The Presource Curse: Anticipation, Disappointment, and Governance after Oil Discoveries*

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Abstract

Major resource discoveries may cause governments and politicians to alter their behavior based on expectations of future revenues. Yet discoveries are notoriously noisy signals. Resource windfalls often arrive later than expected, or never arrive at all. Do public spending and political competition increase in anticipation of future windfalls? Are there long-term consequences of disappointed expectations? I test for subnational evidence of this "Presource Curse" following a wave of offshore oil discoveries in Brazil between 2000-2017. I exploit a quasi-experiment created by Brazil's formulaic sharing rules for oil and gas revenues, which allow municipal governments to predict whether they will benefit from exogenous offshore discoveries. Drawing on an original geolocated dataset of 179 major discovery announcements, I use event studies to estimate dynamic effects of discoveries on municipal public finances, public goods provision, political competition, and firm entry and hiring. To explore the effects of disappointment, I build a model of offshore oil production and royalty allocation to forecast each municipality's expected revenue stream after discovery announcements. I find that disappointment was widespread: only 18 of 48 municipalities affected by discovery announcements between 2000-2017 realized even half their forecast revenues by 2017. Further, I find that municipalities do not exhibit rapid anticipatory responses to discovery announcements, but ten years after a discovery, municipalities where production met expectations enjoy significant increases in revenues (+75%) and spending (+21%) relative to never-treated controls. These places do not, however, show improvements in public goods provision. Disappointed municipalities experience reduced per capita investment (-57%) and education and health spending (-26%) ten years on, suggesting these places are worse off than control municipalities that never received a discovery. Local political competition intensifies after discovery announcements.

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

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1 Introduction

Since 2010, sixteen developing countries representing over half the world's population have experienced giant offshore oil or gas discoveries (Zhang et al., 2019).[1] Discoveries constitute large shocks to expected long-term wealth and, for affected governments, they can feel like winning the lottery. Leaders may make irreversible investments in preparation for an anticipated boom, or borrow against future resource revenues. Discoveries increase expected returns to holding political office, and may encourage both political competition and rent-seeking behaviors (Mihalyi and Scurfield, 2020).

Nevertheless, discoveries are notoriously noisy signals. A fall in world 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, delays between discovery and production are likely to grow, increasing the scope for anticipation and uncertainty (Geiger, 2019). Furthermore, growing pressures to leave fossil fuels in the ground to combat climate change may cause discoveries to remain undeveloped in the future (McGlade and Ekins, 2015).

A rich literature on the resource curse has documented the potentially corrosive relationship between resource revenues and governance (e.g. Ross (2013); Brollo et al. (2013); Caselli and Cunningham (2009)). Yet the anticipation, uncertainty, and frequent disappointment introduced by discoveries encapsulate many countries' resource governance experiences to an equal or greater degree than do revenue receipts. These factors may constitute alternative mechanisms underlying the resource curse. Following Cust and Mihalyi (2017), I refer to the discovery-to-production timing problem and associated challenges as the "Presource Curse."

I test for subnational evidence of the Presource Curse following a wave of major offshore oil discoveries in Brazil during the 2000s and 2010s. In particular, I ask whether major discovery announcements cause anticipatory changes in local governance (i.e. public finances and elections) and firm behavior (entry, exit, and hiring) before revenues begin to flow. Additionally, I ask how often municipalities' discovery expectations are disappointed, and whether disappointed expectations have longer-term consequences, such as reduced revenues, investment, public goods provision, or economic activity.

[1] 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 A.

To answer these questions, I exploit a quasi-experiment created by Brazil's formulaic and long-established offshore oil and gas revenue sharing rules, which allow municipal governments to predict whether they will be future beneficiaries of discoveries. These sharing rules introduce subnational variation amongst comparable local governments. Furthermore, offshore discoveries are plausibly exogenous to municipal governments, as they are made by multinational corporations operating hundreds of kilometers off the coast, servicing their installations from distant staging areas, and responding to international prices and technologies.

Brazil's offshore sharing rules determine revenue allocations based in part on geographical alignment between coastal municipalities and offshore fields. 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.[2] I then reconstruct Brazil's geodesic offshore projection maps to tie each discovery back to affected municipalities.[3] Drawing on a rich municipality-level panel dataset spanning 2000-2017, I implement an event study framework to estimate the dynamic effects of exogenous discovery announcements on municipal public finances, real public goods provision, political competition, and private sector outcomes include firm entry and hiring.

To identify causal effects, I compare municipalities affected by major 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 (Cust et al. (2019); (Cavalcanti et al., 2016)). As a robustness check, I construct pre-matched never-treated control groups using coarsened exact matching.[4] To address threats to causal inference in the context of difference-in-differences with staggered treatment timing (e.g., Goodman Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020), I use the doubly robust group-time average treatment effects approach proposed by Callaway and Sant'Anna (2020) to re-estimate event studies for key outcomes. Results are mostly stable across alternative samples and estimators.

[2] I identify contemporaneous news coverage in *O Globo*, Rio de Janeiro's newspaper of record, for nearly every CVM discovery announcement in the dataset, providing evidence that announcements are salient to municipal decision makers.

[3] I provide a repository containing R code and raw data to reconstruct Brazil's geodesic projection maps at: https://github.com/ekatovich/Brazil_GeodesicProjections

[4] In my preferred matching specification, I match on baseline (year 2000) levels of municipal GDP, population, latitude, distance to state capital, and score on the FIRJAN Municipal Development Index. I also explore stricter matching specifications and matching on baseline revenues and spending as a further robustness check.

To explore effects of discovery disappointment, I build a model of offshore oil production and royalty allocation to forecast each municipality's expected revenue stream after discovery announcements. I then compare the gap between expected and realized revenues, and categorize municipalities into groups of "disappointed" and "satisfied" based on their forecast error. I find that 30 of the 48 Brazilian municipalities affected by oil discovery announcements between 2002-2017 ultimately receive less than 40% of what they could have expected based on standard production assumptions and contemporaneous prices and exchange rates. In other words, disappointment was widespread, though not universal.[5]

My findings indicate that Brazilian municipalities did not exhibit rapid anticipatory fiscal responses to discovery announcements, likely due to constraints imposed by a fiscal responsibility law and difficulties issuing debt. In both disappointed and satisfied municipalities, public spending and hiring remain indistinguishable from controls up to five years after the first discovery announcement. This result contrasts with findings in Mihalyi and Scurfield (2020), who report the worsening of fiscal measures such as debt sustainability in 9 out of 12 African countries affected by major oil discoveries. This contrast may highlight the important role of institutions such as Brazil's fiscal responsibility law in tempering fiscal excesses after discoveries. Likewise, it may illustrate emergent properties of discovery dynamics at the subnational level, where policy options are fundamentally different than those available to national governments.

I estimate conditional random assignment tests to document that neither baseline municipality characteristics (from year 2000) nor political alignment between municipal mayors and state and federal leaders predict where discovery announcements occur or what type of outcome is realized. As production begins and municipalities' "type" is realized (disappointed or satisfied), outcomes for the two groups diverge sharply. Ten years after the first major discovery announcement, per capita revenues and spending in satisfied municipalities increase by 75% and 20%, respectively, relative to counterfactual municipalities that had exploratory wells but no major discoveries. Per capita spending on education and health increases by 28% and 26%, respectively. Strikingly, per capita oil revenues in municipalities where discovery expectations are met increase by 5,441% ten years after the first major announcement, highlighting the radical effects discoveries can exert on public finances. Despite these

[5]Note that this measure of disappointment is based on production realizations only up to 2017. Disappointed municipalities may enjoy later, delayed oil booms. Until then, however, "dud" discoveries and delayed discoveries may have similar effects. Throughout the paper, I refer to effects measured ten years after the first major discovery announcement as "longer-run" effects, acknowledging that true long-run effects are yet to be realized.

dramatic changes in revenues and spending in satisfied municipalities, however, measures of real public good provision and outcomes are unchanged or slightly negative relative to controls in the decade following a major announcement. 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.

In disappointed municipalities, oil revenues remain unchanged ten years after the first major discovery announcements, yet per capita revenues decline by 27% relative to controls, largely as a result of falling tax revenues (-37%) and per capita transfers from federal and state governments (-9%). Consequently, per capita spending declines by 24%, investment by 57%, and education and health spending by 26%. Furthermore, measures of fiscal health deteriorate: the budget share going to investment declines significantly, while the share going to personnel increases significantly. Thus, it appears that, along the dimensions measured here, municipalities that experience major discovery announcements only to be disappointed by unrealized production are worse off in the medium term relative to counterfactual municipalities that never receive a discovery announcement.

In some specifications, both satisfied and disappointed municipalities appear to experience in-migration in years following the first major discovery announcement. Nevertheless, only satisfied municipalities enjoy oil revenue gains that enable them to accommodate this growth. Disappointed municipalities must contend with budget shortfalls and ballooning populations, leading to lower per capita spending and public goods provision. These results suggest that in-country migration may be another emergent property of the Presource Curse at the subnational level.

Turning to private sector outcomes, I find variable results across different control group subsamples. Compared to my preferred control group of municipalities that received exploratory wells but no major discoveries, I find that satisfied municipalities experience declining numbers of manufacturing firms (-25% after ten years) and employees (-27% after ten years), coupled with an 11% reduction in average formal wages. Disappointed municipalities suffer decreases in both number of extractive firms (-27% after ten years) and manufacturing firms (-21% after ten years), with average formal wages falling by 12%. While declines in manufacturing activities after a resource boom align with standard predictions of Dutch Disease models, (Corden and Neary, 1982), formal wages are evidently not acting as a crowding-out mechanism. It is important to note that these comparisons are being made amongst a select group of municipalities that all experience oil activities. Compared to the pre-matched control group, I find that, for satisfied municipalities, the number of extractive firms (including oil and gas) remains unchanged for up to 8 years after the first discovery announcement, and then increases by 49% as production begins to come online. Likewise, extractive employment in these municipalities increases by 125% ten years on relative to matched controls. Extractive employment and firm entry remain unchanged in disappointed municipalities. Since firms are mobile across municipalities, I explore spatial spillovers and neighborhood effects of discovery announcements in an Appendix.

Finally, I examine effects of discovery announcements on political competition in municipal elections between 2000-2016. Since mayors and municipal council members control royalty revenues, and potentially extract personal and political rents, the expected value of holding future office may increase when a discovery is announced (Baragwanath Vogel, 2020). More candidates may enter races, and individuals or companies may increase donations to buy influence with politicians whom they expect will reciprocate in the form of contracts, jobs, or favors. I estimate a difference-in-differences specification where treatment is defined as the occurrence of a discovery announcement in the four years preceding an election. I examine a range of competition-related outcomes, including number of candidates and competitive candidates, value and number of campaign donations, and candidate characteristics.

My findings suggest that discoveries increase the number of candidates running for local council (though not for mayor), and decrease the schooling level of candidates. Discoveries may also increase the value and number of donations made in local elections (significant at the 10% level). Furthermore, are politicians rewarded or punished for the exogenous realization of production and revenues in their municipality? Using my royalty forecasting model, I compute a time-varying measure of discovery disappointment for each election period, and estimate the effects of this measure on mayor and council reelection rates. I find that council candidates are significantly less likely to be reelected when royalties are substantially below expectations at the time of the election. Mayors are marginally less likely to be elected (significant at the 10% level). These results suggest that voters, unable to observe politicians' true quality and honesty, may opt to punish politicians for exogenous negative outcomes.

This paper contributes 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). In particular, I dialogue with the rich literature focused on the economic and political effects of Brazil's royalty transfers. Postali (2015) finds that royalty recipient municipalities in Brazil exert less tax collection effort than comparable municipalities that do not have access to royalties, creating risk of dependency on oil revenues and vulnerability to oil shocks. Considering the political effects of oil royalties, Monteiro and Ferraz (2010) estimate that first-time royalty windfalls increase mayoral re-election rates, but that voters eventually learn local politicians have nothing to do with increased revenues and hold them more accountable for public goods provision in the long-term. Baragwanath Vogel (2020) finds that royalties significantly increase corruption and entry of more corrupt candidates, supporting the hypothesis that discovery announcements may encourage individuals interested in rent-seeking to run for office. Lastly, Cavalcanti et al. (2016) compare economic outcomes in Brazilian municipalities where successful versus unsuccessful wells were drilled between 1940 and 2000. They find that onshore discoveries had positive economic effects over the long term, but that there were no detectable effects from offshore discoveries. My study complements this work by examining effects of major offshore discoveries announced publicly by oil companies since 2000, which were likely more salient and impactful than pre-2000 discoveries.

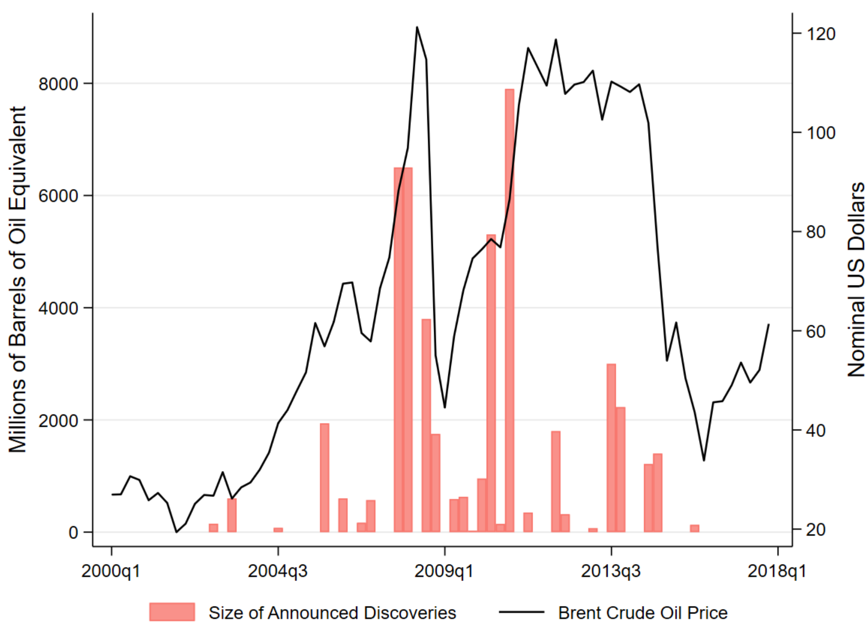
My study contributes directly to the growing literature on the Presource Curse, which has documented long delays, disappointed expectations, fiscal problems, and corruption after major offshore oil and gas discoveries in African countries (Mihalyi and Scurfield (2020); Cust and Mihalyi (2017); Wright et al. (2016)). Vicente (2010) uses a household survey to measure perceived corruption in São Tomé and Príncipe, where large oil discoveries were announced, compared to neighboring Cape Verde, where no discoveries occurred. He finds that perceived corruption was 31-40% higher in São Tomé as a result of the discoveries, and attributes the increase to competition over the expected windfall. Moving to the country level, Vezina (2020) uses event studies to measure a significant increase in countries' arms purchases after oil discoveries, especially when oil prices are high. Closely related to my approach, Arezki et al. (2017) study the effects of news announcements of giant discoveries on macroeconomic variables in a cross-country framework. They compute the net present value of announced discovery volumes as a percentage of GDP and find that national savings rates fall and investment rises after discoveries, but GDP does not increase until five years later.

I contribute causal subnational evidence of the Presource Curse in a novel context. Variation in exogenous discoveries and discovery realizations across space and time allows me to identify the short- and longer-term effects of discoveries on a rich set of outcomes, including public finances, elections, and firm entry and hiring. These findings may have policy implications for the design of resource revenue sharing rules and post-discovery management and planning.

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 (CVM, 2020). Figure 1 illustrates annual announced discovery volumes and world oil prices over this period.

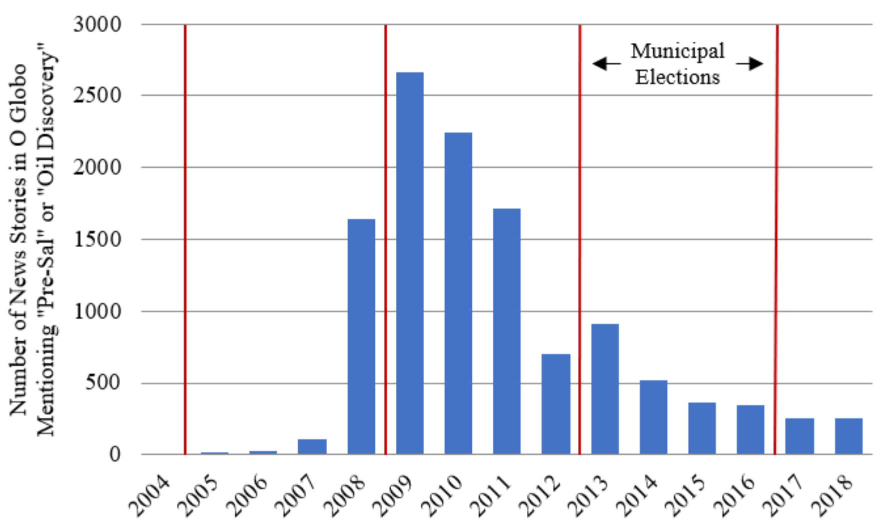
Figure 1: World Oil Prices and Major Offshore Discoveries in Brazil



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.[6]

The Pre-Salt discoveries became a major topic in news media, with over 2600 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.

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



Discovery Announcements

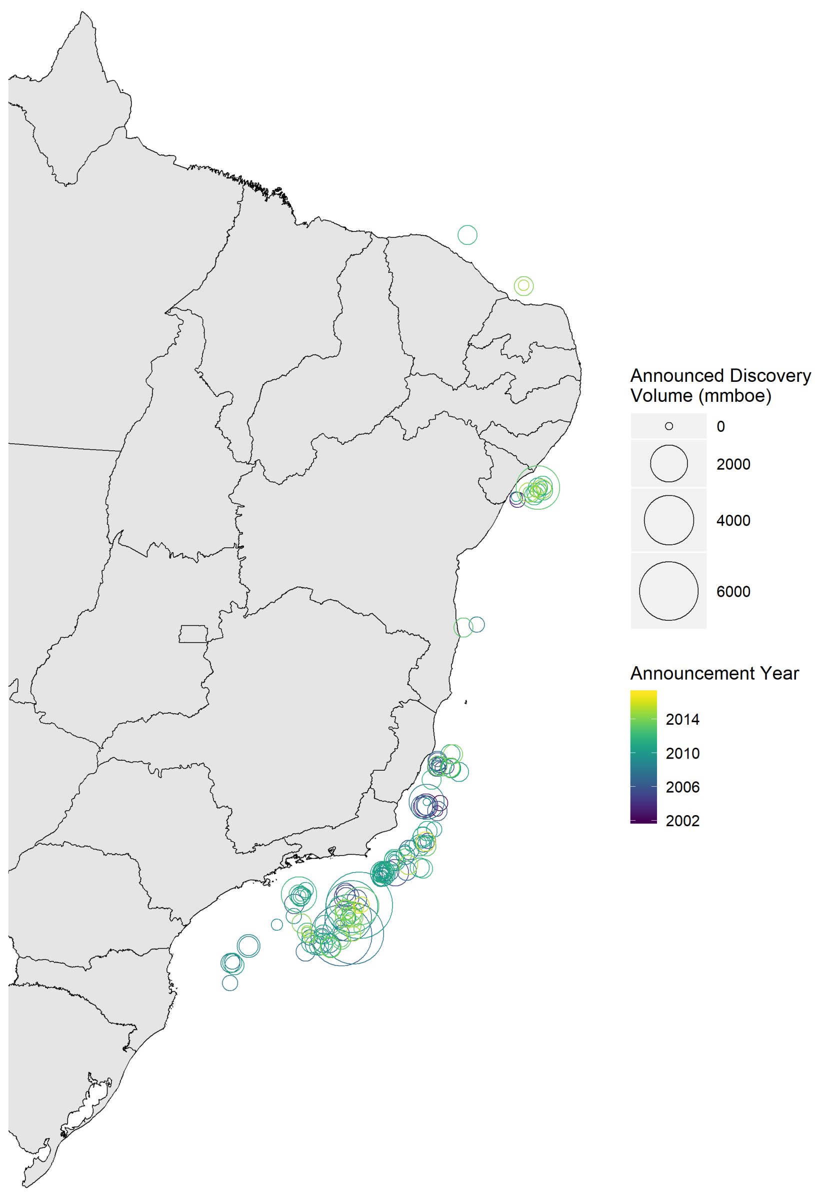
Oil companies during this period announced major discoveries in "communications to the market" filed with the *Comissão de Valores Mobiliários*, Brazil's Securities and Exchange Commission.[7] I compile all communications pertaining to preliminary exploratory drilling results, new oil discoveries, confirmatory discoveries, and declarations of commerciality for 26 major and minor oil companies operating in Brazil

[6] 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).

[7] 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 C, 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.

between 2000 and 2017 (see Appendix B 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.

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).[8]

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 forecasted years to production 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.

[8] In Appendix D, 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

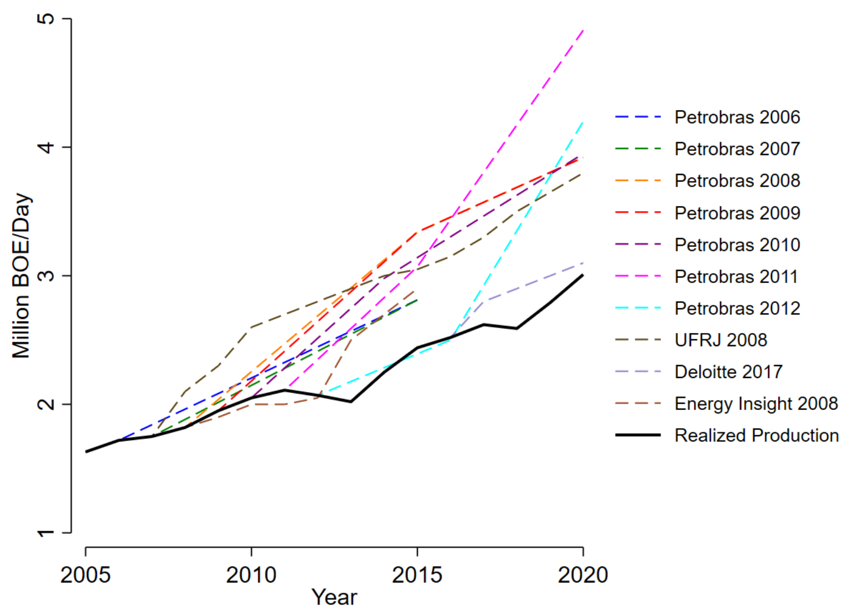
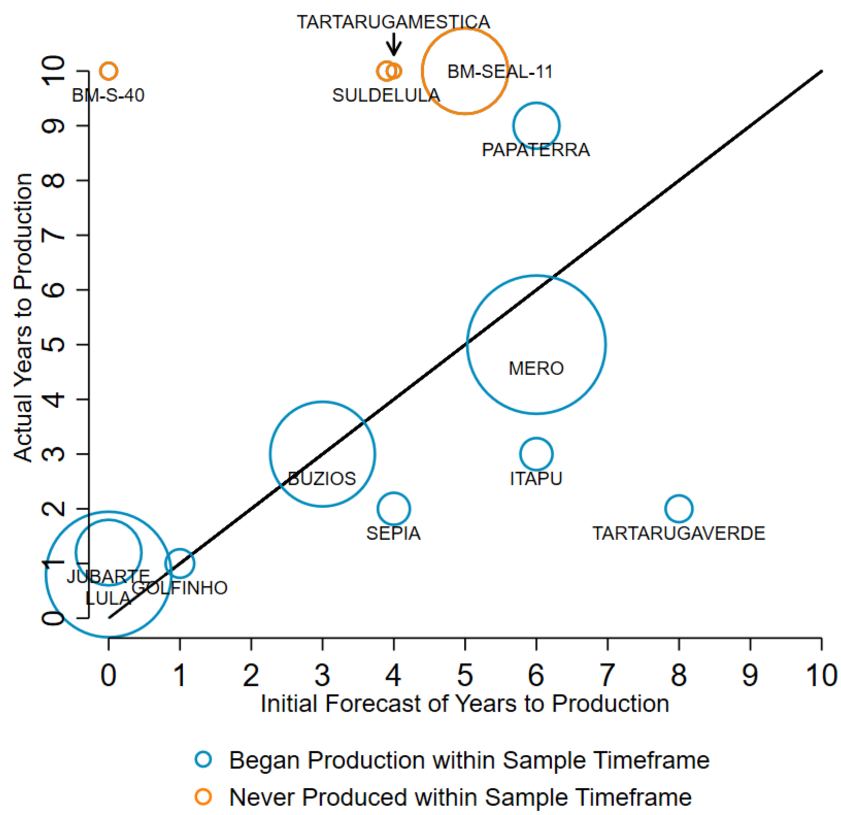


Figure 5: Brazil: Field-Level Time-to-Production Forecasts

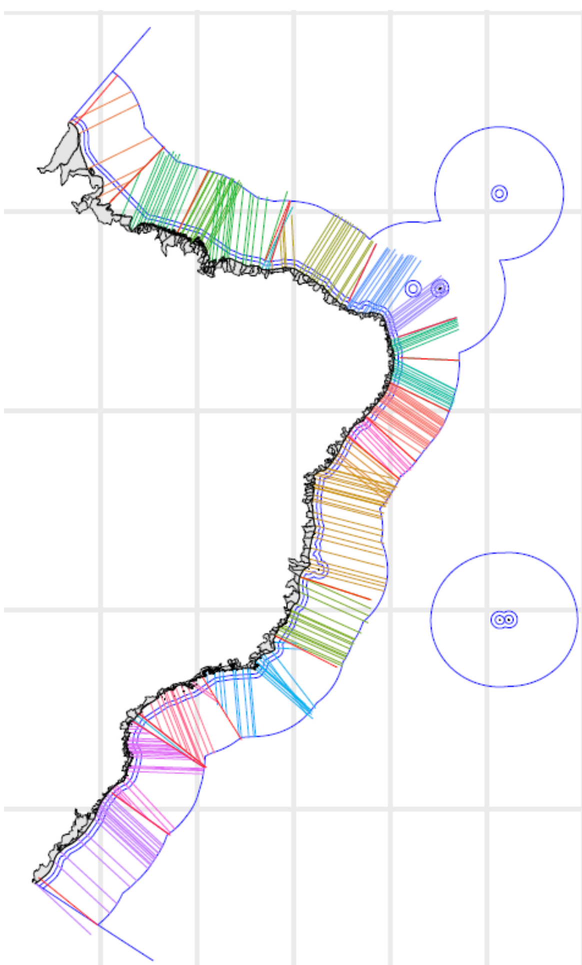


Royalty Distribution

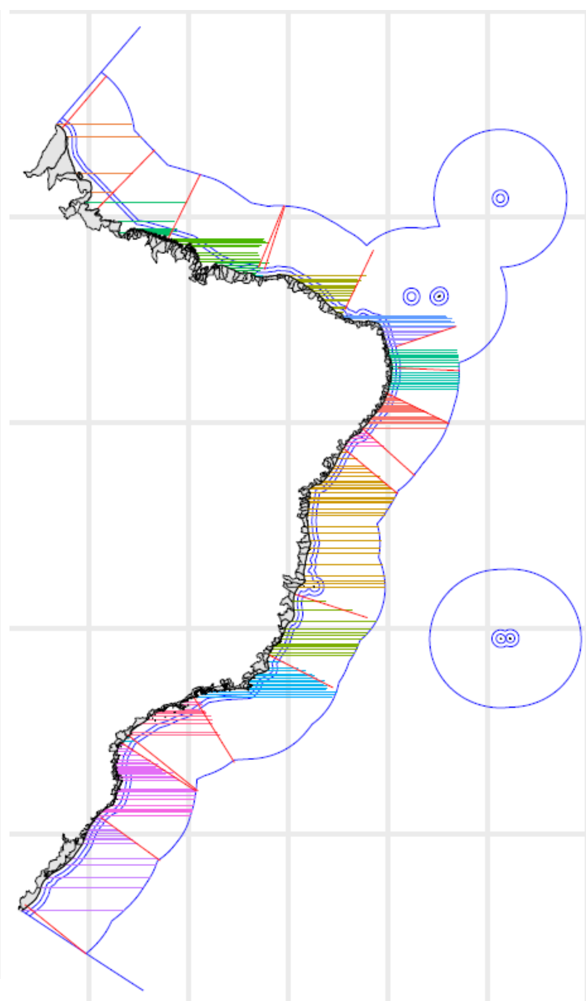
In 1985-1986, Laws 7.453/85 and 7.525/86 established royalty requirements for Brazilian maritime oil production and created a system of orthogonal and parallel geodesic projections of coastal municipal boundaries to determine royalty distribution to coastal municipalities (Piquet and Serra, 2007). Distribution is determined by a formula that takes into account geographical alignment with offshore oil and gas fields, population, the presence of oil and gas infrastructure within municipal boundaries, the specific tax rate applied to each field, and the current volume and price 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).

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

(a) Orthogonal Projections



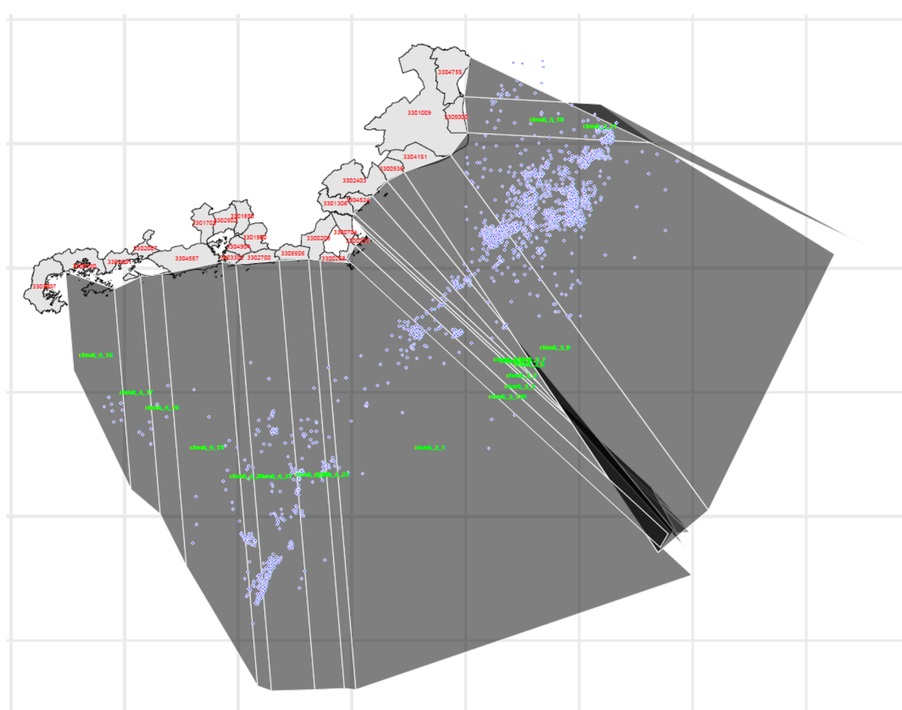
(b) Parallel Projections



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 E. 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.[9] 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



[9]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 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, 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. 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).

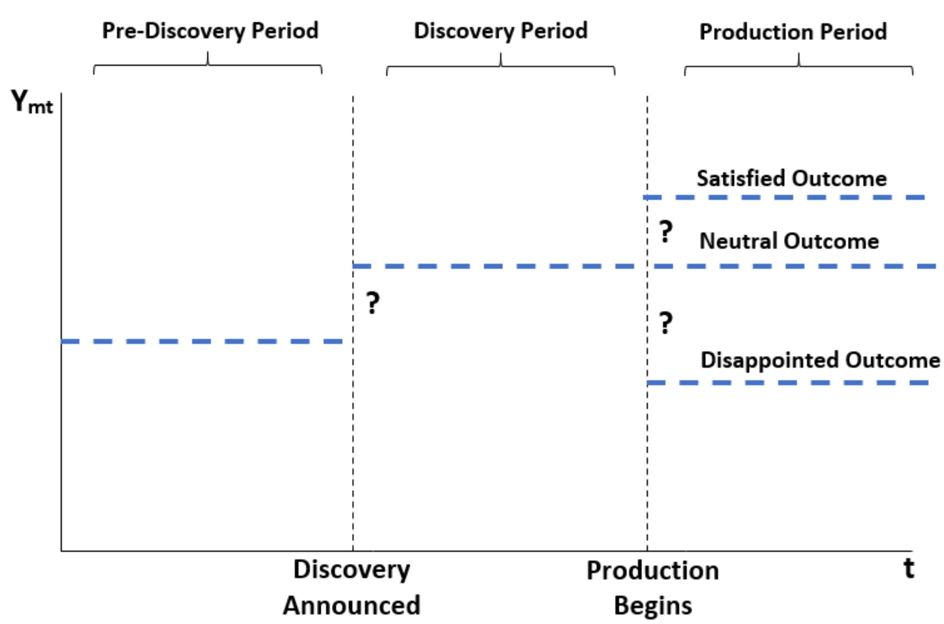
3 Analytical Framework

How might local governments and politicians react to major discovery announcements and the expectation of higher future revenues they create? My objective in this section is to clarify this process through two analytical lenses: the political agent framework (e.g. Brollo et al. (2013); Caselli and Cunningham (2009); Robinson et al. (2006)) and the standard infinite-lived social planner government.

Timing

To begin, I clarify the timing of an offshore oil discovery (Figure 8). Baseline equilibrium outcomes $Y_{m,t0}$ prevail in the pre-discovery period. Due to long delays in the development of offshore oil fields, there is a gap of several years between announcement of an offshore discovery and the initiation of production and revenue generation. I call this gap the discovery period, during which political agents and governments may react in anticipation of future revenues. Finally, production begins and the revenue outcomes of the discovery are realized. These outcomes may vary over two dimensions. First, revenues may arrive earlier or later than anticipated. Second, revenues, when they arrive, may be higher, lower, or equal to the level anticipated. Long delays may have effects similar to lower-than-anticipated revenues. Equilibrium outcomes may change again upon the realization of revenue type (satisfied, neutral, or disappointed), with the direction of the change depending on the outcome in question.

Figure 8: Conceptual Timeline of Discovery Impacts



Following the political agent framework, suppose there is a local government with an elected leader. This leader is a utility maximizer who seeks to appropriate personal rents and win re-election. The leader faces a tradeoff between these two activities since rents available for appropriation are fixed in the baseline (Brollo et al., 2013). The leader is subject to reelection with free entry of challengers. The leader can allocate revenues to personal appropriation, public goods, or patronage, and can set taxes. The relative efficacy of public goods provision versus patronage in winning elections is an important determinant of whether resources are a curse or blessing.[10] 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.[11]

After the Discovery Announcement

After the announcement of an oil discovery, rational political agents update their expectations of future resource revenues. The increase in expected revenues increases the incentive for incumbents to stay in power and for challengers to seize power. Agents' responses to this change determine whether the discovery becomes a curse or a blessing for the community. If the leader substitutes from productive activities such as governing to reelection activities such as campaigning or fundraising, this could hurt governance and worsen outcomes. Likewise, if the leader shifts revenues from public goods to patronage, this could reduce welfare directly (fewer public goods) and indirectly (by, say, giving public jobs or contracts to low-quality political cronies) (Caselli and Cunningham, 2009). The leader can also cut taxes to curry popular support, under the assumption that future resource revenues will fill the gap. This could undermine governing capacity and public goods provision in the present, and could become a curse in the future if the realization of resource revenues is disappointed.

Increased interest in holding office could also prompt the leader to exert more governing effort or to provide more public goods. In this case the discovery would be a blessing for the community. Whether a blessing or curse outcome prevails depends on how well patronage works vis a vis public goods provision. This is likely to depend on prevailing levels of institutional quality and governing capacity. In

[10] In future versions of this work, I will incorporate patronage as another outcome potentially affected by major discovery announcements. To do so, I adopt a methodology introduced by Colonnelli et al. (2019) to measure patronage, defined here as the quid pro quo exchange of campaign donations for government jobs. Using individuals' national ID numbers as a bridge, I link data on each campaign donation in a municipal election to identified data on the universe of municipal employees and their date of hire. This allows me to calculate the rate at which candidates hire campaign supporters for discretionary government jobs upon winning an election. If the expected rents from holding office increase upon announcement of an oil discovery, politicians may be more willing to distribute patronage in order to win.

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

communities with weak institutions and low governing capacity, public goods provision may be inefficient and patronage may be easier, shifting the balance in favor of a suboptimal equilibrium.

The discovery may also influence leaders' behavior through electoral competition. An anticipated increase in the value of holding office after the discovery announcement should increase political competition, which could manifest itself as more candidates running for office or more donations being offered. The 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 increased rents are more valuable for individuals of lower ability, who can earn less in alternative occupations (Brollo et al., 2013). More competition can shorten the time horizon for leaders, increasing their personal appropriation of rents if they think their days are numbered.

After Realization of Revenues

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 may unfold. Furthermore, in this satisfied case leaders may find it easier to appropriate personal rents without the population noticing, leading to an increase in corruption and patronage (Baragwanath Vogel (2020); Brollo et al. (2013)). In the case of a low revenue realization, leaders may be forced to balance budgets by cutting spending or hiring, reducing their reelection rates.

Predictions from the Political Agent Framework

Based on the mechanisms explored above, I make the following informal predictions about the effects of an oil discovery and its revenue realization on local public finances and elections:

1. After a discovery announcement, spending on public goods will rise and taxes will fall as leaders appeal to voters. This reaction could be curtailed by credit or fiscal constraints.

2. After a discovery announcement, political competition and patronage will increase as expected returns to holding office rise.

3. After realization of high revenues, spending will increase, taxation will fall, and political competition and patronage will remain high or increase further.

4. After realization of low revenues, public goods spending will fall, taxation will rise, and incumbent reelection rates will fall.

The Social Planner Framework

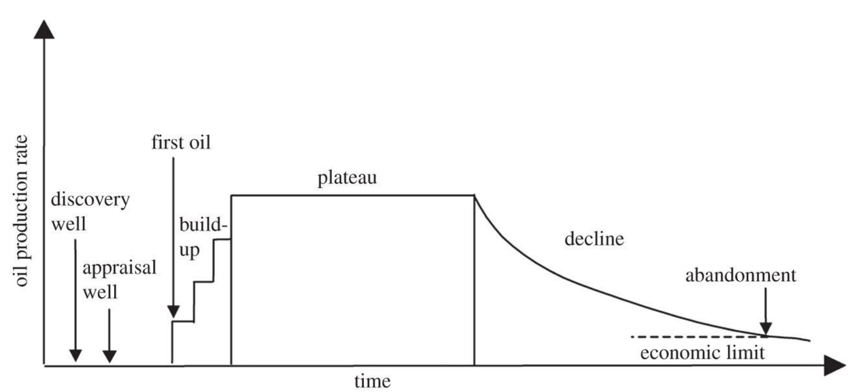
An alternative to the political agent model is the social planner model. In this setup, a local infinite-lived government maximizes welfare over all periods, seeking to smooth public goods consumption between the low-revenue discovery period and the high-revenue production period by borrowing against future revenues (if not credit constrained). This model would also generate prediction 1 and the first parts of predictions 3 and 4. It would not rule out increased political competition, but would be incompatible with an observed increase in patronage following a discovery.

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 simple model of offshore oil production and royalty distribution to forecast each discovery-affected municipality's expected and realized 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 simple 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 9 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 9: Offshore Oil Production Curve



Source: Han et al. (2019)

For each discovery announcement d, let t_0 be discovery year, θ_{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.[12] Then δV is the peak rate of production, where δ 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 ($\mathbb{1}(alignment_{md} = 1)$) according to the geodesic projection maps described above:

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

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 $E(Production_{mdt})$ 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 municipality m and field f:

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

See Appendix F 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 their levels at the time of discovery, in order to focus on expectations as they would have been formed in t_0, rather than as they would have developed according

[12] 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.

to future price and exchange rate trends. I also simplify by converting oil and gas discoveries into oil equivalent units, and by ignoring special participations, which are additional taxes 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 expected growth in per capita revenue between the year of the event and year t over realized revenue growth over this period:

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

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 G, 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 the arbitrary nature of assumptions in the model, I check for robustness to low, medium, and high combinations of parameter assumptions. I vary δ (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 a large gap between expected and realized revenues. The bottom row of figures are "satisfied." In sum, in my preferred specification, 30 Brazilian municipalities are left disappointed by major discovery announcements, while 18 are satisfied. In Appendix H, I report the disappointed/satisfied classification for all discovery-affected municipalities under alternative modeling parameters.

Figure 10: Municipality-Level Per Capita Revenue Forecasts vs Realized Revenues (Selected Examples)

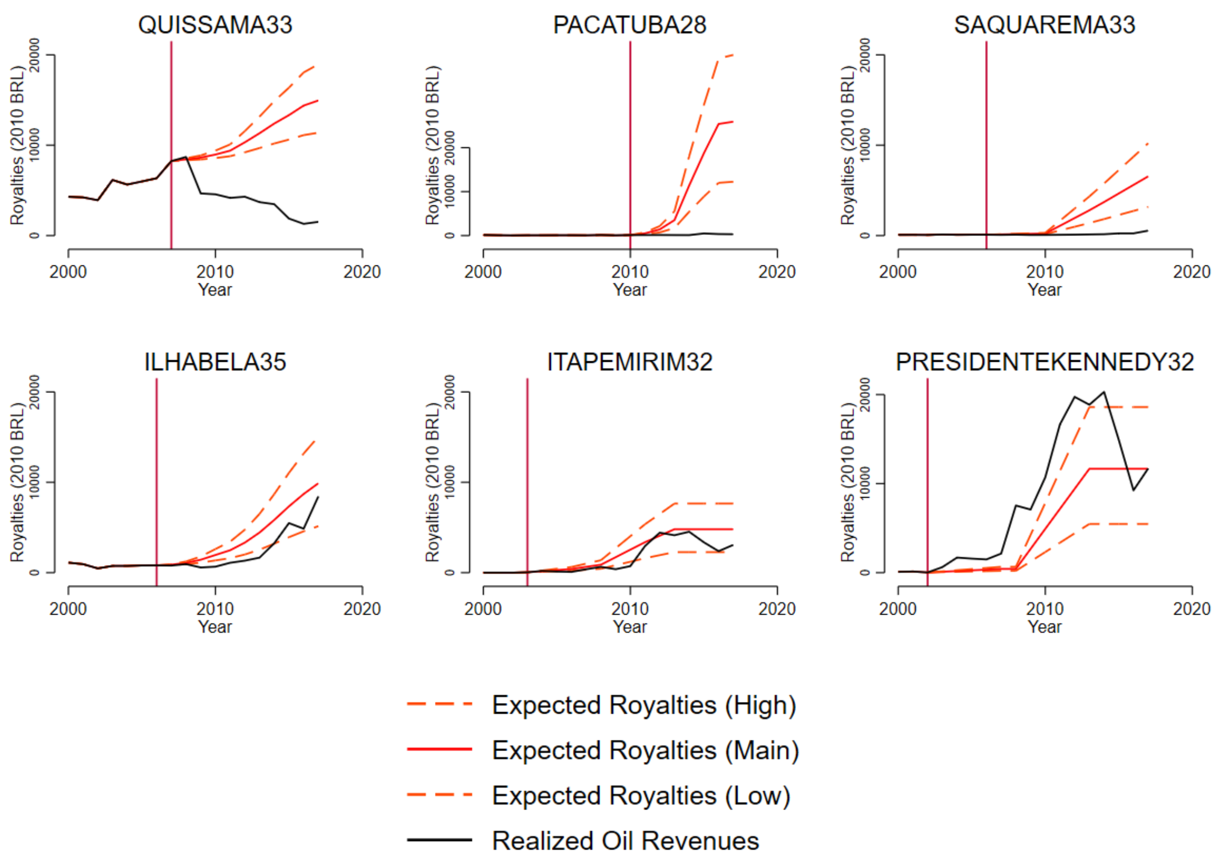
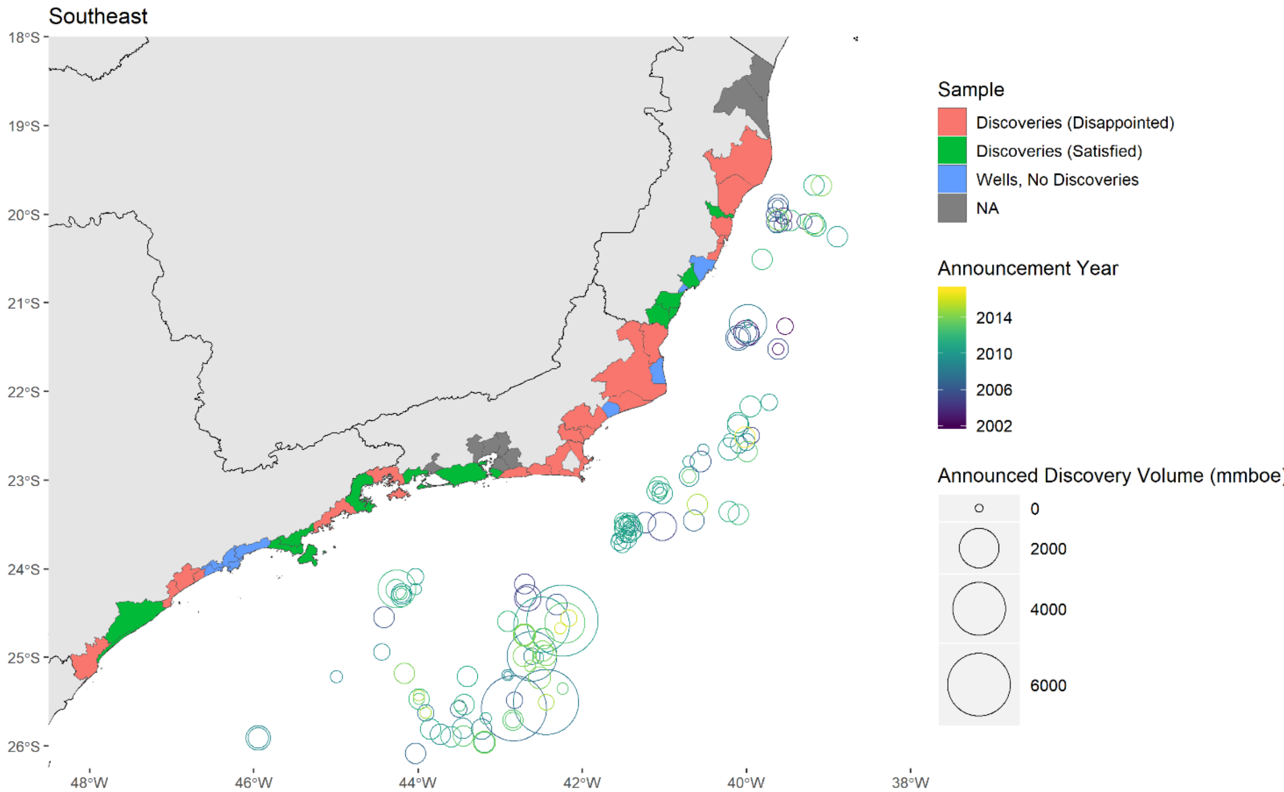


Figure 10 plots all major offshore oil and gas discoveries reported between 2000 and 2017 in Brazil's Southeast region (where most major Pre-Salt discoveries occurred), as well as the treated samples of disappointed and satisfied municipalities identified by my revenue forecasting model. The figure also 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). In Appendix I, I reproduce Figure 11 for the entire Brazilian coastline.

Figure 11: Southeast Brazil: Major Offshore Discoveries and Affected Municipalities



5 Data

The Presource Curse is hypothesized to function through diverse mechanisms. I draw on diverse administrative data sources to build an exceptionally rich municipality-year panel dataset to explore the effects of discovery announcements across a range of outcomes between 2000 and 2017. These include municipal public finance variables such as revenue, spending, and associated sub-categories, federal and state transfers to municipal governments, municipal public hiring, real measures of public goods provision and quality, and private sector indicators such as employment, wages, and firm entry by sector. 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. All 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 J. Table 1 summarizes my data sources.

In Table 2, I present baseline (year 2000) descriptive statistics for each treated subsample ("Disappointed" and "Satisfied"), as well as for alternative control groups. Following Cavalcanti et al. (2016),

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

my preferred control group (which I refer to here as "Wells", consists of municipalities that also received exploratory offshore wells after 1999, but did not receive major discovery announcements. Evidently, this control group differs along a number of dimensions from treated municipalities. Municipalities in the Wells group are located farther 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 average Municipal Development Indices and lower baseline 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 unique 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 their matched subsamples is significantly improved. Finally, 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**			
	Disappoint.	**Satisfied**	**Wells**	**Match (D)**	**Match (S)**	**Coastal**
Latitude	-19.50	-21.82	-13.04	-20.21	-20.00	-16.40
	(6.25)	(3.13)	(9.59)	(7.91)	(8.13)	(9.24)
Dist. from State Capital	116.62	88.59	150.15	192.14	92.79	248.87
	(85.35)	(57.12)	(120.02)	(143.64)	(38.81)	(159.90)
Population (Thousands)	91.88	398.53	55.42	38.11	56.82	32.26
	(122.23)	(1,367.51)	(81.82)	(77.30)	(471.41)	(192.54)
GDP per capita	17,769	13,779	6,552	6,814	7,840	5,443
	(26,418)	(12,003)	(6,735)	(7,261)	(9,641)	(5,978)
Annual Income p.c.	3,135	4,065	1,985	2,474	2,688	2,019
	(131)	(183)	(129)	(92)	(123)	(102)
Income Gini Coefficient	0.57	0.57	0.56	0.55	0.53	0.54
	(0.05)	(0.04)	(0.07)	(0.06)	(0.06)	(0.07)
Municipal Dev.Index	0.60	0.64	0.50	0.57	0.57	0.53
	(0.07)	(0.09)	(0.10)	(0.09)	(0.13)	(0.13)
Urban Share of Pop.	0.83	0.80	0.66	0.68	0.66	0.57
	(0.21)	(0.22)	(0.24)	(0.20)	(0.25)	(0.24)
% HHs w. Water/Sewer	7.76	3.63	20.56	10.03	10.67	13.64
	(8.01)	(3.95)	(19.57)	(12.19)	(15.81)	(16.19)
% Empl. in Extractive	1.07	0.96	1.03	0.44	0.45	0.44
	(2.01)	(1.98)	(3.57)	(1.01)	(0.96)	(1.50)
% Formally Employed	46.14	47.39	34.39	46.19	45.58	36.00
	(12.45)	(12.46)	(16.47)	(15.70)	(19.09)	(19.44)
Municipal Revenue p.c.	1,628	1,729	1,011	969	1,220	1,000
	(1,478)	(1,047)	(809)	(2,993)	(3,840)	(1,496)
Municipal Tax Rev. p.c.	209.3	395.5	123.3	71.4	114.7	41.8
	(224.4)	(438.5)	(276.0)	(459.8)	(596.1)	(225.5)
Municipal Oil Rev. p.c.	420.6	161.8	129.7	15.1	10.2	6.1
	(999.4)	(334.7)	(412.9)	(100.4)	(43.4)	(60.0)
Municipal Spending p.c.	1,222	1,435	807	857	1,062	865
	(973)	(812)	(554)	(2,913)	(3,745)	(1,442)
Municipal Invest. p.c.	161.0	123.1	98.2	55.0	69.7	63.3
	(223.9)	(110.3)	(172.1)	(116.9)	(143.8)	(83.2)
n	30	18	53	836	500	3,902

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 in 2000 or later), 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). All monetary values are in constant 2010 Brazilian Reals. All reported values are from the pre-treatment 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.[13] 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 drop treatment and pre-treatment indicators for year 2017, the last year of my sample, allowing units treated in this year to act as controls. I control for two-way fixed effects, δm and λ_t, and cluster standard errors at the unit level (municipality):

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

In this expression, β_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 problesm for causal inference (de Chaisemartin and D'Haultfœuille, 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 (Cust et al. (2019); Cavalcanti et al. (2016)).

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 studies.[14] Finally, I implement Callaway and Sant'Anna's (2020) group-time average treat-

[13] 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 K.

[14] 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

ment effect approach for key outcomes to address potential bias introduced by the two-way fixed effects specification.

I estimate Equation 4 separately for disappointed and satisfied municipalities, each relative to their own matched control group. 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):

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

Difference-in-Differences: Discovery Effects on Elections

Since municipal elections occur every four years, I opt for a two-way fixed effects difference-in-differences approach, rather than an event study. 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. I consider outcomes reflective of political competition, including number of candidates and competitive candidates, number and value of campaign donations, and candidate characteristics. 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 (δ_m and λ_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.

$$Y_{me} = \delta_m + \lambda_e + \beta T_{me} + \epsilon_{me} \tag{6}$$

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

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.

revenue growth over the previous mandate over expected revenue growth over the same period:

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

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:

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

Here, β 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), as well as across estimators (TWFE versus Callaway and Sant'Anna (2020)).

Identification Challenges

An ideal experiment to evaluate the effects of discovery announcements and later 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 world prices. Thus, geographical features are predictive of offshore oil and gas outcomes. Conditional on fixed geographical features, do pre-discovery municipality characteristics predict where future discoveries occur, or whether discoveries 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 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 drilling is undertaken, a successful versus unsuccessful outcome is as good as random. If it were otherwise, the oil company could have used this information to avoid costly drilling in unsuccessful places.

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 more information about that field. Thus, over time, characteristics of the field can be revealed that make it more or less profitable, including its commercial break-even point, which depends on the exogenous evolution of world oil prices. Further variation in the 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 occurred at different times, world oil price fluctuations introduce additional exogenous variation into revenue realizations. For instance, 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 1) whether wells are drilled in coastal state municipalities; 2) whether a major discovery is announced in municipalities where wells are drilled; and 3) whether expectations are satisfied in municipalities that received discovery announcements. I include a vector of geographical controls (latitude, distance to state and federal capitals) and state fixed effects:

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

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 their realizations were exogenous to municipality characteristics, thus constituting a quasi-experiment.

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

	$\mathbb{1}(Wells = 1)$	$\mathbb{1}(Discovery = 1)$	$\mathbb{1}(Satisfied = 1)$
Outcome	**p-value (FWER-adjusted)**	**p-value (FWER-adjusted)**	**p-value (FWER-adjusted)**
Population	0.261	0.661	0.206
	(0.817)	(0.994)	(0.804)
GDP	0.016	0.902	0.235
	(0.135)	(0.995)	(0.804)
Municipal Develop. Index	0.192	0.163	0.183
	(0.777)	(0.684)	(0.804)
Urban Share of Population	0.484	0.600	0.123
	(0.974)	(0.993)	(0.725)
Income per capita	0.022	0.673	0.404
	(0.135)	(0.994)	(0.804)
Income Gini Coefficient	0.858	0.017	0.192
	(0.992)	(0.119)	(0.804)
% Employed in Extractive	0.046	0.802	0.226
	(0.135)	(0.995)	(0.804)
% Formally Employed	0.667	0.496	0.450
	(0.92)	(0.988)	(0.804)
% Homes w. Water & Sewer	0.755	0.823	0.958
	(0.992)	(0.995)	(0.961)
% Empl. in Public Sector	0.971	0.874	0.047
	(0.992)	(0.995)	(0.502)
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 β_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 local 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, though this is potentially counfounded by the fact that most capitals of coastal states are on the coast, and thus more likely than most municipalities to experience offshore activity. Again, Table 4 supports my claim that offshore outcomes were exogenous to municipality conditions.

Table 4: Conditional Random Assignment: Political Alignment

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

Regressions in the first panel are estimated separately using OLS on cross-sectional municipality-level datasets and controlling for the following geographical controls: distance to federal and state capitals, latitude, and state fixed effects. All distances use the inverse hyperbolic sine transformation. Cumulative party alignment with governor is the number of years since 2000 in which the municipal mayor's political party was the same as the state governor's party. Likewise, cumulative party alignment with president is the number of years in which the mayor's party was the same as the federal president's party. Regressions in the second panel are estimated separately using logit models on municipality-year panel datasets and controlling for the same geographical controls. Contemporaneous party alignment with governor (likewise for president) is an indicator variable that takes a value of 1 in years when the municipal mayor's political party is the same as the state governor's party (or federal president's party). Model p-values associated with parameter β_1 from Equation 9 are reported, with family-wise error rate corrected Romano-Wolf p-values in parentheses where applicable. Estimation used rwolf 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 (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, I also graph sample means of key outcomes for treated subsamples and their control groups in Appendix L, allowing the reader to evaluate differences in levels and "wiggles" as well as trends (McKenzie, 2021).

SUTVA is likely violated locally as a feature of Brazil's revenue sharing rules, which stipulate that 20% of royalties from a field be distributed to municipalities that share a mesoregion (a geographical unit between state and municipality that typically contains around 40 municipalities) with the producer municipality. I assume that revenue spillovers from such widespread sharing dilute spillover effects onto individual municipalities. Furthermore, municipal public finance and electoral outcomes have little potential for spillovers, as changes in one municipalities' budget or electoral conditions are unlikely to affect neighbors. Spatial spillovers may be more likely for firm-level outcomes, since firms can relocate in response to treatment. In Appendix N, I explicitly analyze spatial spillovers from discovery announcements.

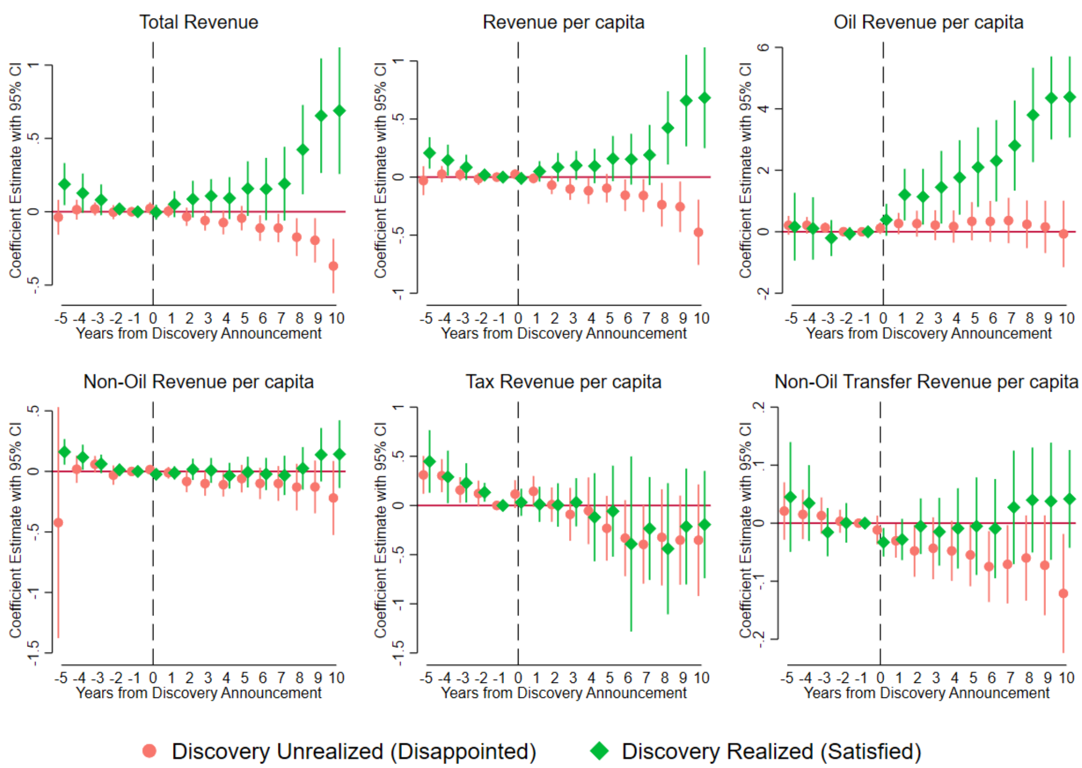
My preferred choice of control group ("Wells") reduces concerns over anticipatory effects. Since both treated and control municipalities in this group are experiencing relatively constant offshore oil exploration activity, it is likely that both groups experience prolonged, low-level anticipation of receiving a discovery announcement. The unpredictable nature of major discoveries means that additional anticipation on top of this shared 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, 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, which allows verification of pre-trends and accounts for the typical delay between discovery announcement and peak production (which ranges from 5-10 years). In Appendix M, 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.

7 Results

In this section I present results from event studies and difference-in-differences specifications. I first focus on standard municipal public finance indicators such as revenues and spending. Figure 12 shows event study results for discovery effects on municipal revenues. I plot estimates for Satisfied (green) and Disappointed (red) treatment 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 J I report alternative estimates using a specification that allows for multiple discovery events per treated unit. In Appendix O, I present summary tables of sample means and sizes, coefficient estimates, and semi-elasticities for key outcomes.

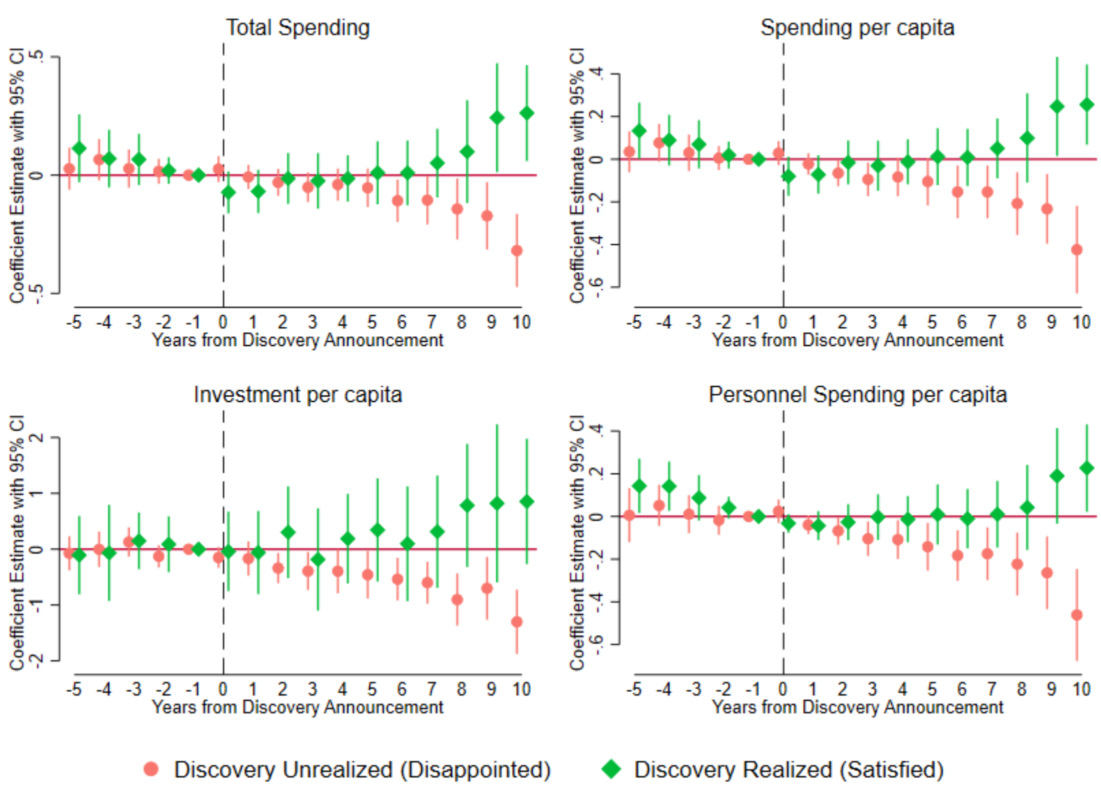
Figure 12: Revenues



Note: For the following figures, 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. Continuous outcomes are transformed using inverse hyperbolic sine transformation; monetary values are deflated to constant 2010 BRL. Note that Y-axes are not uniform, which allows better visual inspection of effects but may conceal scale differences across graphs. 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.

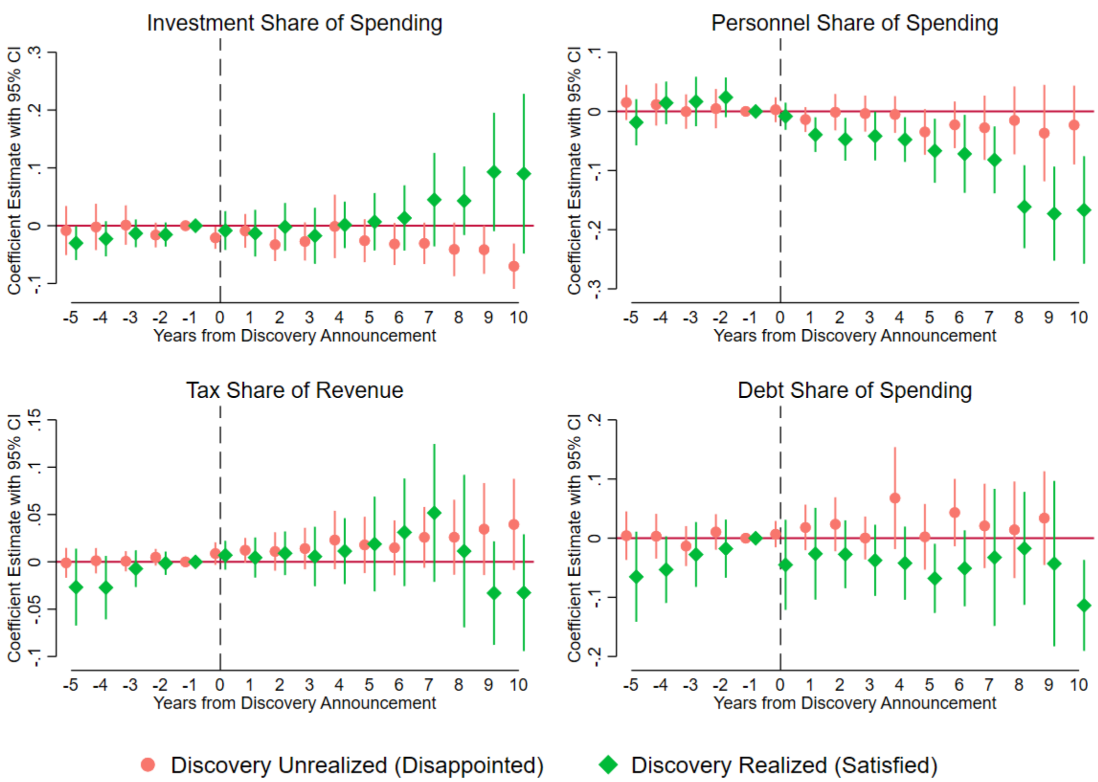
As evidenced in Figure 12, oil revenues increase within one year of discovery announcement in municipalities that will ultimately be classified as Satisfied, or have their "discovery realized." After ten years, discoveries in satisfied municipalities increase per capita oil revenues by 3,698% relative to controls. Municipalities that will ultimately be classified as "Disappointed" never experience an increase in oil revenues. Disappointed discoveries lead to 30% lower per capita revenues in these communities after 10 years. Interestingly, non-oil revenues also decline in disappointed municipalities, in part due to the significant decline in per capita transfer revenues from state and federal governments. Transfers are largely formulaic. I report estimates of discovery effects on each type of state and federal transfer in Appendix P. In disappointed municipalities, transfers pegged to population (FPM) and exports (Lei Kandir) decline significantly after discoveries.

Figure 13: Expenditures



Due to credit constraints and limits on deficit spending imposed by Brazil's fiscal responsibility law, changes in revenue translate closely into changes in spending (Figure 13). In satisfied municipalities, per capita spending increases significantly beginning nine years after the first major discovery announcement, aligning with the typical delay period between discovery and peak production. In contrast, per capita spending declines significantly in disappointed municipalities beginning three years after a discovery. Investment in disappointed municipalities is reduced by 45% relative to controls after 5 years, and by 68% after 10 years. This reduction in investment may compromise long-term growth prospects for disappointed municipalities. Spending on personnel is also reduced in disappointed places, and increases significantly in satisfied municipalities.

Figure 14: Fiscal Health Indicators

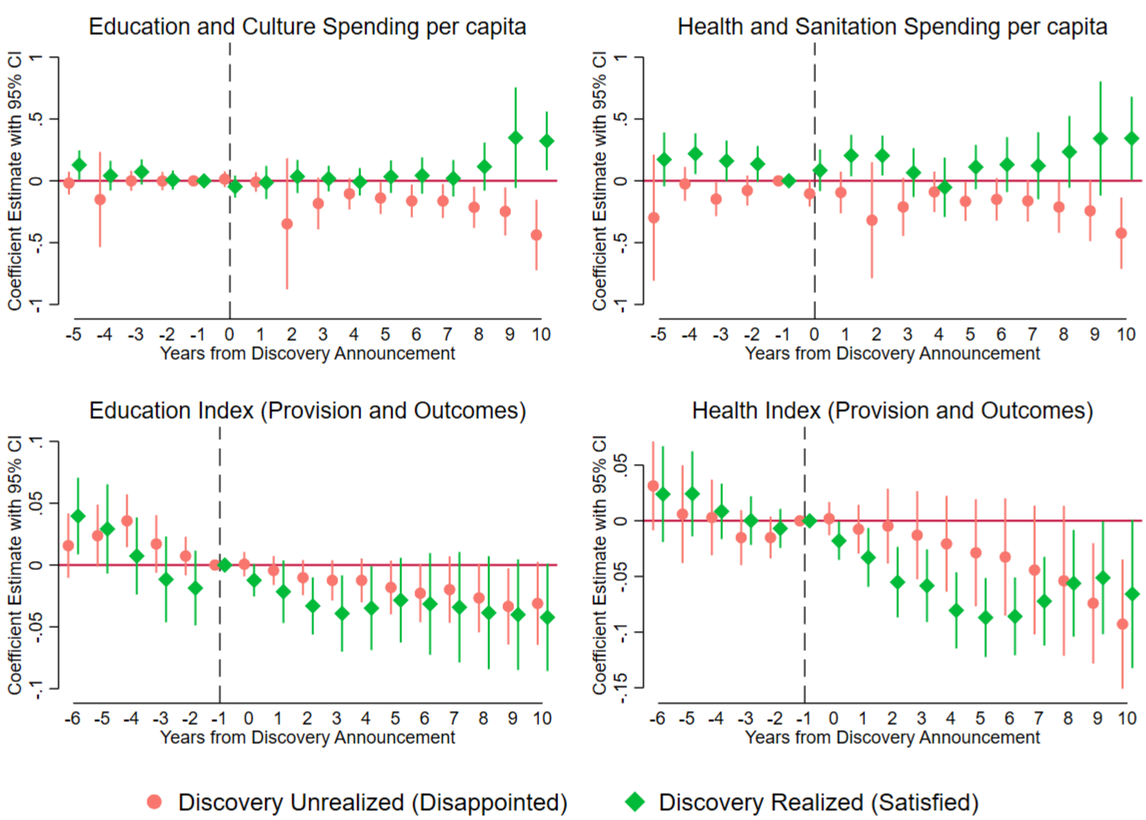


Note: Debt share of revenue is taken by summing expenditures on debt (processed, unprocessed, and liquidating), debt service, debt restructuring, interest, and *restos a pagar*.

Turning to measures of fiscal health proposed by FIRJAN (2019), a Brazilian industry association, Figure 14 illustrates dynamic effects of discovery announcements on the share of municipal budgets going to investment, personnel, and debt management costs, as well as the share of revenues coming from local taxes. Higher investment shares and lower personnel shares are considered healthy. According to FIRJAN, low tax shares are suggestive of low fiscal "autonomy," that is, a municipality's ability to sustain its own budget without state or federal transfers. Finally, the share of "*restos a pagar*" (IOUs issued by municipalities) and costs of interest and debt restructuring is taken as a sign of budgetary imbalances.

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 rise, suggesting municipalities 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. Given that municipalities have limited capacity to issue debt, it is unsurprising there do not appear to be significant effects of discovery announcements on the share of budgets going to debt management.

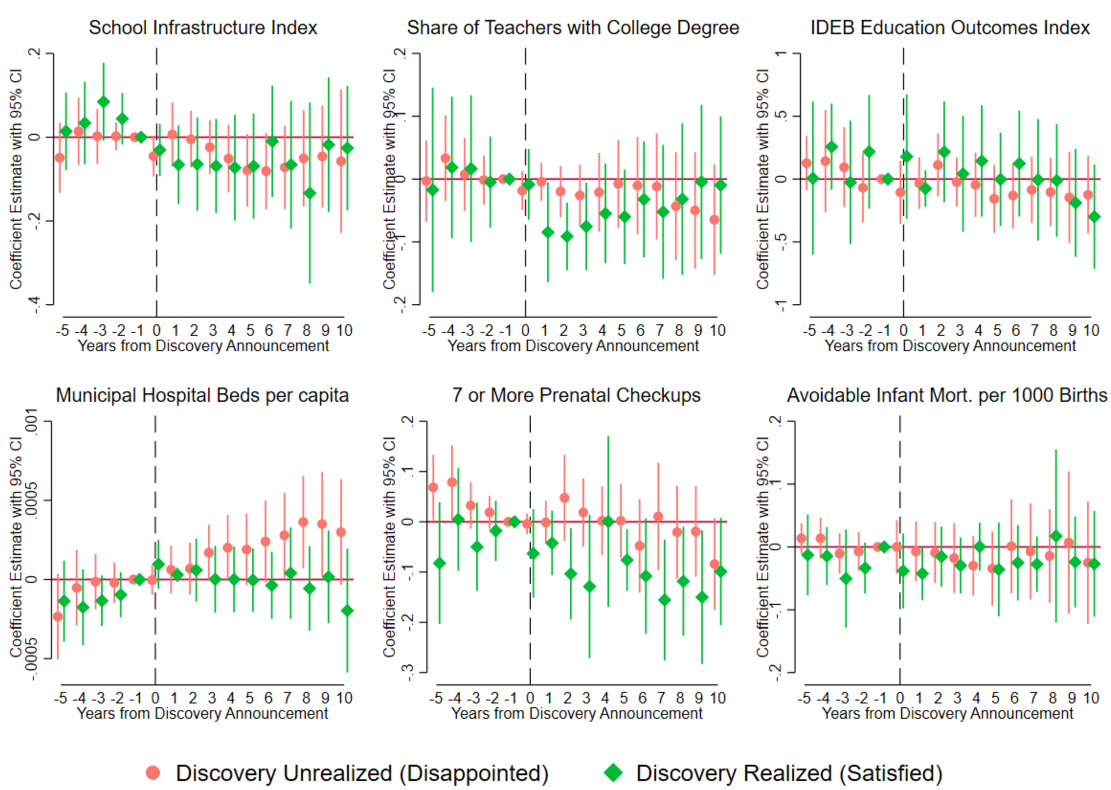
Figure 15: Public Goods Spending & Performance Indices



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.

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 15 illustrates effects of discovery announcements on public good 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 48% after 10 years, and spending on health and sanitation increases by 33%. Despite these significant increases, index measures of public goods provision and outcomes do not reflect significant improvements, and if anything, small declines in these communities. These findings corroborate previous findings by Caselli and Michaels (2013). This 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. Disappointed municipalities experience declining per capita education (-27%) and health (-35%) spending after 10 years.

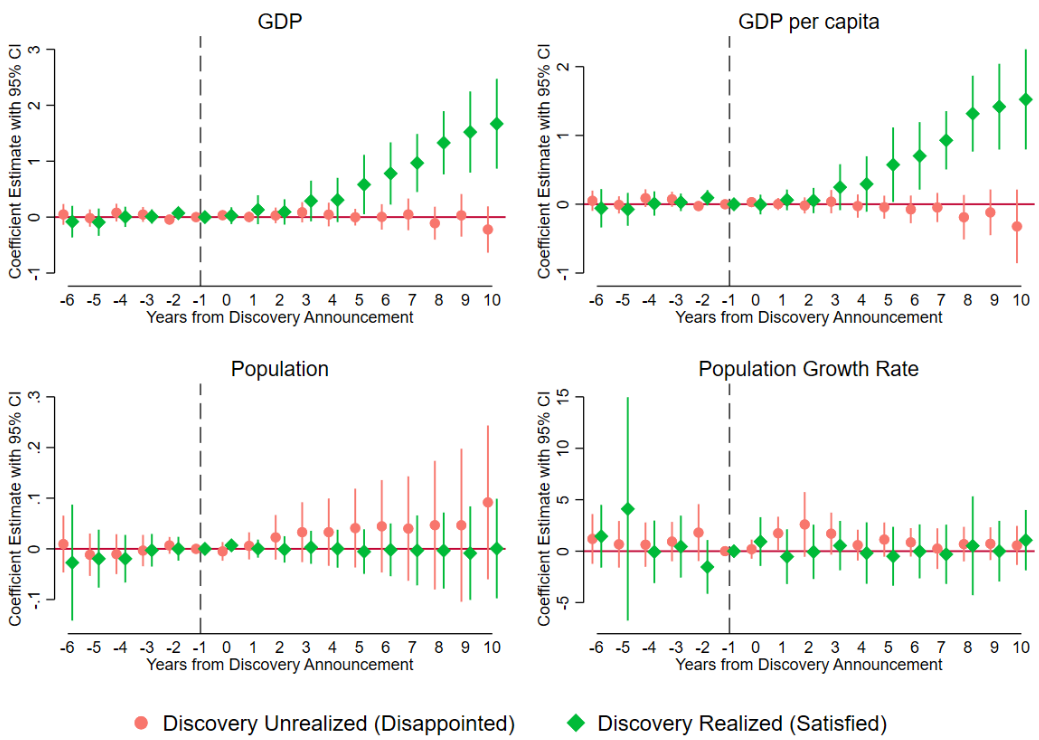
Figure 16: Public Goods Provision and Quality



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.

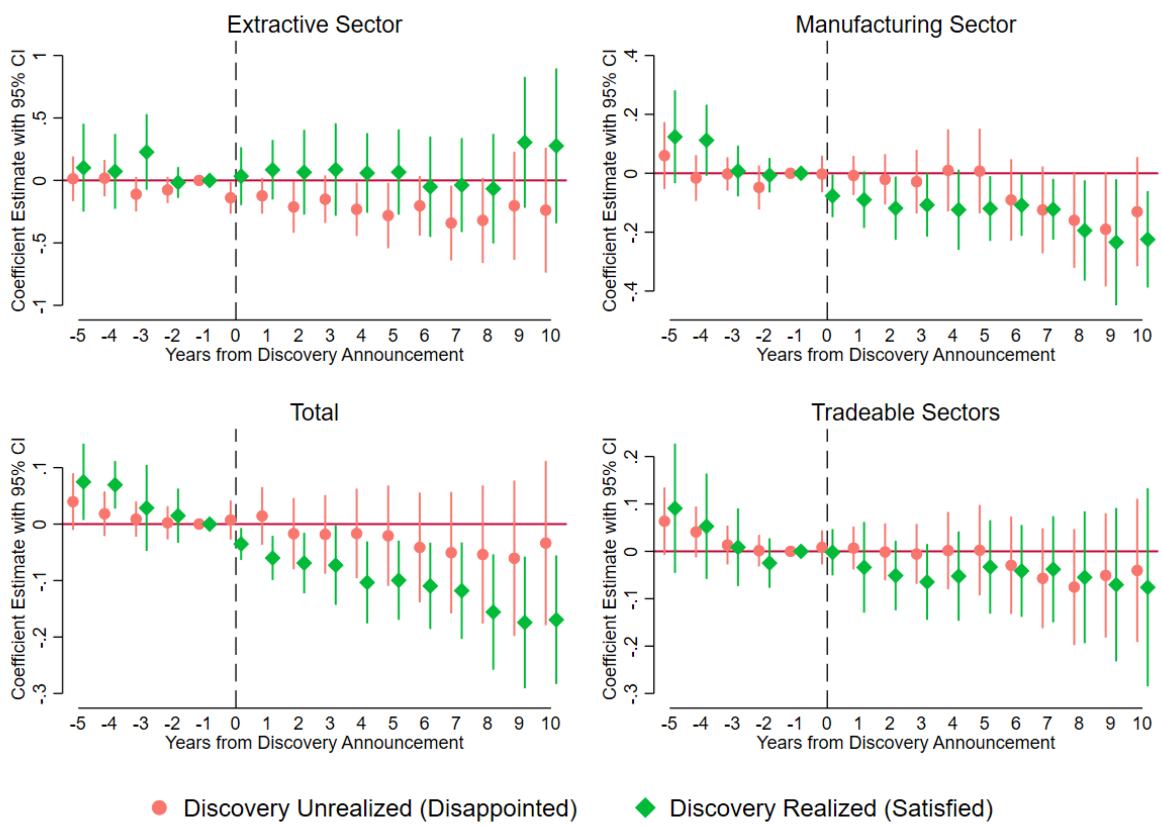
Clearly, disappointed municipalities experience significant per capita declines in revenues and spending after major discovery announcements. How much of this is driven by changes in population? Figure 17 shows discovery effects on population and population growth rates. While population trends upwards in disappointed municipalities (and increases significantly relative to the pre-matched control group) it does not change significantly in either group in the Wells specification. 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 Q). However, as most major discoveries occurred in 2007 or later, I may miss important migration effects due to temporal limitations of this dataset. Realized discoveries have large positive effects on overall economic activity as measured by GDP (+376% ten years on). GDP remains unchanged in disappointed municipalities.

Figure 17: GDP & Population



I next turn to firm entry. While municipalities may not have anticipated oil booms after discovery announcements due to lack of capacity or fiscal/credit constraints, firms should face fewer constraints and may have a profit motive to anticipate revenue booms. Firms may be affected by oil revenues in a number of ways. First, they may benefit from oil revenues received by municipal governments, which are spent in turn on contracts for infrastructure projects and other goods and services. Second, firms may benefit from spillover effects of increased government spending (e.g. more public employees eating at restaurants, buying houses, shopping). Third, firms may be affected directly by offshore oil activities. This is especially likely for firms in the extractive sector, which may set up near offshore fields to provide services and support, and firms in manufacturing, which may likewise set up near offshore fields to connect with upstream linkages generated by the oil sector. As illustrated in Figure 18, the number of extractive firms declines in disappointed municipalities and remains unchanged in satisfied municipalities. The number of manufacturing firms declines in both disappointed and satisfied groups by 21% and 25%, respectively. While most results in this section are stable across samples and estimators, results on firms and employment vary across the Wells and Pre-Matched control groups. I present results for number of firms and employees by sector using the Pre-Matched control group in Appendix S.

Figure 18: Number of Formally Registered Firms



Note: Firms are tabulated from RAIS matched employer-employee records. Sectors are defined according to standard CNAE 2.0 classifications. Tradeable sectors are defined as agriculture, manufacturing, and extractive sectors.

One common concern following major resource discoveries is Dutch Disease, wherein large revenue windfalls and factor demands from the resource sector drive up local prices and crowd out tradeable activities (Cust et al. (2019); Corden and Neary (1982)). I test for symptoms of Dutch Disease after discovery announcements by looking at manufacturing employment and tradeable sector employment in disappointed and satisfied municipalities. In Appendix R, I explore effects on employment and wages in these sectors as well. Results indicate that discoveries have significant negative effects on the number of manufacturing firms in both disappointed (-21% after 10 years) and satisfied (-25% after 10 years) municipalities relative to Wells controls. In Appendix R, I show that manufacturing employment also decreased, while wages across sectors fell. These results provide contradictory evidence regarding Dutch Disease, and require further exploration. In Appendix N, I explore spatial spillovers onto neighboring municipalities, and find that neighbors of discovery-affected communities may have suffered declines in manufacturing firm entry and employment after discovery announcements.

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, 2021; de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2018). Bias comes from the TWFE estimator's inclusion of already-treated units as controls for treated units, and estimators that use only never-treated or to-be-treated units as controls resolve this problem. 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 T, 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. This stability may be because my preferred specifications include a large number of never-treated control units relative to the number of treated units. 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 V.

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

	TWFE Wells	TWFE Pre-Matching	CS Wells	CS Pre-Matching
Total Revenue	-0.20**	-0.07	-0.38***	-0.14
	(0.08)	(0.07)	(0.10)	0.15
Revenue p.c.	-0.26**	-0.23**	-0.54***	-0.37**
	(0.11)	(0.10)	(0.17)	0.19
Tax Revenue p.c.	-0.35	-0.34*	-0.26	-0.30
	(0.23)	(0.18)	(0.29)	0.24
Oil Revenue p.c.	0.16	0.50	-0.03	0.16
	(0.43)	(0.39)	(0.72)	0.69
Transfer Revenue p.c.	-0.07*	-0.06*	-0.14**	-0.15***
	(0.04)	(0.04)	(0.07)	0.06
Total Spending	-0.17**	0.00	-0.30***	-0.03
	(0.07)	(0.07)	(0.10)	0.12
Spending p.c.	-0.23***	-0.14*	-0.46***	-0.25*
	(0.08)	(0.07)	(0.12)	0.14
Investment p.c.	-0.70**	-0.80***	-1.28***	-1.04***
	(0.28)	(0.26)	(0.33)	0.37
Personnel Spending p.c.	-0.26***	-0.16**	-0.52***	-0.29*
	(0.09)	(0.08)	(0.14)	0.15
Education Spending p.c.	-0.25**	-0.19**	-0.46***	-0.32**
	(0.10)	(0.09)	(0.16)	0.14
Health Spending p.c.	-0.24*	-0.33***	-0.43***	-0.33
	(0.12)	(0.11)	(0.15)	0.20
# Extractive Employees	0.24	0.35	0.04	0.39
	(0.52)	(0.49)	(0.76)	0.69
# Mfg. Employees	-0.18	0.31*	-0.25	0.36*
	(0.21)	(0.17)	(0.24)	0.19
# Extractive Firms	-0.20	0.03	-0.13	0.22
	(0.22)	(0.21)	(0.27)	0.25
# Mfg. Firms	-0.19*	0.07	-0.10	0.19**
	(0.10)	(0.09)	(0.10)	0.09
Avg. Formal Wage	-0.11**	-0.02	-0.19***	-0.02
	(0.05)	(0.04)	(0.07)	0.06
GDP p.c.	-0.12	-0.12	-0.34	0.01
	(0.17)	(0.15)	(0.30)	0.35
Population	0.05	0.14*	-0.34	-0.02
	(0.08)	(0.07)	(0.30)	0.06
n (municipality-years)	1,494	15,570	1,494	15,570

Each column reports coefficient estimates and standard errors for the $t+10$ period of event studies for a specific control group-estimator pair. 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

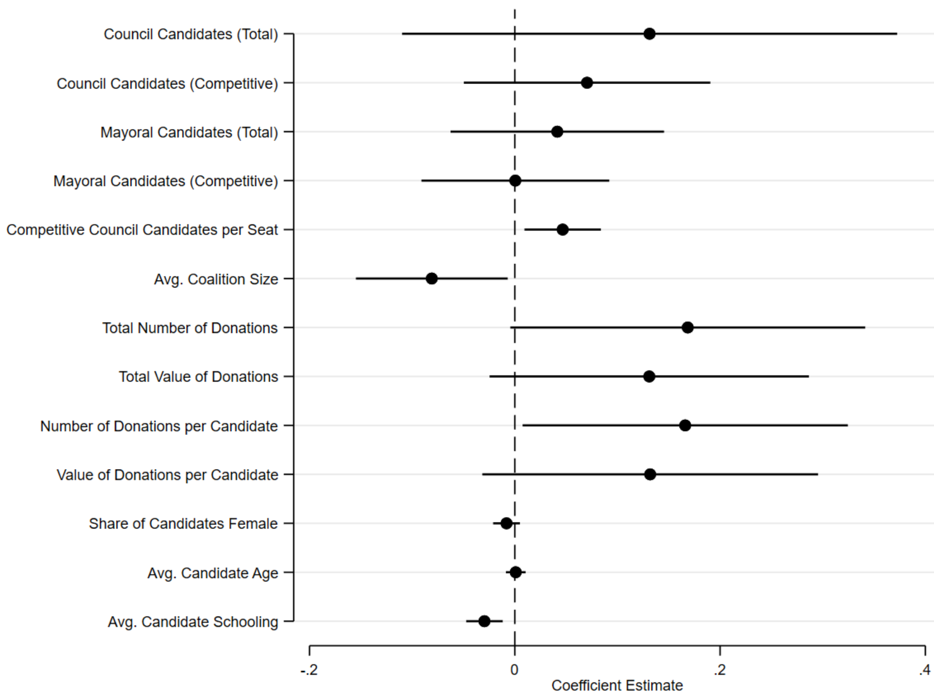
	TWFE Wells	TWFE Pre-Matching	CS Wells	CS Pre-Matching
Total Revenue (Millions)	0.65***	0.83***	0.76***	0.89***
	(0.20)	(0.19)	(0.25)	(0.29)
Revenue p.c.	0.66***	0.77***	0.74***	0.87***
	(0.20)	(0.19)	(0.25)	(0.28)
Tax Revenue p.c.	-0.21	0.07	0.02	0.22
	(0.30)	(0.26)	(0.29)	(0.31)
Oil Revenue p.c.	4.35***	4.49***	4.69***	4.45***
	(0.68)	(0.69)	(0.95)	(1.01)
Transfer Revenue p.c.	0.04	0.08	0.05	0.04
	(0.05)	(0.05)	(0.06)	(0.06)
Total Spending (Millions)	0.24**	0.43***	0.28**	0.45***
	(0.12)	(0.11)	(0.11)	(0.13)
Spending p.c.	0.25**	0.38***	0.25**	0.43***
	(0.12)	(0.11)	(0.11)	(0.13)
Investment p.c.	0.82	0.92	1.44*	1.43
	(0.71)	(0.72)	(0.82)	(0.96)
Personnel Spending p.c.	0.19*	0.32***	0.26**	0.50***
	(0.11)	(0.10)	(0.12)	(0.13)
Education Spending p.c.	0.35*	0.41**	0.35***	0.45***
	(0.20)	(0.19)	(0.13)	(0.10)
Health Spending p.c.	0.34	0.31	0.42**	0.35*
	(0.23)	(0.19)	(0.19)	(0.19)
# Extractive Employees	0.62**	0.87***	0.68**	0.96***
	(0.29)	(0.23)	(0.31)	(0.28)
# Mfg. Employees	-0.21	0.25	-0.26	0.21
	(0.22)	(0.17)	(0.22)	(0.21)
# Extractive Firms	0.30	0.68***	0.39***	0.91***
	(0.26)	(0.24)	(0.36)	(0.34)
# Mfg. Firms	-0.23**	-0.03	-0.19*	0.04
	(0.11)	(0.10)	(0.10)	(0.11)
Avg. Formal Wage	-0.09*	0.00	-0.08	0.14*
	(0.05)	(0.04)	(0.06)	(0.08)
GDP p.c.	1.42***	1.51***	1.59***	1.82**
	(0.31)	(0.30)	(0.53)	(0.71)
Population	-0.01	0.06	0.35***	0.04
	(0.05)	(0.04)	(0.13)	(0.10)
n (municipality-years)	1,278	9,012	1,278	9,012

Each column reports coefficient estimates and standard errors for the $t + 10$ period of event studies for a specific control group-estimator pair. 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

Discovery Effects on Local Elections

Do discovery announcements increase political competition in municipal elections? Figure 19 presents results from difference-in-differences specifications with two-way fixed effects. Results suggest that a major discovery announcement in the four years prior to an election significantly increases the number of candidates running for council, as well as the number of competitive candidates.[15] Discoveries do not have an effect on the number of candidates running for mayor. Discoveries may also increase the number and value of donations, and donations per candidate, though these results are only significant at the 10% level. Finally, discoveries appear to induce less educated candidates to run for election, which may offer evidence that candidates with lower private sector options, who may be interested in rent-seeking, attempt to run for office in expectation of future oil revenues.

Figure 19: Effects of Oil Discovery in Last Four Years on Municipal Electoral Competition



Note: Figure plots coefficient estimates with 95% confidence intervals from a regression of each outcome variable separately on municipality and year fixed effects and a treatment indicator that takes a value of 1 when a major discovery announcement occurred in a municipality's catchment zone during the previous 4-year election period. Standard errors are clustered at the municipality level. Data are drawn from municipal elections in 2004, 2008, 2012, and 2016.

[15]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 θ_{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.

In Table 7, I compare estimates of election competition effects across control samples (Wells and Pre-Matched) and estimators (TWFE and Callaway & Sant'Anna (2020).

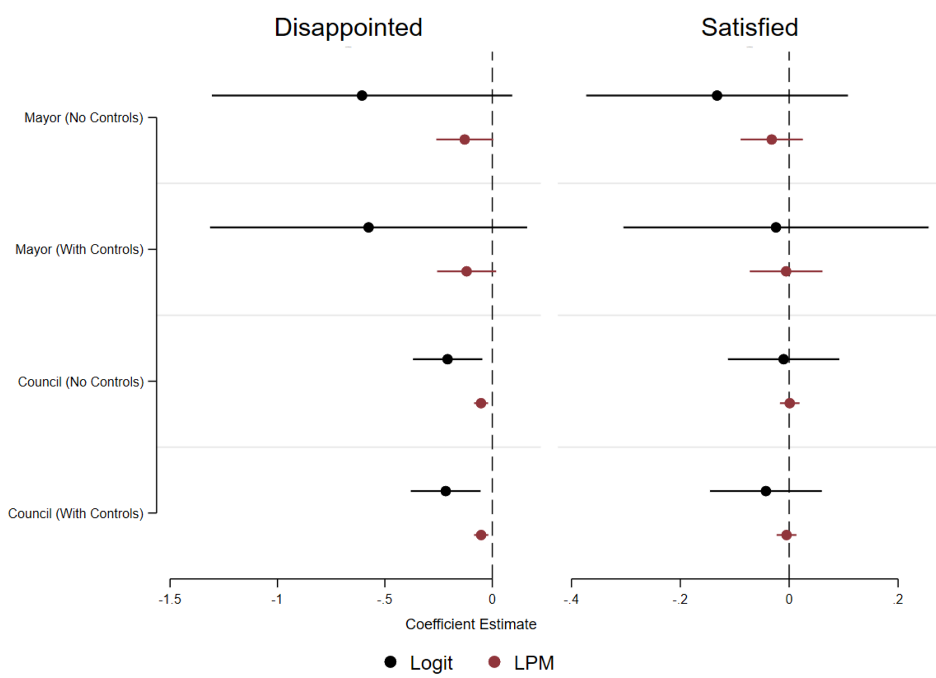
Table 7: Discovery Effects On Electoral Competition: Robustness Across Samples and Estimators

	TWFE Wells	TWFE Pre-Match	CS Wells	CS Pre-Match
Council Candidates (Total)	0.131	0.046	0.172	0.070*
	(0.122)	(0.032)	(0.235)	(0.037)
Council Candidates (Compet.)	0.070	0.061*	0.098*	0.066
	(0.061)	(0.034)	(0.105)	(0.037)
Mayoral Candidates (Total)	0.041	0.035	0.065	0.054
	(0.052)	(0.048)	(0.068)	(0.050)
Mayoral Candidates (Compet.)	0.001	0.008	-0.129***	-0.087*
	(0.046)	(0.047)	(0.045)	(0.046)
Comp. Council Cand. Per Seat	0.047**	0.038**	0.068***	0.033
	(0.019)	(0.018)	(0.025)	(0.022)
Avg. Coalition Size	-0.081**	-0.078***	-0.118*	-0.077*
	(0.037)	(0.028)	(0.062)	(0.041)
Total Number of Donations	0.169*	0.149	0.157*	0.164**
	(0.087)	(0.091)	(0.092)	(0.069)
Total Value of Donations	0.131*	0.119	0.238**	0.114
	(0.078)	(0.083)	(0.120)	(0.113)
Number of Donations per Cand.	0.166**	0.124	0.106	0.040
	(0.080)	(0.081)	(0.095)	(0.086)
Value of Donations per Cand.	0.132	0.095	0.195	-0.006
	(0.082)	(0.085)	(0.137)	(0.128)
Share of Candidates Female	-0.008	-0.016***	-0.010	-0.006
	(0.007)	(0.005)	(0.010)	(0.120)
Avg. Candidate Age	0.001	-0.002	-0.031**	0.000
	(0.005)	(0.004)	(0.014)	(0.011)
Avg. Candidate Schooling	-0.030***	-0.024***	-0.031**	-0.009
	(0.009)	(0.006)	(0.014)	(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, δ_m and λ_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 θ_{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 disconnect between discovery expectations and realized royalties, are incumbent politicians punished with reduced reelection rates? This question relates to Monteiro and Ferraz (2010), who find that politicians are rewarded for early revenue windfalls. Since voters cannot perfectly observe politicians' quality or honesty, they may vote according to observable performance, such as public goods provision or hiring. In this case, disappointment could result in a fiscal crunch, requiring local leaders to cut spending or raise taxes. Figure 20 shows 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, with and without control variables (age, sex, and schooling). 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 less likely to win reelection at the 10% significance level. Satisfaction appears to have insignificant effects on reelection rates. I report results corresponding with Figure 20 in Appendix W.

Figure 20: Effects of Disappointment/Satisfaction with Oil Discoveries on Incumbent Reelection



Note: Column 1 plots coefficient estimates (marginal effects for logit models) with 95% confidence intervals derived from regressions of incumbent election outcomes (reelection = 1) on state and year fixed effects and a binary treatment indicator = 1 when the time-varying measure of forecast error $disappointment_{me} < 0.4$. Control municipalities are all never-treated municipalities in coastal states. Negative coefficients indicate that incumbents were less likely to be reelected when the municipality was disappointed by realized offshore oil and gas revenue receipts at the time of the election. Column 2 plots similar results for municipalities where $disappointment_{me} > 0.8$ (satisfied). Standard errors are clustered at the municipality level in all specifications.

Event Study Robustness to Alternative Matching and Forecasting Specifications

The revenue forecasting model involves a number of 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 the results from these robustness exercises in Appendix U. Results are relatively stable across alternative specifications.

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 2002-2017. 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 30 of the 48 municipalities affected by offshore discovery announcements during this period ultimately received less than 40% 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 may play an important role in controlling anticipatory excesses after major discoveries.

Municipalities where discovery expectations were satisfied enjoy large increases in revenue, spending, and GDP per capita 10 years on from the first discovery announcement, but do not enjoy improvements in real public goods provision or outcomes. Disappointed municipalities experience lower revenues, spending, and investment, as well as worsened indicators of fiscal health (including a lower share of revenues from taxes, indicating reduced fiscal autonomy). Event study results are robust to alternative control groups and forecasting specifications. I conclude that outcomes in disappointed municipalities provide evidence of a Presource Curse. Municipalities that experience major discovery announcements but end up disappointed are left worse off along key development dimensions relative to matched counterfactuals that never receive a discovery announcement.

Political competition increases after discovery announcements, while the average schooling of candidates declines. While increased political competition may bring out better candidates (Galasso and Nannicini, 2011), the lower schooling of candidates suggests discoveries may attract lower-quality rent-seekers to office, as Baragwanath Vogel (2020) finds that royalty receipts do. Finally, when municipalities are disappointed at the time of an election, incumbent politicians' reelection rates are reduced, suggesting voters punish incumbents for negative outcomes, even when these outcomes are exogenous to municipal governance.

By focusing on subnational units, my study highlights a number of emergent properties that may not be apparent at the national level. First, disappointed municipalities in Brazil experience significant in-migration after discovery announcements, reducing per capita revenues and spending. Major resource discoveries affecting only certain parts of a country could provoke population shifts in anticipation of future windfalls. Resource revenue sharing rules that further concentrate the effects of discoveries in certain places will exacerbate migration effects. Large inflows of migrants may swamp local governments. Second, local governments enjoy fundamentally different policy options than do national governments (Agrawal et al., 2020). Local governments often face borrowing constraints, potentially reducing issues related to debt at the subnational level relative to the national level, where governments may seek to borrow against future resource wealth. On the other hand, local governments may potentially 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 typically 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.

I find no evidence of Dutch Disease (crowding out of manufacturing or tradeable activities) at the local level after major oil discovery announcements in Brazil. In fact, manufacturing employment and the number of manufacturing firms increase significantly in both satisfied and disappointed municipalities in years following major discovery announcements, while manufacturing wages remain unchanged. This increase in manufacturing may be due to direct linkages with the oil sector, which generates significant demand for manufactured inputs. In particular, Brazil imposed a local content requirement on the oil sector during this period, requiring oil companies to source inputs (including manufactures such as ships, pipes, pumps, and drilling rigs) from Brazilian suppliers (Rocha, 2018). At the local level, this may have counteracted the effects of real exchange rate appreciation or increases in input costs.

Moving to the country level, interpreting causal effects of the 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). 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 associated services) and reduced investment, exports, and growth in unrelated industrial sectors, thus leading to increased dependence on the oil sector and commodity exports. Supporting this simulation evidence, a review of industrial indicators by de Paula (2016) finds evidence of premature 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.

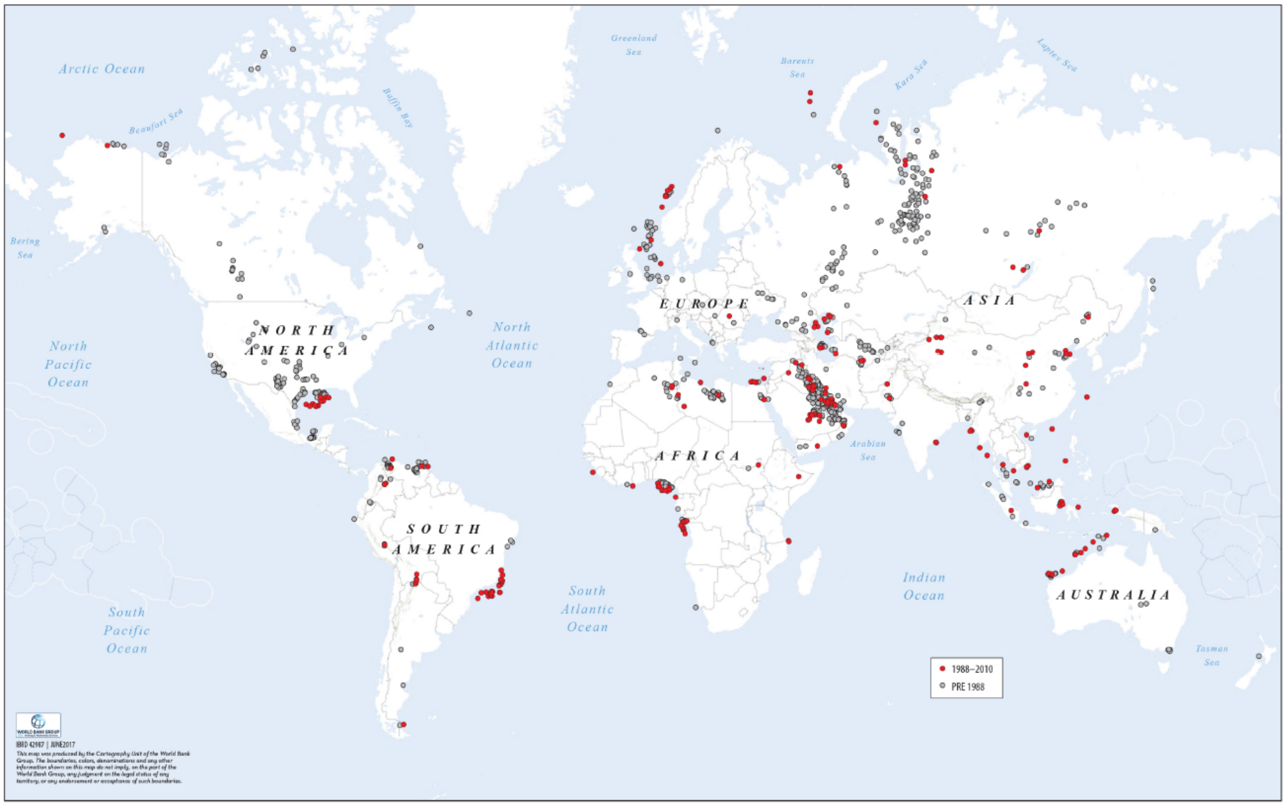
My study highlights a number of policy takeaways. First, revenue allocation rules that concentrate the positive and negative effects of resource discoveries into specific regions may amplify uncertainty and volatility after discoveries. For discovery-affected municipalities, anticipation of future revenue 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, the 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 out the 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, in the oil sector and more broadly. As mentioned previously, a fiscal responsibility law may have helped to avoid municipal fiscal excesses following discovery announcements. Specific to the oil sector, companies making discovery declarations to the Brazilian SEC 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 strong 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.

Appendices

Appendix A Giant Oil Discoveries Around the World

Figure A1: Giant Oil Discoveries Since 1988



Source: Cust and Mihalyi (2017)

Appendix B Oil Company Discovery Announcements

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
Total	**93.2**	**1725**	177

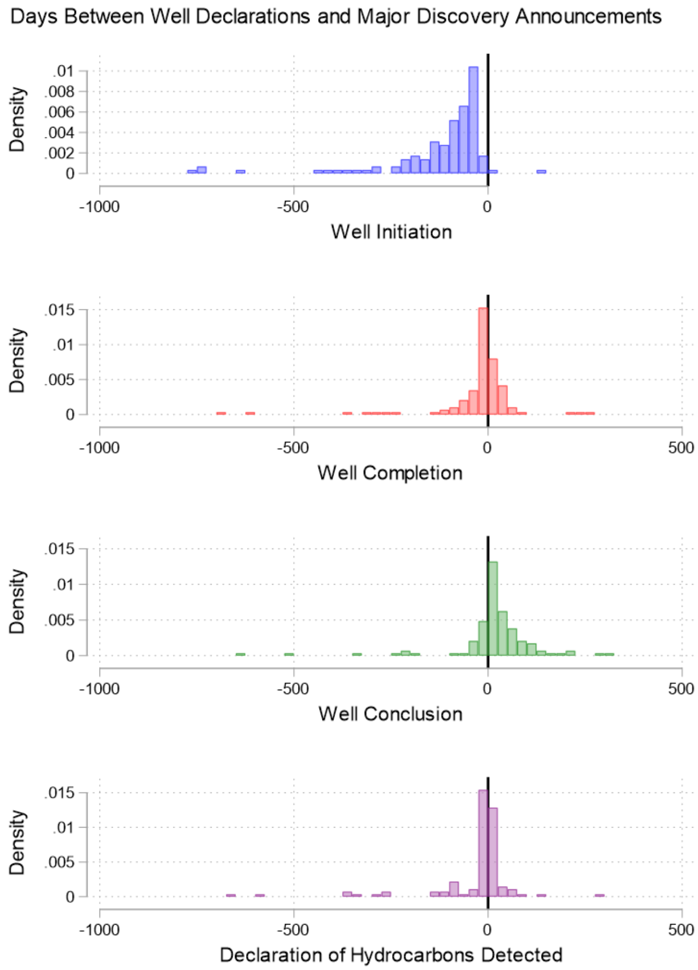
[1] Other operators checked: Anadarko, BP, Devon, Eni, Maha, OP Energia, Repsol Sinopec, Texaco, Vanco, Wintershall, ONGC, Esso, Amerada Hess, Unocal, SHB; no CVM Market Communications available

[2] ANP made 2 discovery announcements that were reported in media but not by companies

[3] 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.

Appendix C Timing of Well Stages Relative to CVM Announcements

Figure C1: Timing of Well Progress Designations Relative to CVM Discovery Announcement



Appendix D Discovery Announcement and News Coverage

Figure D1: 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/PETR4, NYSE: PBR/PBRA, Latibex: XPBR/XPBRA, 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 petróleo leve (cerca de 30º 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 [1 1] 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 petróleo leve (cerca de 30º API) em reservatórios localizados em profundidade de cerca de 5.000 metros, foi comunicada àAgência Nacional do Petróleo, Gás Natural e Biocombustíveis - ANP nesta 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.

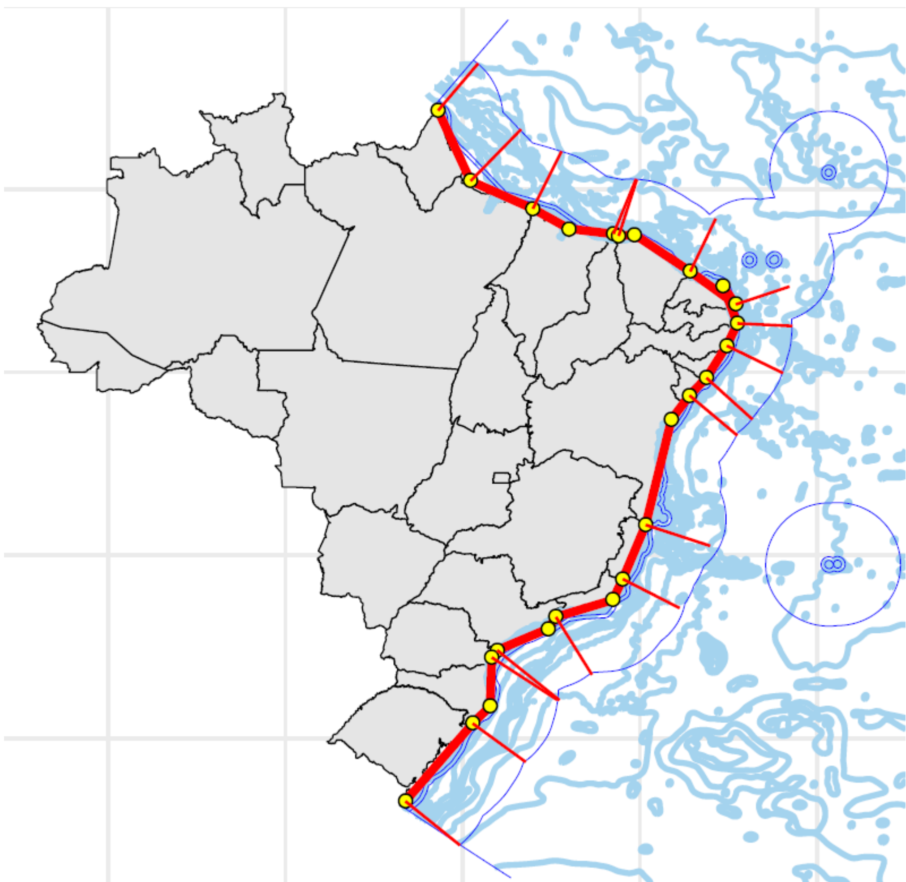
No dia 1º de maio, foi realizada a extração em alto mar do primeiro óleo do Campo de Tupi. O presidente Luiz Inácio Lula da Silva não foi àplataforma Cidade de São Vicente para a extração por questão de segurança, devido ao mau tempo na região, mas participou de uma cerimônia na Marina da Glória, na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

Leia também: Exploração no pré-sal começa sem nova regulação

Appendix E Reconstructing Geodesic Projection Maps

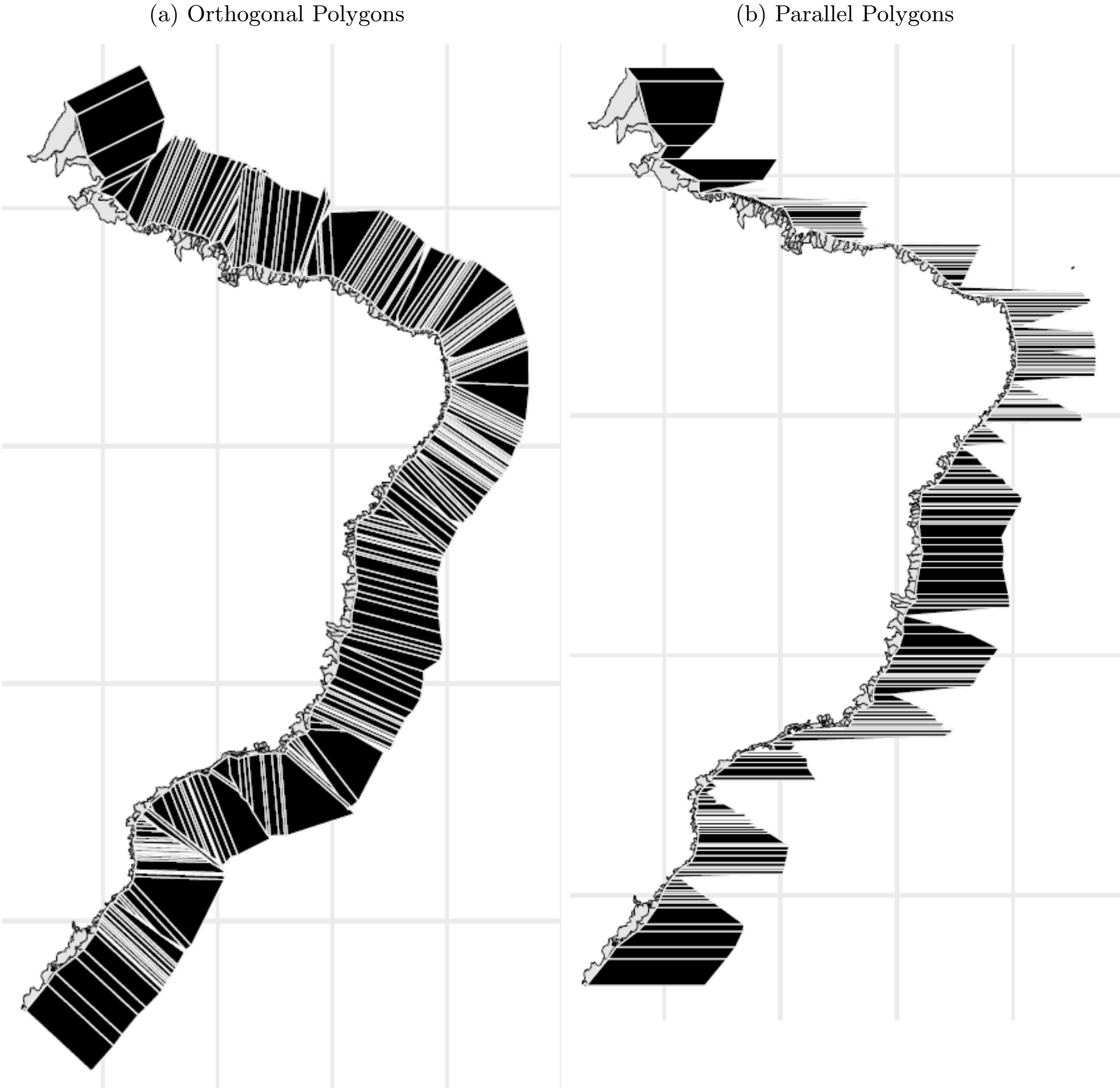
To reconstruct the geodesic projections used by IBGE and ANP to determine municipal offshore oil royalty distribution, I draw on documents from IBGE that define state boundary points and projections rules (IBGE, 2009). I begin by plotting state boundary points and state projections out to Brazil's maritime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

Figure E1: 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 E2: "Catchment Zones" (polygons) for Each Coastal Municipality



(a) Orthogonal Polygons

(b) Parallel Polygons

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

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

Appendix F Municipal Royalty Distribution Formula

The 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, as well as the Brazilian navy, by the National Oil Agency (ANP). Yearly royalties can be determined using the cumulative distribution 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 the 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:

$$W_{my} = \sum_{f} \Big[Alignment_{mfy} * (0.05)(P^{oil}_{fy} * V^{oil}_{fy} + P^{gas}_{fy} * V^{gas}_{fy}) * (0.3)\Big] \tag{10}$$

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^{oil}_{fy} and P^{oil}_{fy} are the reference prices for oil and gas, respectively, V^{oil}_{fy} and V^{gas}_{fy} 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:

$$Z_{my} = \sum_{f} \Big[Alignment_{mfy} * (Tax_{fy} - 0.05)(P^{oil}_{fy} * V^{oil}_{fy} + P^{gas}_{fy} * V^{gas}_{fy}) * (0.225)\Big] \tag{11}$$

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:

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

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}.

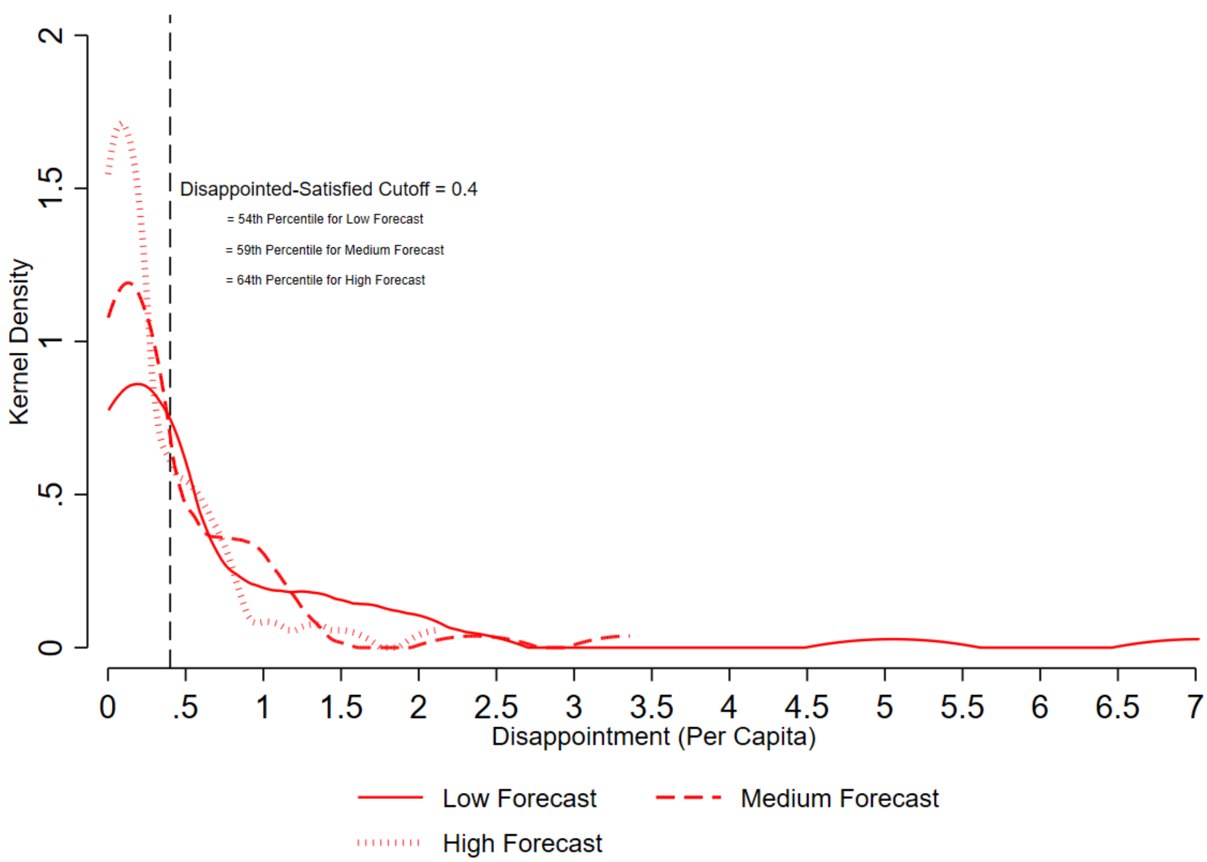
Appendix G Distribution of Forecast Errors

I compute $Disappointment_{mt}$ 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:

$$Disappointment_m = \frac{\frac{Royalties_{m,2017}}{Royalties_{m,t0}}}{\frac{E(Royalties_{m,2017}}{Royalties_{m,t0}}} \tag{13}$$

For the purpose of event studies, I classify municipalities as "disappointed" if $Disappointment_{mt}$ 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 $Disappointment_{mt}$ above 0.4 as "satisfied."

Figure G1: Distribution of Forecast Errors Across Treated Municipalities



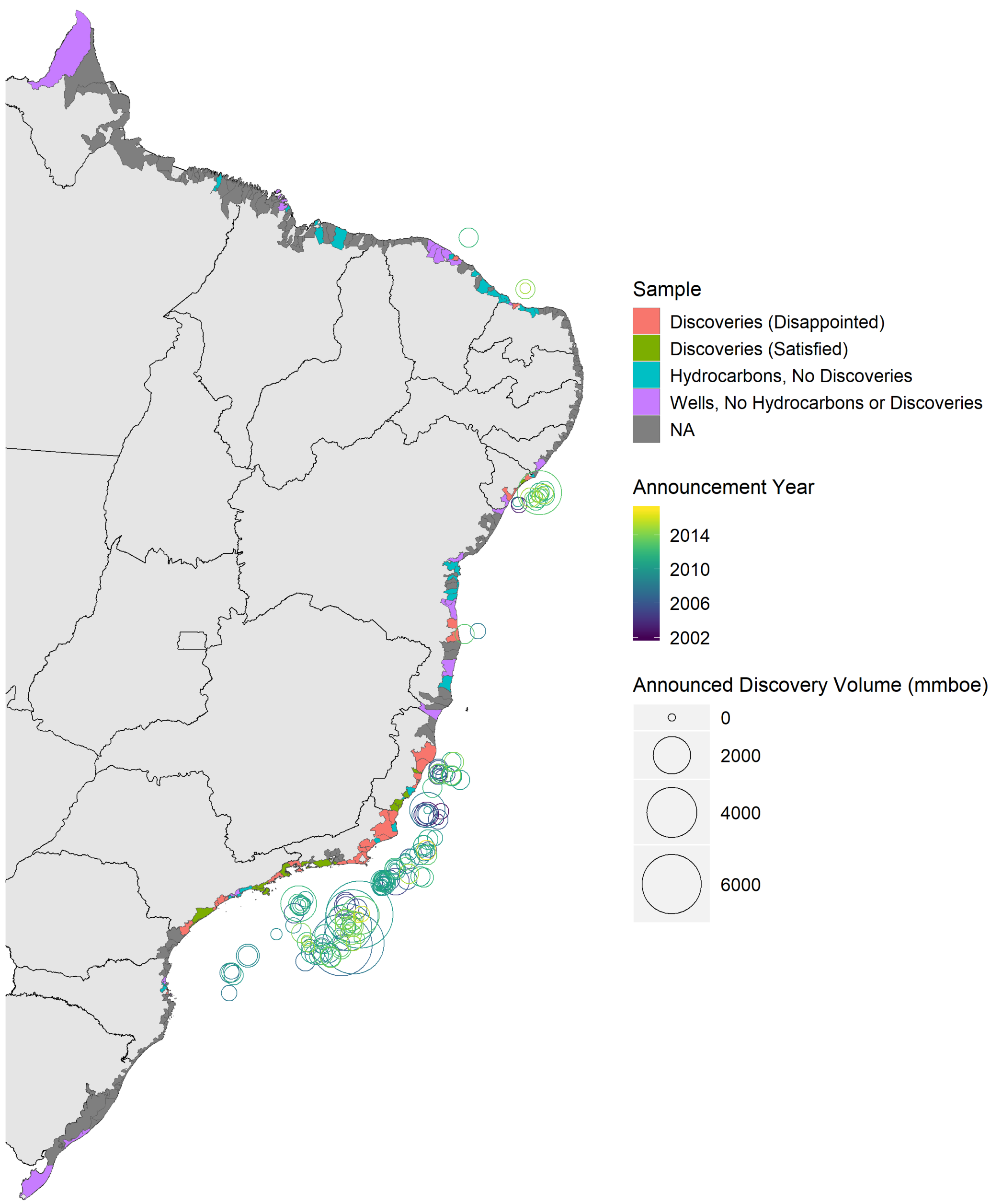
Appendix H Discovery-Affected Municipalities

Table H1: Disappointed/Satisfied Classifications Under Alternative Forecasting Specifications

	Outcome (per capita)			Outcome (total)		
Municipality	**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
SAOFRANCISCODEITABAPOANA33	D	D	D	D	D	D
SAQUAREMA33	D	D	D	D	D	D
SERRA32	D	D	D	D	D	D
UBATUBA35	D	D	D	D	D	D
UNA29	D	D	D	D	D	D
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	D	D	S	S	S
MANGARATIBA33	S	S	S	S	S	S
MARATAIZES32	S	S	D	S	S	D
MARICA33	S	D	D	S	S	D
NITEROI33	S	S	S	S	S	S
PARATI33	S	S	D	S	S	S
PIRAMBU28	S	S	S	S	S	S
PRESIDENTEKENNEDY32	S	S	S	S	S	S
RIODEJANEIRO33	S	S	S	S	S	S
SAOSEBASTIAO35	S	S	S	S	S	S
VITORIA32	S	D	D	S	D	D
Total Disappointed	**30**	**33**	**35**	**28**	**30**	**33**
Total Satisfied	**18**	**15**	**13**	**20**	**18**	**15**
Percent Disappointed	**62.5**	**68.8**	**72.9**	**58.3**	**62.5**	**68.8**

Appendix I Full Brazilian Coastline: Major Offshore Discoveries and Affected Municipalities

Figure I1: Brazil: Major Offshore Discoveries and Affected Municipalities



Appendix J Data Sources and Processing

In this section I explain data sources and preparation for key outcome variables.

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 Reserach (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.

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 in each municipal government in each year. 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 Colonnelli 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 municipalities' geographical characteristics from IPEA.

Appendix K 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 K1: Event Study with Multiple Events: Public Finances

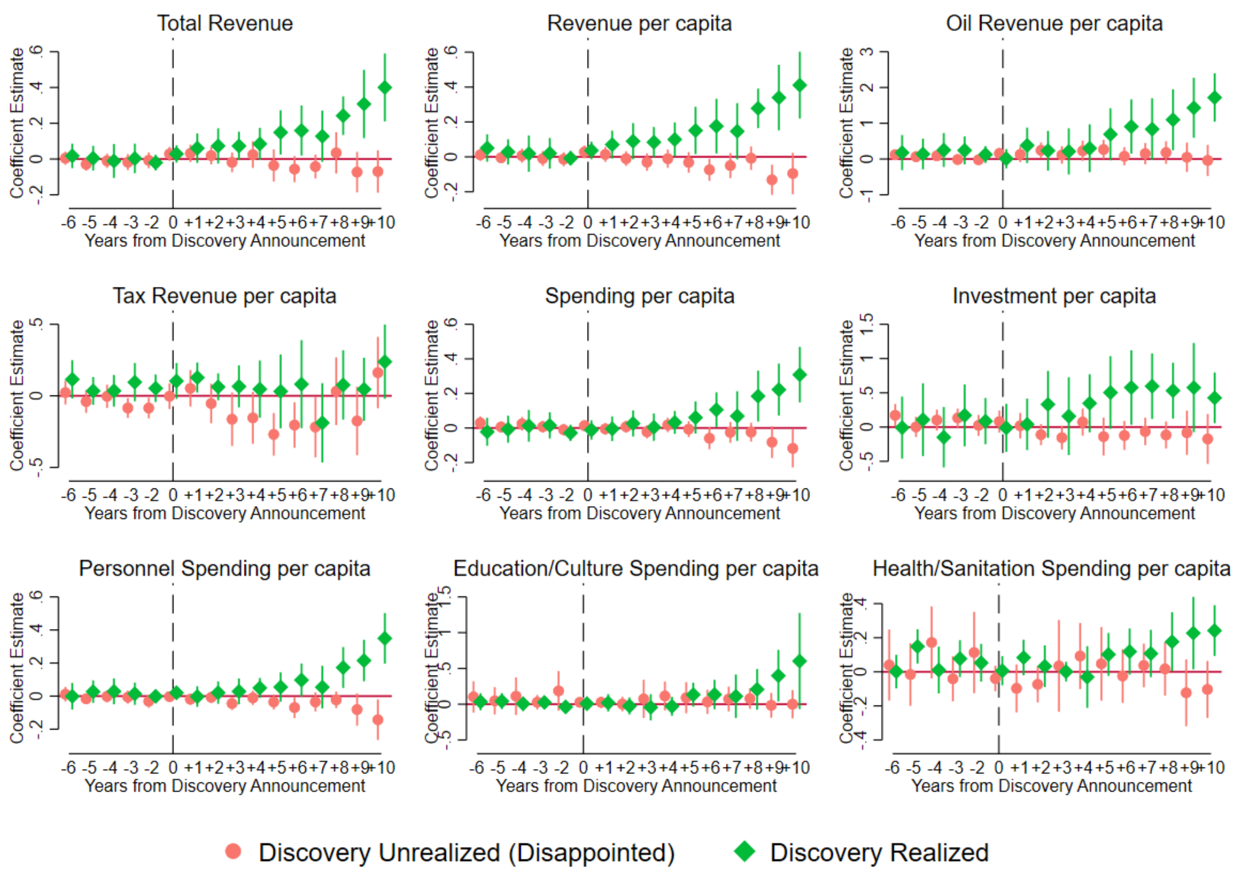


Figure K2: Event Study with Multiple Events: Other Outcomes

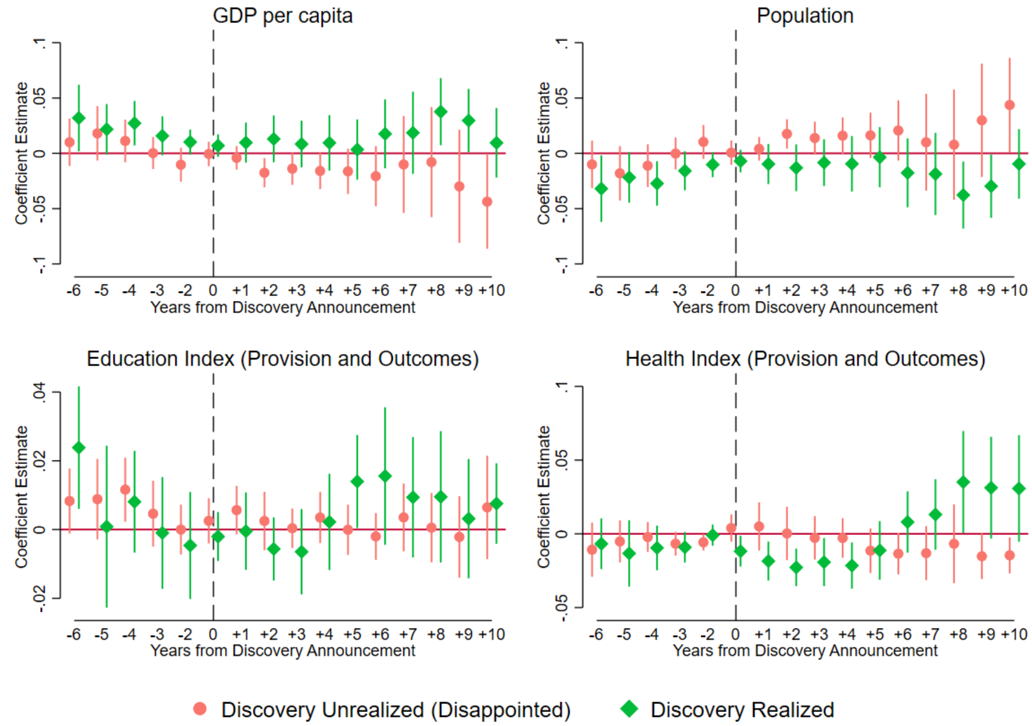
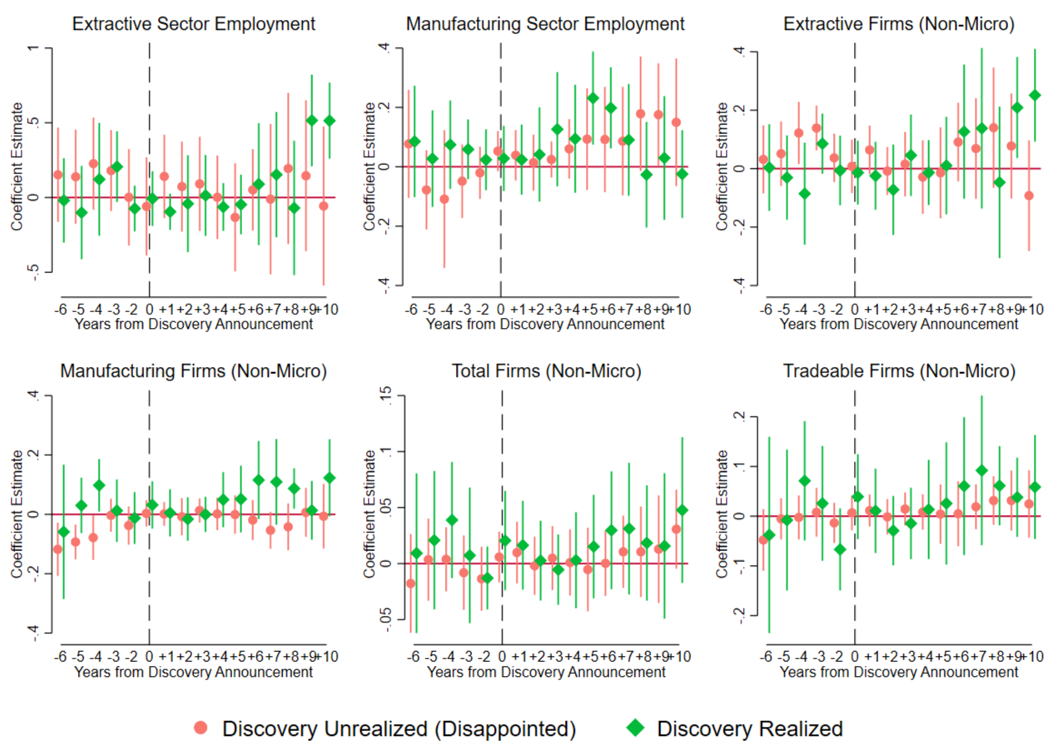


Figure K3: Event Study with Multiple Events: Private Sector Outcomes



Appendix L Assessing Pre-Trends

Figure L1: Sample Means: Treated Municipalities and Never-Treated Controls (Municipalities with Post-2000 Exploratory Wells but No Discoveries (n=53))

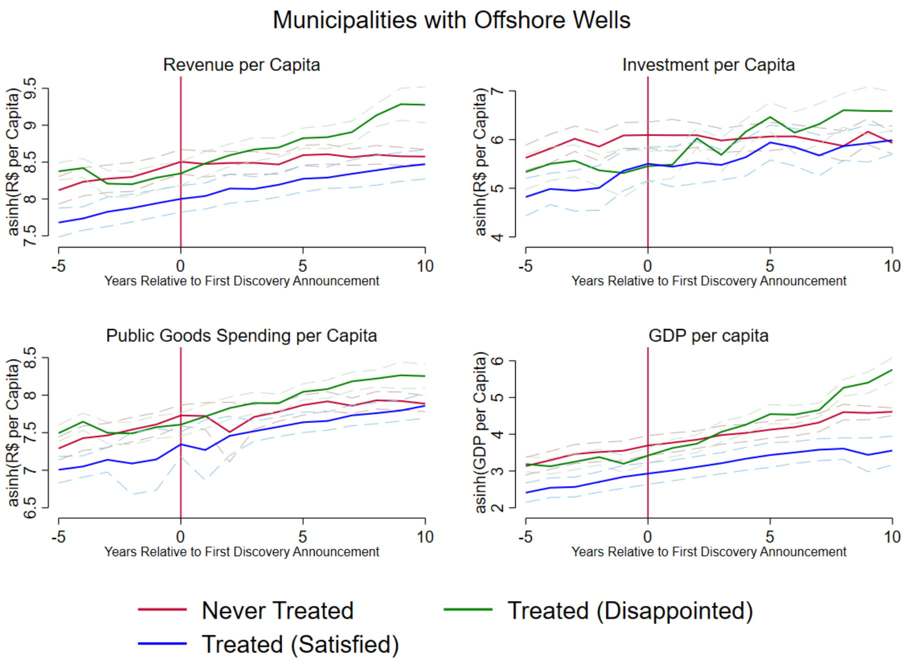


Figure L2: Sample Means: Treated Municipalities and Never-Treated Controls (Coarsened Exact Matching, Separately for Disappointed (n=836) and Satisfied (n=500))

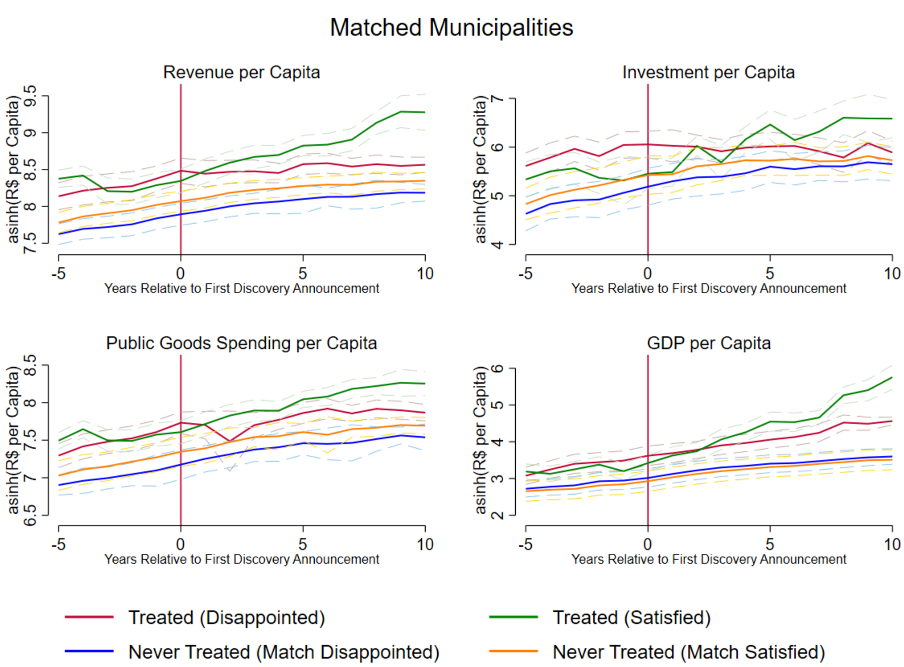
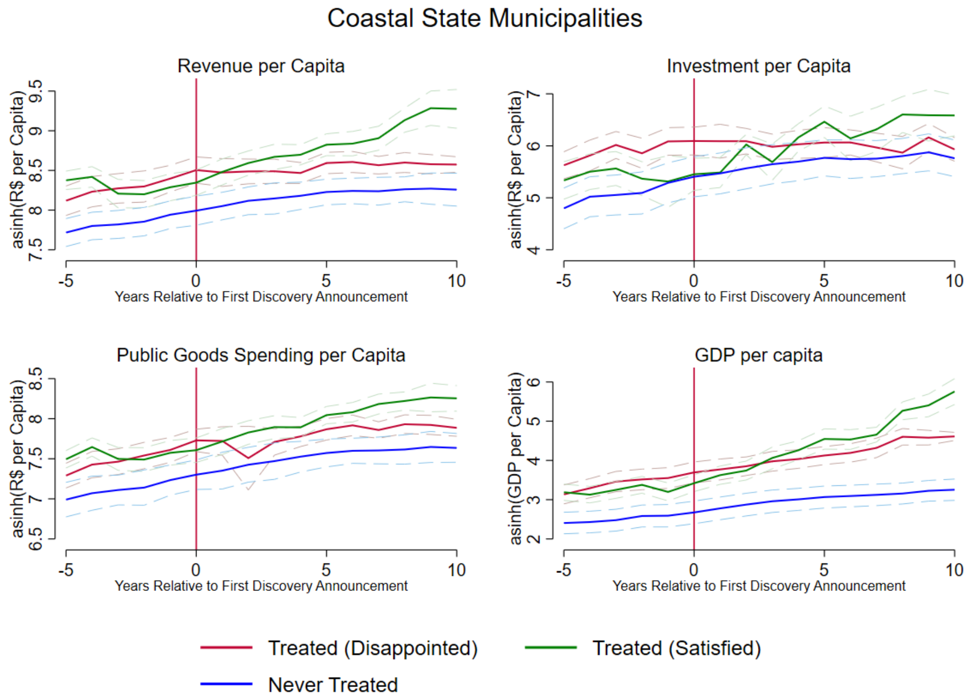
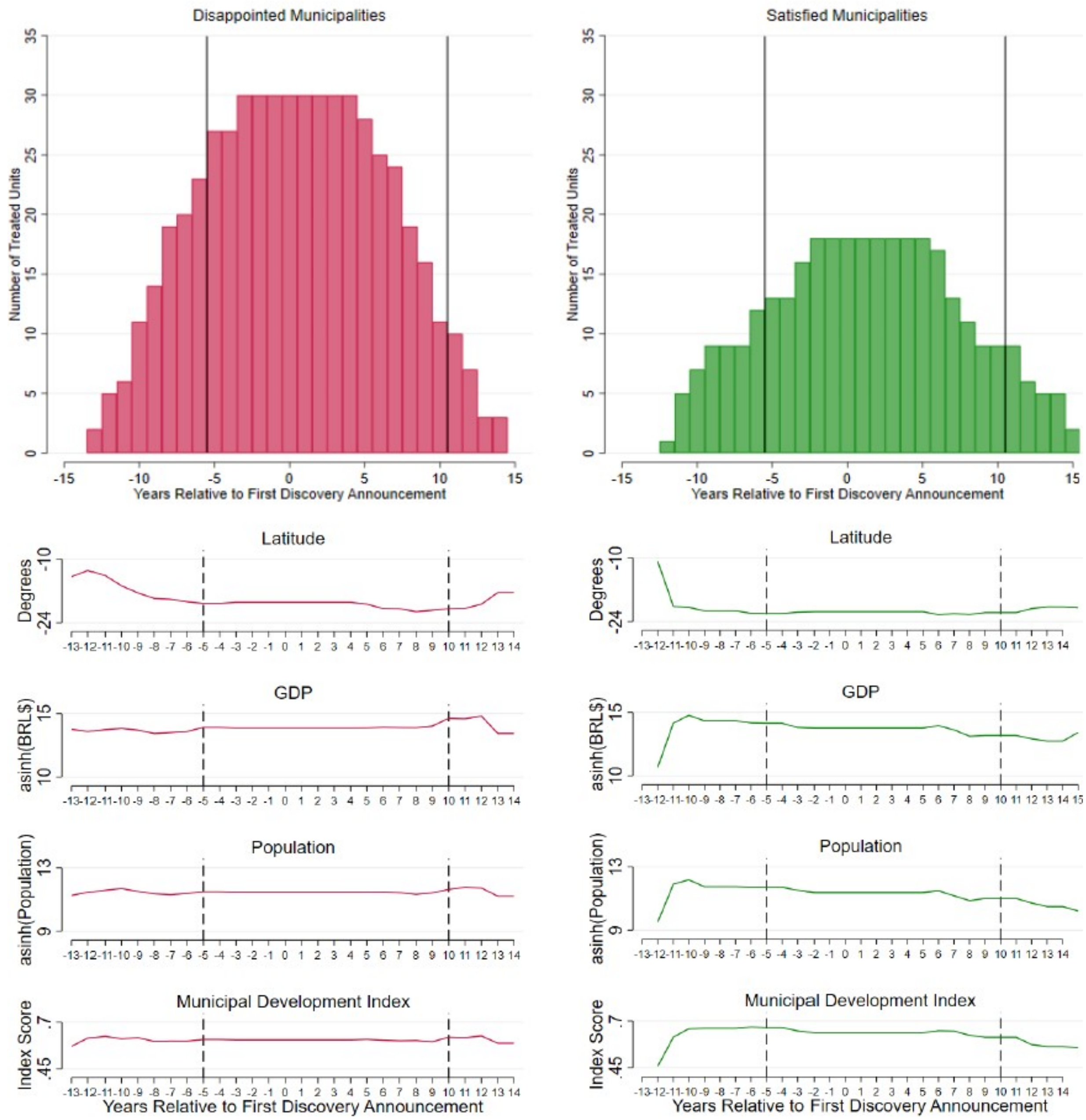


Figure L3: Sample Means: Treated Municipalities and Never-Treated Controls (Municipalities in Coastal States, n=3,902)



Appendix M Panel Balance Across Relative Time Indicators

Figure M1: Treated Unit Balance Across Relative Time Indicators

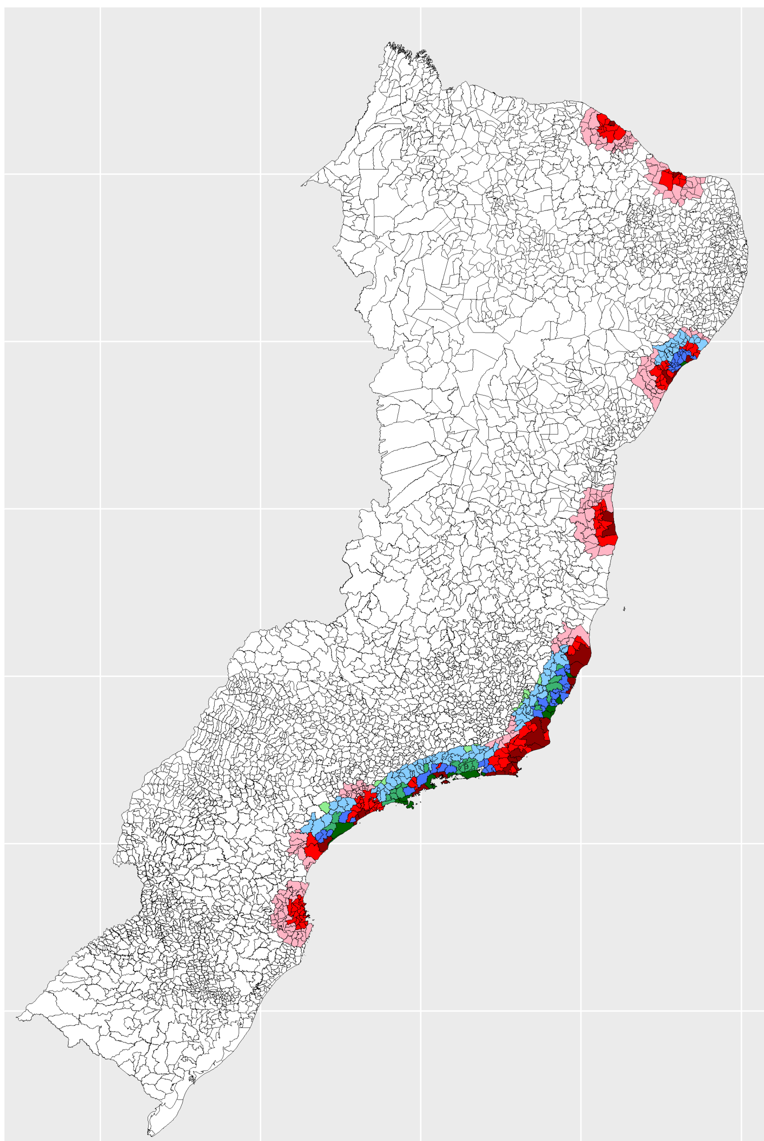


Note: Bar graphs depict number of treated units in each relative time period, where t=0 represents the year of the first major discovery announcement for a municipality. Vertical black lines indicate the extent of periods included in the analysis. Given the limited time-frame in sample (2000-2017), the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

Appendix N 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.

Figure N1: 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 N2: Public Finance Outcomes in Near (0-50km) vs Far (50-100km) Municipalities

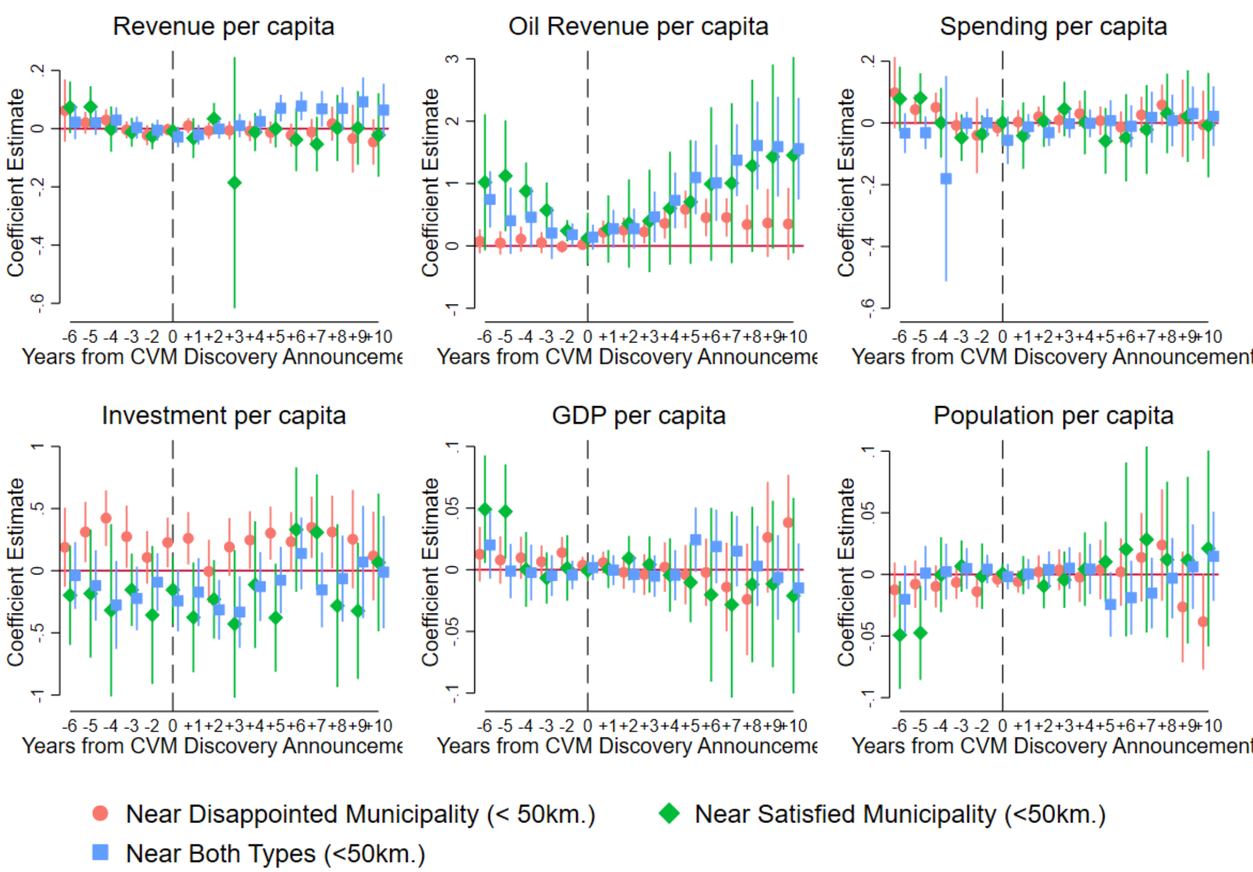


Figure N3: Sectoral Employment in Near (0-50km) vs Far (50-100km) Municipalities

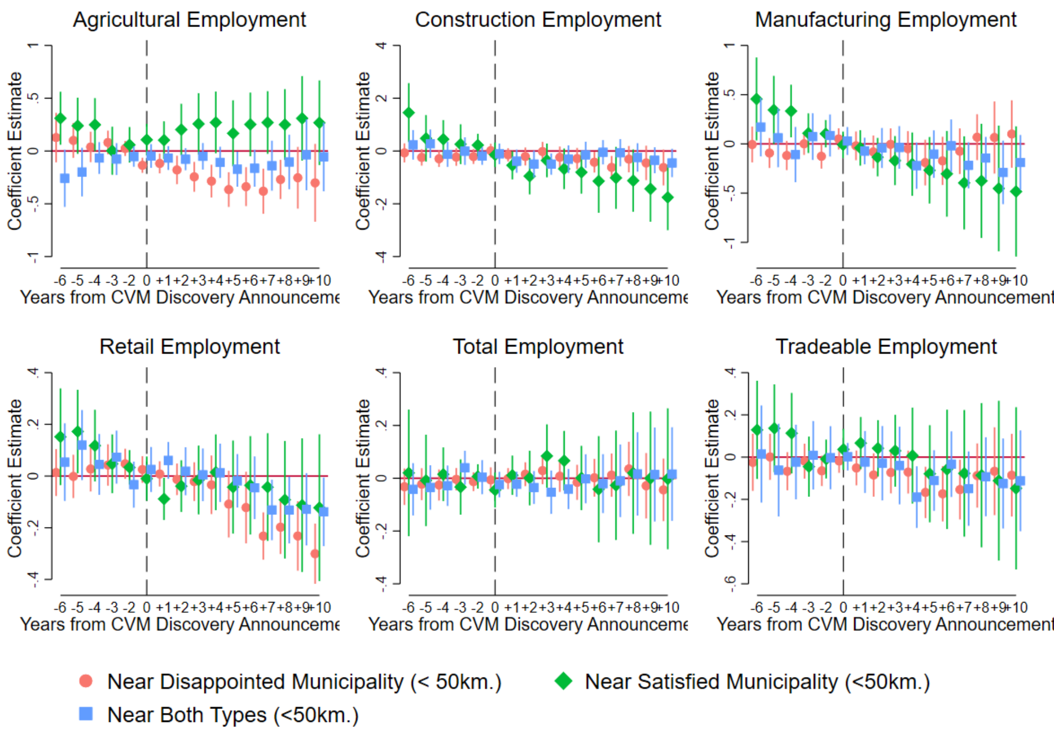
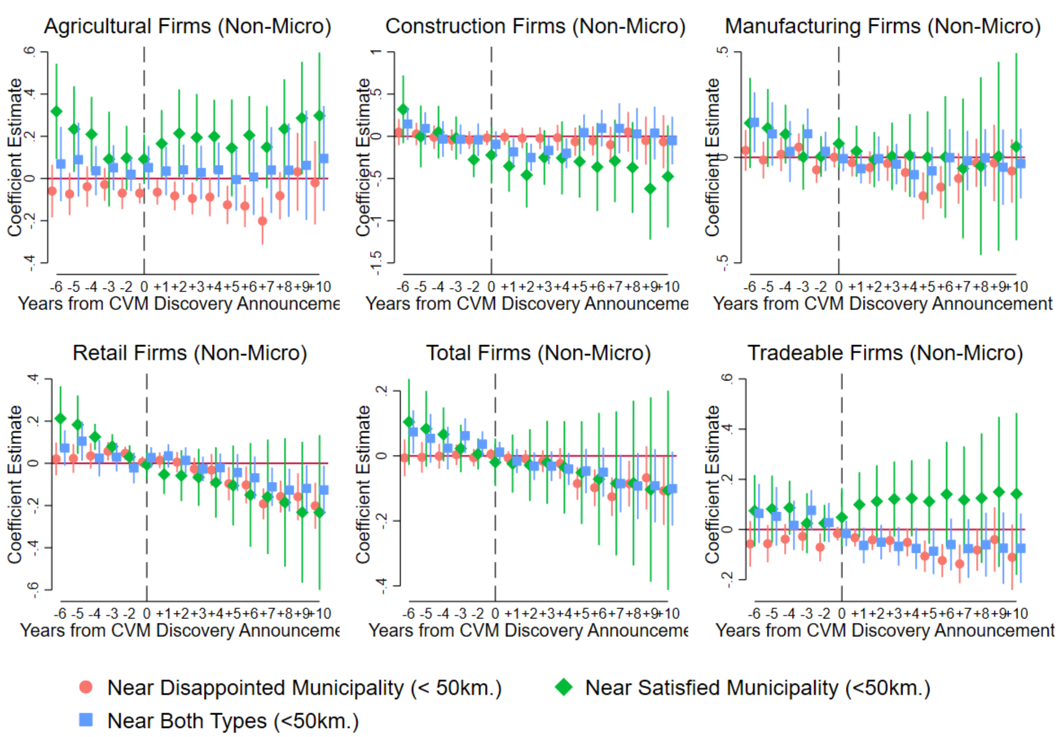


Figure N4: Firm Entry in Near (0-50km) vs Far (50-100km) Municipalities



Appendix O Results and Sample Characteristics

Table O1: Disappointed: Sample Properties, Coefficient Estimates, Semi-Elasticity Estimates

	Sample Properties			Coefficients			Small-n Bias Correct. Elast.		
Outcomes	$\overline{X}$	**n**	**Units**	**1 Year**	**5 Years**	**10 Years**	**1 Year**	**5 Years**	**10 Years**
Total Revenue (Millions)	162	1,392	83	0.00	-0.04	-0.20**	-0.64	-6.28	-20.79***
				(0.02)	(0.04)	(0.08)	(2.12)	(3.92)	(6.02)
Revenue p.c.	2,086	1,392	83	-0.01	-0.10	-0.26**	-2.13	-11.86**	-26.69***
				(0.02)	(0.06)	(0.11)	(2.14)	(5.44)	(8.02)
Tax Revenue p.c.	220	1,392	83	0.14*	-0.23	-0.35	10.93	-27.00**	-37.30***
				(0.08)	(0.17)	(0.23)	(8.75)	(12.09)	(14.28)
Oil Revenue p.c.	473	1,494	83	0.27	0.34	0.16	19.75	20.33	-5.57
				(0.17)	(0.31)	(0.43)	(20.87)	(37.60)	(40.41)
Non-Oil Transfer Rev. p.c.	652	1,440	80	-0.03**	-0.05**	-0.07*	-3.69***	-6.60***	-8.99**
				(0.01)	(0.03)	(0.04)	(1.41)	(2.53)	(3.94)
Total Spending (Millions)	88	1,392	83	-0.01	-0.05	-0.17**	-2.04	-7.12*	-18.77***
				(0.03)	(0.04)	(0.07)	(2.53)	(3.82)	(5.81)
Spending p.c.	1,165	1,392	83	-0.02	-0.10*	-0.23***	-3.45	-12.43**	-23.95***
				(0.03)	(0.06)	(0.08)	(2.42)	(4.89)	(6.23)
Investment p.c.	226	1,423	83	-0.17	-0.46**	-0.70**	-21.69*	-43.14***	-56.92***
				(0.15)	(0.21)	(0.28)	(12.09)	(12.11)	(12.18)
Personnel Spending p.c.	933	1,392	83	-0.04*	-0.14**	-0.26***	-4.92**	-15.64***	-26.42***
				(0.02)	(0.06)	(0.09)	(2.08)	(4.73)	(6.30)
Education Spending p.c.	571	1,392	83	-0.01	-0.14**	-0.25**	-2.93	-15.78***	-25.64***
				(0.04)	(0.06)	(0.10)	(3.87)	(5.47)	(7.26)
Health Spending p.c.	449	1,392	83	-0.09	-0.17**	-0.24*	-12.76*	-18.62***	-26.23***
				(0.08)	(0.08)	(0.12)	(7.38)	(6.47)	(9.10)
GDP per capita	22,362	1,162	83	0.00	-0.04	-0.12	-1.86	-8.14	-18.27
				(0.04)	(0.08)	(0.17)	(4.30)	(7.71)	(13.66)
Population	80,980	1,494	83	0.01	0.04	0.05	-0.09	2.16	0.87
				(0.01)	(0.04)	(0.08)	(1.34)	(3.99)	(7.66)
No. Empl. Extractive	213	1,494	83	-0.01	-0.04	0.24	-14.62	-19.94	-1.73
				(0.30)	(0.37)	(0.52)	(25.77)	(29.41)	(51.17)
No. Empl. Mfg.	2,821	1,494	83	0.02	-0.09	-0.18	-0.68	-15.05	-25.17
				(0.06)	(0.15)	(0.21)	(5.83)	(12.83)	(15.93)
No. Firms Extractive	9.1	1,494	83	-0.12*	-0.28**	-0.20	-14.73**	-29.30***	-26.79*
				(0.07)	(0.13)	(0.22)	(6.14)	(9.32)	(15.92)
No. Firms Mfg.	165.2	1,494	83	-0.01	0.01	-0.19*	-2.38	-2.85	-21.26***
				(0.03)	(0.07)	(0.10)	(3.26)	(7.02)	(7.69)
Avg. Formal Wage (Monthly)	1,034	1,494	83	-0.01	-0.08**	-0.11**	-2.13	-8.88***	-12.42***
				(0.02)	(0.03)	(0.05)	(1.767)	(3.036)	(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 FEs; 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 (Bellemare and Wichman, 2020). *** $p<0.01$, ** $p<0.05$, * $p<0.1$

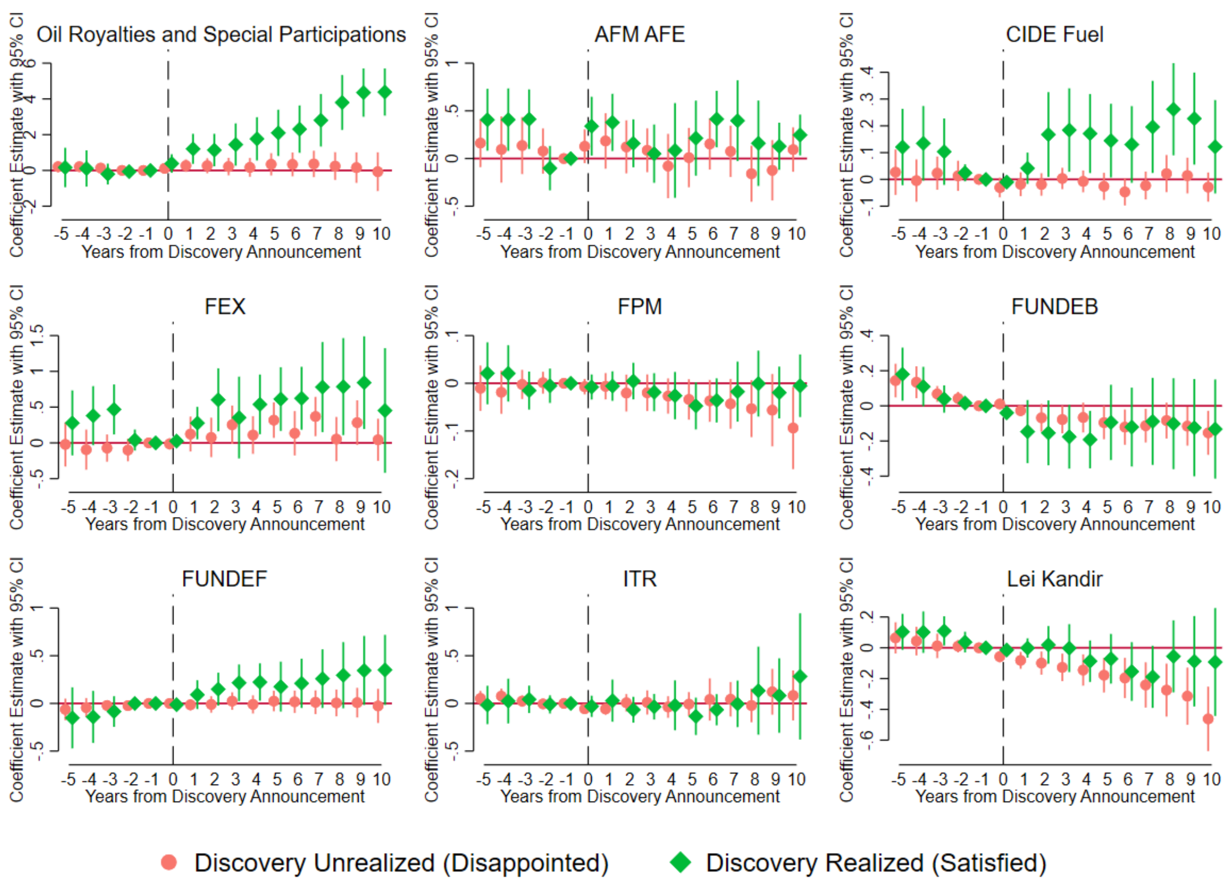
Table O2: Satisfied: Sample Properties, Coefficient Estimates, Semi-Elasticity Estimates

	Sample Properties			Coefficients			Small-n Bias Correct. Elast.		
Outcomes	$\overline{X}$	**n**	**Units**	**1 Year**	**5 Years**	**10 Years**	**1 Year**	**5 Years**	**10 Years**
Total Revenue (Millions)	345	1,211	71	0.05	0.16*	0.65***	3.01	11.74	74.53**
				(0.04)	(0.09)	(0.20)	(4.62)	(10.43)	(34.21)
Revenue p.c.	2,361	1,211	71	0.05	0.16	0.66***	2.74	11.69	75.12**
				(0.04)	(0.10)	(0.20)	(4.54)	(10.91)	(34.60)
Tax Revenue p.c.	279	1,211	71	0.01	-0.06	-0.21	-3.23	-15.98	-30.32
				(0.09)	(0.23)	(0.30)	(8.68)	(19.50)	(20.58)
Oil Revenue p.c.	606	1,278	71	1.21***	2.10***	4.35***	170.90	490.53	5441.63
				(0.42)	(0.65)	(0.68)	(114.05)	(383.00)	(3755.01)
Non-Oil Transfer Rev. p.c.	691	1,224	68	-0.03	-0.01	0.04	-3.63**	-2.59	1.26
				(0.02)	(0.04)	(0.05)	(1.72)	(4.10)	(5.12)
Total Spending (Millions)	206	1,211	71	-0.07	0.01**	0.24**	-8.77	-2.31	20.35
				(0.05)	(0.07)	(0.12)	(4.20)	(6.52)	(13.88)
Spending p.c.	1,264	1,211	71	-0.07	0.01**	0.25**	-9.00	-2.15	20.79
				(0.05)	(0.07)	(0.12)	(4.12)	(6.55)	(14.07)
Investment p.c.	263	1,230	71	-0.06	0.34	0.82	-21.73	11.98	59.35
				(0.37)	(0.46)	(0.71)	(29.22)	(51.91)	(113.07)
Personnel Spending p.c.	997	1,211	71	-0.04	0.01*	0.19*	-5.86	-2.56	14.32
				(0.03)	(0.07)	(0.11)	(3.23)	(6.87)	(12.83)
Education Spending p.c.	627	1,208	71	-0.01	0.03*	0.35	-4.62	0.07	28.02
				(0.07)	(0.07)	(0.20)	(6.40)	(6.61)	(26.10)
Health Spending p.c.	461	1,208	71	0.20**	0.11	0.34	17.62*	6.88	25.42
				(0.08)	(0.09)	(0.23)	(9.90)	(9.61)	(29.05)
GDP per capita	27,043	994	71	0.06	0.57**	1.42***	2.56	55.00	253.10**
				(0.08)	(0.27)	(0.31)	(7.75)	(42.12)	(110.29)
Population	155,964	1,278	71	0.00	-0.01	-0.01	-0.40	-1.63	-3.11
				(0.01)	(0.02)	(0.05)	(0.89)	(2.17)	(4.49)
No. Empl. Extractive	1,258	1,278	71	-0.12	0.13	0.62**	-15.96*	-1.57	60.08
				(0.11)	(0.29)	(0.29)	(9.42)	(28.49)	(46.73)
No. Empl. Mfg.	5,453	1,278	71	-0.11	0.01	-0.21	-14.98	-6.35	-27.45*
				(0.11)	(0.15)	(0.22)	(9.61)	(14.06)	(15.63)
No. Firms Extractive	17.5	1,278	71	0.09	0.07	0.30	2.59	-1.89	18.90
				(0.12)	(0.17)	(0.26)	(12.29)	(16.78)	(31.23)
No. Firms Mfg.	273.8	1,278	71	-0.09*	-0.12**	-0.23**	-10.76**	-13.70***	-25.03***
				(0.05)	(0.06)	(0.11)	(4.29)	(4.76)	(8.05)
Avg. Formal Wage	1,073	1,278	71	-0.03	-0.01*	-0.09**	-4.17	-3.84	-11.06**
				(0.02)	(0.05)	(0.05)	(1.94)	(4.72)	(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 FEs; 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 (Bellemare and Wichman, 2020). *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix P Event Studies: Effects on State/Federal Transfers

Figure P1: Federal and State Transfers in Disappointed and Satisfied Municipalities Relative to Matched Controls



Appendix Q In-Migration (2000-2010)

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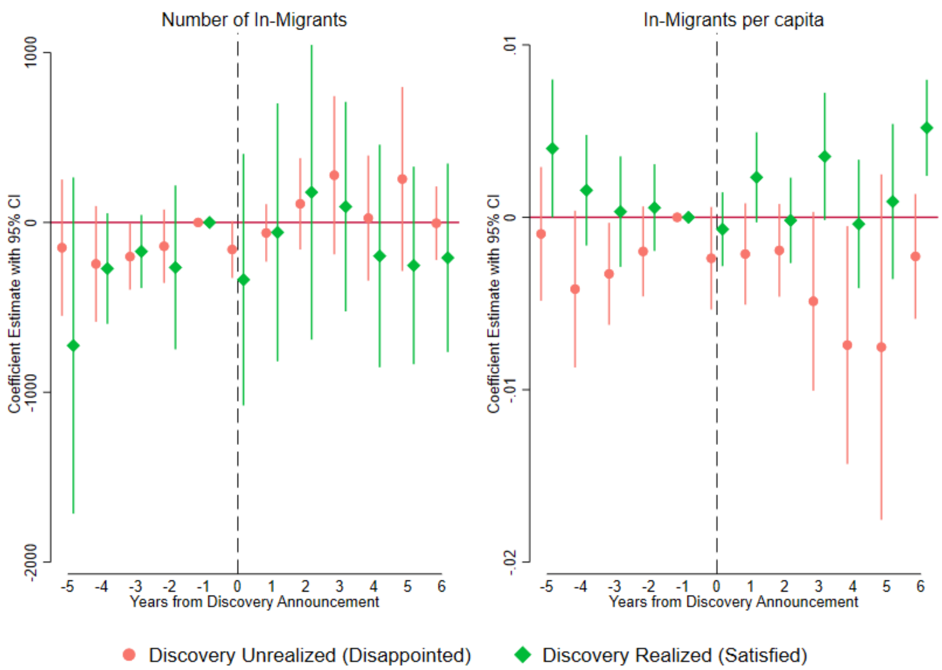
Figure Q2: In-Migration After Discovery Announcements



Appendix R Event Studies: Effects on Employment and Wages

Figure R1: Sectoral Empl. in Disappointed & Satisfied Municipalities Relative to Matched Controls

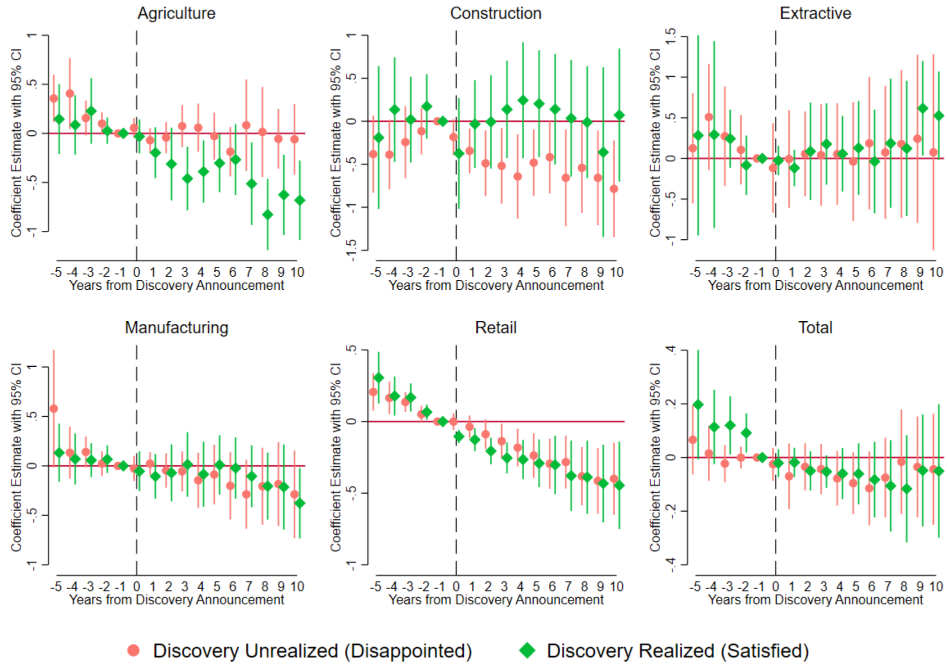
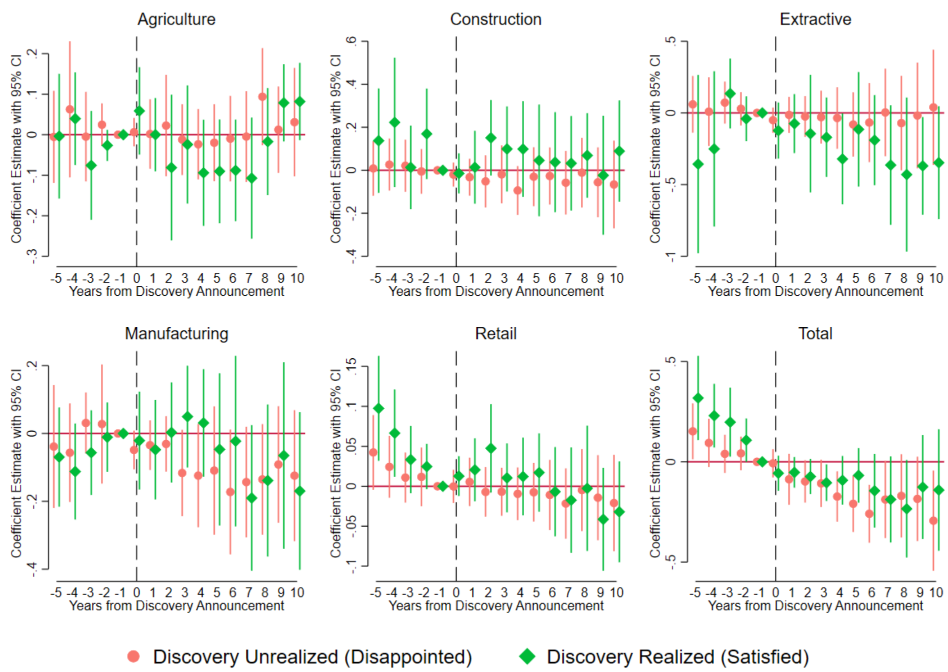


Figure R2: Sectoral Wages in Disappointed and Satisfied Municipalities Relative to Matched Controls



Appendix S Private Sector Outcomes with Pre-Matched Controls

Figure S1: Number of Formally Registered Firms

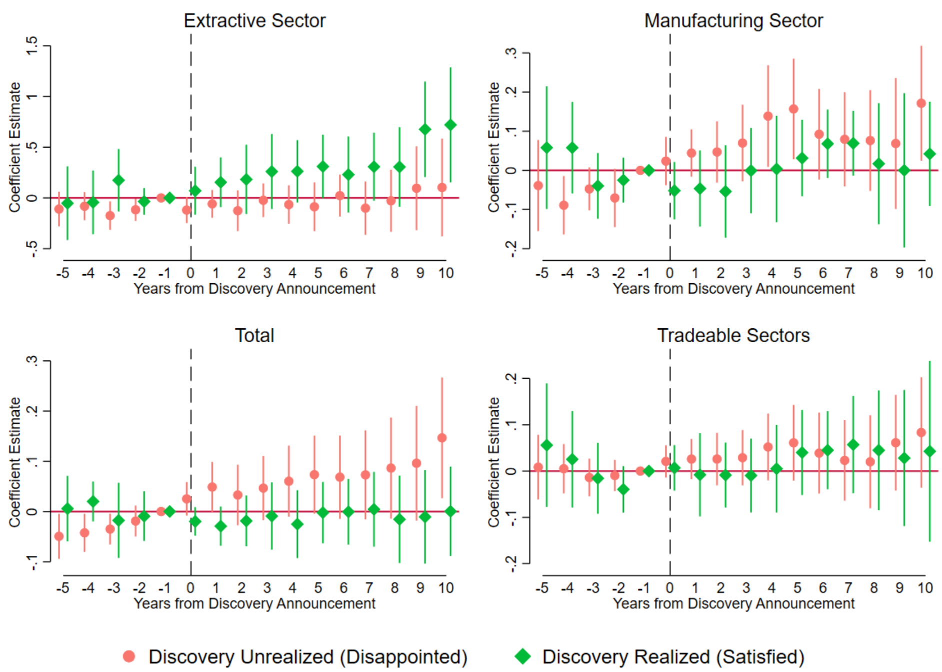
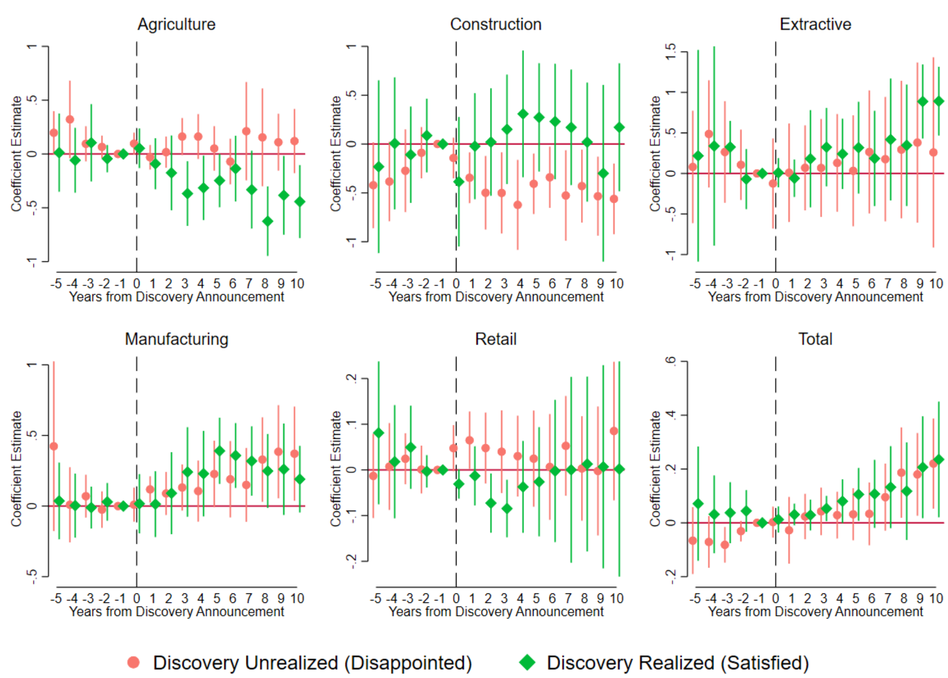


Figure S2: Formal Employment



Appendix T Callaway and Sant'Anna (2020) Event Study Estimates

Figure T1: CS Estimator: Total Revenue and Oil Revenue per capita

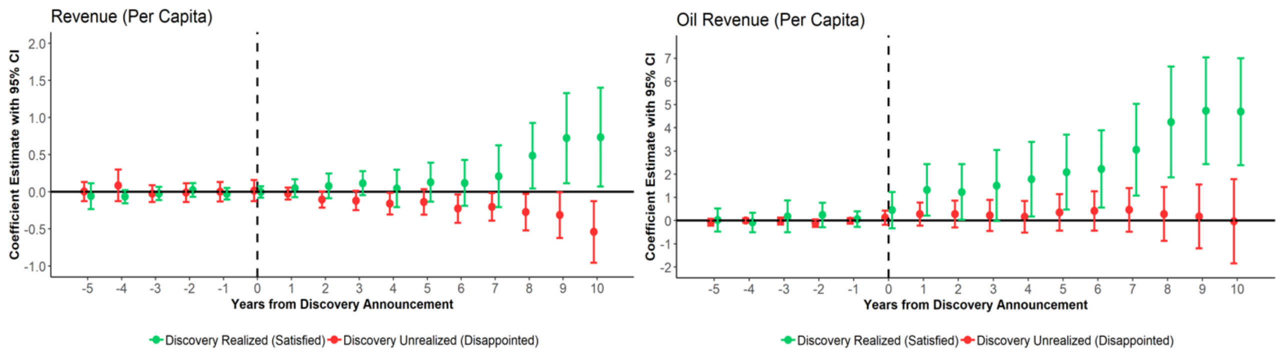


Figure T2: CS Estimator: Spending and Investment per capita

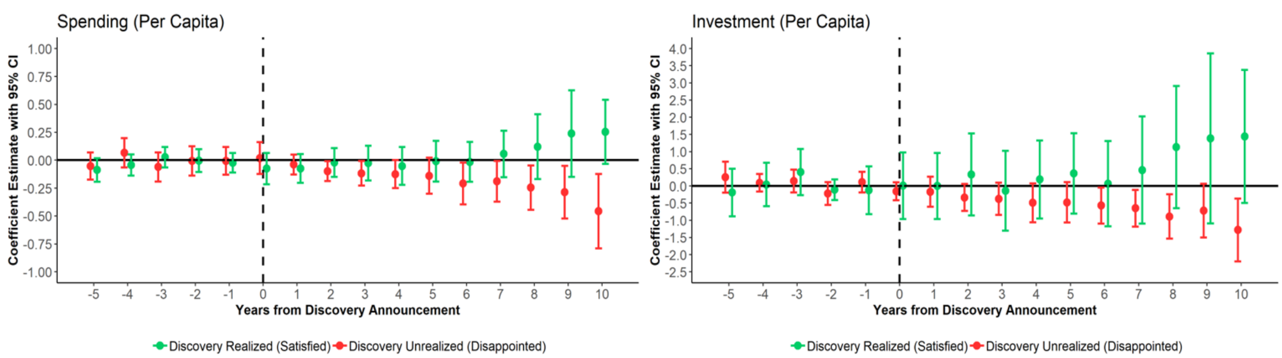


Figure T3: CS Estimator: Education and Health Spending per capita

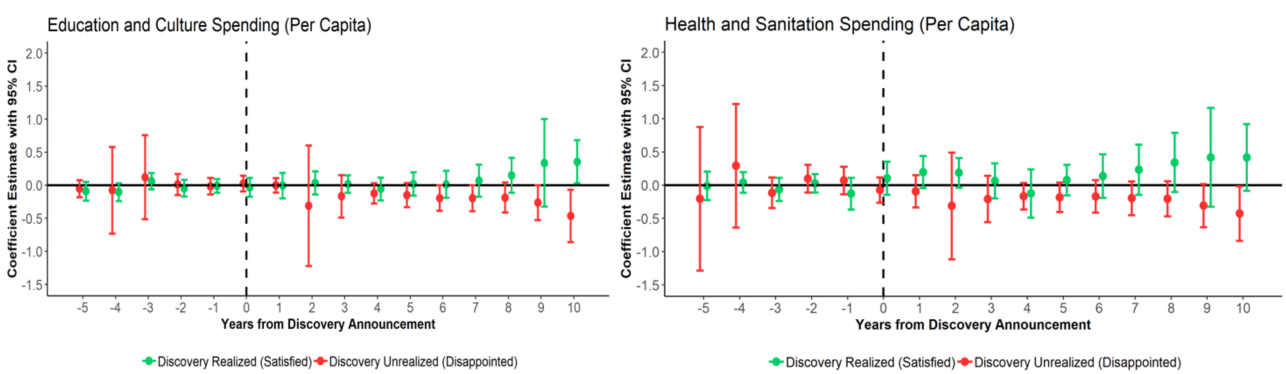


Figure T4: CS Estimator: Public Goods Provision and Quality

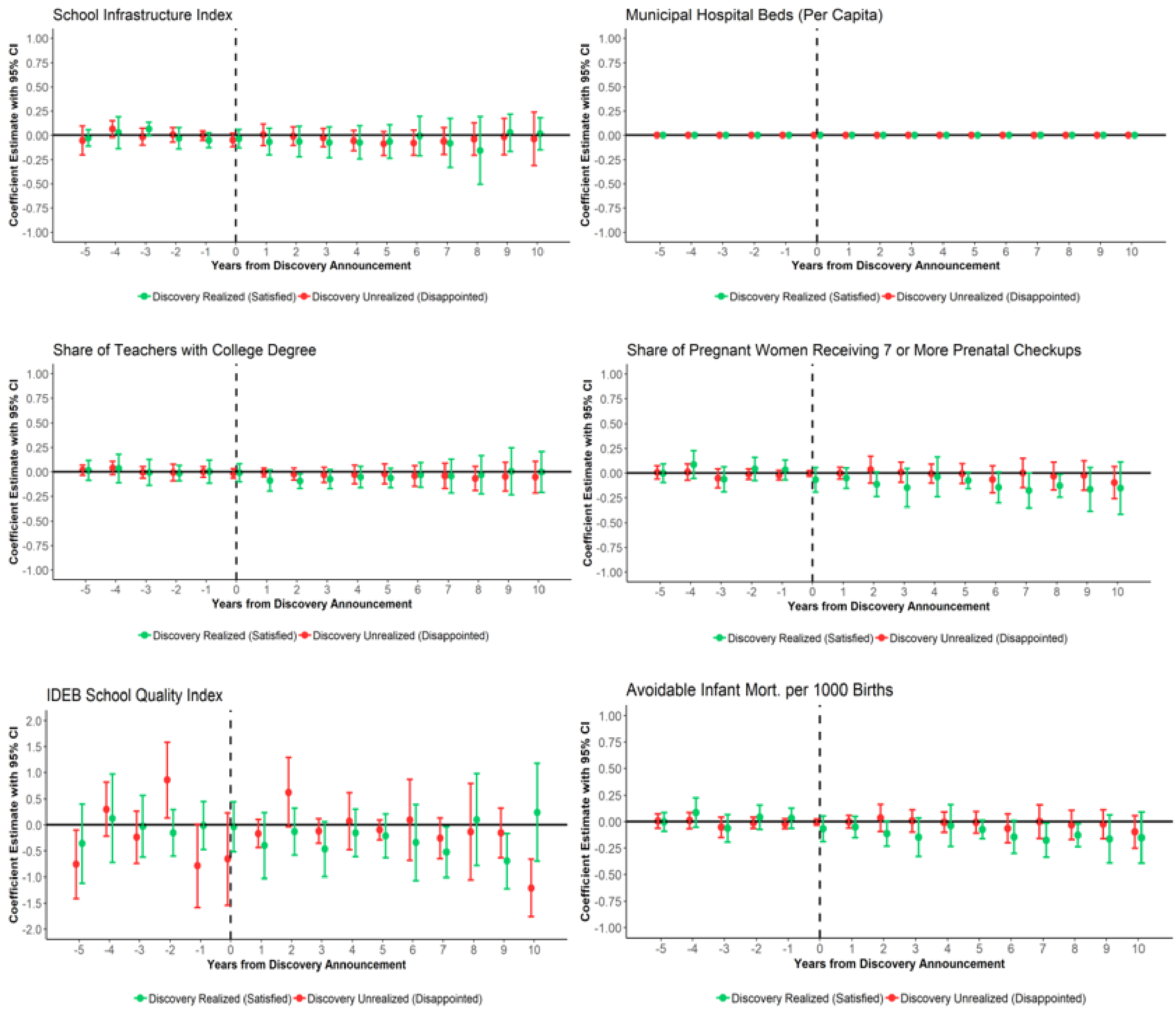
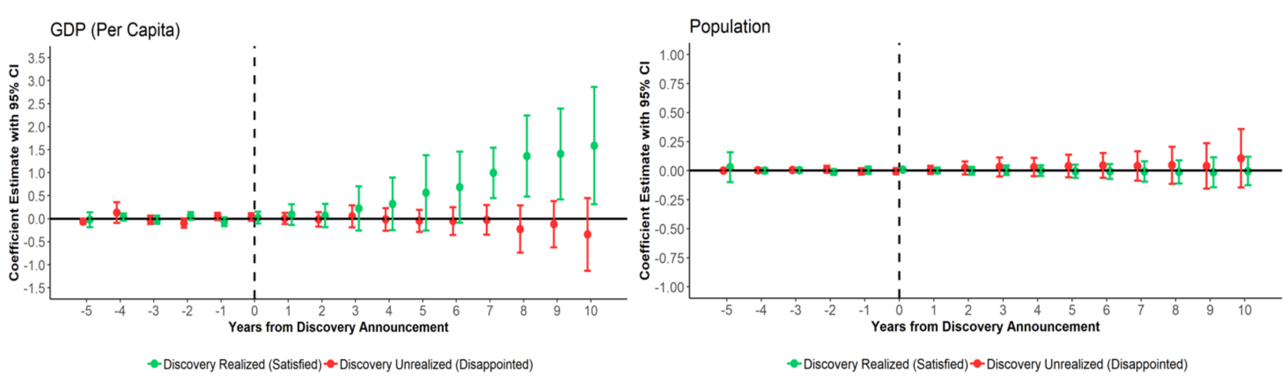


Figure T5: CS Estimator: GDP per capita and Population



Appendix U Robustness to Alternative Forecasting and Matching Parameters

Figure U1: Robustness: Revenues

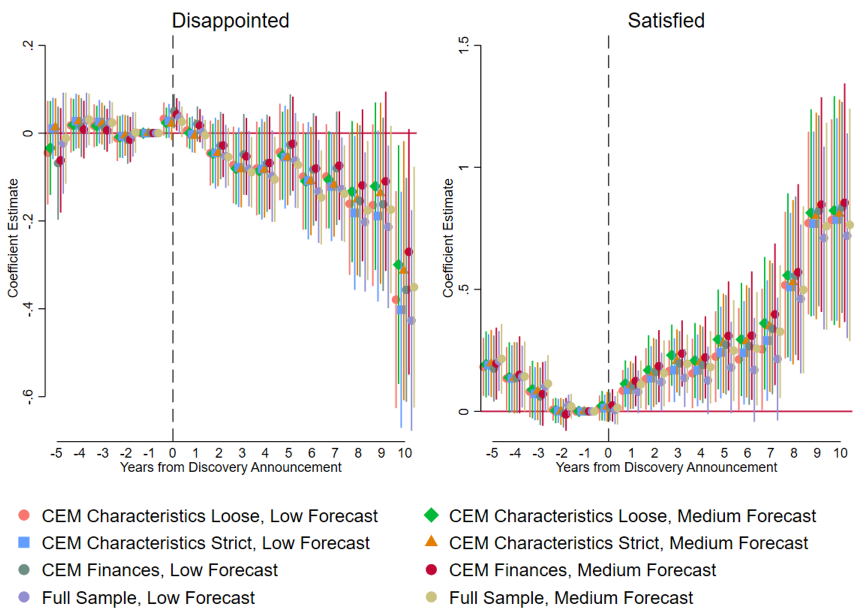


Figure U2: Robustness: Oil Revenues

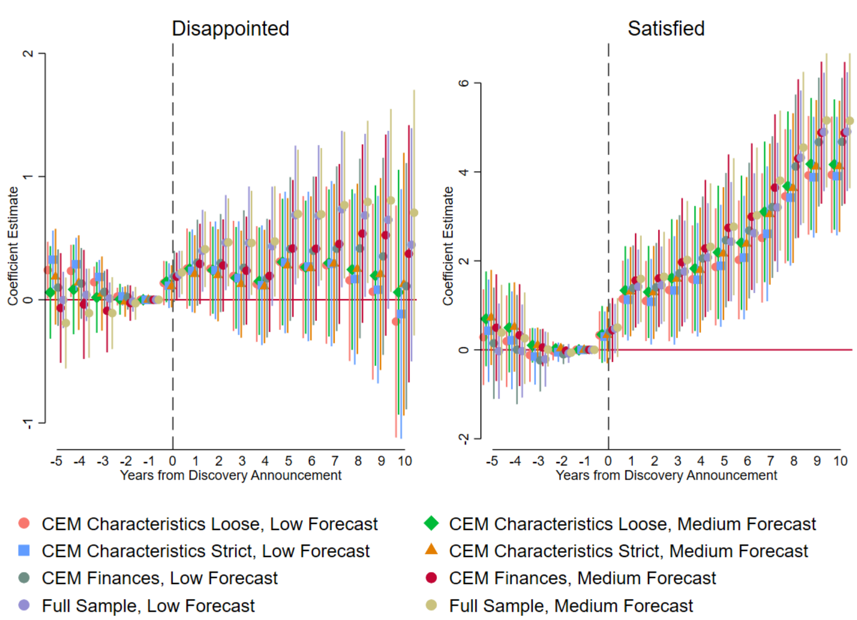


Figure U3: Robustness: Non-Oil Transfer Revenues

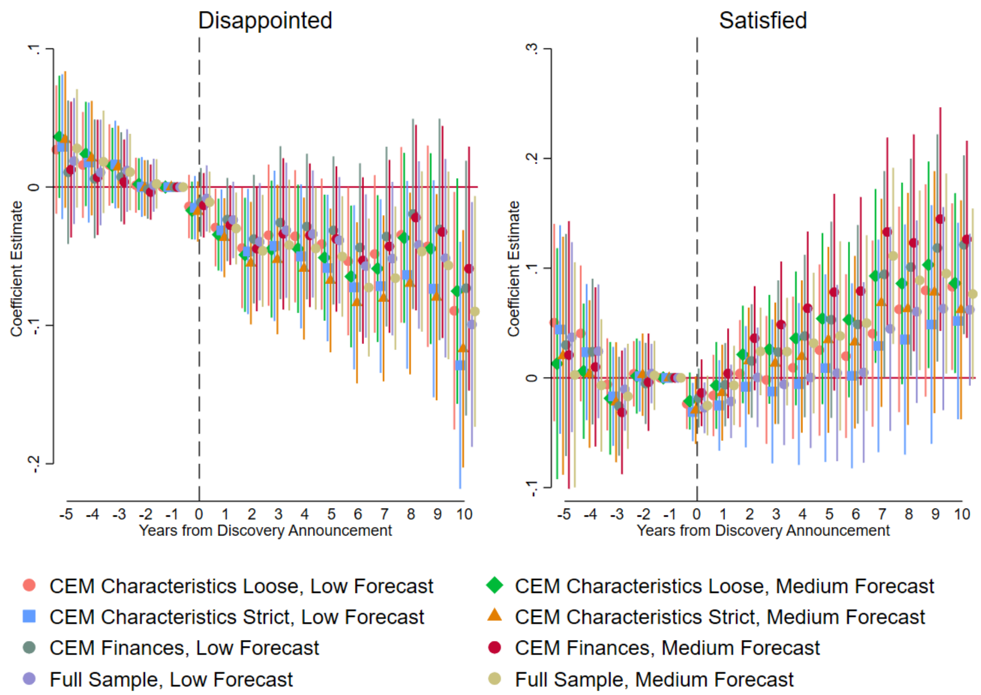


Figure U4: Robustness: Investment

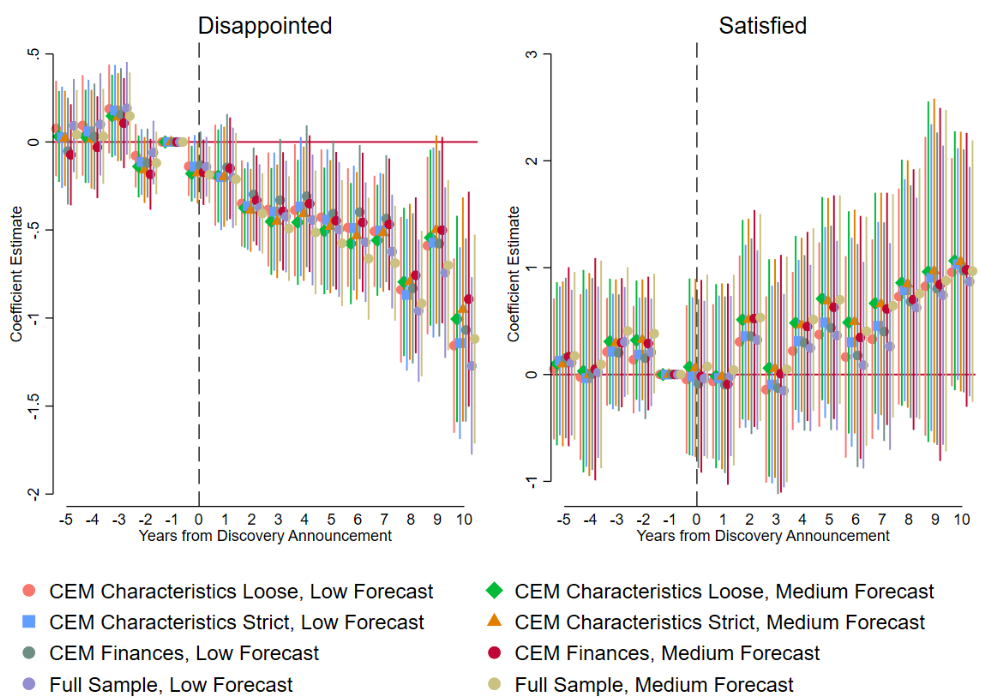


Figure U5: Robustness: Education Spending

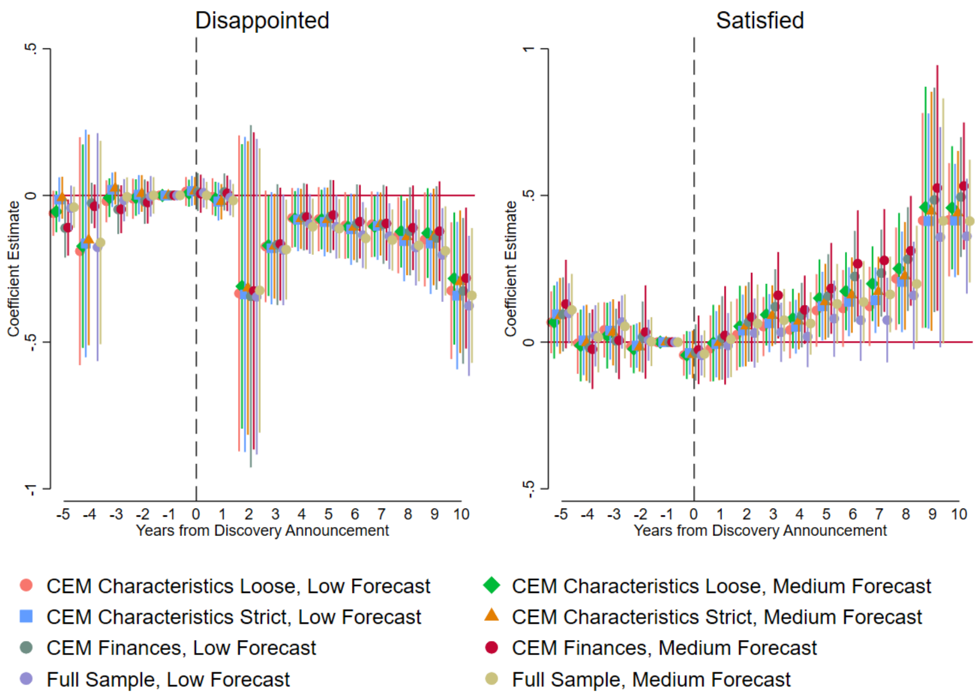


Figure U6: Robustness: Health Spending

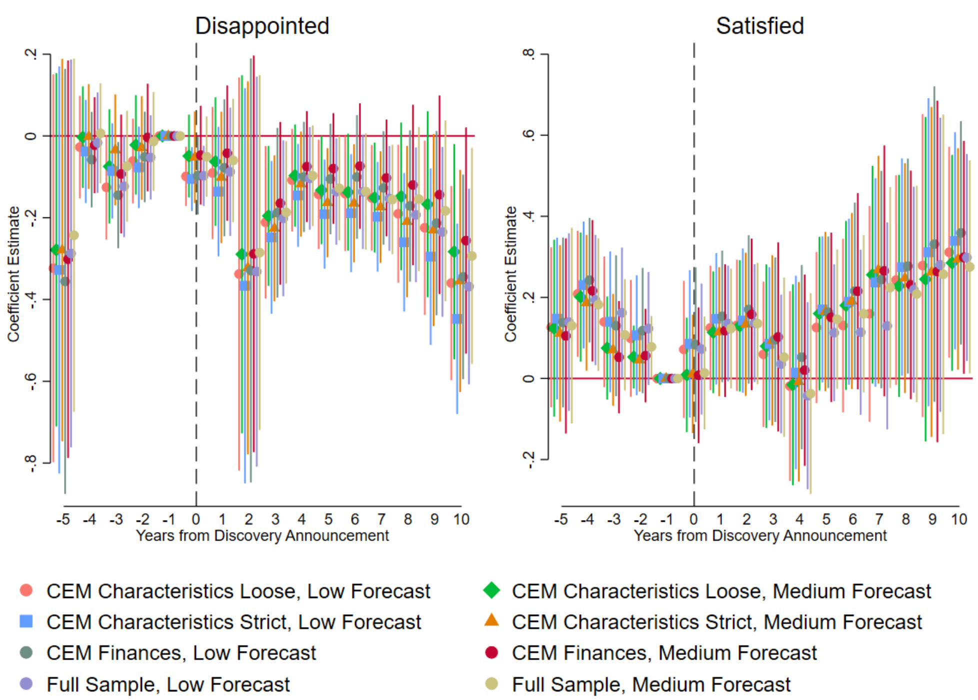


Figure U7: Robustness: Population

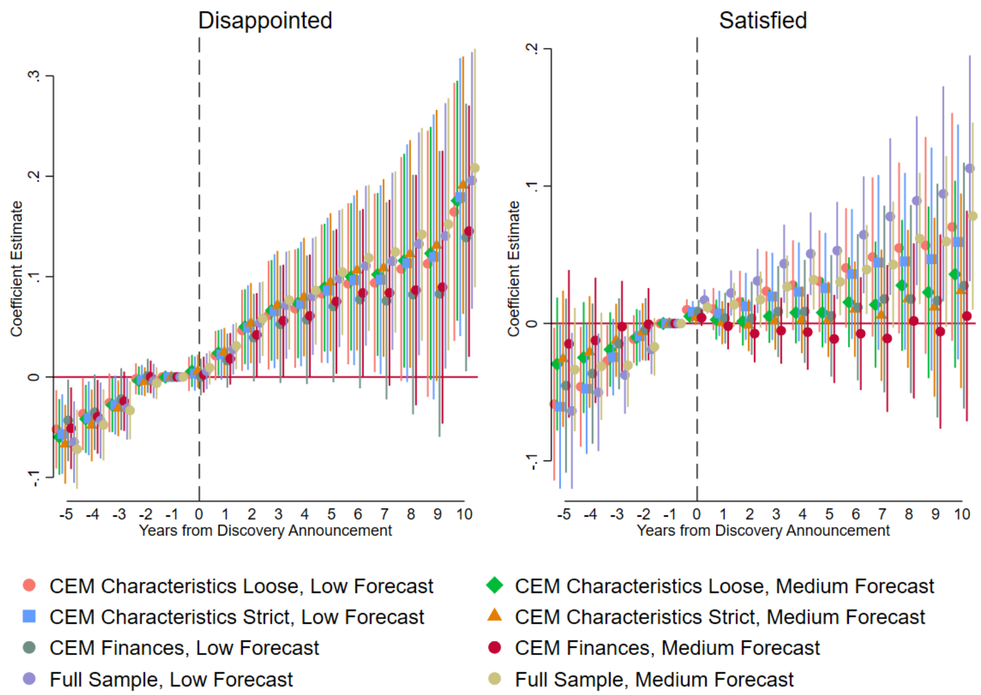
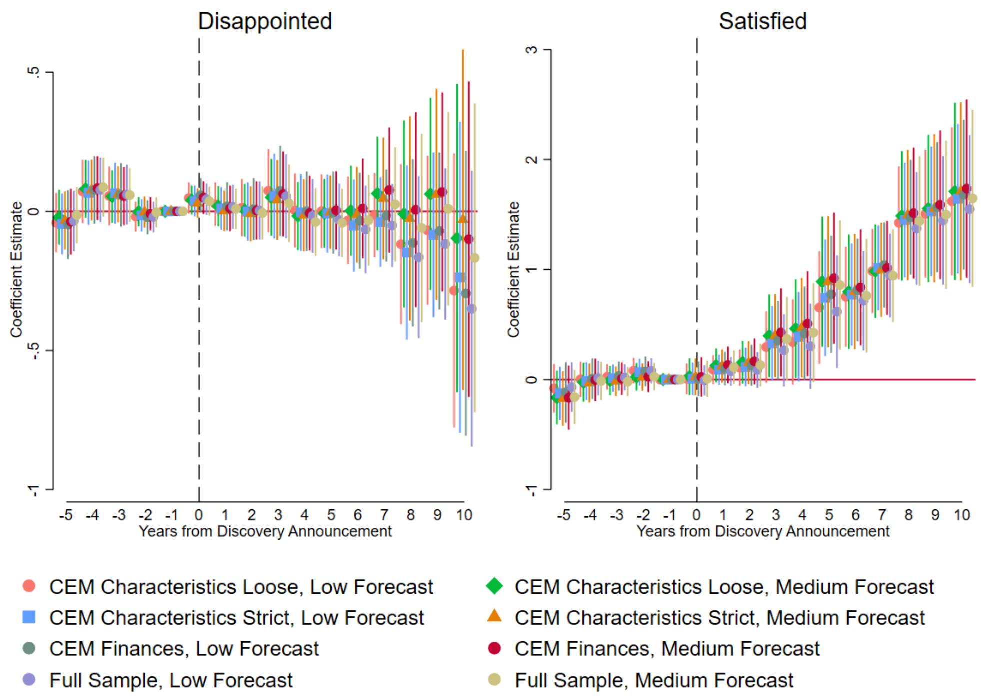


Figure U8: Robustness: GDP



Appendix V Semi-Elasticities Across Samples and Estimators

Table V1: 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
Total Revenue (Millions)	-20.79***	-10.22*	-35.17	-19.67
	(6.02)	(5.97)	(*In Progress*)	
Revenue p.c.	-26.69***	-24.41***	-46.43	-37.29
	(8.02)	(7.42)		
Tax Revenue p.c.	-37.30***	-35.04***	-33.29	-34.26
	(14.28)	(11.62)		
Oil Revenue p.c.	-5.57	35.46	-32.16	-16.70
	(40.41)	(52.28)		
Transfer Revenue p.c.	-8.99**	-7.82**	-15.95	-16.57
	(3.94)	(3.36)		
Total Spending (Millions)	-18.77***	-2.89	-29.35	-9.09
	(5.81)	(6.43)		
Spending p.c.	-23.95***	-16.44***	-40.48	-27.50
	(6.23)	(6.20)		
Investment p.c.	-56.92***	-60.59***	-76.50	-70.49
	(12.18)	(10.43)		
Personnel Spending p.c.	-26.42***	-18.33***	-44.28	-30.34
	(6.30)	(6.45)		
Education Spending p.c.	-25.64***	-20.87***	-42.05	-32.29
	(7.26)	(6.89)		
Health Spending p.c.	-26.23***	-31.61***	-39.41	-34.77
	(9.10)	(7.23)		
# Extractive Employees	-1.73	11.52	-28.71	4.36
	(51.17)	(54.51)		
# Mfg. Employees	-25.17	24.90	-30.63	30.25
	(15.93)	(20.72)		
# Extractive Firms	-26.79*	-7.07	-22.94	9.49
	(15.92)	(19.42)		
# Mfg. Firms	-21.26***	2.54	-13.50	16.11
	(7.69)	(8.73)		
Avg. Formal Wage	-12.42***	-4.22	-19.86	-5.03
	(4.21)	(3.66)		
GDP p.c.	-18.27	-18.08	-39.00	-15.06
	(13.66)	(12.37)		
Population	0.87	10.49	-38.89	-4.86
	(7.66)	(7.95)		
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: $\hat{P} = (e^{(\beta - \frac{\widehat{Var(\beta)}}{2})} - 1) \times 100$. 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 V2: 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
Total Revenue (Millions)	74.53**	107.96***	89.59	111.36
	(34.21)	(39.71)	(*In Progress*)	
Revenue p.c.	75.12**	95.43**	84.06	106.03
	(34.60)	(37.66)		
Tax Revenue p.c.	-30.32	-5.31	-11.39	6.54
	(20.58)	(24.45)		
Oil Revenue p.c.	5441.63	6205.26	6679.58	5057.57
	(3755.01)	(4330.57)		
Transfer Revenue p.c.	1.26	5.40	1.95	1.24
	(5.12)	(5.15)		
Total Spending (Millions)	20.35	45.50***	25.09	47.59
	(13.88)	(16.05)		
Spending p.c.	20.79	37.82**	21.93	43.86
	(14.07)	(15.18)		
Investment p.c.	59.35	75.04	180.02	158.75
	(113.07)	(125.85)		
Personnel Spending p.c.	14.32	30.86**	22.15	53.77
	(12.83)	(13.64)		
Education Spending p.c.	28.02	36.55	33.62	48.37
	(26.10)	(25.93)		
Health Spending p.c.	25.42	23.31	38.15	28.69
	(29.05)	(23.63)		
# Extractive Employees	60.08	112.07**	68.81	126.21
	(46.73)	(49.57)		
# Mfg. Employees	-27.45*	17.70	-30.60	11.25
	(15.63)	(19.43)		
# Extractive Firms	18.90	75.28*	23.16	110.61
	(31.23)	(42.32)		
# Mfg. Firms	-25.03***	-7.53	-21.45	-1.79
	(8.05)	(8.92)		
Avg. Formal Wage	-11.06**	-1.80	-10.12	10.10
	(4.66)	(4.38)		
GDP p.c.	253.10**	290.35**	275.49	330.81
	(110.29)	(116.96)		
Population	-3.11	3.94	272.29	-1.46
	(4.49)	(4.23)		
n (municipality-years)	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: $\hat{P} = (e^{(\beta - \frac{\widehat{Var(\beta)}}{2})} - 1) \times 100$. 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

Appendix W Results: Effects of Discovery Disappointment on Incumbent Reelection

Table W1: Effects of Disappointment or Satisfaction with Oil Discoveries in Last Four Years on Incumbent Reelection Rates

	Disappointed		Satisfied	
	LPM	Logit	LPM	Logit
Mayor (No Controls)	-0.128*	-0.145*	-0.032	-0.032
	(0.068)	(0.086)	(0.029)	(0.029)
Mayor (With Controls)	-0.119*	-0.136	-0.006	-0.006
	(0.070)	(0.089)	(0.034)	(0.034)
State FEs	Y	Y	Y	Y
Election Period FEs	Y	Y	Y	Y
n (candidate-election periods)	10,815	10,815	10,850	10,850
Council (No Controls)	-0.052***	-0.039**	0.001	-0.002
	(0.017)	(0.015)	(0.009)	(0.010)
Council (With Controls)	-0.052***	-0.042***	-0.005	-0.008
	(0.017)	(0.016)	(0.009)	(0.010)
State FEs	Y	Y	Y	Y
Election Period FEs	Y	Y	Y	Y
n (candidate-election periods)	160,169	160,169	160,945	160,945

Table reports coefficient estimates (marginal effects for logit models) with standard errors in parentheses for specification: $P(Reelection_{cme} = 1) = \delta_s + \lambda_e + \beta Disappointed_{me} + X'\mu + \epsilon_{cme}$. Standard errors are clustered at the municipality level in all specifications. $Disappointed_{me}$ 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, $Satisfied_{me}$ 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

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