

# The Presource Curse? Anticipation, Disappointment, and Governance after Oil Discoveries\*

Erik Katovich<sup>†</sup>

[Click here for most recent version](#)

## Abstract

Resource discoveries are often followed by long delays and heterogeneous production realizations. 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 to estimate causal effects of offshore oil and gas discoveries on local governments in Brazil. Using an original geolocated dataset of 179 major discovery announcements, I estimate dynamic effects of discoveries on municipal public finances, public goods provision, and political competition, selection, and patronage. I develop a forecasting model to document that most municipalities (30 of 48) affected by discoveries ultimately fail to receive the revenues they could have expected. Relative to never-treated controls, these disappointed places suffer significant declines in per capita investment and public goods spending after ten years. In contrast, municipalities where discoveries are realized enjoy significant growth in per capita revenues and spending, but do not invest in economic diversification or improve public goods provision. My findings identify how local governments and politicians respond to shocks to expectations and revenues across time. Methodologically, I highlight the importance of accounting for dynamic treatment effects and heterogeneity in discovery realizations after discovery announcements.

**JEL Codes:** H72, Q32, 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, the Sovereign Wealth Fund of Niterói, Universidad de San Andrés, and UW-Madison. 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 has received generous financial support from IRIS and Tinker Nave field research grants. UW-Madison's Institutional Review Board has 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 will become increasingly uncertain in coming decades. 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). Pressures to leave fossil fuels in the ground to combat climate change are likely to cause discoveries to remain undeveloped in the future (McGlade and Ekins, 2015; Welsby et al., 2021).

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>2</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). Following Cust and Mihalyi (2017), I refer to the challenges created by anticipation, uncertainty, and frequent disappointment after discoveries as the “Presource Curse.”

I test for subnational evidence of the Presource Curse following a wave of major offshore oil and natural gas discoveries in Brazil during the 2000s and 2010s. In particular, I ask whether major discovery announcements cause anticipatory changes in local governance (i.e., municipal public finances, public goods provision, and political competition, selection, and patronage) before revenues begin to

---

<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) I present a map of giant discoveries in Appendix A1.

<sup>2</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).

flow. Further, I ask how frequently 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.

To answer these questions, I exploit quasi-experimental variation created by Brazil's formulaic and long-established offshore oil and gas revenue sharing rules. Based on geographical alignment between coastal municipalities and offshore fields, these rules allow municipal governments to predict whether they will be future beneficiaries of discoveries—thus introducing subnational variation amongst comparable local governments. Furthermore, offshore discoveries are plausibly exogenous to municipalities, as they are made by multinational corporations operating hundreds of kilometers offshore, servicing their installations from distant ports, and responding to international prices and technologies. To link discoveries to coastal municipalities, I first 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 then reconstruct Brazil's geodesic offshore projection maps to tie each discovery back to affected municipalities.

I identify causal effects of discovery announcements and subsequent realizations 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 good as random (Cavalcanti et al., 2016; Cust et al., 2019). To quantify heterogeneity in discovery realizations, I forecast each municipality's expected production stream after a discovery announcement based on standard offshore production curves, average delay periods in that municipality's region, and information on estimated reserve volume reported in the announcement. I then convert this expected production stream into an expected revenue stream by applying Brazil's oil and gas royalty distribution formula, and compare expected and realized revenues to categorize municipalities into groups of "disappointed" and "satisfied." I estimate event study specifications around the first major discovery announcement separately for each of these groups relative to never-treated controls.

I find that only 18 of 48 Brazilian municipalities affected by oil discovery announcements between 2000-2017 ultimately receive 50% or more of the revenues they could have expected based on standard production assumptions, contemporaneous prices, and information presented in the original announcement. In other words, disappointment was widespread, though not universal.<sup>3</sup>

Brazilian municipalities did not exhibit rapid anticipatory fiscal responses to discovery announce-

---

<sup>3</sup>Note that this measure of disappointment is based on production realizations only up to 2017. Municipalities that are "disappointed" at this time may enjoy later, delayed oil booms. Until then, however, "dud" discoveries and delayed discoveries likely exert similar effects.

ments, likely 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 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, however, measures of real public goods provision, quality, and outcomes are unchanged or slightly negative relative to controls in the decade following a major announcement.<sup>4</sup> This finding corroborates the conclusion 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 the expected windfalls), per capita oil revenues remain unchanged ten years after the first major

---

<sup>4</sup>Measures of real public goods provision include (i) an index of school infrastructure (library, computer lab, and science lab); (ii) share of primary and secondary teachers with a college degree; (iii) IDEB index of graduation rates and test scores; (iv) municipal hospital beds per capita; (v) share of pregnant women receiving seven or more prenatal checkups; (vi) avoidable infant mortality per 1000 births.

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 by 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 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 trend downwards relative to never-treated controls. Spending on promotion of non-extractive sectors is weakly positive ten years on, suggesting disappointed places may increase efforts to diversify their economies when oil discoveries fail to deliver windfalls.

Next, I examine effects of discovery announcements on political competition, selection, and patronage in municipal elections between 2000-2016. Since mayors and council members control royalty revenues and potentially extract personal and political rents, the expected value of holding future office increases when a discovery is announced. Consequently, more candidates may run for office—especially individuals 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 ([Colonnelli et al., 2019](#)).

I estimate a difference-in-differences specification where treatment is defined as occurrence of a discovery announcement in the four years preceding a municipal election. My findings suggest that discoveries increase competitive candidates running for council and decrease schooling levels of candidates and winners. Discoveries weakly increase the value and number of donations made in local elections. I find no effects on patronage (e.g., the rate at which successful mayoral candidates hire their campaign donors to discretionary public sector jobs). Furthermore, I find that council candidates are significantly less likely to be reelected when oil revenues are substantially below expectations at the time of the election (e.g., when the municipality is disappointed). Mayors are weakly less likely to be elected. These results suggest voters, unable to observe politicians' true quality and honesty, may opt to punish politicians for exogenous negative outcomes. This in turn leads to increased administrative turnover in disappointed places, potentially causing disruptions in public service delivery ([Akhtari et al., 2021; Toral, 2021](#)).

To test robustness of my results to the choice of samples and estimators, I re-estimate my main specifications using pre-matched never-treated control groups constructed through coarsened exact matching ([Iacus et al., 2012](#)). To address threats to causal inference in the context of difference-in-differences with staggered treatment timing and heterogeneous treatment effects (e.g., [de Chaisemartin and D'Haultfoeuille \(2020\)](#), [Goodman-Bacon \(2018\)](#)), I implement the doubly robust group-time av-

verage treatment effects estimator proposed by Callaway and Sant'Anna (2020). My results are stable 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 discovery 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 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 *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. Further, I focus on effects of major offshore discoveries announced publicly since 2000, which were 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 explore identification challenges. In section 7, I present results and robustness checks, and in section 8 I conclude with discussion and policy implications.

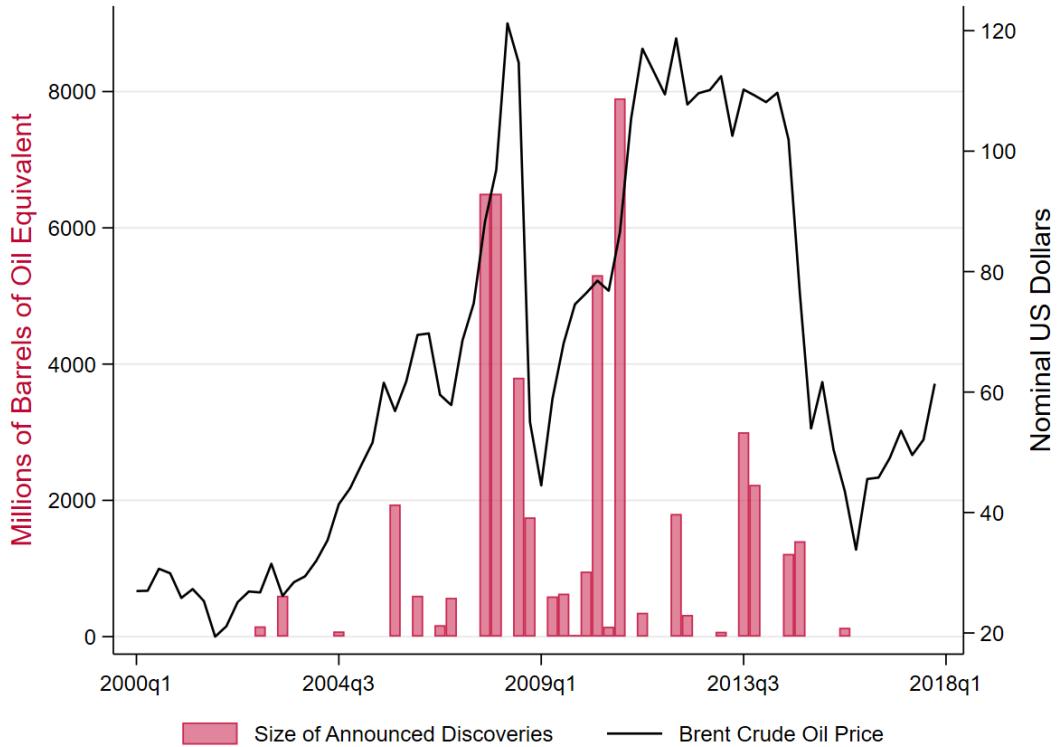
## 2 Context: Oil and Local Governance in Brazil

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, the rise of US shale, 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>

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

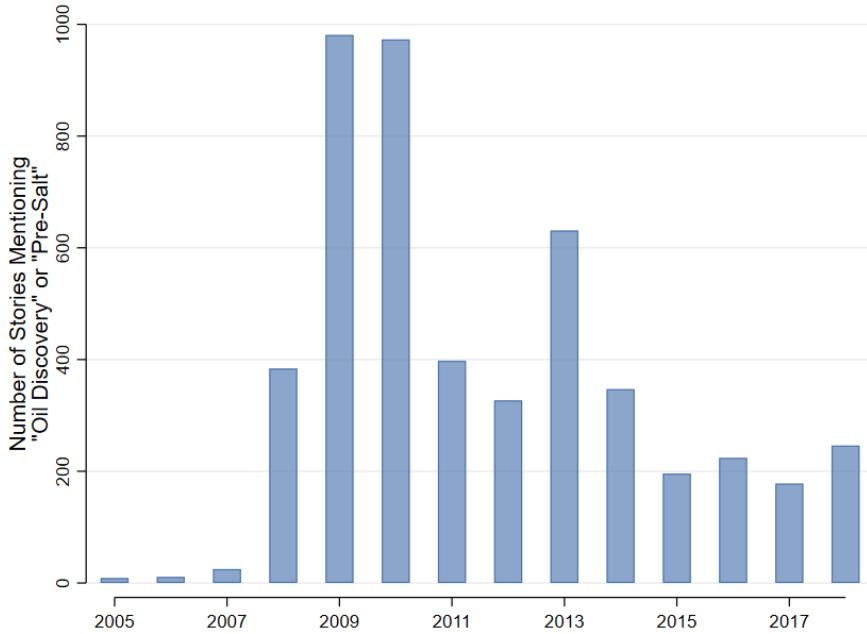


The Pre-Salt discoveries became a major topic in news media, with 981 stories reporting on them in Rio de Janeiro’s *O Globo* newspaper in 2009 alone (Figure 2). This public visibility likely filtered down to municipalities in affected regions, where elections in 2008, 2012, and 2016 may have been influenced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

---

<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 all 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 on pause 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 2: News Coverage of Oil Discoveries in *O Globo*



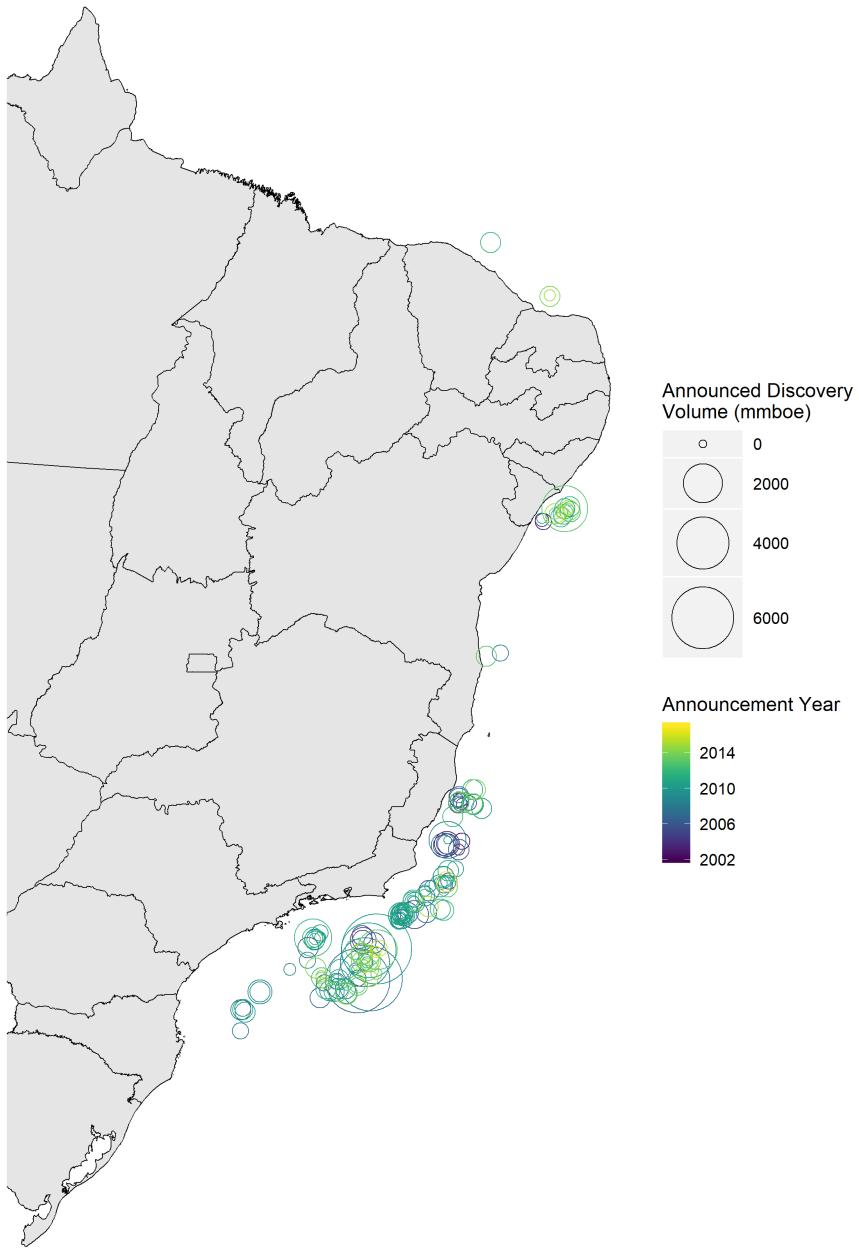
### *Discovery Announcements*

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.

---

<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 A2, 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, again increasing the salience of these events.

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



Media outlets use the CVM declarations as sources when reporting on new oil discoveries. Thus, 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 (available upon request).<sup>7</sup>

### ***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 in 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 potential 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 almost universal. In Brazil, field-level time-to-production forecasts were more heterogeneous. Many fields (especially major fields including 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 timeframe never produced, as of 2018. Evidently, discovery impacts may exhibit significant heterogeneity in timing.

---

<sup>7</sup>In Appendix A3, 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 reported all key information contained in the announcement.

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

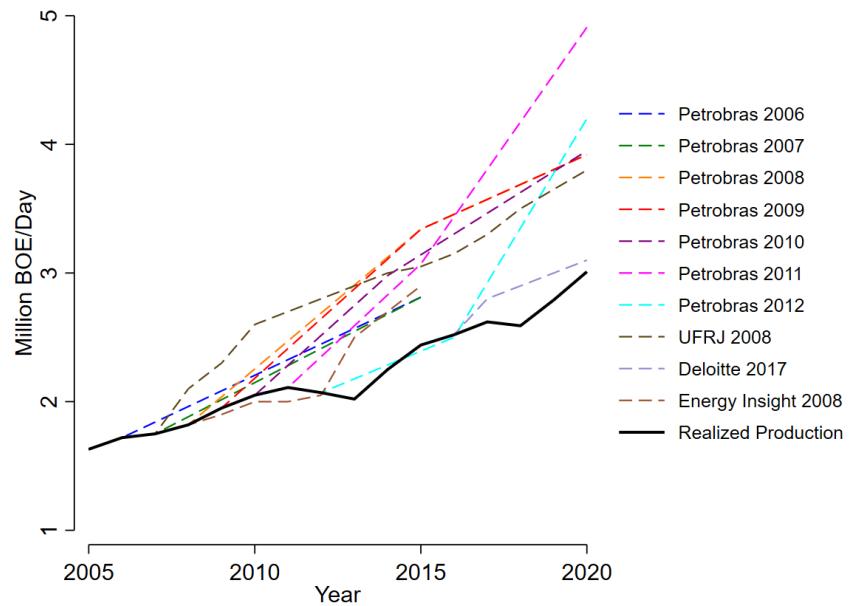
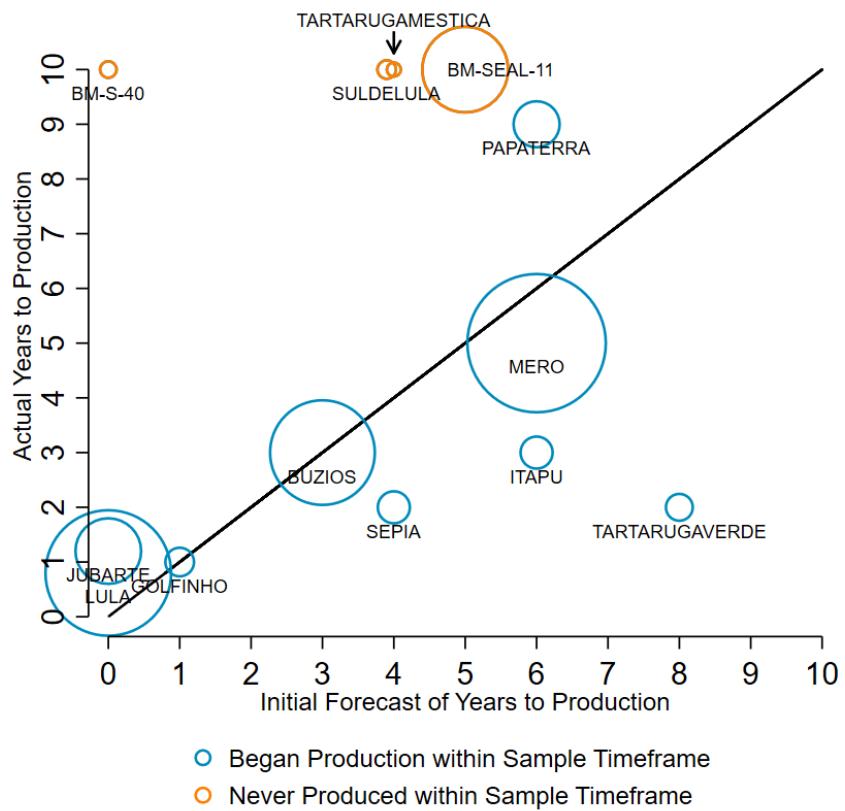


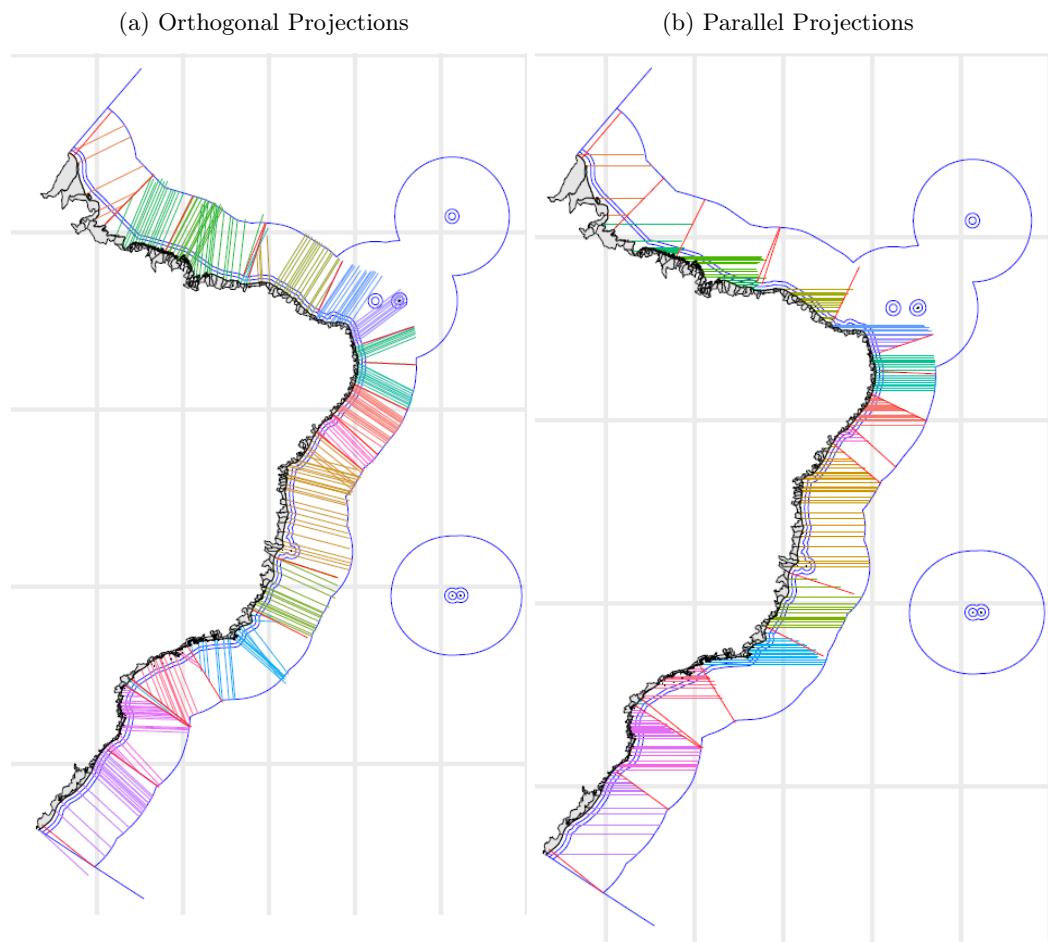
Figure 5: Brazil: Field-Level Time-to-Production Forecasts



### Royalty Distribution

In 1985-1986, Laws 7.453/85 and 7.525/86 established royalty requirements for Brazilian maritime 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 the overwhelming share 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.

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

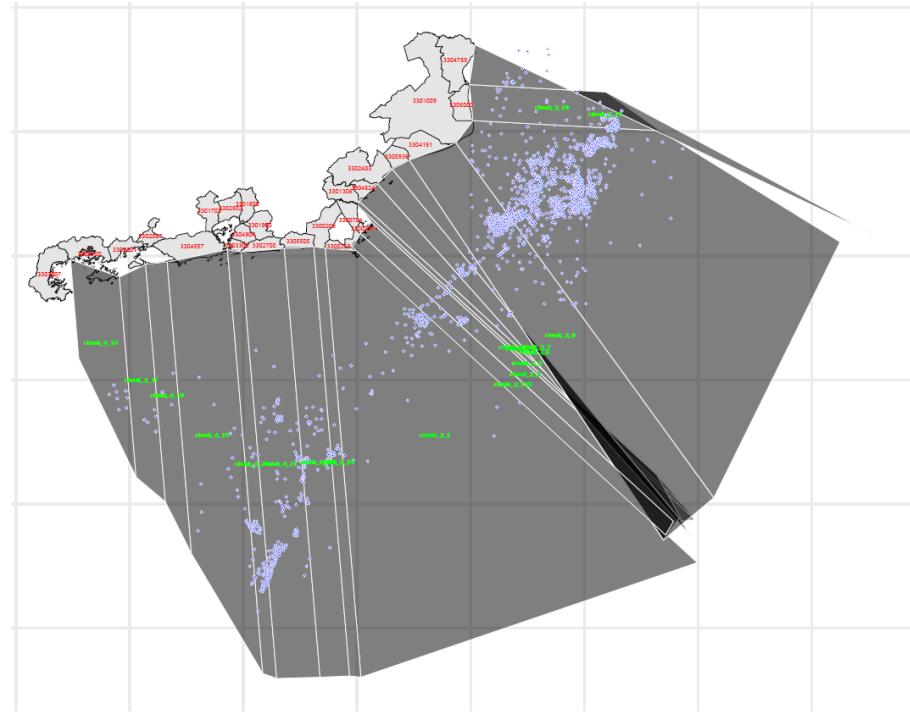


Note: Colors correspond to states. Orthogonal and parallel projections of municipal boundaries are drawn separately for each state, and cut off at state boundary-projections. Projections extend 200 nautical miles (370km.) to Brazil's maritime limit, designated by the solid blue line.

Brazil's use of geodesic boundary projections to determine offshore royalty allocation creates a quasi-experiment in which exogenous offshore discoveries 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 complete 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 plot all wells in the ANP registry within catchment zones created by the geodesic projections, and link wells (including wells cited in discovery announcements) back to their aligned municipality, as illustrated in Figure 7, which presents orthogonal projections for the state of Rio de Janeiro.<sup>8</sup> This well-municipality crosswalk creates the municipality-level treatment variable I use in event studies.

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




---

<sup>8</sup> As apparent in Figure 7, Rio de Janeiro's "orthogonal" projections diverge from the strict 90 degree rule used in other states. In this case, special exceptions to the 90 degree rule were introduced at the time projections were established (1986) to account, allegedly, for large deviations in the coastline that would have privileged certain municipalities over others. The result led to disproportionately large catchment zones for specific municipalities, including Campos dos Goytacazes and Arraial do Cabo.

### *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), which was 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](#)).

### *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).

#### *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>9</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 com-

---

<sup>9</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.

petition 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](#)). More 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 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, 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.

### ***The Benevolent Government Framework***

In this 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, effects 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.

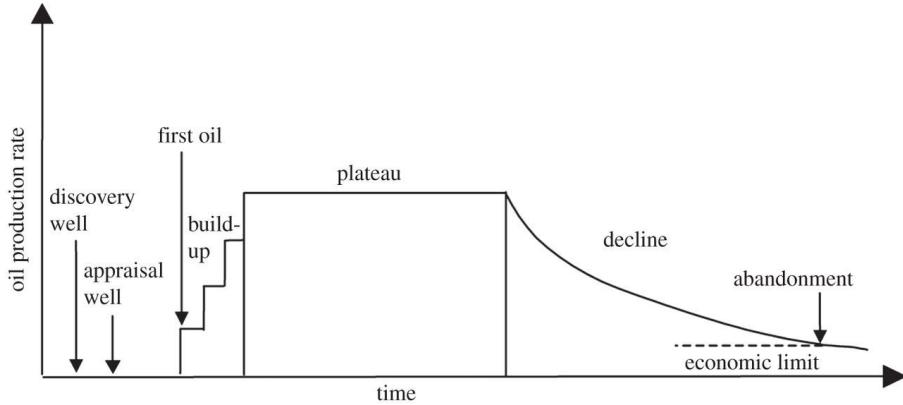
The benevolent government framework generates the following predictions: (i) after a discovery announcement, spending on public goods and personnel will rise and taxes will fall (if not constrained by a single-period balanced budget constraint); (ii) after realization of high revenues, spending on public 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 build 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 group 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 (Han et al., 2019). 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: Han et al. (2019)

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>10</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>10</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  $f$ :

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

See Appendix D 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  $Disappointment_{m,2017} > 0.4$ . 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 A5, I report kernel density plots of  $Disappointment_{m,2017}$  derived from low, medium, and high variations of forecast parameters (described in next paragraph).

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 some municipalities are only aligned with 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, 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 the disappointed/satisfied classification for all discovery-affected municipalities under alternative modeling parameters.

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

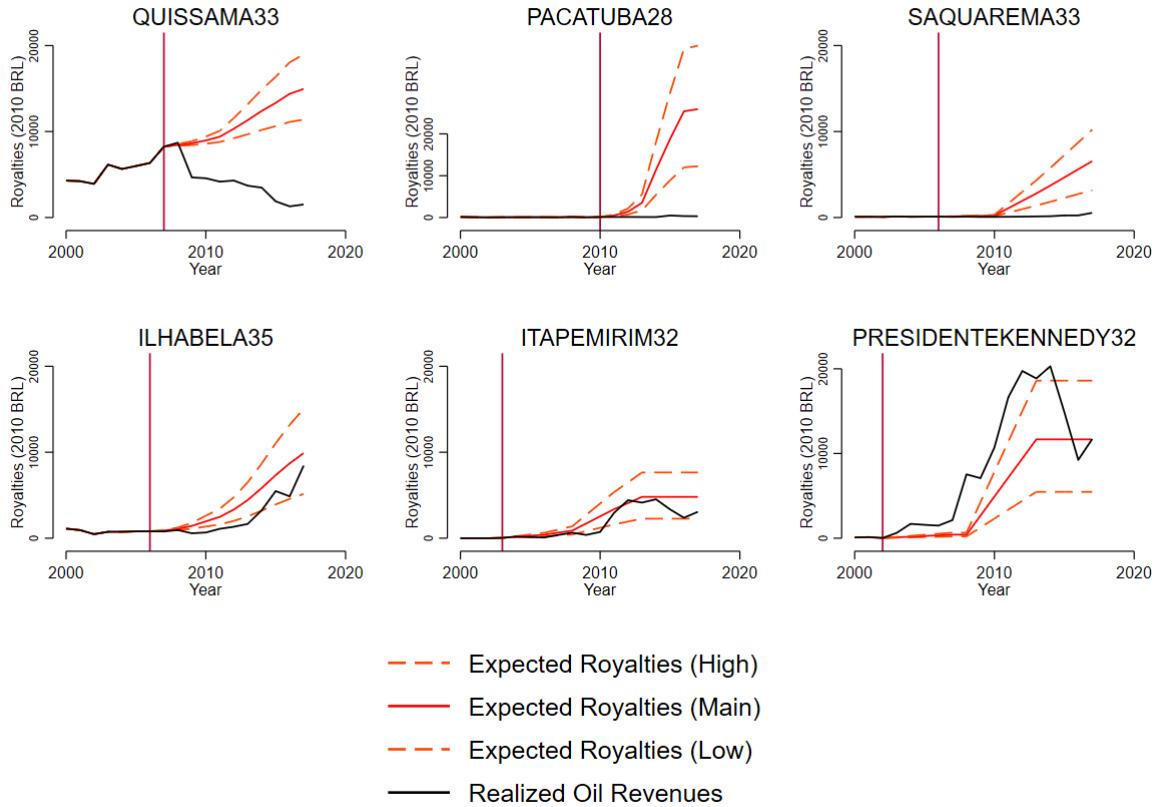
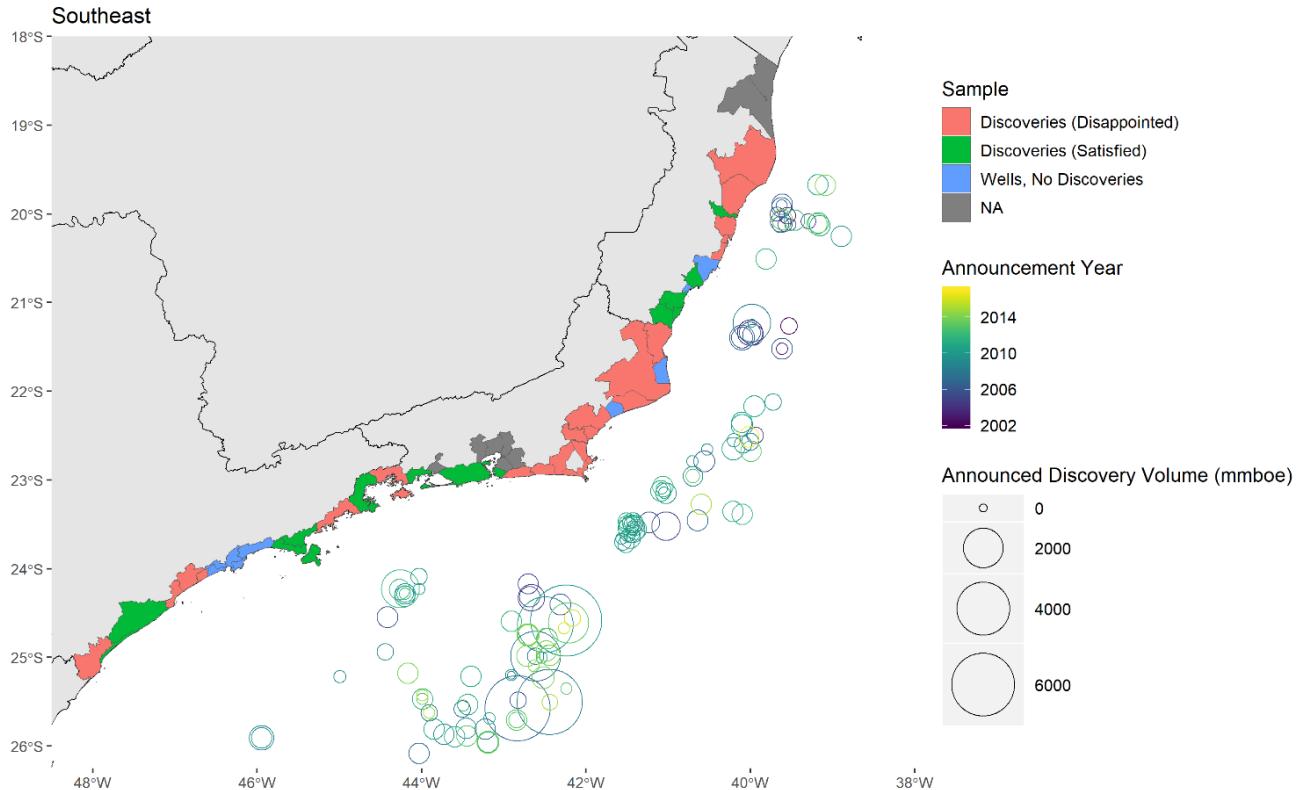


Figure 10 plots all major offshore oil and gas discoveries reported between 2000 and 2017 in Brazil's Southeast region (where most major Pre-Salt discoveries occurred), as well as the treated samples of disappointed and satisfied municipalities identified by my revenue forecasting model. The figure also 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 A6, 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, 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. 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. These differences do not threaten the quasi-experimental nature of this context, given that it is unlikely municipal conditions influence multinational oil companies' offshore drilling operations. Nevertheless, 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. In my main matching specification, I include baseline (year 2000) levels of municipal GDP, population,latitude, distance to state capital, and score on the FIRJAN Municipal Development Index. Balance between treated groups and matched subsamples is significantly improved along some dimensions. 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 offshore 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 Strategies and Identification

I estimate dynamic effects of a discovery announcement on public finance and economic outcomes using an event study framework ([Callaway and Sant'Anna, 2020](#); [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.

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 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'Haultfoeuille, 2020](#)). 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 substantive 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 of (pre-treatment, year 2000) quintiles of GDP, population, distance from state capital, latitude, and municipal development index. As a further robustness check, I match on looser and stricter sets of variables and also match on baseline levels of revenue and expenditure, and re-estimate all event

---

<sup>11</sup>I assume for now that each municipality is treated only once. 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 C1.

studies.<sup>12</sup> Finally, I implement Callaway and Sant'Anna's (2020) group-time average treatment effect approach for key outcomes to address potential bias introduced by the two-way fixed effects specification in a setting with staggered treatment timing and heterogeneous treatment effects.

I estimate Equation 4 separately for disappointed and satisfied municipalities, each relative to Wells and matched control groups. I assume that these types are known to the econometrician, but unknown to treated units until realization of revenues. 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 (30 disappointed and 18 satisfied municipalities):

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

### **Difference-in-Differences: Discovery Effects on Elections**

Since municipal elections occur every four years, I opt for a generalized difference-in-differences approach, rather than an event study, to study discovery effects on political competition, selection, and patronage. I consider treatment to be the announcement of one or more major discoveries in a municipality's catchment zone in the four years leading up to an election. To measure political competition, I compute number of candidates and competitive candidates (total and per seat) and average coalition size ([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.

Newly elected leaders enter office on January 1st of the year after the election. Thus, municipal mandates over this period are 2001-2004, 2005-2008, 2009-2012, and 2013-2016. 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 treatment. For continuous outcome variables, I apply the inverse hyperbolic sine transformation. I cluster standard errors at the municipality level.

---

<sup>12</sup>In these alternative specifications, I match on distance to state capital and latitude (loose, only geographic), and distance to state capital, latitude, GDP, population, municipal development index, percentage of workers employed in the public sector, and income Gini coefficient (strict, including variables that were significantly associated with discovery realization in conditional random assignment tests). There are tradeoffs when matching on stricter sets of variables, in that some treated units fall off common support and are dropped. As my samples of disappointed and satisfied treated units are relatively small, I try to strike a balance between matching rigor and sample retention.

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

Finally, I test whether disappointment in offshore revenue expectations at the time of the election leads to lower reelection rates for incumbent politicians. To assess this, I calculate the ratio of realized revenue growth over the previous mandate over expected revenue growth over the same period:

$$Disappointment_{me} = \frac{\frac{Revenue_{me}}{Revenue_{m,e-1}}}{\frac{E(Revenue_{me})}{Revenue_{m,e-1}}} \quad (7)$$

Based on this time-varying value, I 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  $Disappointment_{me} > 0.4$ . 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:

$$P(Reelection_{cme} = 1) = \delta_m + \lambda_e + \beta Disappointed_{me} + X'\mu + \epsilon_{cme} \quad (8)$$

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. I assess the stability of these two-way fixed effects results across samples (Wells and Matched) and estimators (TWFE and [Callaway and Sant'Anna \(2020\)](#)).

### ***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 treatment group of municipalities that received discovery announcements, the experiment would then randomly assign some municipalities to the disappointed group and others to the satisfied group 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 discovery 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 pre-

dictive of offshore oil and gas outcomes. Conditional on fixed geographical features, do pre-discovery municipality characteristics predict where future discoveries occur, or whether discoveries are successful or disappointed? If municipality characteristics influenced outcomes, or municipal leaders were able to lobby oil companies, this would introduce reverse causality into Equation 4. 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 later revenue 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 due to idiosyncratic events affecting specific oil companies. 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 revenue realizations than discoveries made by Petrobras. 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 state 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:

$$Y_m^{2000} = \alpha + \beta_1 Treatment_m + X'\lambda + \delta_s + \epsilon_m \quad (9)$$

In Table 3, I report the 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 that 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 are not predictive of where discoveries are made or expectations are satisfied. This supports my argument that offshore discoveries and realizations were exogenous to municipality characteristics.

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

Outcome	$\mathbb{1}(\text{Wells} = 1)$	$\mathbb{1}(\text{Discovery} = 1)$	$\mathbb{1}(\text{Satisfied} = 1)$
	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	Municipalities w. Wells	Municipalities w. Discoveries
Observations	277	101	48

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 rwolf 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 also 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 my 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 277	Municipalities w. Wells 101	Municipalities w. Discoveries 48
Observations			

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 also 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 C3, 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. I assume 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 internal 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 C7, I implement the spillover-robust difference-in-differences strategy proposed in [Clarke \(2017\)](#) to explicitly analyze spatial spillovers from discovery announcements.

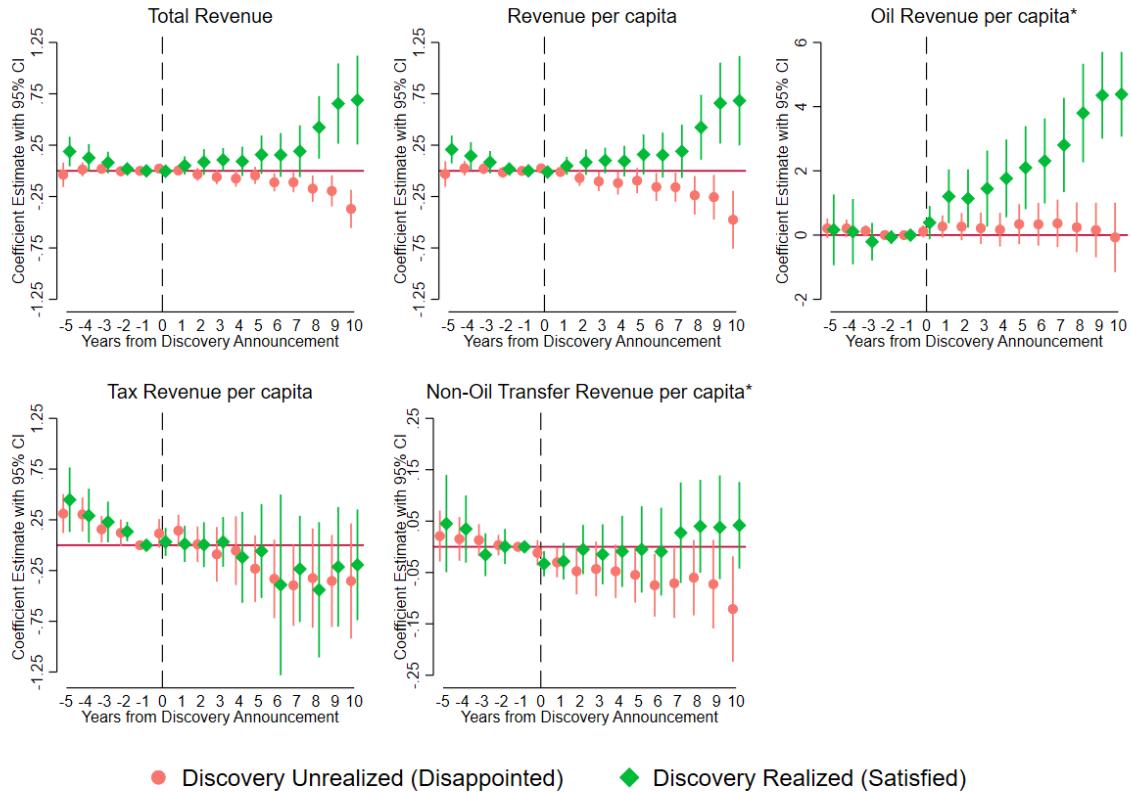
My preferred choice of control group ("Wells") reduces concerns over anticipatory effects since both treated and control municipalities in this group are experiencing relatively constant offshore oil exploration activity. The unpredictable timing of major discoveries means additional anticipation is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

Finally, 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 C6, 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 indicators. 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 C1 I report alternative estimates using a specification that allows for multiple discovery events per treated unit. In Appendix B3, I present summary tables of sample means and sizes, coefficient estimates, and semi-elasticities for key outcomes.

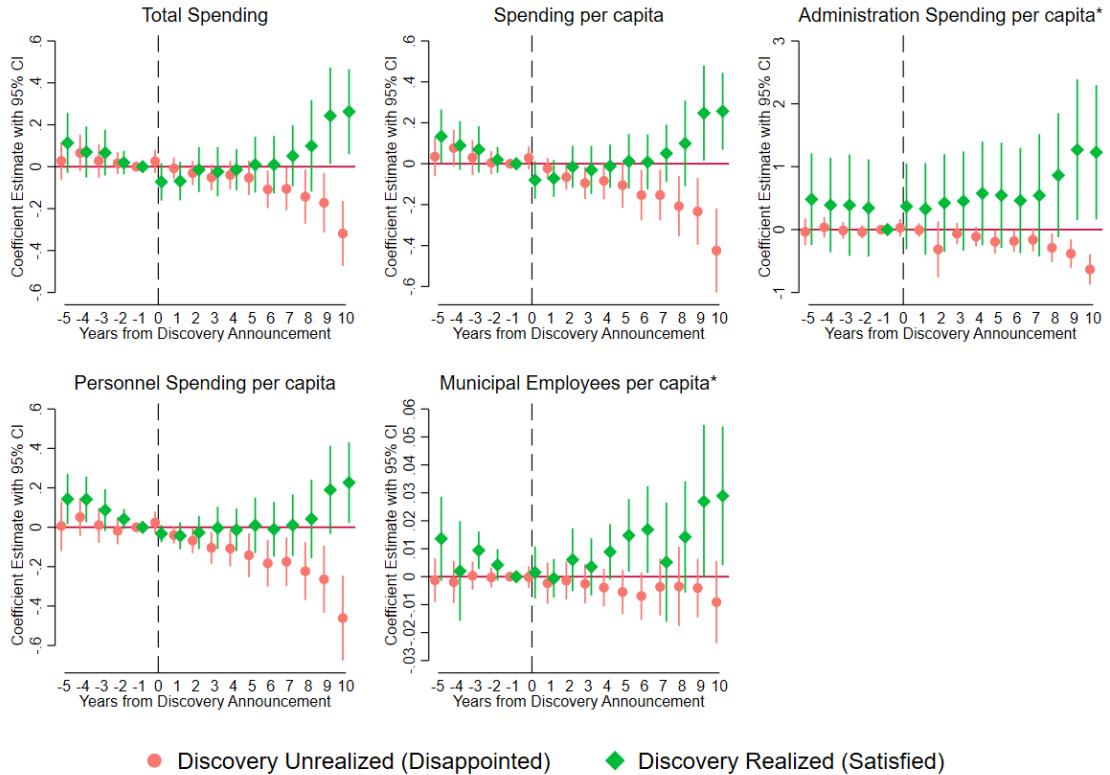
Figure 11: Revenues



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 regress each outcome on relative year indicators (relative to  $t-1$ ) and municipal and year fixed effects. The relative time indicator is always set to -1 for never-treated units. 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 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>13</sup> Tax revenues trend downward in both satisfied and disappointed places after discoveries.

Figure 12: Expenditures and Employment



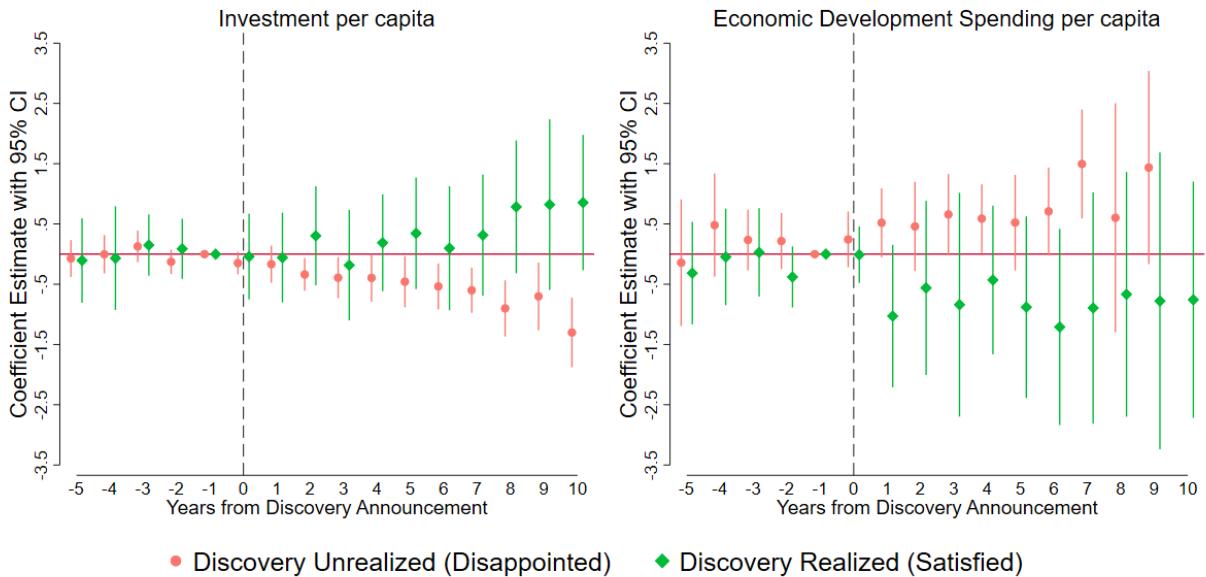
Note: Event studies regress outcomes on relative year indicators (relative to  $t - 1$ ) and municipal and year fixed effects. Relative time indicator is set to -1 for never-treated units. Continuous outcomes are transformed using inverse hyperbolic sine transformation. Monetary values are deflated to 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>13</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 A8. In disappointed municipalities, transfers pegged to population (FPM), students (FUNDEF/FUNDEB), and exports (Lei Kandir) decline significantly beginning two years after discoveries.

Due to limits on deficit spending imposed by Brazil's fiscal responsibility law, 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 but is never statistically distinguishable from never-treated controls. 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 studies regress outcomes on relative year indicators (relative to  $t - 1$ ) and municipal and year fixed effects. Relative time indicator is set to -1 for never-treated units. 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 development spending”). While estimates of this outcome are noisy due to incomplete reporting, Figure 13 suggests that satisfied municipalities never increase spending on economic development of non-extractive sectors, despite increasing spending in nearly every other category. Disappointed municipalities weakly increase spending on economic diversification, suggesting they seek to promote other sectors when oil revenues fail to be realized.

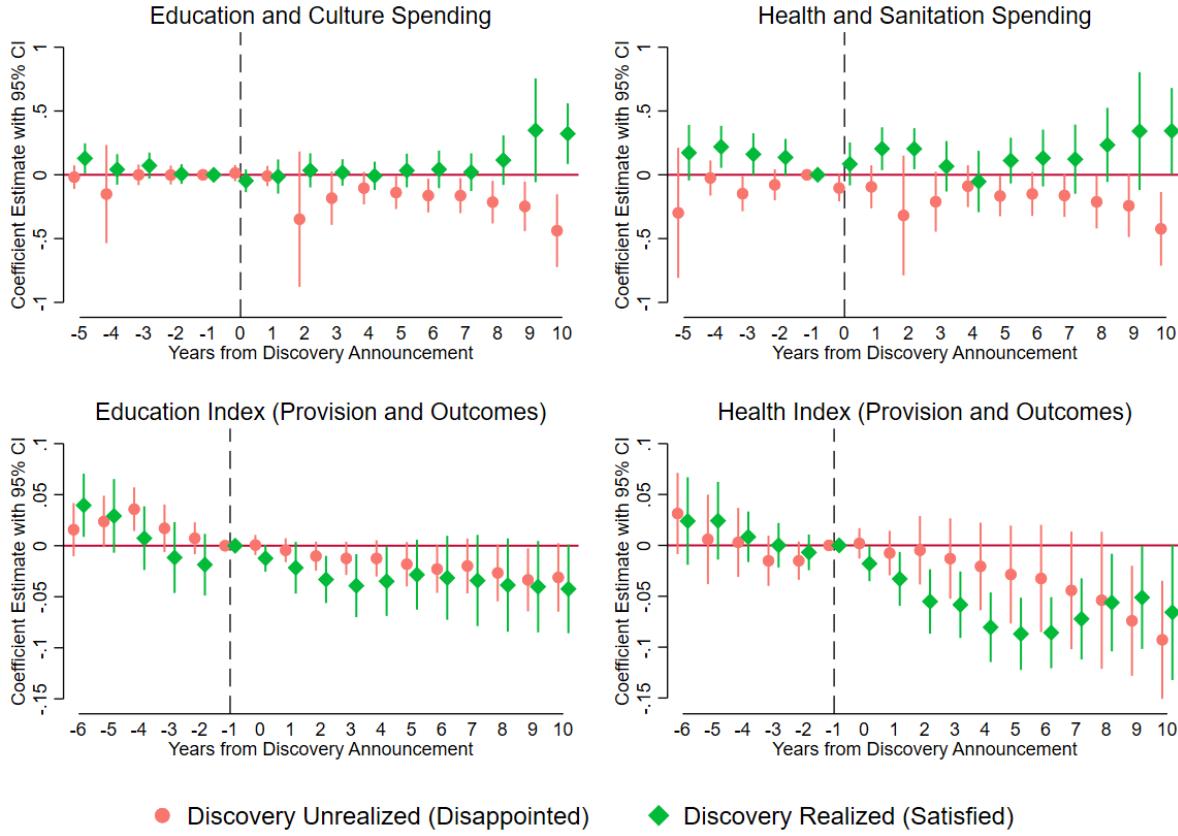
To assess the state of “fiscal health” in discovery-affected municipalities, I compute measures proposed by [FIRJAN \(2019\)](#), a Brazilian industry association, and report estimation results in Appendix A9.<sup>14</sup> In satisfied municipalities, budget share going to investment rises while the 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 A10, 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, 2020). 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 weakly significant or significant declines for these communities. These results corroborate previous findings by [Caselli and Michaels \(2013\)](#). The disconnect between increased spending and lack of improvement in real public goods provision and outcomes suggests that municipalities dealing with major oil booms may lack the capacity to spend new money efficiently or may suffer leakage of revenue windfalls into corruption. Alternatively, improvement in real public goods outcomes may come with

---

<sup>14</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).

Figure 14: Public Goods Spending & Performance Indices



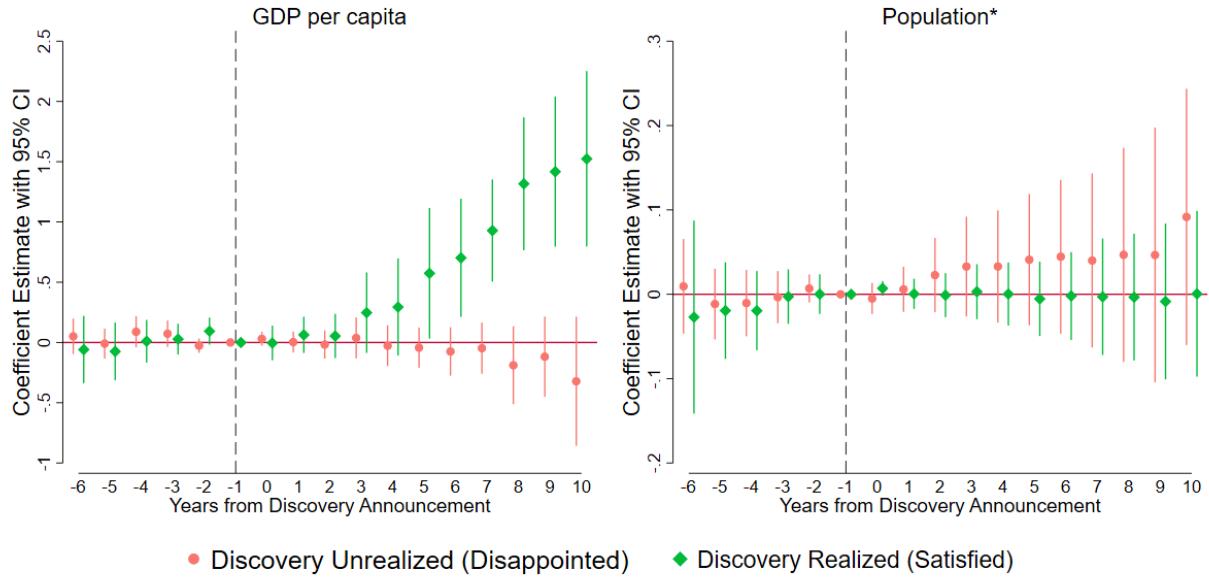
Note: Education and Health Indices are drawn from the FIRJAN Municipal Development Index (FIRJAN, 2020), a comprehensive measure of municipal development published annually by FIRJAN, a nonprofit. The Education Index is an aggregate score ranging from 0-1, composed of the following indicators: early 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 studies regress outcomes on relative year indicators (relative to  $t - 1$ ) and municipal and year fixed effects. Relative time indicator is set to -1 for never-treated units. 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.

too much of a delay to appear in sample. Disappointed municipalities experience declining per capita education (-25.6%) and health (-26.2%) spending after 10 years, and also show weakly declining or significantly declining indices of provision and outcomes. In Appendix A11, 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

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 A12). 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 studies regress outcomes on relative year indicators (relative to  $t - 1$ ) and municipal and year fixed effects. Relative time indicator is set to -1 for never-treated units. 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.

### *Event Studies using Callaway and Sant'Anna (2020) did Estimator*

Event studies with staggered treatment timing and heterogeneous treatment effects may yield biased treatment effect estimates when using a standard two way fixed effects (TWFE) estimator (Goodman-Bacon, 2018; de Chaisemartin and D'Haultfoeuille, 2020; Sun and Abraham, 2020). I implement the doubly robust estimator of group-time average treatment effects proposed by Callaway and Sant'Anna (2020) (CS), which provides an unbiased estimate of the average treatment effect on the treated (ATT). In Tables 5 and 6, I compare key outcomes in the  $t+10$  period across different combinations of control group (wells versus pre-matched) and estimator (TWFE versus CS) to assess robustness of event study results. In general, results are stable across estimators and control samples. In Appendix C11, I reproduce CS-estimator event studies that are directly comparable with the main TWFE specifications,

and find that estimates of dynamic treatment effects remain similar to TWFE estimates. To interpret treatment effect magnitudes for selected outcomes of interest, I compute small sample bias corrected semi-elasticities in the  $t+10$  period for each control group-estimator pair. I report semi-elasticity estimates in Appendix B5.

Table 5: Robustness Across Samples and Estimators: Treatment Effects (10 Yrs After Event) on Selected Outcomes in **Disappointed** Municipalities

<i>Outcome</i>	TWFE Wells	TWFE Pre-Matching	CS Wells	CS Pre-Matching
<i>Total Revenue</i>	-0.20** (0.08)	-0.07 (0.07)	-0.38*** (0.10)	-0.14 0.15
<i>Revenue p.c.</i>	-0.26** (0.11)	-0.23** (0.10)	-0.54*** (0.17)	-0.37** 0.19
<i>Tax Revenue p.c.</i>	-0.35 (0.23)	-0.34* (0.18)	-0.26 (0.29)	-0.30 0.24
<i>Oil Revenue p.c.</i>	0.16 (0.43)	0.50 (0.39)	-0.03 (0.72)	0.16 0.69
<i>Transfer Revenue p.c.</i>	-0.07* (0.04)	-0.06* (0.04)	-0.14** (0.07)	-0.15*** 0.06
<i>Total Spending</i>	-0.17** (0.07)	0.00 (0.07)	-0.30*** (0.10)	-0.03 0.12
<i>Spending p.c.</i>	-0.23*** (0.08)	-0.14* (0.07)	-0.46*** (0.12)	-0.25* 0.14
<i>Investment p.c.</i>	-0.70** (0.28)	-0.80*** (0.26)	-1.28*** (0.33)	-1.04*** 0.37
<i>Personnel Spending p.c.</i>	-0.26*** (0.09)	-0.16** (0.08)	-0.52*** (0.14)	-0.29* 0.15
<i>Education Spending p.c.</i>	-0.25** (0.10)	-0.19** (0.09)	-0.46*** (0.16)	-0.32** 0.14
<i>Health Spending p.c.</i>	-0.24* (0.12)	-0.33*** (0.11)	-0.43*** (0.15)	-0.33 0.20
<i>GDP p.c.</i>	-0.12 (0.17)	-0.12 (0.15)	-0.34 (0.30)	0.01 0.35
<i>Population</i>	0.05 (0.08)	0.14* (0.07)	-0.34 (0.30)	-0.02 0.06
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 for a specific control group-estimator pair. Event studies regress outcomes on relative year indicators (relative to  $t - 1$ ) and municipal and year fixed effects. Relative time indicator is set to -1 for never-treated units. Selected outcomes of interest are reported for brevity. Estimates for the  $t + 10$  period are reported, since, as evidenced in event study graphs, 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 main specification, a two-way fixed effects (TWFE) OLS estimator with municipalities that had offshore exploratory wells drilled since 2000, but no discoveries, as a control group for disappointed municipalities. Column 2 reports results using the TWFE estimator and a control group matched with disappointed municipalities on baseline characteristics using coarsened exact matching. Column 3 and 4 use Callaway and Sant'Anna's (CS) (2020) did package for staggered event studies in R (seed = 25475183, bootstrap iterations = 1,000) with wells and pre-matched control samples, respectively. Standard errors are clustered at the municipality level in all columns. 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 6: Robustness Across Samples and Estimators: Treatment Effects (10 Yrs After Event) on Selected Outcomes in **Satisfied** Municipalities

<i>Outcome</i>	TWFE Wells	TWFE Pre-Matching	CS Wells	CS Pre-Matching
<i>Total Revenue (Millions)</i>	0.65*** (0.20)	0.83*** (0.19)	0.76*** (0.25)	0.89*** (0.29)
<i>Revenue p.c.</i>	0.66*** (0.20)	0.77*** (0.19)	0.74*** (0.25)	0.87*** (0.28)
<i>Tax Revenue p.c.</i>	-0.21 (0.30)	0.07 (0.26)	0.02 (0.29)	0.22 (0.31)
<i>Oil Revenue p.c.</i>	4.35*** (0.68)	4.49*** (0.69)	4.69*** (0.95)	4.45*** (1.01)
<i>Transfer Revenue p.c.</i>	0.04 (0.05)	0.08 (0.05)	0.05 (0.06)	0.04 (0.06)
<i>Total Spending (Millions)</i>	0.24** (0.12)	0.43*** (0.11)	0.28** (0.11)	0.45*** (0.13)
<i>Spending p.c.</i>	0.25** (0.12)	0.38*** (0.11)	0.25** (0.11)	0.43*** (0.13)
<i>Investment p.c.</i>	0.82 (0.71)	0.92 (0.72)	1.44* (0.82)	1.43 (0.96)
<i>Personnel Spending p.c.</i>	0.19* (0.11)	0.32*** (0.10)	0.26** (0.12)	0.50*** (0.13)
<i>Education Spending p.c.</i>	0.35* (0.20)	0.41** (0.19)	0.35*** (0.13)	0.45*** (0.10)
<i>Health Spending p.c.</i>	0.34 (0.23)	0.31 (0.19)	0.42** (0.19)	0.35* (0.19)
<i>GDP p.c.</i>	1.42*** (0.31)	1.51*** (0.30)	1.59*** (0.53)	1.82** (0.71)
<i>Population</i>	-0.01 (0.05)	0.06 (0.04)	0.35*** (0.13)	0.04 (0.10)
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 for a specific control group-estimator pair. Event studies regress outcomes on relative year indicators (relative to  $t - 1$ ) and municipal and year fixed effects. Relative time indicator is set to -1 for never-treated units. Selected outcomes of interest are reported for brevity. Estimates for the  $t + 10$  period are reported, since, as evidenced in event study graphs, 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 main specification, a two-way fixed effects (TWFE) OLS estimator with municipalities that had offshore exploratory wells drilled since 2000, but no discoveries, as a control group for satisfied municipalities. Column 2 reports results using the TWFE estimator and a control group matched with satisfied municipalities on baseline characteristics using coarsened exact matching. Column 3 and 4 use Callaway and Sant'Anna's (CS) (2020) did package for staggered event studies in R (seed = 25475183, bootstrap iterations = 1,000) with wells and pre-matched control samples, respectively. Standard errors are clustered at the municipality level in all columns. 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

### ***Event Study Robustness to Alternative Matching and Forecasting Specifications***

The revenue forecasting model involves assumptions of model parameters and functional forms. Likewise, coarsened exact matching is subject to decisions regarding which variables to include in the matching algorithm and which cutoffs to define. To test the robustness of event study findings to

model selection, I re-estimate event studies for key variables (per capita revenues, oil revenues, non-oil transfer revenues, investment, education spending, health spending, GDP per capita, and population) under two forecasting variations (my preferred model as well as an alternative model with the lowest version of all adjustment parameters, to reflect possible pessimism or caution that could be built into municipalities' discovery expectations), and three matching variations (my preferred medium specification, as well as loose (geographical) matching and strict matching that includes income inequality and share of public employees). I also construct alternative matched control samples by matching on baseline per capita revenues and spending. Finally, I re-estimate event studies using the full sample of municipalities in coastal states. I report results from these robustness exercises in Appendix C16. Results are relatively stable across alternative specifications.

### ***Discovery Effects on Local Elections***

Do discovery announcements increase political competition and fundraising in municipal elections? Do they affect candidate or winner characteristics or levels of public employment patronage? 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 assumes a value of one when a major discovery announcement affected the municipality in the four years leading up to a local election. Each column of Table 7 presents results from a different combination of control sample (Wells and Pre-Matched) and estimator (TWFE and [Callaway and Sant'Anna \(2020\)](#)), to check robustness.

Results suggest that a major discovery announcement in the four years prior to an election significantly increases the number of competitive candidates running for council.<sup>15</sup> Discoveries do not have an effect on the number of candidates running for mayor. Discoveries weakly increase the number and value of donations and donations per candidate. 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 the same context, this evidence may suggest that individuals with lower private sector opportunity costs may attempt to run for office in expectation of future oil rents. In Appendix B7, 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

---

<sup>15</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.

jobs (*cargos comissionados*) after their candidate wins a local election. Results indicate that discovery announcements reduce the average schooling levels of elected candidates. Announcements have no effects on the intensity of patronage.

Table 7: Discovery Effects On Electoral Competition, Fundraising, and Candidate Characteristics: Robustness Across Samples and Estimators

	TWFE Wells	TWFE Pre-Match	CS Wells	CS Pre-Match
<i>Council Candidates (Total)</i>	0.131 (0.122)	0.046 (0.032)	0.172 (0.235)	0.070* (0.037)
<i>Council Candidates (Compet.)</i>	0.070 (0.061)	0.061* (0.034)	0.098* (0.105)	0.066 (0.037)
<i>Mayoral Candidates (Total)</i>	0.041 (0.052)	0.035 (0.048)	0.065 (0.068)	0.054 (0.050)
<i>Mayoral Candidates (Compet.)</i>	0.001 (0.046)	0.008 (0.047)	-0.129*** (0.045)	-0.087* (0.046)
<i>Comp. Council Cand. Per Seat</i>	0.047** (0.019)	0.038** (0.018)	0.068*** (0.025)	0.033 (0.022)
<i>Avg. Coalition Size</i>	-0.081** (0.037)	-0.078*** (0.028)	-0.118* (0.062)	-0.077* (0.041)
<i>Total Number of Donations</i>	0.169* (0.087)	0.149 (0.091)	0.157* (0.092)	0.164** (0.069)
<i>Total Value of Donations</i>	0.131* (0.078)	0.119 (0.083)	0.238** (0.120)	0.114 (0.113)
<i>Share of Candidates Female</i>	-0.008 (0.007)	-0.016*** (0.005)	-0.010 (0.010)	-0.006 (0.120)
<i>Avg. Candidate Age</i>	0.001 (0.005)	-0.002 (0.004)	-0.031** (0.014)	0.000 (0.011)
<i>Avg. Candidate Schooling</i>	-0.030*** (0.009)	-0.024*** (0.006)	-0.031** (0.014)	-0.009 (0.010)
Municipality FEs	Y	Y	Y	Y
Election Period FEs	Y	Y	Y	Y
n (municipality-election periods)	404	3,745	404	3,745

This 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,  $\delta_m$  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. 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 results using a two-way fixed effects (TWFE) OLS estimator with the wells control group. Column 2 reports results using the TWFE estimator and control matched on baseline characteristics. Column 3 and 4 use Callaway and Sant'Anna's (CS) (2020) did package for staggered difference-in-differences in R with wells and pre-matched control samples, respectively. 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

When there is a shortfall between discovery expectations and realized royalties, are incumbent politicians punished with reduced reelection rates?<sup>16</sup> This question relates to Monteiro and Ferraz (2010), who find that politicians are rewarded electorally for early revenue windfalls. Since voters cannot

<sup>16</sup>There is unsettled and mixed evidence (e.g., Healy et al. (2010), Fowler and Pablo Montagnes (2015)) that voters punish or reward incumbent politicians for truly exogenous events.

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.

Table 8: Effects of Disappointment or Satisfaction with Oil Discoveries in Last Four Years on Incumbent Reelection Rates

	Disappointed		Satisfied	
	LPM	Logit	LPM	Logit
<i>Mayor</i>	-0.119*	-0.136	-0.006	-0.006
	(0.070)	(0.089)	(0.034)	(0.034)
Controls	Y	Y	Y	Y
State FEs	Y	Y	Y	Y
Election Period FEs	Y	Y	Y	Y
<i>n</i> (candidate-election periods)	10,815	10,815	10,850	10,850
<i>Council</i>	-0.052***	-0.042***	-0.005	-0.008
	(0.017)	(0.016)	(0.009)	(0.010)
Controls	Y	Y	Y	Y
State FEs	Y	Y	Y	Y
Election Period FEs	Y	Y	Y	Y
<i>n</i> (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. Specifications with controls include candidate age, sex, and education level as explanatory variables. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

### *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?

Decomposing revenue sources reveals that local tax revenues trend downward in disappointed municipalities (declining 37% ten years on from a discovery, significant at the 10% level). Brazilian municipalities collect taxes on real estate transactions (ITBI), service providers (ISS), and urban properties (IPTU) ([Egestor, 2020](#)). Declines in tax revenues may therefore be due to a weakening tax base among real estate and property sectors. The scope for private-sector spillovers from oil discoveries is largely limited to sectors that benefit from public spending, e.g., construction. As firms and workers in this sector are mobile, they could feasibly move to nearby satisfied municipalities that have windfalls to spend on infrastructure, causing reductions in construction activity, and thus ITBI and IPTU tax collections in disappointed municipalities. In Appendix A13, I draw on panel data from RAIS to estimate the dynamic effects of discovery announcements and subsequent realizations on private sector employment, disaggregated by into major sectors. Results indicate that formal construction employment fell significantly in disappointed places after discovery announcements (-25% after 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 A8 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](#)).

Budgetary dynamics may combine with political economic effects to reduce governing capacity

and performance. Lower-education candidates win office after discovery announcements, and disappointment at the time of elections leads to increased political turnover. These dynamics may disrupt public goods provision and increase rent-seeking and corruption among politicians ([Akhtari et al., 2021](#); [Baragwanath, 2020](#); [Toral, 2021](#)).

## 8 Discussion and Policy Implications

Existing literature on the Presource Curse has documented widespread disappointment, fiscal dysfunction, and corruption after major resource 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 public finances, elections, and private sector outcomes.

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 simple 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. Evidently, institutions such as Brazil's Fiscal Responsibility Law 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 do not improve real public goods provision or invest in economic diversification. Disappointed municipalities 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 and forecasting specifications. Results are also robust to estimation with the [Callaway and Sant'Anna \(2020\)](#) *did* estimator. I conclude that outcomes in disappointed municipalities provide evidence of a Presource 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 (depending on my choice of control group). 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 nontradeables.

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 A4). 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. Supporting this simulation evidence, a review of industrial indicators by [de Paula \(2016\)](#) finds evidence of deindustrialization in Brazil over the last decade.

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 major 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. Specific to the oil sector, companies making discovery declarations to Brazil's CVM were for the most part transparent and honest in their statements during this period. However, the case of OGX, a company that made precocious, dramatic discovery announcements before collapsing and leaving fields undeveloped, highlights the importance of requiring corporate good

governance and transparency ([Moreno, 2013](#)). 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.

Finally, my approach to quantifying heterogeneous discovery realizations makes a methodological contribution to the study of the Presource Curse and Resource Curse. By documenting dramatic divergence in outcomes between disappointed versus satisfied places after discoveries, I highlight the importance of taking timing and heterogeneous discovery realizations into account 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.
- de Chaisemartin, C. and D'Haultfoeuille, X. (2020). Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. *American Economic Review*, 110(9):2964–2996.
- de Paula, G. M. (2016). Deindustrialization in Brazil? In *The New Brazilian Economy*, pages 63–85.
- 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.
- Fowler, A. and Pablo Montagnes, B. (2015). College football, elections, and false-positive results in observational research. *Proceedings of the National Academy of Sciences of the United States of America*, 112(45):13800–13804.
- 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.

- Han, E. S., Goleman, D., Boyatzis, R., and McKee, A. (2019). Recovery rates, enhanced oil recovery and technological limits. *Journal of Chemical Information and Modeling*, 53(9):1689–1699.
- 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*.
- Healy, A. J., Malhotra, N., and Mo, C. H. (2010). Irrelevant events affect voters’ evaluations of government performance. *Proceedings of the National Academy of Sciences of the United States of America*, 107(29):12804–12809.
- 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. *EconomiA*, 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*.

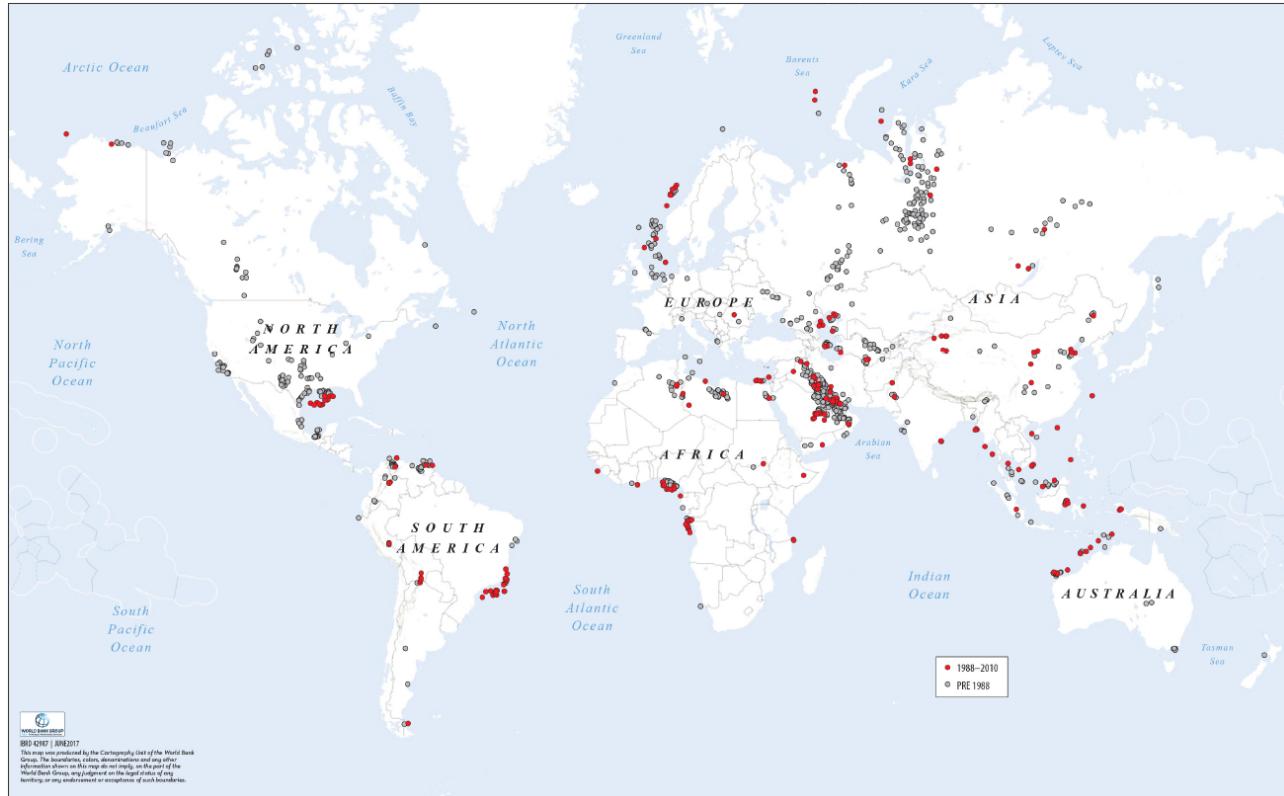
- Monteiro, J. and Ferraz, C. (2010). Does Oil Make Leaders Unaccountable? Evidence from Brazil's offshore oil boom. *Unpublished Working Paper - PUC-Rio*, pages 1–67.
- Moreno, F. (2013). Relembre a trajetória da OGX, da fundação à recuperação judicial.
- Niemi, R. G. and Hsieh, J. F. S. (2002). Counting 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 Construção de Séries Temporais de Alta Frequência das Finanças Públicas Municipais no Brasil com Aplicação para o IPTU e o ISS: 2004-2010. *IPEA Texto para Discussão*, 1632.
- Pacheco, C. (2003). A Aplicação e o Impacto dos Royalties do Petróleo no Desenvolvimento Econômico dos Municípios 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). Contribuições para o debate acerca da repartição dos royalties petrolíferos 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. (2020). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*.
- Tesouro Nacional, . (2018). O Que Voce Precisa Saber Sobre Transferencias Constitucionais e Legais. Technical report, Ministerio da Fazenda.
- 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

## Appendix A Supplementary Figures

Figure A1: Giant Oil Discoveries Since 1988



Source: [Cust and Mihalyi \(2017\)](#)

Figure A2: Timing of Well Stages Relative to CVM Announcements

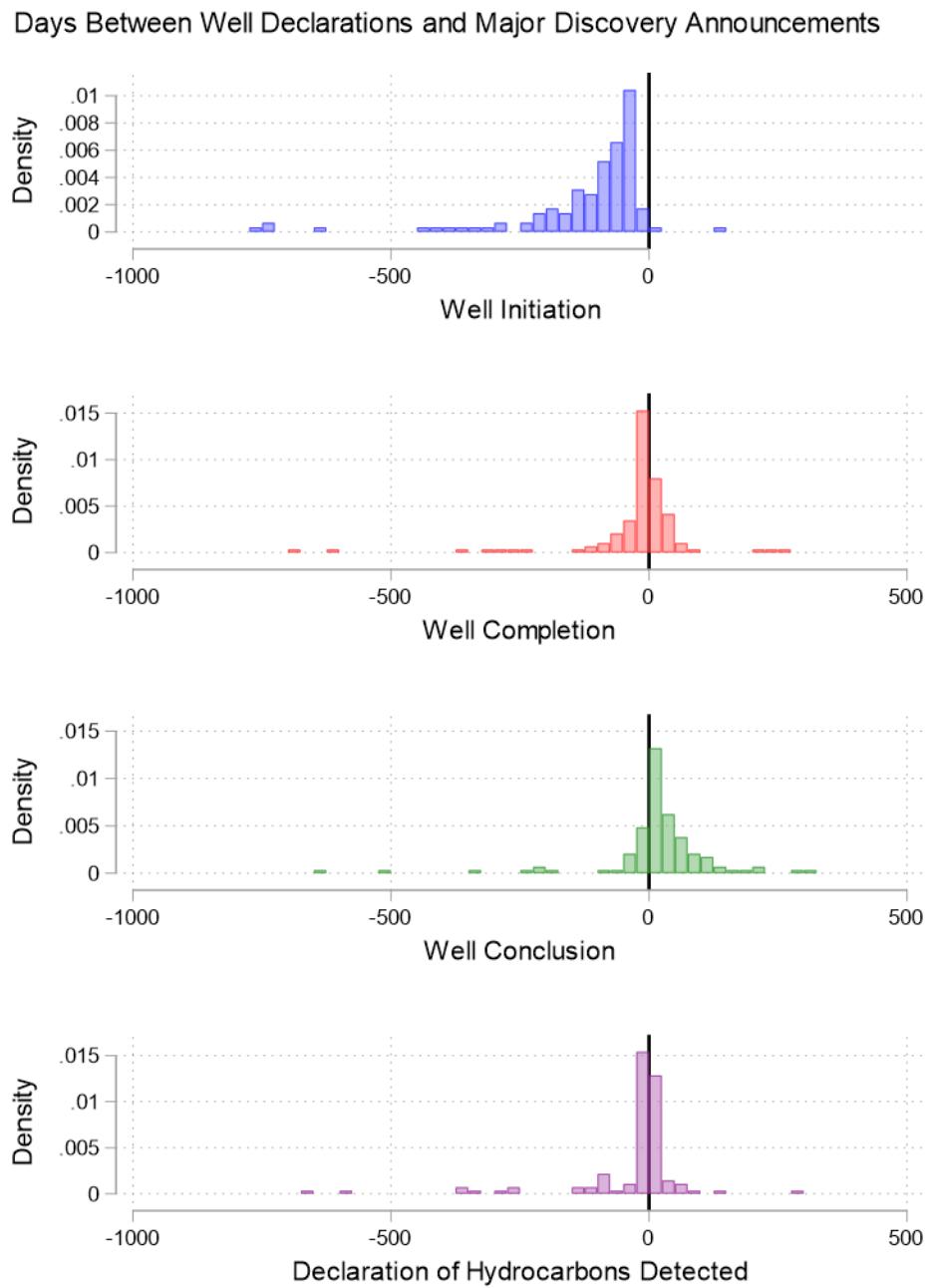


Figure A3: Examples: Offshore Discovery Announcement and Subsequent News Coverage

(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 11 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.

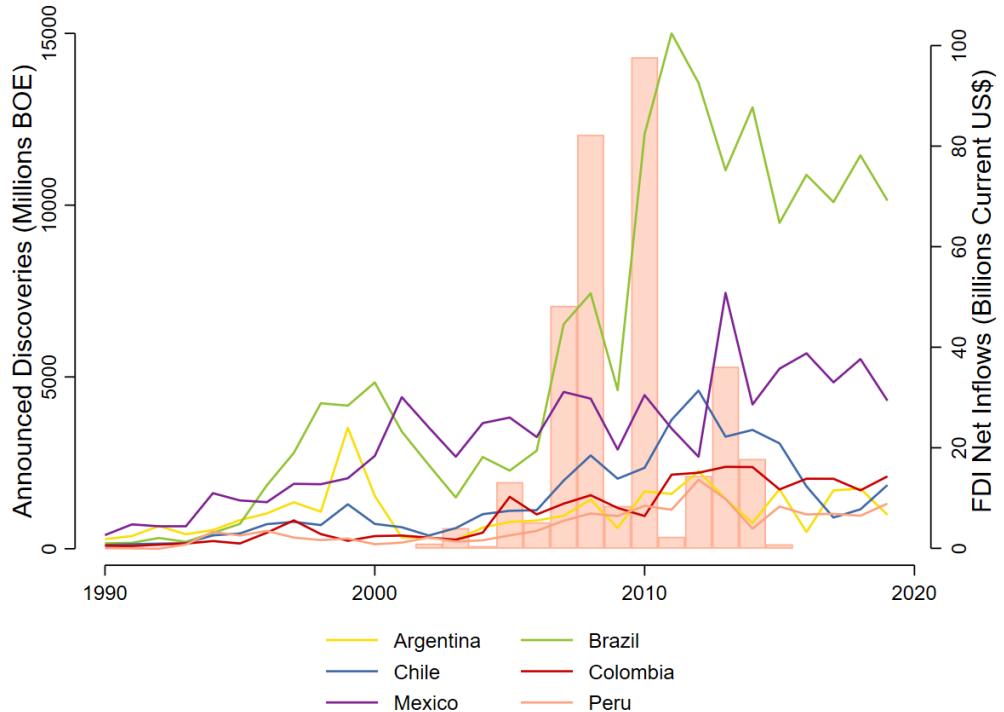
Após a conclusão da perfuração, o consórcio formado pela Petrobras, BG Group e Galp, para a exploração do bloco BM-S-11, onde fica a área de Tupi, dará continuidade às atividades e investimentos previstos no Plano de Avaliação aprovado pela ANP e que prevê a perfuração de outros poços na área.

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".

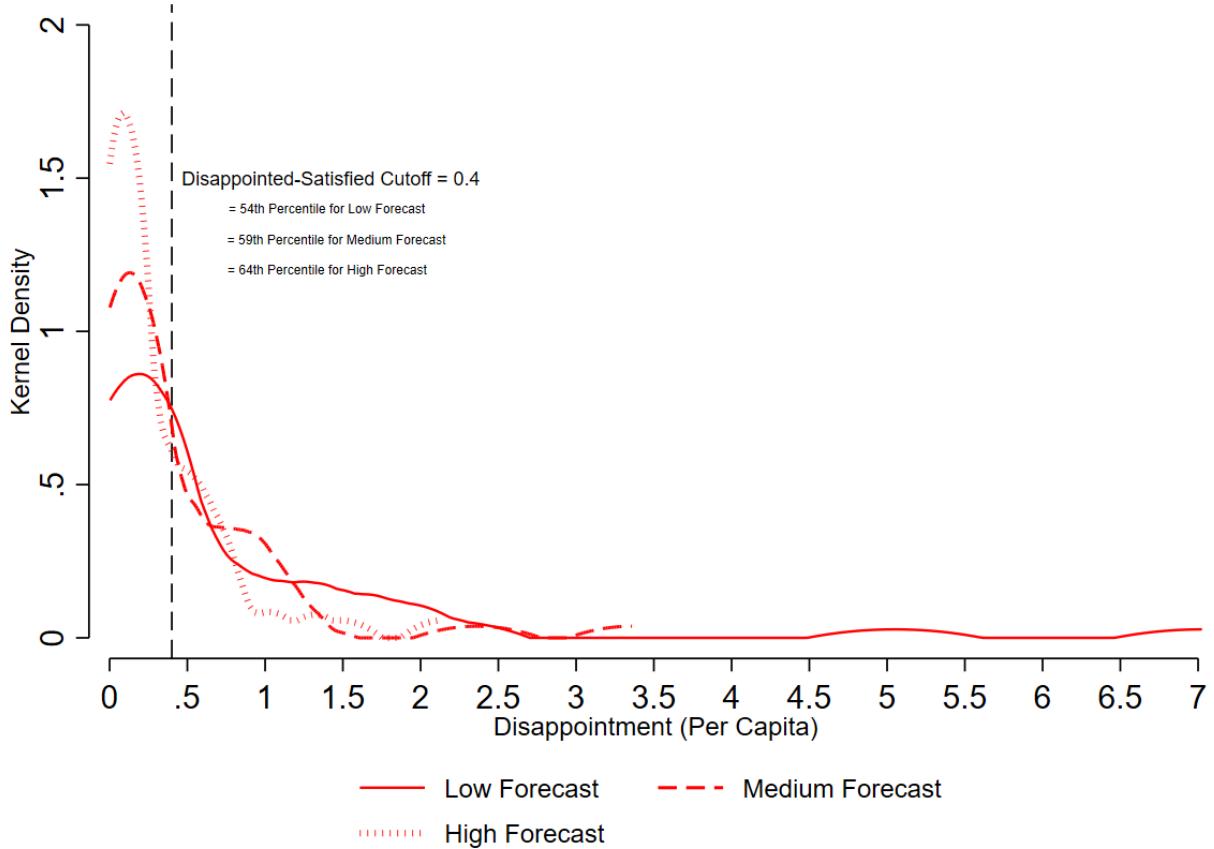
Lêia também: Exploração no pré-sal começa sem nova regulação ©NotíciasFinancieras - ©GDA- Agencia Globo  
All rights reserved

Figure A4: Foreign Direct Investment Net Inflows to Selected Latin American Countries, Overlaid on Brazilian Discovery Announcements



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

Figure A5: 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 A6: Brazil: Major Offshore Discoveries and Affected Municipalities

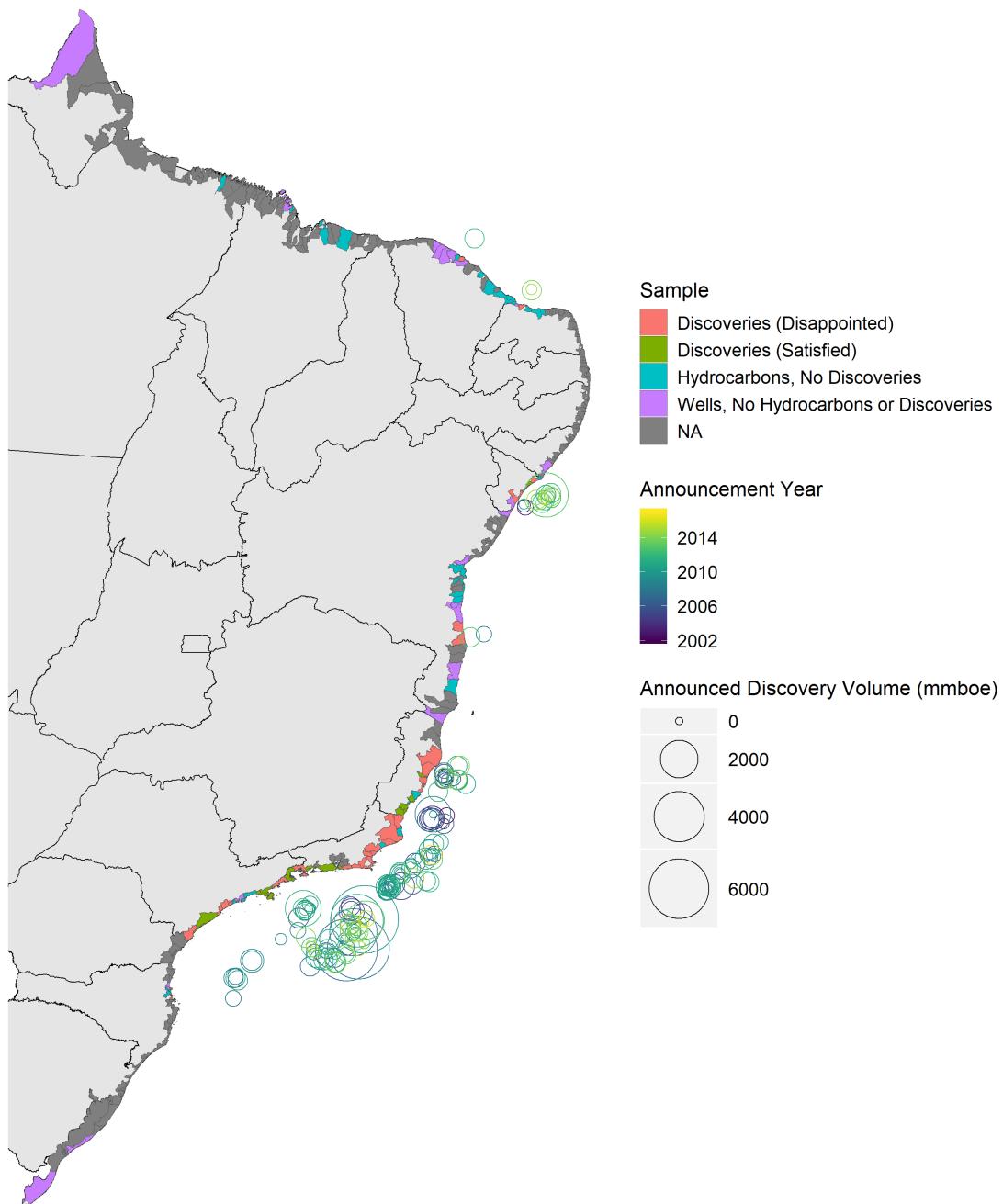
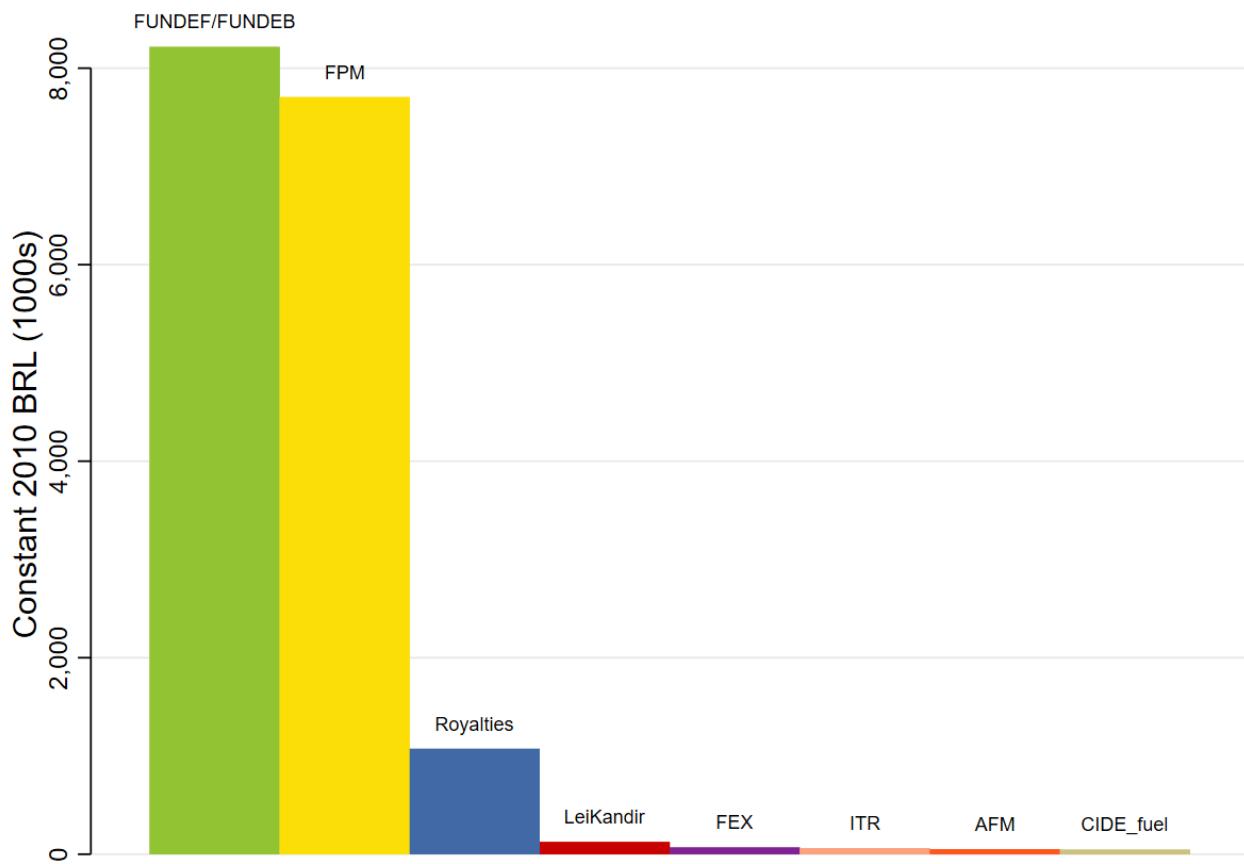
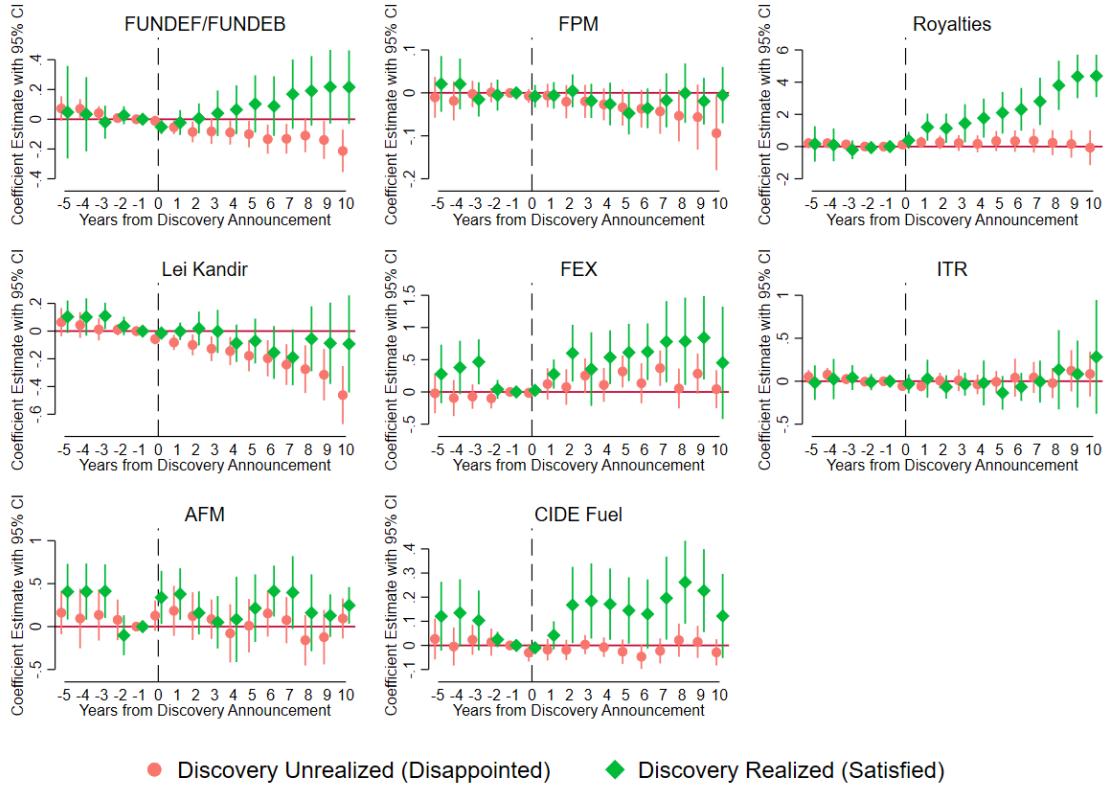


Figure A7: Average Federal Transfers Received by Municipal Government (2000-2017)



Note: As illustrated in Figure P2, 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).

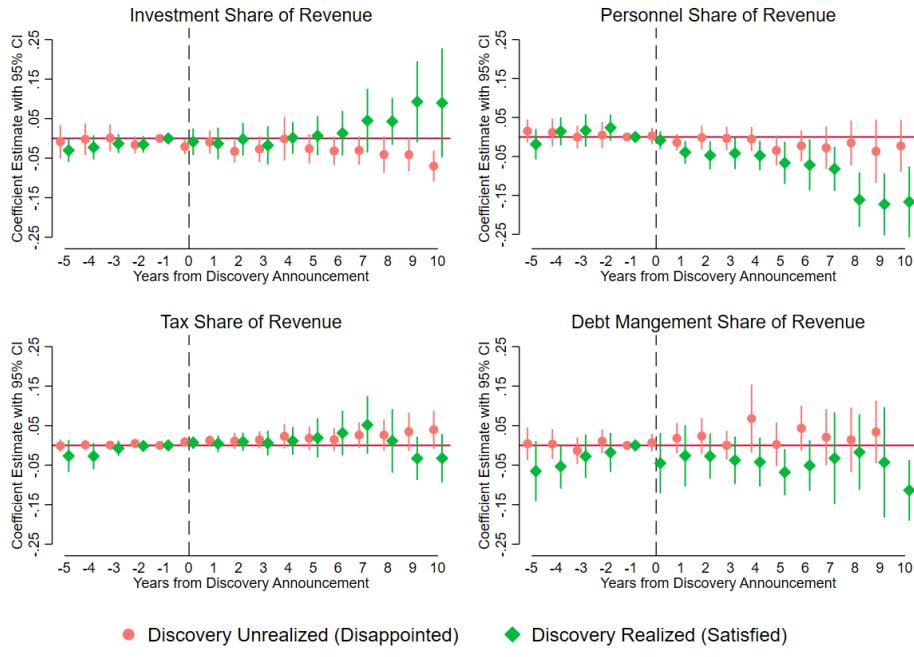
Figure A8: Federal and State Transfers (per capita)



Note: Formulaic federal and state transfers to municipal governments ([Tesouro Nacional, 2018](#)):

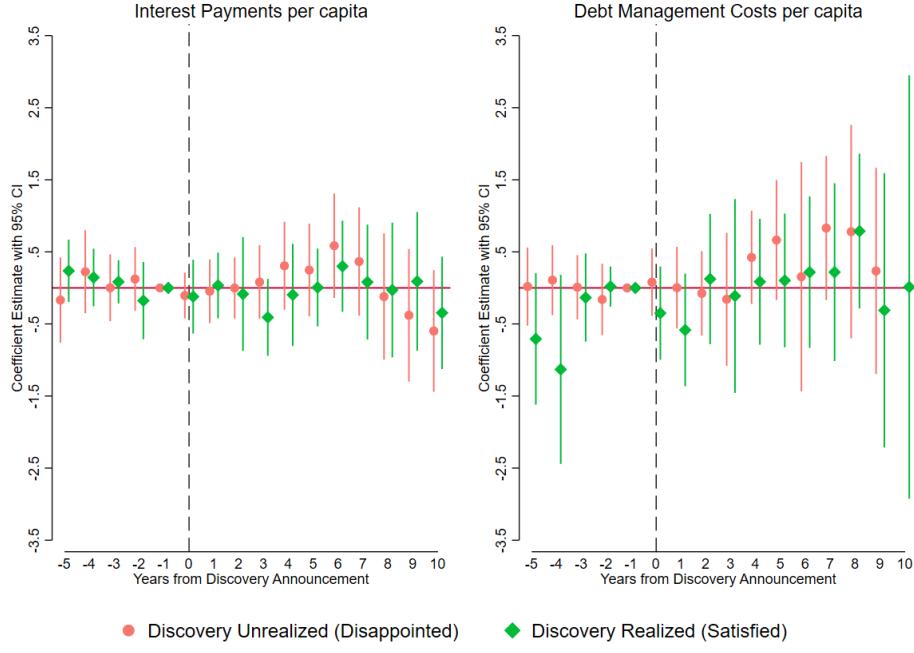
- **FUNDEF (to 2006)/FUNDEB (2007 onwards)** (*Fundo de Manutenção e Desenvolvimento do Ensino Fundamental e de Valorização do Magistério/Fundo de Manutenção e Desenvolvimento da Educação Básica e de Valorização dos Profissionais da Educação*): Federal and state transfers to municipal governments to 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 A9: Fiscal Health Indicators



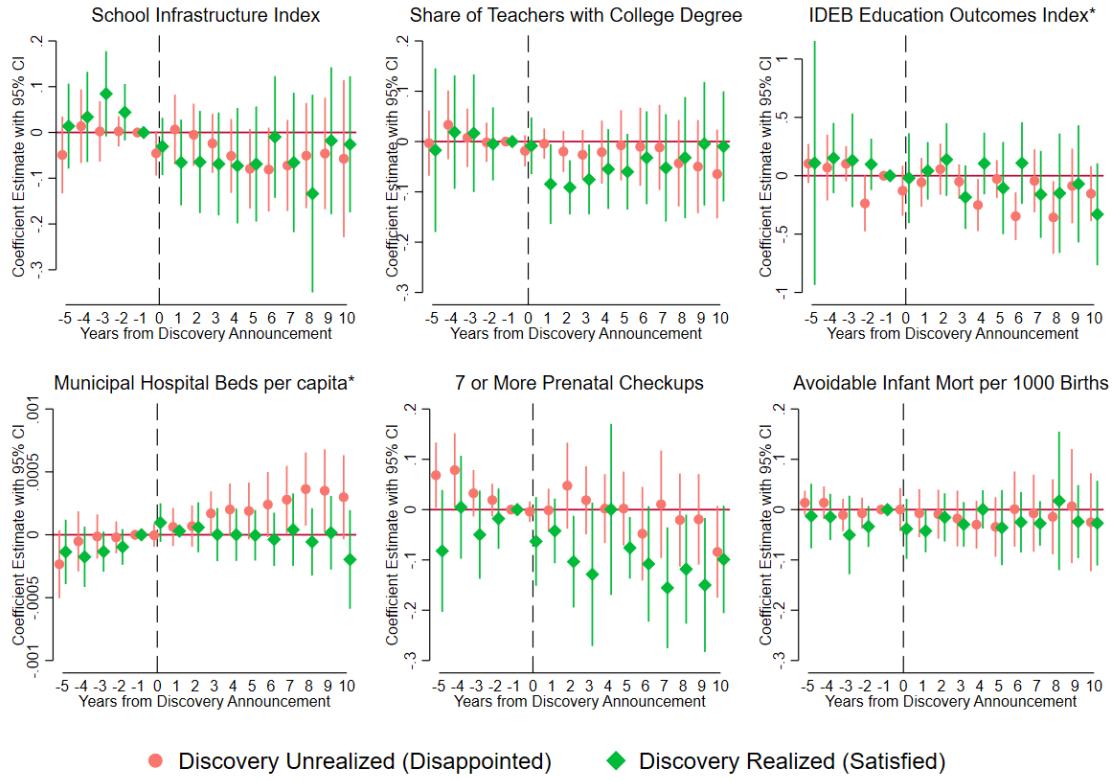
Note: Debt share of revenue is taken by summing expenditures on debt (processed, unprocessed, and liquidating), debt service, debt restructuring, interest, and *restos a pagar*.

Figure A10: Debt



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

Figure A11: Public Goods Provision and Quality



Note: School infrastructure index 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.

\* 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.

In Figure A11, I dig into real public goods provision, quality, and outcomes by estimating dynamic discovery effects on measures of school and health infrastructure (i.e., libraries and science and computer labs; municipal hospital beds), service quality (i.e., share of teachers with a college degree; share of pregnant women receiving seven or more prenatal checkups), and outcomes (IDEB index of graduation rates and test scores; avoidable infant mortalities per 1,000 births). Estimates are noisy and largely insignificant for both disappointed and satisfied municipalities. The share of pregnant women receiving adequate prenatal care appears to fall in satisfied municipalities after discoveries, as does the share of teachers with higher-education in years immediately following discoveries. Surprisingly, municipal hospital beds per capita increase in disappointed municipalities after discoveries, though point estimates are extremely small. In sum, while estimates are noisy and mostly insignificant, it is possible to conclude that real public goods provision and outcomes did not improve in satisfied places, despite significant increases in spending.

Figure A12: In-Migration After Discovery Announcements

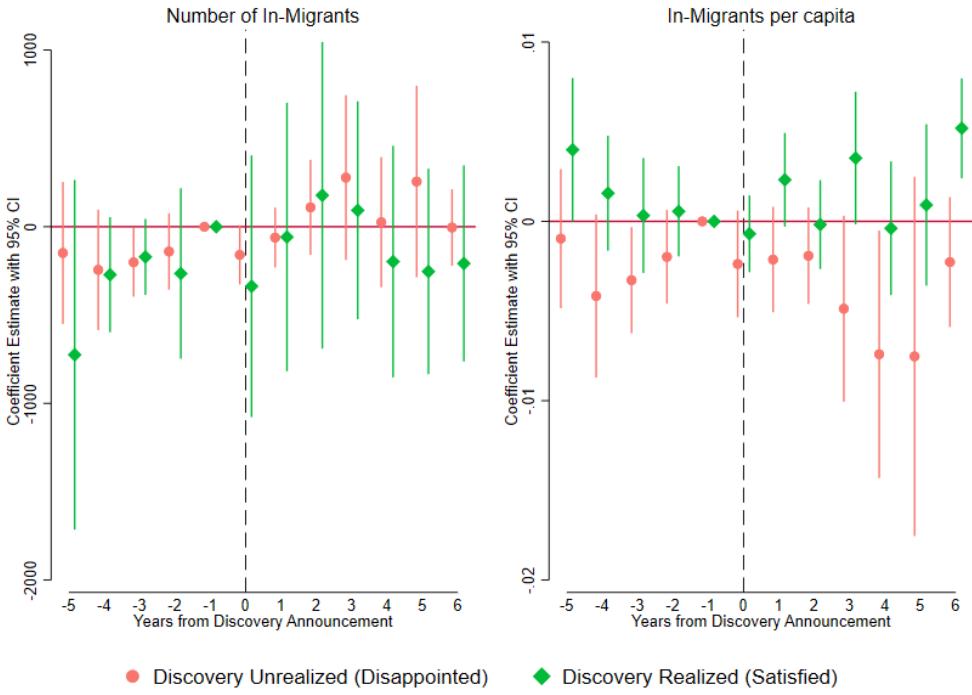
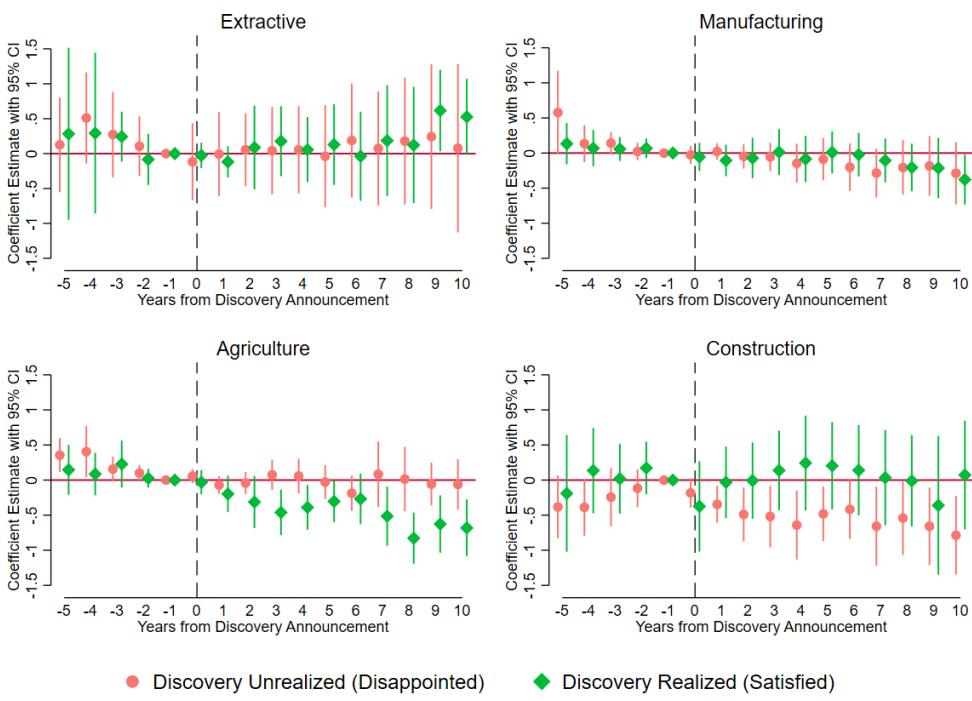


Figure A13: Formal Employment by Sector



## Appendix B Supplementary 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
CAMPOSDOSGOYTACAZES33	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
SAOFRANCISCOODEITABAPOANA33	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
VILAVELHA32	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	S	D	D	S	S
MANGARATIBA33	S	S	S	S	S	S
MARATAIZES32	S	S	D	D	S	D
MARICA33	S	S	D	D	S	D
NITEROI33	S	S	S	S	S	S
PARATI33	S	S	S	D	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
<b>Total Disappointed</b>	<b>30</b>	<b>33</b>	<b>35</b>	<b>28</b>	<b>30</b>	<b>33</b>
<b>Total Satisfied</b>	<b>18</b>	<b>15</b>	<b>13</b>	<b>20</b>	<b>18</b>	<b>15</b>
<b>Percent Disappointed</b>	<b>62.5</b>	<b>68.8</b>	<b>72.9</b>	<b>58.3</b>	<b>62.5</b>	<b>68.8</b>

Table B3: Disappointed: Sample Properties, Coefficient Estimates, Semi-Elasticity Estimates

Outcomes	Sample Properties			Coefficients			Small-n Bias Correct. Elast.		
	$\bar{X}$	n	Units	1 Year	5 Years	10 Years	1 Year	5 Years	10 Years
<i>Total Revenue (Millions)</i>	162	1,392	83	0.00 (0.02)	-0.04 (0.04)	-0.20** (0.08)	-0.64 (2.12)	-6.28 (3.92)	-20.79*** (6.02)
<i>Revenue p.c.</i>	2,086	1,392	83	-0.01 (0.02)	-0.10 (0.06)	-0.26** (0.11)	-2.13 (2.14)	-11.86** (5.44)	-26.69*** (8.02)
<i>Tax Revenue p.c.</i>	220	1,392	83	0.14* (0.08)	-0.23 (0.17)	-0.35 (0.23)	10.93 (8.75)	-27.00** (12.09)	-37.30*** (14.28)
<i>Oil Revenue p.c.</i>	473	1,494	83	0.27 (0.17)	0.34 (0.31)	0.16 (0.43)	19.75 (20.87)	20.33 (37.60)	-5.57 (40.41)
<i>Non-Oil Transfer Rev. p.c.</i>	652	1,440	80	-0.03** (0.01)	-0.05** (0.03)	-0.07* (0.04)	-3.69*** (1.41)	-6.60*** (2.53)	-8.99** (3.94)
<i>Total Spending (Millions)</i>	88	1,392	83	-0.01 (0.03)	-0.05 (0.04)	-0.17** (0.07)	-2.04 (2.53)	-7.12* (3.82)	-18.77*** (5.81)
<i>Spending p.c.</i>	1,165	1,392	83	-0.02 (0.03)	-0.10* (0.06)	-0.23*** (0.08)	-3.45 (2.42)	-12.43** (4.89)	-23.95*** (6.23)
<i>Investment p.c.</i>	226	1,423	83	-0.17 (0.15)	-0.46** (0.21)	-0.70** (0.28)	-21.69* (12.09)	-43.14*** (12.11)	-56.92*** (12.18)
<i>Personnel Spending p.c.</i>	933	1,392	83	-0.04* (0.02)	-0.14** (0.06)	-0.26*** (0.09)	-4.92** (2.08)	-15.64*** (4.73)	-26.42*** (6.30)
<i>Education Spending p.c.</i>	571	1,392	83	-0.01 (0.04)	-0.14** (0.06)	-0.25** (0.10)	-2.93 (3.87)	-15.78*** (5.47)	-25.64*** (7.26)
<i>Health Spending p.c.</i>	449	1,392	83	-0.09 (0.08)	-0.17** (0.08)	-0.24* (0.12)	-12.76* (7.38)	-18.62*** (6.47)	-26.23*** (9.10)
<i>GDP per capita</i>	22,362	1,162	83	0.00 (0.04)	-0.04 (0.08)	-0.12 (0.17)	-1.86 (4.30)	-8.14 (7.71)	-18.27 (13.66)
<i>Population</i>	80,980	1,494	83	0.01 (0.01)	0.04 (0.04)	0.05 (0.08)	-0.09 (1.34)	2.16 (3.99)	0.87 (7.66)
<i>No. Empl. Extractive</i>	213	1,494	83	-0.01 (0.30)	-0.04 (0.37)	0.24 (0.52)	-14.62 (25.77)	-19.94 (29.41)	-1.73 (51.17)
<i>No. Empl. Mfg.</i>	2,821	1,494	83	0.02 (0.06)	-0.09 (0.15)	-0.18 (0.21)	-0.68 (5.83)	-15.05 (12.83)	-25.17 (15.93)
<i>No. Firms Extractive</i>	9.1	1,494	83	-0.12* (0.07)	-0.28** (0.13)	-0.20 (0.22)	-14.73* (6.14)	-29.30*** (9.32)	-26.79* (15.92)
<i>No. Firms Mfg.</i>	165.2	1,494	83	-0.01 (0.03)	0.01 (0.07)	-0.19* (0.10)	-2.38 (3.26)	-2.85 (7.02)	-21.26*** (7.69)
<i>Avg. Formal Wage (Monthly)</i>	1,034	1,494	83	-0.01 (0.02)	-0.08** (0.03)	-0.11** (0.05)	-2.13 (1.767)	-8.88*** (3.036)	-12.42*** (4.21)

Selected results are reported for brevity. Sample includes disappointed municipalities (received less than 40% of revenues expected from major discovery announcements by 2017) and control municipalities (received exploratory offshore wells in catchment zone after 1999, but no major discoveries). Estimates are derived from event studies with fully saturated relative year indicators. Regressions include municipality and year FE; standard errors are clustered at the municipality level. Continuous outcome variables use inverse hyperbolic sine transformation. Monetary values are deflated to constant 2010 BRL. Coefficient estimates and elasticities for 1, 5, and 10 years after the first discovery announcement are reported to convey dynamic effects. Small-n bias corrected semi-elasticities refer to the semi-logarithmic interpretation of the effects of a dummy variable on a continuous outcome transformed by the inverse hyperbolic sine function (Bellemaire and Wichman, 2020). \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table B4: Satisfied: Sample Properties, Coefficient Estimates, Semi-Elasticity Estimates

Outcomes	$\bar{X}$	Sample Properties			Coefficients			Small-n Bias Correct. Elast.		
		n	Units	1 Year	5 Years	10 Years	1 Year	5 Years	10 Years	
Total Revenue ( <i>Millions</i> )	345	1,211	71	0.05 (0.04)	0.16* (0.09)	0.65*** (0.20)	3.01 (4.62)	11.74 (10.43)	74.53** (34.21)	
Revenue p.c.	2,361	1,211	71	0.05 (0.04)	0.16 (0.10)	0.66*** (0.20)	2.74 (4.54)	11.69 (10.91)	75.12** (34.60)	
Tax Revenue p.c.	279	1,211	71	0.01 (0.09)	-0.06 (0.23)	-0.21 (0.30)	-3.23 (8.68)	-15.98 (19.50)	-30.32 (20.58)	
Oil Revenue p.c.	606	1,278	71	1.21*** (0.42)	2.10*** (0.65)	4.35*** (0.68)	170.90 (114.05)	490.53 (383.00)	544.163 (375.01)	
Non-Oil Transfer Rev. p.c.	691	1,224	68	-0.03 (0.02)	-0.01 (0.04)	0.04 (0.05)	-3.63** (1.72)	-2.59 (4.10)	1.26 (5.12)	
Total Spending ( <i>Millions</i> )	206	1,211	71	-0.07 (0.05)	0.01** (0.07)	0.24** (0.12)	-8.77 (4.20)	-2.31 (6.52)	20.35 (13.88)	
Spending p.c.	1,264	1,211	71	-0.07 (0.05)	0.01** (0.07)	0.25** (0.12)	-9.00 (0.12)	-2.15 (4.12)	20.79 (6.55)	
Investment p.c.	263	1,230	71	-0.06 (0.37)	0.34 (0.46)	0.82 (0.71)	-21.73 (29.22)	11.98 (51.91)	59.35 (113.07)	
Personnel Spending p.c.	997	1,211	71	-0.04 (0.03)	0.01* (0.07)	0.19* (0.11)	-5.86 (3.23)	-2.56 (6.87)	14.32 (14.07)	
Education Spending p.c.	627	1,208	71	-0.01 (0.07)	0.03* (0.07)	0.35 (0.20)	-4.62 (6.40)	0.07 (6.61)	28.02 (26.10)	
Health Spending p.c.	461	1,208	71	0.20** (0.08)	0.11 (0.09)	0.34 (0.23)	17.62* (9.90)	6.88 (9.61)	25.42 (29.05)	
GDP per capita	27,043	994	71	0.06 (0.08)	0.57** (0.27)	1.42*** (0.31)	2.56 (7.75)	55.00 (42.12)	253.10** (110.29)	
Population	155,964	1,278	71	0.00 (0.01)	-0.01 (0.02)	-0.01 (0.05)	-0.40 (0.89)	-1.63 (2.17)	-3.11 (4.49)	
No. Empl. Extractive	1,258	1,278	71	-0.12 (0.11)	0.13 (0.29)	0.62** (0.29)	-15.96* (9.42)	-1.57 (28.49)	60.08 (46.73)	
No. Empl. Mfg.	5,453	1,278	71	-0.11 (0.11)	0.01 (0.15)	-0.21 (0.22)	-14.98 (9.61)	-6.35 (14.06)	-27.45* (15.63)	
No. Firms Extractive	17.5	1,278	71	0.09 (0.12)	0.07 (0.17)	0.30 (0.26)	2.59 (12.29)	-1.89 (16.78)	18.90 (31.23)	
No. Firms Mfg.	273.8	1,278	71	-0.09* (0.05)	-0.12** (0.06)	-0.23** (0.11)	-10.76** (4.29)	-13.70** (4.76)	-25.03** (8.05)	
Avg. Formal Wage	1,073	1,278	71	-0.03 (0.02)	-0.01* (0.05)	0.09** (0.05)	-4.17 (1.94)	-3.84 (4.72)	-11.06** (4.66)	

Selected results are reported for brevity. Sample includes satisfied municipalities (received more than 40% of revenues expected from major discovery announcements by 2017) and control municipalities (received exploratory offshore wells in catchment zone after 1999, but no major discoveries). Estimates are derived from event studies with fully saturated relative year indicators. Regressions include municipality and year FE; standard errors are clustered at the municipality level. Continuous outcome variables use inverse hyperbolic sine transformation. Monetary values are deflated to constant 2010 BRL. Coefficient estimates and elasticities for 1, 5, and 10 years after the first discovery announcement are reported to convey dynamic effects. Small-n bias corrected semi-elasticities refer to the semi-logarithmic interpretation of the effects of a dummy variable on a continuous outcome transformed by the inverse hyperbolic sine function (Bellemann and Wichman, 2020). \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table B5: Interpreting Treatment Effects: Small-Sample Bias-Corrected Semi-Elasticities Across Samples and Estimators (10 Yrs After Event), **Disappointed** Municipalities

	TWFE Wells	TWFE Pre-Matching	CS Wells	CS Pre-Matching
<i>Total Revenue (Millions)</i>	-20.79*** (6.02)	-10.22* (5.97)	-35.17 <i>(In Progress)</i>	-19.67
<i>Revenue p.c.</i>	-26.69*** (8.02)	-24.41*** (7.42)	-46.43	-37.29
<i>Tax Revenue p.c.</i>	-37.30*** (14.28)	-35.04*** (11.62)	-33.29	-34.26
<i>Oil Revenue p.c.</i>	-5.57 (40.41)	35.46 (52.28)	-32.16	-16.70
<i>Transfer Revenue p.c.</i>	-8.99** (3.94)	-7.82** (3.36)	-15.95	-16.57
<i>Total Spending (Millions)</i>	-18.77*** (5.81)	-2.89 (6.43)	-29.35	-9.09
<i>Spending p.c.</i>	-23.95*** (6.23)	-16.44*** (6.20)	-40.48	-27.50
<i>Investment p.c.</i>	-56.92*** (12.18)	-60.59*** (10.43)	-76.50	-70.49
<i>Personnel Spending p.c.</i>	-26.42*** (6.30)	-18.33*** (6.45)	-44.28	-30.34
<i>Education Spending p.c.</i>	-25.64*** (7.26)	-20.87*** (6.89)	-42.05	-32.29
<i>Health Spending p.c.</i>	-26.23*** (9.10)	-31.61*** (7.23)	-39.41	-34.77
# Extractive Employees	-1.73 (51.17)	11.52 (54.51)	-28.71	4.36
# Mfg. Employees	-25.17 (15.93)	24.90 (20.72)	-30.63	30.25
# Extractive Firms	-26.79* (15.92)	-7.07 (19.42)	-22.94	9.49
# Mfg. Firms	-21.26*** (7.69)	2.54 (8.73)	-13.50	16.11
Avg. Formal Wage	-12.42*** (4.21)	-4.22 (3.66)	-19.86	-5.03
GDP p.c.	-18.27 (13.66)	-18.08 (12.37)	-39.00	-15.06
Population	0.87 (7.66)	10.49 (7.95)	-38.89	-4.86
n (municipality-years)	1494	15570	1494	15570

To assist readers in interpreting event study coefficients such as those presented in Tables L1 and L2 above, this table presents small-sample bias-corrected semi-elasticities for each of the coefficient estimates reported in previous tables. Event study specifications employed in this paper regress continuous outcome variables transformed by the inverse hyperbolic sine transformation on relative time indicators. Thus, following [Bellemare and Wichman \(2020\)](#) and [Kennedy \(1981\)](#), I calculate semi-elasticities according to:  $\hat{P} = (e^{(\beta - \frac{Var(\beta)}{2})} - 1) \times 100$ . This generates values that may be interpreted as the percentage change in outcome variable  $y_{mt}$  as a result of being treated, relative to never-treated controls. Each column of this table reports semi-elasticities and standard errors for the  $t + 10$  period of event studies for a specific control group-estimator pair. As in Tables L1 and L2, Column 1 reports results using a two-way fixed effects (TWFE) OLS estimator with the wells control group. Column 2 reports results using the TWFE estimator and control matched on baseline characteristics. Column 3 and 4 use Callaway and Sant'Anna's (CS) (2020) did package for staggered event studies in R with wells and pre-matched control samples, respectively. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table B6: Interpreting Treatment Effects: Semi-Elasticities Across Samples and Estimators (10 Yrs After Event), **Satisfied** Municipalities

	TWFE Wells	TWFE Pre-Matching	CS Wells	CS Pre-Matching
<i>Total Revenue (Millions)</i>	74.53** (34.21)	107.96*** (39.71)	89.59 <i>(In Progress)</i>	111.36
<i>Revenue p.c.</i>	75.12** (34.60)	95.43** (37.66)	84.06	106.03
<i>Tax Revenue p.c.</i>	-30.32 (20.58)	-5.31 (24.45)	-11.39	6.54
<i>Oil Revenue p.c.</i>	5441.63 (3755.01)	6205.26 (4330.57)	6679.58	5057.57
<i>Transfer Revenue p.c.</i>	1.26 (5.12)	5.40 (5.15)	1.95	1.24
<i>Total Spending (Millions)</i>	20.35 (13.88)	45.50*** (16.05)	25.09	47.59
<i>Spending p.c.</i>	20.79 (14.07)	37.82** (15.18)	21.93	43.86
<i>Investment p.c.</i>	59.35 (113.07)	75.04 (125.85)	180.02	158.75
<i>Personnel Spending p.c.</i>	14.32 (12.83)	30.86** (13.64)	22.15	53.77
<i>Education Spending p.c.</i>	28.02 (26.10)	36.55 (25.93)	33.62	48.37
<i>Health Spending p.c.</i>	25.42 (29.05)	23.31 (23.63)	38.15	28.69
# Extractive Employees	60.08 (46.73)	112.07** (49.57)	68.81	126.21
# Mfg. Employees	-27.45* (15.63)	17.70 (19.43)	-30.60	11.25
# Extractive Firms	18.90 (31.23)	75.28* (42.32)	23.16	110.61
# Mfg. Firms	-25.03*** (8.05)	-7.53 (8.92)	-21.45	-1.79
Avg. Formal Wage	-11.06** (4.66)	-1.80 (4.38)	-10.12	10.10
GDP p.c.	253.10** (110.29)	290.35** (116.96)	275.49	330.81
Population	-3.11 (4.49)	3.94 (4.23)	272.29	-1.46
<i>n (municipality-years)</i>	1278	9012	1278	9012

To assist readers in interpreting event study coefficients such as those presented in Tables L1 and L2 above, this table presents small-sample bias-corrected semi-elasticities for each of the coefficient estimates reported in previous tables. Event study specifications employed in this paper regress continuous outcome variables transformed by the inverse hyperbolic sine transformation on relative time indicators. Thus, following [Bellemare and Wichman \(2020\)](#) and [Kennedy \(1981\)](#), I calculate

semi-elasticities according to:  $\hat{P} = (e^{(\beta - \frac{\text{Var}(\beta)}{2})} - 1) \times 100$ . This generates values that may be interpreted as the percentage change in outcome variable  $y_{mt}$  as a result of being treated, relative to never-treated controls. Each column of this table reports semi-elasticities and standard errors for the  $t + 10$  period of event studies for a specific control group-estimator pair. As in Tables L1 and L2, Column 1 reports results using a two-way fixed effects (TWFE) OLS estimator with the wells control group. Column 2 reports results using the TWFE estimator and control matched on baseline characteristics. Column 3 and 4 use Callaway and Sant'Anna's (CS) (2020) did package for staggered event studies in R with wells and pre-matched control samples, respectively. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table B7: Effects of Discovery Announcement on Winner Characteristics and Patronage

Winner Characteristics	TWFE Wells	TWFE 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
<i>Election Period FEs</i>	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

## Appendix C Robustness Checks and Extensions

### 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 C1: Event Study with Multiple Events: Public Finances

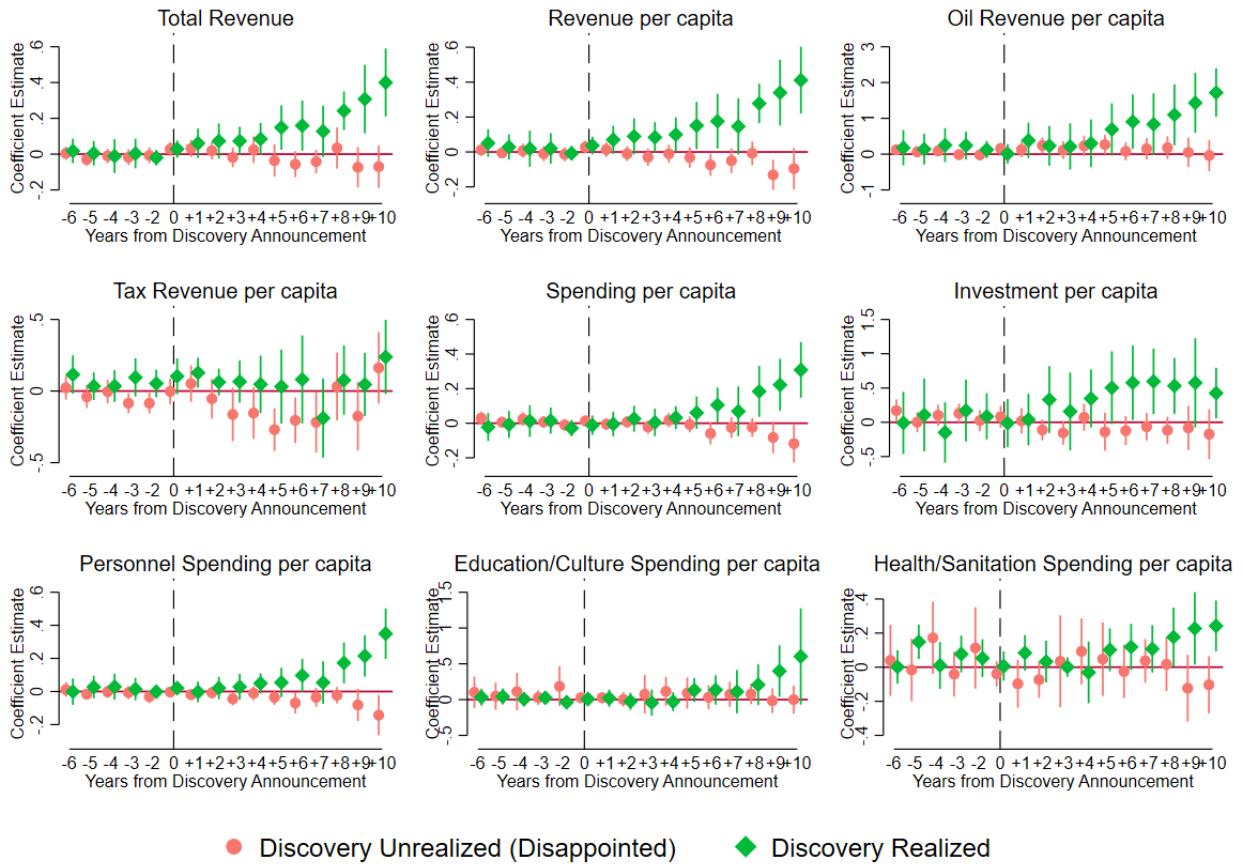


Figure C2: Event Study with Multiple Events: Other Outcomes

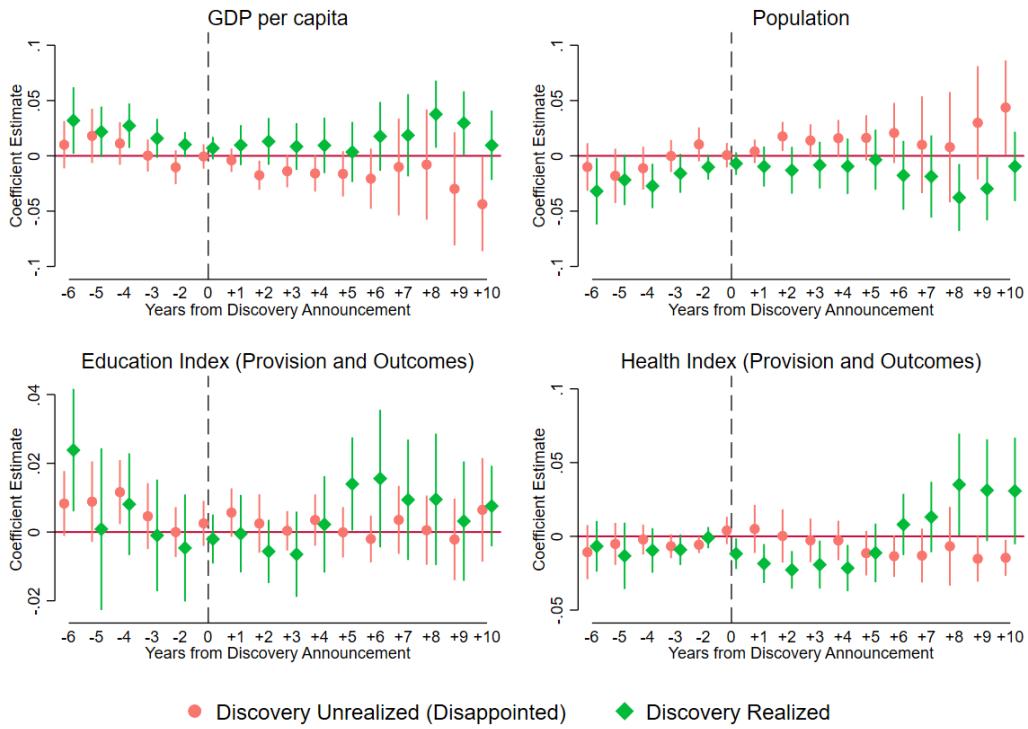


Figure C3: Sample Means: Treated Municipalities and Never-Treated Controls (Municipalities with Post-2000 Exploratory Wells but No Discoveries (n=53))

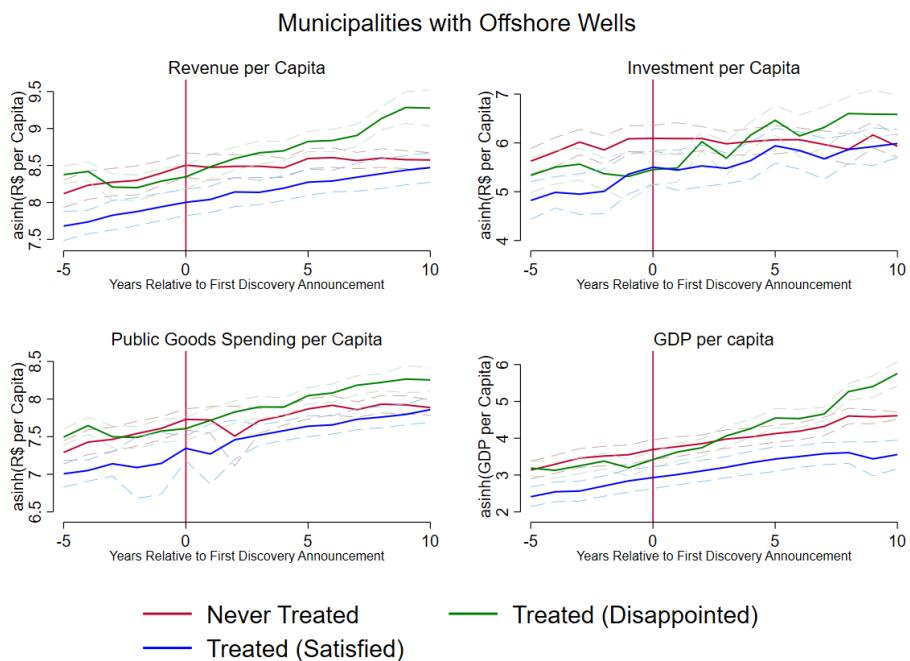


Figure C4: Sample Means: Treated Municipalities and Never-Treated Controls (Coarsened Exact Matching, Separately for Disappointed (n=836) and Satisfied (n=500))

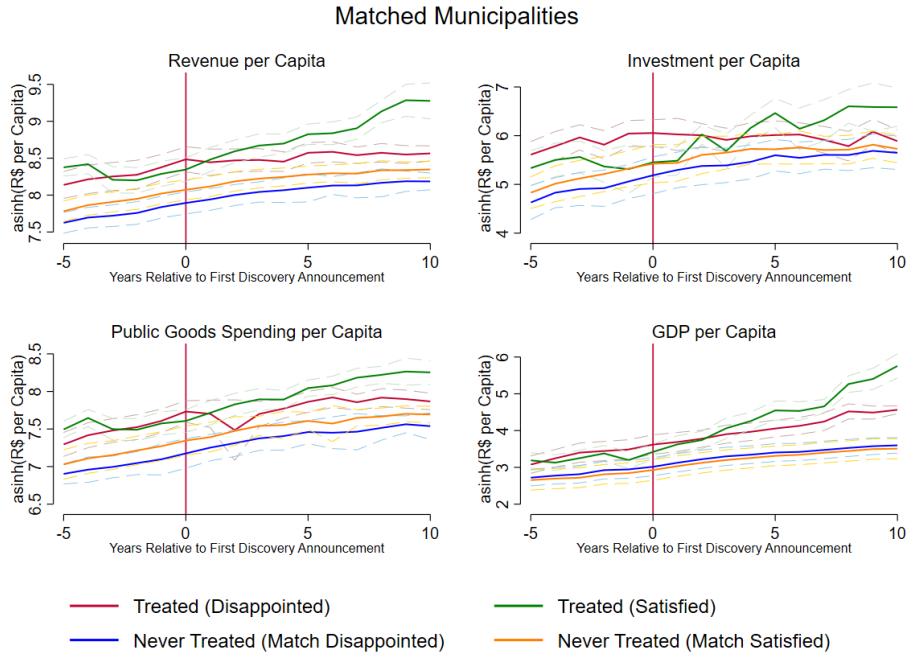


Figure C5: Sample Means: Treated Municipalities and Never-Treated Controls (Municipalities in Coastal States, n=3,902)

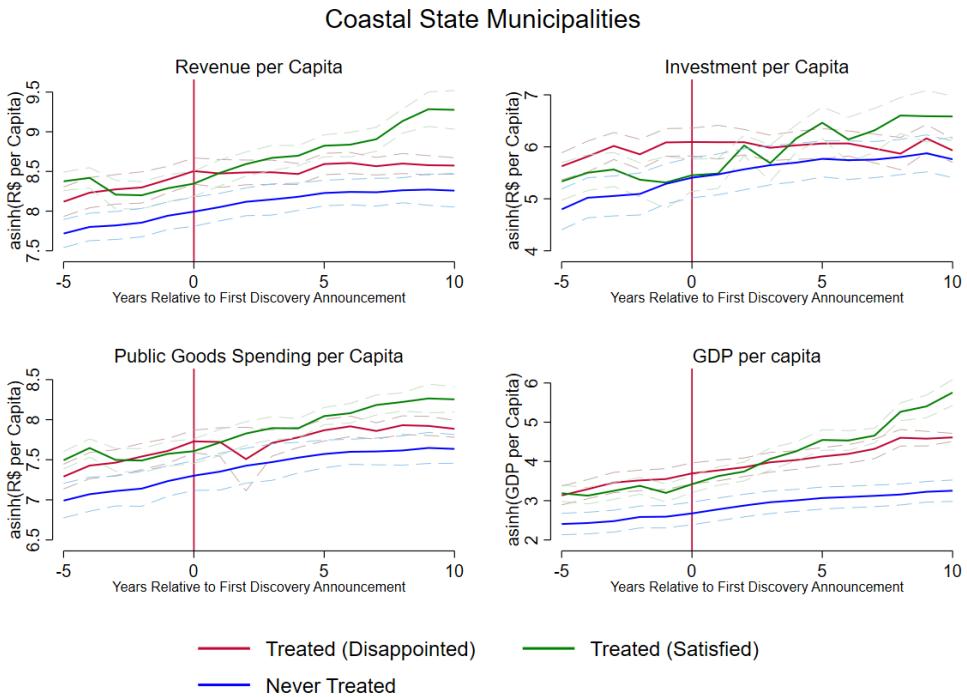
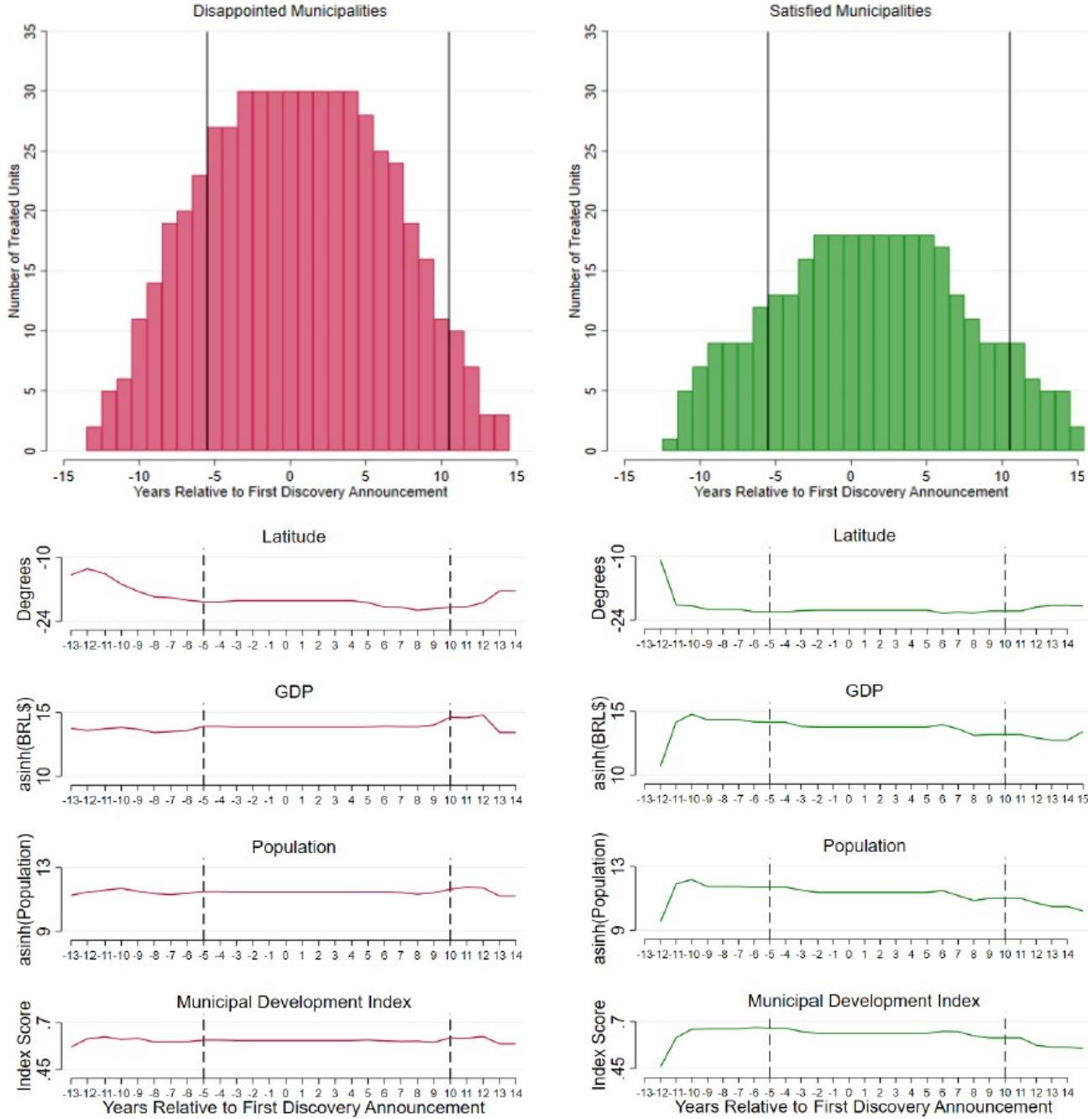


Figure C6: Treated Unit Balance Across Relative Time Indicators

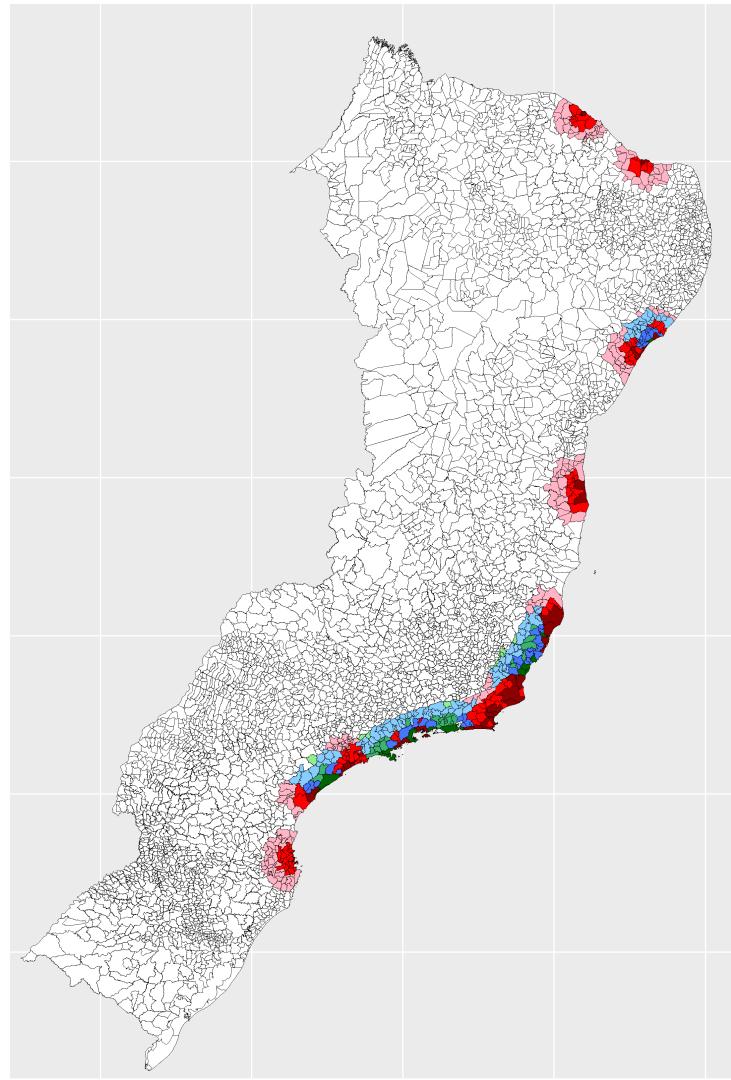


Note: Bar graphs depict number of treated units in each relative time period, where  $t=0$  represents the year of the first major discovery announcement for a municipality. Vertical black lines indicate the extent of periods included in the analysis. Given the limited time-frame in sample (2000-2017), 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.

## Spatial Spillovers from Discovery Announcements

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 C7: 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 C8: Public Finance Outcomes in Near (0-50km) vs Far (50-100km) Municipalities

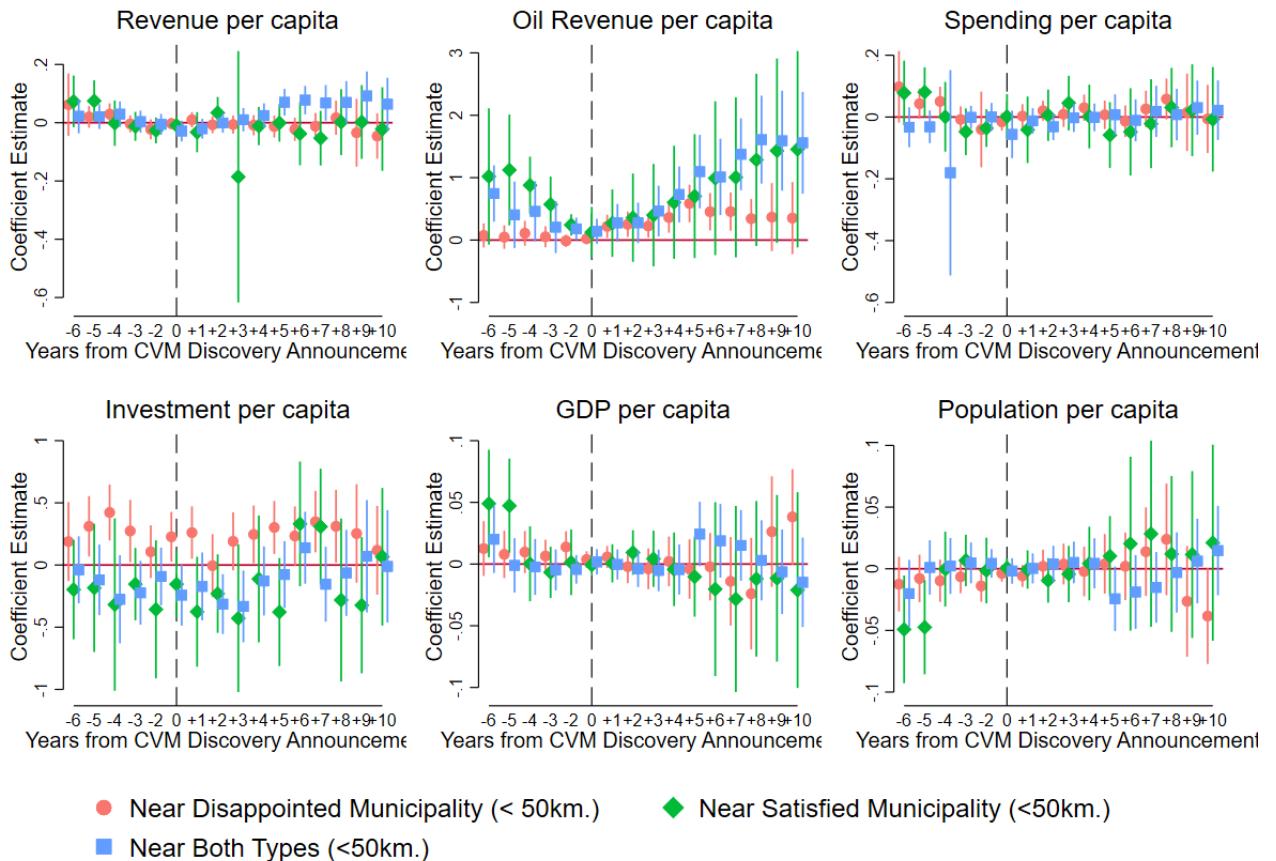


Figure C9: Sectoral Employment in Near (0-50km) vs Far (50-100km) Municipalities

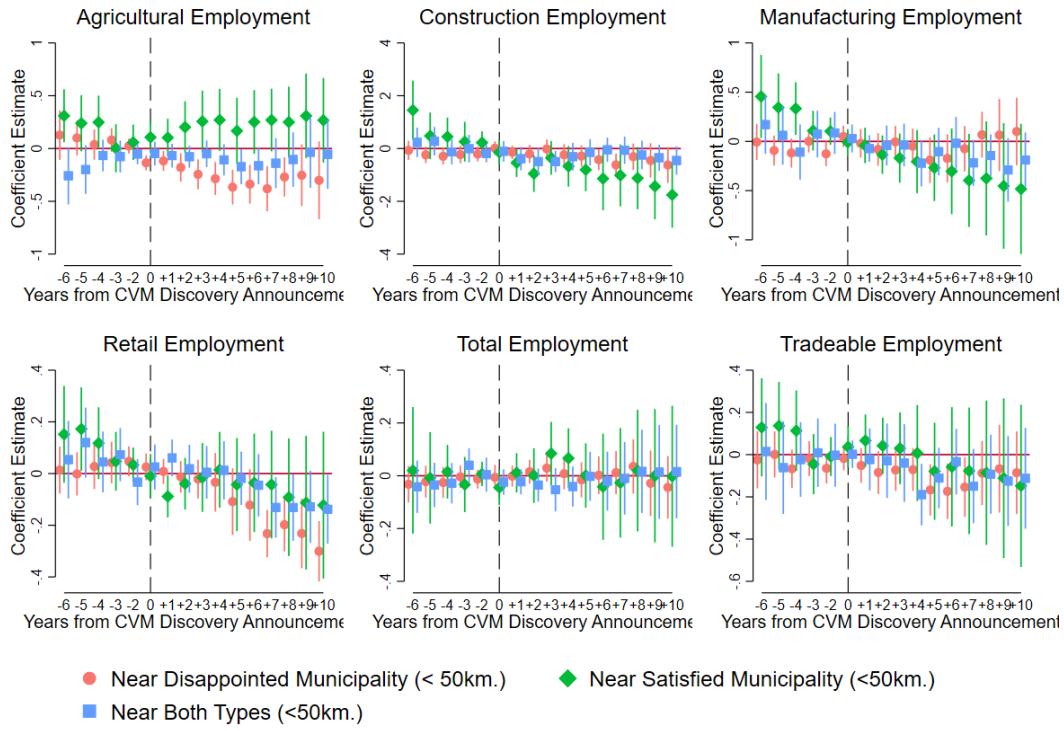
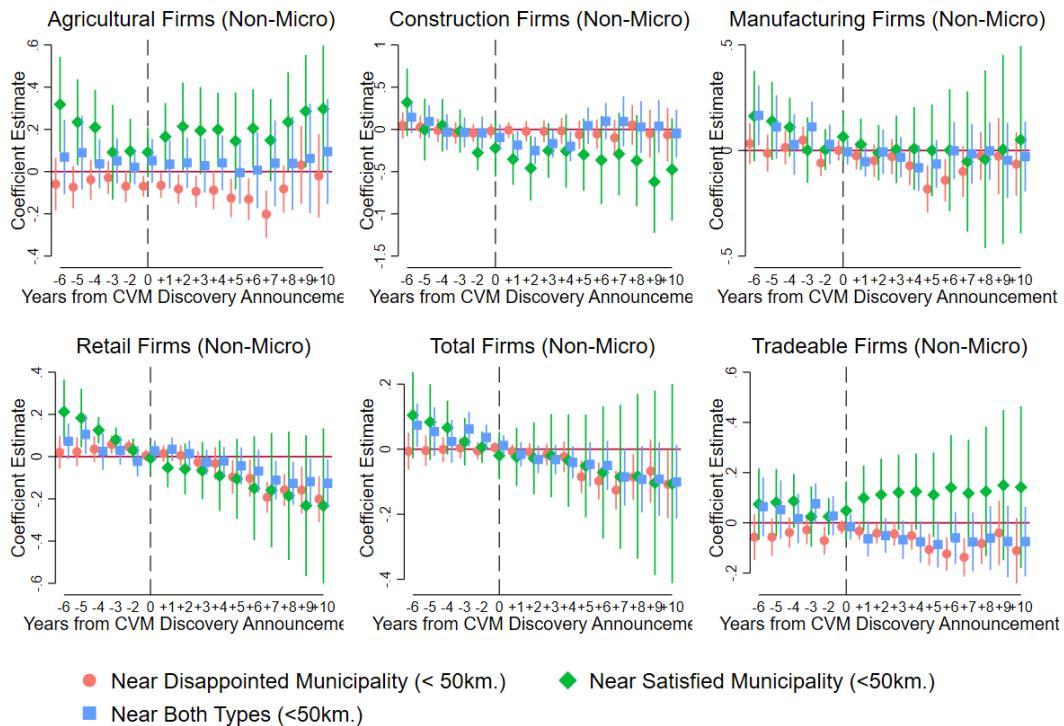


Figure C10: Firm Entry in Near (0-50km) vs Far (50-100km) Municipalities



## Callaway and Sant'Anna (2020) Event Study Estimates

Figure C11: CS Estimator: Total Revenue and Oil Revenue per capita

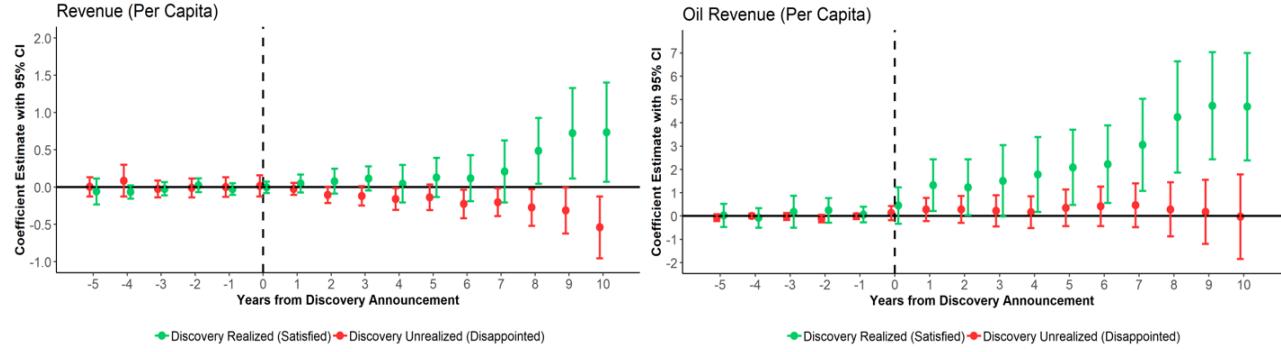


Figure C12: CS Estimator: Spending and Investment per capita

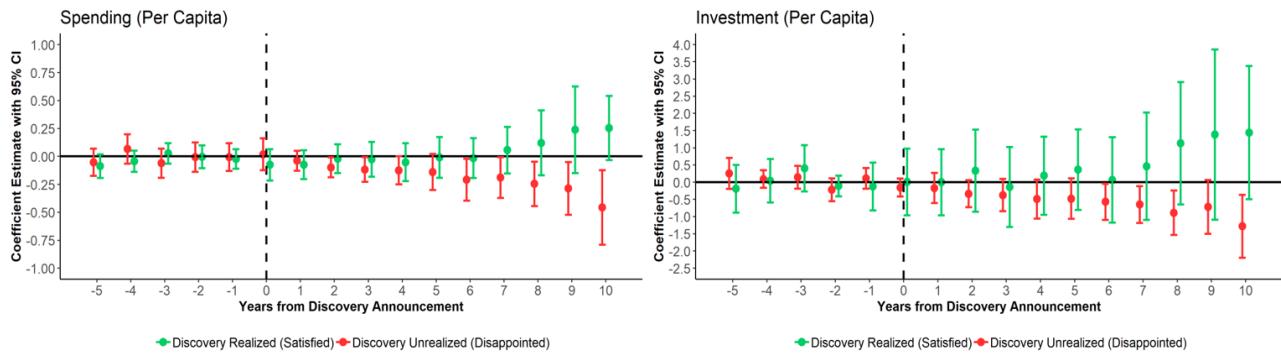


Figure C13: CS Estimator: Education and Health Spending per capita

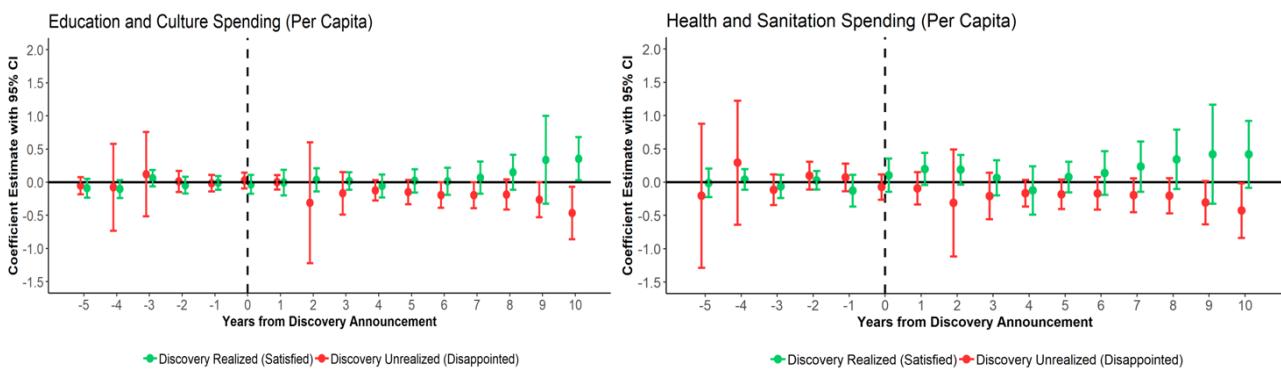


Figure C14: CS Estimator: Public Goods Provision and Quality

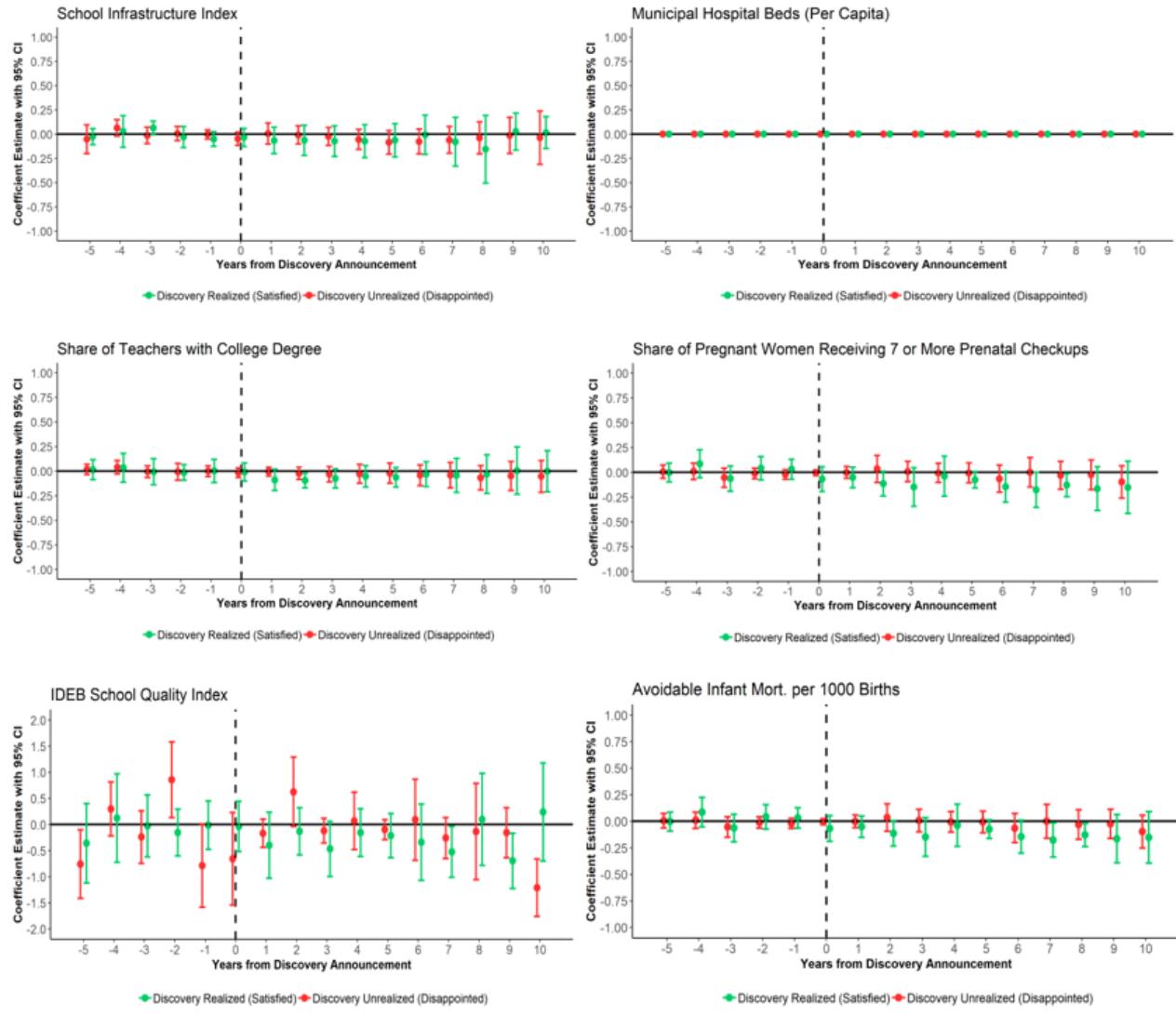
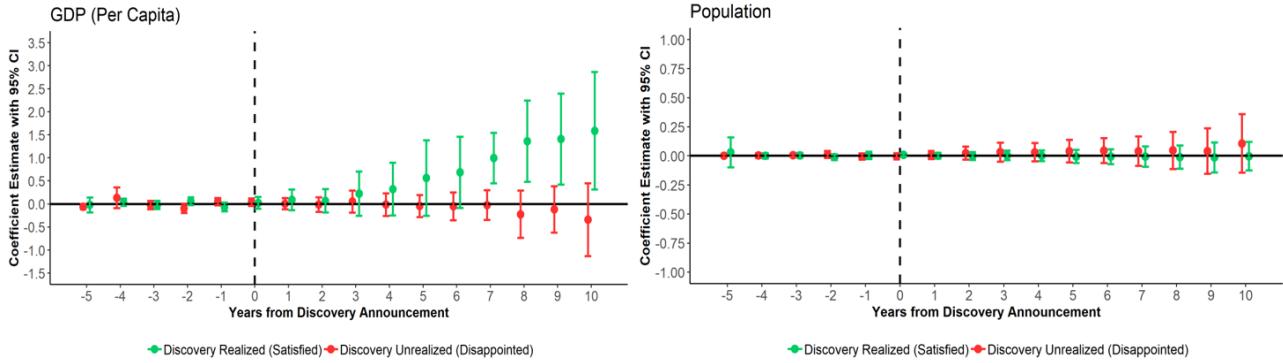


Figure C15: CS Estimator: GDP per capita and Population



## Robustness to Alternative Forecasting and Matching Parameters

Figure C16: Robustness: Revenues

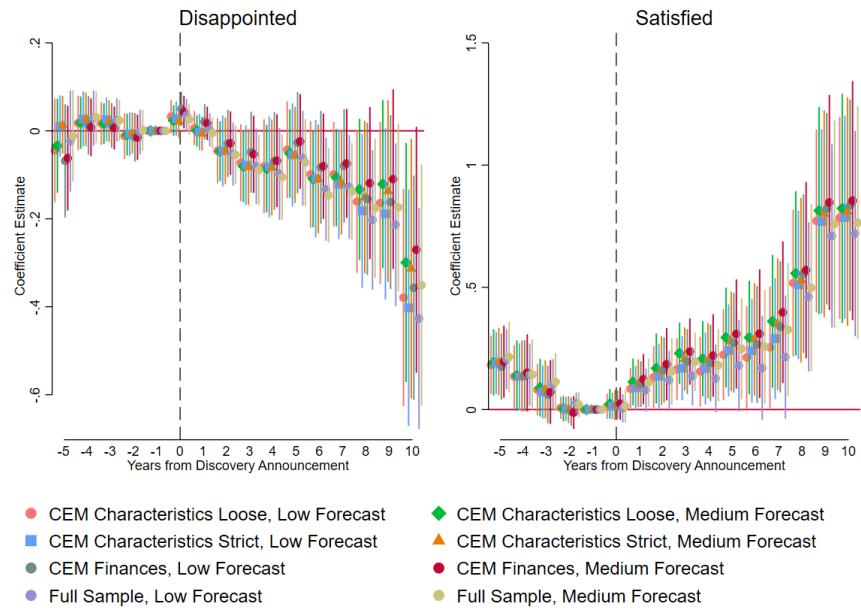


Figure C17: Robustness: Oil Revenues

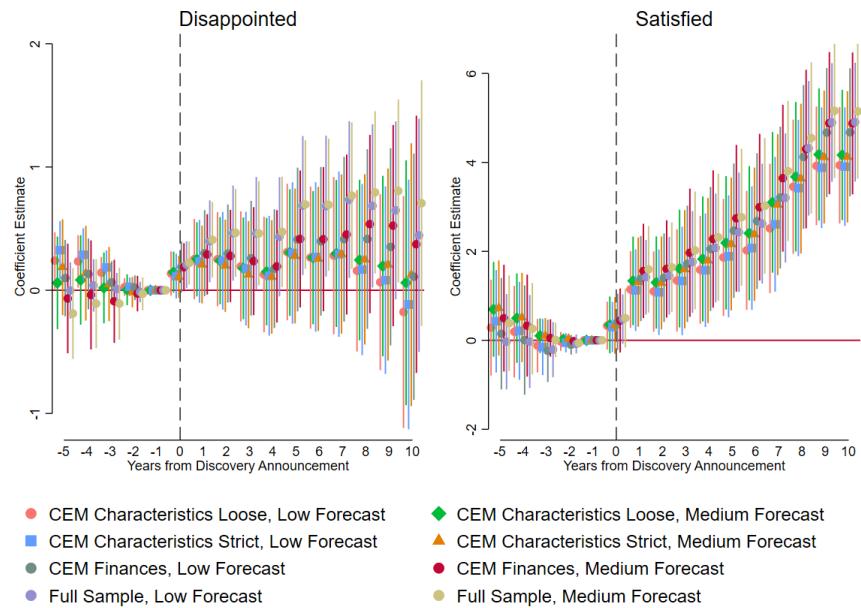


Figure C18: Robustness: Non-Oil Transfer Revenues

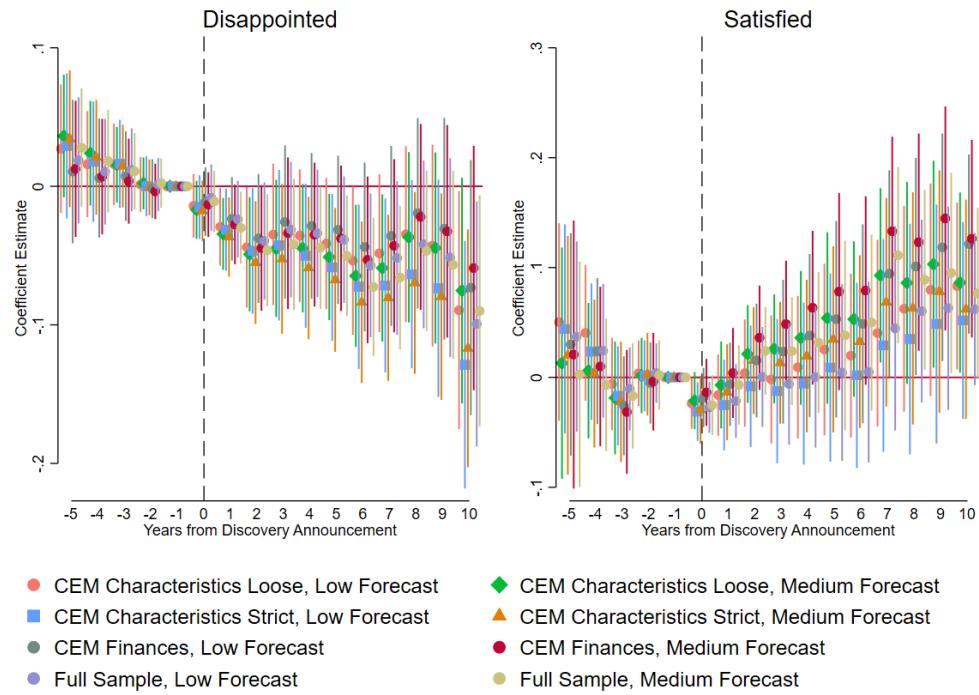


Figure C19: Robustness: Investment

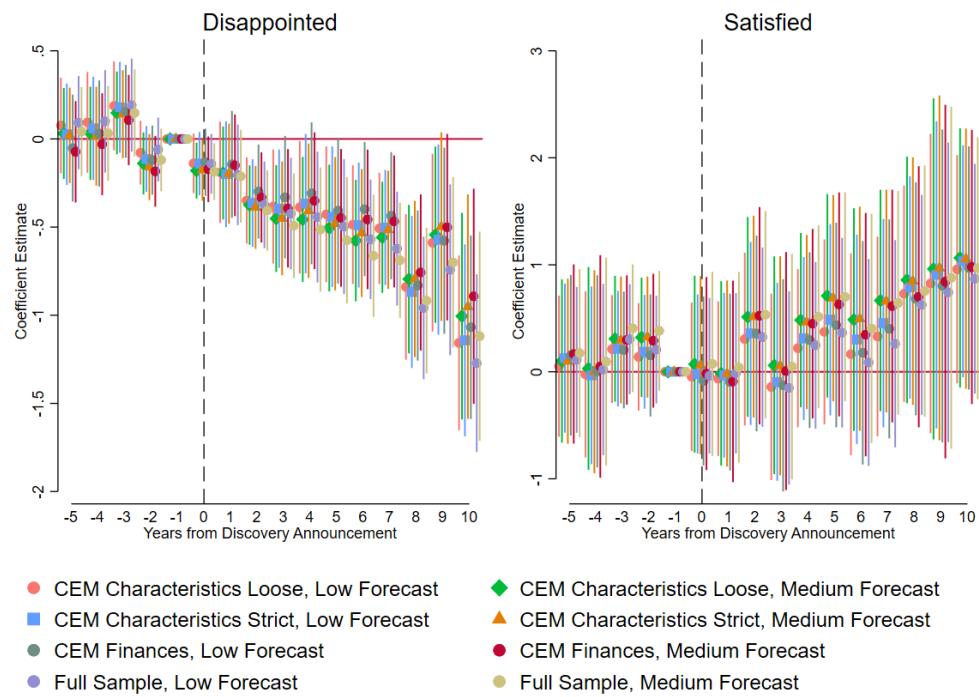


Figure C20: Robustness: Education Spending

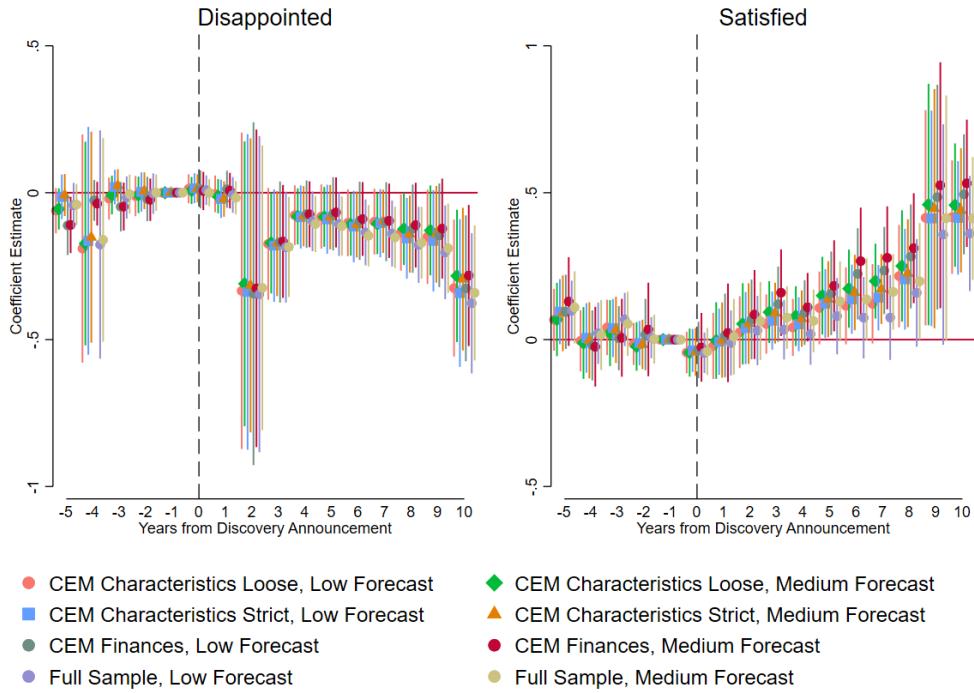


Figure C21: Robustness: Health Spending

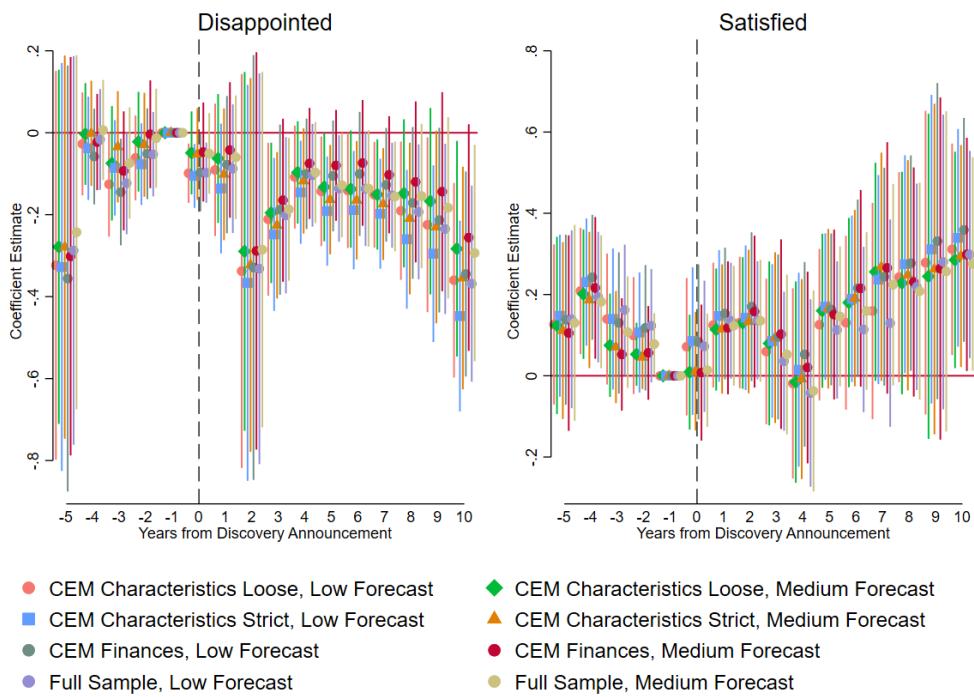


Figure C22: Robustness: Population

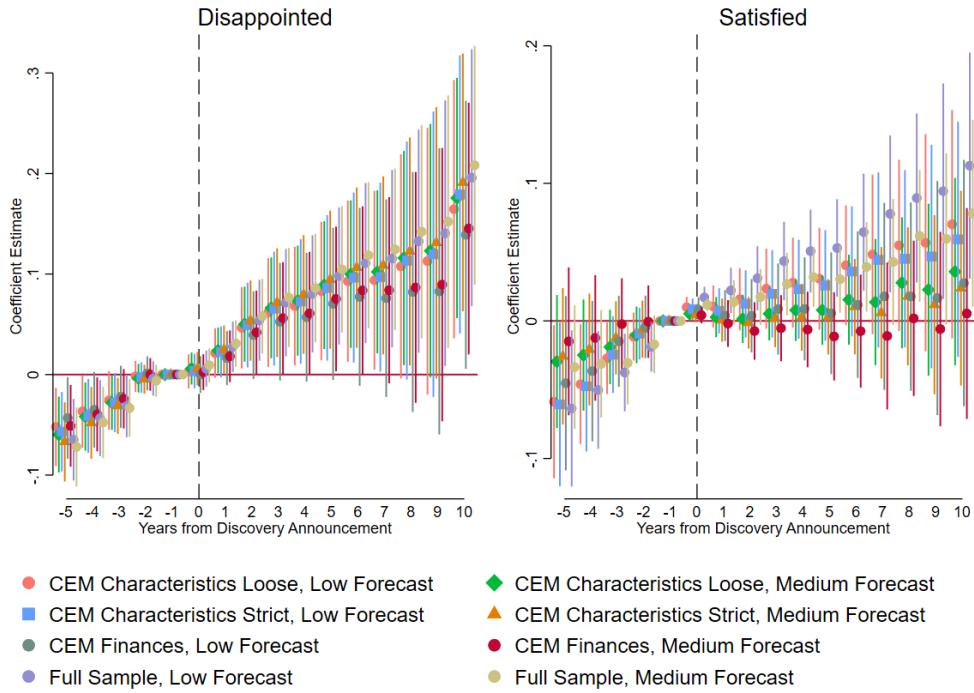
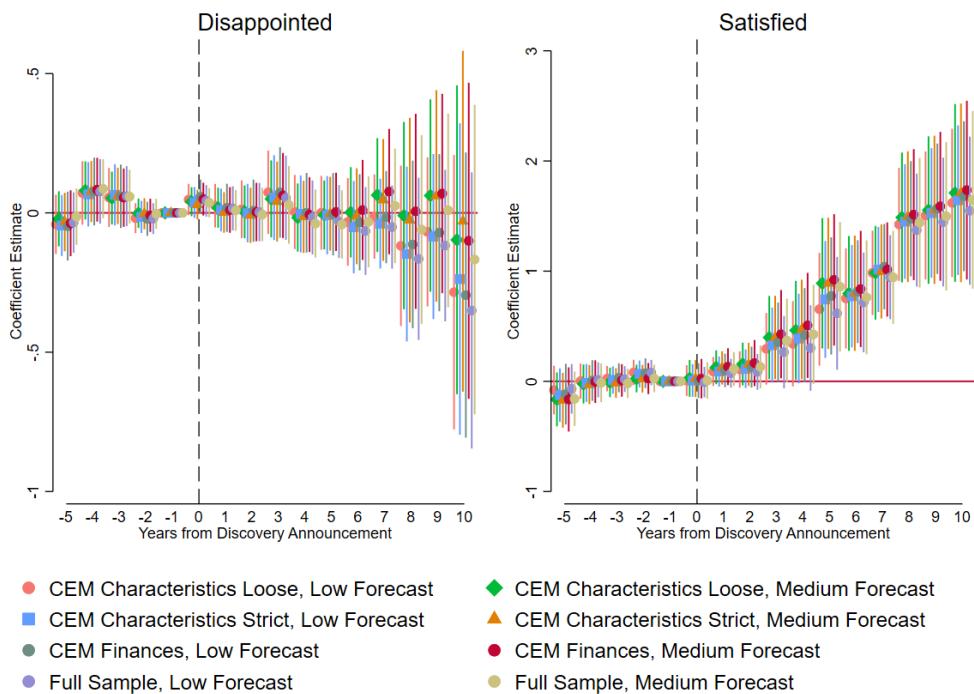


Figure C23: Robustness: GDP

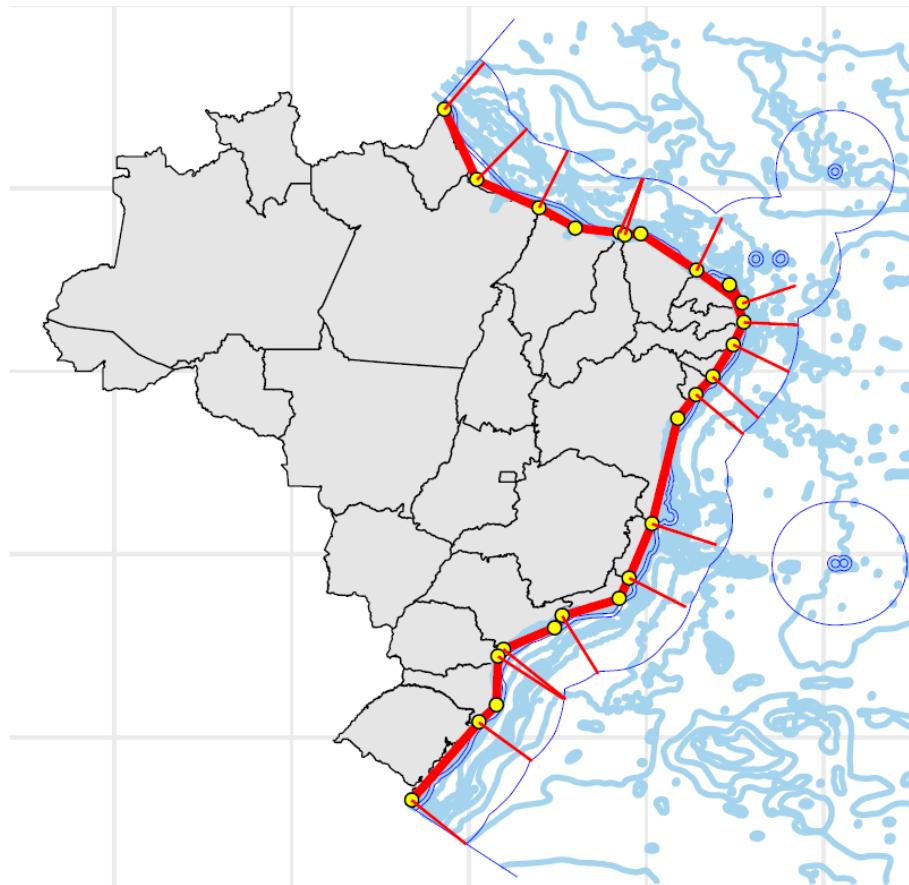


## Appendix D Explanatory Notes

### Reconstructing Geodesic Projection Maps

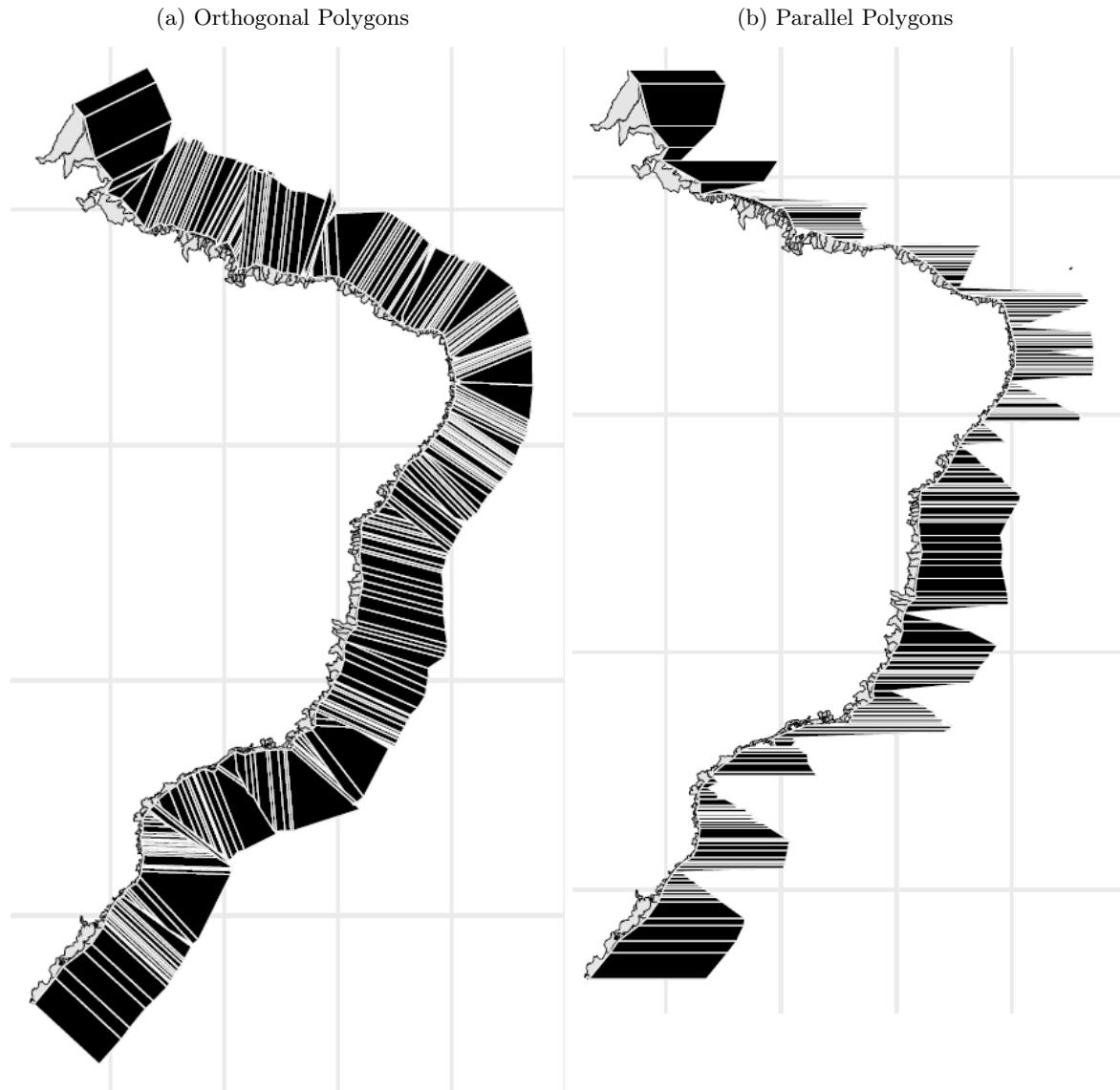
To reconstruct the geodesic projections used by IBGE and ANP to determine municipal offshore oil royalty distribution, I draw on documents from IBGE that define state boundary points and projections rules (IBGE, 2009). I begin by plotting state boundary points and state projections out to Brazil's maritime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

## 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:

$$W_{my} = \sum_f \left[ Alignment_{mfy} * (0.05)(P_{fy}^{oil} * V_{fy}^{oil} + P_{fy}^{gas} * V_{fy}^{gas}) * (0.3) \right] \quad (10)$$

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:

$$Z_{my} = \sum_f \left[ Alignment_{mfy} * (Tax_{fy} - 0.05)(P_{fy}^{oil} * V_{fy}^{oil} + P_{fy}^{gas} * V_{fy}^{gas}) * (0.225) \right] \quad (11)$$

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:

$$R_{my} = \mathbb{1}(neighbor_{my}) * (W_{my} * (f(population_{my}) + g(architecture_{my})) + \mathbb{1}(producer_m) * Z_{my}) \quad (12)$$

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  $Z_{my}$ .

## Data Sources and Processing *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.

### ***Firm Entry, Employment, and Wages***

While the FINBRA data includes spending on public employees, I extract much more detailed 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, new hires, layoffs, average wage, and average education level. I also calculate number of employees, firms, non-micro firms, and 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