announcements, well conclusion follows announcements, and well completion and declaration of hydrocarbon detection bunches closely around announcement date, corroborating CVM announcements as meaningful “discovery” events.

Figure A2: Examples: Offshore Discovery Announcement and Subsequent News Coverage

(a) CVM Discovery Announcement



**Novo poço confirma potencial de petróleo leve em Tupi**

Rio de Janeiro, 04 de junho de 2009 — PETRÓLEO BRASILEIRO S/A - PETROBRAS, [Bovespa: PETR3/PETRA4, NYSE: PBR/PBRA, Latibex: XPBR/XPBR4, BCBA: APBR/APBRA], uma companhia brasileira de energia com atuação internacional, comunica que a perfuração de mais um poço na área de Tupi reforça as estimativas do potencial de 5 a 8 bilhões de barris de óleo leve e gás natural recuperável nos reservatórios do pré-sal daquela área, em águas ultraprofundas da Bacia de Santos. O poço ainda encontra-se em perfuração, na busca de objetivos mais profundos.

A uma distância de 33 km a noroeste do poço pioneiro 1-RJS-628, o novo poço, denominado 4-BRSA-711-RJS (4-RJS-647), confirmou a presença de reservatórios de boa qualidade e a presença de óleo semelhante ao poço pioneiro de Tupi, o que reforça as estimativas iniciais para a área.

Informalmente conhecido como Iracema, este terceiro poço está localizado na área do Plano de Avaliação de Tupi, em lâmina d'água de 2.210 metros, e a cerca de 250 km da costa do Rio de Janeiro.

A descoberta foi comprovada através de amostragens de óleo leve (cerca de 30° API) por teste a cabo, em reservatórios localizados em profundidade de cerca de 5.000 metros, e comunicada à Agência Nacional do Petróleo, Gás Natural e Biocombustíveis - ANP nesta data.

Após a conclusão da perfuração, o Consórcio, formado pela Petrobras (65% - Operadora), BG Group (25%) e Galp (10%), para a exploração do bloco BM-S-11, onde fica a área de Tupi, dará continuidade às atividades e investimentos previstos no Plano de Avaliação aprovado pela ANP e que prevê a perfuração de outros poços na área.

(b) News Story in *O Globo*

**Novo poço confirma potencial de petróleo leve em Tupi**

O Globo<sup>11</sup> | O Globo . O Globo ; Rio de Janeiro [Rio de Janeiro]04 June 2009.

[ProQuest document link](#)

**FULL TEXT**

RIO - A Petrobras informou, nesta quinta-feira, que a perfuração de mais um poço na área de Tupi reforça as estimativas do potencial de 5 a 8 bilhões de barris de óleo leve e gás natural recuperáveis nos reservatórios do pré-sal daquela área, em águas ultraprofundas da Bacia de Santos. O poço ainda encontra-se em perfuração, na busca de objetivos mais profundos.

Localizado a uma distância de 33 quilômetros a noroeste do poço pioneiro 1-RJS-628, o novo poço, denominado 4-BRSA-711-RJS (4-RJS-647), confirmou a presença de reservatórios de boa qualidade e a presença de óleo semelhante ao poço pioneiro de Tupi, o que reforça as estimativas iniciais para a área.

Clique aqui e confira a localização dos blocos do pré-sal

Informalmente conhecido como Iracema, este terceiro poço está localizado na área do Plano de Avaliação de Tupi, em lâmina d'água de 2.210 metros, e a cerca de 250 km da costa do Rio de Janeiro.

A descoberta, comprovada através de amostragens de óleo leve (cerca de 30° API) em reservatórios localizados em profundidade de cerca de 5.000 metros, foi comunicada à Agência Nacional do Petróleo, Gás Natural e Biocombustíveis - ANP nessa quinta-feira.

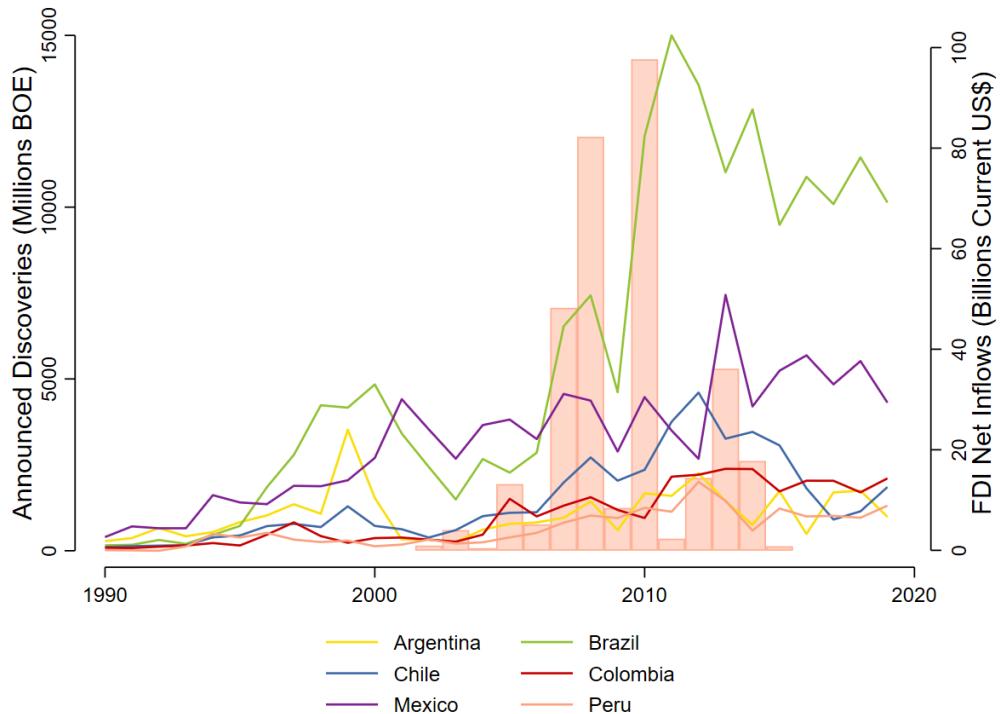
Após a conclusão da perfuração, o consórcio formado pela Petrobras, BG Group e Galp, para a exploração do bloco BM-S-11, onde fica a área de Tupi, dará continuidade às atividades e investimentos previstos no Plano de Avaliação aprovado pela ANP e que prevê a perfuração de outros poços na área.

O petróleo no bloco de Tupi configurou-se, após anúncio em novembro de 2007, na primeira grande descoberta da Petrobras no pré-sal da bacia de Santos.

No dia 1º de maio, foi realizada a extração em alto mar do primeiro óleo do Campo de Tupi. O presidente Luiz Inácio Lula da Silva não foi à plataforma Cidade de São Vicente para a extração por questão de segurança, devido ao mau tempo na região, mas participou de uma cerimônia na Marina da Glória, na companhia do governador do Rio de Janeiro, Sérgio Cabral, durante a qual expressou todo o seu entusiasmo com os avanços da estatal. Ele afirmou que essa conquista da Petrobras equivale "a segunda independência do Brasil".

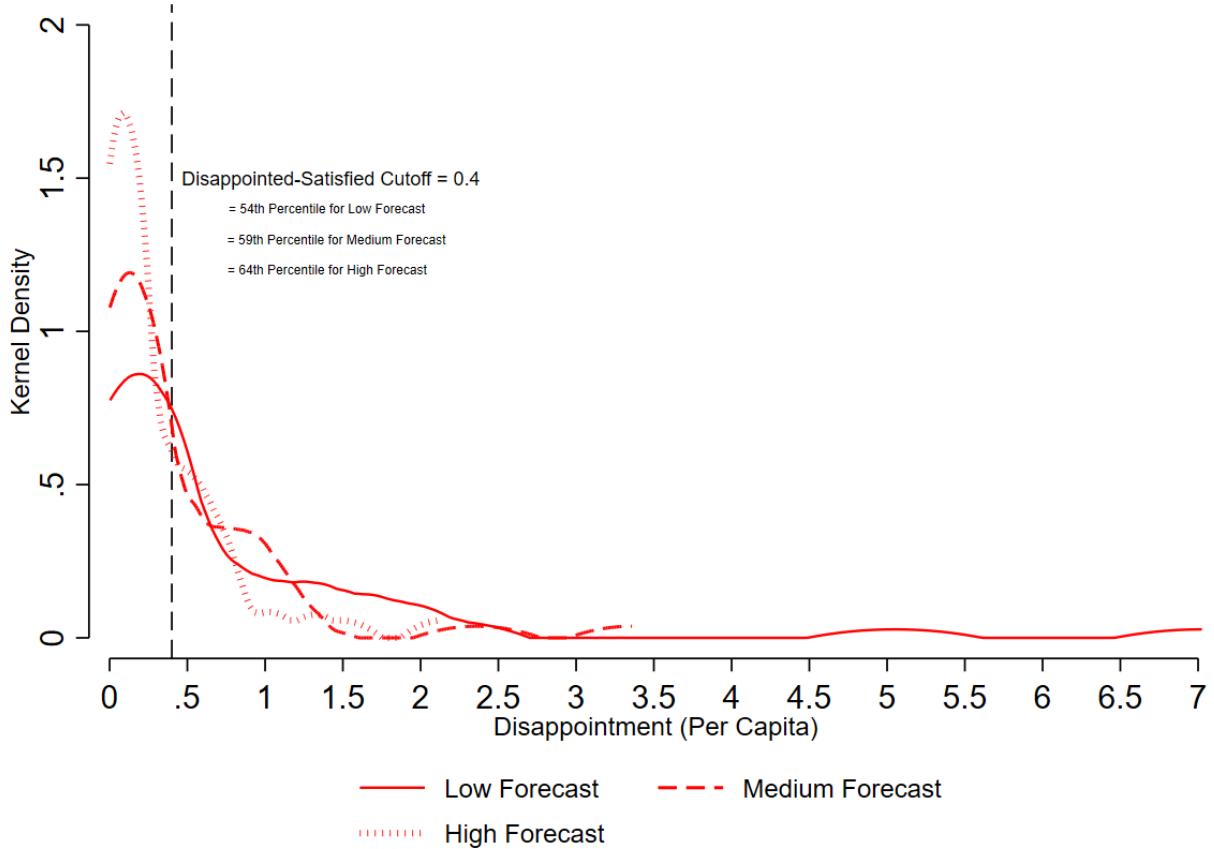
Leia também: Exploração no pré-sal começa sem nova regulação ©NotíciasFinancieras - ©GDA- Agencia Globo  
All rights reserved

Figure A3: Foreign Direct Investment Net Inflows to Selected Latin American Countries, Overlaid on Brazilian Discovery Announcements



Source: [World Bank \(2020\)](#)

Figure A4: Distribution of Forecast Errors Across Treated Municipalities

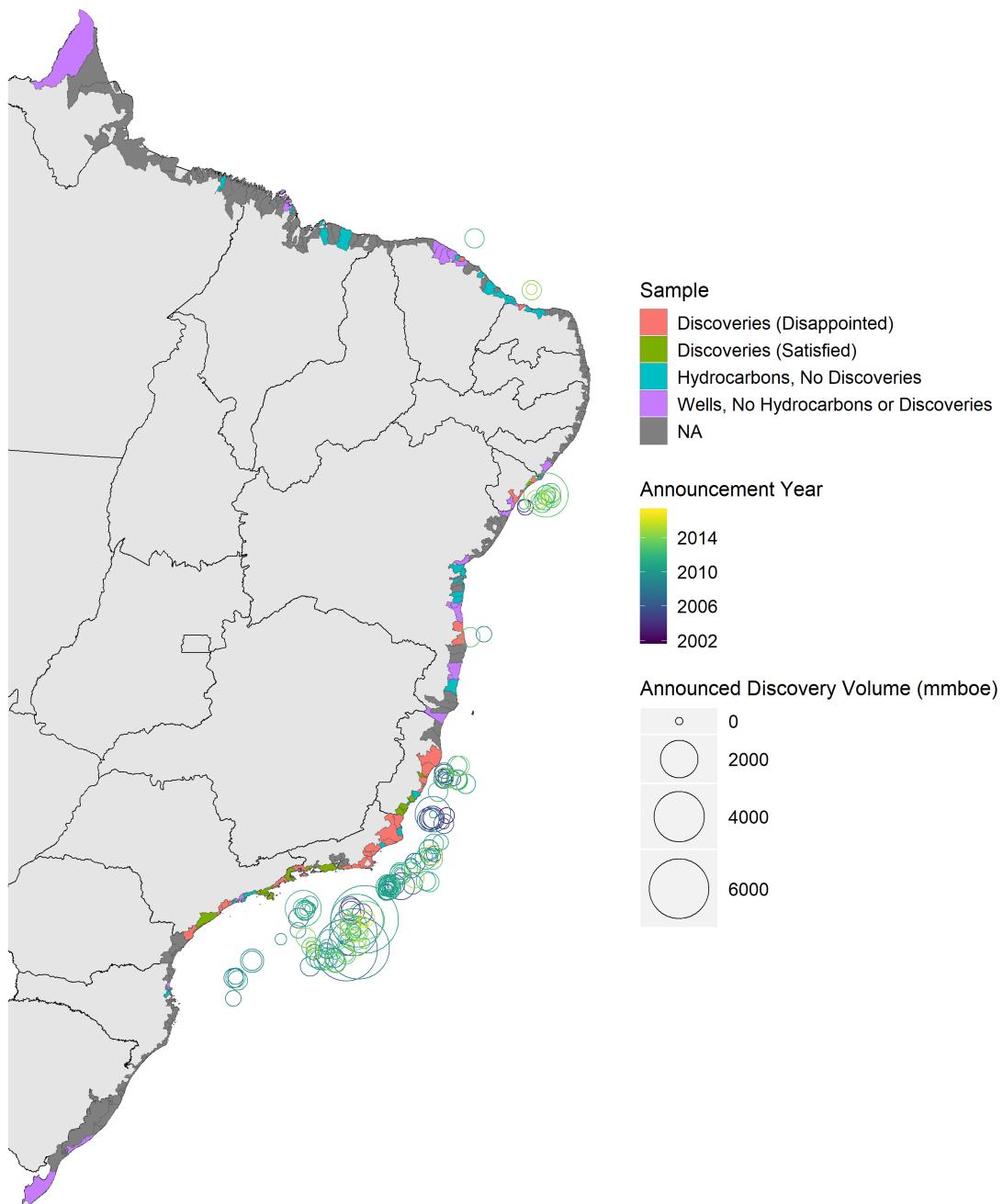


Note: I compute  $Disappointment_{m,2017}$  by comparing expected growth in per capita revenue between the year of the event and the end of the sample with realized growth over this period:

$$Disappointment_{m,2017} = \frac{\frac{Royalties_{m,2017}}{Royalties_{m,t_0}} - E(Royalties_{m,2017})}{\frac{Royalties_{m,t_0}}{E(Royalties_{m,t_0})}}$$

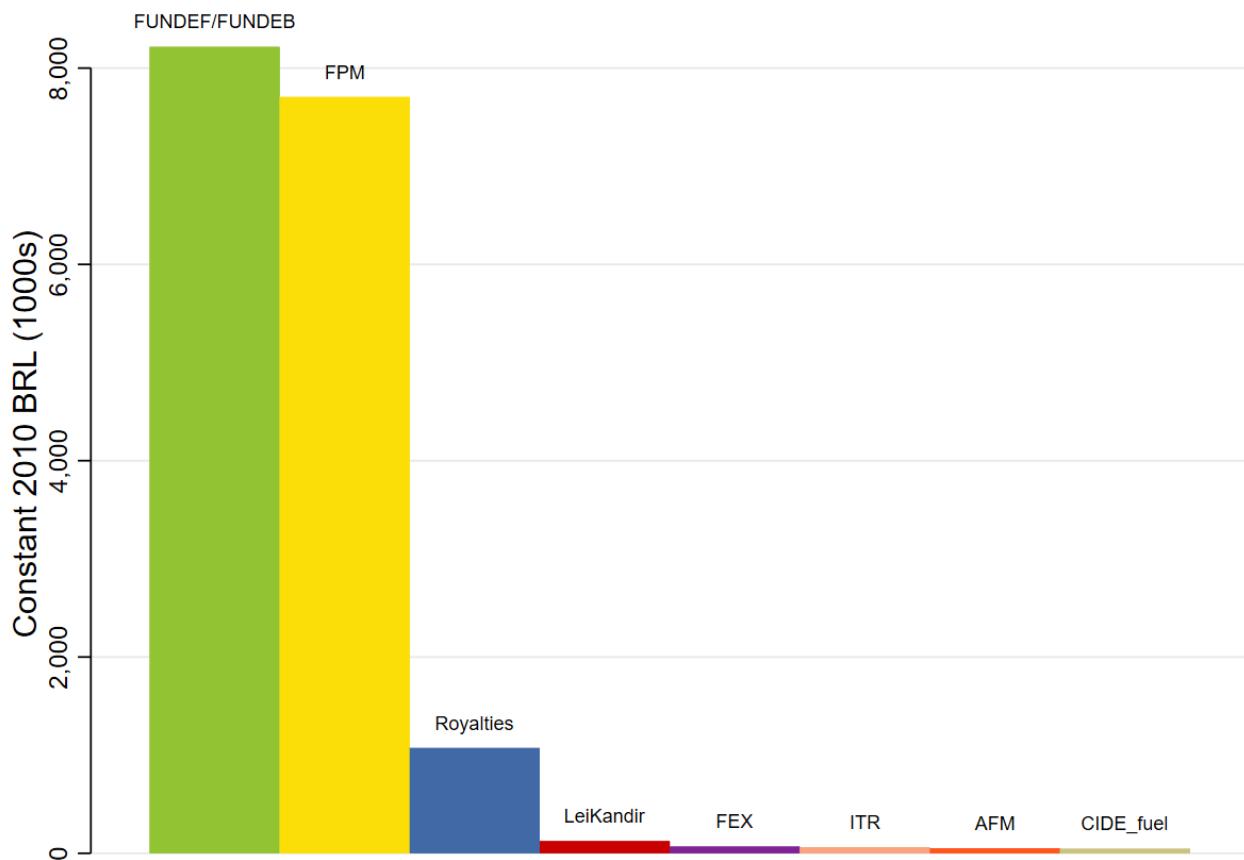
For the purpose of event studies, I classify municipalities as "disappointed" if  $Disappointment_{m,2017}$  is less than 0.4, suggesting their realized oil revenue grew by less than 40% of what they expected by 2017. I classify municipalities values of  $Disappointment_{m,2017}$  above 0.4 as "satisfied."

Figure A5: Brazil: Major Offshore Discoveries and Affected Municipalities



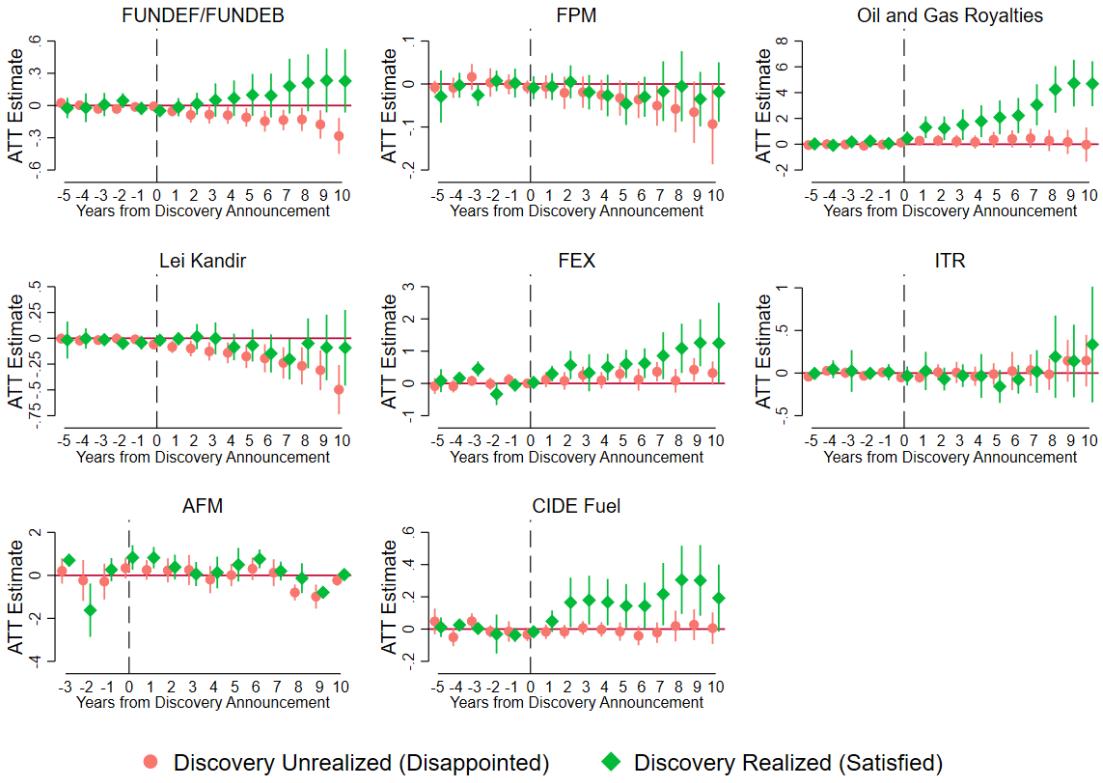
## A.2 Additional Results Figures

Figure A6: Average Federal Transfers Received by Municipal Government (2000-2017)



Note: As illustrated in Figure A7, the largest federal/state transfers to municipalities are FUNDEF/FUNDEB (calculated based on number of students and mode of instruction), FPM (calculated based on population), Royalties (calculated based on value of resource production), and Lei Kandir/FEX (calculated based on value of exported goods and services).

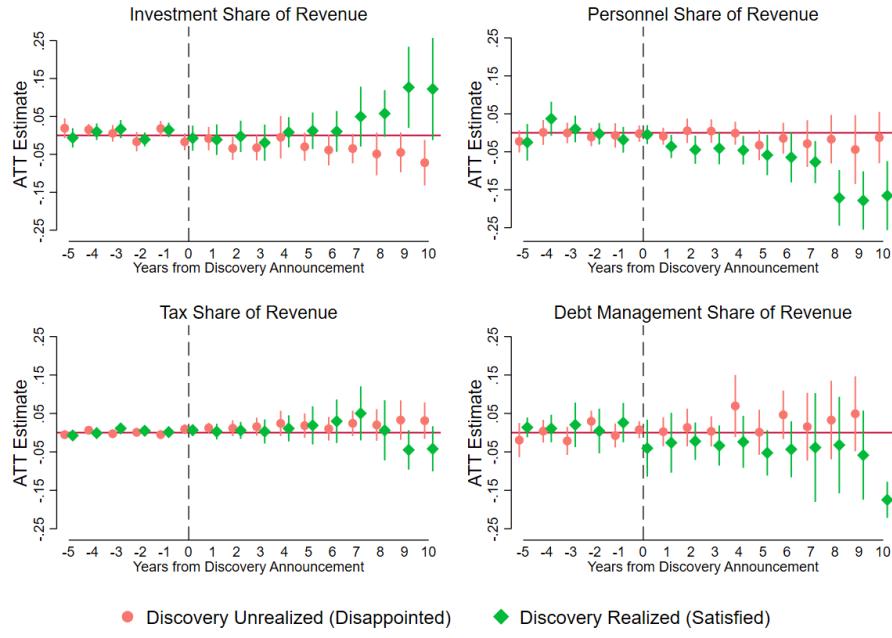
Figure A7: Federal and State Transfers (per capita)



Note: Formulaic federal and state transfers to municipal governments (?):

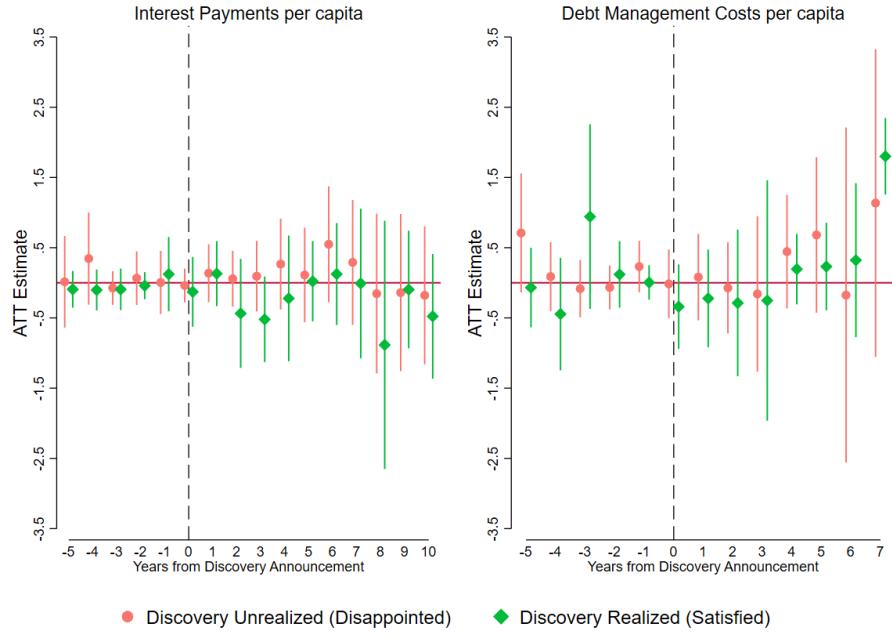
- **FUNDEF (to 2006)/FUNDEB (2007 onwards)** (*Fundo de Manutenção e Desenvolvimento do Ensino Fundamental e de Valorização do Magistério/Fundo de Manutenção e Desenvolvimento da Educação Básica e de Valorização dos Profissionais da Educação*): Federal and state transfers to municipal governments to finance primary and secondary education, calculated based on number of students in different modalities of instruction, as reported in prior year's Basic Education Census. Accounts for 47.4% of total transfers to municipalities between 2000-2017.
- **FPM** (*Fundo de Participação dos Municípios*): Basic federal transfer to municipal governments in proportion to municipal population, calculated each year by IBGE. Accounts for 44.4% of total transfers to municipalities.
- **Royalties**: Financial compensations transferred from federal to specific municipalities affected by oil and gas production, mining, and hydroelectric plants. Calculated in proportion to resource value and other factors. Accounts for 6.2% of total transfers to municipalities.
- **Lei Kandir/FEX** (*Auxílio Financeiro para o Fomento das Exportações*): Federal transfers to municipal governments to compensate for tax dispensation granted to export-oriented goods and services to promote export competitiveness, calculated in proportion to the value of these goods per negotiations between states and the Ministry of the Economy. Accounts for 0.72% and 0.39% of total transfers to municipalities, respectively.
- **ITR** (*Imposto Territorial Rural*): Tax on rural properties, proportional to size and land-use, collected jointly by federal and municipal governments. Municipalities may request to collect fully and retain 100% of revenues. Accounts for 0.35% of total transfers to municipalities.
- **AFM** (*Apoio/Auxílio Financeiro aos Municípios*): Sporadic and exceptional transfer from federal to municipal governments made to support municipalities through moments of transitory financial strain. Accounts for 0.30% of total transfers to municipalities.
- **CIDE-Combustíveis** (*Contribuição de intervenção no domínio econômico incidente sobre as operações realizadas com combustíveis*): federal transfer of portion of revenues from tax on importation and commercialization of gas and select other fuels, assessed per unit. Accounts for 0.27% of total transfers to municipalities.

Figure A8: Fiscal Health Indicators



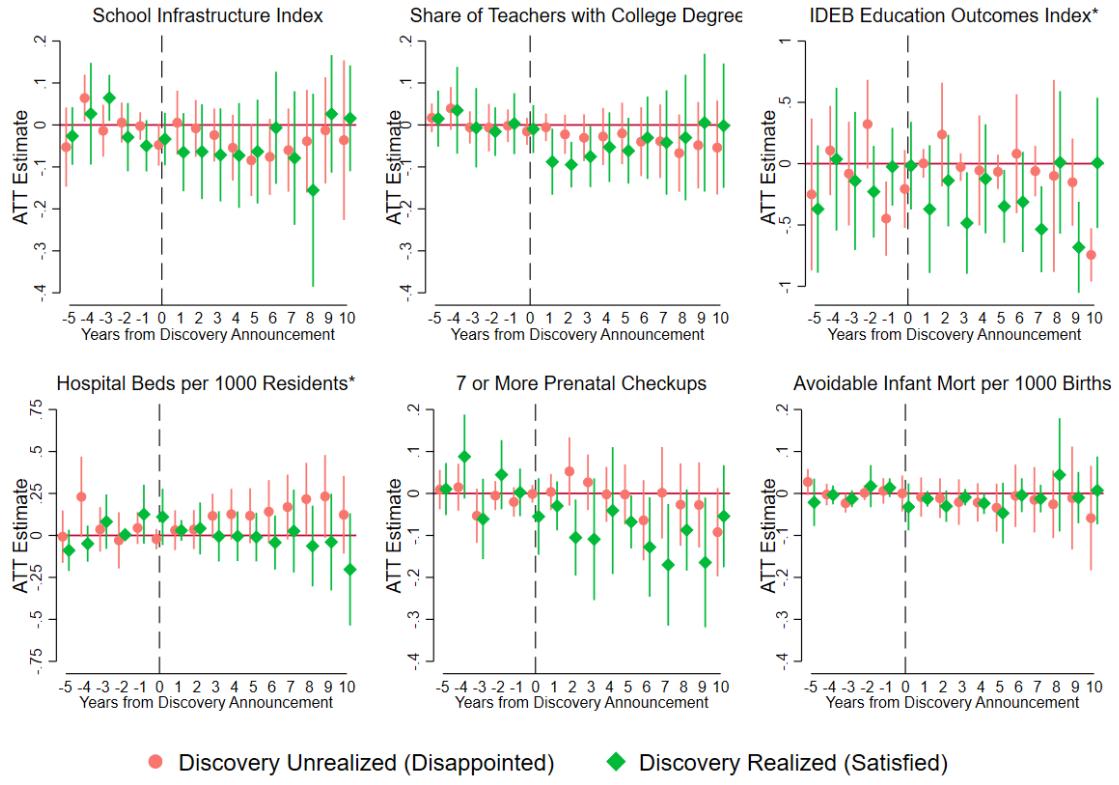
Note: Fiscal health indicators are drawn from [FIRJAN \(2019\)](#) Municipal Development Report. Indicators include municipal public investment as a share of total revenue, spending on municipal personnel as a share of revenue, municipal tax revenue as a share of total revenue (fiscal autonomy or self-sustainability), and debt management costs as a share of total revenue. According to FIRJAN, an NGO, “fiscal health” is indicated by higher investment and tax revenue shares of revenue, and lower personnel spending and debt management shares of revenue.

Figure A9: Debt



Note: Debt share of revenue is calculated by summing expenditures on debt (processed, unprocessed, and liquidating), debt service, debt restructuring, interest, and *restos a pagar*.

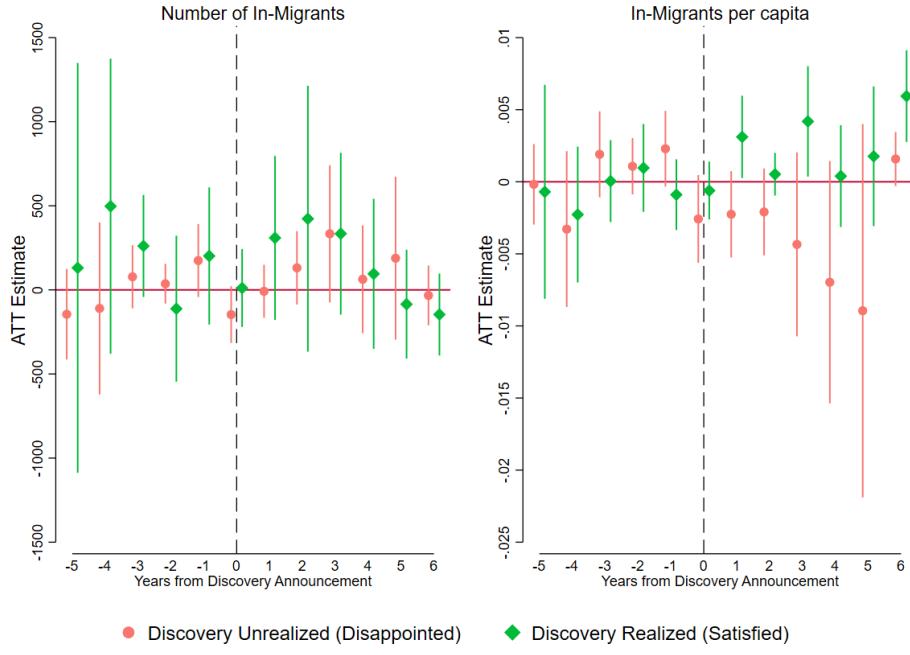
Figure A10: Public Goods Provision and Quality



Note: School infrastructure index, constructed from Basic Education Census, is a simple sum of three indicators: school has library, science lab, and computer lab. IDEB is a biannual measure of school quality, including test scores and graduation rates. Hospital beds per 1000 residents refers to municipal hospital beds only. Prenatal visits measures the share of pregnant women receiving at least the recommended 7 health checkups prior to giving birth. Health data are drawn from DataSUS.

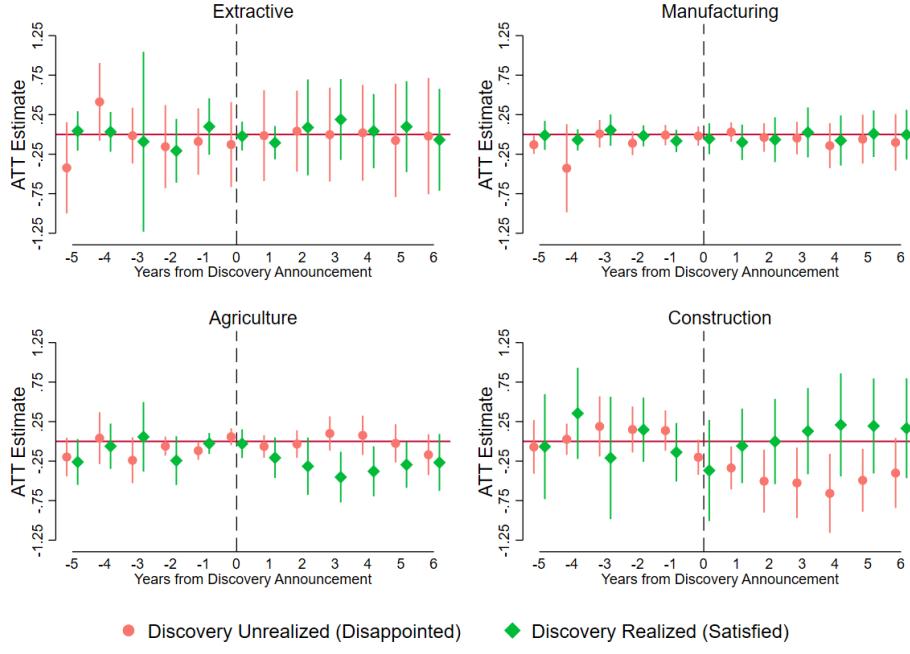
\* Asterisks indicate that a different y-axis scale is used from the rest of the sub-figures, in order to accommodate large differences in scale of effects.

Figure A11: In-Migration After Discovery Announcements



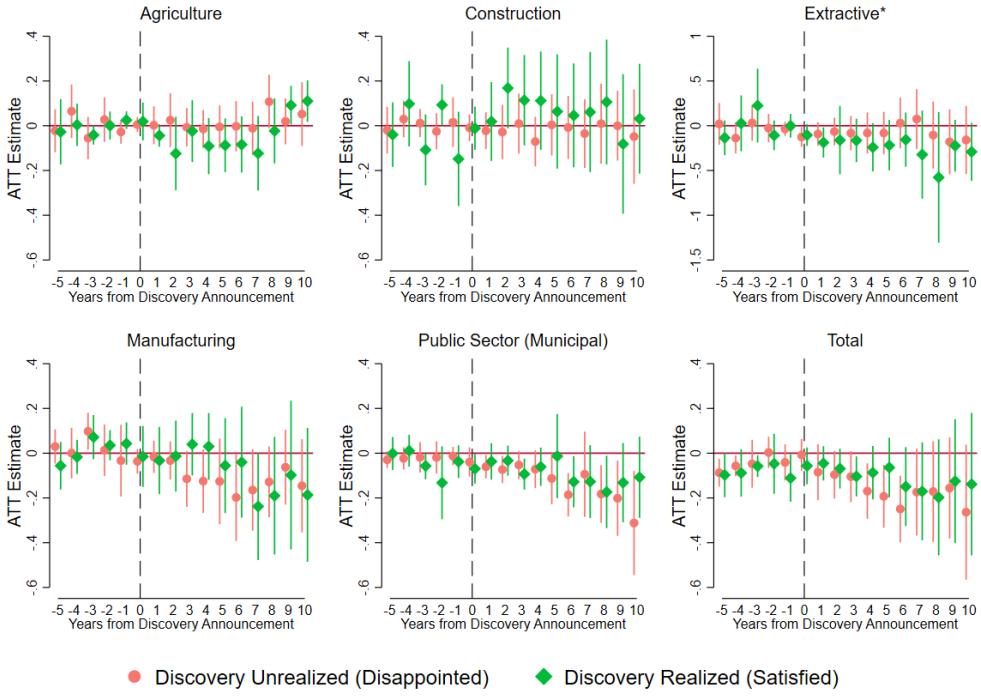
Note: Migration data are calculated from Demographic Censuses of 2000 and 2010. Data for 2020 demographic Census are not yet available, limiting the extent of these figures to the 2000-2010 window. This window misses a large proportion of the later effects of oil discoveries.

Figure A12: Formal Employment by Sector



Note: Data on formal employment are drawn from RAIS. Sectors are defined following the standard procedure in [Dahis \(2020\)](#).

Figure A13: Formal Wages by Sector



Note: Data on formal wages are drawn from RAIS. Sectors are defined following the standard procedure in [Dahis \(2020\)](#).

## B Supplementary Tables

### B.1 Descriptive Tables

Table B1: Oil Company Discovery Announcements to *Comissão de Valores Mobiliários*

Company	% Wells in ANP Database	No. Wells Drilled	No. Discovery Announcements
Petróleo Brasileiro S.A - Petrobras	75.743	1402	134
OGX (Dommo Energia)	5.132	95	36
Equinor Brasil/Energy	5.078	94	0
Shell Brasil	2.485	46	0
Petro Rio O&G/Jaguar	2.107	39	0
Total E&P do Brasil	0.756	14	0
Enauta Energia S.A./Queiroz Galvão E&P	0.648	12	5
Perenco Brasil	0.540	10	0
Karoon Petroleo e Gas S.A.	0.432	8	1
Exxon Mobil Brasil	0.216	4	0
Chevron Brasil	0.054	1	0
<b>Total</b>	<b>93.2</b>	<b>1725</b>	<b>177</b>

<sup>1</sup> Other operators checked: Anadarko, BP, Devon, Eni, Maha, OP Energia, Repsol Sinopec, Texaco, Vanco, Wintershall, ONGC, Esso, Amerada Hess, Unocal, SHB; no CVM Market Communications available

<sup>2</sup> ANP made 2 discovery announcements that were reported in media but not by companies

<sup>3</sup> Petrobras often publishes market communications on behalf of its partners. Since it frequently partners with other companies on specific concessions, many companies' discoveries were reported in Petrobras announcements.

Table B2: Disappointed/Satisfied Classifications Under Alternative Forecasting Specifications

Municipality	Outcome (per capita)			Outcome (total)		
	Low	Medium	High	Low	Medium	High
ANGRADOSREIS33	D	D	D	D	D	D
ARACAJU28	D	D	D	D	D	D
ARACRUZ32	D	D	D	D	D	D
ARARUAMA33	D	D	D	D	D	D
AREIABRANCA24	D	D	D	D	D	D
ARMACAODOSBUZIOS33	D	D	D	D	D	D
ARRAIALDOCABO33	D	D	D	D	D	D
BALNEARIOCAMBORIU42	D	D	D	D	D	D
BARRADOSCOQUEIROS28	D	D	D	D	D	D
CABOFRIO33	D	D	D	D	D	D
CAMPOSOSGOYTACAZES33	D	D	D	D	D	D
CANANEIA35	D	D	D	D	D	D
CANAVIEIRAS29	D	D	D	D	D	D
CASIMIRODEABREU33	D	D	D	S	S	D
ITANHAEM35	D	D	D	D	D	D
ITAPEMA42	D	D	D	D	D	D
ITAPORANGADAJUDA28	D	D	D	D	D	D
LINHARES32	D	D	D	S	D	D
MONGAGUA35	D	D	D	D	D	D
PACATUBA28	D	D	D	D	D	D
PARACURU23	D	D	D	D	D	D
PERUIBE35	D	D	D	D	D	D
QUISSAMA33	D	D	D	D	D	D
RIODASOSTRAS33	D	D	D	D	D	D
SAOFRANCISCODEITABAPOANA33	D	D	D	D	D	D
SAQUAREMA33	D	D	D	D	D	D
SERRA32	D	D	D	D	D	D
UBATUBA35	D	D	D	D	D	D
UNA29	D	D	D	D	D	D
VILABELHA32	D	D	D	D	D	D
ANCHIETA32	S	S	S	S	S	S
CARAGUATATUBA35	S	S	S	S	S	S
FUNDAO32	S	S	S	S	S	S
IGUAPE35	S	S	S	S	S	S
ILHABELA35	S	S	S	S	S	S
ILHACOMPRIDA35	S	S	S	S	S	S
ITAPEMIRIM32	S	S	S	S	S	S
MACAE33	S	D	D	S	S	S
MANGARATIBA33	S	S	S	S	S	S
MARATAIZES32	S	S	D	S	S	D
MARICA33	S	D	D	S	S	D
NITEROI33	S	S	S	S	S	S
PARATI33	S	S	D	S	S	S
PIRAMBU28	S	S	S	S	S	S
PRESIDENTEKENNEDY32	S	S	S	S	S	S
RIODEJANEIRO33	S	S	S	S	S	S
SAOSEBASTIAO35	S	S	S	S	S	S
VITORIA32	S	D	D	S	D	D
Total Disappointed	30	33	35	28	30	33
Total Satisfied	18	15	13	20	18	15
Percent Disappointed	62.5	68.8	72.9	58.3	62.5	68.8

Note: D indicates that the municipality was Disappointed and S indicates that the municipality was satisfied under alternative forecasting definitions.

## B.2 Results Tables

Table B3: Revenues: ATT Estimates and Sample Characteristics

### Disappointed Municipalities

Time	Total Rev.		Rev. p.c.		Oil Rev. p.c.		Tax Rev. p.c.		Transfer Rev. p.c.	
	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
-5	-0.025	0.026	-0.026	0.027	-0.070	0.053	-0.111**	0.045	0.008	0.013
-4	-0.003	0.021	-0.010	0.021	0.002	0.035	-0.050	0.057	-0.004	0.011
-3	0.019	0.027	0.014	0.027	-0.016	0.059	-0.086***	0.032	-0.002	0.011
-2	-0.020	0.022	-0.035	0.025	-0.116*	0.062	-0.074**	0.037	-0.008	0.015
-1	-0.007	0.022	-0.009	0.023	-0.015	0.047	-0.117*	0.062	-0.005	0.010
0	0.009	0.021	0.004	0.019	0.129	0.098	0.058	0.059	-0.010	0.012
+1	0.000	0.021	-0.018	0.021	0.277	0.178	0.156**	0.077	-0.029**	0.015
+2	-0.042	0.032	-0.084**	0.041	0.281	0.216	0.008	0.087	-0.048**	0.023
+3	-0.06*	0.036	-0.112**	0.049	0.226	0.247	-0.079	0.143	-0.043*	0.026
+4	-0.063	0.043	-0.117**	0.059	0.168	0.262	-0.029	0.180	-0.047*	0.025
+5	-0.049	0.043	-0.106*	0.061	0.349	0.314	-0.223	0.172	-0.058**	0.026
+6	-0.111**	0.047	-0.165**	0.072	0.416	0.339	-0.299	0.201	-0.08***	0.030
+7	-0.105**	0.051	-0.166**	0.070	0.460	0.378	-0.391*	0.201	-0.075**	0.032
+8	-0.192***	0.068	-0.277***	0.095	0.284	0.419	-0.323	0.245	-0.067*	0.036
+9	-0.203**	0.081	-0.287**	0.113	0.177	0.478	-0.311	0.232	-0.09**	0.044
+10	-0.408***	0.103	-0.52***	0.157	-0.030	0.676	-0.268	0.296	-0.142**	0.058
DV (IHS)	19.38		8.37		4.35		6.07		6.88	
DV	130,495,959		2,158		38.73		216.34		486.31	
n	1,392		1,392		1,494		1,392		1,441	
Units	83		83		83		83		81	

### Satisfied Municipalities

Time	Total Rev.		Rev. p.c.		Oil Rev. p.c.		Tax Rev. p.c.		Transfer Rev. p.c.	
	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
-5	-0.035	0.060	-0.068	0.060	0.027	0.181	-0.282***	0.092	-0.034	0.033
-4	-0.066*	0.037	-0.068*	0.036	-0.083	0.149	-0.18***	0.059	-0.015	0.029
-3	-0.022	0.032	-0.026	0.033	0.182	0.217	-0.057	0.057	-0.022	0.015
-2	-0.005	0.027	0.003	0.027	0.244	0.197	-0.050	0.051	0.014	0.011
-1	-0.031	0.021	-0.035	0.026	0.059	0.108	-0.16***	0.052	-0.003	0.017
0	-0.007	0.029	-0.014	0.030	0.45*	0.273	0.022	0.066	-0.033**	0.013
+1	0.059	0.046	0.055	0.045	1.317***	0.431	0.010	0.088	-0.027	0.018
+2	0.089	0.062	0.087	0.062	1.23***	0.468	-0.006	0.110	-0.002	0.024
+3	0.114**	0.057	0.108*	0.061	1.509***	0.607	0.039	0.120	-0.012	0.029
+4	0.101	0.074	0.103	0.076	1.783***	0.629	-0.069	0.223	-0.008	0.034
+5	0.143	0.096	0.146	0.100	2.085***	0.671	-0.025	0.231	-0.006	0.043
+6	0.128	0.107	0.130	0.112	2.221***	0.695	-0.373	0.452	-0.004	0.044
+7	0.212	0.137	0.214	0.143	3.055***	0.814	-0.143	0.249	0.035	0.054
+8	0.481***	0.163	0.489***	0.171	4.246***	0.921	-0.334	0.342	0.050	0.054
+9	0.739***	0.215	0.748***	0.221	4.733***	0.923	-0.001	0.307	0.040	0.063
+10	0.76***	0.238	0.754***	0.243	4.689***	0.889	0.010	0.287	0.045	0.053
DV (IHS)	19.27		8.34		4.08		6.37		6.83	
DV	116,902,735		2,094		29.56		292.03		462.59	
n	1,211		1,211		1,278		1,211		1,225	
Units	71		71		71		71		69	

Tables report coefficient estimates and robust asymptotic standard errors for municipal revenue outcomes for disappointed (top) and satisfied (bottom) samples. Disappointed municipalities received less than 40% of revenues expected from discovery announcements by 2017; satisfied municipalities received more than 40%. Never-treated control units are municipalities that received exploratory offshore wells in catchment zone after 1999, but no discoveries. Transfer Revenues per capita exclude oil and gas transfers. DV reports the mean of the dependent variable in period t-1. Specifications include municipality and year fixed effects and cluster standard errors at municipality-level. Estimates are generated using Callaway and Sant'Anna (2020) *csid* estimator with bootstrapped standard errors (seed=39627236). Continuous outcome variables use inverse hyperbolic sine transformation. Monetary values are deflated to constant 2010 BRL. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table B4: Expenditure and Public Employment: ATT Estimates and Sample Characteristics

Disappointed Municipalities										
Time	Total Spend.		Spend. p.c.		Admin. Spend. p.c.		Personnel Spend. p.c.		Mun. Empl. p.c.	
	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
-5	-0.076**	0.034	-0.076**	0.037	-0.005	0.068	-0.088**	0.036	0.000	0.002
-4	0.004	0.025	-0.003	0.025	-0.109	0.077	-0.008	0.025	-0.001	0.001
-3	-0.009	0.023	-0.014	0.024	0.017	0.061	-0.002	0.032	0.001	0.002
-2	-0.041	0.025	-0.056*	0.028	-0.041	0.045	-0.055**	0.027	-0.001	0.002
-1	-0.028	0.026	-0.030	0.026	-0.007	0.060	-0.013	0.029	0.000	0.002
0	-0.006	0.029	-0.011	0.027	0.001	0.070	-0.009	0.027	0.000	0.002
+1	-0.002	0.027	-0.020	0.025	-0.011	0.054	-0.036	0.023	-0.003	0.004
+2	-0.029	0.031	-0.071**	0.033	-0.355	0.242	-0.064*	0.034	-0.002	0.003
+3	-0.040	0.033	-0.092**	0.041	-0.085	0.089	-0.091**	0.042	-0.003	0.004
+4	-0.026	0.035	-0.08*	0.046	-0.125	0.081	-0.097**	0.046	-0.004	0.003
+5	-0.050	0.043	-0.107*	0.056	-0.18*	0.096	-0.145***	0.056	-0.005	0.004
+6	-0.087*	0.050	-0.142**	0.066	-0.138	0.093	-0.173***	0.063	-0.008*	0.005
+7	-0.093*	0.054	-0.154**	0.063	-0.132	0.097	-0.175***	0.064	-0.004	0.005
+8	-0.156**	0.068	-0.242***	0.077	-0.277**	0.135	-0.267***	0.080	-0.004	0.008
+9	-0.18**	0.076	-0.264***	0.086	-0.396***	0.129	-0.311***	0.093	-0.004	0.006
+10	-0.318***	0.092	-0.43***	0.121	-0.621***	0.167	-0.498***	0.130	-0.009	0.008
DV (IHS)	18.71		7.70		6.55		7.51		0.05	
DV	66,775,902		1,104		349.62		913.11		0.05	
n	1,392		1,392		1,313		1,392		1,494	
Units	83		83		83		83		83	

Satisfied Municipalities										
Time	Total Spend.		Spend. p.c.		Admin. Spend. p.c.		Personnel Spend. p.c.		Mun. Empl. p.c.	
	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
-5	-0.059	0.052	-0.092**	0.039	-0.051	0.093	-0.121**	0.049	-0.002	0.004
-4	-0.044	0.041	-0.046	0.041	-0.232*	0.121	-0.001	0.043	-0.012	0.008
-3	0.030	0.038	0.027	0.037	0.046	0.062	-0.009	0.036	0.010	0.007
-2	-0.025	0.032	-0.017	0.033	-0.071	0.107	-0.017	0.030	0.000	0.002
-1	-0.027	0.030	-0.031	0.033	-0.363	0.397	-0.05*	0.026	-0.004	0.003
0	-0.073	0.045	-0.081*	0.046	0.335	0.281	-0.028	0.023	0.002	0.005
+1	-0.055	0.045	-0.059	0.045	0.313	0.301	-0.029	0.034	0.000	0.004
+2	-0.005	0.052	-0.007	0.050	0.409	0.330	-0.019	0.040	0.006	0.005
+3	-0.017	0.058	-0.022	0.058	0.446	0.340	0.006	0.051	0.003	0.005
+4	-0.012	0.051	-0.010	0.054	0.592	0.384	-0.005	0.055	0.009*	0.005
+5	0.007	0.068	0.010	0.068	0.555	0.372	0.013	0.072	0.014**	0.006
+6	-0.011	0.068	-0.008	0.069	0.432	0.382	-0.012	0.074	0.016**	0.007
+7	0.061	0.073	0.063	0.073	0.693	0.577	0.044	0.086	0.007	0.011
+8	0.111	0.111	0.120	0.107	1.074*	0.620	0.081	0.108	0.016	0.011
+9	0.247*	0.130	0.256*	0.132	1.561**	0.781	0.235*	0.132	0.029*	0.015
+10	0.283***	0.110	0.277***	0.105	1.535**	0.745	0.278**	0.121	0.029**	0.013
DV (IHS)	18.75		7.83		6.13		7.58		0.05	
DV	69,501,078		1,257		229.72		979.31		0.05	
n	1,211		1,211		1,138		1,211		1,278	
Units	71		71		71		71		71	

Tables report coefficient estimates and robust asymptotic standard errors for municipal expenditure and public employment outcomes for disappointed (top) and satisfied (bottom) samples. Disappointed municipalities received less than 40% of revenues expected from discovery announcements by 2017; satisfied municipalities received more than 40%. Never-treated control units are municipalities that received exploratory offshore wells in catchment zone after 1999, but no discoveries. Spending variables refer to current (realized) spending. DV reports the mean of the dependent variable in period t-1. Specifications include municipality and year fixed effects and cluster standard errors at municipality-level. Estimates are generated using Callaway and Sant'Anna (2020) csid estimator with bootstrapped standard errors (seed=39627236). Continuous outcome variables use inverse hyperbolic sine transformation. Monetary values are deflated to constant 2010 BRL. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table B5: Investment and Economic Diversification: ATT Estimates and Sample Characteristics

Disappointed Municipalities				
Time	Investment p.c.		Diversif. Spends p.c.	
	Coef.	St. Err.	Coef.	St. Err.
-5	0.215	0.155	0.202	0.413
-4	0.047	0.094	-0.078	0.361
-3	0.202*	0.118	0.296	0.354
-2	-0.242**	0.116	-0.233	0.294
-1	0.144	0.106	-0.273	0.265
0	-0.146	0.100	-0.020	0.328
+1	-0.191	0.159	0.032	0.366
+2	-0.334**	0.140	-0.013	0.342
+3	-0.418**	0.175	-0.225	0.359
+4	-0.398**	0.199	-0.171	0.345
+5	-0.46**	0.213	-0.213	0.314
+6	-0.533***	0.205	-0.311	0.332
+7	-0.612***	0.197	-0.023	0.355
+8	-0.873***	0.245	-0.142	0.359
+9	-0.655**	0.291	-0.013	0.436
+10	-1.262***	0.332	0.084	0.485
DV (IHS)		6.03		3.17
DV̄		207.86		11.88
n		1,423		1,494
Units		83		83

Satisfied Municipalities				
Time	Investment p.c.		Diversif. Spend. p.c.	
	Coef.	St. Err.	Coef.	St. Err.
-5	-0.190	0.264	-0.053	0.493
-4	0.057	0.246	0.806*	0.417
-3	0.398	0.246	0.410	0.277
-2	-0.117	0.128	-0.782**	0.319
-1	-0.128	0.248	0.261	0.216
0	-0.003	0.356	-0.229	0.248
+1	-0.004	0.373	-0.532	0.440
+2	0.339	0.410	-0.210	0.411
+3	-0.141	0.458	-0.393	0.426
+4	0.315	0.415	-0.428	0.447
+5	0.394	0.455	-0.615	0.492
+6	0.078	0.525	-0.721	0.551
+7	0.485	0.591	-1.092	0.774
+8	1.156*	0.688	-1.244	0.873
+9	1.440	0.892	-0.848	0.960
+10	1.471**	0.698	-1.066*	0.625
DV (IHS)		5.54		3.58
DV̄		127.34		17.92
n		1,230		1,278
Units		71		71

Tables report coefficient estimates and robust asymptotic standard errors for municipal expenditure and public employment outcomes for disappointed (top) and satisfied (bottom) samples. Disappointed municipalities received less than 40% of revenues expected from discovery announcements by 2017; satisfied municipalities received more than 40%. Never-treated control units are municipalities that received exploratory offshore wells in catchment zone after 1999, but no discoveries. Investment refers to public municipal investment (e.g., infrastructure). Economic diversification spending is the sum of municipal spending to promote industry, services, and agriculture. DV̄ reports the mean of the dependent variable in period t-1. Specifications include municipality and year fixed effects and cluster standard errors at municipality-level. Estimates are generated using Callaway and Sant'Anna (2020) *csdid* estimator with bootstrapped standard errors (seed=39627236). Continuous outcome variables use inverse hyperbolic sine transformation. Monetary values are deflated to constant 2010 BRL. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table B6: Public Goods Spending and Provision: ATT Estimates and Sample Characteristics

Disappointed Municipalities									
Time	Ed. Spending p.c.		Health Spending p.c.		Ed. Index		Health Index		
	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	
-5	-0.067	0.046	-0.282	0.315	-0.010	0.012	-0.032**	0.016	
-4	-0.156	0.194	0.000	0.051	0.004	0.009	-0.012	0.012	
-3	0.193	0.192	-0.039	0.053	-0.013	0.011	-0.024***	0.007	
-2	-0.039	0.040	-0.020	0.042	-0.011	0.009	-0.003	0.007	
-1	-0.005	0.037	0.052	0.060	-0.009	0.007	0.000	0.005	
0	-0.032	0.031	-0.074	0.053	-0.004	0.003	-0.005	0.006	
+1	-0.002	0.037	-0.066	0.090	-0.007	0.005	-0.010	0.011	
+2	-0.366	0.291	-0.352	0.257	-0.013**	0.006	-0.003	0.018	
+3	-0.196*	0.112	-0.23*	0.125	-0.011	0.008	-0.007	0.020	
+4	-0.135**	0.060	-0.145**	0.071	-0.011	0.009	-0.017	0.023	
+5	-0.158**	0.068	-0.165**	0.078	-0.013	0.012	-0.026	0.027	
+6	-0.182***	0.070	-0.155*	0.092	-0.015	0.012	-0.030	0.031	
+7	-0.178**	0.073	-0.151	0.102	-0.015	0.015	-0.056	0.039	
+8	-0.232***	0.082	-0.255**	0.101	-0.017	0.015	-0.081	0.050	
+9	-0.253**	0.099	-0.286**	0.132	-0.05***	0.014	-0.162***	0.027	
+10	-0.439***	0.152	-0.408***	0.148	-0.052***	0.018	-0.208***	0.021	
$\bar{DV}$ (IHS)	6.97		6.82		0.69		0.73		
$\bar{DV}$	532.11		457.99		0.75		0.80		
n	1,392		1,392		996		996		
Units	83		83		83		83		

Satisfied Municipalities									
Time	Ed. Spending p.c.		Health Spending p.c.		Ed. Index		Health Index		
	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	
-5	-0.102*	0.055	-0.057	0.082	-0.015	0.012	-0.016**	0.007	
-4	-0.094*	0.055	0.063	0.064	-0.031***	0.008	-0.02*	0.011	
-3	0.063	0.041	-0.069	0.062	-0.019**	0.008	-0.009	0.006	
-2	-0.060	0.047	-0.001	0.049	-0.007	0.015	-0.007	0.006	
-1	-0.019	0.040	-0.141*	0.077	0.007	0.015	-0.006	0.004	
0	-0.041	0.048	0.092	0.091	-0.016***	0.005	-0.013*	0.007	
+1	0.003	0.071	0.223**	0.089	-0.023*	0.013	-0.031**	0.013	
+2	0.048	0.068	0.188**	0.084	-0.029***	0.010	-0.046***	0.016	
+3	0.020	0.052	0.043	0.097	-0.028**	0.014	-0.055***	0.017	
+4	-0.034	0.057	-0.074	0.125	-0.032*	0.017	-0.076***	0.018	
+5	0.013	0.067	0.082	0.090	-0.033*	0.018	-0.087***	0.019	
+6	0.004	0.070	0.136	0.112	-0.044*	0.027	-0.101***	0.016	
+7	0.032	0.084	0.195	0.158	-0.049	0.035	-0.105***	0.016	
+8	0.143	0.103	0.332**	0.158	-0.088***	0.018	-0.112***	0.019	
+9	0.366	0.226	0.449	0.274	-0.086***	0.020	-0.114***	0.017	
+10	0.378***	0.122	0.455**	0.205	-0.087***	0.019	-0.139***	0.025	
$\bar{DV}$ (IHS)	7.01		6.71		0.78		0.82		
$\bar{DV}$	553.83		410.28		0.86		0.92		
n	1,208		1,208		852		852		
Units	71		71		71		71		

Tables report coefficient estimates and robust asymptotic standard errors for municipal expenditure and public employment outcomes for disappointed (top) and satisfied (bottom) samples. Disappointed municipalities received less than 40% of revenues expected from discovery announcements by 2017; satisfied municipalities received more than 40%. Never-treated control units are municipalities that received exploratory offshore wells in catchment zone after 1999, but no discoveries. Education and Health Indices are drawn from the FIRJAN Municipal Development Index (FIRJAN, 2020), a comprehensive measure of municipal development published annually by FIRJAN, a nonprofit. The Education Index is an aggregate score ranging from 0-1, composed of the following indicators: early childhood enrollment rates, graduation rates, grade-age distortion, hours spent in class, share of teachers with college degrees, and IDEB test scores. The Health Index is an aggregate score ranging from 0-1, composed of the following indicators: proportion of pregnant women receiving >7 pre-natal visits, deaths of undefined causes, and avoidable infant mortality.  $\bar{DV}$  reports the mean of the dependent variable in period t-1. Specifications include municipality and year fixed effects and cluster standard errors at municipality-level. Estimates are generated using Callaway and Sant'Anna (2020) *csdid* estimator with bootstrapped standard errors (seed=39627236). Continuous outcome variables use inverse hyperbolic sine transformation. Monetary values are deflated to constant 2010 BRL. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table B7: GDP and Population: ATT Estimates and Sample Characteristics

Disappointed Municipalities					
Time	GDP p.c.		Population		
	Coef.	St. Err.	Coef.	St. Err.	
-5	-0.062***	0.024	0.000	0.006	
-4	0.13*	0.079	0.002	0.007	
-3	-0.025	0.031	0.004	0.005	
-2	-0.098***	0.036	0.011	0.012	
-1	0.046	0.029	-0.007	0.009	
0	0.038	0.030	-0.005	0.009	
+1	0.005	0.044	0.006	0.013	
+2	-0.013	0.059	0.023	0.022	
+3	0.050	0.087	0.031	0.028	
+4	-0.016	0.092	0.031	0.031	
+5	-0.046	0.089	0.039	0.037	
+6	-0.053	0.110	0.044	0.044	
+7	-0.025	0.115	0.040	0.049	
+8	-0.224	0.185	0.046	0.063	
+9	-0.120	0.182	0.041	0.076	
+10	-0.345	0.290	0.106	0.084	
$\bar{DV}$ (IHS)		3.51	11.70		
$\bar{DV}$		16.71	60,286		
n		1,162	1,494		
Units		83	83		

Satisfied Municipalities					
Time	GDP p.c.		Population		
	Coef.	St. Err.	Coef.	St. Err.	
-5	-0.023	0.053	0.030	0.039	
-4	0.035	0.032	0.000	0.009	
-3	-0.024	0.037	0.003	0.008	
-2	0.06*	0.035	-0.012	0.009	
-1	-0.067**	0.033	0.002	0.012	
0	0.022	0.048	0.008*	0.004	
+1	0.089	0.081	0.001	0.009	
+2	0.066	0.092	-0.001	0.013	
+3	0.225	0.181	0.003	0.016	
+4	0.320	0.212	0.000	0.018	
+5	0.563*	0.299	-0.006	0.021	
+6	0.684**	0.278	-0.007	0.025	
+7	0.997***	0.210	-0.007	0.034	
+8	1.361***	0.301	-0.011	0.037	
+9	1.406***	0.348	-0.013	0.046	
+10	1.587***	0.458	-0.004	0.049	
$\bar{DV}$ (IHS)		3.35	11.62		
$\bar{DV}$		14.23	55,651		
n		994	1,278		
Units		71	71		

Tables report coefficient estimates and robust asymptotic standard errors for municipal expenditure and public employment outcomes for disappointed (top) and satisfied (bottom) samples. Disappointed municipalities received less than 40% of revenues expected from discovery announcements by 2017; satisfied municipalities received more than 40%. Never-treated control units are municipalities that received exploratory offshore wells in catchment zone after 1999, but no discoveries.  $\bar{DV}$  reports the mean of the dependent variable in period t-1. Specifications include municipality and year fixed effects and cluster standard errors at municipality-level. Estimates are generated using Callaway and Sant'Anna (2020) *csidid* estimator with bootstrapped standard errors (seed=39627236). Continuous outcome variables use inverse hyperbolic sine transformation. Monetary values are deflated to constant 2010 BRL. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table B8: Formal Private Sector Employment: ATT Estimates and Sample Characteristics

Disappointed Municipalities								
Time	Extractive		Manufacturing		Agriculture		Construction	
	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
-5	-0.422	0.294	-0.131**	0.059	-0.198	0.124	-0.070	0.173
-4	0.412*	0.251	-0.428	0.284	0.041	0.167	0.027	0.101
-3	-0.015	0.180	0.008	0.089	-0.239	0.148	0.189	0.194
-2	-0.155	0.268	-0.111	0.077	-0.060	0.061	0.151	0.148
-1	-0.090	0.215	-0.008	0.065	-0.118**	0.059	0.137	0.130
0	-0.128	0.274	-0.020	0.062	0.058	0.054	-0.201*	0.114
+1	-0.015	0.293	0.030	0.063	-0.064	0.073	-0.336**	0.139
+2	0.041	0.262	-0.039	0.092	-0.034	0.089	-0.504**	0.203
+3	-0.002	0.303	-0.046	0.105	0.100	0.110	-0.525**	0.228
+4	0.020	0.309	-0.142	0.145	0.077	0.127	-0.658***	0.255
+5	-0.076	0.366	-0.060	0.158	-0.025	0.123	-0.493**	0.203
+6	-0.021	0.376	-0.101	0.182	-0.169	0.132	-0.401*	0.226
+7	-0.148	0.385	-0.168	0.190	0.108	0.244	-0.694**	0.304
+8	0.079	0.431	-0.127	0.187	0.040	0.255	-0.408	0.274
+9	0.185	0.524	-0.098	0.213	-0.122	0.155	-0.454*	0.275
+10	0.043	0.656	-0.248	0.232	-0.006	0.198	-0.497*	0.280
$\bar{DV}$ (IHS)		3.42	7.21		5.98		7.39	
$\bar{DV}$		15.27	676.45		197.72		809.85	
n	1,494		1,494		1,494		1,494	
Units	83		83		83		83	

Satisfied Municipalities								
Time	Extractive		Manufacturing		Agriculture		Construction	
	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
-5	0.044	0.127	-0.012	0.094	-0.261*	0.148	-0.066	0.339
-4	0.033	0.128	-0.068	0.069	-0.061	0.146	0.355	0.294
-3	-0.092	0.581	0.055	0.101	0.057	0.224	-0.209	0.395
-2	-0.207	0.206	-0.018	0.068	-0.243	0.157	0.147	0.207
-1	0.100	0.182	-0.082	0.072	-0.027	0.068	-0.138	0.189
0	-0.022	0.093	-0.054	0.100	-0.029	0.093	-0.369	0.327
+1	-0.105	0.108	-0.100	0.114	-0.207	0.130	-0.057	0.241
+2	0.089	0.309	-0.066	0.145	-0.315*	0.185	-0.002	0.275
+3	0.189	0.261	0.025	0.161	-0.452***	0.163	0.129	0.278
+4	0.042	0.240	-0.077	0.162	-0.379**	0.161	0.209	0.333
+5	0.098	0.293	0.009	0.150	-0.295**	0.149	0.195	0.307
+6	-0.068	0.329	-0.003	0.161	-0.267	0.184	0.166	0.322
+7	0.131	0.433	-0.081	0.176	-0.48**	0.236	0.172	0.362
+8	0.106	0.479	-0.148	0.201	-0.819***	0.203	0.242	0.374
+9	0.786**	0.348	-0.086	0.265	-0.725***	0.201	-0.182	0.622
+10	0.679**	0.301	-0.255	0.213	-0.759***	0.208	0.227	0.480
$\bar{DV}$ (IHS)		3.94	6.83		5.86		6.75	
$\bar{DV}$		25.70	462.59		175.36		427.03	
n	1,278		1,278		1,278		1,278	
Units	71		71		71		71	

Tables report coefficient estimates and robust asymptotic standard errors for municipal expenditure and public employment outcomes for disappointed (top) and satisfied (bottom) samples. Disappointed municipalities received less than 40% of revenues expected from discovery announcements by 2017; satisfied municipalities received more than 40%. Never-treated control units are municipalities that received exploratory offshore wells in catchment zone after 1999, but no discoveries. Formal employees are calculated from RAIS linked employer-employee administrative records, with sectors defined according to Dahis (2020).  $\bar{DV}$  reports the mean of the dependent variable in period t-1. Specifications include municipality and year fixed effects and cluster standard errors at municipality-level. Estimates are generated using Callaway and Sant'Anna (2020) *csdid* estimator with bootstrapped standard errors (seed=39627236). Continuous outcome variables use inverse hyperbolic sine transformation. Monetary values are deflated to constant 2010 BRL. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table B9: Effects of Discovery Announcement on Winner Characteristics and Patronage

<b>Winner Characteristics</b>	Wells	Pre-Match
<i>Winners' Age</i>	0.118 (0.691)	0.045 (0.629)
<i>Winner Share Female</i>	0.011 (0.018)	0.008 (0.018)
<i>Winners' Avg. Schooling</i>	-0.150* (0.089)	-0.142* (0.076)
<b>Patronage (Mayors Only)</b>		
<i>No. Donors Hired to Commissioned Posts</i>	-0.013 (0.045)	-0.197 (0.222)
<i>Share of Donors Among Commissioned Hires</i>	0.000 (0.000)	0.000 (0.003)
<i>Share of Commissioned Hires Among Donors</i>	0.000 (0.001)	-0.007 (0.005)
<b>Patronage (All Politicians)</b>		
<i>No. Donors Hired to Commissioned Posts</i>	-0.039 (0.186)	0.137 (0.169)
<i>Share of Donors Among Commissioned Hires</i>	-0.002 (0.003)	-0.001 (0.003)
<i>Share of Commissioned Hires Among Donors</i>	-0.011 (0.007)	-0.008 (0.006)
Municipality FEs	Y	Y
Election Period FEs	Y	Y
n (municipality-election periods)	404	3,745

Table reports results from estimation of the following difference-in-differences specification:  $Y_{me} = \delta_m + \lambda_e + \beta T_{me} + \epsilon_{me}$ , where  $Y_{me}$  are outcomes measuring average characteristics of winning candidates (mayor and municipal council) and measures of patronage intensity,  $\delta_m$  and  $\lambda_e$  are municipality and election period FEs, and  $T_{me}$  is a binary treatment dummy that takes a value of 1 if a major offshore oil or gas discovery was announced during the previous four-year election period in a municipality  $m$ 's offshore catchment zone.  $T_{me}$  may turn on multiple times for a municipality. Standard errors are clustered at the municipality level in all specifications. Column 1 reports coefficient estimates and standard errors using a two-way fixed effects (TWFE) OLS estimator with the wells control group. Column 2 reports results using the TWFE estimator and treated and control groups matched on baseline characteristics. Measures of patronage intensity are generated by merging complete registries of campaign donors to successful municipal candidates (mayors or all politicians) with complete registries of formal employees from RAIS using unique stable ID number (CPFs). Using these merges, I keep instances in which campaign donors to successful candidates are hired during that candidate's term in office into a commissioned public job, which are filled at the discretion of local politicians. Three measures of patronage intensity are regressed on discovery treatment: (i) number of campaign donors to successful candidates who are hired to commissioned posts during those candidates' terms in office; (ii) share of total commissioned hires who were campaign donors; (iii) share of campaign donors who are hired to commissioned posts. Number of donors is transformed using the inverse hyperbolic sine transformation. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

## C Robustness Checks and Extensions

### C.1 Main Results: Robustness Across Samples and Estimators

Table C1: Treatment Effects (10 Yrs After Discovery) in **Disappointed** Municipalities

	CS Wells	CS Matched	TWFE Wells	TWFE Matched
<i>Total Revenue (Millions)</i>	-0.41*** (0.10)	-0.24*** (0.10)	-0.20** (0.08)	-0.07 (0.07)
<i>Revenue p.c.</i>	-0.52*** (0.16)	-0.46*** (0.17)	-0.26** (0.11)	-0.23** (0.10)
<i>Tax Revenue p.c.</i>	-0.27 (0.30)	-0.18 (0.27)	-0.35 (0.23)	-0.34* (0.18)
<i>Oil Revenue p.c.</i>	-0.03 (0.68)	0.46 (0.69)	0.16 (0.43)	0.50 (0.39)
<i>Transfer Revenue p.c.</i>	-0.14** (0.06)	-0.13*** (0.05)	-0.07* (0.04)	-0.06* (0.04)
<i>Total Spending (Millions)</i>	-0.32*** (0.09)	-0.10 (0.09)	-0.17** (0.07)	0.00 (0.07)
<i>Spending p.c.</i>	-0.43*** (0.12)	-0.32*** (0.13)	-0.23*** (0.08)	-0.14* (0.07)
<i>Investment p.c.</i>	-1.26*** (0.33)	-1.33*** (0.34)	-0.70** (0.28)	-0.80*** (0.26)
<i>Personnel Spending p.c.</i>	-0.50*** (0.13)	-0.35*** (0.14)	-0.26*** (0.09)	-0.16** (0.08)
<i>Education Spending p.c.</i>	-0.44*** (0.15)	-0.37*** (0.14)	-0.25** (0.10)	-0.19** (0.09)
<i>Health Spending p.c.</i>	-0.41*** (0.15)	-0.46*** (0.11)	-0.24* (0.12)	-0.33*** (0.11)
<i>Education Index</i>	-0.05*** (0.02)	-0.04*** (0.01)	-0.03* (0.02)	-0.01 (0.01)
<i>Health Index</i>	-0.21*** (0.02)	-0.17*** (0.01)	-0.09*** (0.03)	-0.03 (0.02)
<i>GDP p.c.</i>	-0.35 (0.29)	-0.24 (0.32)	-0.12 (0.17)	-0.12 (0.15)
<i>Population</i>	0.11 (0.08)	0.22*** (0.09)	0.05 (0.08)	0.14* (0.07)
# Extractive Employees	0.04 (0.66)	0.34 (0.69)	0.24 (0.52)	0.35 (0.49)
# Mfg. Employees	-0.25 (0.23)	0.28 (0.19)	-0.18 (0.21)	0.31** (0.17)
# Construct. Employees	-0.50* (0.28)	-0.46*** (0.21)	-0.79*** (0.28)	-0.56*** (0.18)
Avg. Formal Wage	-0.26* (0.15)	-0.04 (0.14)	-0.11** (0.05)	-0.02 (0.04)
n (municipality-years)	1,494	15,570	1,494	15,570

Note: Each column reports coefficient estimates and standard errors for the  $t + 10$  period of event studies of **disappointed** municipalities for a specific control group-estimator pair. Event study specifications include fully saturated relative time indicators and municipality and year fixed effects, and cluster standard errors at the municipality-level. Estimates for the  $t + 10$  period are reported since significant effects did not emerge immediately after discovery announcements, but rather grew cumulatively over time as offshore fields reached full production or disappointment was realized. Column 1 reports results from the preferred specification, which uses the [Callaway and Sant'Anna \(2020\)](#) (CS) *csd* estimator (with bootstrapped standard errors, seed=39627236) and municipalities that had offshore exploratory wells drilled since 2000, but no discoveries, as a control group. Column 2 reports results using the CS estimator and a control group matched with disappointed municipalities on baseline characteristics using Coarsened Exact Matching. Columns 3 and 4 report results from wells and matched control groups using the two-way fixed effects (TWFE) OLS estimator. Monetary values are deflated to constant 2010 BRL\$. All outcomes are transformed using the inverse hyperbolic sine transformation. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

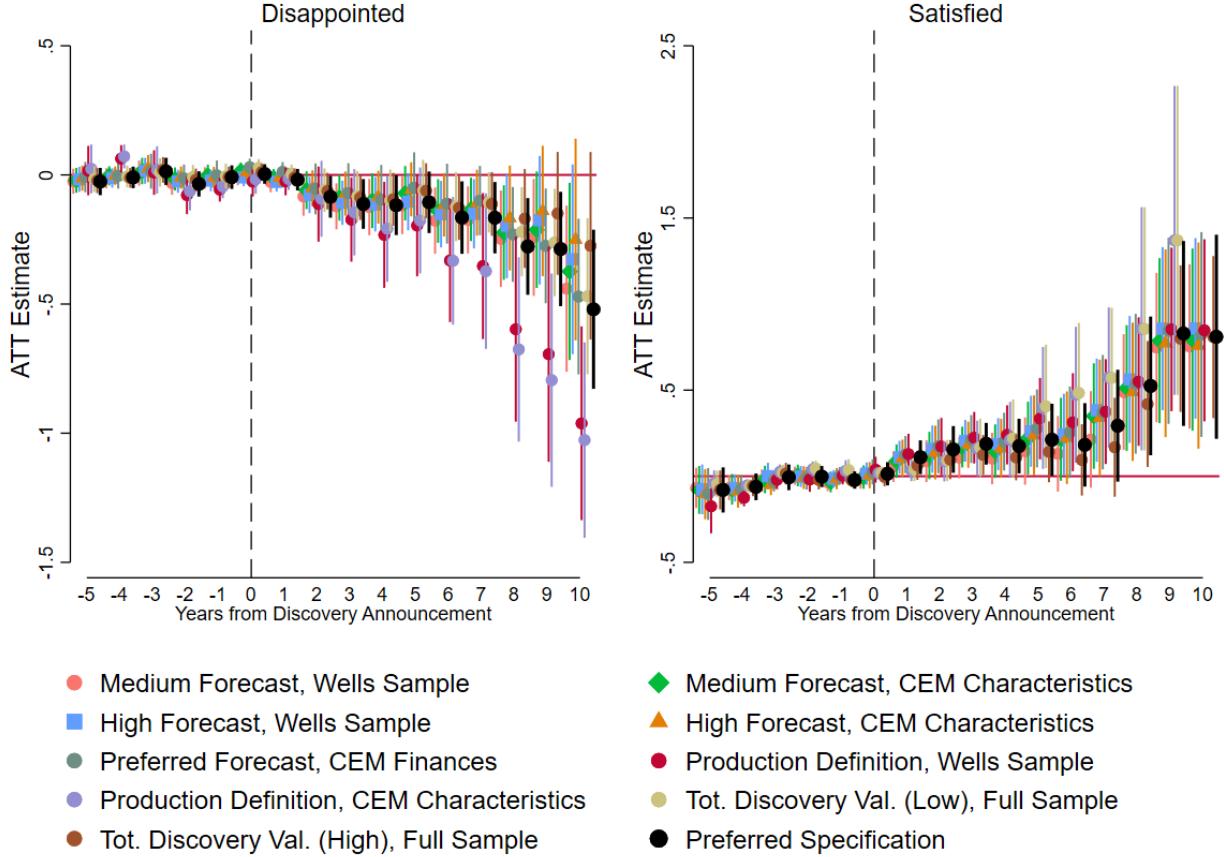
Table C2: Treatment Effects (10 Yrs After Discovery) in **Satisfied** Municipalities

	CS Wells	CS Matched	TWFE Wells	TWFE Matched
<i>Total Revenue (Millions)</i>	0.76*** (0.24)	0.92*** (0.27)	0.65*** (0.20)	0.83*** (0.19)
<i>Revenue p.c.</i>	0.75*** (0.24)	0.86*** (0.27)	0.66*** (0.20)	0.77*** (0.19)
<i>Tax Revenue p.c.</i>	0.01 (0.29)	0.27 (0.27)	-0.21 (0.30)	0.07 (0.26)
<i>Oil Revenue p.c.</i>	4.69*** (0.89)	5.32*** (0.94)	4.35*** (0.68)	4.49*** (0.69)
<i>Transfer Revenue p.c.</i>	0.05 (0.05)	0.09* (0.05)	0.04 (0.05)	0.08 (0.05)
<i>Total Spending (Millions)</i>	0.28*** (0.11)	0.45*** (0.11)	0.24** (0.12)	0.43*** (0.11)
<i>Spending p.c.</i>	0.28*** (0.11)	0.37*** (0.11)	0.25** (0.12)	0.38*** (0.11)
<i>Investment p.c.</i>	1.47** (0.70)	1.42* (0.79)	0.82 (0.71)	0.92 (0.72)
<i>Personnel Spending p.c.</i>	0.28** (0.12)	0.42*** (0.12)	0.19* (0.11)	0.32*** (0.10)
<i>Education Spending p.c.</i>	0.38*** (0.13)	0.43*** (0.10)	0.35* (0.20)	0.41** (0.19)
<i>Health Spending p.c.</i>	0.46** (0.21)	0.30* (0.22)	0.34 (0.23)	0.31 (0.19)
<i>Education Index</i>	-0.09*** (0.02)	-0.08*** (0.00)	-0.04* (0.02)	0.00 (0.02)
<i>Health Index</i>	-0.14*** (0.03)	-0.11*** (0.02)	-0.07* (0.03)	0.02 (0.03)
<i>GDP p.c.</i>	1.59*** (0.46)	1.75*** (0.58)	1.42*** (0.31)	1.51*** (0.30)
<i>Population</i>	-0.004 (0.05)	0.06 (0.03)	-0.01 (0.05)	0.06 (0.04)
# Extractive Employees	0.68** (0.30)	0.85*** (0.26)	0.62* (0.29)	0.87*** (0.23)
# Mfg. Employees	-0.26 (0.21)	0.18 (0.19)	-0.21** (0.22)	0.25 (0.17)
# Construct. Employees	0.23 (0.48)	0.19 (0.49)	0.07 (0.39)	0.17 (0.33)
Avg. Formal Wage	-0.14 (0.16)	0.09 (0.16)	-0.09 (0.05)	0.00 (0.04)
n (municipality-years)	1,278	9,012	1,278	9,012

Note: Each column reports coefficient estimates and standard errors for the  $t + 10$  period of event studies of **satisfied** municipalities for a specific control group-estimator pair. Event study specifications include fully saturated relative time indicators and municipality and year fixed effects, and cluster standard errors at the municipality-level. Estimates for the  $t + 10$  period are reported since significant effects did not emerge immediately after discovery announcements, but rather grew cumulatively over time as offshore fields reached full production or disappointment was realized. Column 1 reports results from the preferred specification, which uses the Callaway and Sant'Anna (2020) (CS) *csdid* estimator (with bootstrapped standard errors, seed=39627236) and municipalities that had offshore exploratory wells drilled since 2000, but no discoveries, as a control group. Column 2 reports results using the CS estimator and a control group matched with satisfied municipalities on baseline characteristics using Coarsened Exact Matching. Columns 3 and 4 report results from wells and matched control groups using the two-way fixed effects (TWFE) OLS estimator. Monetary values are deflated to constant 2010 BRL\$. All outcomes are transformed using the inverse hyperbolic sine transformation. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

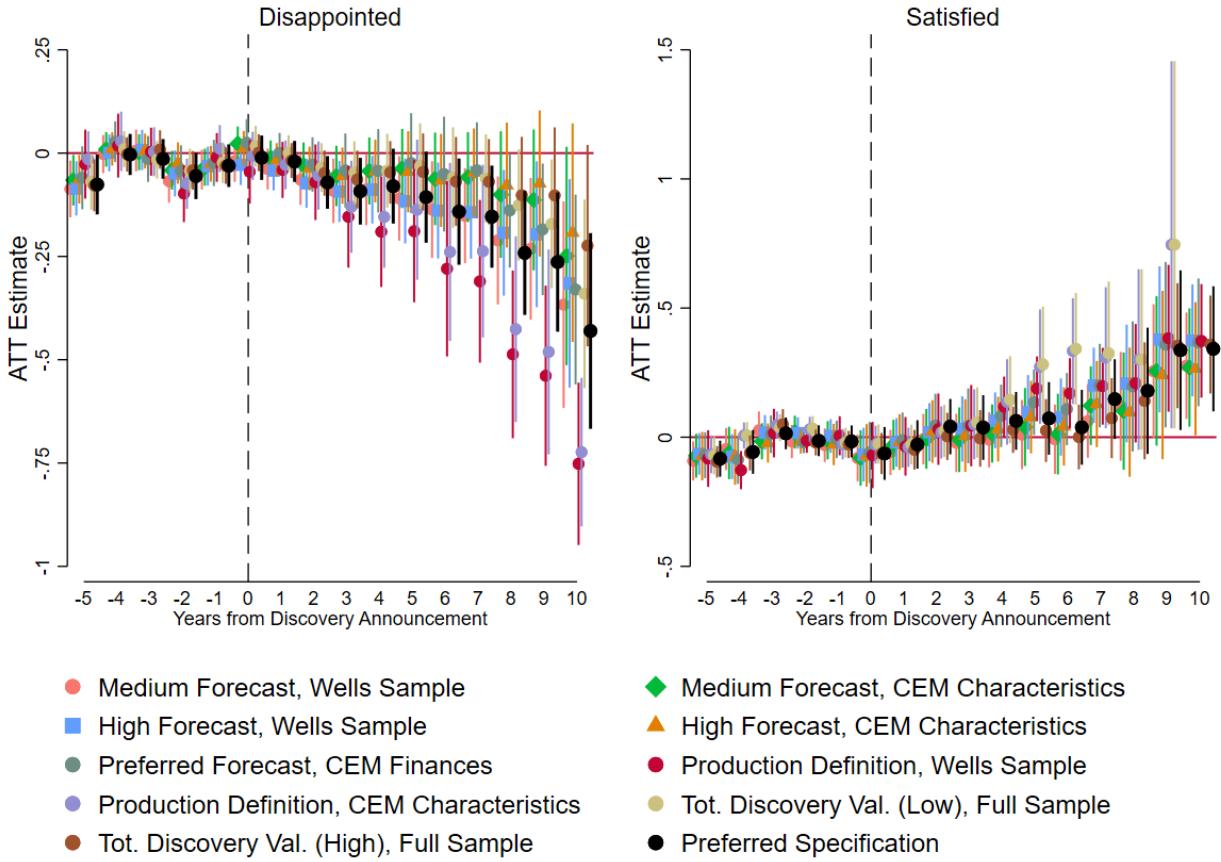
## C.2 Robustness to Alternative Forecasting and Matching Parameters

Figure C1: Robustness: Revenues per capita



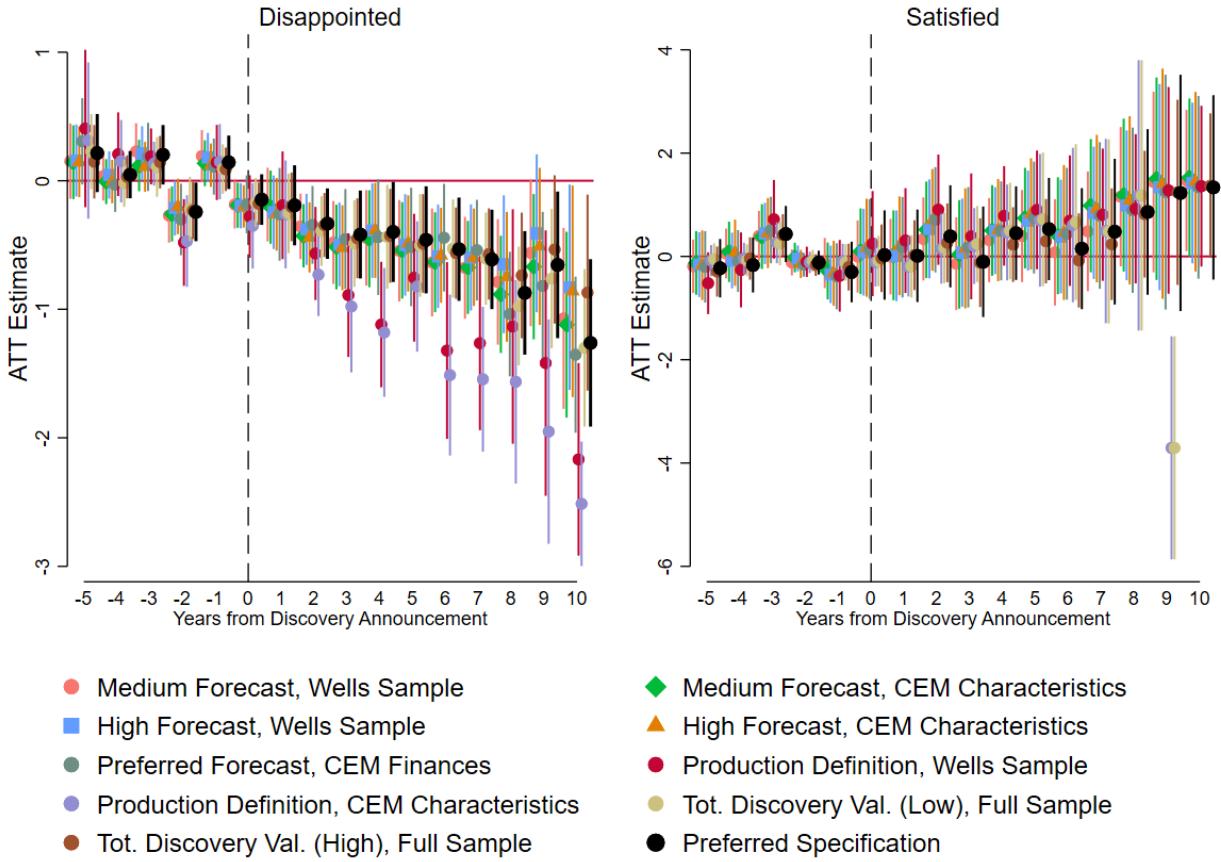
Note: Event studies are estimated separately using Callaway and Sant'Anna (2020) estimator with municipality and year FEs and standard errors clustered at municipality-level, and plotted together for visual comparison. The **Preferred Specification** (also reported in main results section) uses conservative low forecasts of per capita discovery expectations to categorize disappointed and satisfied municipalities, and compares treated places to the Wells control group, which received offshore exploratory drilling but no discoveries during study period. **Medium Forecast, Wells Sample** and **Medium Forecast, CEM Characteristics** use more optimistic forecasting parameters to categorize disappointed and satisfied municipalities, and compare treated units to Wells control group and never-treated municipalities matched on baseline characteristics using Coarsened Exact Matching (CEM) procedure, respectively. **High Forecast, Wells Sample** and **High Forecast, CEM Characteristics** use even more optimistic forecasting parameters to categorize disappointed and satisfied treatment groups. Municipality classifications using these alternative forecasting parameters are reported in Appendix B2. **Preferred Forecast, CEM Finances** uses main forecasting parameters and a control group of never-treated municipalities that match treated units on baseline public finance variables (outcomes). **Production Definition, Wells Sample** and **Production Definition, CEM Characteristics** use an alternative categorization of discovery-treated municipalities into disappointed and satisfied, wherein satisfied municipalities are those that produce more than twice as much oil equivalent in 2017 as they did at the time of discovery announcement; disappointed municipalities are those that produce less than twice as much. This definition avoids assumptions built into the expectations forecasting model. **Total Discovery Value (Low), Full Sample** and **Total Discovery Value (High), Full Sample** use total, rather than per capita discovery volume announcements to compute forecasts, with the former using conservative low forecasts and the latter using optimistic high forecasts, and use the full sample of all municipalities in coastal states as a control group. Revenues per capita are transformed using inverse hyperbolic sine and deflated to constant 2010 BRL.

Figure C2: Robustness: Expenditures per capita



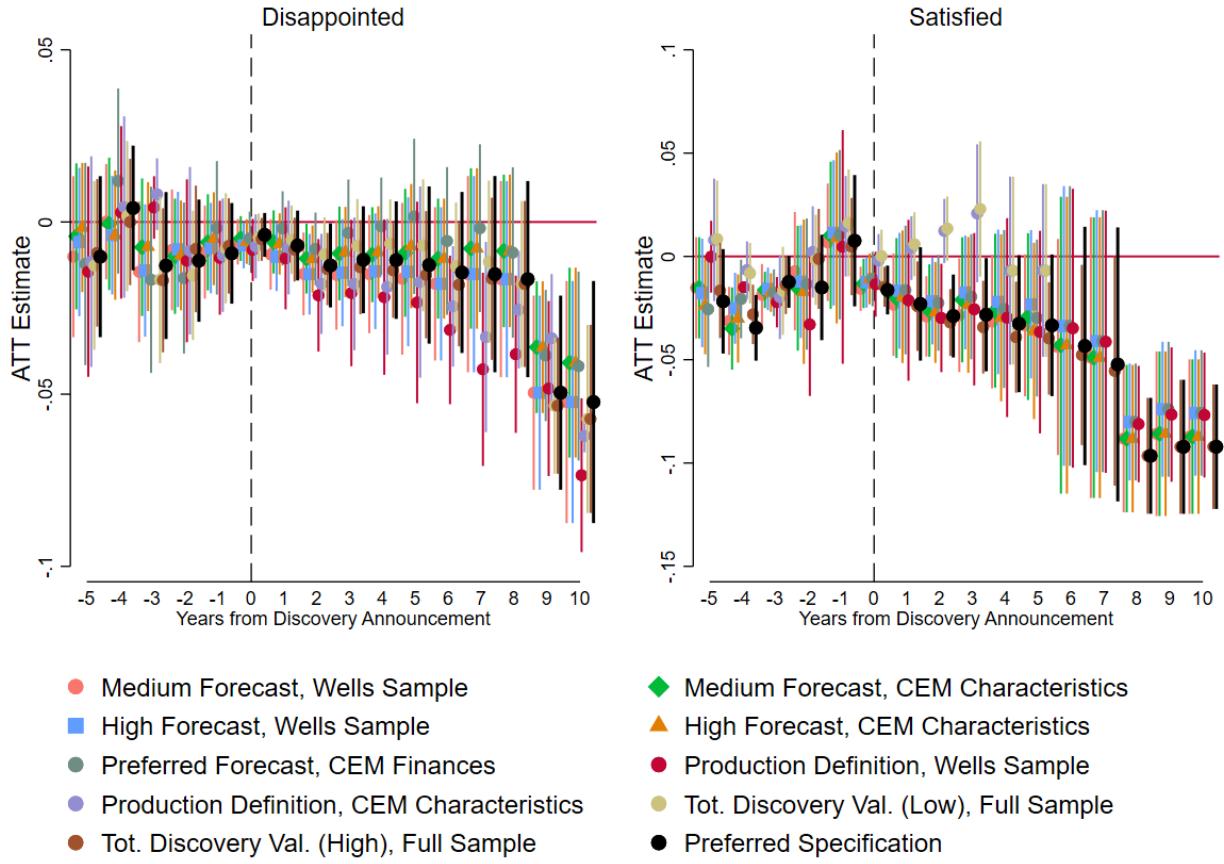
Note: Event studies are estimated separately using [Callaway and Sant'Anna \(2020\)](#) estimator with municipality and year FE and standard errors clustered at municipality-level, and plotted together for visual comparison. The **Preferred Specification** (also reported in main results section) uses conservative low forecasts of per capita discovery expectations to categorize disappointed and satisfied municipalities, and compares treated places to the Wells control group, which received offshore exploratory drilling but no discoveries during study period. **Medium Forecast, Wells Sample** and **Medium Forecast, CEM Characteristics** use more optimistic forecasting parameters to categorize disappointed and satisfied municipalities, and compare treated units to Wells control group and never-treated municipalities matched on baseline characteristics using Coarsened Exact Matching (CEM) procedure, respectively. **High Forecast, Wells Sample** and **High Forecast, CEM Characteristics** use even more optimistic forecasting parameters to categorize disappointed and satisfied treatment groups. Municipality classifications using these alternative forecasting parameters are reported in Appendix B2. **Preferred Forecast, CEM Finances** uses main forecasting parameters and a control group of never-treated municipalities that match treated units on baseline public finance variables (outcomes). **Production Definition, Wells Sample** and **Production Definition, CEM Characteristics** use an alternative categorization of discovery-treated municipalities into disappointed and satisfied, wherein satisfied municipalities are those that produce more than twice as much oil equivalent in 2017 as they did at the time of discovery announcement; disappointed municipalities are those that produce less than twice as much. This definition avoids assumptions built into the expectations forecasting model. **Total Discovery Value (Low), Full Sample** and **Total Discovery Value (High), Full Sample** use total, rather than per capita discovery volume announcements to compute forecasts, with the former using conservative low forecasts and the latter using optimistic high forecasts, and use the full sample of all municipalities in coastal states as a control group. Expenditures per capita are transformed using inverse hyperbolic sine and deflated to constant 2010 BRL.

Figure C3: Robustness: Investment per capita



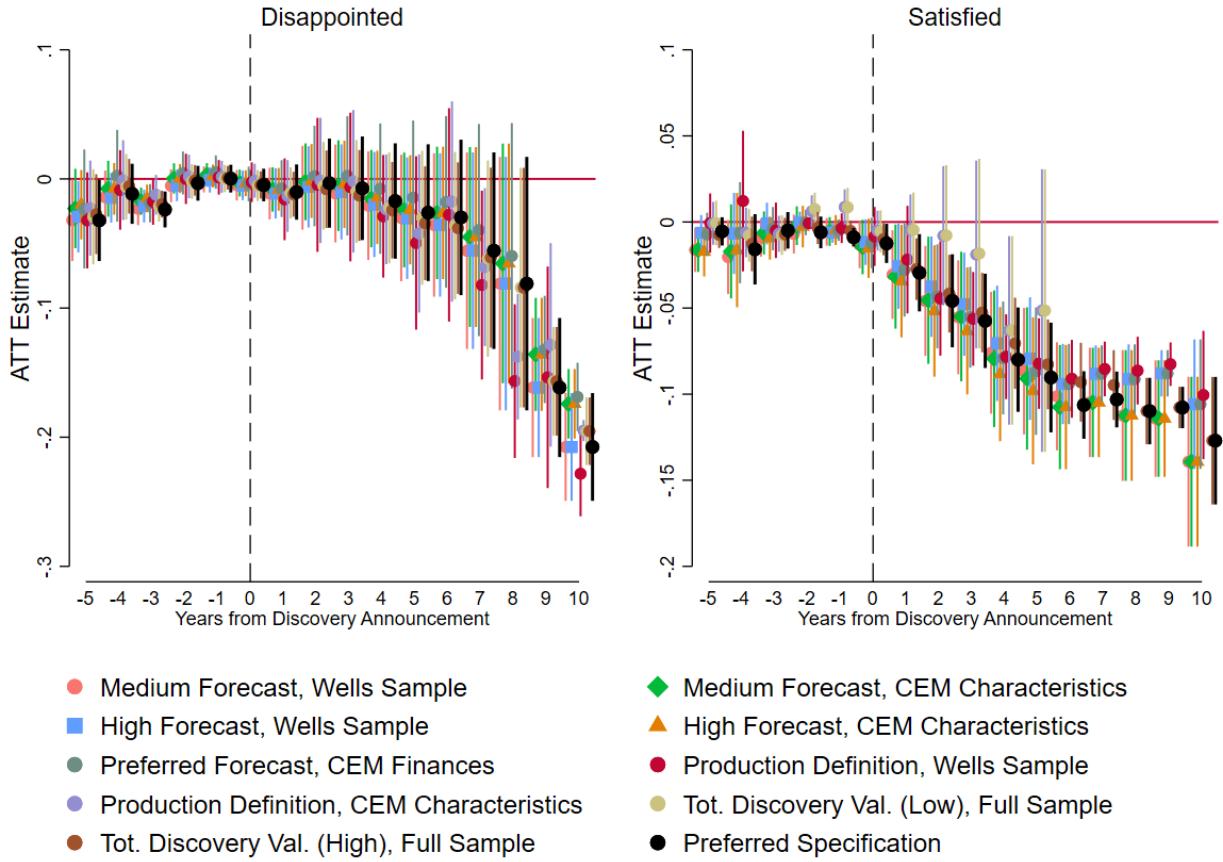
Note: Event studies are estimated separately using [Callaway and Sant'Anna \(2020\)](#) estimator with municipality and year FEs and standard errors clustered at municipality-level, and plotted together for visual comparison. The **Preferred Specification** (also reported in main results section) uses conservative low forecasts of per capita discovery expectations to categorize disappointed and satisfied municipalities, and compares treated places to the Wells control group, which received offshore exploratory drilling but no discoveries during study period. **Medium Forecast, Wells Sample** and **Medium Forecast, CEM Characteristics** use more optimistic forecasting parameters to categorize disappointed and satisfied municipalities, and compare treated units to Wells control group and never-treated municipalities matched on baseline characteristics using Coarsened Exact Matching (CEM) procedure, respectively. **High Forecast, Wells Sample** and **High Forecast, CEM Characteristics** use even more optimistic forecasting parameters to categorize disappointed and satisfied treatment groups. Municipality classifications using these alternative forecasting parameters are reported in Appendix B2. **Preferred Forecast, CEM Finances** uses main forecasting parameters and a control group of never-treated municipalities that match treated units on baseline public finance variables (outcomes). **Production Definition, Wells Sample** and **Production Definition, CEM Characteristics** use an alternative categorization of discovery-treated municipalities into disappointed and satisfied, wherein satisfied municipalities are those that produce more than twice as much oil equivalent in 2017 as they did at the time of discovery announcement; disappointed municipalities are those that produce less than twice as much. This definition avoids assumptions built into the expectations forecasting model. **Total Discovery Value (Low), Full Sample** and **Total Discovery Value (High), Full Sample** use total, rather than per capita discovery volume announcements to compute forecasts, with the former using conservative low forecasts and the latter using optimistic high forecasts, and use the full sample of all municipalities in coastal states as a control group. Municipal public investment per capita is transformed using inverse hyperbolic sine and deflated to constant 2010 BRL.

Figure C4: Robustness: Education Provision and Outcomes



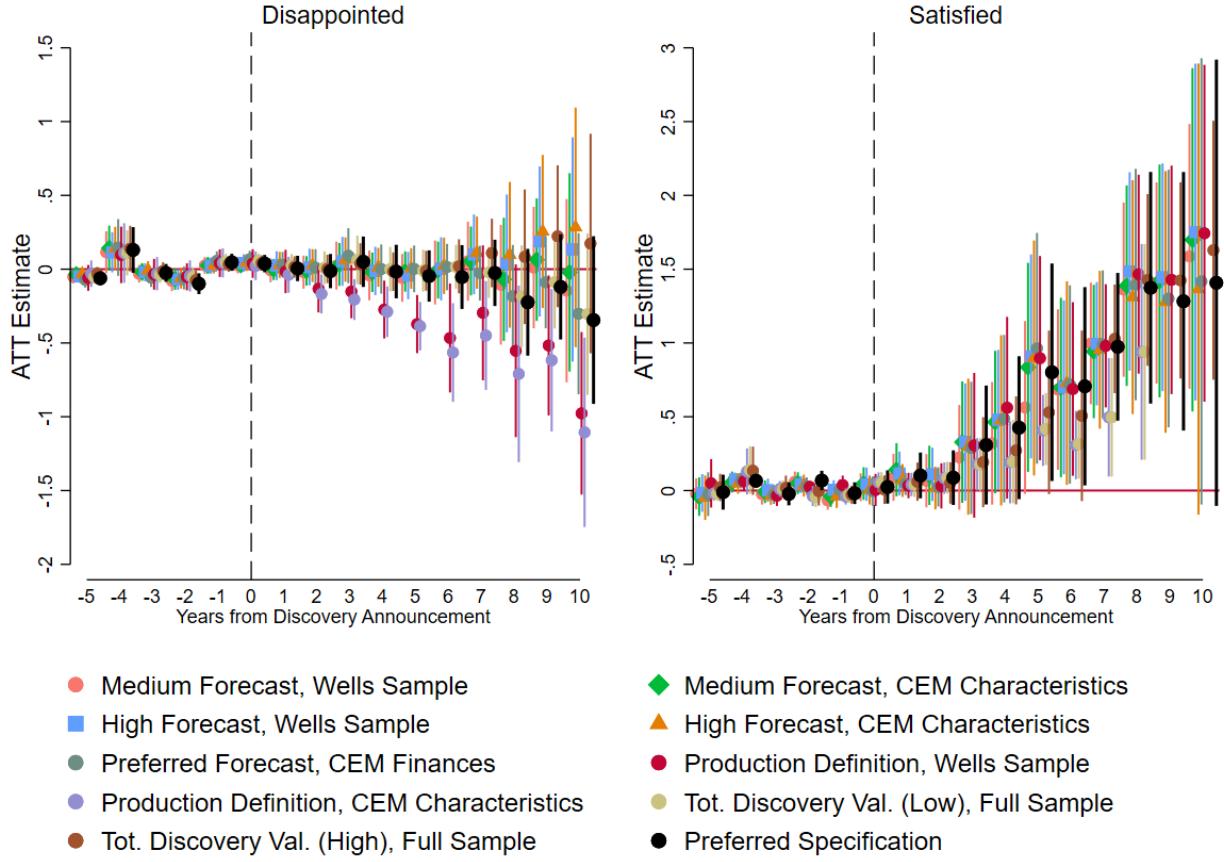
Note: Event studies are estimated separately using [Callaway and Sant'Anna \(2020\)](#) estimator with municipality and year FE and standard errors clustered at municipality-level, and plotted together for visual comparison. The **Preferred Specification** (also reported in main results section) uses conservative low forecasts of per capita discovery expectations to categorize disappointed and satisfied municipalities, and compares treated places to the Wells control group, which received offshore exploratory drilling but no discoveries during study period. **Medium Forecast, Wells Sample** and **Medium Forecast, CEM Characteristics** use more optimistic forecasting parameters to categorize disappointed and satisfied municipalities, and compare treated units to Wells control group and never-treated municipalities matched on baseline characteristics using Coarsened Exact Matching (CEM) procedure, respectively. **High Forecast, Wells Sample** and **High Forecast, CEM Characteristics** use even more optimistic forecasting parameters to categorize disappointed and satisfied treatment groups. Municipality classifications using these alternative forecasting parameters are reported in Appendix B2. **Preferred Forecast, CEM Finances** uses main forecasting parameters and a control group of never-treated municipalities that match treated units on baseline public finance variables (outcomes). **Production Definition, Wells Sample** and **Production Definition, CEM Characteristics** use an alternative categorization of discovery-treated municipalities into disappointed and satisfied, wherein satisfied municipalities are those that produce more than twice as much oil equivalent in 2017 as they did at the time of discovery announcement; disappointed municipalities are those that produce less than twice as much. This definition avoids assumptions built into the expectations forecasting model. **Total Discovery Value (Low), Full Sample** and **Total Discovery Value (High), Full Sample** use total, rather than per capita discovery volume announcements to compute forecasts, with the former using conservative low forecasts and the latter using optimistic high forecasts, and use the full sample of all municipalities in coastal states as a control group. Education provision and outcomes are measured using an index provided by [FIRJAN \(2019\)](#). The Education Index is an aggregate score ranging from 0-1, composed of the following indicators: early childhood enrollment rates, graduation rates, grade-age distortion, hours spent in class, share of teachers with college degrees, and IDEB test scores.

Figure C5: Robustness: Health Provision and Outcomes



Note: Event studies are estimated separately using [Callaway and Sant'Anna \(2020\)](#) estimator with municipality and year FEs and standard errors clustered at municipality-level, and plotted together for visual comparison. The **Preferred Specification** (also reported in main results section) uses conservative low forecasts of per capita discovery expectations to categorize disappointed and satisfied municipalities, and compares treated places to the Wells control group, which received offshore exploratory drilling but no discoveries during study period. **Medium Forecast, Wells Sample** and **Medium Forecast, CEM Characteristics** use more optimistic forecasting parameters to categorize disappointed and satisfied municipalities, and compare treated units to Wells control group and never-treated municipalities matched on baseline characteristics using Coarsened Exact Matching (CEM) procedure, respectively. **High Forecast, Wells Sample** and **High Forecast, CEM Characteristics** use even more optimistic forecasting parameters to categorize disappointed and satisfied treatment groups. Municipality classifications using these alternative forecasting parameters are reported in Appendix B2. **Preferred Forecast, CEM Finances** uses main forecasting parameters and a control group of never-treated municipalities that match treated units on baseline public finance variables (outcomes). **Production Definition, Wells Sample** and **Production Definition, CEM Characteristics** use an alternative categorization of discovery-treated municipalities into disappointed and satisfied, wherein satisfied municipalities are those that produce more than twice as much oil equivalent in 2017 as they did at the time of discovery announcement; disappointed municipalities are those that produce less than twice as much. This definition avoids assumptions built into the expectations forecasting model. **Total Discovery Value (Low), Full Sample** and **Total Discovery Value (High), Full Sample** use total, rather than per capita discovery volume announcements to compute forecasts, with the former using conservative low forecasts and the latter using optimistic high forecasts, and use the full sample of all municipalities in coastal states as a control group. Education provision and outcomes are measured using an index provided by [FIRJAN \(2019\)](#). The Health Index is an aggregate score ranging from 0-1, composed of the following indicators: proportion of pregnant women receiving >7 pre-natal visits, deaths of undefined causes, and avoidable infant mortality.

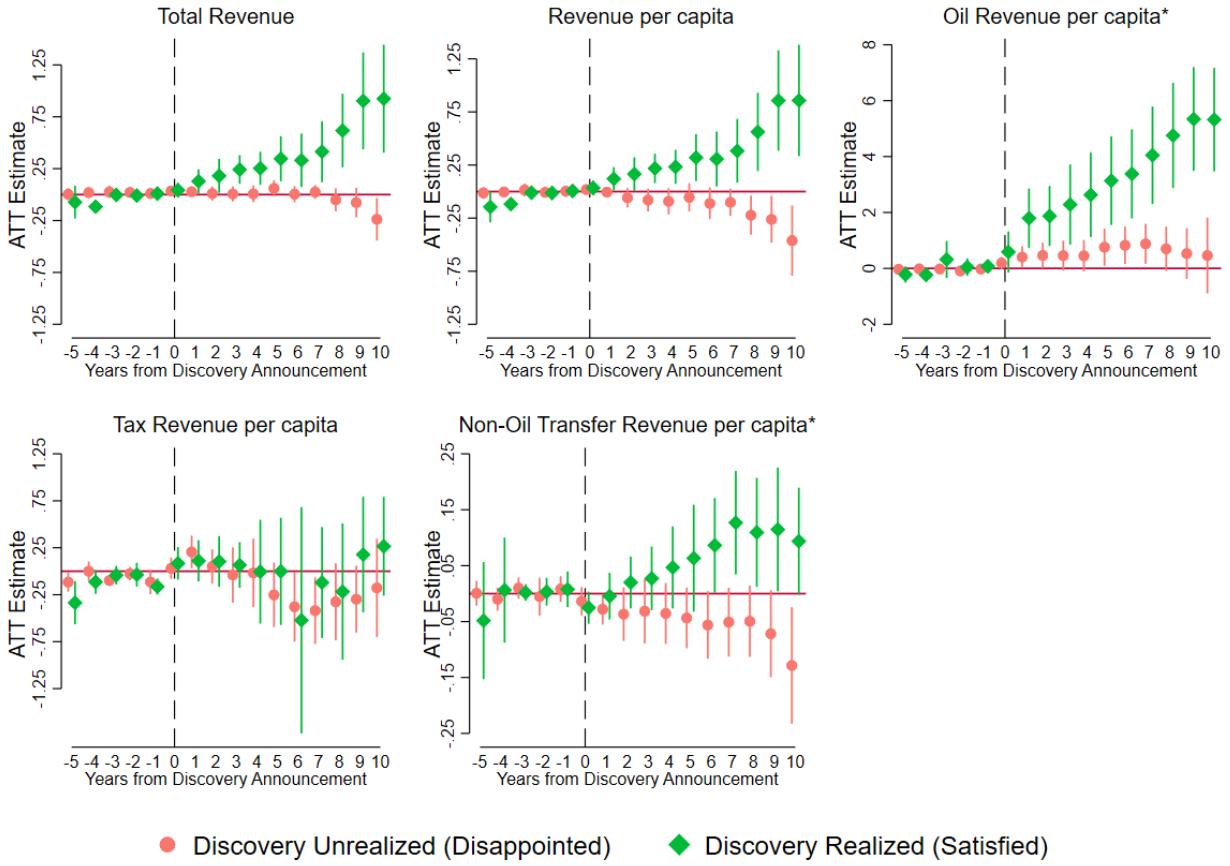
Figure C6: Robustness: GDP



Note: Event studies are estimated separately using [Callaway and Sant'Anna \(2020\)](#) estimator with municipality and year FEs and standard errors clustered at municipality-level, and plotted together for visual comparison. The **Preferred Specification** (also reported in main results section) uses conservative low forecasts of per capita discovery expectations to categorize disappointed and satisfied municipalities, and compares treated places to the Wells control group, which received offshore exploratory drilling but no discoveries during study period. **Medium Forecast, Wells Sample** and **Medium Forecast, CEM Characteristics** use more optimistic forecasting parameters to categorize disappointed and satisfied municipalities, and compare treated units to Wells control group and never-treated municipalities matched on baseline characteristics using Coarsened Exact Matching (CEM) procedure, respectively. **High Forecast, Wells Sample** and **High Forecast, CEM Characteristics** use even more optimistic forecasting parameters to categorize disappointed and satisfied treatment groups. Municipality classifications using these alternative forecasting parameters are reported in Appendix B2. **Preferred Forecast, CEM Finances** uses main forecasting parameters and a control group of never-treated municipalities that match treated units on baseline public finance variables (outcomes). **Production Definition, Wells Sample** and **Production Definition, CEM Characteristics** use an alternative categorization of discovery-treated municipalities into disappointed and satisfied, wherein satisfied municipalities are those that produce more than twice as much oil equivalent in 2017 as they did at the time of discovery announcement; disappointed municipalities are those that produce less than twice as much. This definition avoids assumptions built into the expectations forecasting model. **Total Discovery Value (Low), Full Sample** and **Total Discovery Value (High), Full Sample** use total, rather than per capita discovery volume announcements to compute forecasts, with the former using conservative low forecasts and the latter using optimistic high forecasts, and use the full sample of all municipalities in coastal states as a control group. Municipal GDP per capita is transformed using inverse hyperbolic sine and deflated to constant 2010 BRL.

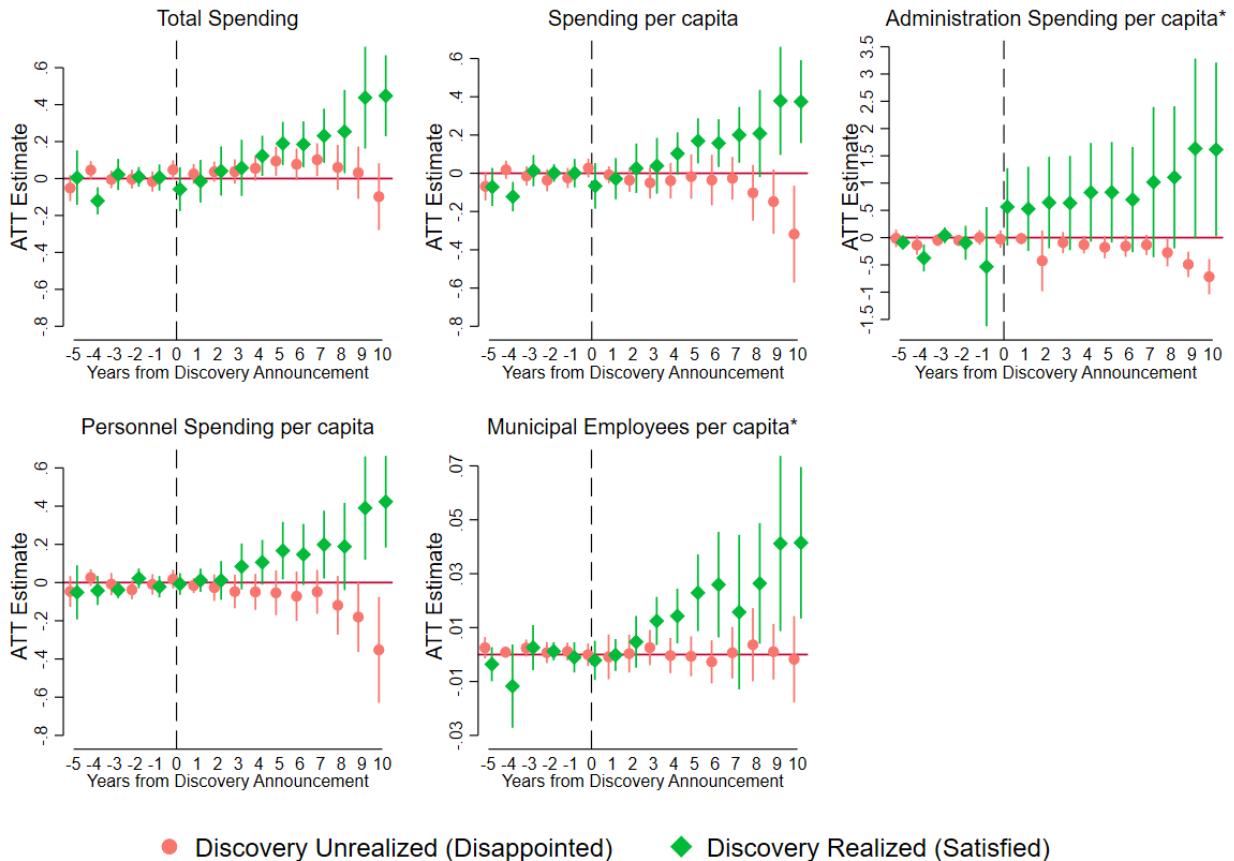
### C.3 Event Studies with Matched Controls

Figure C7: Revenues



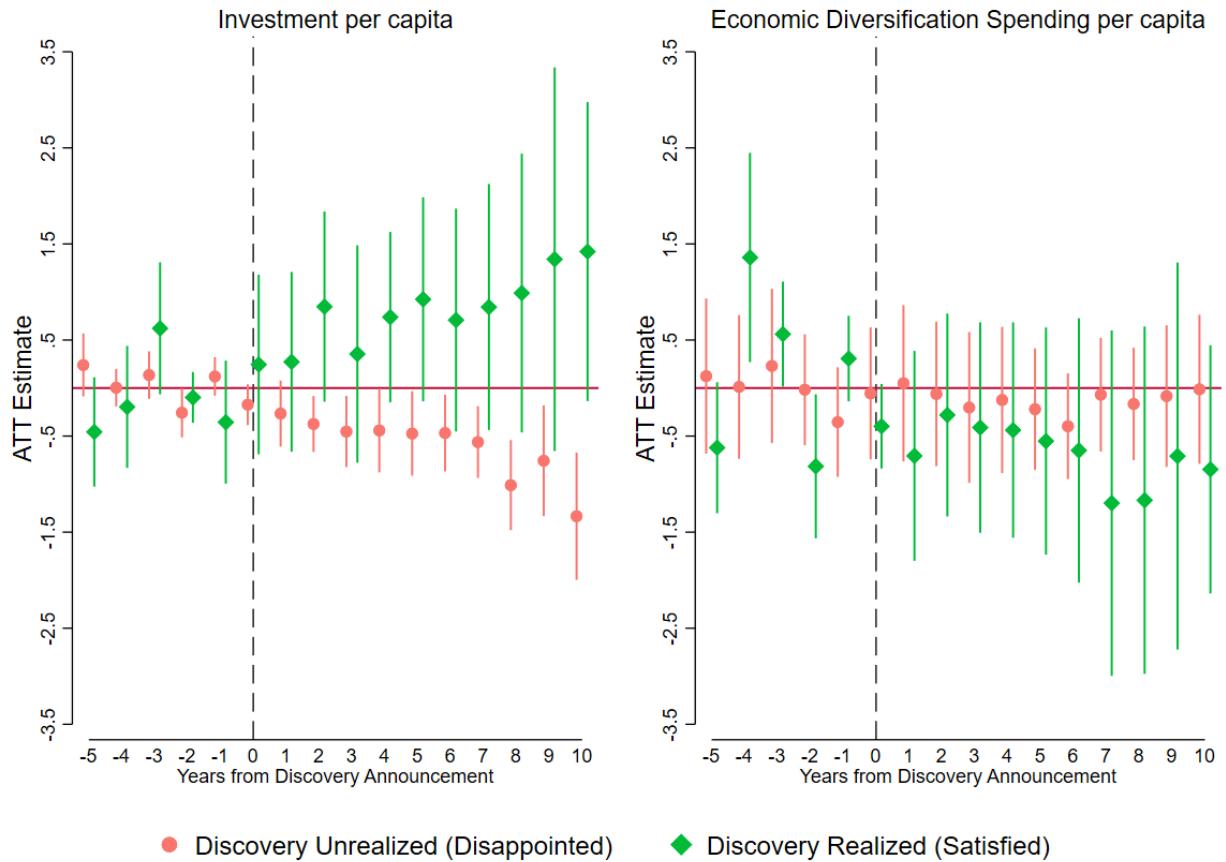
Note: For all of the following figures, event studies are estimated separately for Disappointed and Satisfied municipalities relative to Matched Controls—identified using Coarsened Exact Matching on pre-treatment GDP, population, distance from state capital, latitude, and municipal development index—and superimposed on the same graph for visual comparison. Event study specifications include municipal and year fixed effects and are estimated using Callaway and Sant'Anna (2020) *csdid* estimator. Continuous outcomes are transformed using inverse hyperbolic sine transformation. Monetary values are deflated to constant 2010 BRL. Standard errors are clustered at the municipality level and 95% confidence intervals are reported. \*Asterisks indicate that a different y-axis scale is used from the rest of the sub-figures, in order to accommodate large differences in scale of effects.

Figure C8: Expenditures and Employment



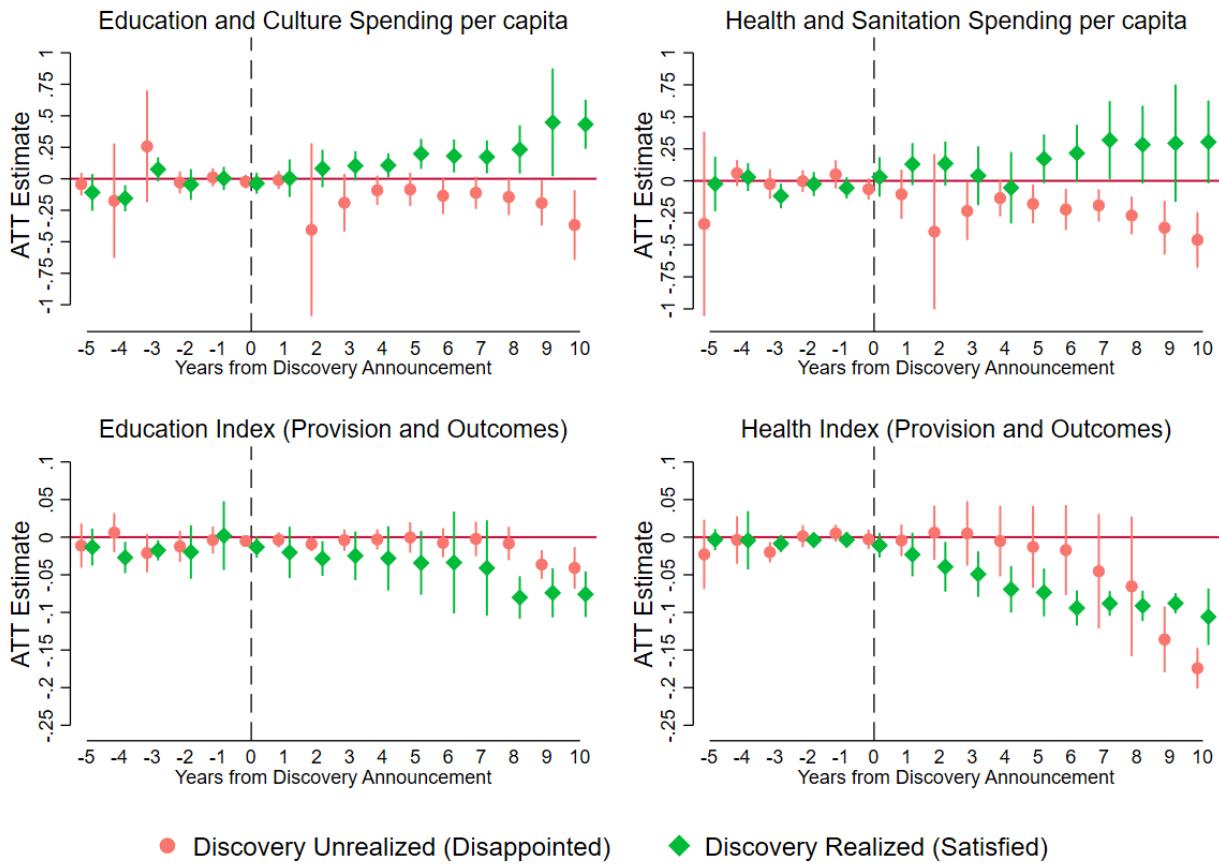
Note: For all of the following figures, event studies are estimated separately for Disappointed and Satisfied municipalities relative to Matched Controls—identified using Coarsened Exact Matching on pre-treatment GDP, population, distance from state capital, latitude, and municipal development index—and superimposed on the same graph for visual comparison. Event study specifications include municipal and year fixed effects and are estimated using Callaway and Sant'Anna (2020) *csdid* estimator. Continuous outcomes are transformed using inverse hyperbolic sine transformation. Monetary values are deflated to constant 2010 BRL. Standard errors are clustered at the municipality level and 95% confidence intervals are reported. \*Asterisks indicate that a different y-axis scale is used from the rest of the sub-figures, in order to accommodate large differences in scale of effects.

Figure C9: Public Investment and Economic Diversification



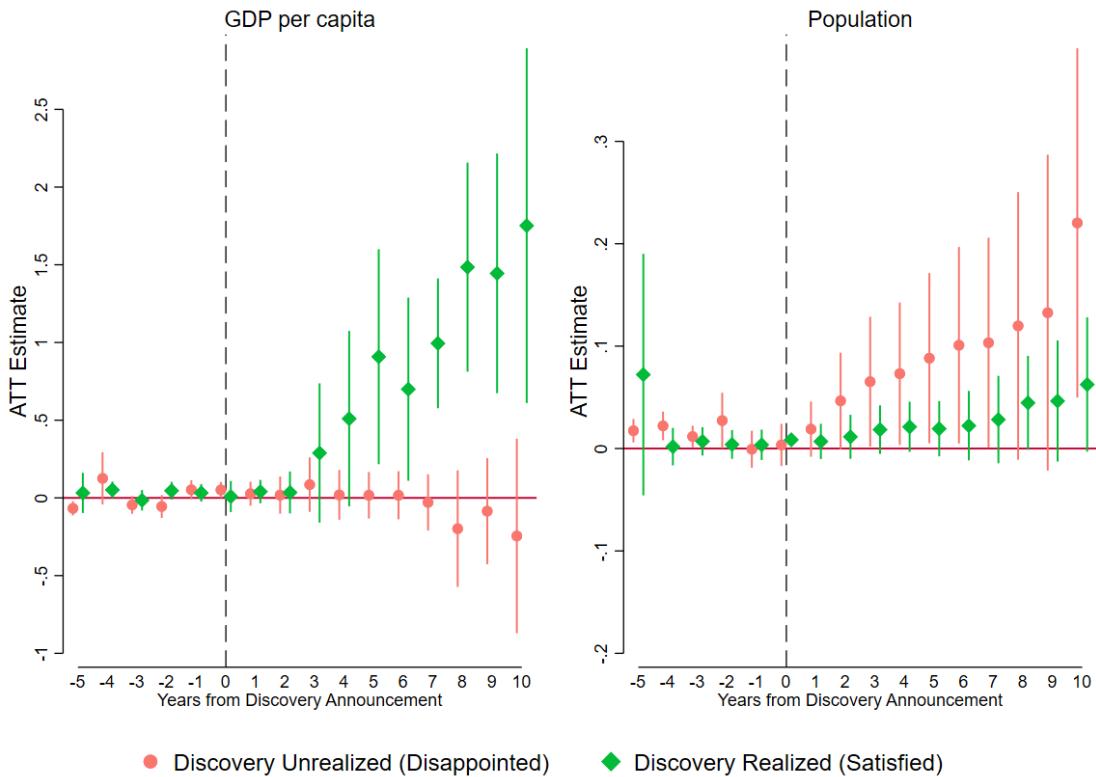
Note: For all of the following figures, event studies are estimated separately for Disappointed and Satisfied municipalities relative to Matched Controls—identified using Coarsened Exact Matching on pre-treatment GDP, population, distance from state capital, latitude, and municipal development index—and superimposed on the same graph for visual comparison. Event study specifications include municipal and year fixed effects and are estimated using Callaway and Sant'Anna (2020) *csdid* estimator. Continuous outcomes are transformed using inverse hyperbolic sine transformation. Monetary values are deflated to constant 2010 BRL. Standard errors are clustered at the municipality level and 95% confidence intervals are reported. \*Asterisks indicate that a different y-axis scale is used from the rest of the sub-figures, in order to accommodate large differences in scale of effects. Investment refers to public municipal investment (e.g., infrastructure). Economic development spending is the sum of municipal spending to promote industry, services, and agriculture.

Figure C10: Public Goods Spending & Performance Indices



Note: For all of the following figures, event studies are estimated separately for Disappointed and Satisfied municipalities relative to Matched Controls—identified using Coarsened Exact Matching on pre-treatment GDP, population, distance from state capital, latitude, and municipal development index—and superimposed on the same graph for visual comparison. Event study specifications include municipal and year fixed effects and are estimated using Callaway and Sant'Anna (2020) *csdid* estimator. Continuous outcomes are transformed using inverse hyperbolic sine transformation. Monetary values are deflated to constant 2010 BRL. Standard errors are clustered at the municipality level and 95% confidence intervals are reported. \*Asterisks indicate that a different y-axis scale is used from the rest of the sub-figures, in order to accommodate large differences in scale of effects. Education and Health Indices are drawn from the FIRJAN Municipal Development Index (FIRJAN, 2020), a comprehensive measure of municipal development published annually by FIRJAN, a nonprofit. The Education Index is an aggregate score ranging from 0-1, composed of the following indicators: early childhood enrollment rates, graduation rates, grade-age distortion, hours spent in class, share of teachers with college degrees, and IDEB test scores. The Health Index is an aggregate score ranging from 0-1, composed of the following indicators: proportion of pregnant women receiving >7 pre-natal visits, deaths of undefined causes, and avoidable infant mortality.

Figure C11: GDP & Population



Note: For all of the following figures, event studies are estimated separately for Disappointed and Satisfied municipalities relative to Matched Controls—identified using Coarsened Exact Matching on pre-treatment GDP, population, distance from state capital, latitude, and municipal development index—and superimposed on the same graph for visual comparison. Event study specifications include municipal and year fixed effects and are estimated using [Callaway and Sant'Anna \(2020\)](#) *csdid* estimator. Continuous outcomes are transformed using inverse hyperbolic sine transformation. Monetary values are deflated to constant 2010 BRL. Standard errors are clustered at the municipality level and 95% confidence intervals are reported. \*Asterisks indicate that a different y-axis scale is used from the rest of the sub-figures, in order to accommodate large differences in scale of effects.

## C.4 Event Studies with Multiple Events

Following the method proposed in [Sandler and Sandler \(2014\)](#), I estimate an event study specification that is identical to my preferred specification (including coarsened exact matched controls and fully saturated relative year indicators), with the inclusion of relative time dummies for each discovery announcement that occurred within a municipality between 2002 and 2017, rather than time indicators relative to only the first discovery. I report results from this alternative specification in Figures I1-I3. Results remain relatively similar to those found when focusing only on first events.

Figure C12: Event Study with Multiple Events: Public Finances

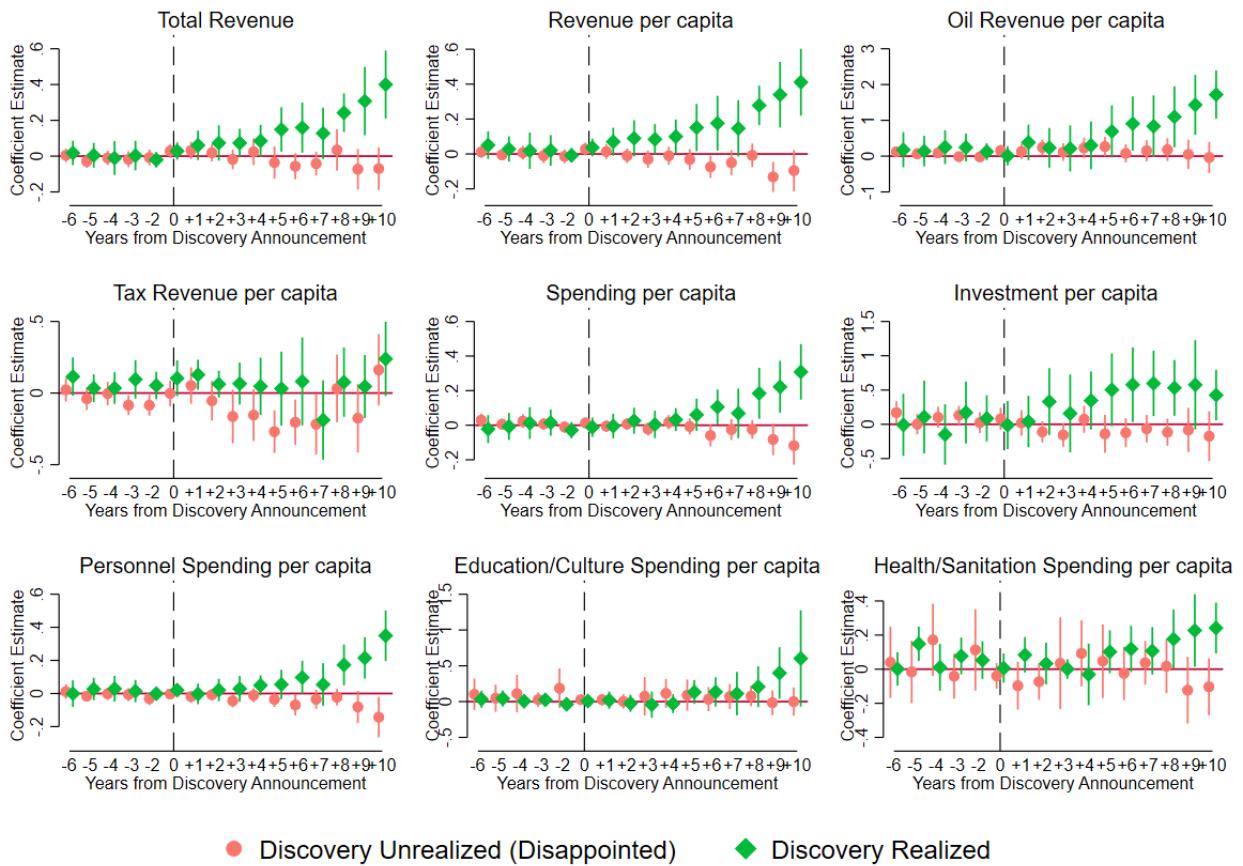
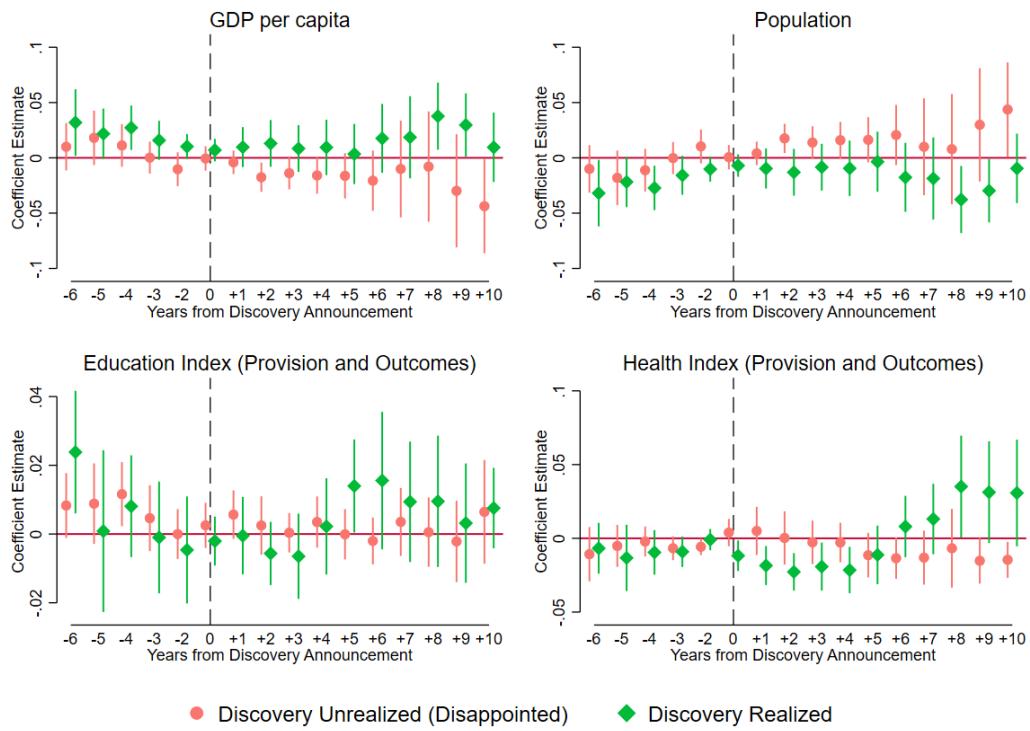


Figure C13: Event Study with Multiple Events: Other Outcomes



## C.5 Sample Means for Treated and Control Groups Over Time

Figure C14: Sample Means: Treated Municipalities and Never-Treated Controls (Municipalities with Post-2000 Exploratory Wells but No Discoveries (n=53))

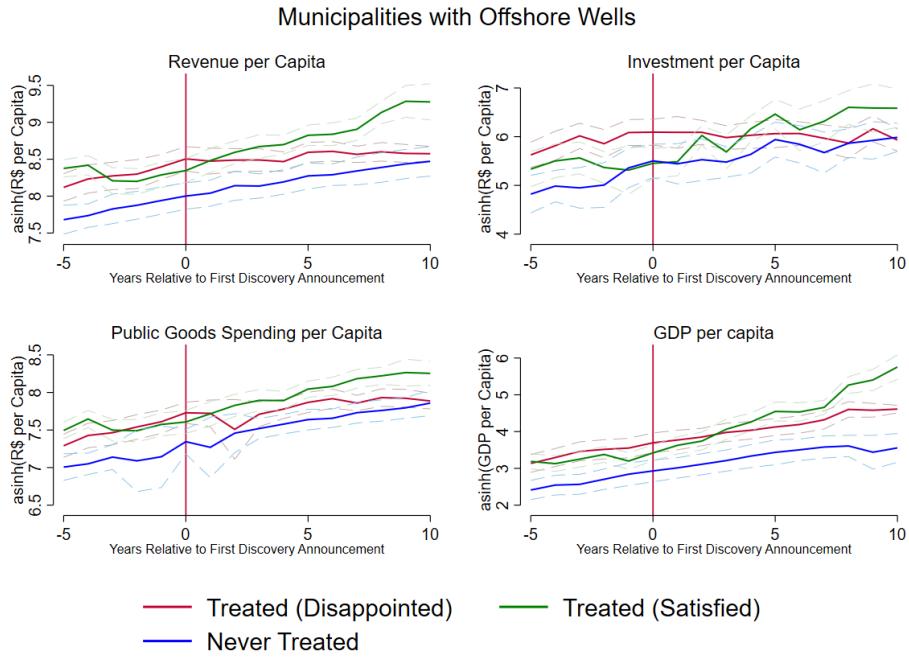


Figure C15: Sample Means: Treated Municipalities and Never-Treated Controls (Coarsened Exact Matching, Separately for Disappointed (n=836) and Satisfied (n=500))

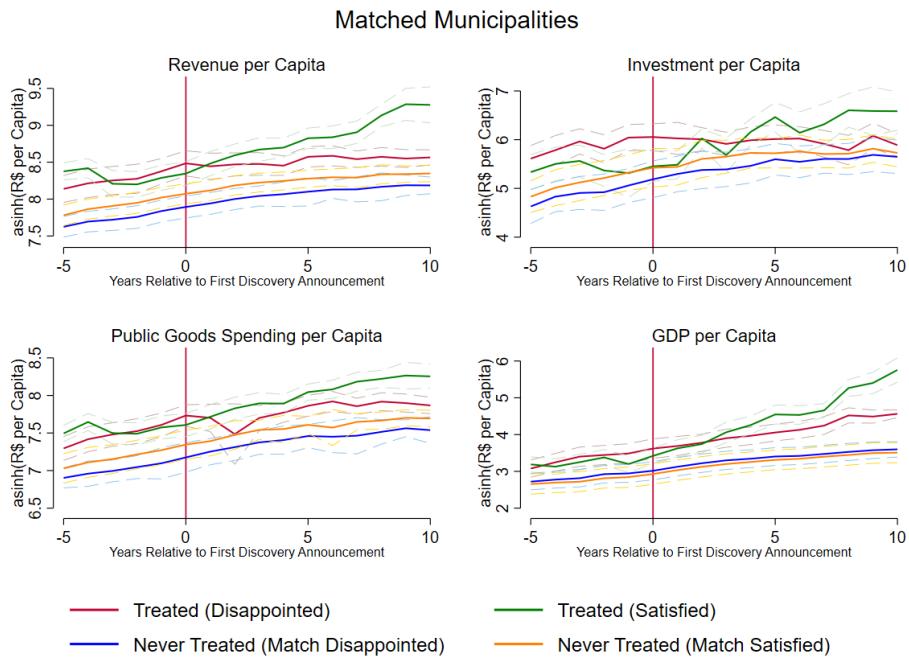
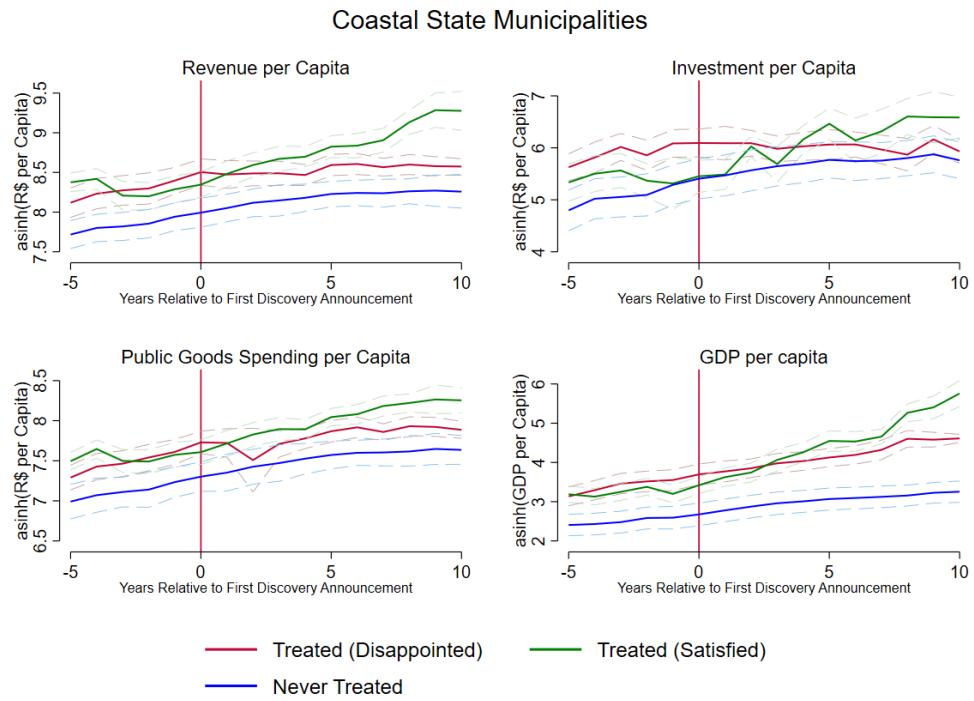
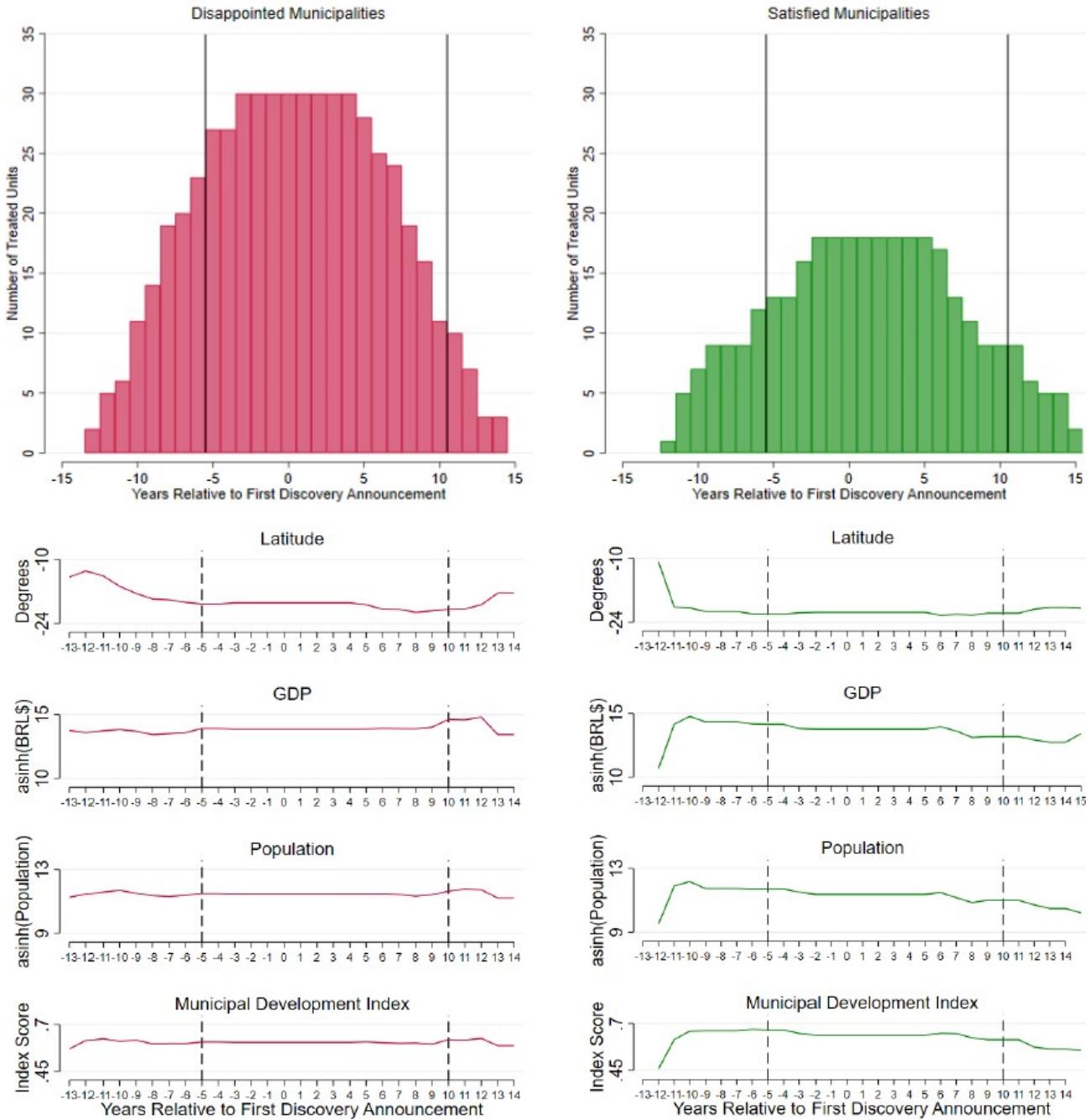


Figure C16: Sample Means: Treated Municipalities and Never-Treated Controls (Municipalities in Coastal States, n=3,902)



## C.6 Sample Balance Across Relative Time Indicators

Figure C17: Treated Unit Balance Across Relative Time Indicators

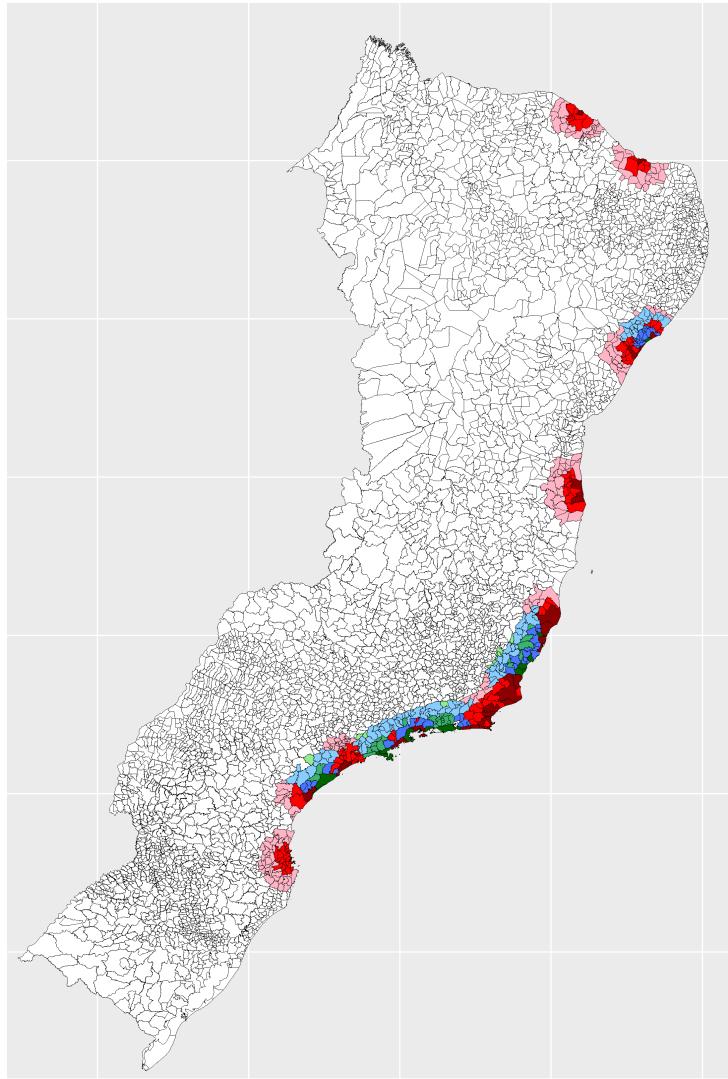


Note: Bar graphs depict number of treated units in each relative time period, where  $t=0$  represents the year of the first major discovery announcement for a municipality. Vertical black lines indicate the extent of periods included in the analysis. Given the limited time-frame in sample (2000-2017), the number of treated units observed declines as relative years become more distant. Since only a small number of municipalities receive discovery announcements, I do not impose a balanced sample requirement in event studies, as this would substantially reduce statistical power in periods distant from  $t=0$ . Further, I extend event studies forward to  $t+10$  since there is an approximate 10-year delay between discovery and peak production in offshore fields. To assess whether panel imbalance may lead to problems of comparability in the treated group across time, I plot means of key baseline characteristics (latitude, GDP, population, and municipal development index) across the range of relative year indicators.

## C.7 Spatial Spillovers from Discoveries

Do discovery announcements create spatial spillovers onto neighboring municipalities? Spillovers may be expected, since the design of revenue sharing rules leads neighbors to expect small revenue receipts of their own from a producer municipality's discovery treatment. Other mechanisms that could potentially cause spillovers include firm or migration movements toward or away from discovery-treated municipalities, or local general equilibrium effects, such as increases in factor prices near discovery-affected locales. I estimate spatial spillovers onto non-treated neighbors following the spillover-robust difference-in-difference specification proposed by [Clarke \(2017\)](#).

Figure C18: Spatial Spillovers: Near/Far Municipalities (0-50km and 50-100km) from Disappointed, Satisfied, and Both



Analyzing spatial spillovers from satisfied and disappointed municipalities is complicated by the tight geographical bunching of these two groups, leading to neighbors that are near both types. To deal with this, I create three treatment types and three accompanying control groups: 1) municipalities near/far from disappointed (0-50 km. and 50-100 km., respectively); 2) municipalities near/far from satisfied (0-50 km. and 50-100 km., respectively); and 3) municipalities near/far from both (0-50 km. and 50-100 km., respectively). I map these groups in Figure J1, where dark red and green are treated units, medium red and light red are near and far from disappointed, respectively, medium green and light green are near and far from satisfied, and medium blue and light blue are near and far from both. I then estimate event study specifications where the nearby municipalities are the treated group, the far municipalities are the control group, and treated units are omitted. As always, I estimate event studies separately for each group and plot all three groups together on the same graph.

Figure C19: Public Finance Outcomes in Near (0-50km) vs Far (50-100km) Municipalities

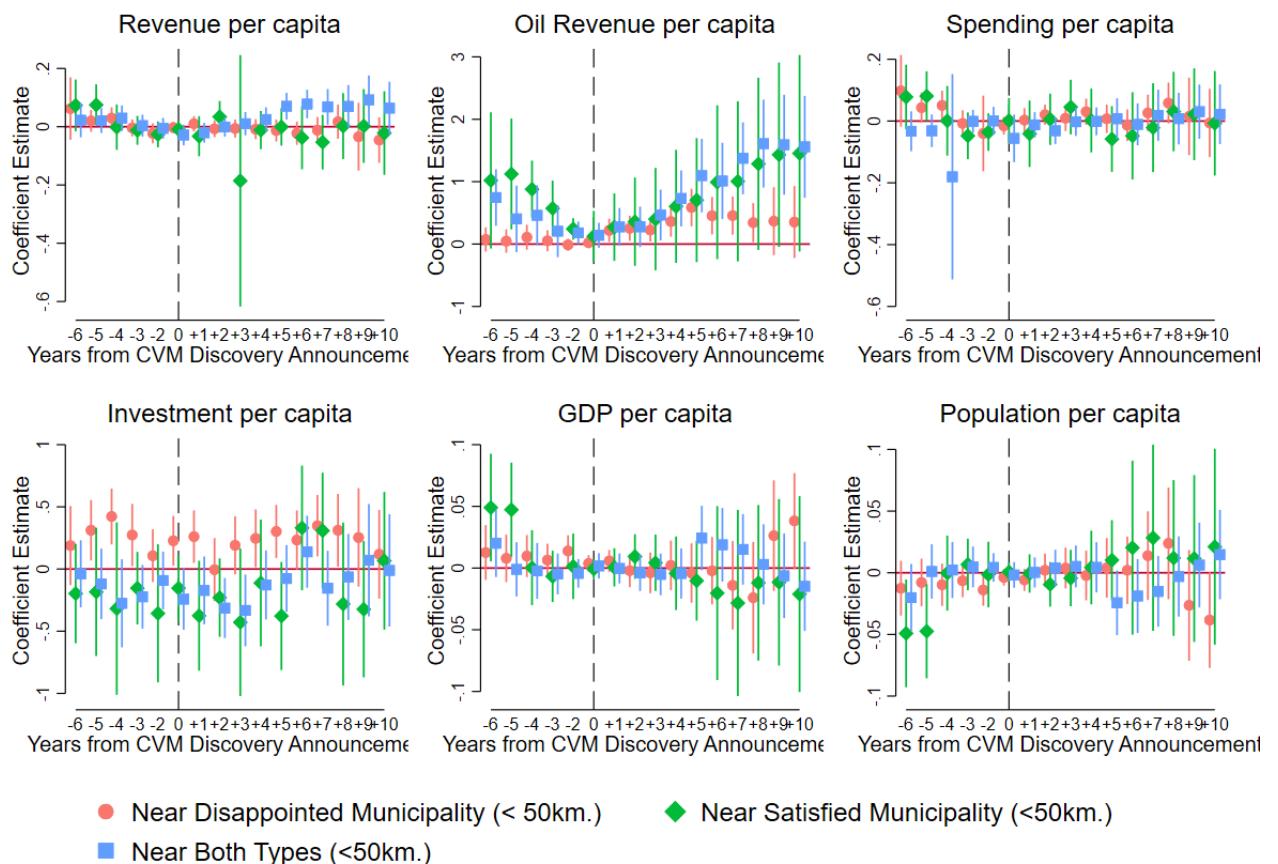


Figure C20: Sectoral Employment in Near (0-50km) vs Far (50-100km) Municipalities

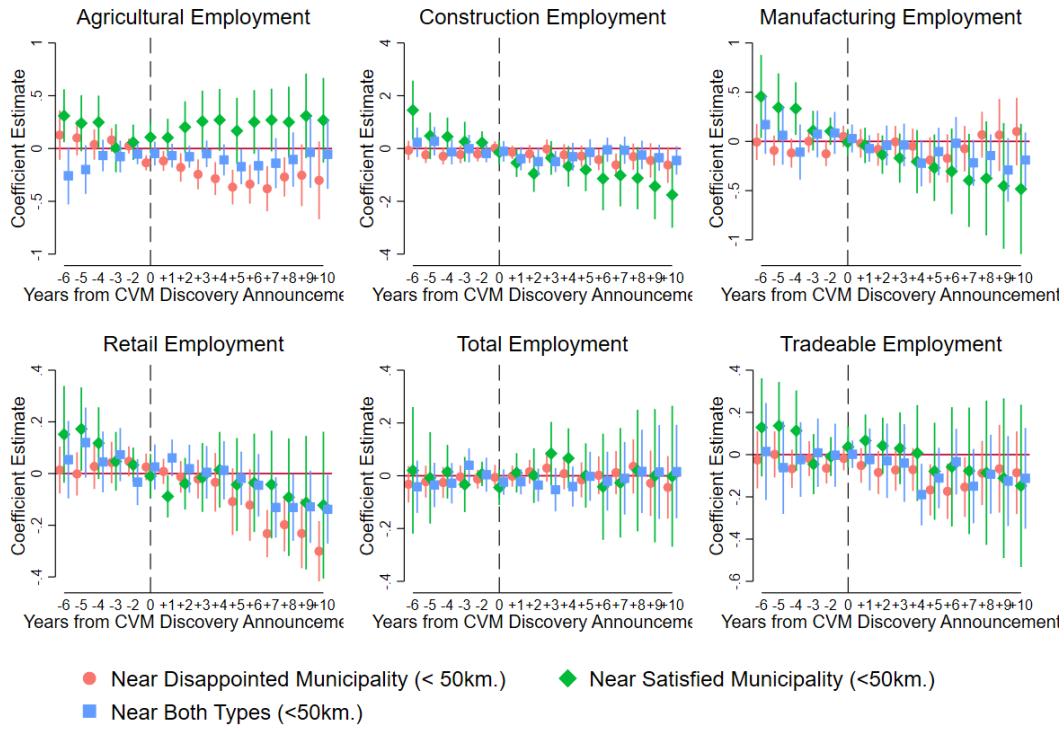
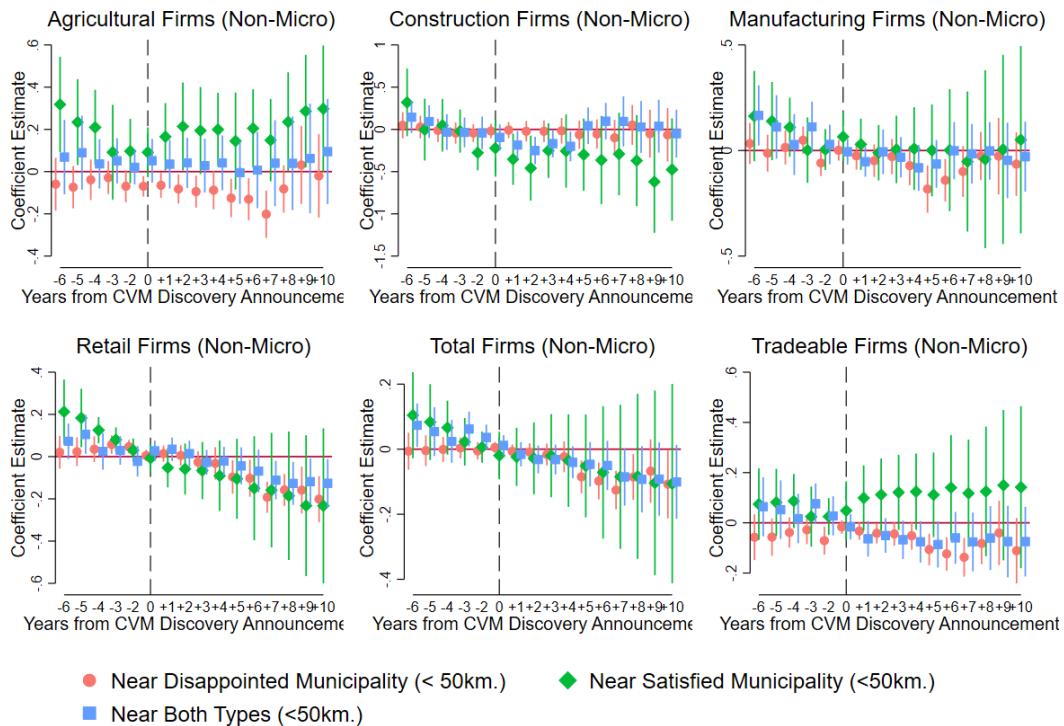


Figure C21: Firm Entry in Near (0-50km) vs Far (50-100km) Municipalities

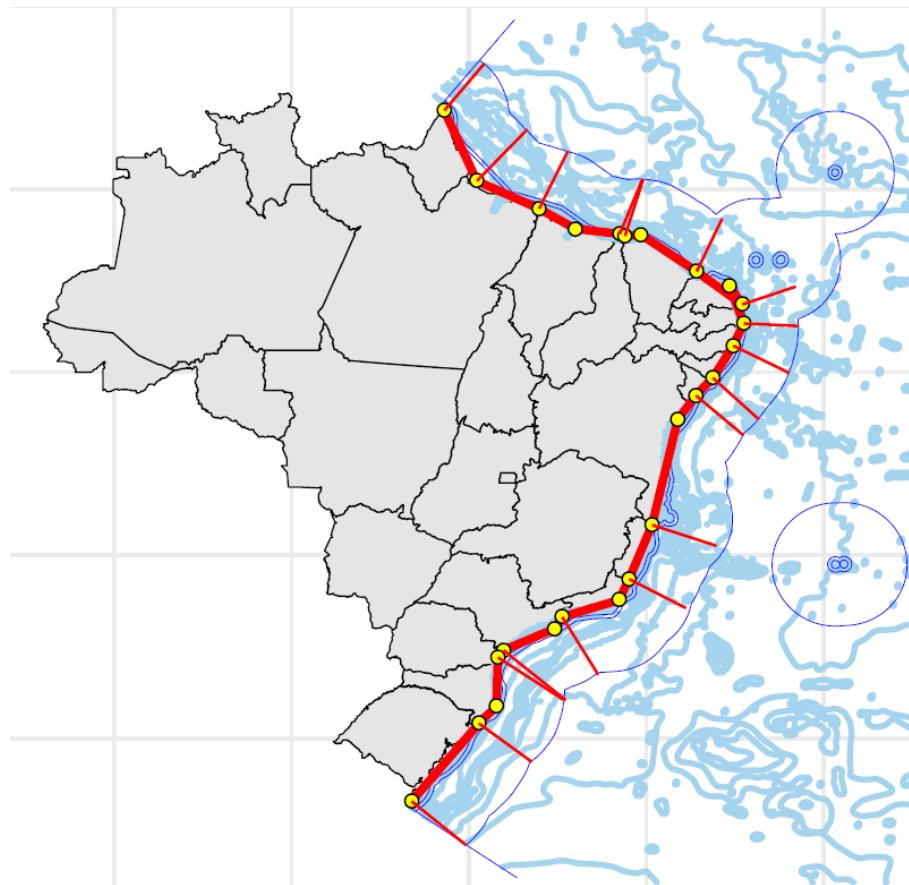


## D Explanatory Notes

### D.1 Reconstructing Geodesic Projection Maps

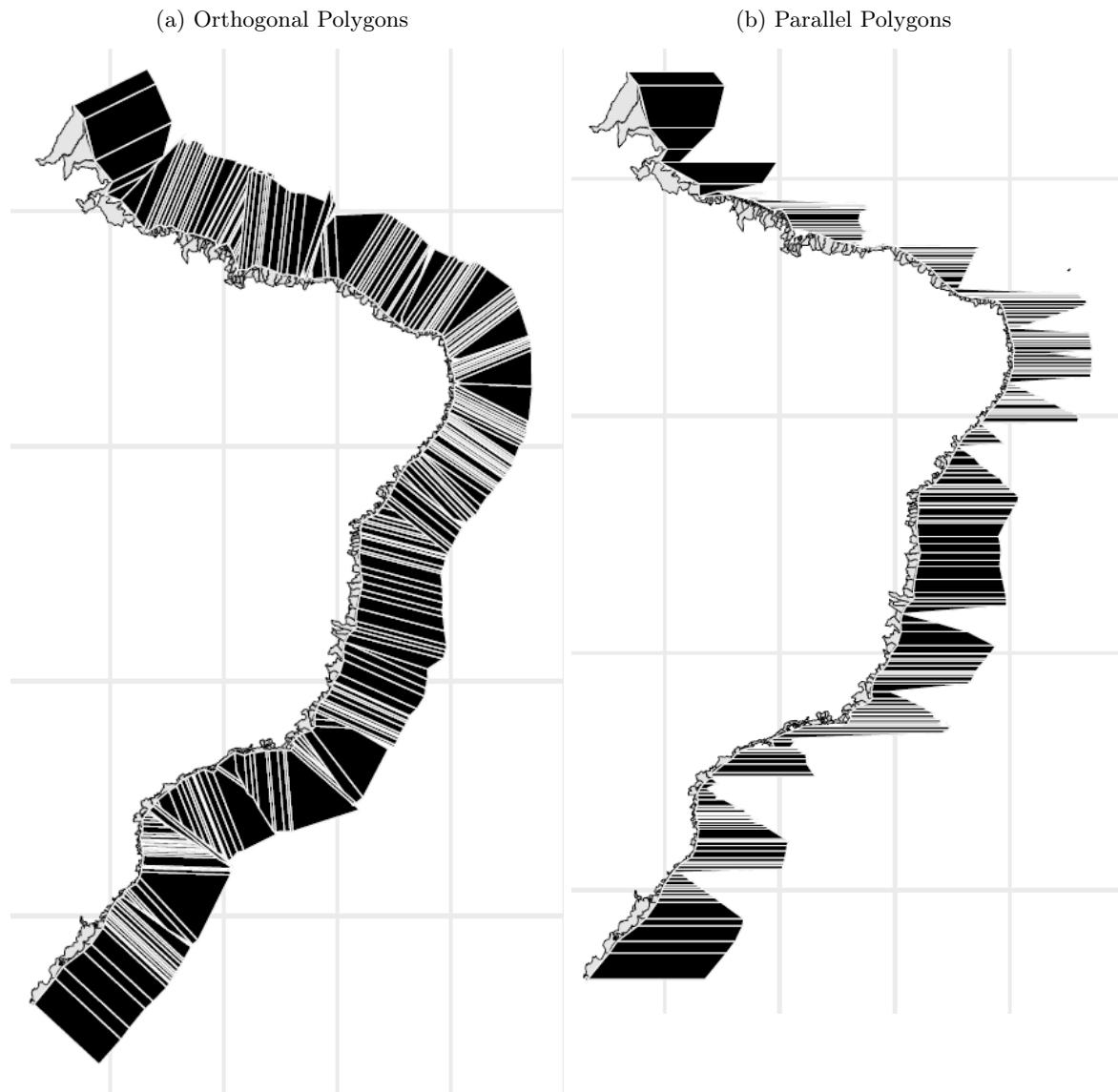
To reconstruct the geodesic projections used by IBGE and ANP to determine municipal offshore oil royalty distribution, I draw on documents from IBGE that define state boundary points and projections rules (IBGE, 2009). I begin by plotting state boundary points and state projections out to Brazil's maritime limit, as illustrated in Figure D1.

Figure D1: Brazil: Coastal Line and State Boundary Projections



I next generate orthogonal and parallel projections of each coastal municipal boundary out to the maritime limit, cutting off projections when they intersect state boundaries. I manually adjust boundary projections to account for special exceptions to standard rules, as in the case of Rio de Janeiro. I next create catchment zones for each municipality by generating polygons with vertices defined by coastal boundary points and the intersections of coastal boundary projections with the maritime limit. Figure D2 illustrates these catchment zones.

Figure D2: "Catchment Zones" (polygons) for Each Coastal Municipality



Finally, I plot all wells (including discovery wells) within these catchment zones. I create a crosswalk file that ties each catchment zone to its aligned municipality, and use this file to attach municipality code identifiers to each catchment zone. This allows me to collapse the well registry to the municipality level. I provide a complete R code and raw data package at:

[https://github.com/ekatovich/Brazil\\_GeodesicProjections](https://github.com/ekatovich/Brazil_GeodesicProjections)

This repository contains everything necessary to recreate the geodesic projections shown here.

## D.2 Municipal Royalty Distribution Formula

Allocation of offshore oil royalties in Brazil follows a formula first established in 1986 (Laws 7.453/85 and 7.525/86), and modified by the far-reaching Petroleum Law of 1997 (Law 9.478/97). Royalties are distributed monthly to federal, state, and municipal governments and the Brazilian navy by the National Oil Agency (ANP). Yearly royalties can be determined using cumulative values reported in December of each year. The royalty distribution formula is complex, and readers are referred to the ANP's Royalties Calculation Guide (in Portuguese) for a full description ([ANP, 2001](#)).

Royalties are assessed on gross value of offshore production. The royalty allocation formula is divided into two main parts: (i) the first 5%, and (ii) royalties in excess of the first 5%. The first 5% of gross production value in field  $f$  in year  $y$ , denoted  $W_{my}$  are allocated to municipality  $m$  according to:

$$W_{my} = \sum_f \left[ Alignment_{mfy} * (0.05)(P_{fy}^{oil} * V_{fy}^{oil} + P_{fy}^{gas} * V_{fy}^{gas}) * (0.3) \right] \quad (9)$$

where  $Alignment_{mfy}$  is the share of field  $f$  that is geographically aligned with the orthogonal or parallel projections of municipality  $m$ 's boundaries onto the continental shelf, 0.05 is the first 5% tax rate,  $P_{fy}^{oil}$  and  $P_{fy}^{gas}$  are the reference prices for oil and gas, respectively,  $V_{fy}^{oil}$  and  $V_{fy}^{gas}$  are the volumes of oil and gas produced, respectively, and 0.3 is the share of first 5% royalties allocated to municipalities. Royalties allocated to  $m$  are summed across all relevant fields,  $f$ , since municipal boundaries may align with multiple fields.

Royalties in excess of the first 5% are allocated according to:

$$Z_{my} = \sum_f \left[ Alignment_{mfy} * (Tax_{fy} - 0.05)(P_{fy}^{oil} * V_{fy}^{oil} + P_{fy}^{gas} * V_{fy}^{gas}) * (0.225) \right] \quad (10)$$

where everything is defined as in Equation 9, except that the royalty tax rate is set at  $Tax_{fy} - 0.05$ , a field-specific tax rate determined by the productivity of each field. Rates typically range from 5% (implying no royalties in excess of the base 5%) to 12% for very productive fields. 22.5% of royalties in excess of 5% of gross value of production are allocated to municipalities, leading the formula in Equation 10 to be multiplied by 0.225. Total royalties allocated in year  $y$  to municipality  $m$  are then calculated using the following formula:

$$R_{my} = \mathbb{1}(neighbor_{my}) * (W_{my} * (f(population_{my} + g(infrastructure_{my})) + \mathbb{1}(producer_m) * Z_{my}) \quad (11)$$

In this final formula, the first 5% of royalties are allocated to municipality  $m$  if it is a neighbor of

a producer municipality (including if it is a producer itself). If  $m$  is in the mesoregion of a producer municipality or is itself a producer municipality, the first 5% royalties it receives are weighted according to functions of municipal population and hosting of oil and gas infrastructure, such as pipelines, terminals, or refineries. If  $m$  is a producer municipality, it receives the full value of  $Z_{my}$ .

### D.3 Data Sources and Cleaning Procedures

#### *Municipal Public Finances*

I create a panel (2000-2017) on municipal public finances using FINBRA/SICONFI, the System of Fiscal and Accounting Information for the Brazilian Public Sector, organized by the Brazilian National Treasury. This dataset contains over 700 accounting variables related to municipal public finances, including disaggregated spending, investments, and IOUs to contractors or other entities ("*Restos a Pagar*"). I supplement these data with public finances data from the Institute for Applied Economic Research (IPEA), which cleans and simplifies the raw FINBRA data. The main variables I extract from these datasets are total revenues and spending, spending disaggregated by category (education, health, public safety, infrastructure, environment, culture, personnel, administration, and others), investment, and tax revenues. [Orair et al. \(2010\)](#) argues that municipal spending and investments are the variables most likely to be affected by positive or negative shocks to revenue.

#### *Municipal Elections*

I draw data on the 2000, 2004, 2008, 2012, and 2016 municipal elections from the *Tribunal Supremo Eleitoral* (TSE), or Supreme Electoral Tribunal. The TSE publishes disaggregated data on each mayoral and city council candidate in each election, including name, ID number, age, education level, occupation, political party, number of votes and donations received, and campaign spending. The TSE also publishes parallel datasets with information on each donation, including name and ID number of the donor, recipient, and donation value. Using these data, I construct a municipality-level panel with standard measures of political competition, including number of candidates, win margin, size of party coalitions, voter turnout, and candidate quality (proxied by education). I also observe whether each candidate is an incumbent or not, allowing me to measure reelection rates and detect differences in outcomes between candidates who are or are not eligible for reelection.

#### *News Coverage*

I use news coverage in *O Globo*, Rio de Janeiro's newspaper of record, as a proxy for national-level

salience of major offshore discoveries. I also corroborate most CVM discovery announcements with contemporaneous news coverage. I use the search terms “*descoberta de petróleo*” (oil discovery) and “*pré-sal*” (Pre-Salt) within archived news records for *O Globo* dating from 2005-2017, maintained by the International Newsstream Database.

### ***Formal Employment and Wages***

I extract much more detailed employment data from the *Relação Anual de Informações Sociais* (RAIS), or Annual Report of Social Indicators. This dataset contains information on the universe of formal employees in Brazil, including wages and job category. It also contains a variable indicating the institutional category of each employer, allowing me to identify exactly which employees were employed by municipal governments. Using these detailed employment data, I create a municipality-level panel for years 2000-2017 with information for each municipal government on number of public employees. I also calculate number of employees and average wages for economic sectors (agriculture, extractive, manufacturing, construction, retail, other services, and government).

### ***Public Goods Provision and Quality***

To measure real provision and quality of public goods at the municipality level, I focus on two essential areas: education and health. For education outcomes, I draw on the Basic Education Census (2000-2017) to construct a school infrastructure index, which is a simple sum of indicators for whether a municipal public school has a library, computer lab, and science lab. I also draw on the Basic Education Census to compute the ratio of teachers with some higher education over the total number of teachers in municipal public schools. I collapse both of these measures from the school to municipality level. Finally, I draw on biannual data from IDEB, which reports data on test scores and outcomes such as graduation rates. I report the main IDEB index score as a measure of realized school quality. For health outcomes, I draw on municipality-level data from Brazil’s universal public health system, SUS, including share of pregnant women receiving 7 or more prenatal visits, avoidable infant mortalities, and municipal hospital beds.

### ***Patronage***

Adopting a methodology proposed by [Colonelli et al. \(2019\)](#), I measure patronage as the rate at which winning mayoral candidates appoint their campaign donors to municipal public employment. While most public jobs in Brazil require individuals to pass an exam in order to qualify, each mayor is allotted a number of "commissioned posts" where they can appoint whoever they want. I can see whether these

posts are more often filled by campaign supporters in municipalities that get discoveries, under the hypothesis that the perceived value of holding office may have increased in these places, prompting greater patronage efforts. As a more general measure, I can observe whether the quality of municipal employees (proxied by their education levels) increases or decreases in affected municipalities. One effect of patronage could be to overlook qualified workers and appoint political supporters instead. This would show up as lower educational levels for municipal employees as a whole, or in specific areas such as administration or commissioned positions. This approach to measuring patronage, an inherently hard-to-observe phenomenon, is relatively new in the political economy of development literature, and relies on Brazil's uniquely rich employment and campaign donations datasets.

### ***Baseline Municipal Characteristics and Institutional Capacity***

Finally, I draw on municipal-level data for the year 2000 from the Demographic Census (IBGE, 2000) and FIRJAN Municipal Development Index (FMDI), a composite index of government capacity measured by formal employment statistics (share of workers formalized, formal income levels, and formal income Gini), education statistics (preschool enrollment rates, elementary school completion rates, year-on-year student progress rates, share of teachers with university of education, and test scores), and health statistics (share of mothers receiving adequate pre-natal care, undefined deaths, preventable infant deaths, and intensive care beds). I draw data on geographical characteristics from IPEA.

### ***Oil Royalties and Special Participations***

I draw on monthly data on oil and gas royalties and quarterly data on special participations distributed to Brazilian municipalities, made available by Brazil's National Oil Agency (ANP) for the years 1999-2017. I make raw data and code available to construct municipality-level monthly and yearly panels of royalty and special participation receipts for this period at: at:

[https://github.com/ekatovich/Royalties\\_and\\_SpecialParticipations](https://github.com/ekatovich/Royalties_and_SpecialParticipations)

The final panel produced by these scripts is balanced, e.g. contains observations for each of Brazil's 5570 municipalities for each of the months between January 1999 and December 2017. The scripts produce datasets at the monthly and yearly levels, and quarterly for special participations. All monetary values are deflated into constant 2010 Brazilian Reals using Brazil's Indice Nacional de Precos ao Consumidor, published by IBGE. Geographical unit codes for municipality, microregion, mesoregion, and UF (state) are attached to each municipality name string reported in the raw royalties and special participations datasets, facilitating merges with other municipality-level dataset.