Pontifícia Universidade Católica
do Rio de Janeiro

Joana C.M. Monteiro

Effects of Natural Resource Abundance and Neighborhood Violence on Economic Development

Tese de Doutorado

Thesis presented to the Postgraduate Program in Economics of the Departamento de Economia, PUC–Rio as partial fulfillment of the requirements for the degree of Doutor em Economia

Advisor: Prof. Claudio Ferraz

Rio de Janeiro
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Joana Monteiro holds a BA in economics from the Federal University of Rio de Janeiro (UFRJ), a MA in economics from the Catholic University of Rio de Janeiro (PUC-Rio) and now a Ph.D in economics from PUC-Rio. During her Ph.D., she visited Harvard University as a research fellow at the Center for International Development. Joana does research on economic development and has performed studies on different topics such as informality, labor market, natural resources, political economy, violence and education. Before enrolling in the Ph.D. program, Joana worked at DAI Brazil, a Brazilian subsidiary of Development Alternatives, Inc. (www.dai.com). During three years at DAI Brazil, she had the opportunity to participate in projects in different parts of Brazil, Mexico and Guatemala for clients such as USAID, IADB, local governments and public institutions.

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Abstract

Monteiro, Joana C.M.; Ferraz, Claudio (Advisor). **Effects of Natural Resource Abundance and Neighborhood Violence on Economic Development**. Rio de Janeiro, 2010. 141p. Tese de Doutorado — Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro.

This thesis is comprised of three articles. The first two chapters study the effects of natural resource abundance on economic development by analyzing Brazil's offshore oil boom and the distribution of royalties to municipalities. In the first chapter, we examine the impact of this oil boom on local economies. We show that oil production has little economic impact on the municipalities, other than in the public sector. By far, the most important effect is on the number of public employees, which increased a great deal from 1997 to 2006. Few improvements were found on health and educational services. The second chapter analyzes oil effects on local politics. We show evidence that oil does not make leaders unaccountable and that a democratic system is crucial to avoid the negative effects of resource abundance. Our results indicate that, although oil windfall creates a large incumbency advantage in the short run, voters reward incumbents by reappointing them to office as long as they are not completely informed of the size of the extraordinary revenue and see increases in public employment as an indication of mayor's ability. In the medium run, as information about the resources increases and a larger public sector does not translate into more public goods and services, citizens oust the incumbent and select new candidates. The third chapter investigates a different subject. We analyze the relationship between neighborhood violence and school achievement, by exploring time and geographical variation in Rio de Janeiro's drug battles. We find that schools close to areas that experience more variation in armed conflicts over time perform worse in standardized math exams, while no significant effect is found on language exams. Violent events are also associated with an increase in grade repetition and dropout for 5th graders. In terms of mobility across schools, we find no significant effects of violence on students' transfers and new admissions during the school year. We also discuss the mechanisms that can explain these results and provide evidence that violence is associated with an increase in teacher absenteeism.

Keywords

Political accountability; natural resources; oil; neighborhood violence; school achievement.

Resumo

Monteiro, Joana C.M.; Ferraz, Claudio (Orientador). **Efeitos da Abundância de Recursos Naturais e da Violência Local sobre Desenvolvimento Econômico**. Rio de Janeiro, 2010. 141p. Tese de Doutorado — Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro.

Essa tese é composta por três artigos. Os dois primeiros estudam os efeitos da abundância de recursos naturais através da análise do recente *boom* de petróleo no Brasil e a distribuição de royalties para os municípios. No primeiro capítulo, estuda-se os efeitos do *boom* de petróleo sobre a economia local. Mostra-se que os efeitos da produção de petróleo são pequenos, limitando-se ao impacto sobre o setor público. O maior impacto encontrado é sobre o número de funcionários públicos municipais, que aumenta consideravelmente entre 1997 e 2006. São encontrados efeitos modestos sobre a oferta de educação e saúde. O segundo capítulo analisa os efeitos do royalties de petróleo sobre a política local. Os royalties aumentam a probabilidade de reeleição dos prefeitos na primeira eleição que sucede o *boom* de receitas, mas essa vantagem não persiste nas eleições seguintes. Os resultados são consistentes com um processo de aprendizado por parte dos eleitores, que somente reconduzem os prefeitos ao poder quando têm conhecimento limitado sobre o choque positivo de receitas e interpretam o aumento de funcionários públicos como um sinal de habilidade do prefeito. Contudo, os resultados indicam que a abundância de recursos não acabou com a responsabilização dos prefeitos e que um sistema democrático é crucial para restringir o uso irresponsável desses recursos. O terceiro capítulo investiga um outro tema: a relação entre violência local e desempenho escolar. Explora-se a variação no espaço e no tempo do conflito de drogas no Rio de Janeiro para entender o impacto desses conflitos sobre as escolas municipais. Encontra-se que as áreas que sofreram mais variação de conflitos armados ao longo do tempo têm um desempenho inferior em testes padronizados de matemática. A exposição aos conflitos também é associada a aumento na reprovação e do abandono de alunos do quinto ano do ensino fundamental. Discute-se ainda os canais que podem explicar a piora do desempenho e identifica-se que violência está associada a maior ausência de professores nas escolas.

Palavras–chave

Responsabilização política; recursos naturais; violência; desempenho escolar.

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1 Brazil's Oil Boom and its Effects on Local Economies

1.1 Introduction

An abundance of natural resources can be a blessing or a curse. While some countries are able to exploit resource riches to improve their welfare, many others are doomed by such discoveries (e.g. Botswana and Nigeria). Despite the existence of many studies that examine the effects of resource abundance on economic performance across countries, great controversy still exists over the true effects of resource booms (Haber & Menaldo (2010), Hodler (2006), Lederman & Maloney (2007), Sachs & Warner (1995), Mehlum et al. (2006), Rodriguez & Sachs (1999), Ross (1999), Ross (2001), Ross (2009)). There are two main reasons for this lack of consensus. First, there are inherent difficulties in controlling for other factors that co-vary with both resource abundance and economic performance in cross-country regressions. Second, resource endowment is usually measured by production, which is endogenous to country level of development and institutions, thus making it hard to interpret the results as causal estimates of the effect of resource abundance.

This chapter examines the impact of oil booms on Brazil's local economies. Specifically, we study how oil windfall is invested by municipalities and whether it improves living standards. We do so by using variation across municipalities benefited from Brazil's recent oil production[1] boom and new rules for distributing oil royalties[2] to drilling regions. Over the last twelve years, oil output in Brazil more than doubled, from 307 in 1997 to 663 million barrels in 2008. Moreover, royalty payments increased from 5 to 10 percent of the production value and were indexed to oil's international price. Hence, royalty payments to municipalities increased by twenty-seven-fold in real terms from R$ 167 million in 1997 to R$ 4.7 billion in 2008, creating several "new" oil-rich municipalities. For comparison, the FPM, the main federal transfer to municipalities in Brazil, increase by one-fold in the period. Municipalities lucky

[1]We use the term oil to denote oil and natural gas production since oil corresponds to the bulk of oil and gas production.

[2]We use the denomination royalty loosely throughout the paper to refer to royalties plus special quotas ("participações especiais". ANP calls the sum of both payments as "participações governamentais".

enough to be situated in front of an offshore oil field according to the geographic lines benefited disproportionately and received a huge windfall. To have an idea of the size of the budget impact, the top beneficiaries, on average, saw their municipal budget increase three times in real terms between 1997 and 2000, and doubled that number between 2000 and 2004.

This paper presents innovations which allow a better estimation of the effects of oil booms on development. First, because most oil production is offshore and oil revenue is distributed according to a fixed geographical rule, we can use it as an exogenous windfall to incumbent. We also instrument royalty revenue by oil output in order to only assess the variation that results from production and price shocks. Second, we analyze oil royalties paid by Petrobras and other multinational companies to the Federal Government, which, in turn, redistribute them to municipalities. This allows us to circumvent the potential endogeneity in the decision to extract oil since we compare municipalities that do not influence production decisions. Moreover, by using variation across local governments within a country, we keep constant all the variation in macro institutions that might also affect long-term economic growth. Finally, since royalty payments increased considerably during the last decade, we have enough temporal variation in the data which allows for the estimation of fixed-effect regressions. Therefore, by using panel-data for municipalities we are able to control for all potential geographical characteristics that are likely to affect resource availability, economic growth potential, and economic outcomes.

We provide evidence that oil windfall does not have major impact on local economies. The number of firms in different sectors, the private payroll and the non-industrial GDP do not change as a consequence of more oil funds. The main impact appears to have occurred on the municipal public budget, which enjoyed a large boost due to royalty payments. Although municipalities report to have increased all their expenses, we are not able to find significant improvements in local economies. By far, the most important impact is on the number of public employees, which increased a great deal from 1997 to 2008. An one standard-deviation increase in royalty revenue is associated with an average annual increase of 10 percent in the number of municipal employees, which implies that the municipal public sector increased more than two-fold in the twelve years under analysis. Most of this increase was driven by non-tenured employees. About 25 percent of the new employees were hired to provide more health and educational services, but this increase was not translated into better education outcomes nor accompanied by an increase in the number of health clinics or hospitals.

Taken together, these results indicate that oil rents did not guarantee economic development, and that municipalities lost a great opportunity to improve their living standards. However, we don't find evidence to support the resource curse story, since municipalities' situations have not worsened due to these revenues. We should emphasize that these results indicate medium run effects and the long run consequences of oil revenues could be more pervasive, especially by finding that municipalities use oil windfall to increase current expenses and boost the public sector rather than investing in areas that can promote long run economic development.

This paper relates to the literature that aims to understand the impacts of natural resource abundance. Some cross-country studies find that nations that specialize in the production of natural resources grow less (Sachs & Warner (1995), Rodriguez & Sachs (1999)) and tend to be less democratic (Ross (1999) Ross (2001)). These findings, however, have been challenged by several authors that use alternative measures of natural resource specialization (Lederman & Maloney (2007)), or studies that use within-country variation (Michaels (2009)). Another line of research argues that an increase in the stock of natural resources induces rent-seeking which distorts the incentives for productive investment (Baland & Francois (2000), Lane & Tornell (1996), Tornell & Lane (1999), Torvik (2002)). Finally, Gylfason (2001) and Leamer et al. (1999) argue that politicians in resource-rich environments do not have incentives to spend on education and the lack of human capital accumulation reduces long run growth.

This study complements recent papers that use geographical variation in oil availability within countries to examine the effects of oil abundance on long run economic development and the quality of government. Michaels (2009) uses geological variation in oil abundance in U.S. counties to investigate the effects of oil specialization. He finds that the development of the oil sector increased education and per capita income without causing ill effects on industrialization or inequality. More related to this study is Caselli & Michaels (2009) who use variation in oil abundance among Brazilian municipalities to assess the effects of resource abundance on local economic activity, public spending, public good provision, and living standards. They find only modest effects on non-oil GDP and public good provision, and no significant improvements in living standards, leading them to conclude that most of the oil royalties received by municipalities go missing. We employ a different empirical strategy than Caselli & Michaels (2009) by focusing on municipalities located on the Brazilian coast and by exploring within variation in addition to using oil output as an instrument for royalty revenue. Moreover, we look at a different time period and different databases, which explain why both papers find different results in respect to public employment. Overall, though, our paper corroborates Caselli & Michaels (2009) main message that oil windfall does not promote increases in living standards.

The remainder of the chapter is organized as follows. Section 2 describes the institutional background. Section 3 explains the methodology and section 4 describes the data used. Section 5 presents the empirical findings. Finally, section 6 concludes the chapter.

1.2 Institutional Framework

Brazil has extracted oil since 1939, but oil production became important only in the mid-1970s, when oil fields in Campos Basin, on the coast of Rio de Janeiro, were discovered and the increase in international oil prices made offshore production viable.[3] The industry prospects improved during the 1980s when the first giant oil fields were found as shown in Figure 1.1.[4] An important industry upturn occurred in 1997, with the enactment of Law no. 9478, named the Oil Law, which phased out the state oil extraction monopoly.[5] Oil output increased and more than doubled between 1997 and 2008, reaching 663 million barrels in 2008. Figure 1.2 shows that offshore oil output drove this increase, by tripling from less than 200 million barrels a year in 1994 to 600 million barrels in 2008, while onshore output was stable around 65 million barrels a year in this period.

Ten states produce oil in Brazil but production is highly concentrated in Rio de Janeiro, which is responsible for 92% of offshore or 82% of Brazilian oil output. Looking within the states, 53 municipalities have onshore oil wells and 73 are classified as producing municipalities because they face offshore oil fields (see below for a formal description of "facing" municipalities). The industry which supports offshore activities is concentrated in one city, Macaé, which is located in the north of the state of Rio de Janeiro.[6]

Oil companies must pay up to 10 percent of output value in royalties to federal, state and local governments. The legislation that determines the value and the beneficiaries of royalty revenue was modified several times. Onshore royalties were introduced in 1953 and were paid to states and municipalities.

[3]The most notable oil fields discovered in mid-1970s were Garoupa (1974), Namorado (1975), Badejo (1975), Enchova (1976), Bonito (1977) e Pampo (1977). The first offshore well drilled in the country was in Sergipe in 1968. Bregman (2006)

[4]In 1984, Petrobras discovered Albacora, the first giant oil field in deep waters, which consolidated Campos Basin as the main production zone in the country.

[5]From 1953 to 1997, only Petrobras, the Brazilian state-company, produced oil in Brazil. The new rules exposed Petrobras to international competition but the company is still by far the largest player in Brazil's oil market.

[6]Macaé was selected by Petrobras in the 1970s as the base for offshore activities due to its geographic proximity to Campos Basin.

Offshore royalties were created in 1969, but only benefited the federal government. In 1985, during the re-democratization period and following a political movement to decentralize fiscal revenues, Law 7.453/85 was enacted and offshore royalties began to be paid to states, municipalities and the Navy.[7] In this decision, one key issue was to determine which municipalities were affected by offshore oil production. Politicians chose a geographic criteria and classified municipalities into four groups: producing municipalities, secondary zones, neighboring municipalities and non-affected municipalities. In 1986, Decree 93.189/86 classified as 'producing municipalities' those that lie in front of an oil well according to orthogonal and parallel lines to the Brazilian coast. These lines were not the object of political bargain since, by law, they were designed by the National Bureau of Statistics (IBGE) based on the geodesic lines orthogonal to the Brazilian coast which are used as reference in nautical letters. Figure 1.3 illustrates the criteria for the coast of Rio de Janeiro.[8]

The main modification in the oil royalty rule occurred with the enactment of Oil Law in 1997. This law increased royalty payments from 5 to 10 percent of the output value and indexed the reference price to the oil international price. In addition, the Law created special quotas ("participações especiais") or extra payments received from highly productive oil fields.[9] The second parcel of 5% of royalty payments followed a different rule than the previous one and benefited even more producing municipalities (see Annex for details).[10] The new legislation was followed by the upward trajectory of international prices and two large Brazilian Real devaluations. All these facts together induced an enormous increase in royalty payments from R$ 190 million in 1997 to R$ 10.9 billion in 2008.

Taken together, royalty payment rules imply that local governments are the main beneficiaries of oil windfall. In 2008, municipalities directly received 34 percent of royalty payments, followed by states, which received 30%, the Ministry of Science and Technology (16%), the Ministry of Navy (12%) and a special fund (8%).[11] This level of decentralization of natural

[7]This Law only entered into effect in 1986, after being regulated by Law 7.525/86 and Decree 93.189/86. Law 7.453/85 was proposed by Senators Nelson Carneiro (PMDB - RJ) and Passos Pôrto (PDS - SE), whose aim was to introduce offshore royalties by following the same rule which was used for onshore royalties. For details on the political bargains made to approve Laws 7.453/85 and 7.525/86 see Serra (2005).

[8]There was another modification in the rule in 1989. Law 7.990/89 included municipalities with transportation facilities from and to oil sites in the list of benefited municipalities.

[9]The special quotas were paid for the first time in 2000 and about 30 municipalities received it in 2008.

[10]Serra(2005) argues that the new rule for royalty payments was not the object of much debate during the approval of the Oil Law because this Law was dealing with more important topic by that time, the phase-out of the state monopoly in oil production.

[11]Actually, the value received by local governments is even greater because they indirectly

resource compensation is not observed in other countries (Serra, 2005).

These rules also imply that geographic location is the main determinant of who receives what and how much of the oil windfall each municipality gets. The largest share of royalty revenue that goes to municipalities is paid to 'producing municipalities" because they are considered the ones most affected by oil production. In addition, the proximity to these municipalities determines the status of 'neighboring cities". However, the amount paid to each municipality depends not only on geographic position, but also on population and the location of production plants, pipelines and transportation facilities (see Annex for details on the payment rule).

Every month an oil windfall is paid to the Brazilian Treasury, which in turn distributes it to the beneficiaries. Municipalities are free to allocate this income, with two restrictions. They cannot use this rent to hire public employees on a permanent basis, nor can they pay debts with it.[12] The Tribunal de Contas of each state (TCEs) is the institution in charge of auditing the allocation of royalty revenues. This windfall can be invested in different types of public goods and services. Local governments in Brazil are the main providers of basic education and basic health services. In addition, they are responsible for local transportation and infrastructure. Security, however, is supplied by state governments and few Brazilian municipalities have a local police.

1.3 Empirical Strategy

Our main objective is to understand oil revenue impact on local economies. Specifically, we want to estimate:

$$y_{it} = \rho R_{it} + X_{it}\beta + c_i + \lambda_t + u_{it} \tag{1-1}$$

where y_{it} denotes municipality i outcome at year t (e.g. public employment and wages, educational and health supply measures), R_{it} indicates royalty value paid to municipality i at time t, X_{it} is a vector of municipality characteristics that vary over time such as population, c_i is a municipality fixed-effect, λ_t is a year fixed-effect and u_{it} is a random shock.

However, oil windfall is not exogenous to local economies because it depends on the geographic proximity to an oil field, population and the location of oil facilities. The main concern is related to the location of oil plants and facilities which may vary over time and are not perfectly observed by us. In

receive 80% of the special fund and 25% of the payments that go to state governments. This implies that municipalities receive 47.6 percent of royalty revenue. In our analysis, we only take into account the direct payments to municipalities.

[12]The only exception is a debt with the Federal Government, which can be paid with this income.

order to deal with this potential problem, we follow Caselli & Michaels (2009) and apply an instrumental variable approach, using the following equation as a first stage equation:

$$R_{it} = \gamma_1 Z_{it} + X_{it}\gamma_2 + c_i + \lambda_t + \epsilon_{it} \tag{1-2}$$

where Z_{it} denotes oil production value and ϵ_{it} indicates non-observable characteristics that explain royalty payments, such as oil producing plants.

The validity of this approach depends on two main assumptions: (i) Z_{it} has a significant effect on R_{it} and (ii) the only impact of Z_{it} on Y_{it} is through R_{it} (the exclusion restriction). The first assumption is guaranteed by the royalty rule, which generates a strong first stage, as a fraction of oil output is paid in royalties to municipalities where drilling is done. In addition, the rule allocates offshore output among municipalities according to lines that lie parallel and orthogonal to the Brazilian coast, creating a geographic instrument. Figure 1.4 shows the map of the Brazilian coast with producing and non-producing municipalities and the location of oil fields. We believe that this figure makes explicit the fact that, conditional on being on the coast, the status of 'producing municipality' is quite random.

However, Figure 1.4 also highlights that benefited municipalities are not evenly distributed in Brazil, instead, they are mainly on the Brazilian coast. If coastal municipalities are systematically different from other Brazilian municipalities, and indeed they are, a simple comparison between benefited and non-benefited municipalities may have biases. To account for this problem, we restrict our analysis to coastal municipalities in producing states. This provides a sample of 159 municipalities distributed among the states of Ceará, Rio Grande do Norte, Alagoas, Sergipe, Bahia, Espírito Santo, Rio de Janeiro, São Paulo e Paraná.[13] In addition, we exclude the top 1 percent of municipalities in royalty distribution in order to deal with outliers, which implies excluding two municipalities from the sample (Quissamã and Rio das Ostras).[14] As robustness checks, we replicate most of the results in the annex using two alternative samples and show that our findings are, in most cases, not sensitive to sample selection. We use a full-sample that includes all the 2,157 municipalities from the nine producing coastal states and in a third sample we restrict our analysis

[13] Although the state of Amazonas also produces oil, we exclude it from the analysis because it only has onshore production. Santa Catarina also produces oil but its output is small, intermittent and attributed to just two municipalities, which led us to exclude it from the sample.

[14] Some results are quite sensitive to the exclusion of these two cities because they are huge outliers. Quissamã received 86% more royalty payments per capita than the third municipality in the rank and 160% more than the fifth municipality, while Rio das Ostras earned 64% more than the third municipality and 128% more than the fifth in the list of most benefited municipalities in per capita terms.

to the 124 onshore and offshore producing municipalities.[15]

The second main assumption in the identification strategy (the exclusion restriction) requires that oil output does not generate any direct effect on outcome variables, for instance, through economic impacts or income effects. We believe that this is plausible because 90% of oil is produced offshore in Brazil and services and industrial plants that support offshore production are concentrated in one city (Macaé).[16] Although we cannot test this assumption, we provide evidence in the empirical results that oil production does not have any economic effect on local economies other than through the municipal budget.

Therefore, our main empirical specification employs a panel IV strategy, described by equations (1-1) and (1-2). Table 1.1 shows the first-stage regression for the three samples used in this work. The F-statistics is greater than 230 for all samples, confirming that we have a strong first stage relationship.

Our approach is different from the one used in Caselli & Michaels (2009) in several ways. First, we focus on offshore production variation by looking only at coastal municipalities. The next section presents summary statistics that show that this sample gives us a better control group than the one that uses all municipalities. Second, our analysis covers a different period. We explore annual variation of royalty payments between 1997 and 2008, the period when the oil boom was most remarkable. In addition, we were able to construct royalty payments and oil output series for 1996-1998, which allow us to understand royalty effects before the boom. In turn, Caselli & Michaels (2009) analyze variation on outcome data mainly from 1991 and 2000, having few outcomes whose values were gathered more recently. Third, our analysis of the impact of royalty revenue on public goods supply and municipal expenses explore a within-variation in addition to the IV strategy, leading to more clean estimates. Finally, our unit of analysis is the municipality rather than the AMC ('área minima de comparação). In Brazil, the fact that many municipalities split during the 1990s led to the creation of the AMC concept, which aggregates municipalities according to their original political borders and allows comparisons across decades. While this is an easy way to deal with municipal divisions, the results generated by this strategy do not have a clear economic interpretation. The main concern is related to public budget analysis and the size of municipal civil service. For instance, consider a municipality which was split in three during the 1990s. AMC measures

[15]We also exclude Quissamã and Rio das Ostras from these alternative samples to guarantee comparability.

[16]In the empirical section, we run the regressions with and without Macaé and the results do not change.

compare the municipal budget of one municipality in 1991 with the sum of three municipal budgets in 2000. The problem is that all municipalities have a minimum structure and the sum of three budgets is probably larger than a hypothetical one that would include the three. We don't need to rely on AMC analysis because municipality divisions are not a concern in the sample and period under analysis (1997-2008),[17] which allow us to understand the impact of royalties on municipalities, which is the actual political division.

Finally, there is a possible concern related to the endogeneity of oil output Z_{it}. One may argue that municipalities can try to influence oil output from each oil field in order to influence the amount of royalties they receive. We believe that this possibility is highly unlikely in the Brazilian context. Production and investment are carried out by Petrobras and other multinational companies, respond to long-term decisions and involve budgets in the billions of dollars. It seems highly unlikely that tiny municipalities and local politicians can influence multinational companies' plans, and there is no anecdotal evidence in support of this idea. In the empirical section, we provide direct evidence that endogeneity of oil output due to local political influence is not a concern in the context under analysis.

1.4 Data

We use several data sources in this study. Agência Nacional de Petróleo (ANP) is the main source of information for the oil sector in Brazil and provides data on oil output, oil fields location and royalty payments to municipalities from 1999 to 2008. We complement this data with information on oil output from the Oil and Gas Journal (Oil & Special (1999)).[18] The December editions of this magazine report oil output per oil field in Brazil and other countries from 1991 to 1997. This allows us to construct the series of oil output and to recover royalty payments data for the 1990s. As a result, we have oil output and royalty payments series from 1995 to 2008, which let us understand how municipalities were affected by oil windfall before and after the boom in royalty payments promoted by the Oil Law. This is the first work that provides oil data at the municipal level for the 1990s. In the Annex we explain in details how we built oil production annual values, how we linked oil output to specific municipalities and how we recovered royalty payments series. We

[17] Ten among the 159 coastal municipalities were installed in 1997 and have their first election in 1996, so we have all outcome information for them. Six municipalities in the states under analysis were created in 2001 but just one, Jequiá da Praia in Alagoas, is on the coast. This municipality is not included in the sample.

[18] We are grateful to Gabriela Egler for showing us this data and making it available to us.

double checked our calculation and we show that the 1994-1997 royalty series constructed based on Oil and Gas Journal data is almost equal to the one provided by ANP at the state level (correlation 0.9997).

In order to understand whether oil windfall improves living standards, we gathered information on how municipalities spend their budget and on local public goods provision. Data on public finance, including revenues and expenses, are available from Brazil's National Treasury through the 'Finanças do Brasil' (FINBRA) database from 1997 to 2008. Educational outcomes are provided by Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira (INEP) from 1996 to 2006. The number of municipal health clinics and hospitals are available at DATASUS's site for the periods of 1998-2002 and 2006-2008. Information on municipal public employees for the 1996-2008 period was gathered from the Social Security Registry of all formal workers in Brazil (RAIS), and collected by the Brazilian Ministry of Labor. We also use RAIS to obtain information on private employees, total payroll and number of firms per sector in order to estimate oil windfall effects on economic activity. This analysis is also complemented with information on municipalities' GDP available from the IBGE for the period 1999-2007.

The analysis to identify endogeneity issues is based on geocoded information regarding when and where oil fields were discovered in Brazil. We gathered this data from ANP's Exploration and Production Database (Banco de Dados de Exploração e Produção de Petróleo - BDEP). Finally, we got complementary information to account for differences in municipal characteristics that may confound the results. Since oil output is concentrated in the Brazilian coast, we gathered data on municipalities' geographic position to use as controls in the regressions that do not use municipal fixed-effects. IPEA provides information on geographic characteristics such as latitude, longitude, altitude and distance to the state capital. We also use demographic characteristics such as percentage of urban households, infant mortality and percentage of illiterate population available from the 1991 and 2000 population census as controls in some regressions and to understand differences among municipalities before the oil boom. In addition, we use the IBGE inter-census population estimates to obtain yearly data on municipal population, which are used in all regressions. All monetary variables used throughout the analysis have been deflated using IPCA index and represent real values on 2008 prices. In the annex, we provide the sources of all variables.

Table 1.2 shows summary statistics for royalty payments in each political mandate. There were 103 oil producing municipalities in 1997 and this number increased to 123 in 2008 as new oil fields entered into production. These municipalities received on average R$ 133 per capita per year in the 1997-2000 electoral mandate, which was equivalent to 9% of their municipal revenue or to 2 percent of Brazil's per capita income in 2000. Royalty payments increased more than three-fold on average in the period under analysis, reaching R$ 478 per capita per year in the 2005-2008 period, or 15 percent of municipal revenue. Producing municipalities are concentrated on the Brazilian coast, which is the location of 58 percent (71 out of 123) of oil producing municipalities. This group receives larger royalty payments (R$ 697 per capita per year in 2005-2008) because they face highly productive offshore oil fields. There are more 2,000 municipalities in the nine oil producing states and some of them also receive royalties because they are neighboring municipalities or have oil facilities. However, the amount received by this group is quite small, being about R$ 10 per capita per year or 0.6 percent of municipal revenues in 2005-2008 period.

Table 1.3 provides information on how oil producing and non-producing municipalities differ in terms of municipal characteristics. Columns (1) and (2) show that producing municipalities had worse economic indicators than non-producing municipalities in 1991. Producing municipalities had a higher percentage of urban population, larger illiterate population, lower household per capita income, higher poverty rate, lower human development index, higher infant mortality and lower percentage of households with water pipes. More importantly to our analysis, the evolution of these variables between 1991 and 2000 show that they follow more or less the same growth pattern, but producing municipalities experienced a larger population growth and a lower reduction in mortality rates. We also see striking differences between political characteristics in 1996 and geographic characteristics. There are more producing municipalities close to the sea, to the equator, to state capitals and in low altitudes, which reflect the fact that most of producing municipalities are on the Brazilian coast.

These differences led us to concentrate our analysis on municipalities on the Brazilian coast. Columns (4) and (5) compare average characteristics from producing and non-producing municipalities on the Brazilian coast. Most of the differences previously observed disappear. These two groups of municipalities were very similar in 1991, with the only exception that producing municipalities were slightly more unequal. These municipalities also followed a similar trend between 1991 and 2000. The only difference found is that producing municipalities made more progress in reducing poverty and experienced a lower increase in income inequality. Table 1.3 also shows that political and geographic characteristics are not statistically different between producing and non-producing municipalities on the coast. The similarity of observable characteristics between coastal municipalities that produce and do not produce oil make us confident about using coastal municipalities as our main sample.

1.5 Empirical Results

We begin the empirical analysis by providing evidence that endogeneity in oil output is not a concern in the context under analysis. We present the timing of oil discoveries and the relation between having a oil field discovered in its boundaries and municipal political alignment. We then show evidence that oil production does not have any economic effect on local economies rather than through the public sector. We follow by investigating how municipalities spend oil windfall. We show that municipalities report having increased all their expenses but do not change their budget composition. Oil windfall is associated with a large increase in the number of non-tenured employees, which in particular increased from 1999 to 2006. No significant impacts on education or on health supply were found.

1.5.1 Determinants of Oil Discovery and Production

As briefly discussed in the Empirical Section, there are few reasons to believe that local municipalities have the capacity to influence Petrobras and other multinational company plans on where and when to drill an oil field. Figure 1.1 shows that the largest oil fields in terms of 2008 oil output were discovered in the mid-1980s and in 1996. Therefore, for mayors to influence drilling locations in order to receive more royalties would require that the same political groups were in power in oil-rich municipalities for more than 10 years (from mid-1980s to 2000s) and that mayors from oil-rich areas could anticipate or influence the enactment of the Oil Law in 1997, which was responsible for the major increase in royalty revenue. Although both facts seems unlikely, Table 1.4 provides direct evidence that mayors indeed do not influence discoveries and output from oil fields. We explore the association between the timing of discoveries and initial production of new oil fields and municipalities political alignment. Each observation is one municipality. The sample covers the period from 1993 to 2008 and includes all Brazilian municipalities that have at least one oil field (onshore or offshore) discovered within its boundaries in any moment in time. In column 1, the dependent variable is equal to one if an oil field within a municipality's borders was discovered in the respective year, while in column 2 the dependent variable indicates whether oil began to be extracted on the respective year. The regressions include a dummy indicating whether the party in power in the municipality is from the same political coalition of the federal government, party dummies, year and city effects. We see that the fact that the party in power in the municipality is from the same federal government political coalition is not associated with the municipality having an oil field discovered within its borders or with the year oil field entered into production. In addition, we see that few, if any, parties have a higher or lower probability than PT (the Workers Party, which governed the country from 2003 to 2010, and the omitted party in this regression) of influencing the timing of oil production. Finally, columns 3 and 4 look at the time gap between discovering the oil field and beginning its production and confirm that there is no indication of municipal political influence on oil production decisions.[19]

1.5.2 Impact on Economic Activity

One of the main hypotheses in our empirical strategy is that oil output does not affect municipal outcomes through other channels than the public budget. We believe that this assumption can be supported because 90% of oil produced in Brazil comes from offshore wells and most of municipalities which face oil fields does not suffer any externality from oil output. Table 1.5 presents evidence on that direction by showing oil output effects on population and different variables of economic activity. The results presented in columns 1-10 are from panel regressions that include municipal and year effects as controls. With exception of column 1, all measures are in per capita terms. We present the results for three samples. Panel A includes all municipalities from the nine producing states. Panel B shows our preferred specification that includes coastal municipalities from nine producing states, while panel C sample is composed only of oil producing municipalities.

Table 1.5 shows that oil output is associated with population changes in the sample which include all municipalities from producing states. However, this result is not robust to the use of other samples which do not show any impact of oil windfall on population. This difference among samples probably reflects the fact that oil producing municipalities are concentrated on the Brazilian coast, which historically have larger population growth, and reinforce the importance of focusing on the coastal municipalities sample. Columns 2-5 reveal that oil output does not affect the number of firms in any sector in benefited municipalities. Columns 6-8 indicate that oil output does not impact the number of private employees nor the private companies payroll. However, we find a positive impact on public payroll, reinforcing the idea that oil output

[19] The sample used in columns 3 and 4 is smaller because regressions are conditioned on the municipality having an oil field discovered between 1993 and 2008

effect occurs mainly through the public sector. Finally, columns 9-10 show the effect of oil output on municipal GDP per capita. We see that oil production is associated with an increase in total GDP per capita (column 9). However, this result should be interpreted with caution. Municipal GDP in Brazil is not directly computed. The National Bureau of Statistics (IBGE) computes the state GDP and then divides each sector's GDP among municipalities according to reference variables (variáveis de rateio). The key issue in our analysis is that the reference variable used to divide mineral industry GDP is precisely the royalty rule. Hence, the estimated association between oil output and industry GDP is tautological. To assess whether oil output affects municipal economic activity, it is more informative to look at non-industry GDP, which we measured by subtracting industry GDP from total GDP. Column 10 indicates that there is no effect on this variable. Table 1.5 also shows that the results are robust to alternative samples. As an additional exercise, we checked that the results are robust to the presence of Macaé on the sample, the municipality that concentrates oil facilities for offshore production (results not shown and available upon request).

Our findings complement Caselli & Michaels (2009) paper, which shows that oil windfall does not affect municipal non-industry GDP pc. We extend this evidence by showing that oil windfall does not affect other variables of economic activity, such as number of firms, private payroll and number of private employees.

1.5.3 Municipal Budget

We now turn to assess how oil windfall impacts municipal budget and how municipalities report spending this money. Table 1.6 shows how oil windfall impacts municipal revenue. Panel A indicates the royalty effect on components of municipal revenue measured in R$ per capita, while Panel B shows the impact of oil windfall on each expense as a share of total revenue. The results are from panel-IV regressions that cover the period from 1997 to 2008 period and use municipal and year effects as controls. This analysis includes only municipalities that report the most revenues and expenses, which results in a smaller sample than in other exercises. In column 1 we see that each Real per capita received as royalty payment generates 1.13 Reais in total revenue. Column 2 indicates that an increase in tax revenue can explain approximately half of this 0.13 additional cents.[20] A one-standard-deviation increase in oil windfall is associated with an increase in R$ 0.03 per capita in tax revenue,

[20]The two main taxes under municipal authority are the property tax (IPTU) and a service tax (ISSQN).

which represents a 14 percent increase in this revenue. This result indicates that one of the problems of resource abundance pointed out by the literature - the reduction in the incentive to tax - is not present in the Brazilian context. Panel B shows that this increase in tax revenue was only sufficient to keep the share of tax revenue on total budget. The other remaining cents (0.07 out of 0.13) of additional impact on total revenue should be a result of the additional transfers that oil-producing municipalities receive from the state and federal governments (see footnote 11).

Columns 3 and 4 look at the effects of royalty revenues on two other federal transfers. FPM stands for "Fundo de Participação dos Municipios" and it is the most important transfer to municipalities in Brazil, while FUNDEF is the acronym for Fundo de Desenvolvimento da Educação Fundamental (Basic Education Development Fund) and is a fund to finance education.[21] The idea is to understand whether the federal government tries to offset royalty payment by reducing other transfers. Columns 3 and 4 indicate that this does not occur since oil windfall is not associated with changes in both transfers. Naturally, we estimate a reduction of both transfers as a share of total budget since they do not increase while the total budget is boosted by royalty revenues.

Table 1.7 investigates how municipalities report to allocate revenue. Each column presents the coefficients from panel IV regressions of different types of expenses on royalty payments instrumented by oil output. Column 1 shows that for every Real received, 63 cents are allocated in current expenses,[22] while 23 cents are used for investments and 1 cent for debt amortization, but this last effect is not statistically different from zero. From the 63 cents used for current expenses, 19 cents or 30 percent is allocated to payroll and other direct labor costs, and 20 cents are spent with other types of labor and service hiring (see columns 3 and 4). These results indicate that oil-rich municipalities apply equivalent amount of resources on payroll and on "other labor and service contracts", which include consulting services, outsourced services and labor hired on a temporarily basis than on payroll. We interpret this result as a reflection of law restrictions to the use of royalty revenues, which do not allow municipalities to use royalty revenue to hire public employees on a permanent basis. A way to circumvent this restriction is to hire people through other means. When we disaggregate "other labor and service contracts" by its components,[23] we see that the bulk of this expense is used to pay for

[21]FUNDEF is composed by municipal, state and federal contributions whose resources are redistributed to municipalities according to the number of school enrollments to finance education expenses. In 2007, FUNDEF was replaced by FUNDEB.

[22]These include all direct and indirect labor cost, interest payments and other current expenses

[23]Consulting services, outsourced services and labor hired on a temporarily basis (locação

outsourced services provided by companies (results not shown and available under request). This budget line can include several expenses, including two famous expenses in oil-rich municipalities: free live concerts and labor hiring through NGOs. Both expenses are usually cited by the media in scandals about the use of public funds in oil-rich municipalities and have been object of police investigation.[24] Panel B shows the impact of oil windfall on each expense as a share of total revenue. We see that oil revenues do not affect much the composition of public budget. Payroll expenses were slightly reduced as a proportion of total budget while investments suffered a small percentage increase.

Columns 6 to 10 offer another way to look at budget allocation by examining the destination of expenses. We observe that local governments report spending similar amounts in all areas, with the exception of transportation. Expenses with administration and planning are the main destination of oil revenues, receiving 21 cents of every Real received as royalty payments, followed by housing and urbanization (18 cents), health and sanitation (17 cents), education and culture (16 cents) and transportation (2 percent but not statistically different from zero). This implies that the areas that receive the largest improvements are housing and urbanization (41 percent increase in expenses for each standard-deviation increase in royalty revenue), followed by administration and planning (33%), health and sanitation (30%) and education and culture (19%). As a share of total expenses, Panel B indicates that education and health expenses were slightly reduced, while housing and urbanization increased a little.

Although this analysis so far offers insight into how municipalities apply oil windfall, we cannot use it as strong evidence of public goods provision. We have two main concerns with these data. First, the simple report that the municipality spent resources on a service does not necessary imply that the service was delivered in an efficient way. Our second concern is related to the fact that data on municipal public finance are self-declared by municipalities to the Brazilian National Treasury and some municipalities do not report their finances every year.[25] Campos dos Goytacazes, the largest recipient of royalty

de mão-de-obra + contrato por tempo determinado).

[24] In 2008, the federal police arrested 14 people in Campos dos Goytacazes charged with fraud in public procurement of hire outsourced services. In particular, two companies received about R$ 15 million to organize live concerts in the city with non-famous singers. In addition, Campos dos Goytacazes' mayor between 2005 and 2008 is charged of using NGOs and Foundations to divert more than R$ 200 million by hiring 16,000 outsourced employees. See `http://oglobo.globo.com/pais/mat/2008/05/30/ministerio_publico_federal_pede_justica_afastamento_dos_17_vereadores_de_campos-546596081.asp`

[25] Caselli and Michaels (2009) use 2001 values to impute the missing observations for 2000 in order not to lose many municipalities. We do not perform any imputation. We do not

revenues in absolute terms, for instance, only disclosed information on its public expenses on 2000 and 2006.[26] If oil benefited municipalities have a higher probability of not disclosing their public accounts, this can limit the capacity of these data to inform how municipalities are investing royalty revenues. Indeed, a regression of the probability of declaring FINBRA on a dummy on whether the municipality is an oil producing site (onshore or offshore) shows that producers's municipalities have a 4.5 percentage point lower probability of disclosing their public accounts (results not shown).[27]

With these caveats in mind, we turn to look to de facto public good provision.

1.5.4 Public Goods and Service Provision

Public Employment

A major destination of public expenses is the payroll. In order to shed light on public employment trends, Figure 1.5 shows the evolution of the median number of municipal employees per 1000 habitants in coastal producing and non-producing municipalities from 1997 to 2008. We see that although the median levels in the two groups of municipalities are quite similar in 1997 and 1998, they began to diverge in 1999, exactly when municipalities were most affected by the the large boost in royalty payments caused by the Oil Law.[28] Both groups increased substantially the number of public employees, but producing municipalities began to increase municipal public employment earlier and did it at a faster pace.

Table 1.8 examines whether the largest increases in municipal public employment occurred in municipalities benefited by the highest increases in royalty payments. It shows the results of IV regressions covering 1997-2008 period and use population, municipality and year effects as controls. In column 1, the dependent variable is the number of municipal employees per 1,000 habitants on September 30th. We use the employment level on September 30th because this is the record available closest to the election,

need it because we use several years of data, and we do not think this is appropriate as municipalities can allocate their budget in different ways from one year to another.

[26]The only record for "other labor and service contracts" is from 2006. In this year, this municipality spent R 387 million with these contracts, which corresponds to 31 percent of its total expenses or 122 percent of its payroll.

[27]This result is not robust to the inclusion of municipalities fixed-effects.

[28]Although Oil Law was enacted in June 1997, decree 2.705/98 which detailed the rules for paying the new parcel was just enacted in August 1998. The incremental part of royalty payments was paid for the first time in October 1998 because royalties are due two months after production. This information was provided by ANP technicians.

which takes place every four years in the first weekend of October.[29] Column 1 shows that for each R$ 1,000 per capita received, municipalities hire more 7.22 public employees per 1,000 habitants. This result is highly statistically significant (standard error=1.44) and quite important in economic terms. It implies that municipalities hired more 3.4 employees per 1000 habitants for every standard-deviation increase in royalty revenues, which is equivalent to an annual average growth of 10 percent in the number of public employees. Alternatively, this means that oil-rich municipalities on average multiplied the number of employees by more than two-fold in the twelve years under analysis. In the annex Table 1.11, we show that this estimation is robust to alternative measures of public employees, to different samples and to the inclusion of outliers. In particular, the estimate for the royalty impact on municipal employment is quite similar if we use the 'Perfil dos Municípios Brasileiros: Gestão Pública" database, a survey carried out by IBGE that investigates various aspects of the public administration, such as budgetary and planning procedures, and the number of public employees.[30]

Note that municipalities are forbidden to use royalty income to hire employees on a permanent basis. However, it is widely believed in Brazil that a large share of royalty revenues was used to hire employees.[31] In practice, municipalities have several options for hiring more employees: they can reallocate expenses in order to use the regular budget to pay for hirings, they can bring in temporarily employees or they can hire people indirectly, by establishing contracts with companies which hire people in their place (see footnote 24 on corruption scandals related to this last point). Since the data on Ministry of Labor only consider direct employees, these results should be viewed as a lower bound for the effects on royalties on public employment.

Column 2 in Table 1.8 shows the results of a regression which assesses whether oil windfall affected municipal public sector wages between 1999 and

[29]The RAIS database includes the information on the employment level on December 31st but also discloses monthly hirings and firings. We calculate the level on September 30th as EmploymentLevel9/30 = EmploymentLevel12/31 - (HiringOctNovDec - FiringOctNovDec). In addition, we did a correction in this measure to account for huge variations in reported employment levels in certain years. Since we believe that these drastic variations are misreports, we replaced by missing any record that reports an annual decrease of more than 75% in the number of employees followed by an increase of more than 200% in the following year. As a result, we lose 60 observations out of 1864 in the sample that includes only coastal municipalities. We performed this correction because we don't want artificial jumps in employment level to affect within-estimates. However, the result is robust to the use of corrected or uncorrected measure.

[30]This research was carried out in 1999, 2001, 2002, 2004, 2005, 2006 and 2008.

[31]See, for instance, an article at Estado de São Paulo: "Lucro com petróleo banca farra de contratacões em municípios" (Oil revenues support excessive employment in municipalities), at `http://www.estadao.com.br/estadaodehoje/20080414/not_imp156256,0.php`

2008.[32] In order to account for differences in price levels among municipalities, we use the ratio between the average wage in public sector and the average rate in the private sector as a measure. The average of this variable is 1.17 in Brazil for the period from 1999 to 2008, indicating that public employees earn, on average, 17 percent more than private sector employees.[33] Column 2 shows that oil windfall raises the relative public-private wage, which increases by 0.06 for each R$ 1000 per capita received. However, this estimate is quite noisy (standard error=0.06) and is not statistically different from zero.

In column 3 to 5 we shed light on the composition and quality of the payroll increase. Columns 3 and 4 divide the number of employees between those with and without tenure. Column 3 indicates that the effect on the number of employees with tenure is small and not statistically different from zero. Column 4 shows that most of new employees (96% percent) were hired on a temporary-basis and don't have tenure. A one-standard-deviation increase in royalty payments is associated with the hiring of more 6.9 employees without tenure per 1000 habitants, which represents an average annual increase of 58 percent. Both results are consistent with the fact that, by law, municipalities cannot use oil windfall to hire employees on a permanent basis.

Column 5 shows the results of a regression that uses the percentage of public employees with a college degree as a dependent variable. The point estimate is negative and indicates that in oil-rich municipalities, a one-standard-deviation increase in royalty revenue promotes a decrease of 1 percentage point in the percentage of public employees with a college degree. However, this estimate can only be distinguished from zero at a 13 percent confidence level. In order to understand the significance of this result, it worth mentioning that the public sector in all Brazilian municipalities suffered a boost in the period under analysis. Between 1999 and 2008, municipal employment in per capita terms increased 64 percent (from 22 to 36 employees per 1000 habitants). There was also a major improvement in the average educational level: the percentage of employees with college degrees changed from 7 percent to 25 percent. What our results indicate, therefore, is that oil-rich municipalities experienced a even starker growth in public sector and that, even though they also improved the educational level of its employees, they did so at a more reduced level than other Brazilian municipalities. We cannot tell whether this difference is a consequence of intentional decisions by public authorities to hire people with low levels of education or whether it is

[32] This measure is not available for 1997 and 1998.

[33] The relative wage suffered a huge increase in the period under analysis. In 1999, the first year in our sample, the relative wage in Brazil was 0.95. In 2008, this ratio jumped to 1.35.

a consequence of a supply constraint in the number of habitants with college degrees in oil-rich municipalities.[34]

In sum, the results present on Table 1.8 indicate that oil windfall is associated with a huge expansion in the public sector and that the majority of new employees don't have tenure.

Education and Health Supply

Table 1.9 looks at the impact of oil windfall on education outcomes. In all regressions, royalty value is instrumented by oil output and population, and we use year and municipal dummies as controls. In Panel A we look at the contemporaneous effect of royalty payments, while in Panel B we use a 2-year lag in order to account for the fact that some investments might take longer to take effect. Column 1 investigates whether the oil windfall was used to increase the number of professionals in education services. We see that oil windfall is associated with an increase in the number of professionals who work at schools. Panel A indicates that municipalities hire more 0.46 education professionals per 1000 habitants for every standard-deviation increase in royalty payments, which represents an increase of 5 percent. This effect is even larger if we estimate the impact of royalty payments received two years earlier. Panel B indicates that a one standard-deviation increase in royalty payments is associated with 1.1 more education professionals two years later, which is equivalent to a 12 percent increase.

In the remaining columns of Table 1.9, we regress school enrollment, three indicators of education supply (number of school per habitants between 5 and 19 years old, percentage of teachers with college degree and number of school hours per day) and two indicators of education performance (percentage of students with slow school progress and school dropout) on royalty revenue per capita. For most of the indicators, the period of analysis is from 1996 to 2006, but we analyze shorter periods for some outcomes due to data constraints. Neither Panel A nor Panel B shows that oil windfall improves any of the education outcomes under analysis.

Overall, Table 1.9 indicates that oil windfall increases the number of education professionals, corroborating the previous results that oil royalties increase the number of public employees, but has negligible effects on other

[34] A supply constrain may emerge in two cases. If fewer people in oil-rich municipalities have college degrees, local governments would not be able to hire enough highly-skilled people. However, this does not seem to be the case since educational levels in oil-rich municipalities are higher than those in non-recipients in the year 2000 (4.31 years of schooling in comparison with 4.07). But even with better levels of education in oil-rich municipalities, a supply constraint would emerge if the additional public sector demand is more than the additional level of people with a college degree.

education outcomes that indicate education supply and performance. Our results are in accordance with Caselli & Michaels (2009) paper, which finds that the only effect of oil windfall on education outcomes is through the increase in the number of teachers. We use a different database and find a similar result.

Turning to health outcomes, Table 1.10 looks at whether oil windfall is associated with an increase health resources. In this Table, we exclude the three largest beneficiaries of royalty revenues.[35] Again, Panel A looks at the contemporaneous effect of royalty payments, while in Panel B we use a 2-year lag in order to account for the fact that some investments might take some time to take effect. Column 1 indicates a positive impact on the number of health professionals per 1000 habitants. A one standard-deviation increase in royalty payments is associated with 0.35 more health professionals if we use the contemporaneous value of the royalty value (Panel A) or with 0.56 more employees if we consider a 2-year royalty lag (Panel B). This represents a considerable boost in the number of health employees, since these estimates imply an annual increase of 22 percent and 35 percent in the number of health professionals, depending on the royalty measure we use. Columns 2 and 3 investigates whether the increase in health expenses shown in Table 1.7 were accompanied by more health clinics or hospitals administered by local governments. We don't have a complete series for the period under analysis and these regressions cover data from 1998 to 2002 plus 2006 and 2008.[36] Both Panel A and Panel B show that oil windfall is not associated with increases in the number of health clinics or hospitals per 100,000 habitants.

1.6 Conclusion

This chapter explains Brazil's offshore oil boom and investigates how oil royalties affect producing municipalities. We show that oil production has little economic impact on the municipalities, other than in the public sector. Oil revenues increase municipal revenue directly and also positively impacts tax revenue. We don't find any evidence that the federal government tries to offset royalty transfer by reducing the two other main transfers made to municipalities, which is a reasonable result since FPM and FUNDEF allocation

[35] A closer look at the data reveals that Quissamã and Carapebus promoted a substantial increase in the number o health clinics between 1998 and 2000. These municipalities are the first and third largest beneficiaries of royalty revenues. Since their performance is sufficient to drive all the results we decided to exclude the top three royalty beneficiaries in this exercise rather than the top two.

[36] We add two databases to construct number of clinics and hospital series. Data from 1998 to 2002 is from Cadastros Extintos do SUS, while data from 2006 and 2008 was gathered from CNES database. Results for number of hospitals should be interpreted with caution because it is not clear that this variable is comparable in both series.

follows independent and fixed rules. Municipalities report having increased all their expenses but do not change their budget composition much. According to municipal reports, the areas that receive the largest improvements in expenses are housing and urbanization (41 percent increase in expenses for each standard-deviation increase in royalty revenues), followed by administration and planning (33%), health and sanitation (30%) and education and culture (19%).

Looking at de facto provision of public goods and services, we observe that the major destination of oil revenues is the hiring of municipal employees. Our results indicate that oil-rich municipalities increased the number of public employees by 10 percent per year on average for each one-standard deviation increase in royalty revenues, which means that on average they multiplied the number of employees by more than two-fold in the twelve years under analysis. The bulk of these new employees don't have tenure, which is consistent with the fact that, by law, municipalities cannot use oil windfall to hire employees on a permanent basis.

The analysis of education and health supply indicates that some of the new employees were hired to provide education and health services. The comparison of results indicates that among the new public employees, 14% were hired to provide education services and 11% to supply health services. Considering that, on average, 25% of municipal employees in Brazil are related to education supply and 5% provide health services, these results indicate that a reasonable number of health professionals were hired. However, the duties of the other 75% of new hired employees is an open question. Some of them are probably hired to provide administrative and bureaucratic services but there is no way to assess if these services have been improved.[37] Other outcomes of education and health supply do not indicate any significant improvement in health and education. The results for health resources are particularly striking since this area received a 30% increase in expenses, according to municipality reports.

The analysis of oil royalties' impact on public goods and services presented here is not exhaustive due to data constraints. Ideally, we would like to investigate whether oil revenues were translated into more urban infrastructure such as electricity, running water, sewage, housing quality and pavement. Unfortunately, most of this information is only provided at the municipal level by the Brazilian Census, and we need to wait for the results of the 2010 Census to be disclosed. However, the modest improvements in education and health,

[37]We can rule out the possibility that extra employees are being hired to promote security since is the responsibility of state governments. Only the state capitals have a police force.

which are the main areas under municipal authority, suggest that municipalities have created few improvements in living standards. This result is even starker if we consider the size of the windfall in the last twelve years. Therefore, our results indicate that oil-rich municipalities lost a great opportunity to develop, although they do not suggest that municipalities are worse due to oil windfall, which would be necessary to support a resource curse story.

This research is particularly important for policy-making in Brazil and countries that discover new natural resource fields. Oil revenues are likely to be magnified by recent oil discoveries in Brazil. One new field discovered in 2007 (the Tupi) is expected to produce between 5 billion and 8 billion barrels while a new field discovered in 2008 might contain as much as 33 billion barrels (Economist (2008)). As noted by Economist (2008), "This would make it the third-largest field ever found and would raise Brazil to eighth position in the global oil rankings". These announcements are also stimulating a debate over the best use of royalty revenues and its distribution, which requires empirical evidence in order to inform the policy debate.

The next chapter investigates whether local politics are affected by oil windfall. We also analyze whether the huge increase in the number of public employees had a political motivation.

Table 1.1: First-stage

Dependent variable:	Royalty per capita		
	All municipalities	Coastal municipalities	Producing municipalities
	(1)	(2)	(3)
Oil output per capita	0.028	0.028	0.027
	(0.002)***	(0.002)***	(0.002)***
Constant	0.000	0.036	0.027
	(0.002)	(0.016)**	(0.023)
Observations	25857	1882	1486
R^2	0.602	0.686	0.678
Municipalities	2157	157	124
F-stat	252.7	234.0	241.9

Notes: The results presented in columns 1-3 are from regressions that cover the period from 1997 to 2008 and include municipal and year effects as controls. Column 1 includes all municipalities from the nine oil producing states. Column 2 includes municipalities on the coast of the nine producing states, while column 3 sample is composed only by oil producing municipalities (offshore and onshore). Royalty and oil output data are measured in R$ 1000 per habitant and are deflated by the consumer price index, representing 2008 values. Robust standard errors clustered at the municipality are reported in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence. F-stat is the Kleibergen-Paap Wald rk F statistic for a weak instrument test.

Table 1.2: Royalty Summary Statistics

	All oil producing municipalities	Oil producing municipalities on the coast	Non-producing municipalities
	(1)	(2)	(3)
Number of municipalities			
1996	103	56	2,050
2000	106	60	2,053
2004	106	60	2,053
2008	123	71	2,036
Average royalties per capita (R$)			
1997-2000	133	189	2
2001-2004	375	545	6
2005-2008	478	697	10
Royalty standard-deviation (R$)			
1997-2000	346	451	22
2001-2004	838	1,070	44
2005-2008	1,026	1,300	61
Royalties / Municipal revenue			
1997-2000	9.0%	10.9%	0.2%
2001-2004	15.4%	18.0%	0.4%
2005-2008	14.6%	18.0%	0.6%

Notes: This table reports the number of municipalities, average per capita royalty payments, royalty standard deviation and the share of oil royalties on municipal revenue for the three political mandates under analysis and for three group of municipalities. Column 1 includes all oil producing municipalities in Brazil that produce onshore and/or offshore oil. Column 2 is a subgroup of column 1 and includes all oil producing municipalities located on the Brazilian coast. Column 3 contains municipalities that do not produce oil and are located in one of the nine oil producing states in Brazil under analysis (CE, RN, AL, SE, BA, ES, RJ, SP and PR).

Table 1.3: Municipal Characteristics

	All municipalities in oil producing states			Coastal municipalities in oil producing states		
	Oil producers	Non-producers	Dif.	Oil producers	Non-producers	Dif.
	(1)	(2)	(3)	(4)	(5)	(6)
Number of municipalities	103	2050		56	103	
Socio-demographic characteristics						
Level 1991						
Population	68,214	37,138		104,911	138,673	
% urban population	0.65	0.56	***	0.68	0.63	
Average years of schooling	3.16	3.07		3.49	3.35	
% of illiterate (pop > 25 years)	0.41	0.37	**	0.37	0.39	
Household income per capita	105	136	***	125	137	
Poverty rate	65	55	***	60	58	
Gini index	0.53	0.52	*	0.54	0.52	**
Human Development Index	0.58	0.61	***	0.6	0.6	
Infant mortality	0.09	0.07	***	0.08	0.08	
% of households w/ electricity	0.81	0.77		0.82	0.78	
% of households w/ water pipes	0.48	0.59	***	0.53	0.53	
Variation between 1991-2000						
Population	0.21	0.1	***	0.28	0.29	
% urban population	0.15	0.21		0.09	0.18	
Average years of schooling	0.43	0.43		0.42	0.46	
% of illiterate (pop > 25 years)	-0.29	-0.29		-0.31	-0.32	
Household income per capita	0.34	0.38		0.37	0.41	
Poverty rate	-0.16	-0.18		-0.19	-0.14	*
Gini index	0.06	0.08		0.06	0.12	***
Human Development Index	0.17	0.15	*	0.16	0.18	
Infant mortality	-0.31	-0.37	***	-0.33	-0.33	
% households w/ electricity	0.19	0.26	*	0.21	0.2	
% households w/ water pipes	0.66	1.65		0.67	0.79	
Level 1997						
Num of public employees (1000 hab)	24.1	23.8		21	20.7	
Revenue net of royalties (R$ pc)	708	686		831	689	
% educ. expenses on total budget	0.27	0.27		0.27	0.25	
% health expenses on total budget	0.15	0.17	**	0.14	0.16	
Political characteristics (1996)						
Party reelection	0.27	0.21		0.27	0.18	
Number of candidates	3.81	2.99	***	4.09	4.35	
Effective number of candidates	2.43	2.22	***	2.45	2.42	
Margin of victory	0.14	0.17	*	0.14	0.18	
Candidates's aver. years of schooling	12.1	11.7		11.9	11.8	
% candidates with college degree	0.37	0.37		0.37	0.35	
Geographic Characteristics						
Latitude	-11.4	-17.3	***	-13	-14.8	
Longitude	38.5	44.7	***	39.5	40	
Altitude	48.4	432.6	***	22.3	20.2	
Distance to state capital	100.9	260	***	105.5	119.2	

Notes: This table presents a comparison of the mean socio-demographic, political and geographic characteristics of oil producing and non-producing municipalities. Columns 1-2 compare all municipalities from the nine oil producing states under analysis (CE, RN, AL, SE, BA, ES, RJ, SP and PR) and columns 4-5 compare municipalities on the coast of these states. Column 3 (6) indicates whether the difference between columns 1-2 (4-5) is significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table 1.4: Political Alignment and Timing of Oil Field Discoveries and Initial Output

	Year of discovery	Year of initial output	Gap between initial output and discovery (days)	Gap between initial output and discovery (years)
	(1)	(2)	(3)	(4)
Municipality aligned with federal government	-0.010 (0.014)	0.002 (0.017)	82.3 (403.0)	0.14 (1.00)
Party: PRB	-0.001 (0.043)	-0.087 (0.042)**		
Party: PDS/PP/PPB	-0.027 (0.031)	-0.008 (0.034)	-49.2 (549.3)	0.20 (1.36)
Party: PDT	-0.017 (0.036)	-0.055 (0.037)	706.2 (504.6)	2.07 (1.23)*
Party: PTB	-0.017 (0.040)	-0.043 (0.033)	59.8 (475.6)	0.48 (1.16)
Party: PMDB	-0.033 (0.034)	-0.045 (0.033)	133.9 (442.7)	0.96 (1.08)
Party: PL/PR	-0.025 (0.033)	-0.010 (0.044)	266.0 (488.3)	0.99 (1.11)
Party: PPS	0.031 (0.063)	0.045 (0.050)	420.3 (475.8)	1.03 (1.29)
Party: PFL/DEM	-0.008 (0.033)	-0.009 (0.031)	-5.8 (468.8)	0.22 (1.13)
Party: PMN	0.102 (0.102)	-0.006 (0.062)	532.3 (453.3)	1.53 (1.22)
Party: PRN	0.235 (0.186)	-0.018 (0.038)	-475.3 (508.6)	-1.25 (1.32)
Party: PSB	-0.064 (0.039)	-0.046 (0.039)	-684.5 (547.6)	-1.55 (1.37)
Party: PSD	0.007 (0.056)	0.006 (0.039)	-52.5 (508.6)	0.25 (1.32)
Party: PV	-0.049 (0.032)	-0.190 (0.034)***		
Party: PSDB	-0.002 (0.030)	-0.012 (0.031)	-260.4 (470.0)	-0.44 (1.19)
Party: PT do B	-0.041 (0.032)	-0.075 (0.042)*		
Observations	2155	2155	69	69
R^2	0.042	0.038		
Municipalities	133	133	43	43

Notes: This table reports regression coefficients of the timing of oil field discoveries and initial production on municipal political alignment. In column 1, the dependent variable is equal to one if an oil field within municipality borders was discovered in the respective year, while in column 2 the dependent variable indicates whether oil began to be extracted on the respective year. Columns 3 and 4 dependent variables are the time gap in days and years, respectively, between discover the oil field and beginning its production. All regressions cover the period 1993-2008 and include a dummy indicating whether the party in power in the municipality is from the same political coalition of the federal government, party dummies, and year effects. Columns 1 and 2 also include municipal fixed effects. The omitted party is PT, the Workers Party and the one which run the federal government between 2003 and 2010. In columns 1 and 2, the sample comprises all Brazilian municipalities who had at least one oil producing field within their borders (onshore or offshore) between 1993 and 2008. Regressions present in columns 3 and 4 include only municipalities who had an oil field discovered within their borders in the respective year between 1993 and 2008. Robust standard errors clustered at the municipality are reported in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table 1.5: Oil Output Impact on Economic Activity

	Log population	Total	Number of firms pc: Manu-facturing	Number of firms pc: Trade	Services	Number of private employees pc	Public payroll pc	Private payroll pc	GDP pc	Non-industrial GDP pc
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A - All municipalities in oil producing states										
Oil output pc	0.0169	1.229	-0.073	-0.054	0.973	0.098	0.398	0.141	0.512	-0.004
	(0.00821)**	(1.510)	(0.079)	(0.519)	(1.032)	(0.114)	(0.108)***	(0.135)	(0.034)***	(0.007)
Observations	25857	25857	25857	25857	25857	21556	21556	21556	19399	19399
R^2	0.176	0.353	0.090	0.492	0.214	0.068	0.458	0.058	0.150	0.114
Municipalities	2157	2157	2157	2157	2157	2157	2157	2157	2157	2157
Panel B -Coastal municipalities										
Oil output pc	0.0009	2.452	0.124	1.049	0.969	0.161	0.279	0.212	0.502	-0.008
	(0.0056)	(1.741)	(0.099)	(0.639)	(1.117)	(0.130)	(0.074)***	(0.151)	(0.036)***	(0.010)
Observations	1882	1882	1882	1882	1882	1569	1569	1569	1412	1412
R^2	0.496	0.288	0.081	0.355	0.198	0.072	0.367	0.063	0.456	0.108
Municipalities	157	157	157	157	157	157	157	157	157	157
Panel C -Oil producing municipalities										
Oil output pc	0.0037	2.263	0.097	0.795	1.032	0.155	0.291	0.189	0.497	-0.006
	(0.0058)	(1.592)	(0.095)	(0.563)	(1.030)	(0.127)	(0.083)***	(0.146)	(0.036)***	(0.012)
Observations	1486	1486	1486	1486	1486	1239	1239	1239	1115	1115
R^2	0.510	0.359	0.151	0.398	0.172	0.128	0.402	0.091	0.523	0.107
Municipalities	124	124	124	124	124	124	124	124	124	124

Notes: Panel A regressions include all municipalities from the nine oil producing states under analysis. Panel B includes municipalities on the coast of the nine oil producing states, while panel C sample is composed only by oil producing municipalities. All regressions exclude the municipalities on the top 1% of royalty distribution (Quissamã and Rio das Ostras). The results presented in columns 1-5 are from regressions that cover period 1997-2008. Columns 6-8 include 1999-2008 years, while columns 9-10 cover 1999-2007 period. All regressions include municipal and year effects as controls. All measures are in per capita terms. Robust standard errors clustered at the municipality are reported in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table 1.6: Municipal Revenue

	Total revenue pc	Tax revenue pc	FPM transfers pc	FUNDEF transfers pc
	(1)	(2)	(3)	(4)
Panel A - R$ per capita				
Royalties pc	1.13	0.06	-0.01	-0.01
	(0.04)***	(0.01)***	(0.01)	(0.01)
Observations	1620	1619	1620	1354
R^2	0.73	0.12	0.63	0.63
Municipalities	157	157	157	157
Y mean	1.23	0.20	0.22	0.16
Panel B - Share of total revenue				
Royalties pc		0.0005	-0.04	-0.03
		(0.0051)	(0.01)***	(0.01)***
Observations		1619	1620	1354
R^2		0.08	0.44	0.23
Municipalities		157	157	157
Y mean		0.14	0.25	0.15

Notes: This table reports the effects of royalty payments on public revenues in municipalities located on the coast of the nine oil producing states (CE, RN, AL, SE, BA, ES, RJ, SP and PR). These regressions exclude the municipalities on the top 1% of royalty distribution (Quissamã and Rio das Ostras) and include only municipalities reporting most revenues and expenses. In all regressions, royalty value is instrumented by oil output and population, and use year and municipal effects as controls. All regressions cover 1997-2008 period. In Panel A, the dependent variables are measured in R$ 1000 per capita and, in Panel B, they are computed as a share of total revenue. Royalty data are measured in R$ 1000 per habitant and are deflated by the consumer price index, representing 2008 values. On column 3, FPM stands for Fundo de Participação dos Municipios . FPM is the most important transfer to municipalities in Brazil. FUNDEF on column 4 is the acronym for Fundo de Desenvolvimento da Educação Fundamental (Basic Education Development Fund) and is composed by municipal, state and federal contributions, whose resources are redistributed to municipalities according to the number of school enrollments to finance education expenses. In 2007, FUNDEF was replaced by FUNDEB. Robust standard errors clustered at municipality are reported in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table 1.7: Municipal Expenses

	Current expenses pc	Payroll pc	Other labor and service pc	Invest-ment pc	Debt amortization pc	Administration and planning pc	Education and culture pc	Health and sanitation pc	Housing urbanization pc	Transport-ation pc
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A - R$ per capita										
Royalties pc	0.63	0.19	0.20	0.23	0.01	0.21	0.17	0.17	0.18	0.02
	(0.13)***	(0.04)***	(0.04)***	(0.04)***	(0.01)	(0.06)***	(0.02)***	(0.04)***	(0.03)***	(0.02)
Observations	1620	1619	934	1620	1469	1620	1620	1620	1620	1620
R^2	0.61	0.40	0.41	0.22	0.17	0.18	0.57	0.59	0.28	0.04
Municipalities	157	157	154	157	157	157	157	157	157	157
Y mean	1.04	0.48	0.41	0.16	0.02	0.27	0.35	0.24	0.18	0.02
Panel B - Share of total revenue										
Royalties pc	-0.05	-0.06	-0.00	0.02	-0.00	0.00	-0.03	-0.01	0.02	-0.00
	(0.02)***	(0.01)***	(0.01)	(0.01)**	(0.00)	(0.01)	(0.01)***	(0.00)**	(0.01)*	(0.00)
Observations	1620	1619	934	1620	1469	1620	1620	1620	1620	1620
R^2	0.09	0.10	0.07	0.11	0.11	0.06	0.18	0.22	0.01	0.11
Municipalities	157	157	154	157	157	157	157	157	157	157
Y mean	0.86	0.39	0.28	0.12	0.02	0.21	0.31	0.19	0.14	0.02

Notes: This table reports the effects of royalty payments on public expenses in municipalities located on the coast of the nine oil producing states (CE, RN, AL, SE, BA, ES, RJ, SP and PR). These regressions exclude the municipalities on the top 1% of royalty distribution (Quissamã and Rio das Ostras) and include only municipalities reporting most revenues and expenses. In all regressions, royalty value is instrumented by oil output and population, and use year and municipal effects as controls. All regressions cover 1997-2008 period. In Panel A, the dependent variables are measured in R$ 1000 per capita and, in Panel B, they are computed as a share of total revenue. Current expenses include all direct and indirect labor cost, interest payments and other current expenses. Payroll expenses include direct labor expenses, payroll taxes, outsourced labor and other labor expenses, and do not include pensions. Other labor and service contracts include consulting services, outsourced services and labor hired on a temporarily basis (locação de mão-de-obra + contrato por tempo determinado). Payroll (column 2) and other labor and service contracts (column 3) are subdivisions of current expenses (column 1). Royalty data are measured in R$ 1000 per habitant and are deflated by the consumer price index, representing 2008 values. Robust standard errors clustered at municipality are reported in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table 1.8: Public Employment

	Number of employees	Relative public/private wage	Number of employees with tenure	Number of employees without tenure	Percentage of employees with college degrees
	(1)	(2)	(3)	(4)	(5)
Royalties pc	7.22	0.06	0.44	6.94	-0.02
	(1.44)***	(0.06)	(2.81)	(2.71)**	(0.01)
Observations	1807	1547	1807	1807	838
R^2	0.47	0.35	0.25	0.09	0.31
Municipalities	157	157	157	157	157

Notes: This table reports the effects of royalty payments on municipal public employment in municipalities located on the coast of the nine oil producing states (CE, RN, AL, SE, BA, ES, RJ, SP and PR). These regressions exclude the municipalities on the top 1% of royalty distribution (Quissamã and Rio das Ostras). In all regressions, royalty value is instrumented by oil output and population, year and municipal effects are used as controls. All employment variables are measured in per 1000 habitants. Columns 1, 3 and 4 cover 1997-2008 period and regression in column 2 includes 1999-2008 years. The dependent variable in column 5 is from the "Perfil dos Municípios Brasileiros: Gestão Pública" database and cover 1999, 2001, 2002, 2004, 2005, 2006 and 2008. The number of employees in column 1, 3 and 4 relates to all employees hired by the local municipality on September 30th. The relative public-private wage is the ratio between public and private sector wages. Columns 3 and 4 are subdivisions of column 1. Royalty payments are the value received in the contemporaneous year, are measured in R$ 1000 per habitant and are deflated by the consumer price index, representing 2008 values. Robust standard errors clustered at municipality are reported in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table 1.9: Education Supply

	Education professionals per 1000 hab (1999-2008)	Schools per young habitants (1999-2006)	Enrollment per young habitants (1999-2006)	Number of teachers with college degree (1996-2006)	School hours per day (1996-2006)	% of students with slow school progress (1996-2006)	School dropout rate (1996-2005)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Royalties pc	0.96	-0.00	10.92	-3.06	-0.04	0.02	-1.03
	(0.47)**	(0.16)	(18.99)	(3.30)	(0.03)	(1.40)	(1.04)
Observations	1524	1255	1255	1521	1706	1552	1550
R^2	0.19	0.12	0.03	0.51	0.09	0.70	0.27
Municipalities	157	157	157	157	157	157	157
Royalties pc	2.17	0.07	25.60	-0.40	-0.02	-0.78	-2.47
(2 years lag)	(0.82)***	(0.16)	(24.18)	(4.93)	(0.03)	(1.92)	(1.54)
Observations	1524	1255	1255	1521	1696	1552	1540
R^2	0.20	0.12	0.04	0.51	0.08	0.70	0.27
Municipalities	157	157	157	157	157	157	157

Notes: This table reports the effects of royalty payments on education supply in municipalities located on the coast of the nine oil producing states (CE, RN, AL, SE, BA, ES, RJ, SP and PR). Panel A reports the contemporaneous effect of royalty payments on different education outcomes as indicated in each column, while Panel B reports the effect of the amount received two years before. Education professionals include all public employees hired by the municipality who work at schools. The data are from RAIS database and refers to employment level on December 31st. Schools per young habitants and enrollment per young habitants are, respectively, the number of schools and enrollment in elementary school divided by the number of habitants between 5 and 19 years-old. Dropout rate refers to the average rate of student who drop out the school during the school year. The period covered in each regression varies as indicated in the columns due to data availability. Regressions exclude the municipalities on the top 1% of royalty distribution (Quissamã and Rio das Ostras). In all regressions, royalty value is instrumented by oil output and population, and use year and municipal effects as controls. Royalty and oil data are measured in R$ 1000 per habitant and are deflated by the consumer price index, representing 2008 values. Robust standard errors clustered at municipality are reported in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence. Robust standard errors clustered by municipalities are reported in parentheses.

Table 1.10: Health Supply

	Health professionals per 1000 hab	Municipal clinics per 100,000 hab	Municipal hospitals per 100,000 hab
	(1)	(2)	(3)
Royalties pc	0.70	-2.47	-0.59
	(0.18)***	(2.92)	(0.62)
Observations	1514	1207	1207
R^2	0.38	0.07	0.04
Municipalities	156	156	156
Royalties pc (2 years lag)	1.11	1.04	-0.58
	(0.39)***	(1.99)	(0.66)
Observations	1514	1207	1207
R^2	0.37	0.07	0.02
Municipalities	156	156	156

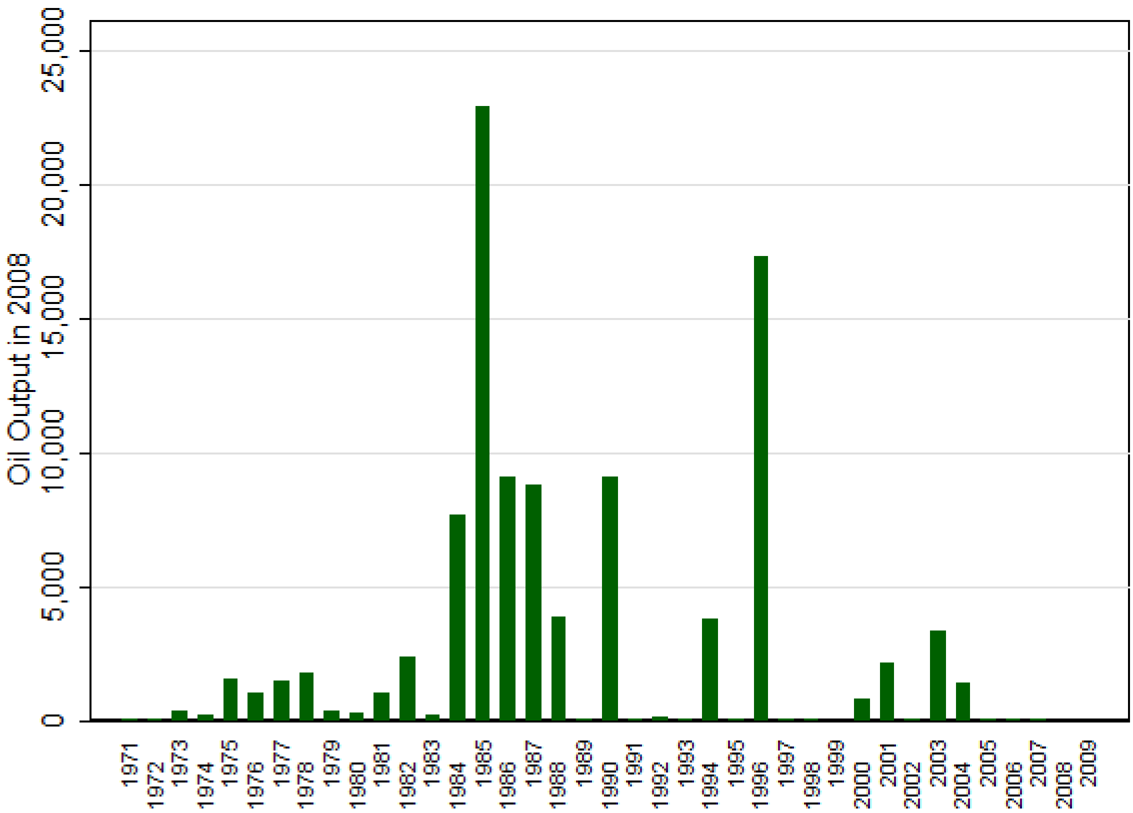
Notes: This table reports the effects of royalty payments on health supply in municipalities located on the coast of the nine oil producing states (CE, RN, AL, SE, BA, ES, RJ, SP and PR). Panel A reports the contemporaneous effect of royalty payments on different health outcomes as indicated in each column, while Panel B reports the effect of the amount received two years before. Health professionals include all public employees hired by the municipality who provide health services. The data is from RAIS database and refers to employment level on December 31st. Health clinics are the sum of 'unidades basicas de saude' and 'postos de saude'. Hospital units include 'Ambulatório de Unidade Hospitalar Geral' and 'Ambulatório de Unidade Hospitalar Especializada' in CNES database and 'Hospital Dia', 'Hospital Geral' and 'Hospital Especializado' in Cadastros Extintos do SUS database. We considered only health units managed by the local government. Regression presented in column 1 uses annual data from 1999 to 2008, while regressions presented in columns 2 and 3 are based on annual data from 1998 to 2002 plus 2006 to 2008. The regressions exclude the three largest beneficiaries of royalty revenue (Quissamã, Rio das Ostras and Carapebus). In all regressions, royalty value is instrumented by oil output and population, year and municipal effects are used as controls. Royalty and oil data are measured in R$ 1000 per habitant and are deflated by the consumer price index, representing 2008 values. Robust standard errors clustered at municipality are reported in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table 1.11: Robustness Checks

	(1)	(2)	(3)	(4)
Sample	Coastal municipalities	All municipalities	Oil producing municipalities	Coastal municipalities
Outliers	No	No	No	Yes
Public Employment				
Number of employees on 9/30	7.24	11.23	7.62	4.60
(RAIS corrected)	(1.44)***	(2.30)***	(1.67)***	(1.07)***
Number of employees on 9/30	6.74	10.84	7.09	4.32
(RAIS uncorrected)	(1.42)***	(2.21)***	(1.69)***	(1.04)***
Number of employees on 12/31	6.41	9.90	7.35	4.27
(RAIS corrected)	(1.70)***	(2.58)***	(2.07)***	(0.98)***
Number of employees on 12/31	5.92	9.53	6.85	3.99
(RAIS uncorrected)	(1.63)***	(2.48)***	(2.00)***	(0.94)***
Number of employees with	0.44	2.70	-0.19	0.32
tenure on 9/30	(2.81)	(2.85)	(3.01)	(1.90)
Number of employees without	6.94	8.55	7.82	4.32
tenure on 9/30	(2.71)**	(3.03)***	(3.13)**	(1.76)**
% of employees	-0.02	-0.02	-0.01	-0.02
with college degree	(0.01)	(0.01)	(0.01)	(0.01)**
Number of teachers 31/12	0.91	1.44	1.44	0.06
	(0.47)*	(0.54)***	(0.55)***	(0.83)
Number of physicians 31/12	0.70	0.77	0.51	0.33
	(0.18)***	(0.17)***	(0.21)**	(0.27)
Number of employees	6.87	7.85	6.54	5.44
(MUNIC)	(1.94)***	(2.08)***	(1.85)***	(1.12)***
Relative wage	0.06	0.07	0.03	0.09
	(0.06)	(0.06)	(0.07)	(0.04)**
Education supply				
Schools per	0.08	0.50	0.18	0.09
young habitants	(0.16)	(0.15)***	(0.16)	(0.04)**
School enrollment per *	25.76	22.94	40.99	9.23
young habitants	(24.18)	(22.66)	(26.73)	(12.67)
Num of teachers with	-0.36	5.81	8.09	0.30
college degree	(4.93)	(4.77)	(6.01)	(1.18)
Hours of school per day	-0.02	-0.06	-0.05	-0.03
	(0.03)	(0.06)	(0.06)	(0.01)***
% of students with slow	-0.80	-5.24	-0.40	-0.06
school progress	(1.93)	(2.01)***	(1.96)	(0.57)
School dropout	-2.46	-3.08	-0.85	-1.20
	(1.54)	(1.32)**	(1.68)	(0.50)**
Health supply				
Municipal clinics per 100,000 hab	1.51	0.20	0.05	-0.14
	(1.82)	(1.93)	(2.23)	(1.16)
Municipal hospitals per 100,000 hab	-0.51	0.33	-0.38	-0.54
	(0.59)	(0.50)	(0.59)	(0.35)

Notes: Each entry is the coefficient and correspondent robust standard-error of regressing the dependent variable indicate in the line on royalty revenue. All regressions use annual data and control for population, municipal and year effects. Each column indicates a different sample as explained in the top of the table. In all regressions, royalty value is instrumented by oil output. We use the contemporaneous value of royalty payments in public employment regressions and the 2-year lag in the education and health supply regressions. Outliers refer to the municipalities on the top 1% of royalty distribution (Quissamã and Rio das Ostras).

Figure 1.1: Oil Field Output in 2008 by Year of Field Discovery



Notes: This graph shows the distribution of 2008 oil output based on the year that the oil field was discovered (indicated on the x-axis). Oil output is measured in R$ million.

Figure 1.2: Oil Production 1994-2008

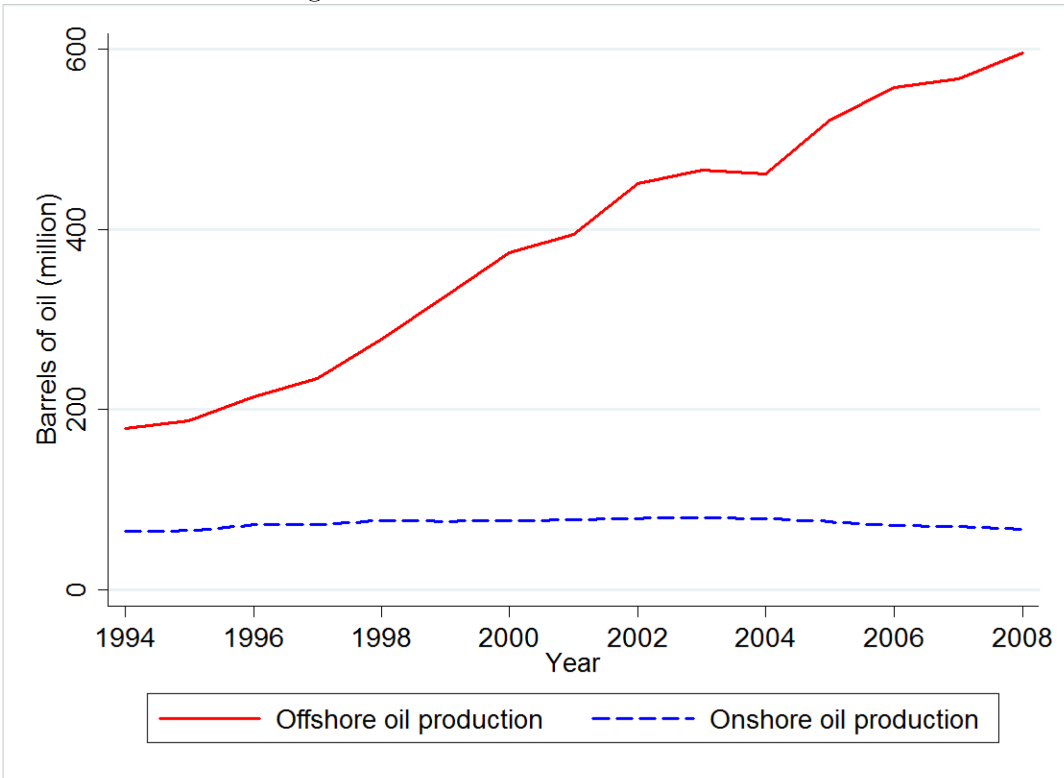
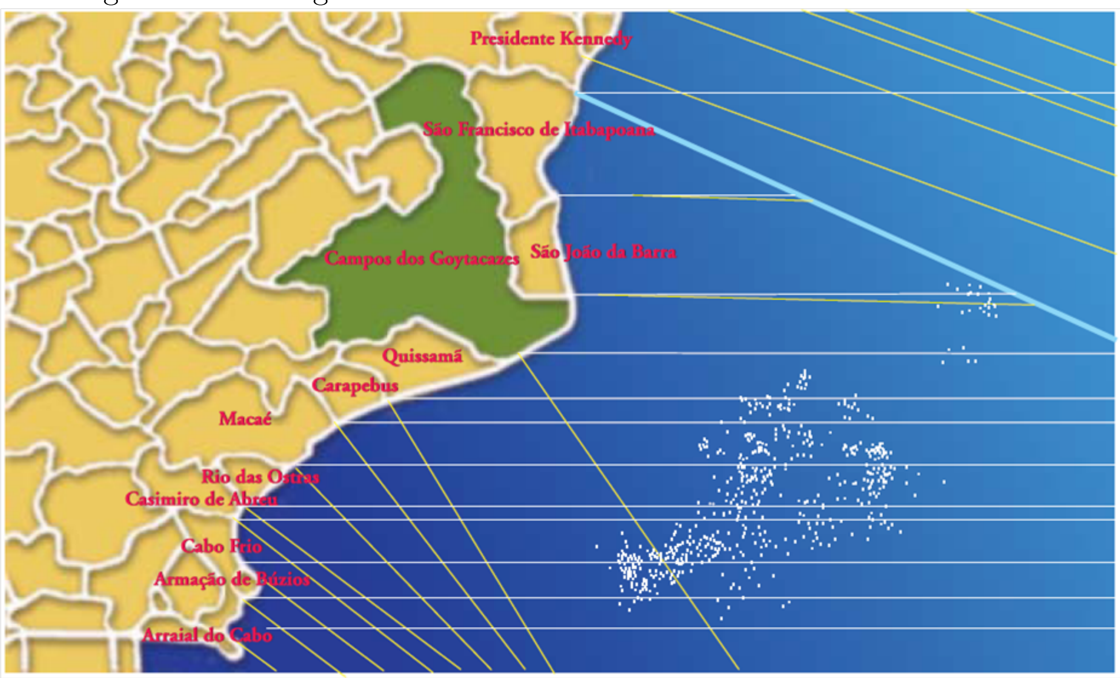


Figure 1.3: Orthogonal and Parallel Lines on Rio de Janeiro Coast



Notes: This figure shows the orthogonal and parallel lines that lies on the coast of the state of Rio de Janeiro. These lines are the criteria used to determine which municipalities face oil fields. The dots indicate oil wells. Source: ANP (2001b). Guia dos Royalties de Petróleo e do Gás Natural.

Figure 1.4: Location of Producing and Non-producing Municipalities

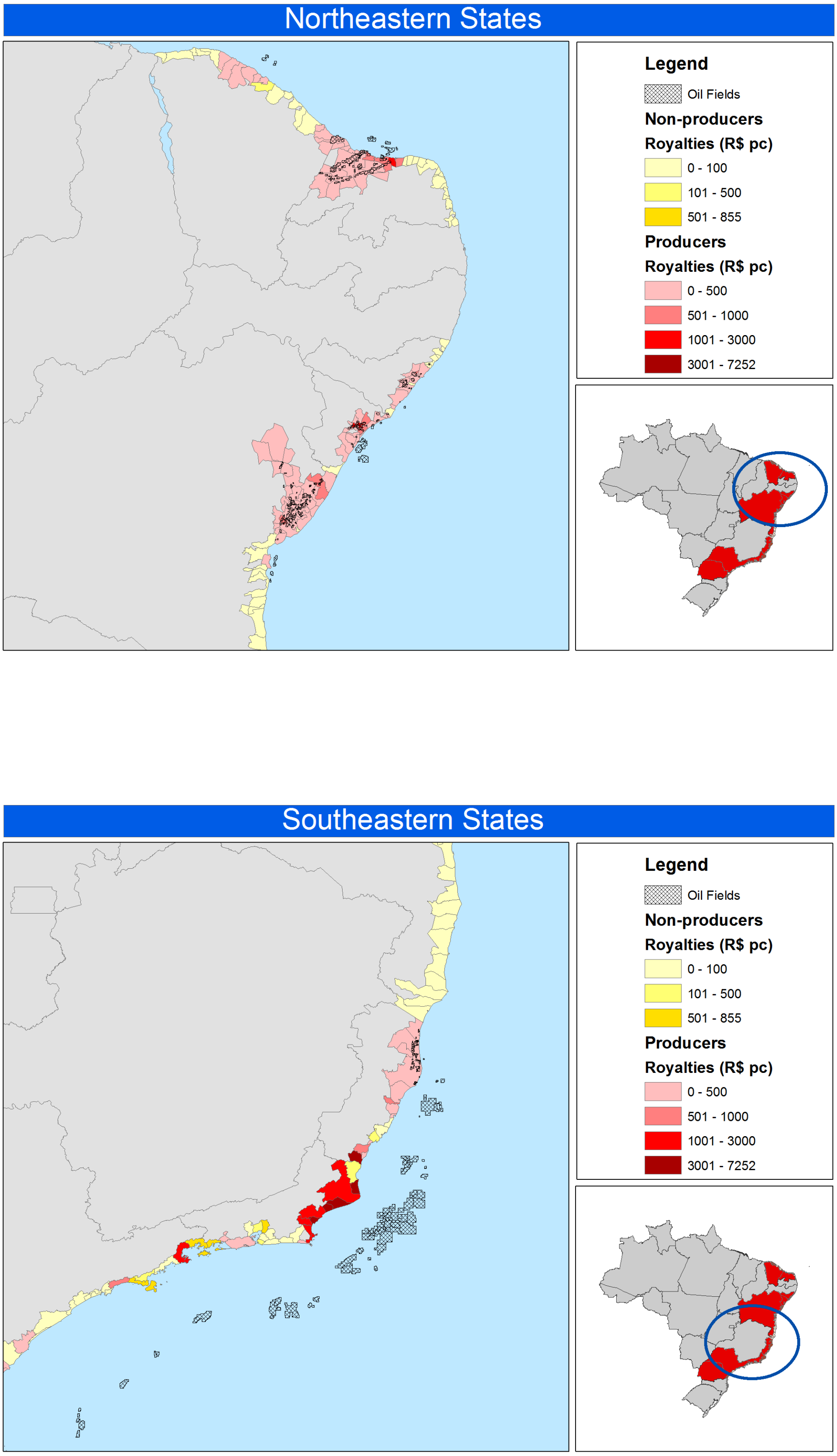
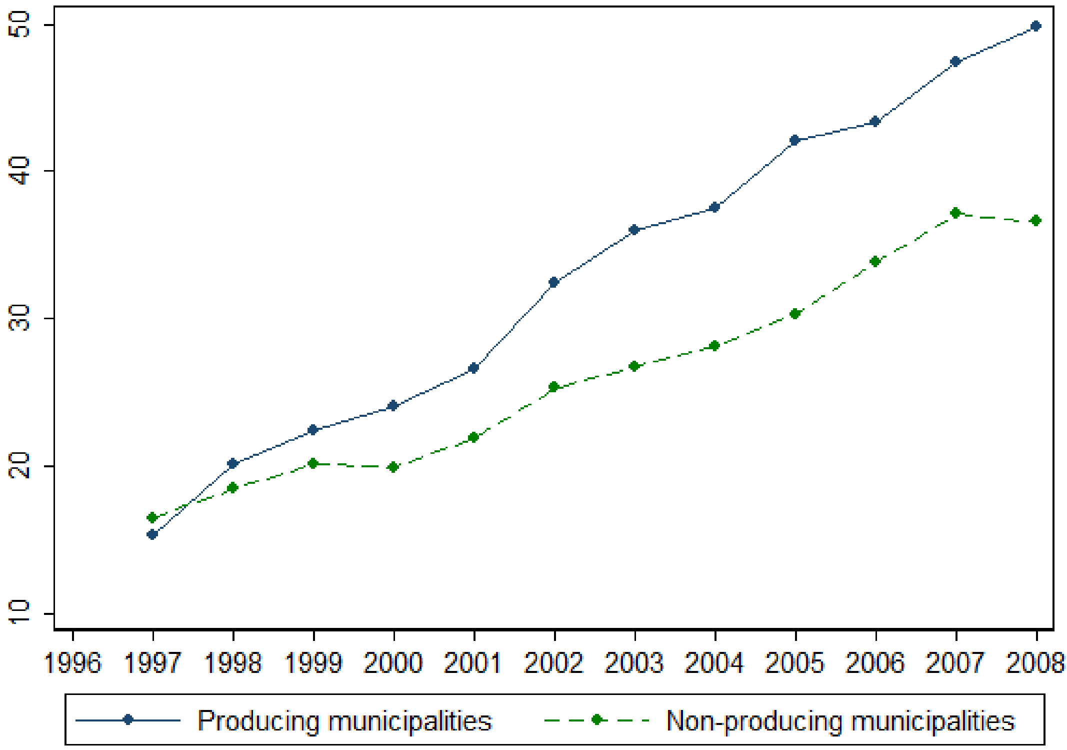


Figure 1.5: Municipal Employees in Oil Producing and Non-producing Municipalities 1997-2008



Notes: This figure shows the median number of municipal employees per 1000 habitants on September 30th between 1997 and 2008 for two group of municipalities. Producing municipalities are municipalities on the coast of the nine oil producing states under analysis that have oil extracted from an oil field within their borders in the reference year. Non-producing municipalities are the other municipalities on the coast of these nine oil producing states (those which do not produce oil).

2 Does Oil Make Leaders Unaccountable?

2.1 Introduction

Several studies argue that the limited effects of natural resource abundance on long run economic development should be explained by the behavior of those who control the state (Ross (1999); Caselli & Cunningham (2009); Caselli (2006); Robinson et al. (2006)). In particular, a large body of literature argues that natural resource wealth impairs democracy, perpetuates autocratic regimes, and induces misgovernance (Barro (1999); Jensen & Wantchekon (2004); Ross (2001), Tsui (2010)). Most of the studies is inspired by the experience of autocratic governments and focuses on understanding regime changes (Dunning (2008); Haber & Menaldo (2010)), how natural resource abundance can bring about political instability (Caselli (2006)) or can help autocratic rulers perpetuate their power (Acemoglu et al. (2004)). Much less is known about the political economy effects of resource abundance in a democratic context, where elections should make politicians accountable and political competition can balance an incumbent's power.

This chapter examines whether oil booms affect local democracy in Brazil's municipalities. Specifically, we study how electoral outcomes, the behavior of politicians in power, electoral competition and political selection change as municipalities are endowed with a fiscal windfall from oil boom. We do so by using a similar empirical strategy employed in the first chapter of this dissertation, i.e., we explore variation across municipalities benefited from Brazil's recent oil production[1] boom and new rules for distributing oil royalties[2] to drilling regions.

We begin the analysis by developing a theoretical model in order to understand how oil windfall affects politicians' and voters' behavior. In the model, voters know that the municipality receives oil royalties but they cannot perfectly assess the amount received. Voters can only observe the amount

[1]We use the term oil to denote oil and natural gas production since oil corresponds to the bulk of oil and gas production.

[2]We use the denomination royalty loosely throughout the paper to refer to royalties plus special quotas ("participações especiais". ANP calls the sum of both payments as "participações governamentais".

of public goods provided and they know that this depends on the total revenue and on the incumbent's ability, which is not observed. Therefore, oil windfall allows the incumbent to signal a higher ability and voters respond by reappointing the mayor for office. This incumbency advantage can persist as long as voters are sufficiently unaware about the royalty revenue. Once voters become more informed, the difficulty in signaling higher ability reduces the incumbency advantage as well as the incentive to provide more public goods, and mayors end up diverting more funds.

We take these predictions from the model to the data to test the validity of that story. We first analyze oil windfall impact on mayor and party reelection and we provide evidence that royalty payments create a large incumbency advantage in the short run. In 2000, the first election after the boom, when all mayors could run for reelection, a one-standard-deviation increase in royalty value increase reelection chances by 16 percentage points, which implies a increase of 32 percent in reelection chance. However, this effect disappears in the medium run since there is no incumbency advantage in 2004 and 2008. We then analyze political competition and selection and show that the limited impact on these outcomes indicates that the incumbency advantage estimated for 2000 should be explained by the behavior of who are in power rather than through a decrease in political competition or by changes on the pool of candidates. We follow by analyzing the timing and composition of the increase in public employment, which is the main destination of royalty revenues according to the results presented in chapter 1. We show that public employment increased in particular between 1998-2000 and 2002-2004, but the enlargement of public sector in the two years before the election explains reelection only in 2000. This result supports our model prediction, as long as we believe that voters interpret the increase in public employment as a signal of incumbent's ability only in 2000 and information about oil windfall increases over time. We show evidence that confirms these hypotheses by arguing that the pattern of public employment increase is not compatible with a clientelistic story. In addition, we show indications that the awareness level about oil windfall increased over the years and that mayors from municipalities with local media presence have more difficulty getting reelected in 2008.

Taken together, these results do not indicate that oil makes leaders unaccountable. Although oil windfall creates a large incumbency advantage in the election after the boom, voters reward incumbents by reappointing them to office as long as they are not completely informed of the size of the extraordinary revenue and see increases in public employment as an indication of mayor's ability. In the medium run, as information about the resources increases and a larger public sector does not translate into more public goods and services, citizens oust the incumbent and select new candidates. Thus, our results indicate that a democratic system is crucial to avoid the negative effects of resource abundance and that institutions such as elections, media presence and constraints on executive power play an important role in restraining the irresponsible use of oil revenues.

To the best of our knowledge, this is the first empirical paper that focuses on understanding the political economy effects of resource abundance on a democracy. Our paper is directly related to two theoretical works that analyze the mechanisms through which the natural resource abundance can affect politicians incentives in a democratic context. Caselli & Cunningham (2009) argue that revenue effect occur through two main channels: by increasing the value of staying in power and by increasing the competition for power. Robinson et al. (2006) show that incumbent politicians can spend revenues from natural resources in patronage in order to influence future elections.

This paper relates to recent empirical literature that aims to understand the political economy effects of resource windfalls. Vicente (2010) examines the effect of oil discovery announcements in São Tomé and Principe on measures of perceived corruption. Brollo et al. (2010) investigate the effect of federal transfers on reelection outcomes, political selection and corruption in Brazilian municipalities. They look at different types of federal transfers to municipalities and also show that they increase election outcomes, but, contrary to us, find an impoverishment of the pool of candidates.[3] Litschig & Morrison (2010) estimate that higher federal transfers in Brazil lead to higher spending and educational outcomes, which therefore improve incumbent party reelection probability. Our findings also complement a literature on voters' rationality. In particular, our work is related to Wolfers (2007) who presents a model where voters cannot discern between incumbent's competence and luck. We find results in line with his work, which shows that governors in oil-producing states are likely to be reelected following a rise in oil prices, while their counterparts in the rust-belt are likely to be ousted. However, his analysis does not allow a comparison between short and medium-term effects as we do in this study.

The remainder of the chapter is organized as follows. Section 2 present a brief case study of Campos dos Goytacazes in order to illustrate how oil windfall can impact local politics. Section 3 sketches a theoretical framework. Section 4 explains the empirical strategy that is quite similar to the one

[3]However, the mechanism highlighted in their work is different from ours. Their model states that an incumbency advantage arises due to the impoverishment of the pool of candidates, while in our model there is an incumbency advantage because voters are unable to assess royalty value.

employed in chapter 1 and the data. Section 5 presents the empirical findings. Finally, section 6 concludes the chapter.

2.2 Oil Royalties and Malfeasance: the case of Campos dos Goytacazes

To illustrate how oil windfall can impact the political environment of local economies we now briefly discuss the case of Campos dos Goytacazes, a municipality located in the north of Rio de Janeiro state and the largest beneficiary of royalty revenues in Brazil. It received R$ 1 billion or 24 percent of total royalties distributed to local governments in 2008.

Campos is known for being the political cradle of Anthony Garotinho, an ambitious politician who governed the state of Rio de Janeiro between 1998 and 2002. He was also the second runner-up in the 2002 presidential election. Garotinho started his political career as the mayor of Campos in 1989, two years after the city began to receive revenues from royalties. The oil revenues and his populist profile won him widespread popularity. In 1992, he elected his candidate for succession and in the 1996 ballot he came back to power, where he stayed for two years until successfully running for state governor.

During the 2000's, when oil windfall dramatically increased from R$ 50 million in 1999 to R$ 1 billion in 2008, the municipality witnessed a series of unique political events. The 2004 election was remarkable. There were reports of vote-buying, two radio stations were turned off and charged with illegal propaganda, R$ 316,000 in cash was found in one party's office the day before the election, people were arrested and charged with electoral fraud and federal troops were sent to the municipality in order to guarantee ballot security. In addition, the state governor, Rosinha Garotinho, moved the state office headquarters to Campos a few days before the election in order to influence its outcome. At the end, the incumbent's candidate won over Garotinho's candidate by a narrow margin, but both had their candidature suspended by the Electoral Court. The local legislature president assumed power and was elected mayor by a new election that took place in 2006.

The analysis of incumbents' behavior sheds light on the intention behind all this effort to get in office. Arnaldo Vianna, Campos's mayor from 1998 to 2004, is charged with having US$ 35 million in a private foreign bank account. He was accused of malfeasance by state attorneys and had his candidature for the 2008 election suspended. He became infamous for using public resources to finance free live concerts. His successor, Alexandre Mocaiber, was temporarily suspended from office in 2008 accused of fraud in public procurements. The federal police investigation estimated that R$ 240 million was misappropriated from public resources and that 20,000 public employees were illegally hired only in the first trimester of 2008.[4]

This type of story is not unique. Other oil-rich municipalities accumulate political scandals as well. Carapebus, the third largest recipient in per capita terms in 2007, almost replicated Campos history in its 2008 election. The frontrunner did not have his votes computed because the Electoral Court suspended his candidature due to improper use of public funds during his previous administration. A new ballot was set since the second place candidate in the election could not be nominated for mayor due to problems with the Justice Department. São Francisco do Conde, in Bahia, which is Brazil's largest per capita GDP due to the location of an oil refinery and 26th place in royalty per capita distribution, almost went to the 2008 ballot without candidates: three out of four candidates faced accusations of malfeasance. At the end, two candidates ran for mayor.

2.3 Theoretical Framework

This section develops a simple framework to understand voters' and politicians' behavior in municipalities affected by oil windfall. We extend Wolfers (2007) model by adding a second source of uncertainty and formalizing the politicians' problem when there are reelection concerns. The basic idea is that voters know that the municipality receives oil royalties but they cannot perfectly assess the amount received. Voters can only observe a fraction of the public budget and the amount of public goods provided. They know that public goods depend on the total revenue and on the incumbent's ability, which is not fully observed. Politicians care about private rents and have reelection concerns. We show that shocks that increase the budget but are not observed by voters create an incumbency advantage, because the resulting benefits are interpreted as due to incumbent's superior ability. In addition, the provision of public goods is positively affected by the unobserved level of royalty shock, while private rents only increase with shock variance.

The mechanism behind this model is that incumbent's main incentive to provide a higher level of public goods is to signal that he is an able politician and increase his reelection chances. The asymmetry of information on the size of public budget increases the attractiveness of this signaling device since more unobserved revenue facilitates it. By facing the opportunity of easily influencing the election, the mayor chooses to increase the amount of public goods and constrain the diversion of public goods in order to provide a strong

[4] Source: `http://noticias.uol.com.br/ultnot/eleicoes/eleito/campos.jhtm` and Globo On Line, 11/03/2008. "Prefeito de Campos é afastado do cargo e acusados de envolvimento em fraudes da prefeitura são presos".

signal.

The model also sheds light on how politician's incentives are different when the revenue shock is observed by voters. When voters are aware about the size of budget shock, the effects are non-linear. At low levels of budget revenue, the reelection chances increases with the size of observed revenue, but after a certain threshold, the effect becomes negative. In this circumstance, his reelection incentive is reduced and he chooses to pocket all the extra revenue.

2.3.1 Basic Model

There are two periods that are divided by an election. In every period, the municipality receives a budget shock whose total value is only observed by the politician in power. Voters are aware that the municipality receives royalties but they assess its value as b_t. However, the total amount also depends on a random shock θ_t, which is not observed by voters and is distributed according to $N(0, v_t)$. In addition, municipalities also receive a constant tax revenue and federal transfers, which generate the revenue T'. Hence, the total budget is composed by an observed part $T_t = T' + b_t$ plus an unobserved component θ_t, such that $B_t = T_t + \theta_t$.

Voters have the same preferences over the public good g.[5] The politician utility is $W_t = r_t + p_I R$, where R is the present value of politician's second period rents. Hence, the politician in power allocates the budget between public goods g and private rents r. Rents are constrained to be nonnegative and smaller than the total budget $0 \leq r_t \leq \bar{r} < B_t$. The government budget constraint is:

$$T_t + \theta_t = \frac{g_t + r_t}{a} \;\Rightarrow\; g_t = a(T_t + \theta_t) - r_t \tag{2-1}$$

where a is the politician's ability. A higher value of a indicates that the politician can provide more public goods or divert more money with the same level of resources. This ability is private information, permanent over time and is a random variable distributed according to $N(\mu, \sigma)$.

The time of the game is as follows: (1) Nature determines royalty value $b_1 + \theta_1$. (2) The politician in power determines r_1 knowing the value of $b_1 + \theta_1$ and his ability. g_1 is residually determined in order to satisfy the budget constraint. (3) Voters observe g_1 and T_1 but neither a nor θ_1. They also do not observe private rents r_1 but compute its value as r_1^e based on the available information. (4) Election takes place. If the incumbent is reelected, the ability of the politician in power remains a. If he loses the election, an opponent

[5]This public good is a generic definition of a vector of public services and goods provided by the municipality such as education, health services and infrastructure.

is appointed with a competence level drawn from the same distribution. (5) Period 2 rents are set and g_2 is residually determined. (6) Game ends.

In period 2, the incumbent has no reelection incentives and sets $r_2 = \bar{r}$ and $g_2 = a(T_2 + \theta_2) - \bar{r}$. In period 1, the politician in power faces a trade-off between pleasing voters and being reelected or diverting all the money for his own enrichment. His optimal decision depends on voters' behavior. Voters want to elect a high ability politician because this provides a high second-period utility. Therefore, voters rely on the observed value of public goods g_1, on their assessment of public budget T_1 and on their estimation of period 1 private rents r_1^e to evaluate the incumbent's ability. This information provides them with the signal $\tilde{a} = \frac{(g+r^e)}{T}$, whose variance is $\tilde{\sigma} = \sigma + \frac{\sigma v + \mu^2 v}{T^2}$. (In these expressions and the ones that follow we omit the subscripts that indicate period 1 in order to simplify the algebra). Voters rely on this signal and uses Bayes's rule to update their prior assessment of the incumbent's ability. They estimate the incumbent's ability as:

$$a^p = E(a/g, T, r^e) = \frac{\mu\tilde{\sigma} + \frac{(g+r^e)}{T}\sigma}{\tilde{\sigma} + \sigma} \tag{2-2}$$

$$= \frac{\mu(\sigma T^2 + \sigma v + \mu^2 v) + (g + r^e)T\sigma}{2\sigma T^2 + \sigma v + \mu^2 v}$$

A citizen will vote for the incumbent if the expected ability of the incumbent plus an idiosyncratic ideological bias for the incumbent $\delta_i \sim U[-\frac{1}{2\epsilon}, \frac{1}{2\epsilon}]$ is greater than the challenger expected ability:

$$E(a/g, T, r^e) + \delta_i > E(a) = \mu \tag{2-3}$$

Therefore, the probability that the incumbent is reelected is:

$$P_I = \frac{1}{2} + \frac{\epsilon\sigma}{\tilde{\sigma} + \sigma}\left[\frac{(g + r^e)}{T} - \mu\right] \tag{2-4}$$

The incumbent set rents in order to maximize his utility, $W_t = r_t + p_I R$, being constrained by the reelection probability (2-4) and the budget constraint (2-1). The first order condition is:

$$F.O.C.: \; 1 + \frac{\partial p_I}{\partial r}R = 0$$

where

$$\frac{\partial p_I}{\partial r} = \frac{\partial\left(\frac{\epsilon\sigma}{\tilde{\sigma}+\sigma}\left[\frac{g+r^e}{T}\right]\right)}{\partial r} = \frac{-\epsilon\sigma T}{\sigma(2T^2 + v) + \mu^2 v} < 0 \tag{2-5}$$

This expression shows that the equilibrium level of private rents is constrained by its marginal effect on reelection probability. Anything that decreases this marginal effect will increase the diversion of public funds because it reduces the punishment that the incumbent suffers in terms of reelection chances. Therefore, *the level of private rents increases with the variance of the revenue shock* (v)*, with the average of politician's ability* (μ) *and with election uncertainty (lower* ϵ*), while it decreases with the variance of political ability* (σ)*. An increase in the size of observed share of public budget* (T) *has a U-shape effect on rents* (See appendix for details).

In order to understand the intuition behind these results, it is necessary to understand first the source of incumbency advantage, whose equilibrium level is:

$$P_I^* = \frac{1}{2} + \frac{\epsilon\sigma}{\tilde{\sigma}+\sigma}\left[\frac{a(T+\theta)}{T} - \mu\right]$$

$$= \frac{1}{2} + \frac{\epsilon\sigma T^2}{(2\sigma T^2 + \sigma v + \mu^2 v)}\left[a + \frac{\theta}{T} - \mu\right]$$

This equation states that *the probability of being reelected increases with the incumbent's ability (a) and the value of royalty shock* (θ)*, while it is reduced with the variance of royalty shock (v) and with the average of politician's ability* (μ)*. The effect of the observed budget shock (T) on reelection probability is ambiguous.* This expression follows directly when we substitute the budget constraint (2-1) on the reelection expression (2-4), considering the fact that in equilibrium the incumbent optimal choice of r must be consistent with voters' conjectures regarding this choice: $r = r^e$. The partial effects of each parameter are shown in Appendix C.

Finally, the equilibrium level of public goods is:

$$g^* = a(T+\theta) - r^*$$

where r^* is implicitly determined by (2-5). This expression implies that *the period-1 level of public goods increases with incumbent's ability (a), with the value of royalty shock* (θ) *and with the variance of political ability* (σ)*, while it is reduced with the variance of royalty shock (v), with the average of politician's ability* (μ) *and with election uncertainty (lower* ϵ*). The effect of the observed budget shock (T) on the level of public goods is ambiguous.*

This model has two sources of asymmetry of information - politician's ability and the size of royalty shock - which reinforce each other and increase the incumbent's incentive to signal that he has a high level of ability. In order to better understand it, suppose first that the royalty shock (θ) is zero and set $\overline{g} = \mu T - \tilde{r}$ as the average level of public goods. In this case, only better-than-average politicians $(a > \mu)$ or politicians who restrict the diversion of public funds are able to provide $g > \overline{g}$. Hence, voters would correctly interpret $\overline{g}$ as a signal of high ability (or low corruption) and mayor would be reelected with probability greater than 1/2. This incumbency advantage increases with the ability difference between the incumbent and the challenger.

The royalty shock changes the incumbent's decision by increasing his capacity to signal that he is a high-ability politician. This revenue enables the mayor to provide a higher level of public goods and since voters do not observe the size of the shock, they interpret any $g > \overline{g}$ as higher political ability. Note that royalty revenue allows even incumbents with ($a < \mu$) to signal they are high-ability politicians.

The efficacy of this signaling device depends on the parameters of the economy. The incentive to signal increases with the variance of politicians' ability (σ) and decreases with the variance of the signal ($\tilde{\sigma}$). The intuition is that when σ is too high, voters know that the prior does not provide much information on politician's ability and, hence, give more weight to the signal in order to assess incumbent's ability (see expression 2-2). In this case, providing a high g is very effective to attract votes. This also explains why the level of private rents is lower when σ is larger. A similar argument applies to the effect of $\tilde{\sigma}$, which have the opposite effect of σ.

Both the size and variance of the royalty shock affect the mayor's decision. An increase in the variance of the shock (v) reduces the electoral advantage since voters recognize that they are not able to predict the size of total budget and therefore consider that the signal is a poor measure of the incumbent's ability. This motivates mayors to divert more funds. Large unobservable shocks (high θ) increase the incumbency advantage and the provision of public goods, while not affecting private rents.

The effect of the observed budget T on reelection and private rents is less straightforward. The size of revenue has two opposite effects on reelection probability, generating a inverted U-shape relationship between reelection probability and revenue. This happens because an increase in T reduces the size of the signal but also reduces its variance. For low levels of T, the reduction on the signal variance is sufficient to stimulate voters to rely on signal information to assess the incumbent's ability. This increases the attractiveness of the signaling device and make the incumbent divert fewer funds in order to provide a higher signal. As a result, the incumbency advantage increases. However, as T grows, the size of the signal is reduced. Hence, it becomes too difficult to signal high ability, which, in turn, increases the incentive to divert funds and give up reelection.

Therefore, this model predicts that the effect of a budget increase depends on whether this increase is observable or not. A positive budget shock unobservable to voters increases the supply of public goods and the incumbency advantage. In turn, an observable increase in the budget raises reelection probability and reduces private rents only when the budget size is small, having the opposite effects and the budget increases.

The last parameter which plays a role in an incumbent's decision is ϵ, which measures the election uncertainty. The more uncertain the election outcome (lower ϵ), the lower the electoral advantage and the larger the incentive to seeking private rents.

2.3.2 Discussion

The model sketched above provides predictions for empirical analysis. The main testable hypothesis is that as long as voters are unaware about the size of oil windfall, oil revenues should generate an incumbency advantage and an increase in the public good provision. But as long as voters are informed about the size of oil windfall (b_t or T_t increases relative to θ) this incumbency advantage should be reduced, as well as the provision of public goods. These predictions should be compared with the results shown in the first chapter that indicate that the only impact of oil windfall on public goods and services is the increase in the number of public employees. We can interpret public employment as a public good as long as we consider that voters appreciate the enlargement of the public sector. This can be true because voters believe that a greater number of employees is a precondition for improving public services such as health and education, or because they have ideological preferences for a larger state, or even because they assess a higher probability of being hired as a public employee. However, several authors have argued that public employment is a type of private transfer that politicians make in order to obtain political support (Alesina et al. (2000), Robinson & Verdier (2003), Robinson et al. (2006)). Therefore, in order to validate our model we also need to provide evidence that voters interpret public employment as a public good rather than a private transfer. We assess that issue and model predictions in the empirical section.

2.4 Empirical Strategy and Data

To understand the impact of royalties on local politics we analyze three political mandates: 1997-2000, 2001-2004 and 2005-2008. The empirical strategy is similar to the one employed in the first chapter. We follow an IV strategy where we instrument royalty value by oil output and focus on offshore production variation by looking only at coastal municipalities. However, we do not use municipal fixed effects due to the existence of term limits in Brazil.

The fact that mayors cannot run for two subsequent reelections implies that reelection estimates are conditional on being mayor in the first term. Hence, the sample of municipalities changes every election, which makes the within estimates hard to interpret. Therefore, we run the following equations to estimate royalty effect on political outcomes:

$$\begin{aligned} y_i &= \rho R_i + X_i\beta + u_i \\ R_i &= \gamma_1 Z_i + X_i\gamma_2 + \epsilon_i \end{aligned} \tag{2-6}$$

where y_i denotes municipality i political outcome (e.g. an indicator variable for whether the mayor was reelected, the number of political candidates), R_i indicates royalty value paid to municipality i, X_i is a vector of municipality characteristics such as latitude, longitude, altitude, distance to the state capital, dummy for state capital, population, population density, dummy for coastal municipality and state dummies, and u_i is a random shock. We use royalty and output values in the election year but the results are similar if we use the values accumulated in the political term.

In order to understand short and medium-term effects, we run one regression per election year. We should emphasize that the first political mandate under analysis, from 1997 to 2000, was marked not only by the extraordinary increase in royalty revenue but also by the Reelection amendment, which was enacted in June 1997 and allowed mayors to be reelected once. This period is of special interest because mostly of the revenue shock was arguably unanticipated and all the mayors could run for reelection. Figure 2.1 presents a graph which illustrate the timing of the local elections, the reelection amendment and the enactment of the Oil Law. We also show the evolution of royalty payments made to municipalities, which increased by twenty-seven times in real terms from R$ 167 million in 1997 to R$ 4.7 billion in 2008.

In addition to the data already described in chapter one, we collected further information to understand the royalty impact on local politics. We use electoral data for local elections in 1996, 2000, 2004 and 2008 provided by Tribunal Superior Eleitoral (TSE). We relied on TSE microdata to construct measures of electoral competition and performance such as vote shares, effective number of political parties and margin of victory. In addition, TSE also provided us with a list of candidates and parties elected in 1992, which allowed us to construct the 1996 party reelection variable.[6]

In order to understand the mechanisms that explain reelection results, we collected several pieces of information. To gather information on voters' aware-

[6]There is no available information for 1996 election in Espírito Santo state and most of Rio Grande do Norte municipalities.

ness about oil windfall, we performed a websearch on two newspapers to look for news about 'petroleo' (oil), 'royalties' and 'municipios' that were published in each year from 1998 to 2008. We performed the search for O Globo and Folha de São Paulo.[7] In addition, we got data on local media presence from Donos da Midia, a NGO who built a database which contain the names of all radio, televisions and newspapers which disclose local content. The Donos da Midia database contains information for 2,686 Brazilian municipalities, which include 77 municipalities (out of 157) from our main sample. This data is for 2007. In order to shed light on law enforcement, we got information from Tribunal de Contas do Rio de Janeiro, which is the institution responsible for auditing royalty revenues allocated by Rio de Janeiro's municipalities. They provide us with information on which municipalities were audited between 2003 and 2008. The objective of the audits under analysis is to verify whether the municipality has any irregularities with respect to municipal public employment.

2.5 Empirical Results

We begin this section by investigating whether oil windfall creates an incumbency advantage. We show that there is a large incumbency advantage in the election that follows the oil windfall boom, but this effect disappears in the medium run. We then investigate why there is an incumbency advantage just in the short run. We analyze political competition and selection and show that these channels cannot explain reelection results. We follow by investigating the timing and composition of the public employment increase and show that employment increased mainly in the first two political mandates, but only in the first one did voters reward incumbents that enlarged the public sector by reappointing them to office. Finally, we explore whether an information story, as sketched in the model, is plausible in the context under analysis. We provide evidence on voters' awareness level about oil windfall over the years and on the role of local media in promoting political accountability.

2.5.1 Reelection Effects

Table 2.1 assesses the effects of oil revenue on election outcomes. Panel A looks at mayor reelection in each election after the oil boom (2000, 2004 and 2008) and considers only municipalities where the mayor is in her or his first term and, hence, can run for reelection.[8] The dependent variable is an indicator variable equal to one if the incumbent mayor was reelected. All regressions

[7]These are the only two newspapers we were able to search by key word and data in the internet.

[8]Note that in 2000 all mayors were in their first term since this was the first election for which reelection was allowed.

use oil output as an instrument for royalty payments, and use state fixed effects and municipal characteristics as controls (population, urbanization rate, population density, distance to the state capital, altitude, longitude, latitude, area, a dummy for whether the municipality is a state capital). We estimate a large significant effect for 2000, which indicates that a one-standard-deviation increase in royalty value increases reelection chances by 16 percentage points, which implies a increase of 32 percent in reelection chance. The point estimates for 2004 and 2008 are also positive but cannot be distinguished from zero. Note that most of the mayors from oil-rich municipalities were reelected in 2000, which implies that they faced term limits in 2004. Therefore, the test for 2004 may lack power since only 24 oil-rich municipalities were first term mayors in 2004.

In Panel B we repeat this econometric exercise, but use as the dependent variable a dummy indicating whether the political party was reelected. In addition to check the robustness of our results, the use of party reelection allows us to incorporate the 1996 election in the analysis and understand what was happening in these municipalities before the oil windfall boom. In this exercise, municipalities are on the sample no matter whether the mayor is in the first or second-term.[9] [10] The results using party reelection as a dependent variable reassure that oil windfall creates an incumbency advantage in 2000 and also indicate an increase in reelection probability in 2004. The estimated coefficient presented in column 1, Panel B, indicates that an increase of one standard-deviation in royalty payments raises party reelection chances by 20 percentage points in 2000 and in 16 percentage points in 2004. This implies that on average party reelection probability increased by 69 percent in oil-rich municipalities in 2000 and 50 percent in 2004. We also find no effects for party reelection in 1996, when most of the municipalities were already receiving royalties but at much lower levels. This result is very important because it supports the idea that local politics were affected only when royalty values reached a substantial amount, as happened from 1999 onwards, and confirms that our analysis covers the period when most effects occurred.

Table 2.8 shows that these findings are robust to alternative samples. No matter whether we consider coastal municipalities, all the 2,151 municipalities from the nine oil producing states or the 124 onshore and offshore producing municipalities, we estimate that both mayor and party reelection increase in

[9]The sample is composed of 119 municipalities rather than 157 in 1996 because there is no available information on the 1996 election for Espírito Santo state and for most of the Rio Grande do Norte municipalities.

[10]For municipalities created between 1993 and 2001, we use information on the party in power in the original municipality to construct party reelection.

2000. The effects for 2004 are always positive but only statistically significant in some samples, which reinforce the idea that the test for 2004 may lack power. Most importantly, we estimate no oil windfall impact on mayor and party reelection in 1996 and 2008 elections, which confirms the finding that oil windfall creates an incumbency advantage only in the short run.[11]

The comparison between mayor and party effects also deserves some comments. Mayors can run for reelection under a different political affiliation than the one under which they got into power, so party estimates can be an underestimate (overestimate) of mayors' incumbency advantage in the case that mayors are more (less) associated than parties with benefits of royalty revenues. Our results indicate that oil windfall impact is larger in party reelection than on mayor reelection and that parties were able to incorporate the incumbency advantage when mayors faced term limits.

2.5.2 Political Competition and Selection

We next turn to understanding royalty impact on political competition and selection. Our model does not consider entry into politics but other studies have addressed the theoretical channels through which resource abundance can affect political competition. Caselli & Cunningham (2009) argue that resource revenue can increase competition over power because the value of attaining office and capturing oil revenue increase to all individuals and this may affect the entry of challengers and the effort they put on the process. On the other hand, resource revenues also increase the value of staying in power and can give means for incumbents to influence elections. Potential opponents can estimate the advantage of the incumbent and refrain from running for office, reducing political competition. Therefore, the effects on political competition is a matter of empirical investigation. In our context, this channel may explain our reelection results if we estimate a reduction in political competition in 2000 and/or an increase in 2008.

We assess whether oil windfall affects political competition in Table 2.2. We use three measures of political competition: the number of candidates running for mayor, the number of effective candidates and the incumbent's margin of victory. While the first variable gives us an indication of pre-election

[11] We also test royalty impact on mayor reelection using alternative econometric specifications. We use a panel for the 2000, 2004 and 2008 elections and let the royalty coefficient vary per election. No matter if we use municipal fixed effects or not, we estimate a positive and statistically significant effect for 2000 and 2004 and none for 2008. In addition, we use the share of royalty payments in total municipal revenue as an alternative measure of royalty payments. We estimate that an increase in oil windfall equivalent to 10 percent of municipal revenue raises mayor reelection probability by 26 percentage points in 2000 and 22 percentage points in 2004 (results not shown and available upon request).

competition, the other two variables show how competitive each election was by taking into account the vote-shares. We regress each dependent variable on royalty payments per capita instrumented by oil output per capita, and use as controls the state fixed effects and municipal characteristics (population, urbanization rate, population density, distance to the state capital, altitude, longitude, latitude, area, a dummy for whether the municipality is a state capital). To compare our reelection results, in all regressions we consider only municipalities where the mayor is in his or her first term.

The point estimate shown in column 1 indicates that oil revenues reduced political competition in 2000, but the effect is too noisy and cannot be distinguished from zero. Column 2 shows that oil windfall is associated with a reduction in the number of political candidates in 2004. A one-standard-deviation increase in royalty revenues decreases the number of candidates by 8 percent in 2004. We don't find a statistically significant effect for 2008. Panels B and C look at post-election competition. Panel B shows that a one-standard-deviation increase in royalty payments is associated with a decrease in the effective number of candidates in 5 percent in 2000 and in 12 percent in 2004. No effect was found for 2008. Panel C indicates that royalty payments dramatically increase the incumbent's margin of victory in 2000. A one-standard-deviation increase in royalty payments doubled the incumbent's margin of victory in 2000 (7 points increase in incumbent's vote share). Overall, the results shown in Panels A-C indicate that there is a negative association between oil revenues and post-election political competition in 2000 and 2004 and no effect in 2008. More importantly, the fact that we don't find effects on pre-election competition in 2000 and 2008 indicates that the incumbency advantage cannot be explained by fewer candidates running for mayor.

Panels D-F look at political selection by analyzing changes in the opponents' average characteristics. The link between oil windfall and political selection can be considered under a citizen-candidate framework, where any citizen can enter the electoral race if the benefits of entry exceed the costs (Osborne & Slivinski (1996)). Oil revenues can induce the entry of citizens with high opportunity cost, since it may increase the rewards from office.[12] We try to assess this channel by considering the opponents' average education and previous experience. In Panels D and E , we regress opponents' average years of schooling and the percentage of candidates with college degree on royalty payments using the same econometric specification used in Panels A-C. We find no effects of oil windfall on opponents' education in all the three

[12]These rewards from office are not necessary private rents and can include ego-rents and present and future financial compensations.

elections under analysis. Finally, Panel F shows royalty effect on the percentage of candidates that had a highly skilled occupation before running for mayor. We coded as highly-skilled any occupation that requires a college degree or is associated with civil service. We see that oil revenue is not associated with changes in this variable.

Overall, Table 2.2 indicates that the incumbency advantage estimated for 2000 should be explained by the behavior of those in power rather than through a decrease in political competition or by changes on the pool of candidates.

2.5.3 Timing and Composition of Public Employment

In the first chapter we showed that oil windfall is associated with a large boost in the public sector. In order to understand whether this fact can explain the incumbent's electoral advantage, we need to understand in which political mandate this increase was most remarkable. Table 2.3 investigates this issue by analyzing the variation of the number of employees in the two years before each election. This exercise follows the econometric specification used in chapter one. Each column shows the coefficients of a regression that include two years of data - the election year and 2 years before - and as controls use the population, municipal fixed effects and year dummies and instrument royalty value by oil output. We analyze royalty impact on three measures of employment: total employment, non-tenured employment and percentage of non-tenured employees. Employment data refers to September 30th of each year, which is the register closest to the election.[13] We consider just the municipalities whose mayors are in the first term to be able to understand electoral motivation but the results are similar with we include the 157 municipalities. Column 1 shows that an one-standard-deviation increase in royalty revenues between 1998 and 2000 is associated with 2.2 additional employees per 1000 habitants, which is equivalent to an increase of 9 percent. Columns 2 and 3 indicates that this increase was driven mostly by tenured employment. The number of non-tenured employees decreased 22 percent for every standard-deviation increase in royalty revenues between 1998-2000. Alternatively, the percentage of non-tenured employees decreased by 6 percentage points in the same period. Columns 4-6 indicates that the boost in the public sector was even larger in the second political mandate under analysis. Between 2002 and 2004, a one-standard-deviation increase in royalty revenues raised the number of employees in 5 per 1000 habitants, which represents an increase of 15 percent (column 4). However, the composition changed toward more non-tenured employees, which constitute the majority of vacancies filled in this period. A one-standard-

[13]Elections take place every four years in the first weekend of October.

deviation increase in royalties between 2002 and 2004 is associated with an increase of 5 percentage points in the share of non-tenured employees in the total employment (see column 6). Finally, Table 2.3 indicates that no new jobs were created between 2006 and 2008 due to an increase in oil windfall. These results confirm the trends we see on Figure 1.5 in the first chapter: total public employment in oil-rich municipalities began to increase in 1999 and followed an upward trend until 2006 and stabilized in 2007 and 2008. In addition, Figure 2.2 shows that in 1999 and 2000, there was a marked change in employment composition, when tenured employment suffered a huge boost and non-tenured jobs decreased. In 2001-2004, the increase in public employment was led by new non-tenured jobs.

Table 2.3 shows that the incumbency advantage more or less followed increases in public employment. This evidence supports our model's predictions as long as we show that municipalities that experienced the largest increases in the public sector were the ones whose voters reappointed the mayor for office with a higher probability. Table 2.4 investigates that question. For each election year, we regress a variable indicating whether the mayor was reelected on the two-year variation of the total number of employees per capita (columns 1, 4 and 7), on the two-year variation of number of non-tenured employees per capita (columns 2, 5 and 6) and on the variation of the proportion of non-tenured employees (columns 3, 6 and 9). All employment measures are instrumented by the two-year variation of oil output. We observe that each employment per 1000 habitants created between 1998 and 2000 caused by oil output variation is associated with an increase of 5 percentage points in reelection probability. However, the composition of public employees does not affect mayor reelection. We also see that more public employment is not associated with reelection in 2004 or in 2008. These results indicate that employing more people was an effective strategy to attract votes in 2000 but not in 2004 and 2008. According to our model, this is explained by voters interpreting public employment as a signal of political ability only in 2000.

However, two other stories are compatible with the employment and reelection results and do not necessarily support our model. The first one is that voters have preferences for a large public sector but there is a limit on how much the mayor can enlarge it. Once you reach that limit, mayors cannot keep hiring people, and thus lose the election. Indeed, there are several laws in Brazil that limit mayors ability to keep hiring people. First, 'Lei de Responsabilidade Fiscal' determines that municipal and state governments cannot spend more than 60 percent of the net current revenue on payroll.[14] Second, the royalty law

[14]Lei Complementar n 101, 4 de maio de 2000.

does not allow the use of royalty revenues to hire employees on a permanent basis. Finally, the government can hire new employees on a temporary basis just to perform very special duties, such as to combat epidemics and carry out the census.[15] Therefore, the fact that we find that public employment does not increase between 2006 and 2008 can be a result of law enforcement. We analyze this issue by gathering information on which municipalities were audited by Tribunal de Contas of Rio de Janeiro state from 2003 and 2008. The audits under analysis had the specific aim of investigating public employment irregularities. In Table 2.5, we regress the number of employees per capita on royalty revenues, a dummy variable indicating whether the municipality was audited in the current or previous year and an interaction variable of auditing dummy and the amount of royalties received on that year. We also include the geographic controls and instrument royalty value and the interaction variable by oil output and oil output interacted with the auditing dummy. We observe that in 2004, an increase in royalty revenues is associated with a large increase in public employment but no differential effect is found for municipalities which were audited in 2003 and/or 2004. However, in 2008, the interaction variable has a negative and significant effect of similar magnitude of royalty effect. This implies that the audit process was effective in 2008 in restraining public employment increases, since municipalities that received royalties and were audited in 2007 and/or 2008 did not increase the number of employees, while the other non-audited oil-rich municipalities enlarged the public sector in that year. Therefore, Table 2.3 cannot allow us to disregard the idea that public employment halted its increase due to constraints on the executive branch, and this caused the loss in incumbency advantage.

The second alternative story is the clientelistic story, as rationalized by Robinson & Verdier (2003) and Robinson et al. (2006) models. The argument in Robinson & Verdier (2003) is that offers of employment in the bureaucracy is a credible policy to obtain political support because optimal employment contracts concede rents to workers due to moral hazard and employment in the bureaucracy is an attractive way for politicians to generate rents.[16] Therefore, our results could simply indicate that as long as incumbents exchange jobs for political support, they can get reelected. Once they stop doing it, they are ousted from power. Although it is difficult to assess the clientelistic story, the analysis of composition of public employment can shed light on it. The

[15]Lei n 8.745, 9 de dezembro de 1993

[16]There is a large number of papers which relate patronage and resource-rich economies. Collier (2007), for instance, points out that "patronage politics can be a more cost-effective use of public money to attract votes than the provision of public goods, yet it is too expensive to be feasible". Therefore, we could see more patronage practices in resource-rich economies just because resource wealth provides funds to bribe voters.

clientelism story sketched in Robinson & Verdier (2003) is consistent with an increase in non-tenured employment since according to their model it is crucial for mayors to be able to fire workers, otherwise voters' promise of political support would not be credible. Table 2.4 indicates that it is the total number of employees rather than the number of non-tenured employees that guaranteed electoral success in 2000. In addition, the most remarkable increase in the number of non-tenured employees occurred in the second political mandate under analysis (2001-2004), when most of the mayors from oil-rich municipalities faced term limits and when we don't estimate a positive association between more employment and higher reelection probability. Table 2.6 confirms this argument. We show the increase in public employees per political term, splitting the sample in 2004 and 2008 by whether the mayor is in a first or second term. We see that the increase in public sector in 2004 happened in both types of municipalities, while in 2008 oil windfall is not associated with more public employees in both groups. In order to support the clientelistic story, we would need to see an increase in public employment just in municipalities where the mayor is in his first term.

Thus, the results presented in this section indicate that mayors from oil-rich municipalities used royalty revenues to hire tenured employees at the beginning of oil boom and then changed their strategy toward non-tenured employees. These results also suggest that voters from oil-rich municipalities have become more demanding throughout the years and are no longer satisfied with increases in the public sector. Although this result support our model and may indicate that voters stop to interpret public employment as a signal of incumbent's ability, we cannot rule out the story that public employment stopped increasing due to constraints on the executive branch. Finally, we don't find support for the clientelistic story in which public employment is a type of private transfer used to obtain political favors.

2.5.4 Information

To reconcile our model with the results presented, we still need to provide evidence regarding model's main hypothesis, i.e., that voters are not fully informed about oil windfall. In addition, we need to show that voters' awareness increased throughout the years. Unfortunately, we don't have any objective measure of voters' information about oil windfall that varies over time, but we circumvent this caveat with alternative evidence.

We believe that the characteristics of Brazilian oil production and royalty distribution rule challenge voters' assessment of royalty value. The lion's share of oil production in Brazil is located offshore and the inland basis is concentrated in one municipality (Macaé). Therefore, voters would be unaware of this oil windfall unless this revenue is made public by the media, politicians or informed citizens. Even more difficult for voters to assess is the exact amount received. Royalty payments depend on the international oil prices, the exchange rate, the production and quality levels of each oil well and their proximity to oil fields. Therefore, royalty revenue varies a great deal across municipalities and over the years and voters need to update their information frequently. Although they can do that by assessing the ANP website, there is evidence that, in the first years of oil boom (at least), the awareness level was quite low. A survey carried out on September 2002 in Campos dos Goytacazes, the largest beneficiary of royalty revenues, indicates that 58 percent of the respondents were not familiar with the term royalties.[17] For those who knew the meaning of royalties, 56 percent pointed out that they didn't know how the revenue was invested.

However, we believe that voters' awareness has increased along the years and with the increase in oil windfall. In municipalities where this money represents a key part of the total budget, informed citizens, the media, political challengers and think tanks improved their technologies to disclose information to the average citizen. Local initiatives to disclose information on royalty values have come out since 2004, at least in the most benefited municipalities. The InfoRoyalties website was created in June 2004 by a local research center in order to deliver information on royalty payments and their use. Regional blogs have been posted in order to freely discuss local politics and public budget.[18]

Two other facts suggest that voters awareness has increased over the years. One is related to voters' and politicians' capacity to predict royalty payments. Although most of the municipalities under analysis have produced oil since the mid-1980s, the stake that they get from this production increased dramatically with the Oil Law in a way that was difficult to anticipate. Figure 2.3 shows the actual and predicted value of royalty payments for 1997-2000, 2001-2004 and 2005-2008 periods.[19] This figure shows that the values received in 1999 and 2000 were much larger than what was possible to predict based

[17]Survey of 1,400 respondents detailed at UCAM, Petroleo, Royalties e Regiao, Boletim, Ano 1, Numero 1, Setembro/2003.

[18]Roberto Moraes blog is a case in point. Posted for the first time in August 2004, it has drawn more than 1.4 million readers since then and had an active role in the 2004 and 2008 election debate.

[19]To predict 1997-2000 royalty payments, we first used the royalty payments average annual growth rate from 1994 to 1996 to calculate $PredictedRoyalties_{1997} = Royalties_{1996} * (1 + AverageGrowth1994 - 1996)$. We then used the formula $PredictedRoyalties_{t+1} = PredictedRoyalties_t * (1 + AverageGrowth1994 - 1996)$ where $t = 1997, 1998, 1999$. We follow the same procedure to predict royalty payments for 2001-2004 using the 1997-2000 average real growth rate; and to predict 2005-2008 payments based on the 2001-2004 average real growth rate.

on previous revenues. Therefore, it was harder for both politicians and voters to estimate royalty revenues. However, for the periods of 2001-2004 and 2005-2008, the previous revenue growth rate was a much better proxy of the following years payments. What we want to emphasize with Figure 2.3 is that it became easier over the years to predict royalty payments.

In addition, in 2007, a particular event increased the information provided regarding royalty payments. In November, Petrobras announced the discovery of Tupi, a giant oil field equal to all Norway's reserves. As noted by Economist (2007), Tupi was the world's second largest strike in 20 years. Two other announcements followed Tupi in early 2008, and the Federal government launched a huge propaganda campaign about what were termed 'pre-sal discoveries', which promised to put Brazil among the five largest oil producers in the World. The promise of a huge windfall spurred politicians to debate the royalty rule, which until then was considered undebatable by the Federal government.[20] A special concern is to increase the number of beneficiary states and municipalities, since the current rule determines that the state of Rio de Janeiro and its municipalities received 43 percent of all oil royalty payments in 2008. In order to follow and stimulate this discussion, newspapers have produced many articles about royalty payments, their beneficiaries and their use. Figure 2.4 shows the number of articles with the words 'petróleo" (oil), 'royalties" and 'municípios" (municipalities) published by year since 1998 by Folha de São Paulo and O Globo, two Brazilian major newspapers.[21] We see that the average number of articles were about ten until 2006. In 2007, the year of the first major discovery announcement, the number tripled to 30 and in 2008, an election year, 100 news articles were published about the topic. We believe that this graph indicates that more information was provided to voters in 2008 than in previous elections.

Another way to investigate whether information play a role in voters' decision is to explore variation in media coverage across municipalities. Table 2.7 shows the effect of the presence of media with local content on the 2008 reelection outcome. We regress mayor reelection on royalty payments, a variable indicating whether the municipality has local media and an interaction variable of royalty payments and a media dummy. We also include the geographic controls and instrument royalty value and the interaction variable by oil output, and oil output interacting with the media dummy. Along the columns, we vary the measurement of media presence among local radio, television and

[20] See `http://oglobo.globo.com/pais/noblat/post.asp?cod_post=80899`

[21] Information for O Globo is only available from 2003 onwards. We are still trying to obtain the same information from other newspapers from the beneficiary states.

newspaper.[22] These regressions only include the 77 municipalities (out of 157) for which the measures of media presence are available. We observe that mayors from oil-rich municipalities have a lower probability of getting reelected when there is a local TV or a local newspaper. Although we don't have information on the content disclosed by these medias, the fact that they are local imply that they have a higher probability of disclosing information on local issues than other state or national medias. The size of royalty payments in oil-rich municipalities budget and the threat of losing this revenue turn royalty revenues into an important topic for discussion. Unfortunately, we just have data on local media presence for 2008, which does not allow us to understand how their impact changed over time which is crucial to understand the differential effect of royalty rents on the 2000 and 2008 elections. However, Table 2.7 supports the idea that information is crucial for political accountability in oil-rich municipalities.

2.6 Conclusion

In this paper we empirically assess the political mechanisms that explain how natural resource booms affect economic development. We do that by studying the recent boom of oil production in Brazil and the distribution of oil royalties to municipalities. We provide evidence that royalty payments create an incumbency advantage in the election that follows a oil windfall boom. We estimate that a one-standard-deviation increase in royalty value raised reelection chances by 16 percentage points in 2000 (an increase of 32 percent in reelection chance). However, we show that this effect disappears in the medium run, by estimating no incumbency advantage in 2004 and 2008. We also show that the incumbency advantage estimated for 2000 and 2004 should be explained by the behavior of those who are in power, since oil revenues do not impact political selection in any election or pre-election competition in 2000.

We then investigate why voters reelected the incumbents only after the beginning of oil boom. We first analyze whether the enlargement of public sector can explain reelection results. In particular, we investigate when the boost in public sector occurred and whether the municipalities that experienced the larger increases in the public sector are the ones whose voters were more likely to reappoint their mayor for office. We show that municipalities increased the number of public employees mainly in the 1997-

[22]In column 1, we use the number of local radio stations rather than an indicator variable for whether the municipality has a local station because almost all municipalities have at least one local radio.

2000 and 2001-2004 political mandates, but while the first increase was based on more tenured employees, the expansion of the municipal public sector in the second political mandate under analysis relied on non-tenured jobs. The efficacy of this strategy as a way to obtain political support changed over time. Only in 2000 did voters reward the incumbents who created more jobs. We also show that two institutions were able to constrain the irresponsible use of oil revenues. Audits restrained the increase in public employment and local media exerted a pressure on mayors from oil-rich municipalities, who had more difficult in getting reelected.

Our findings are compatible with a learning story presented by our model. The idea is that voters are not fully informed about the amount of royalties received by the municipality where they live. This revenue enables the mayor to provide a higher level of public employment and since voters do not observe the size of the revenue shock, they interpret increases in public employment as a signal of political ability and reward the incumbent by reappointing him to office. Oil revenues have continued to increase throughout the years, as well as voters' awareness about these resources, which increases their demand for improvements and consequently the level of public goods that mayors need to provide to signal high ability. If mayors face a trade-off between diverting money for private use or providing public goods and being reelected, the increase in voters' awareness can make the second strategy less attractive, due to the increasing difficulty in influencing election outcome. Therefore, changes in voters' awareness decrease the probability of reelection and increase the diversion of public funds. However, the result that audits stopped the increase in public employment does not allow us to disregard the idea that constraints on the executive branch restrained the enlargement of the public sector and this caused the loss in incumbency advantage.

Thus, our results indicate that oil does not make leaders unaccountable, and that a democratic system is crucial to avoid the negative effects of resource abundance. Elections, media presence and constraints on executives are all institutions that play a role in restraining the irresponsible use of oil revenues. However, these institutions were not sufficient to guarantee prosperity since our results indicate that Brazilian oil-rich municipalities missed a great opportunity to develop economically after their windfall.

Table 2.1: Mayor and Party Reelection

	1996	2000	2004	2008
	(1)	(2)	(3)	(4)
	A-Dependent variable: Mayor reelection			
Royalties pc		0.59	0.17	0.07
		(0.15)***	(0.18)	(0.14)
Municipalities		157	79	117
	B-Dependent variable: Party reelection			
Royalties pc	1.28	0.72	0.32	-0.00
	(1.53)	(0.16)***	(0.14)**	(0.05)
Municipalities	119	157	157	157

Notes: This table reports the effects of royalty payments on mayor and party reelection in municipalities located on the coast of the nine oil producing states (CE, RN, AL, SE, BA, ES, RJ, SP and PR). Regressions exclude the municipalities on the top 1% of royalty distribution (Quissamã and Rio das Ostras). All regressions use oil output as an instrument for royalty value and control for population, state fixed effects and municipal characteristics (population, urbanization rate, population density, distance to the state capital, altitude, longitude, latitude, area, a dummy for whether the municipality is a state capital). Each column indicates one election year: 1996, 2000, 2004 and 2008. Panel A dependent variable is a dummy variable indicating whether the mayor was reelected. Regressions on Panel A consider only municipalities where the mayor is in his first term. Panel B dependent variable is a dummy variable indicating whether the party was reelected. For municipalities created between 1993 and 2001, we use information on the party in power in the original municipality to construct party reelection. The sample in column 1, panel B, is smaller because there is no information on 1996 election for Espírito Santo state and for most of Rio Grande do Norte's municipalities. We use the contemporaneous value of royalty rents and oil output. Both are measured in R$ 1000 per habitant and are deflated by the consumer price index, representing 2008 values. Robust standard errors are reported in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table 2.2: Political Competition and Selection

	2000	2004	2008
	(1)	(2)	(3)
	A-Dependent variable: Number of candidates		
Royalties pc	-0.47	-0.65	0.27
	(0.38)	(0.32)**	(0.56)
Municipalities	157	79	114
	B-Dependent variable: Effective number of candidates		
Royalties pc	-0.45	-0.56	-0.05
	(0.18)**	(0.22)***	(0.17)
Municipalities	157	79	114
	C-Dependent variable: Incumbent margin of victory		
Royalties pc	0.26	-0.03	0.06
	(0.07)***	(0.08)	(0.07)
Municipalities	127	61	83
	D-Dependent variable: Opponents' years of schooling		
Royalties pc	0.68	0.63	0.77
	(1.37)	(0.88)	(0.64)
Municipalities	155	78	117
	E-Dependent variable: Opponents' college degree		
Royalties pc	0.06	0.16	0.12
	(0.16)	(0.12)	(0.10)
Municipalities	155	79	117
	F-Dependent variable: Opponents' highly-skilled occupation		
Royalties pc	-0.00	-0.02	0.13
	(0.20)	(0.10)	(0.11)
Municipalities	154	77	117

Notes: This table reports the effects of royalty payments on political competition and selection in municipalities located on the coast of the nine oil producing states (CE, RN, AL, SE, BA, ES, RJ, SP and PR). Regressions exclude the municipalities on the top 1% of royalty distribution (Quissamã and Rio das Ostras). All regressions use oil output as an instrument for royalty value and control for population, state fixed effects and municipal characteristics (population, urbanization rate, population density, distance to the state capital, altitude, longitude, latitude, area, a dummy for whether the municipality is a state capital). Each column indicates one election year: 2000, 2004 and 2008. All regressions consider only municipalities where the mayor is in his first term. Panel A dependent variable is the number of candidates who run for mayor. Panel B dependent variable is the effective number of candidates who run for mayor, which is computed by dividing one by the Herfindahl index. Panel C dependent variable is the incumbent's margin of victory, which is the difference in vote-share between the incumbent who is running for reelection and the closest opponent. Panel C considers only municipalities whose mayors ran for reelection. Panel D-F considers opponents' average characteristics. College degree indicates the percentage of candidates with a college diploma. Highly-skilled occupation in column F refers to the percentage of candidates that have a highly-skilled occupation before running for mayor. We use the contemporaneous value of royalty rents and oil output. Both are measured in R$ 1000 per habitant and are deflated by the consumer price index, representing 2008 values. Robust standard errors are reported in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table 2.3: Public Employment by Political Mandate

	Total	Non-tenured	% non-tenured	Total	Non-tenured	% non-tenured	Total	Non-tenured	% non-tenured
	1998-2000			2002-2004			2006-2008		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Royalties pc	10.33	-10.37	-0.27	10.63	10.56	0.11	-2.01	1.90	0.05
	(4.49)**	(3.31)***	(0.11)**	(1.47)***	(1.51)***	(0.05)**	(2.60)	(2.47)	(0.05)
Observations	274	274	274	146	146	146	232	232	232
Municipalities	137	137	137	73	73	73	116	116	116

Notes: This table reports the effects of royalty payments on municipal public employment by political mandate. The dependent variable is the total number of public employees per 1000 habitants in columns 1, 4 and 7; total number of non-tenured employees per 1000 habitants in columns 2, 5 and 8; and the percentage of non-tenured employees on total employment in columns 3, 6 and 9. All employment measures are from September 30th of the years indicated in the columns. All regressions consider only municipalities where the mayor is in his first term. Royalty payments are the value received in the contemporaneous year, are measured in R$ 1000 per habitant and are deflated by the consumer price index, representing 2008 values. Population, municipal fixed effects and year dummies are included as controls and royalty value is instrumented by oil output. We consider only municipalities from the nine oil producing states (CE, RN, AL, SE, BA, ES, RJ, SP and PR) and exclude the municipalities on the top 1% of royalty distribution (Quissamã and Rio das Ostras). Robust standard errors clustered at municipality are reported in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table 2.4: Public Employment and Reelection

Dependent variable:	Mayor reelection 2000			Mayor reelection 2004			Mayor reelection 2008		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Total employees pc	0.05			-0.01			-0.07		
	(0.03)*			(0.02)			(0.04)		
Non-tenured employees pc		-0.08			-0.00			-0.38	
		(0.07)			(0.01)			(1.09)	
% of non-tenured employees			-4.85			-0.25			19.77
			(7.31)			(0.73)			(52.23)
Observations	137	137	137	73	73	73	116	116	116
F-stat	3.423	1.431	0.358	7.476	13.78	6.055	2.973	0.111	0.110

Notes: This table reports regressions coefficients of a dummy variable indicating whether the mayor was reelected on two-year change of municipal employment instrumented by two-year change of oil output per capita. These regressions use as controls state fixed effects and municipal characteristics (population, urbanization rate, population density, distance to the state capital, altitude, longitude, latitude, area, a dummy for whether the municipality is a state capital). The sample used include only municipalities whose mayor is on his first term. We consider only municipalities from the nine oil producing states (CE, RN, AL, SE, BA, ES, RJ, SP and PR) and exclude the municipalities on the top 1% of royalty distribution (Quissamã and Rio das Ostras). Robust standard errors are reported in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence. F-stat is the Kleibergen-Paap Wald rk F statistic for a weak instruments test.

Table 2.5: Auditing

Dependent variable:	Number of employees pc 2004	2008
	(1)	(2)
Royalties pc * audit	2.72	-21.58
	(23.69)	(5.70)***
Royalties pc	25.11	23.97
	(12.65)**	(5.47)***
Audit	-3.77	17.50
	(4.61)	(6.89)**
Observations	88	88
F-stat	37.41	87.00

Notes: This table reports the effects of royalty payments and audits on municipal public employment. The dependent variable is the total number of public employees per 1000 habitants on September 30th of the years indicated in the columns. Audit is a dummy variable indicating whether the municipality was audited by TCE-RJ in the current and/or previous year. These regressions use as controls municipal characteristics: population, urbanization rate, population density, distance to the state capital, altitude, longitude, latitude, area, a dummy for whether the municipality is a state capital. We instrument royalty value and the interaction variable by oil output and oil output interacted with the auditing dummy. Royalty payments are the value received in the contemporaneous year, are measured in R$ 1000 per habitant and are deflated by the consumer price index, representing 2008 values. The sample includes only Rio de Janeiro municipalities. Robust standard errors are reported in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence. F-stat is the Kleibergen-Paap Wald rk F statistic for a weak instruments test.

Table 2.6: Public Employment and Electoral Incentives

Dependent variable:	Number of employees pc				
	First term 1998-2000	First term 2002-2004	Second term 2002-2004	First term 2006-2008	Second term 2006-2008
	(1)	(2)	(3)	(4)	(5)
Royalties pc	10.33	10.63	8.36	-2.01	-0.21
	(4.49)**	(1.47)***	(2.91)***	(2.60)	(0.23)
Observations	274	146	154	232	76
R^2	0.12	0.18	0.27	0.18	0.25
Municipalities	137	73	77	116	38

Notes: This table reports the effects of royalty payments on municipal public employment by political mandate. The dependent variable is the total number of public employees per 1000 habitants on September 30th of the years indicated in the columns. First term (second term) indicates municipalities where the mayor is in his first term (second term). Royalty payments are the value received in the contemporaneous year, are measured in R$ 1000 per habitant and are deflated by the consumer price index, representing 2008 values. Population, municipal fixed effects and year dummies are included as controls and royalty value is instrumented by oil output. We consider only municipalities from the nine oil producing states (CE, RN, AL, SE, BA, ES, RJ, SP and PR) and exclude the municipalities on the top 1% of royalty distribution (Quissamã and Rio das Ostras). Robust standard errors clustered at municipality are reported in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table 2.7: Media Presence

Dependent variable:	Mayor reelection in 2008		
Media variable:	Number of local radio stations	Local TV	Local newspaper
	(1)	(2)	(3)
Royalties pc * Media	-0.02	-0.26	-0.29
	(0.02)	(0.16)*	(0.19)
Royalties pc	0.18	0.19	0.17
	(0.15)	(0.15)	(0.18)
Media	0.04	0.09	0.06
	(0.03)	(0.23)	(0.20)
Observations	77	77	77
R^2	0.17	0.17	0.17
F-stat	8.041	9.482	7.015

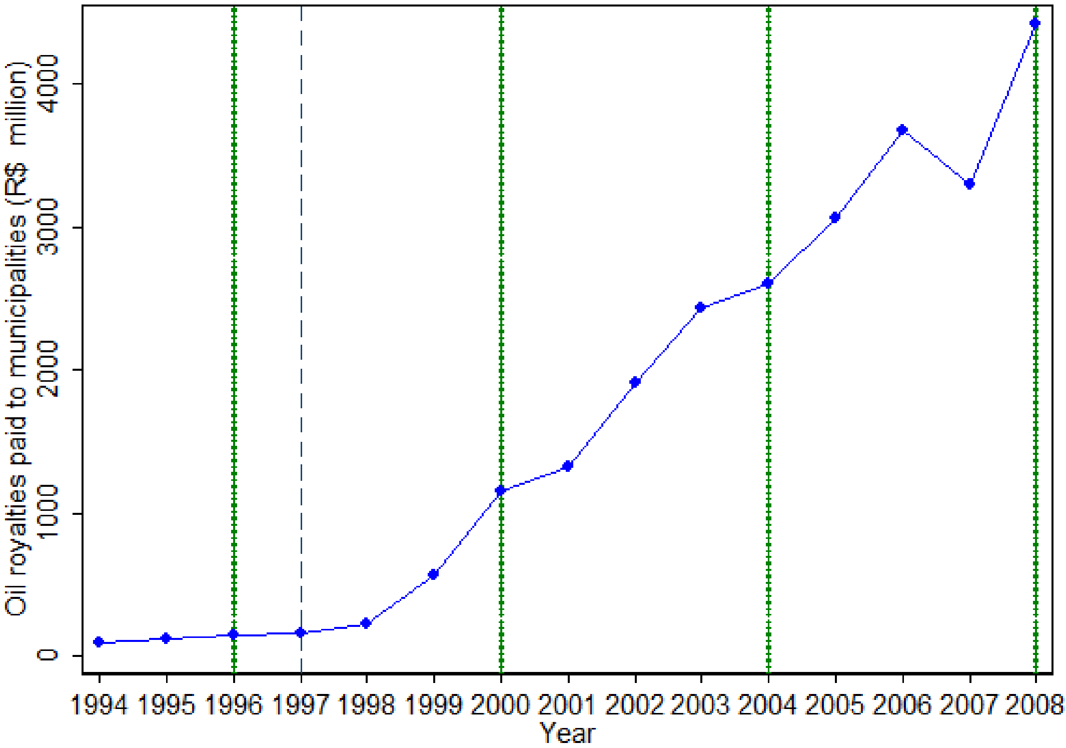
Notes: This table reports the effects of royalty payments and local media presence on mayor reelection. The dependent variable is a dummy indicating whether the mayor was reelected in 2008. In column 1, media is the number of local radio stations. In column 2, media is a dummy variable indicating whether the municipality has a television channel with local transmission, while column 3 media variable is a dummy indicating whether the municipality has a local newspaper. These regressions use as controls state fixed effects and municipal characteristics (population, urbanization rate, population density, distance to the state capital, altitude, longitude, latitude, area, a dummy for whether the municipality is a state capital). We instrument royalty value and the interaction variable by oil output and oil output interacted with media dummy. Royalty payments are the value received in the contemporaneous year, are measured in R$ 1000 per habitant and are deflated by the consumer price index, representing 2008 values. The sample includes only 77 municipalities out of the 157 coastal municipalities for each the media information is available. Robust standard errors are reported in parentheses. Significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence. F-stat is the Kleibergen-Paap Wald rk F statistic for a weak instruments test.

Table 2.8: Robustness of Reelection Results

	Coastal municipalities	All municipalities	Oil producing municipalities
	(1)	(2)	(3)
Panel A - Mayor reelection			
Royalties pc 2000	0.59	0.26	0.47
	(0.15)***	(0.13)*	(0.25)*
Obs	157	2151	124
Royalties pc 2004	0.17	0.32	0.53
	(0.18)	(0.19)*	(0.26)**
Obs	79	1236	65
Royalties pc 2008	0.07	0.04	0.06
	(0.14)	(0.08)	(0.18)
Obs	117	1608	91
Panel B - Party reelection			
Royalties pc 1996	1.28	0.90	0.86
	(1.53)	(1.04)	(1.47)
Obs	119	1867	99
Royalties pc 2000	0.72	0.68	0.62
	(0.16)***	(0.15)***	(0.27)**
Obs	157	2151	124
Royalties pc 2004	0.32	0.32	0.22
	(0.14)**	(0.11)***	(0.21)
Obs	157	2151	124
Royalties pc 2008	-0.00	0.00	0.02
	(0.05)	(0.05)	(0.07)
Obs	157	2151	124

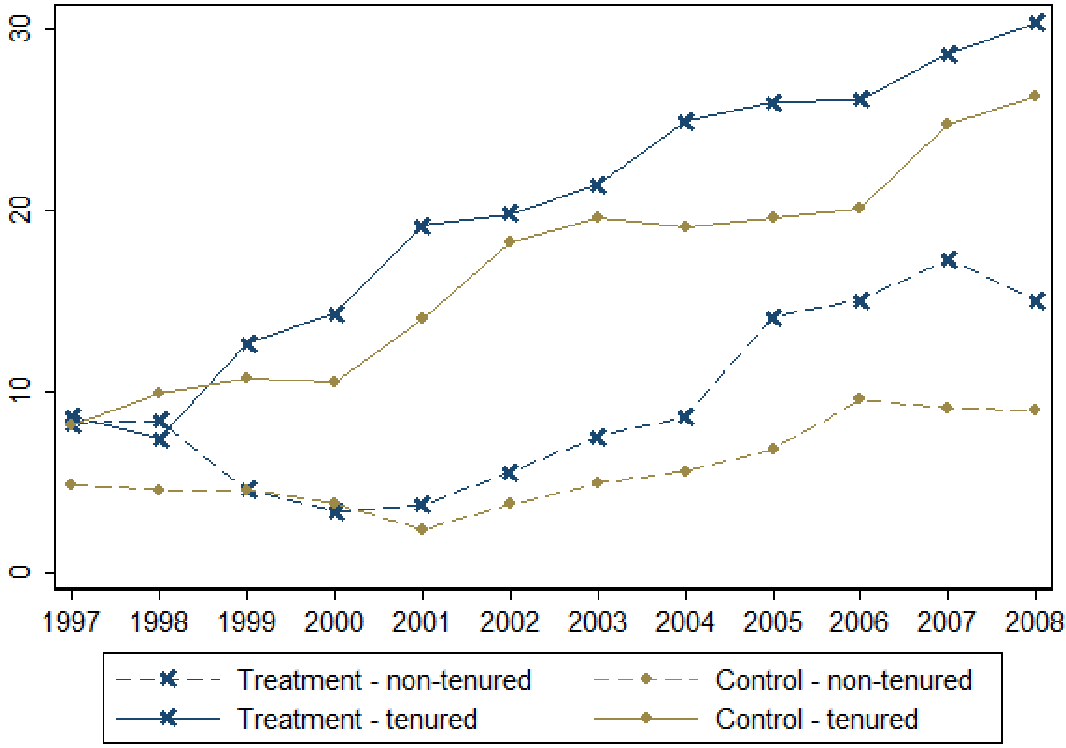
Notes: Each entry is the coefficient and correspondent standard-error of a regression of mayor reelection (Panel A) and party reelection (Panel B) on royalty value per capita instrumented by oil output per capita. Each line refers to a different election year and each column indicates a different sample as explained in the top of the table. All regressions control for population, year effects, state fixed effects and municipal characteristics (population, urbanization rate, population density, distance to the state capital, altitude, longitude, latitude, area, a dummy for whether the municipality is a state capital). Regressions on Panel A consider only municipalities where the mayor is in his first term.

Figure 2.1: Royalty Payments to Brazilian Municipalities 1994-2008



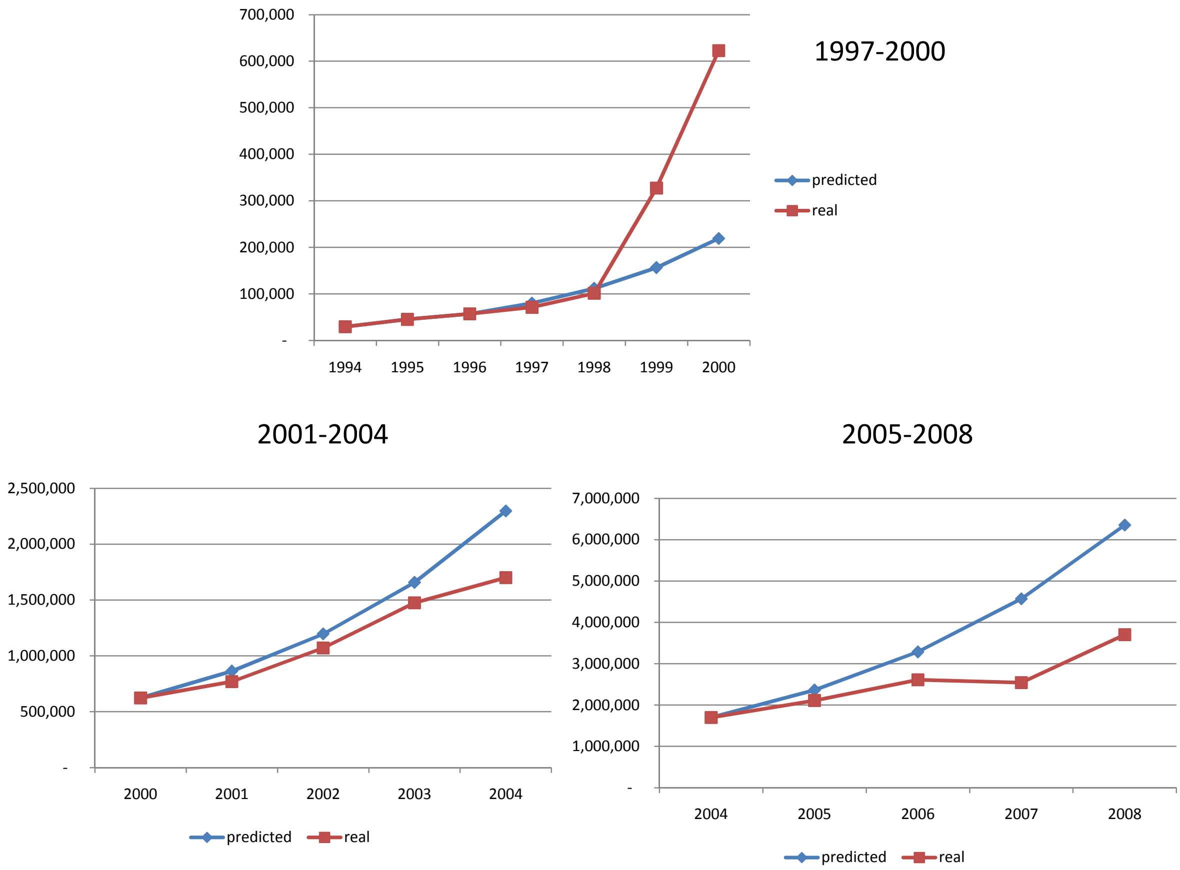
Notes: This figure show the evolution of royalty payments to municipalities from 1994 to 2008. Royalty payment unit is R$ million and corresponds to 2008 real value. The solid vertical lines indicate municipal election years. The dash vertical line indicates the year of enactment of Oil Law.

Figure 2.2: Number of Tenured and Non-tenured Employees 1997-2008



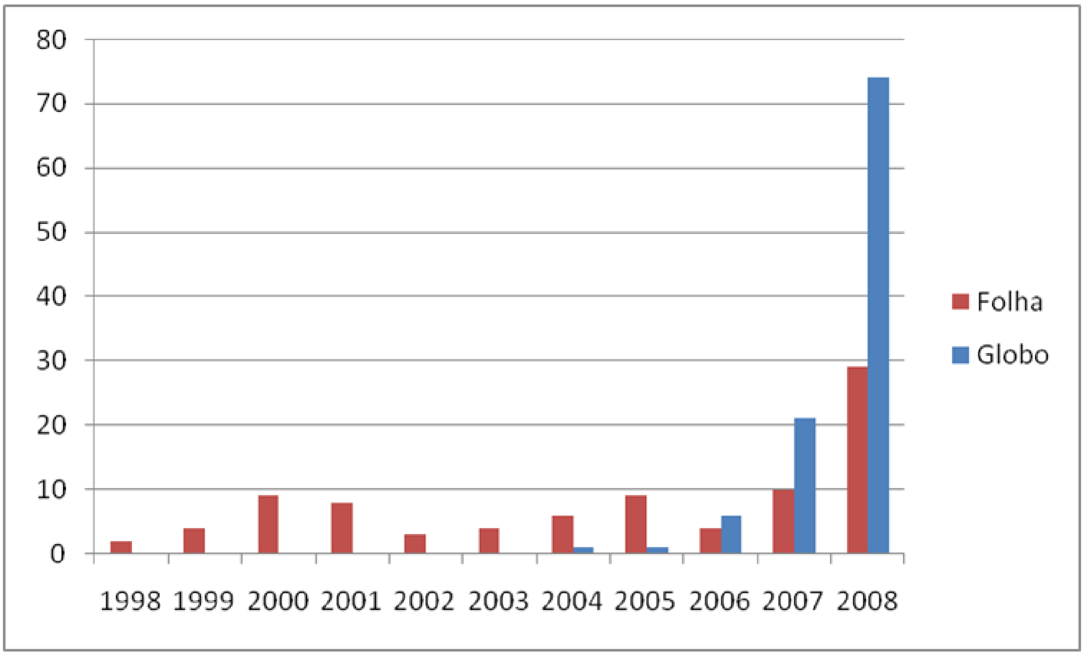
Notes: This figure shows the median number of tenured and non-tenured municipal employees per 1000 habitants on September 30th between 1997 and 2008 for two group of municipalities. Producing municipalities are coastal municipalities that have oil extracted from an oil field within their borders in the reference year. Non-producing municipalities are coastal municipalities which do not produce oil.

Figure 2.3: Actual and Predicted Royalties



Notes: This figure shows the actual and predicted values of royalty payments for 1997-2000, 2001-2004 and 2005-2008 political mandates. To predict 1997-2000 royalty payments, we first use the royalty payments average annual growth rate from 1994 to 1996 to calculate $PredictedRoyalties_{1997} = Royalties_{1996} * (1 + AverageGrowth1994 - 1996)$. We then used the formula $PredictedRoyalties_{t+1} = PredictedRoyalties_t * (1 + AverageGrowth1994 - 1996)$ where $t = 1997, 1998, 1999$. We follow the same procedure to predict royalty payments for 2001-2004 using 1997-2000 average real growth rate; and to predict 2005-2008 payments based on 2001-2004 average real growth rate.

Figure 2.4: Newspaper Coverage



Notes: This figure shows the number of articles with the words 'petróleo" (oil), 'royalties" and 'municípios" (municipalities) published by year by Folha de São Paulo (since 1998) and O Globo (since 2003).

3 Neighborhood Violence and School Achievement: Evidence from Rio de Janeiro's Drug Battles

3.1 Introduction

Violence plagues children across both developed and developing countries, particularly those living in poor urban areas (UNICEF (2006)). There are many reasons to believe that violence has an adverse role in children's educational outcomes, and consequently on their way out of poverty. However, empirical evidence on violence consequences has remained remarkably sparse and mixed. Two main empirical challenges have limited our understanding of the subject. First, neighborhood violence is hardly disentangled from other types of socioeconomic disadvantages that also lead to poor education outcomes (Case & Katz (1991), Aizer (2007), Mayer & Jencks (1989)). Second, it is difficult to characterize and measure local violence. Whenever available, data are usually aggregated at the city level, masking deep variations within cities (Glaeser et al. (1996)), and possibly leading to measurement error of localized violence.

In this paper, we assess whether and how children's educational outcomes are affected by armed conflicts among drug gangs in Rio de Janeiro. Throughout recent decades, several slums scattered across the city have been dominated by heavily-armed drug gangs, which have used the territory to sell drugs and hide from police (Silva et al. (2008), Misse (1999)). Local violence skyrockets when gangs decide to fight each other. Slums are the conflicts epicenter, where risks of life can reach civil war records.[1] Our objective in this paper is to understand whether and how these conflicts affect young children attending the municipal schools located in the proximities of conflict areas. In particular, we examine how student achievement is affected, how students

[1] An assessment made by Extra Newspaper based on police records indicated that 60 percent of the homicides that occurred in the four most violent neighborhoods in Rio de Janeiro metropolitan area (areas 7, 9, 15 and 20) in 2009 were related to drug trade. Source: `http://extra.globo.com/geral/casodepolicia/post.asp?t=morte-obrigatoria-trafico-causa-60-dos-homicidios-nas-zonas-fatais&cod_Post=314033&a=443` This implies that drug trade was responsible for 1,195 deaths only in these areas of the city. Considering the definition of civil war as those internal conflicts that count more than 1,000 battle deaths in a single year (Blattman & Miguel (2010)), this number easily indicates that the drug battles in Rio de Janeiro resemble a civil war.

and teachers respond to these conflicts, and which channels might explain the impact of exposure to violence on educational outcomes. In order to identify where and when armed conflicts take place, we built a novel database on reports about armed conflicts among drug gangs to an anonymous police hotline. We then associate the reports with slums, match this information with educational data by exploring distances between schools and slums, and explore the variation in violence across time and space to identify violence impacts on educational outcomes.

Our results indicate that schools close to areas that experience more variation in armed conflicts over time perform worse in standardized math exams, while no significant effect is found for language exams. Our estimates indicate that a school that experience high levels of violence exposure (percentile 90 of the distribution of violence) scores 0.17 standard deviations less on standardized math tests than a school less exposed to violence (percentile 10). These violent events are also associated with higher grade repetition and dropout rates, particularly for nonwhite students. In terms of mobility across schools, we find no significant effects of violence on students' transfers and new admissions during the school year. Finally, we also discuss the mechanisms underlying these results. We show that violence is associated with higher teacher absenteeism. Violent events during both school and vacation periods affect student achievement, which suggest that violence may have disruptive effects in both the school and the household environments.

This paper contributes to different bodies of literature. Psychologists and psychiatrists have long suggested a positive correlation between children's exposure to local violence and mental health disorders, restricted emotional development, learning problems and truancy (Margolin & Gordis (2000), Fowler et al. (2009), Lynch (2003), Schwartz & Gorman (2003)). In economics, Grogger (1997) shows that violence within schools may reduce the likelihood of high school graduation and the probability of college attendance. Severnini & Firpo (2009) show for a sample of Brazilian schools that students who attend schools which face high levels of school violence usually have lower performance on proficiency tests. However, difficulty in characterizing and measuring violence as well as disentangling its effects from other types of disadvantages has put these evidences into perspective.

The few studies that have tried to overcome identification problems have found little support to a causal relationship between local violence and individual outcomes. Ludwig & Kling (2007) find no support for contagious theories, in which neighborhood violence would induce more crime among individuals. Instead, they find that race segregation may play a more important role in understanding variation across neighborhoods in violent crime than has been thought. Aizer (2007) uses a cross-section survey linking neighborhood violence and children's achievement in math and reading test scores. The author shows that once the family background and other forms of disadvantages are controlled, measures of violence that otherwise negatively affect achievement become mostly insignificant.[2] Thus, while psychologists and psychiatrists have raised concerns over the pervasive role of violence in child development, econometric evidence is scant and still far from a definitive answer for whether the effects of local violence on children's education is a first order policy concern, or if it does only reflect other disadvantages and its side effects.

This paper is also related to two other strands of literature. There is a growing research that aims to understand the effects of extremely violent episodes such as civil conflicts and wars on children's schooling (Akresh & Walque (2008), Shemyakina (ming), León (2009)). However, this literature cannot say much on the mechanisms that explain poor outcomes since it studies major disruptive events, which involve economic and political chaos, and institutional and infra-structure degradation. Finally, this study also contributes to the literature on the social costs of violence (Soares (2006), Lynch & Rasmussen (2001), Hamermesh (1999), Cerqueira et al. (2007)). The results presented in this paper reveal a detrimental effect of violence on individuals' lifetime earnings potential through its impacts on student achievement and accumulation of human capital. Furthermore, our results indicate that violence might create poverty traps, since it makes the way out of poverty more difficult by decreasing student achievement in poor and violent areas.

This paper is organized as follows. Section 2 describes the institutional background, the dynamics of drug gangs conflicts in Rio de Janeiro, and the city's public primary school system. Section 3 presents the data and descriptive statistics of drug conflicts and primary education in Rio de Janeiro between 2004 and 2009. Section 4 discusses a conceptual framework, while section 5 presents our empirical model. Section 6 shows the results, and Section 7 presents robustness checks. Section 8 concludes.

3.2 Institutional Background

[2]There is also a large amount of literature that aims to understand the effects of neighborhoods on individual outcomes (Ludwig et al. (2001), Kling et al. (2005), Kling et al. (2007), Oreopoulos (2003)). In particular, Jacob (2004) explores a natural experiment caused by forced relocation of families due to public housing closure in order to understand the effect of neighborhoods on children's education outcomes. He finds that neighborhoods do not affect education achievement and that family or individual characteristics are much more important to understand differences in educational outcomes than neighborhood influence.

3.2.1 Violence in Rio de Janeiro

Rio de Janeiro is internationally famous for its violence. In 2009, 2,155 people were murdered in the city, which is equivalent to a homicide rate of 32 per 100,000 habitants. This rate is comparable to the ones verified in the most violent cities in United States, such as Detroit (40 murders per 100,000 habitants), Baltimore (37) and Newark (26).[3] This record, already high for international standards, masks striking differences of violence exposure across the city. Poor neighborhoods in the North zone of the city experienced 60.3 deaths for 100,000 inhabitants in 2009, while the South zone rich neighborhoods recorded a homicide rate of approximately 6.6.[4]

Rio de Janeiro violence took off in the early 1980s. This period is marked by the constitution of Comando Vermelho (CV), the first organized drug gang formed in the city, and the entrance of cocaine, which was brought from Bolívia, Peru and Colombia (Dowdney (2003)). Drug dealers relied on the marijuana trade network already established in Rio de Janeiro's slums to sell cocaine. The dominance of slums became crucial to protect the illicit trade. Their geography marked by tiny streets and corners as well as their lawlessness turned slums into an important market for drugs and a strategic place to hide from police (Silva et al. (2008)). The higher profitability of cocaine trade changed drug trade dynamics and led to increasing quarrels among drug members. As a result, some members left Comando Vermelho and created Terceiro Comando (TC) in the late 1980s (Misse (1999)). In the 1990s two other gangs, Amigos dos Amigos (ADA) and Terceiro Comando Puro (TCP), were created by dissidents of the two former gangs. This fractionalization of drug gangs led to more armed conflicts to conquer slums and the increasing militarization of drug gangs (Misse (1997)).

These drug gangs, and more recently the militia[5], are intermittently involved in conflicts where the arsenals employed are similar to the ones found in wars. There is scant research on what triggers these conflicts. Table 3.1 analyzes whether the number of conflicts correlates with slum and neighborhood characteristics. We present coefficients from cross-section regressions of the number of days with conflicts between 2004 and 2009 in

[3] These rates take into account murders and nonnegligent manslaughters, which are defined as the willful (nonnegligent) killing of one human being by another. Source: FBI's Uniform Crime Reporting (UCR) Program.

[4] These rates are from AISP 9 (60.3 deaths per 100,000 inhabitants). The rate of 6.6 deaths per 100,000 inhabitants was registered in AISP 23. Source: `http://www.isp.rj.gov.br/ResumoAispDetalhe.asp?cod=200912&mes=Dezembro&ano=2009&tp=Mensal`

[5] The militias are groups of policemen and firefighters who pretend to provide security in the neighborhood and charge for this and other 'services" by coercion.

each slum on slum and neighborhood characteristics.[6] Table 3.1 indicates that geographic characteristics such as slum steepness, distance from slum to main roads, slum area and neighborhood population density are good predictors of the number of days with violence, while neighborhood income per capita or income inequality are not associated with the amount of conflicts in slums. This table supports the idea that some slums are more exposed to violence than others because they are strategic places where drug dealers hide from police (a notion supported by the importance of slum steepness in predicting violence) and because of logistical factors (see variable distance to main roads). However, factors that are usually associated with crime, such as income levels and inequality, do not play a role in explaining the prevalence of conflict.

Note that this evidence does not imply that some slums are always embattled, while others are always at peace. Indeed, Figure 3.3, which sketches, for each year, the number of days with conflicts in the ten most violent slums, indicates that levels of violence vary considerably across time.

So, what determines the dynamics of drug conflicts? Newspaper coverage as well as sociologic literature suggest that conflicts are not strategically planned. Baptista et al. (2000) emphasize that Rio de Janeiro's drug gangs are controlled by a group of independent leaders who are inexperienced and young, while Misse (1997) and Souza (2001) argue that gangs do not have a hierarchical structure ruled by a drug baron in the models that we find in Colombia or in the Italian mafia. The reading of the newspaper suggests that these conflicts occur when the unstable power equilibrium among drug gangs is broken by a successful gang overthrow, the imprisonment or release of a gang leader or betrayals. Some fragments of newspaper and blog articles exemplify this argument:

Drug dealers from Morro dos Macacos reobtained the control of three slums in Agua Santa with the support of drug dealers from Rocinha and São Carlos (...) The area was under militia control since last year. The conflict lasted five hours. According to the police department, the invasion was led by Luciano de Oliveira Felipe, known as Cotonete, who is the former slum traffic manager. He was deposed one year ago and was hidden in Morro dos Macacos. (Source: Meia Hora, 6/12/2009)

In addition, drug leaders' release or imprisonment also seems to trigger conflicts as indicated in the following article:

[6] We use in this exercise all the information available at the slum and neighborhood levels

Three people died and eight were wounded after Vila dos Pinheiros invasion by Baixa do Sapateiro drug dealers…The invasion was led by Nei da Conceição Cruz, known as Facão, the main leader of Terceiro Comando Puro (TCP). The conflict began at 10 pm and lasted the whole night. The operation was supported by Matemático.(...) Facão and Matemático left jail last month after winning in Court the right to work outside jail and come back to sleep. Both criminals did not return to jail after the first day under the new sentence. (Source: Meia Hora, 5/31/2009)

In Annex B we transcribe more articles that support our argument. These fragments of newspapers also indicate how violent these events are. People who live in conflict areas and close to them are the most affected. The freedom of movement is drastically affected, the chance of being hit by stray bullets is considerable, and people who are associated with a drug gang can be evicted from their homes or murdered when a new gang assumes control. In addition, these transcriptions show that conflict duration can vary a lot. Conflicts to depose a gang can take hours or days and are usually followed by attempts to reconquer the territory by the former gang. This effort to regain control can occur in the same week or a few months later, depending on how much support the deposed gang can gather from other drug dealers. Therefore, when one conflict begins, it is hard to predict when it is going to be ended.

The impact of these conflicts on city daily routine can be attested with the answers from a victimization survey carried out in 2007. Fear of a stray bullet (60%) and being caught by a gunfight (44%) were mentioned as the violent events of which respondents were most afraid, followed by robberies (37%).[7]

Most of these drug conflicts occur in slums, which does not imply that all slums are controlled by drug gangs and are constantly under conflict. We use slums as the translation for 'favelas", which is defined by the Rio de Janeiro's City Plan as areas characterized by tiny and irregular streets, irregular plot size, poor urban services and irregular settlements.[8] There are 979 slums in Rio de Janeiro according to Instituto Pereira Passos, which concentrate 1.093 million people or 19 percent of the city population (2000 Census data). Figure

[7]This survey was carried out by DATAUFF and interviewed 4,000 people in the Rio de Janeiro metropolitan area. The percentage shown corresponds to answers from people who live in the city of Rio de Janeiro

[8]The definition of favela is given by article 147 of Rio de Janeiro's Plano Diretor (Law number 16/1992): "Para fins de aplicação do Plano Diretor Decenal, favela é a área predominantemente habitacional, caracterizada por ocupação da terra por população de baixa renda, precariedade da infra-estrutura urbana e de serviços públicos, vias estreitas e de alinhamento irregular, lotes de forma e tamanho irregular e construções não licenciadas, em desconformidade com os padrões legais.

3.1 shows the map of the city of Rio de Janeiro with slum borders and indicates that slums are quite widespread around the city.

Although slums are poverty enclaves, not all people in favelas are poor and not all the urban poor live in favelas (Perlman (2010)). Access to urban infrastructure, especially water and electricity distribution, has improved a lot in slums in the last two decades and nowadays are not markedly different from other city areas (Vianna (2008)). But social inequalities are still persistent. In 2007-2008, slum habitants earned 49 percent less than other city habitants and have an average of 3.5 years less education than other city areas (6.4 years of study versus 9.9) (Neri (2010)).

3.2.2 Rio de Janeiro municipal education system

The municipal administration is the main elementary school provider in Rio de Janeiro. The municipal system is one of the largest in Latin America, comprising 1063 elementary schools and 550,000 students.[9] First to fifth graders, which are the focus of our analysis, correspond to 46 percent of the students in the system.[10] There are no school districts in the city and students can choose any school to attend. Some schools have more demand than others, which implies that some students do not end up in their first school choice.[11] The public school network is complemented by the private system, although private coverage is low among poor students. Only 2.5 percent of slum inhabitants attend private schools, while 12.7 percent of other city inhabitants study in the private system (Neri (2010)).

About 36,000 teachers and 13,099 employees work in the municipal school system. All professionals are hired through public exams. Wages are the same across schools but vary with seniority and additional duties. Recently hired teachers are allowed to choose among open placements across different regions, but do not have control over the specific school where they are going to work in the chosen region. There is mobility across schools between years, but it depends on seniority. After three years working in the system, professionals can apply to transfer to another school. Conversations with professionals suggest that some teachers indeed manage to move away from violent areas between years. Within years, however, teachers can only respond to violence shocks with absenteeism and attrition.

[9] Numbers for 2009 gathered at `http://www.rio.rj.gov.br/web/sme/exibeconteudo?article-id=96310`.

[10] We use 1st to 5th graders to refer to students who attend the 1st to 4th grades in the older grade system or the students who are enrolled in the first five years of elementary school according to the new system.

[11] See **?** and **?** for a discussion of the process of registration in public schools in Rio de Janeiro.

Figure 3.1 shows school and slum distribution in the city and indicates that both are widespread. This widespread school distribution and the fact that 98% of children at school age attend school in Rio de Janeiro indicates that school coverage is not a main concern in the city. However, there are several issues related to school quality. An assessment made by the Municipal Secretariat of Education in 2009 showed that 15% of students (28,000) at the 4th, 5th and 6th grades were actually functional illiterates (Prefeitura (2009)). In addition, inequalities across the city are still persistent. Neri (2010) shows that slum inhabitants study less 1 hour and 15 minutes per week compared to other city inhabitants, due to a combination of higher dropout, lower school load and higher absenteeism.

There are several anecdotal evidences of the effects of these conflicts on school routine, mainly among those really close to conflict areas. Problems range from interruption of classes for hours or days, risk of being hit by stray bullets in the way in or out of the school (or sometimes even inside), students' and teachers' emotional disturbs, among others. The headlines from O Globo, the main Rio de Janeiro daily newspaper, exemplify this:

'Teacher is shot by a stray bullet in front of school in Senador Camará", O Globo, March 4th, 2010.

'Gun conflict in Fazendinha left children, who are in their way out of school, in panic", O Globo, June 18th, 2007.

'Boy is shot by a stray bullet inside a school and arrives dead at the hospital", O Globo, July, 16, 2010.

In addition, in a visit to schools located in a highly violent area, we heard several examples of how these conflicts affect school routine. One of the schools did not open for almost an entire month in 2006, when drug gangs were fighting for slum control; teachers and students are intermittently threatened by students connected with drug dealers; and several children, especially the ones who live in the most isolated areas of the slums or the ones with family connections with drug dealers, miss classes or drop out during conflicts. They also mention that children easily identify the bell ringing in the middle of the classes as a signal to leave the classroom and protect themselves from stray bullets in the corridor.

3.3 Data

3.3.1 Violence data

Any understanding of the consequences of Rio de Janeiro's drug conflicts requires finer data about where and when conflicts take place. This is necessary because violence exposure varies dramatically across and within neighborhoods. Official crime data, which is provided by Instituto the Segurança Pública (ISP), cannot track differences in violence exposure since it records information gathered by police stations and then aggregates it for 18 city areas. In addition, ISP does not track information on when and where conflicts happen but only on homicides, which is one of the outcomes of these conflicts. Therefore, we created a novel database for this research based on anonymous reports to Disque-Denúncia, describing that a gun fight occurred in a specific place.

Disque-Denúncia (DD) is a crime hotline that any person can call to report a problem for which she desires the intervention of a public authority. The central was created in 1995 and sits inside the Police Authority of the state of Rio de Janeiro but is managed by an NGO. The calls received by the central are directly forwarded to Civil and Military police, who decide whether and how to respond to each report. All the reports are anonymous and are neither recorded nor tracked. DD works 24 hours a day, 7 days a week and its phone number is broadly disclosed around the city (e.g. on supermarket bags and on buses).

The reports are registered in a database which contains the date, location and description of each event. People call to report any kind of crime and irregularities such as assaults, the location of criminals and bodies, and noise complaints. We got from DD all reports that mention a gun fight among drug gangs between 2003 and 2009 in the city of Rio de Janeiro. We read all reports to guarantee that they described a gunfight and to standardize the addresses provided. The address and the description of the events allow us to associate most of the reports to a specific slum, following the city slum map provided by Instituto Pereira Passos. This procedure generated a list containing all the slums of the city and the dates when a conflict took place. We then aggregated the data per slum and by year by counting the number of days that at least one report of armed conflict was registered in Disque-Denúncia. Annex 1 describes in detail how we built the database.

With the violence measure per slum, we created a measure of violence per school by using GIS tools and considering the distance between each school and each slum. Hence, we defined that the exposure to violence of each school

s at time t is equal to:

$$V_{st} = \sum_j D_{sj} v_{jt}$$

where v_{jt} is the violence level at slum j at time t and D_{sj} is a measure of distance between school s and slum j. Our preferred measure of distance is:

$$D_{sj} = \frac{1}{d_{sj}}$$

where d_{sj} measures the linear distance from school s to slum j closest border. By using this weight, V_{st} considers that each school is exposed to the whole city's violence but gives a higher weight to the violence that occurred closer to the school. Therefore, a particular school is on the top of our violence distribution if it is located closer to one or more violent slums. It can have relatively little exposure to violence if it is surrounded by peaceful slums. We use the logarithm of V_{st} as our main violence measure in the empirical analysis in order to reduce the influence of sharp outliers. We also use as an alternative measure of distance in robustness checks the weight $D_{sj} = 1$ if $d_{sj} \leq x$ meters, which adds the violence of slum j only if it is within x meters from school s, where $x = \{5, 250, 500\}$.

Reports to Disque-Denúncia as a direct measure of violence may raise potential concerns. In section 3.5.2 we provide evidence that Disque-Denúncia reports are indeed a good proxy for violence by comparing it with homicide rates, principals' reports about school violence and by cross-checking with newspaper information.

This research also relies on violence information provided by two newspaper blogs. Plantão de Polícia[12] from Meia Hora newspaper and Casos de Polícia[13] from Extra are to our knowledge the two best information sources of daily violence in Rio de Janeiro's poor areas. We extracted from these blogs the news we transcribed in the Institutional Background section and in the annex. We also used the information provided by these blogs in order to check whether Disque-Denúncia provides a good picture of drug gangs conflicts.

3.3.2 Educational data

In order to determine the impact of drug gang violence on education, we use three databases for educational variables. Students' achievement is measured by Prova Brasil, a national standardized exam applied to all fifth

[12] `http://one.meiahora.com/noticias/cat/plantao-de-policia_26.html`
[13] `http://extra.globo.com/geral/casodepolicia/`

graders in 2005, 2007 and 2009.[14] All students from Rio de Janeiro's schools that had more than 30 students in the fifth grade in 2005 or more than 20 in 2007 and 2009 were supposed to take this exam. The exam is composed of two tests that measure math and language (Portuguese) skills. Unfortunately, the Prova Brasil dataset does not permit students' identification, so we are not able to follow students across time, and we need to rely on score averages at the school level. In addition, students answer a survey about their social-demographic profile, while teachers and principals provide information on their experience and school conditions. In 2007, the principals answered specific questions on violence exposure at school, which we use to compare with our violence data. Prova Brasil dataset is provided by Instituto Anisio Teixeira (INEP). INEP also organizes the Educational Census which provides yearly information on school inputs such as the number of teachers, the number of classrooms, class size, etc. Finally, we use administrative data from Rio de Janeiro's Secretaria Municipal de Educação (SME) from 2004 to 2009. SME gathers information from students' profiles (e.g. date of birth, race, parents' education, religion) when they enter the municipal system and then tracks all their movement within the system. This information includes all public schools each student attended, the grade in which they are enrolled and if and when they transferred between schools. These data allow us to calculate school averages for students' demographics, grade repetition rates, dropout, transfers and new admissions.

3.3.3 Other data

This work relies heavily on geocoded information, which was provided by Instituto Pereira Passos (IPP). Key information is the slum borders, which is based on satellite pictures. This information is not only precise but quite detailed since it defines different slum borders even within large slum areas. As a result, the given definition led to 979 slums (rather than about 300 given by other definitions) which allows us to better localize each violent event. IPP also provides shape files with municipal schools' location, Rio de Janeiro's main roads and neighborhood limits. Based on these shape files, we used GIS tools to calculate the area and population density of Rio de Janeiro's neighborhoods and distances from slums to schools and main roads. In order to understand the determinants of conflicts, we gathered from IPP income per capita, gini index, and population, calculated at neighborhood level based on the 2000

[14]Prova Brasil is also applied to ninth grade students. However, we do not explore this exam because we want to avoid reverse causality. More drug conflicts can lead to more demand for soldiers (older boys), which might impact students' schooling decisions.

IBGE Census. We also obtained information on the slum area for 1999 and 2004. The NASA website provided gridpoints information on Rio de Janeiro's elevation which allowed us to calculate slum steepness. Finally, we got from IPP a list with slum alternative names necessary to match Disque-Denúncia reports to slums.

3.3.4 Summary statistics

Table 3.2 provides Disque-Denúncia descriptive statistics. There were 3,571 reports registered as 'gunfights between drug gangs' from January 1st, 2004 to December 31st, 2009. However, the analysis of the database showed that 444 reports do not describe a gunfight, which led us to exclude them from our analysis.[15] In addition, we exclude another 243 reports that we were not able to associate with a specific slum, leading to a final sample of 2,884 reports.[16] The matching of 92% of the reports to slums confirms the idea that slums are the main conflict battlefield but does not indicate that slum is a synonym of conflict. Table 3.2 shows that about one-third of the slums (289 out of 979) experienced at least one conflict between 2004 and 2009 according to Disque-Denúncia. We refer to this group as violent slums. We see that the average number of reports in violent slums is 1.7 per year or 10 between 2004 and 2009. In our analysis we use the number of days with conflicts in each slum rather than the number of reports in order to deal with the fact that one person can call several times to report the same conflict, leading to striking outliers. We therefore use as our main violence variable the number of days in which there was at least one report about a gunfight. The mean value of this variable in violent slums is 1.4 per year and the standard deviation is 3. The dynamics of these events in the ten most violent slums are exemplified in Figure 3.3. This Figure indicates that violence peaks in different years depending on the slum, which suggests that gunfights are not strategically orchestrated at the city level.

Violence information is associated with the 736 schools (out of 1065) that comprise our sample. These schools are the ones who did Prova Brasil in at least two years between 2005 and 2009. Table 3.3 indicates the proximity of these schools to slums. As already indicated in Figure 3.1, there is no poor supply of schools close to slum areas. We see that 47% of schools are within 250 meters from at least one slum, while 73% are within 500 meters.

[15]The reports that were excluded mention the threat of conflicts among drug gangs, the location of drug dealers, or complement previous information. They are excluded because they do not mention that an armed conflict took place on the specific date.

[16]We were not able to localize the other 243 reports because they do not provide a specific address, or they mention a street that is not inside a slum or close to a slum border.

We focus our analysis on children who attend the first five years of elementary school. The main period of analysis is 2004-2009, the years in which Rio de Janeiro's education administrative data is available. Table 3.4 presents education summary statistics. We show school averages for the whole sample and for violent and non-violent schools. We define as violent schools the ones exposed to violence within 250 meters at any moment between 2004 and 2009. There are 199 violent schools in our sample and 537 schools non-affected by violence within a 250-meter radius. We see that there are marked differences between violent and non-violent schools. Violent schools have worse performance. They do worse in Prova Brasil and have higher failure and dropout rates. However, it is not clear whether the worse performance can be attributed to violence, since these schools enroll more disadvantaged students. Violent schools have a higher share of students with illiterate mothers and fathers, a lower share of whites and less students who live with their parents at home. Interestingly, violent schools have a higher proportion of students who study close to their homes, which indicates that proximity to their household should be an important reason for students to choose these worse performing schools. Violent schools also have more students on average but are not much different from non-violent schools in relation to infrastructure.

The bottom of Table 3.4 also shows principals' reports on the Prova Brasil survey about whether specific events occurred in the school in 2007. Violent schools have a considerably higher incidence of class interruption, students' absence, and drug consumption and trade close to school. These differences not only show that our violence measure correctly indicates schools exposed to drug conflict but also suggest some of the channels through which violence may affect achievement.

In summary, schools exposed to violence are associated with lower achievement. Although this suggests a negative association between violence and achievement, these schools are also attended by students from more disadvantaged households. In the next sections, we discuss the strategies used to disentangle the violence effects on student achievement from other confounding factors.

3.4 Conceptual Framework

In this section, we lay out a simple statistical model that guides our empirical estimations. We first define individuals' exposure to local violence. In a second step, we set a basic model for cognitive achievement in order to highlight the likely channels through which local violence may impact students' performance.

3.4.1 Exposure to violence

Assume $j \in (1, ..., J)$ indexes J slums. We denote the violence level at slum j at moment t as v_{jt}. We assume that v_{jt} depends on two terms. The first term $\overline{v}_j$ is a constant that captures the idea that each slum has an intrinsic level of violence, which depends on neighborhood fixed effects, such as geographic characteristics and strategic position. The second component u_{jt} is an error term uncorrelated with neighborhood characteristics and that follows the normal distribution $u_{jt} \sim N(0, \sigma_j)$. Deviations of u_{jt} might be triggered by events of betrayal and revanchism, responses to threats or imprisonment of gang leaders, and other gang reactions. Therefore,

$$v_{jt} = \overline{v}_j + u_{jt}, \;\; u_{jt} \sim N(0, \sigma_j) \tag{3-1}$$

We assume that child i is exposed to violence v_{jt} depending on the distances between epicenter j to both her household and her school s. Hence, we define students' exposure to local violence by:

$$V_{ist} = \sum_j D_{ij} v_{jt} + \sum_j D_{sj} v_{jt} \tag{3-2}$$

where D_{ij} is the weight that captures the distance between student's i household and slum j, and D_{sj} is the weight that considers the distance between school s and slum j. Replacing (3-1) in (3-2) and rearranging the terms, we have the equation:

$$V_{ist} = (\overline{V}_s + \overline{u}_{st}) + (\overline{V}_i + \overline{u}_{it}) = V_{st} + V_{it} \tag{3-3}$$

Where $\overline{V}_k = \sum_j D_{jk} \overline{v}_j$ and $\overline{u}_{kt} = \sum_j D_{jk} u_{jt}$ for $k = \{s, i\}$. This equation states that students' total exposure to violence is a combination of exposure to violence at school and at home.

3.4.2 Student achievement

Let y_{ist} be a measure of cognitive achievement for child i attending school s at time t. A simple specification of the production function considers that individual's family inputs F_{it} are combined with school resources S_{st} leading to a process of knowledge acquisition as described by the function:

$$y_{ist} = Y\left[F_{it}, S_{st}\right] \tag{3-4}$$

We properly define each of these terms below.

Family inputs

We assume that F_{it} follows the specification

$$F_{it} = F\left[W_{it}, M_{it}(V_{it}), A_{it}(V_{it})\right] \tag{3-5}$$

Or its linearized version

$$F_{it} = \phi_0 + \phi_1 W_{it} + \phi_2 M_{it}(V_{it}) + \phi_3 A_{it}(V_{it}) \tag{3-6}$$

Where W_{it} is an index of the individual's family socioeconomic status at t, $M(\cdot)$ is the individual's mental capacity, and $A(\cdot)$ is the individual's effort to attend classes. We allow $M(\cdot)$ and $A(\cdot)$ to be influenced by an innate cognitive capacity endowment C_i, and also by the individual's exposure to local violence at home V_{it}. Both $M(\cdot)$ and $A(\cdot)$ can be expressed respectively as $M_{it} = \gamma_M C_i + \beta_M V_{it}$ and $A_{it} = \gamma_A C_i + \beta_A V_{it}$. Combining expressions for $M(\cdot)$ and $A(\cdot)$ and equation (3-6), we have:

$$F_{it} = \Phi_0 + \Phi_1 W_{it} + \Phi_2 C_i + \Phi_3\left(V_{it}\right) \tag{3-7}$$

Where Φ_0 collects constant terms, $\Phi_1 = \phi_1$, $\Phi_2 = \phi_2\gamma_M + \phi_3\gamma_A$ and $\Phi_3 = \phi_2\beta_M + \phi_3\beta_A$. In sum, according to the family input channel, shocks of local violence might impact children' outcomes as long as $\Phi_3 \neq 0$. From the standard literature on education production functions we may assume that both ϕ_2 and ϕ_3 are positive parameters (i.e., mental capacity and class attendance are positively correlated with student's achievement). On the other hand, research by psychologists and psychiatrists suggests that $\beta_M < 0$ once exposure to violence might cause mental health disorders and other behavior disturbances. We complement this framework also supposing that $\beta_A < 0$, once local violence may alter students' attendance given the risks it imposes to an individual's movement between household and school. Finally, note that we do not impose any constraints on the relationship between local violence and the family socioeconomic status.

School resources

We assume that S_{st} follows the specification

$$S_{st} = S\left[T_{st}(V_{st}), I_{st}\right] \tag{3-8}$$

Or its linearized version

$$S_{st} = \theta_0 + \theta_1 T_{st}(V_{st}) + \theta_2 I_{st} \tag{3-9}$$

Where T_{st} is, for simplicity, an index measuring the contribution of the representative school's teacher for the student's achievement at time t, and the last term I_{st} represents the school's physical resources. Note that we define T_{st} depending on the level of exposure to violence at the school, while we assume that I_{st} is not affected by violence since these conflicts do not lead to school infrastructure losses. This assumption means that violence effects might occur only through human resources. We assume that T_{st} follows the linear equation

$$T_{st} = \varphi_0 + \varphi_1 HT_{st}(V_{st}) + \varphi_2 MT_{st}(V_{st}) \tag{3-10}$$

Thus, we allow local violence to affect T_{st} via two main channels. First, it may impact teacher's yearly hours of work HT_{st} as defined by expression $HT_{st} = \pi_{1s} + \pi_{ht}V_{st}$. The parameter π_{ht} captures the idea that violence around the school may affect teachers absenteeism, which may even lead to an extreme situation of job attrition during the year, or $HT_{st} = 0$. Second, violence may impact teachers' mental health and their capacity to concentrate and teach. This effect is captured by the expression $MT_{st} = \pi_{2s} + \pi_{mt}V_{st}$. Besides these two channels, local violence might impact the school's human resources via other effects. For instance, the school's staff may react to violent events with more effort in order to alleviate its effects on children. We otherwise omit further extensions in order to simplify the analysis. We combine (3-9) and (3-10) and re-write equation (3-9) as:

$$S_{st} = \Theta_0 + \Theta_1 V_{st} \tag{3-11}$$

where Θ_0 collects constant terms and $\Theta_1 = \theta_1(\varphi_1\pi_{ht} + \varphi_2\pi_{mt})$. We may assume θ_1, φ_1 and φ_2 as positive parameters. Thus, Θ_1 would be a negative parameter as long as local violence negatively affects teachers' yearly hours of work and mental health.

3.5 Empirical Strategy

In this section, we present the statistical models used in our estimations. We first adapt the conceptual framework from the previous section to our empirical setting and to the available data, in order to lay out the baseline regressions. Second, we discuss estimation challenges and potential caveats.

3.5.1 Empirical model

In order to define our empirical model for student achievement, we first linearize equation (3-4):

$$y_{ist} = \rho_0 + \rho_1 F_{it} + \rho_2 S_{ist} + \varepsilon_{ist} \tag{3-12}$$

Where ε_{ist} is an additive measurement error term in test scores. The combination of equation (3-12) with equations (3-7) and (3-11), as well as the introduction of time fixed effects to control for differences among test scores across time, result in the following equation:

$$y_{ist} = \lambda_1 W_{it} + \lambda_2 C_i + \lambda_3 V_{it} + \lambda_4 V_{st} + \mu_s + \vartheta_t + \varepsilon_{ist} \tag{3-13}$$

Where ϑ_t indicates time fixed effects and μ_s represents school fixed effects. This last term collects time-fixed characteristics of the individual's school and its surroundings, such as the school's physical resources, the average quality of human resources and the intrinsic level of exposure to violence in the neighborhood. We are interested in estimates for $\lambda_3 = \rho_1 \Phi_3$ and $\lambda_4 = \rho_2 \Phi_1$, which capture the effects of the exposure to violence at the student's school and household, respectively.

Next we adapt model (3-13) to our empirical setting and to the data available. Our main measure of student achievement is Prova Brasil test scores, which assess math and language skills amongst 5th graders. As mentioned, we are not able to follow students across time and must rely on score averages at the school level. Furthermore, we do not observe individual exposure to violence at home (V_{it}), but only exposure to violence at school (V_{st}). Therefore, we are able to estimate:

$$\overline{y}_{st} = \psi_1 V_{st} + \overline{X}'_{st} \psi_2 + \mu_s + \vartheta_t + \varepsilon^*_{st} \tag{3-14}$$

Where $\overline{y}_{st}$ is the average math or language test scores at school s in year t for 5th graders. The variable of interest is the exposure to violence V_{st}, that varies only at the school level and across time. In our estimations we use the logarithm of this variable to reduce the influence of outliers. The vector $\overline{X}_{st}$ includes controls for student socioeconomic status, which are averaged at the school level and also vary across time. The set of variables in $\overline{X}_{st}$ includes the number of 5th graders per school, average age, share of whites, share of boys and students' mothers education. School and time fixed effects are controlled respectively for μ_s and ϑ_t. Our sample includes the 736 municipal schools that participated in Prova Brasil in at least two years and covers 2005, 2007 and 2009. These are the years in which Prova Brasil was applied. We focus our study on young children from elementary school once they are not subject to soldiering in drug gangs. Thus, we avoid the reverse causality that might occur if conflicts increase the demand for soldiers, which in turn would reduce demand for education.

3.5.2 Empirical challenges

The first caveat underlying our empirical model (3-14) occurs as long as the error term ε^*_{st} includes an omitted variable that captures the students' exposure to violence at home, or $\varepsilon^*_{st} = \varepsilon_{st} + \psi^*\overline{V}_{it}$, where $\overline{V}_{it}$ measures the average exposure to violence at home across students from school s in year t. If $\overline{V}_{it}$ is omitted, and both exposure to violence at school and at home are positively correlated, V_{st} overstates the impact of exposure to violence at school once it also captures effects from $\overline{V}_{it}$. This is a reasonable assumption once students tend to live close to their schools. This concern only affects our understanding of how the total violence effect is disentangled. The positive bias we may find when estimating ψ_1 in model (3-14) due to this limitation should be interpreted as part of the total effect of local shocks of violence.

The second potential caveat relates to individuals' mobility and selection. Achievement test scores are taken at the end of the school year, though any impact violence may have on student's transfers or dropout during the year is of concern. Suppose the error term ε^*_{st} can be re-expressed as $\varepsilon^*_{st} = \varepsilon_{st} + \psi^*\overline{C}_{st}$, where $\overline{C}_{st}$ captures a non observable component of students' cognitive capacity, averaged at the school level. Different arguments may support either a positive or a negative bias on coefficient $\widehat{\psi}_1$ due to the relationship between violence during the school year, students' mobility and $\overline{C}_{st}$. Suppose for instance that violence raises the opportunity costs for less capable students to attend classes. This effect may trigger higher dropout rates among this group of students, leading to higher levels of $\overline{C}_{st}$ at the end of the year, and consequently to a downward bias on $\widehat{\psi}_1$. On the other hand, if violence is associated with low achievement, we can also suppose that more capable students may search for schools in less violent areas during the school year. This effect would bias $\widehat{\psi}_1$ upwards. Thus, the bias direction on $\widehat{\psi}_1$ due to students' mobility is a matter of empirical investigation.

In order to overcome this caveat we proceed as follows. We calculate students' mobility, drop out, retention and attrition using administrative records at the student and school levels. This information allow us to identify whether violence correlates with patterns of grade repetition, dropping out of school and transferring out of school. The administrative records also include students' socioeconomic characteristics, allowing us to examine whether shocks of violence have heterogeneous effects on individuals according to their socioeconomic status. Hence, along with model (3-14), we complement our analysis by estimating the following equations:

$$\overline{z}_{st} = \tau_1 V_{st} + \overline{X}^{'}_{st}\tau_2 + \mu_s + \vartheta_t + \omega_{st} \tag{3-15}$$

Where $\overline{z}_{st}$ indicates the share of students from school s who repeat a grade at the end of the year t or who drop out of or switch to another school in year t. Finally, we complement regressions (3-15) with specifications at the student level for 5th graders which include interactions between violence and socioeconomic characteristics. The linear equations are given:

$$z_{ist} = \kappa_1 V_{st} + (V_{st} * X^{'}_{ist})\kappa_2 + X^{'}_{ist}\kappa_3 + \mu_s + \vartheta_t + \omega_{st} \tag{3-16}$$

Where z_{ist} is a dummy variable at the student level indicating grade repetition, dropping out or transferring. The second term of the right side of equation (3-16) represents interactions between V_{st} and 5th graders' socioeconomic characteristics, included in X_{ist}. The terms μ_s and ϑ_t control, respectively, for school and time fixed effects.[17] We apply this strategy to identifying whether student attrition, retention, as well as drop out rates differ by gender, race, age and mother's education level.

Finally, there is a potential concern related to our measure of violence, since we do not track actual violence, but the number of reports about conflicts. We should stress that the use of such reports to measure violence would be of

[17]Ideally, we should control for student and school fixed effects, but we cannot include both because they are highly correlated. In addition, z_{ist} does not vary much over time. This leads us to choose school, rather than student, fixed effects.

concern to our analysis only if the propensity to report in some neighborhoods changes over time, due to factors also correlated with student outcomes. In order to investigate further this concern, we test several validity checks.

One way to check the validity of Disque-Denúncia data is to cross-check it with official homicide data. Figure 3.4 shows how the number of homicides in the city of Rio de Janeiro and levels of violence documented in Disque-Denúncia reports changed between 2004 and 2009. Note that we are interested in understanding the trends in both variables, rather than comparing levels of violence. The trends in both series are remarkably similar. Both indicate that 2004 was the most violent year; that after 2004, violence declined; but that violence had peaked again by 2009. The largest difference between the two variables occurs in 2006, when a reduction in the number of reports was not followed by a decrease in the number of homicides. Figure 3.5 shows the yearly correlation between the number of homicides and the number of days with conflicts, aggregated per AISP (the city division used by the police department). We observe that in all years, there is a strong correlation between the two measures, which vary from 0.48 in 2004 to 0.74 in 2006 and 2007. Therefore, comparing the number of homicides to Disque-Denúncia shows that Disque-Denúncia data provide a reasonable picture of variations in violence across time and space.

In addition, comparing Disque-Denúncia data with homicide data offers clues to whether the propensity to report changes over time. Figure 3.5 indicates that each AISP consistently tends to be situated above or below the prediction lines, suggesting that a regional propensity to over or under-report is constant over time. Table 3.5 formalizes this finding by showing the actual and predicted homicide based on the number of days with reports in each AISP and year, and on whether the region over or under-reported violence each year. This exercise indicates that 11 AISPs always over-report violence, i.e., have a predicted homicide level greater than the actual number, while five AISPs always under-report. Only AISPs 14 and 31 demonstrate changes in their propensity to report over time.[18] These two AISPs are located in Rio de Janeiro's Western Zone, a region which was marked during the period under analysis by increasing in militia dominance. There is evidence that the militia intimidates the local population (see Cano & Ioot (2008) and Soares et al. (2010)) which can change the propensity to report conflicts. Although it is not

[18]AISP 14 includes the following neighborhoods: Anchieta, Guadalupe, Parque Anchieta, Ricardo de Albuquerque, Campo dos Afonsos, Deodoro, Jardim Sulacap, Magalhães Bastos, Realengo, Vila Militar, Bangu, Gericinó, Padre Miguel and Senador Camará. AISP 31 includes Barra da Tijuca, Camorim, Grumari, Itanhangá, Joá, Recreio dos Bandeirantes, Vargem Grande and Vargem Pequena

clear what the militia's effect on student outcomes might be, we deal with this concern in the robustness check by excluding Rio de Janeiro's Western Zone from the sample.

Another way to validate our data is to compare it with principals' answers about student exposure to violence at school. In Prova Brasil survey, principals were asked about whether specific events had happened at their schools in 2007. In Table 3.4 we showed how the means of these variables differed between violent and non-violent schools. Table 3.6 shows the correlation between our main measure of violence and principals' answers, after controlling for principals' characteristics (e.g. how long they have been on the job, their education), students' average characteristics (e.g. share of blacks, share of females) and school inputs. Each column has a different dependent variable that indicates whether the cited event happened in a given school in 2007. Table 3.6 indicates that Disque-Denúncia measure is correlated with violent events associated with drug traffic and consumption outside schools but not with robbery and violence inside schools. This corroborates our argument that we explore drug traffic violence and not other types of violence, such as robberies and assaults.

A final way of checking the validity of our measure of violence is to compare Disque-Denúncia reports with newspapers coverage. We read all the news about violence in Casos de Policia and Plantão de Policia blogs in 2009. All the conflicts among drug gangs that were mentioned in the blogs corresponded to at least one report in our database. But Disque-Denúncia offered a much more complete picture of gang conflicts because it cited events that were not covered by the newspapers. Unfortunately, the information provided in the blogs gives few details of events, which made it difficult to draw systematic comparisons between newspaper and Disque-Denúncia coverage.

3.6 Results

This section presents our empirical results as follows. We first present the baseline estimations for achievement test scores. Second, we show results for students' mobility, dropout and grade repetition. Finally, we discuss mechanisms that could explain the relationship between violence and student achievement.

3.6.1 Achievement test scores

The baseline results for model (3-14) are presented in Table (3.7). Panel A presents results for achievement test scores in math, while Panel B shows results for language. The first column follows a random effect specification, which includes only year fixed effects. We see that violence is negatively correlated with students' achievement in both panels, and highly robust in Panel A. In the second column we include controls for students' socioeconomic composition. As a result, we see that significance vanishes in both Panels. This result is consistent with cross-section empirical evidence supporting the importance of socioeconomic disadvantages in explaining children's outcomes vis-a-vis local violence effects. However, the third column shows that once school fixed effects are introduced in our longitudinal analysis, the negative effects of violence stand out, particularly in Panel A. We provide evidence below that this result is robust to more flexible measures of violence, controlling for outliers and selecting for different samples. The estimated coefficient in column 3, Panel A, indicates that moving from the bottom decile (p10) to the top decile (p90) of the distribution of violence across schools is associated with a score 1.76 points lower in math exams. This implies that a school in a highly violent area scores 0.17 standard deviations less on standardized math tests than schools in relatively peaceful areas.[19]

The differences between math and language results are of particular interest. Coefficients and significance are higher in Panel A, much in line with the idea that violence effects on math scores are relatively more pervasive once math learning is more demanding in terms of concentration and instruction.

3.6.2 Mobility, dropout and grade repetition

As mentioned above, any impact violence may have on students' patterns of mobility and dropout during the year is of concern once achievement test scores are taken at the end of the school year. Yet, lower math scores can be driven by changes in the composition of students during the year. For instance, if more capable students move from schools in violent areas towards less violent areas during the year, we overstate the estimated effect of violence. Furthermore, besides this potential bias, the relationship between violence and mobility patterns is also of policy concern by its own, particularly if violence hit more disruptively the probability of dropping out of those children from more disadvantaged households.

Table 3.8 presents results for model (3-15) for 5th grade students. The first columns shows that exposure to violence raises the rate of grade repetition for 5th graders. This result reinforces the evidence from Table 3.7 once grade repetition can be thought as an alternative measure for student achievement - yet endogeneity must be of concern given that schools may adjust repetition criterion in response to local violence. The estimated coefficient for 5th graders'

[19] We use as reference math score standard deviation across schools in the base year (2005).

repetition rates indicates that students from schools in the top decile of the distribution of violence have a failure rate 2 percentage points higher than students from schools situated in the bottom decile (p10). The magnitude of this effect represents 23% of the sample average.

Along with the results from Table 3.7, the result from column 1 may otherwise show an upward biased coefficient if more capable students leave the schools under violent conflicts during the year. Thus, in the remaining columns we examine dropout and mobility patterns. In the second column, we see that exposure to violence is significantly associated with higher rates of dropout for 5th graders. The estimated coefficient indicates that dropout rates are 3 percentage points higher in a school localized in a violent area (p90 of the distribution of violence) when compared to schools from relatively peaceful areas (p10). The magnitude of this effect represents 10% of the sample average. Columns 3 and 4 show, respectively, that neither the share of students transferred out of school nor the share of new admissions during the year are significantly associated with violence. Thus, Table 3.8 shows that violence affects students selection only by increasing the number of those students who drop out. This effect may have a selective impact on the remaining pool of students at the end of the year depending on the type of students that are more likely to dropout due to violent events. In order to examine further this question, we run model (3-16) for 5th graders, at the student level, interacting violence and students' observable characteristics.

Table 3.9 displays the results. Each column interacts separately violence and socioeconomic characteristics, while column 6 shows the more complete specification, where violence shocks are interacted simultaneously with different socioeconomic characteristics. The results that stand out in this last specification suggest that violence impacts relatively more the probability of dropping out of black and brown students. In Rio de Janeiro, nonwhite families and their children are more likely to live in slums and impoverished areas. The heterogeneous effect we find in Table 3.9 can reflect, for instance, that nonwhite students are more exposed to violent events - probably because their households, within slums, are closer to conflicts. On the other hand, race may capture other non-observable characteristics associated with poverty and socioeconomic disadvantages, which generally influence the individual's background and cognitive development. Thus, we can reasonably assume that nonwhite students are both those relatively more exposed to violence, and those who face more severe opportunities to learn and develop cognitive skills due to other disadvantages. This assumption suggests that violence might push the pool of remaining students at the end of the year towards a group of relatively high performing pupils. Therefore, selection bias might underestimate our estimated effects of violence exposure on student achievement and grade repetition. Thus, the effects of exposure to violence found in Table 3.7 might be a lower bound for the real impact.

3.6.3 What could explain violence effects on student achievement?

In this section we discuss two likely channels that can explain the negative effect of violence on student achievement. First, we explore the timing of the violence during the school year. Different effects of violence during classes or during vacations can shed light on the relative role of exposure to violence at home vis-a-vis exposure to violence at school. Thus, we can examine further to what extent violence effects work through the school vis-a-vis the household environment. Second, we study the relationship between violence and the patterns of teacher absenteeism and medical leaves. The relationship between violence and teachers' behavior during the year may help us to confirm whether violence work at the school environment through its human resources.

Violence Timing

We use our baseline model (3-14) in order to estimate the specifications shown in Table 3.10. The first column uses as variable of interest our measure of violence computing only the number of days with conflicts that occurred during school months (from February to June, and from August to October, up to Prova Brasil exams). In the second column, we consider only violent events that took place on January and July, months of school vacations. We see negative and significant effects in both specifications, with similar coefficients in terms of magnitude. This result suggests that the household environment channel might be of substantial importance, while we cannot reject the importance of the school environment channel. The result for the effects of violence during the vacation period suggest that exposure to violence can affect children outcomes via mental health and psychological disorders, as discussed in section 3.4.2.

Teacher absenteeism and medical leaves

In this section we examine the relationship between violence and teacher absenteeism.We use administrative records to build variables on teachers' unexcused absences and medical leaves at the school level. Unexcused absences are reported by the school's principal and is subject to endogeneity once the principal may under-report absenteeism in response to violence and safety threats. We believe that only events of long term absences are reported. Thus, unexcused absences may be interpreted as a combination of absences and turnover. Medical leaves are the main cause of teacher absenteeism in the municipal system. This type of leave is conceded only after medical examination supervised by the department of human resources of the municipal education authority.

For both types of teacher absenteeism, we calculate two variables at the school level for the years between 2004 and 2008. The first one is the sum of days of absence taken by all teachers of the school during the year. The second variable considers the average absence length, which is the former variable (sum of days of absence taken by all teachers of the school during the year) divided by the number of absence requests.[20]

These indicators are then used as dependent variables in our baseline model (3-14), which also includes as controls the number of teachers and students per school, as well as the the other controls used previously - school and year fixed effects and students' composition. Table 3.11 shows the results. Violence has a positive impact on unexcused absences but it does not affect medical leaves. Schools situated in the percentile 90 of the distribution of violence register more 3.06 days of absence during the year when compared to those schools situated in the percentile 10. This effect represents 17% of the sample average. The same variation in violence is associated with an increase in the average length of absences of 1.42 days, which represents 24% of the sample average. Taking into account that schools might under-report absences in response to external violent events, the evidence so far suggests that violence has disruptive effects on the school environment and student achievement through the human resources channel.

3.7 Robustness

In this section we present robustness checks for our results for achievement test scores in math presented in Table 3.7. Table 3.12 tests different empirical specifications. In the first two columns, we examine the role of past and future violent conflicts. In column 1 we include two lags of violence. Past violence coefficients help us to examine whether past violence is a significant input for contemporary achievement. We find no evidence that past violence is significantly associated with contemporaneous achievement, while the coefficient of contemporaneous violence remains at a similar magnitude and significant at 10%. Column 2 includes future violence in addition to lagged violence. This specification tests whether reverse causality is of concern and whether strict exogeneity assumption holds. We observe that coefficients of future shocks

[20] If a teacher absence for 30 days in the year divided in two spells, she enters twice in the denominator. If she absences 30 days uninterruptedly, she counts only once.

show no significant effects on contemporaneous achievement. However, significance of contemporaneous violence does not hold under this specification. This result must be driven by the fact that our sample is severely restricted only to variations between 2005 and 2007. Information from 2009 was discarded once we have no available data on violence for 2010 and 2011.

Specification in column 3 follows a weighted regression, using as weights the average number of 5th graders per school across time. This specification reduces the influence of small schools, in which few students contribute to the average score. We observe that our main result is robust to the use of weights. The point estimate of 0.97 is almost the same as the one presented in Table 3.7 (-1.09) and a little more noisier (standard-deviation of 0.52), but still significant at 10 percent confidence level.

Table 3.13 presents another set of robustness checks. We test whether our results are robust to sample selection, to the exclusion of outliers and to alternative measures of violence. In Panel A, we use the same measure of violence used throughout the paper. The first column shows our baseline result, the same displayed in Table 3.7, Panel A. In the second column we exclude outliers, i.e., the schools with extremely high records of violence. We define as outliers the schools from the top 1% of the respective violence distribution (the violence measure varies in each line). We see that the point estimate is marginally reduced, but still significant at 10%. In the third column, we exclude schools situated in Rio de Janeiro's Western Zone, where we are more subject to violence measurement error due to the presence of militia (see section 3.5.2). We observe that the point estimate almost double and significance is maintained at 5%. This result indicates that moving from percentile 10 to percentile 90 of violence distribution is associated with a 0.23 standard deviation decrease in math test scores. Specification in column 4 restricts the sample only to those schools located within 500 meters from a slum, while in column 5 we use only those schools distant at least 500 meters from a slum. We observe in column 4 that the significance and the magnitude of the coefficient are very similar to our baseline specification in the first column, while significance in column 5 vanishes. This result indicates that violence has very localized impacts.

In Panel B, we test whether another measure of violence, the standard deviation of our baseline measure of violence within years, is also associated with lower test scores. This measure is calculated as $ViolenceSD_{st} = \sum_j D_{sj} sd_{jt}$, where sd_{jt} is the standard deviation of the number of days with conflicts across months in each slum, and D_{sj} is the inverse of the distance between school s and slum j. As we can see from this specification, positive variations in this measure are associated with lower test scores no matter the sample we use. This result suggests that unevenly distributed violent events negatively affect student achievement.

In Panel C we use a much more flexible measure of violence in order to test not only robustness, but also the sensibility of our findings according to the distances between schools and slums. We use alternative measures of violence based on buffers of 5 meters, 250 meters and 500 meters around the school. Our three alternative measures of violence indicate the number of days with conflicts in slums within, respectively, a radius of 5, 250 and 500 meters from each school. In order to test the alternative specifications we use the baseline model (3-14), changing only samples and violence measures. We change the violence measure as indicated in the rows, and the sample as indicated in the columns of Table 3.13. We observe that in all columns the effect drops with buffer size, which is another result supporting the view that violence shocks have localized effects. The strongest effect is found for schools located within slums (buffers of 5 meters). Almost all measures are robust to alternative samples, particularly the 250-buffer. Row 4 in column A indicates that an additional day with conflict within 250 meters of distance from the school is associated with a 0.025 standard deviation decrease in math test scores. The results are robust to all measures of violence when we restrict our sample to those schools located within 500 meters from a slum.

Finally, in Panel D we alternatively use as a measure of violence the number of reports instead of the number of days with reports within the buffer of 250 meters. We find significant coefficients at 10%, with only column 3 as an exception.

Therefore, the set of results found so far seem robust to different measures of violence and sample selections. Given the magnitude of the estimated effects, the evidence suggests that exposure to local violence may have a substantial and pervasive role in children outcomes, particularly on those that study and live closer to the epicenters of violent events.

3.8 Conclusion

This paper investigates whether and how armed conflicts among drug gangs in Rio de Janeiro's slums affect children's educational outcomes. We explore time and geographical variation in localized violent events in order to identify causal effects of neighborhood violence on student achievement. By estimating a negative effect of violence on students' math scores, we find support to the view that exposure to violence has disruptive effects on children's outcomes .

This study provides several contributions. First, we develop a novel database, which contains precise information on whether and when drug conflicts happened, which allows a much better understanding of the problem. Second, we provide a better test to estimate violence impacts on schooling than the previous literature. This is possible once our dataset allows us to compare schools that are managed by the same municipal system and are, therefore, under the same rules and incentives, but vary in their distance to violence epicenters. In addition, the longitudinal structure of the data allows us to use school fixed effects and control for intrinsic characteristics of schools that are correlated with students and neighborhood demographics, leading to more precise estimates of violence impact.

Our results show that violence reduction should be a priority policy since its effects have a far-reaching impact beyond the great number of deaths caused by violent events. Our results support the view that violence accentuates the poverty trap, since it is particularly acute in poor areas. By decreasing the quality of learning in these areas, it makes the way out of poverty even harder for those children from disadvantaged households.

Table 3.1: Determinants of Drug Conflicts

	Dependent variable: Number of days with conflicts in the slum between 2004 and 2009								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Slum characteristics:									
steepness	0.27								0.15
	(0.03)***								(0.06)**
distance to main road		-1.03							-0.44
		(0.34)***							(0.17)**
area (1999)			0.04						0.03
			(0.00)***						(0.01)***
Neighborhood characteristics:									
population density				227.2					131.95
				(66.1)***					(52.18)**
population					-0.17				0.53
					(0.44)				(0.36)
% youngsters on						-65.0			-57.02
population (13-19 years)						(30.8)**			(45.35)
income pc							0.93		-1.90
							(1.14)		(1.37)
gini index								-3.64	8.10
								(6.43)	(5.77)
Observations	825	825	825	825	825	825	825	825	825
R^2	0.10	0.01	0.14	0.0	0.00	0.0	0.00	0.00	0.20

Notes: This Table presents coefficients from cross-section regressions of the number of days with conflicts between 2004 and 2009 on slum and neighborhood characteristics. The unit of analysis is the slum. Slum steepness is the standard deviation of the altitude within the slum calculated using GIS and NASA raster data. Distance to main road measures the smallest linear distance from slum border to a city main road, where the main roads are defined by Instituto Pereira Passos. Neighborhood information refers to population statistics, income per capita and gini index from the city neighborhood that the slum fall within. Asterisks indicate significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table 3.2: Disque-Denúncia Database Summary Statistics

Number of reports between 2004-2009		3,571		
Reporting gunfight		3,127		
on slums		2,884		92%
other places		243		8%
Number of slums		979		
with at least one report of gunfight		289		30%
without reports of gunfight		690		69%
Slums with conflicts				
	Number of reports		Number of days	
	per year	2004-2009	per year	2004-2009
mean	1.7	10	1.4	8
sd	5	18	3	14
p50	0	3	0	3
p90	4	26	4	22
max	85	146	41	96

Table 3.3: School Distribution

Proximity to slums	Number	%	% cumulative
Inside	25	3.4	3.4
Within 5-250 meters	321	43.6	47.0
Within 250-500 meters	192	26.1	73.1
More than 500 meters	198	26.9	100.0
Total	736		

Notes: This table presents the distribution of the 736 schools used in our sample by proximity to at least one slum. The proximity measure considers the linear distance between each school and the closest slum border.

Table 3.4: Education Summary Statistics

	Total		With violence =< 250 meters		Without violence =< 250 meters		
	Mean	sd	Mean	sd	Mean	sd	
Students' achievement and mobility:							
IDEB	4.65	(0.75)	4.36	(0.70)	4.75	(0.74)	***
math score	199.9	(18.9)	194.4	(17.6)	201.9	(19.0)	***
portuguese score	185.7	(16.9)	179.4	(15.4)	188.1	(16.8)	***
failure	0.09	(0.06)	0.1	(0.06)	0.09	(0.06)	***
dropout	0.04	(0.04)	0.05	(0.05)	0.03	(0.04)	***
school transfers	0.16	(0.06)	0.16	(0.06)	0.16	(0.06)	***
school admissions	0.18	(0.08)	0.16	(0.08)	0.19	(0.08)	***
Students' characteristics:							
% men	0.52	(0.03)	0.52	(0.03)	0.52	(0.03)	***
% white	0.37	(0.10)	0.33	(0.10)	0.38	(0.10)	***
mean age	9.25	(0.92)	9.11	(0.76)	9.3	(0.96)	***
% illiterate father	0.03	(0.02)	0.03	(0.03)	0.02	(0.02)	***
% illiterate mother	0.02	(0.02)	0.03	(0.02)	0.02	(0.02)	***
% mother and father at home	0.45	(0.13)	0.43	(0.12)	0.46	(0.13)	***
% live close to school	0.68	(0.24)	0.81	(0.16)	0.63	(0.24)	***
% evangelical	0.22	(0.08)	0.23	(0.08)	0.21	(0.08)	***
School characteristics:							
number students	537	(248)	574	(268)	524	(238)	***
pupil- teacher ratio	25.0	(6.7)	24.7	(6.5)	25.1	(6.7)	
% teachers with college degree	0.6	(0.24)	0.59	(0.22)	0.6	(0.24)	**
% principals < 4years on the job	0.04	(0.21)	0.05	(0.22)	0.04	(0.20)	
% with science lab	0.1	(0.30)	0.07	(0.25)	0.11	(0.32)	***
% with computer lab	0.38	(0.49)	0.45	(0.50)	0.36	(0.48)	**
% with free meal	0.99	(0.10)	0.99	(0.11)	0.99	(0.09)	
% with sport court	0.59	(0.49)	0.6	(0.49)	0.59	(0.49)	
Principal reported problem with:							
suspended classes	0.05	(0.21)	0.14	(0.35)	0.01	(0.11)	***
teachers' absence	0.04	(0.20)	0.04	(0.20)	0.04	(0.20)	
students' absence	0.07	(0.26)	0.10	(0.30)	0.06	(0.24)	*
drug consumption close to school	0.24	(0.43)	0.39	(0.49)	0.18	(0.39)	***
drug traffic close to school	0.22	(0.42)	0.44	(0.50)	0.14	(0.34)	***

Notes: This table presents a comparison of the average students' achievement and mobility, students' characteristics, school characteristics and principals' reports between schools with and without violence within 250 meters from the school. Column 1 reports the means for the 736 schools used in our sample. Column 3 reports the means for the 199 schools exposed to violence within 250 meters in any moment between 2004 and 2009, while column 5 reports the means for the other 537 schools non-exposed to violence in the period. Columns 2,4 and 6 report standard errors for each sample. Asterisks presented in column 7 indicates whether the difference in means between columns 3 and 5 are significantly different than zero at 99 (***), 95 (**), 90 (*) percent confidence.

Table 3.5: Testing for under-reporting

AISP		2004	2005	2006	2007	AISP		2004	2005	2006	2007
1	Homicide rate	118	83	71	74	16	Homicide rate	129	149	150	170
	Pred homicide	184	266	205	174		Pred homicide	217	165	180	183
	Under-reporting	0	0	0	0		Under-reporting	0	0	0	0
2	Homicide rate	36	20	33	23	17	Homicide rate	80	49	59	38
	Pred homicide	97	100	66	58		Pred homicide	151	150	107	54
	Under-reporting	0	0	0	0		Under-reporting	0	0	0	0
3	Homicide rate	153	135	166	199	18	Homicide rate	138	150	133	123
	Pred homicide	221	215	252	322		Pred homicide	88	105	66	72
	Under-reporting	0	0	0	0		Under-reporting	1	1	1	1
4	Homicide rate	45	33	43	22	19	Homicide rate	16	19	11	12
	Pred homicide	82	70	60	63		Pred homicide	91	77	86	72
	Under-reporting	0	0	0	0		Under-reporting	0	0	0	0
5	Homicide rate	38	55	42	37	22	Homicide rate	209	137	110	115
	Pred homicide	76	60	55	49		Pred homicide	140	80	81	91
	Under-reporting	0	0	0	0		Under-reporting	1	1	1	1
6	Homicide rate	54	67	79	88	23	Homicide rate	37	41	33	28
	Pred homicide	186	155	143	169		Pred homicide	101	140	91	63
	Under-reporting	0	0	0	0		Under-reporting	0	0	0	0
9	Homicide rate	617	532	480	454	27	Homicide rate	238	182	232	231
	Pred homicide	178	205	273	345		Pred homicide	155	80	164	151
	Under-reporting	1	1	1	1		Under-reporting	1	1	1	1
13	Homicide rate	17	14	14	18	31	Homicide rate	50	46	51	38
	Pred homicide	68	57	45	49		Pred homicide	68	57	45	49
	Under-reporting	0	0	0	0		Under-reporting	0	0	1	0
14	Homicide rate	372	368	414	339	39	Homicide rate	305	326	344	327
	Pred homicide	412	301	444	294		Pred homicide	136	123	102	77
	Under-reporting	0	1	0	1		Under-reporting	1	1	1	1

Notes: This Table presents the actual and predicted homicide rate of each Área Integrada de Segurança Pública (AISP), which is a division of Rio de Janeiro used by Police Authority to provide crime statistics. In order to calculate predicted homicide, we run yearly regressions of homicide rate on the number of days with reports about armed conflicts. We then used the estimated coefficient to generate predicted homicide. Under-reporting indicates whether the predicted homicide rate is lower than the actual homicide rate.

Table 3.6: Principals' reports about school violence

Caused by:	Teachers robbed inside school non-students	Teachers robbed inside school students	Drug consumption outside school non-students	Drug traffic inside school non-students	Drug traffic outside school non-students	Gangs outside school	Gangs inside school
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Violence	-0.013	0.001	0.058	-0.003	0.080	0.050	0.010
	(0.007)*	(0.007)	(0.026)**	(0.009)	(0.026)***	(0.018)***	(0.010)
Observations	570	418	595	564	593	650	646
R^2	0.049	0.080	0.088	0.035	0.136	0.088	0.040

Notes: This table reports the results of cross-section regressions of the dependent variable indicated in each column on DD violence measure (the logarithm of the weighted sum of all slums' days with armed conflicts, where the weight is the inverse of the distance from each slum to the school). The dependent variables are binary indicators for whether the cited event happened in the school in 2007. We include as controls principals' characteristics (how long she is on the job in the school, previous experience as principal, age range, education, whether she has another job), students' average characteristics (share of whites, average asset index, share that live close to school) and school characteristics (number of students and whether the school has kitchen, principal room, science lab, computer lab, free meals, sport court, and teachers' room). Robust standard errors in parentheses, significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.7: Achievement Regressions at the School Level - Math and Language Test Scores

	Effects of violence on Math and Language test scores		
	(1)	(2)	(3)
	Panel A: Math		
Violence	-1.025	-0.261	-1.094
	(0.381)***	(0.352)	(0.533)**
	Panel B: Language		
Violence	-0.662	-0.127	-0.272
	(0.380)*	(0.328)	(0.495)
Common Specifications:			
Observations	2,125	2,117	2,117
Number of schools	736	736	736
School FE	NO	NO	YES
Students composition	NO	YES	YES
Year FE	YES	YES	YES

Notes: Robust standard errors in parentheses, adjusted for clustering at the school level in all specifications. Significance: *** p<0.01, ** p<0.05, * p<0.1. Dependent variables are school's average achievement test scores in math (Panel A) and language (Panel B) for 5th graders in years 2005, 2007 and 2009. All regressions include year fixed effects. Columns (2) and (3) include number of 5th graders per school and controls for 5th graders' socioeconomic composition, averaged at the school level: average age, share of white students, share of boys, students' mothers education (share of mothers with incomplete primary, complete primary, complete secondary and college). Only column (3) includes school fixed effects. Variable of interest (violence) is the logarithmic of the sum of days of conflicts per slum weighed by the distance between school and slums.

Table 3.8: Grade Repetition, Dropout and Mobility in the 5th Grade

	Grade Repetition	Dropout	Transfers (out of school)	Admissions (in)
	(1)	(2)	(3)	(4)
Violence	1.253	0.188	-0.099	-0.900
	(0.605)**	(0.087)**	(0.278)	(0.624)
Observations	4,215	4,218	4,218	4,218
Number of schools	736	736	736	736

Notes: Robust standard errors in parentheses clustered at the school level. Significance: *** p<0.01, ** p<0.05, * p<0.1. Dependent variables are schools' average rates of grade repetition (share of students that repeated the grade over total students at the end of the year in that grade), dropout (share of dropout over total students at the beginning of the year), transfers and new admissions (both analogously to dropout) among 5th graders. The sample includes years between 2004 and 2009. All regressions include school and year fixed effects, and students' composition per grade and year: number of students, average age, share of white students, share of boys, students' mothers education (share of mothers with incomplete primary, complete primary, complete secondary and college). Variable of interest (violence) is the logarithmic of the sum of days of conflicts per slum weighed by the distance between school and slums.

Table 3.9: Heterogeneity in Students' Dropout

	Dependent variable: Dropout in 5th grade					
	(1)	(2)	(3)	(4)	(5)	(6)
Violence (V)	0.0006	0.0004	0.0008	-0.0010	0.0142	0.0125
	(0.0010)	(0.0010)	(0.0011)	(0.0012)	(0.0100)	(0.0100)
V * (High educated mother)		0.0018				0.0013
		(0.0011)*				(0.0009)
V * Boys			-0.0003			-0.0001
			(0.0010)			(0.0009)
V * (Brown or Black)				0.0022		0.0026
				(0.0009)**		(0.0009)***
V * (Age)					-0.0012	-0.0013
					(0.0009)	(0.0009)
Observations	392,843	392,843	392,843	392,843	392,843	392,843

Notes: This table presents coefficients from regressions of student's dropout in the 5th grade on violence and violence interacted with students' characteristics. Observations are at the student level. Variable of interest (violence) is the logarithmic of the sum of days of conflicts per slum weighed by the distance between school and slums. All regressions include school and year FE and controls for number of students at 5th grade and students' characteristics: age, dummy indicating boys, race (brown or black) dummy, high educated mother (high school or college degree) dummy. Sample covers years between 2004 and 2009. Standard errors adjusted for clustering at the school level in parentheses, significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.10: Violence Timing

	Dependent variable: Math test scores	
	(1)	(2)
Violence during classes	-1.177	
	(0.546)**	
Violence during vacations		-1.278
		(0.568)**
Observations	2117	2117
Number of schools	736	736

Notes: This table presents coefficients from regressions of Prova Brasil math score on violence. The variable violence is the logarithmic of the sum of days of conflicts per slum weighed by the distance between school and slums. Violence during school period is the number of days with conflicts from February to June and from August to October (until Prova Brasil application). Violence during vacation period includes the number of days with conflicts in January and July. All regressions include school and year fixed effects and controls for the number of students at fifth grade and fifth graders' average characteristics (share of men, average age, share of whites, mother's education). The period of analysis covers the years in which Prova Brasil was applied (2005, 2007 and 2009). Standard errors adjusted for clustering at the school level in parentheses, significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.11: Teachers Absenteeism

	Unexcused Absences		Medical Leaves	
	Number of Days of Absence	Absence Length	Number of Days of Absence	Absence Length
	(1)	(2)	(3)	(4)
Violence	1.900	0.856	3.170	0.440
	(0.822)**	(0.423)**	(8.609)	(0.742)
Y mean	18.18	6.03	394.6	28.4
Observations	4,035	4,035	4,035	4,035
Number of schools	736	736	736	736

Notes: This table presents coefficients from regressions of teachers' absenteeism on violence. Dependent variables are measured at the school level as follows. Number of days of absence refer to the sum of days of absence taken by all teachers of the school during the year. Column (1) refers to unexcused absences while column (3) refers to absences due to health problems. Analogously, in columns (2) and (4) absence length refers to the sum of days of absence taken by all teachers of the school during the year divided by the number of absence requests. Violence is the logarithmic of the sum of days with conflicts in each slum weighed by the distance between school and slums. All regressions include school and year fixed effects, and controls for the number of teachers in the school, the number of students at fifth grade and fifth graders' average characteristics (share of men, average age, share of whites, mother's education). Sample covers years between 2004 and 2009. Standard errors adjusted for clustering at the school level in parentheses, significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.12: Econometric Specification Robustness Checks

	Dependent variable: Math test scores		
	(1)	(2)	(3)
Violence t-2	0.383	0.518	
	(0.641)	(0.930)	
Violence t-1	0.193	0.051	
	(0.771)	(1.234)	
Violence t	-1.024	-0.476	-0.982
	(0.527)*	(0.886)	(0.511)*
Violence t+1		-0.650	
		(1.272)	
Violence t+2		0.381	
		(0.977)	
Observations	2117	1411	2117
Number of schools	736	736	736

Notes: This table presents coefficients from regressions of Prova Brasil math score on violence. The variable violence is the logarithmic of the sum of days of conflicts per slum weighed by the distance between school and slums. All regressions include school and year fixed effects and controls for the number of students at fifth grade and fifth graders' average characteristics (share of men, average age, share of whites, mother's education). In column 3, we weighted the regression by the average number of 5th graders over the sample period. The period of analysis cover the years in which Prova Brasil was applied (2005, 2007 and 2009), except by column 5 which include only 2005 and 2007. Standard errors adjusted for clustering at the school level in parentheses, significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.13: Sample and Measure of Violence Robustness Checks

Dependent variable:	Math test scores				
Sample:	Full Sample	Exclude Outliers	Without Western Zone	Slum Distance < 500m	Slum Distance > 500m
	(1)	(2)	(3)	(4)	(5)
	Panel A: Baseline measure of violence				
Violence	-1.094	-1.011	-1.922	-1.075	1.660
(baseline)	(0.533)**	(0.591)*	(0.774)**	(0.537)**	(4.908)
Schools	736	729	418	538	198
	Panel B: Violence standard deviation				
Violence	-0.004	-0.009	-0.003	-0.004	-0.029
Sd deviation	(0.001)***	(0.004)**	(0.001)**	(0.001)***	(0.537)
Schools	736	729	418	538	198
	Panel C: Buffers (number of days)				
Buffer 5 meters	-0.413	-0.457	-0.355	-0.424	
(num of days)	(0.192)**	(0.688)	(0.204)*	(0.190)**	
Schools	736	728	418	538	
Buffer 250 meters	-0.334	-0.344	-0.264	-0.339	
(num of days)	(0.145)**	(0.172)**	(0.157)*	(0.145)**	
Schools	736	728	418	538	
Buffer 500 meters	-0.140	-0.154	-0.163	-0.150	
(num of days)	(0.083)*	(0.102)	(0.093)*	(0.082)*	
Schools	736	728	418	538	
	Panel D: Buffers (number of reports)				
Buffer 250 meters	-0.222	-0.231	-0.176	-0.224	
(num of reports)	(0.119)*	(0.131)*	(0.129)	(0.118)*	
Schools	736	729	418	538	

Notes: Each table entry represents a regression coefficient of the math test scores on violence. The violence measure is indicated in each line, while in the columns we vary the sample used. The baseline measure (Panel A) is the logarithmic of the sum of days of conflicts per slum weighed by the distance between school and slums. Violence standard deviation (Panel B) is the variation across the number of days with conflicts in each month of the year. The variables buffers (Panel C) indicate the number of days with conflicts in slums within, respectively, 5 meters, 250 meters and 500 meters from school. In Panel D buffer 250m refers to the number of reports about conflicts in slums within 250 meters from school. All regressions include school and year fixed effects and controls for the number of students at 5th grade and 5th graders' average characteristics (share of boys, average age, share of whites, mother's education). Column 2 excludes outliers, the top 1% of the distribution of the respective measure of violence. Column 3 excludes slums and schools from Rio de Janeiro's Western Zone. The period of analysis cover the years in which Prova Brasil was applied (2005, 2007 and 2009). Standard errors adjusted for clustering at the school level in parentheses, significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.14: Grade Repetition, Dropout and Mobility - 1st to 4th grades

	Effects of Violence per Grade Rates Grade Repetition, Dropout and Mobility			
	1st	2nd	3rd	4th
	Panel A: Grade Repetition			
Violence	0.020	-0.034	0.151	-0.027
	(0.146)	(0.091)	(0.310)	(0.243)
Observations	4,069	4,085	4,115	4,280
Number of schools	700	703	701	732
	Panel B: Dropout			
Violence	0.195	0.025	0.159	0.041
	(0.157)	(0.095)	(0.080)**	(0.097)
Observations	4,069	4,086	4,116	4,281
Number of schools	700	703	701	732
	Panel C: Transfers (out)			
Violence	0.088	0.127	0.132	0.105
	(0.281)	(0.197)	(0.231)	(0.283)
Observations	4,069	4,086	4,116	4,281
Number of schools	700	703	701	732
	Panel D: New Admissions (in)			
Violence	0.413	0.113	0.126	0.416
	(0.502)	(0.273)	(0.324)	(0.369)
Observations	4,069	4,086	4,116	4,281
Number of schools	700	703	701	732

Notes: Robust standard errors in parentheses clustered at the school level. Significance: *** p<0.01, ** p<0.05, * p<0.1. Dependent variables are schools' rates of grade repetition (share of students that repeated grade over total students at the end of the year), dropout (share of dropout over total students at the beginning of the year), transfers and new admissions (both analogously to dropout). All dependent variables are per grade and the sample includes years between 2004 and 2009. All regressions include school and year fixed effects, and students' composition per grade and year: number of students, average age, share of white students, share of boys, students' mothers education (share of mothers with incomplete primary, complete primary, complete secondary and college). Variable of interest (violence) is the logarithmic of the sum of days of conflicts per slum weighed by the distance between school and slums. Samples change across columns because some schools do not offer all grades every year.

Figure 3.1: Slum and School Distribution

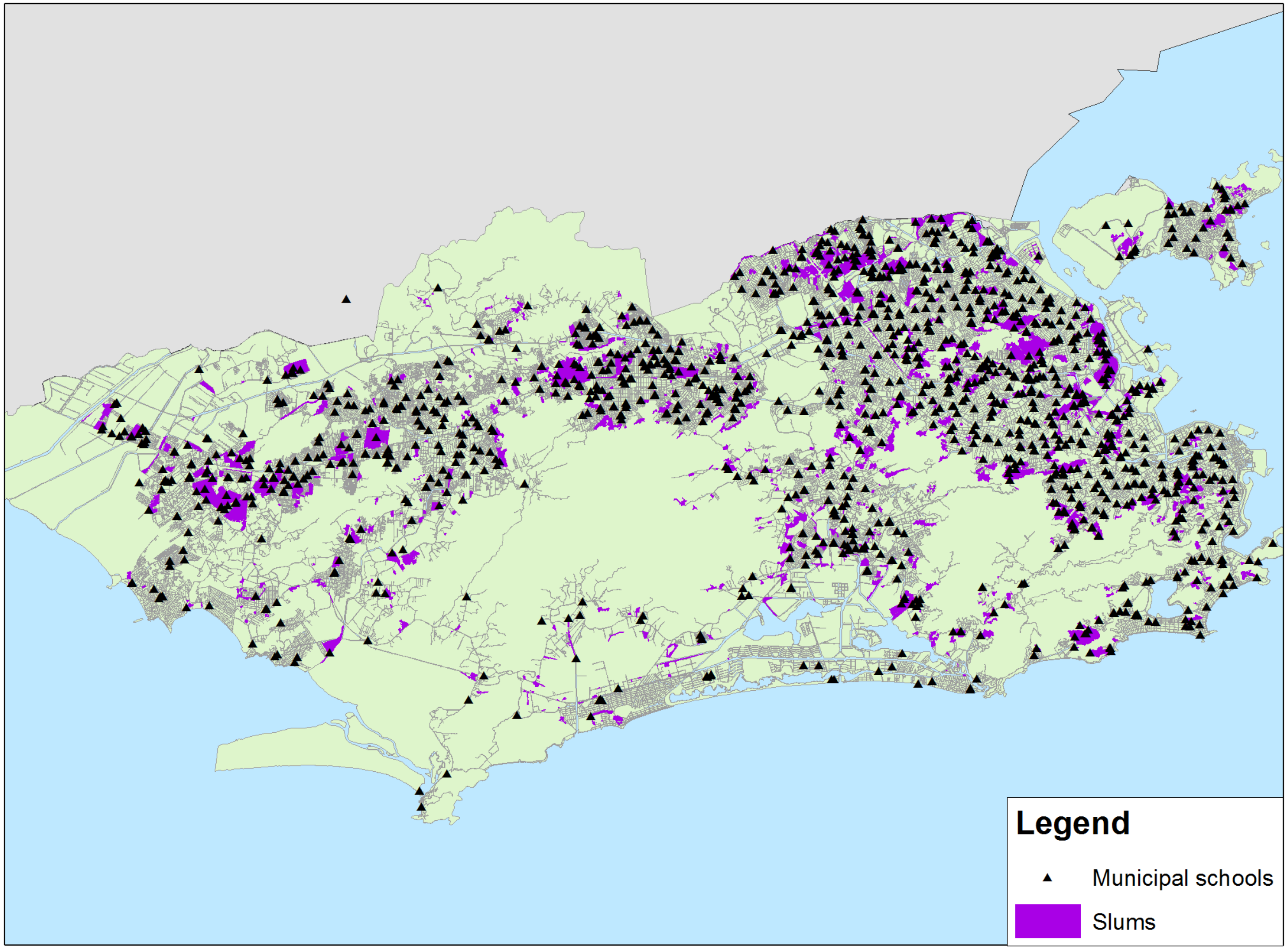


Figure 3.2: Number of Days with Reports about Gunfight 2004-2009

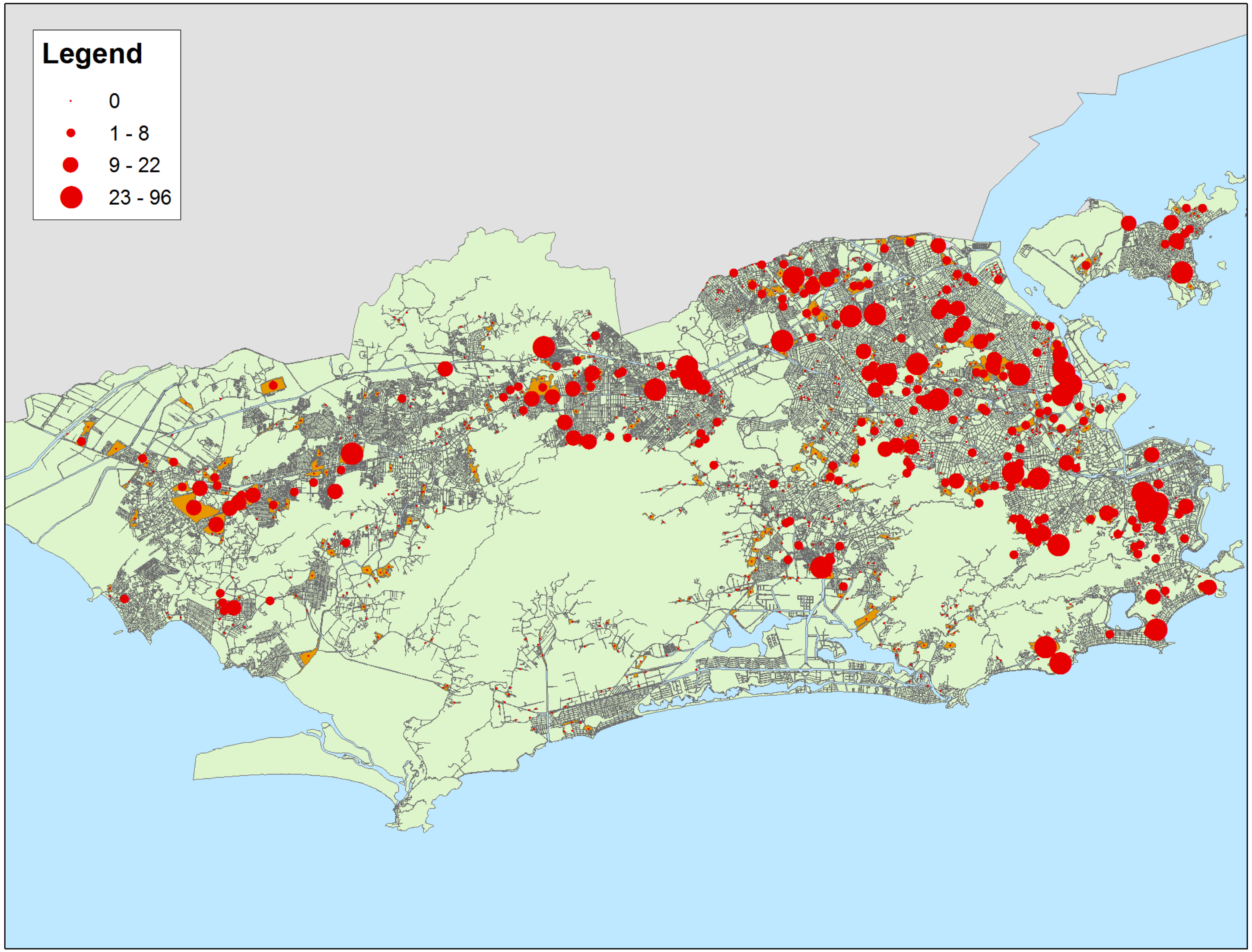
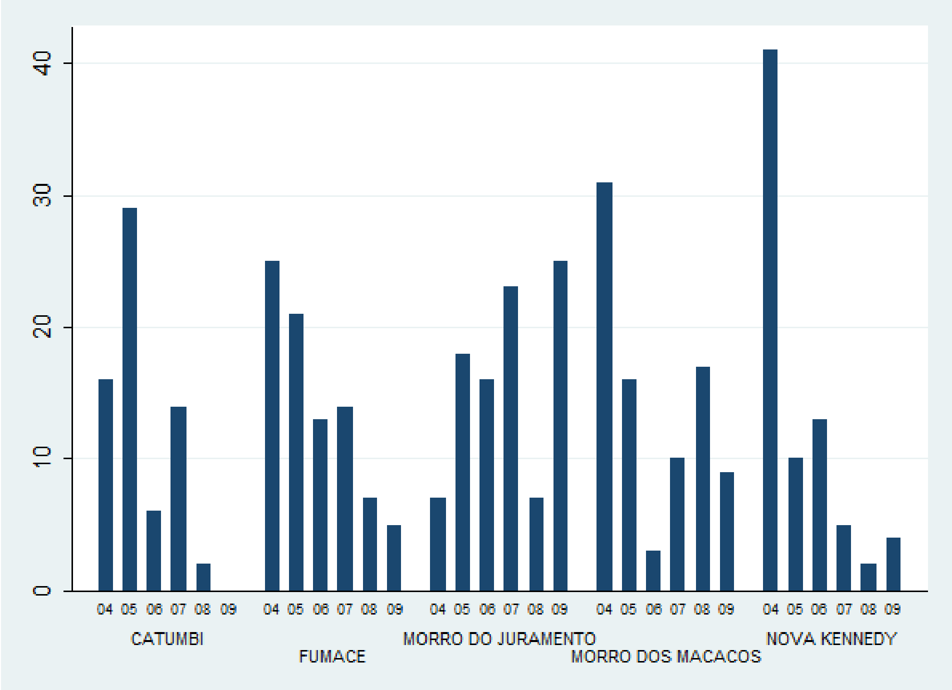


Figure 3.3: Number of Days with Reports about Gunfight per Year in Selected Slums 2004-2009



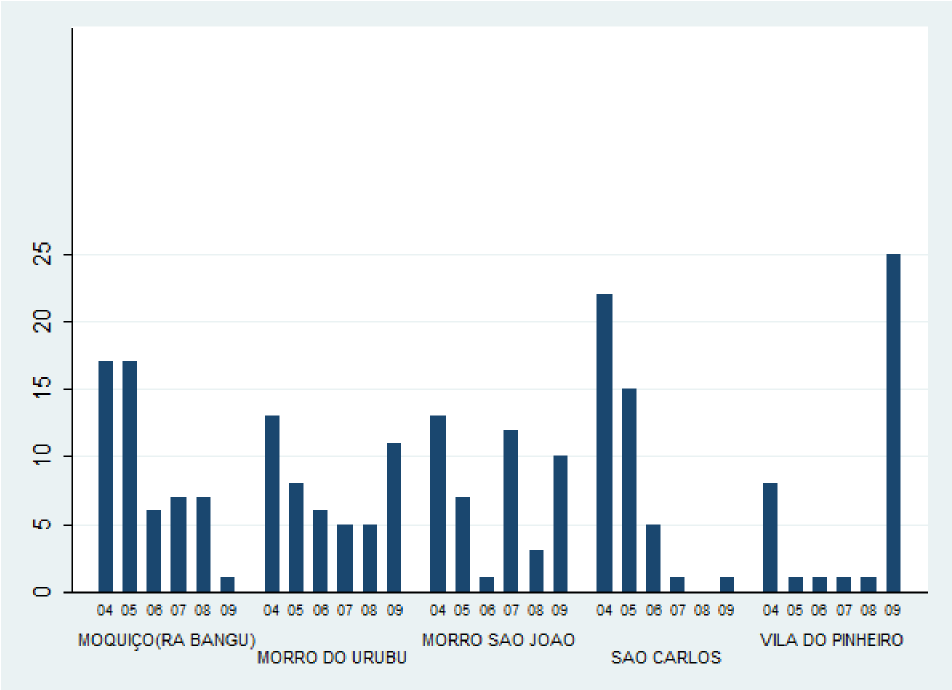
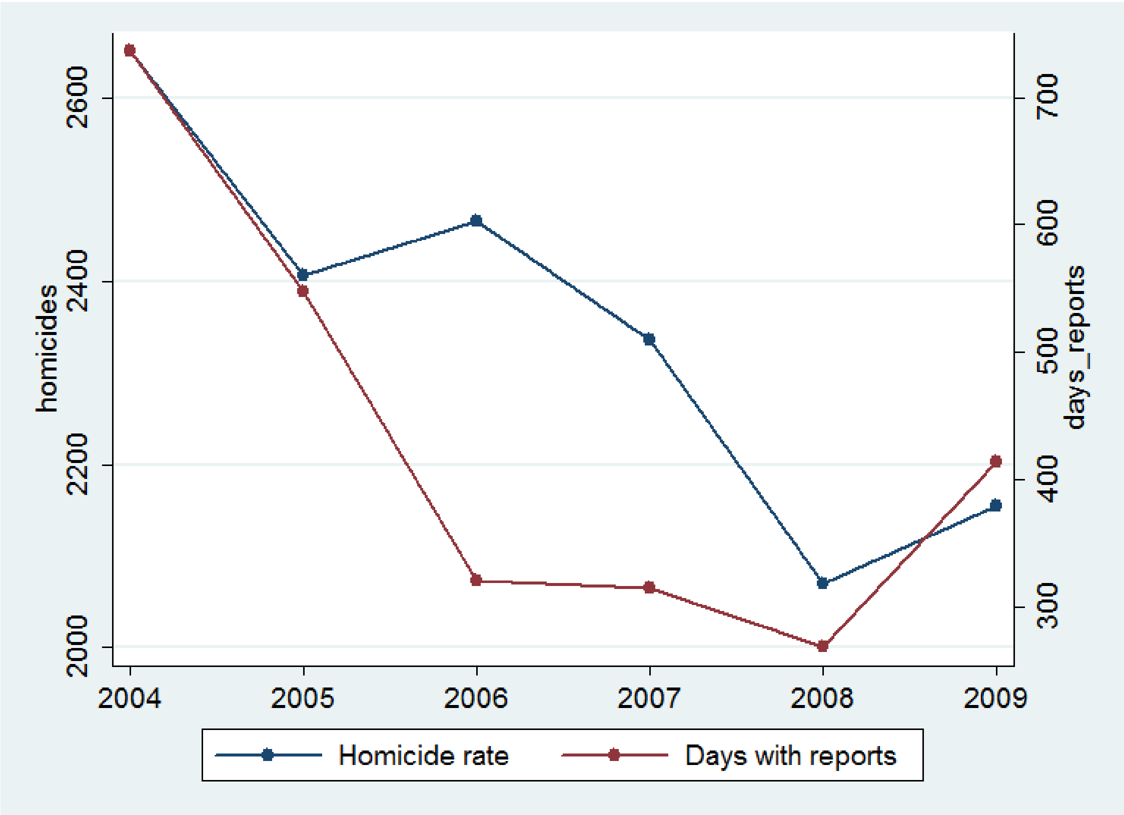
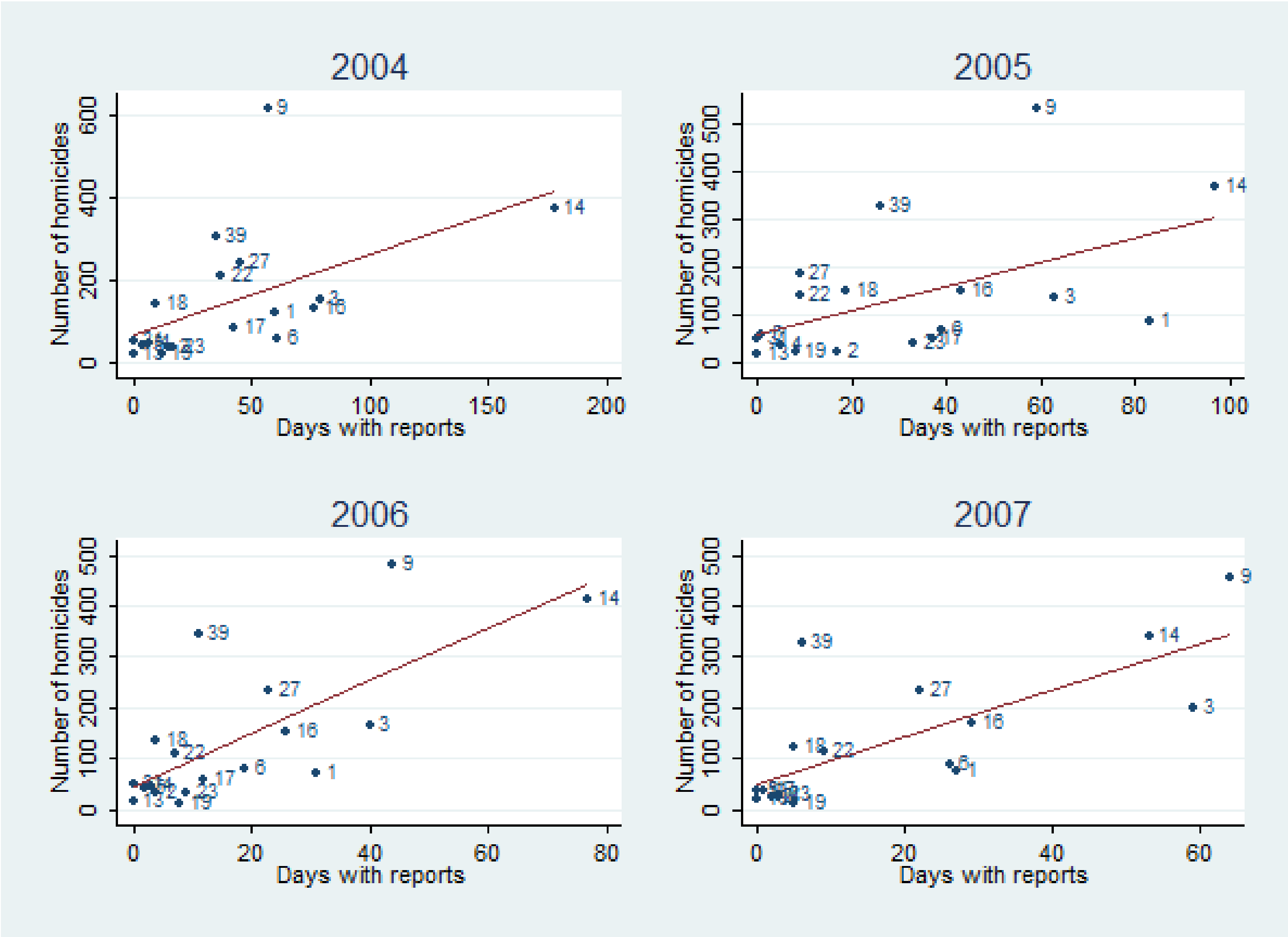


Figure 3.4: Homicides and Number of Days with Conflicts 2004-2009



Notes: This figure compares the number of homicides and the levels of violence documented in Disque-Denúncia reports between 2004 and 2009. The left y-axis indicates the number of homicides in the city of Rio de Janeiro. The right y-axis indicates the sum of the number of days with reports about gunfight in all Rio de Janeiro's slums.

Figure 3.5: Homicides and Number of Days with Conflicts per AISP



Notes: This figure shows the correlation between the number of homicides in the city of Rio de Janeiro and the number of days with conflicts in Rio de Janeiro's slums. Both measures are aggregated per AISP (the city division used by the police department). Each panel indicates a different year.

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A Royalty Rule

Oil producers in Brazil must pay 10 percent of the production value as royalties to different government bodies. The rule to distribute oil royalties is determined by two main pieces of legislation and depends on whether the oil is produced onshore or offshore.

5 percent parcel

Law 7.990/89 and Decree 01/91 determine the distribution of the first 5 percent of royalty payments. For onshore production, royalty distribution is straightforward: municipalities where the well is located receive 20% of royalty payments.

The distribution of royalties from offshore production follows a more complex rule. Municipalities affected by oil output receive 30 percent of total royalty payments from offshore wells. The production of the whole state is added up and divided among municipalities which are classified into three categories: (A) main production zone, (B) secondary production zone and (C) neighboring municipalities.

The main production zone comprehends municipalities which are in front of oil wells or which have in their territory three or more oil plants. The criteria to determine which municipality is 'facing' each oil well are based on parallel and orthogonal lines extracted from nautical letters. Main producing zone municipalities receive together 60% of royalty payments due to municipalities. The distribution of royalty payments within this group follows a population size rule. The National Bureau of Statistics (IBGE) is responsible to disclose municipality population every year, which is used to define the participation coefficient for each population range. This participation coefficient aims to attribute greater shares for larger municipalities but do not follow a linear rule. The law also guarantees that municipalities which concentrate production facilities should receive at least one third of the share distributed to municipalities in the main production zone. Hence, the share that each municipality in the main zone receive depends on its location, population and oil producing plants and the ones from its neighbors.

The secondary production zone receives 20% of royalty payments due to municipalities and is composed by municipalities which are crossed by pipelines. The neighboring municipalities receives the remaining 10% of municipal share. A municipality is classified in this group if it borders the main producing zone or if it is from the same mesoregion of main production zone municipalities. The mesoregion is a geographic classification established by IBGE and is not related to royalty payments or oil output. The distribution within these zones also takes into account the population size rule.

Therefore, the share of royalties that municipality i receives from offshore production is :

$$royalties_i = \tag{A-1}$$

$$municshare_{Ais} * 0.6 * 0.3 * 0.05 * OutputState \; if \; i \in A = MainProductionZone$$

$$municshare_{Bis} * 0.2 * 0.3 * 0.05 * OutputState \; if \; i \in B = SecondProductionZone$$

$$municshare_{Cis} * 0.1 * 0.3 * 0.05 * OutputState \; if \; i \in C = NeighbMunicipalities$$

where $municshare_{jis}, \; j \in \{A, B, C\}$ is the municipal share of municipality i from state s. This share depends on municipality population and the number and population of other municipalities in the same group at the same state such that $\sum_i municshare_{jis} = 1$ for each state.

The royalty rule also guarantees 10% of royalty payments to municipalities which have facilities to support transportation to and from oil sites. This share is equally distributed among all the municipalities in Brazil who have this kind of facility, but it considers in different groups municipalities with facilities which support onshore fields and the ones that support offshore fields.

Second 5 percent parcel

The Oil Law (9.478/97) enacted in 1997 and regulated by Decree 2.705/98 increased royalty payments from 5% to up to 10% but determined different criteria to distribute the second parcel of royalty payments.[1]

In relation to onshore royalties, few changes were introduced. Municipalities where the oil field is located receives 15% of its royalty payments (0.15 x 0.05 x OutputField).[2]

In turn, the rule to distribute royalties from offshore fields was dramatically simplified. 22 percent of the second parcel of royalty payments from offshore production is paid to municipalities located in front of the field. The criteria to determine which municipality is 'facing' each field are also based on the same parallel and orthogonal lines to the Brazilian coast. A combination of both lines creates the 'facing quotas', which are the percentage of each oil field located in front a each municipality. Hence, the amount that each coastal

[1]The size of the second parcel varies with exploration risk involved in the oil field under contract and range from 1 to 5 percent.

[2]The change of nomenclature from well to field is not accidentally. Law 9.478/97 use the field as a reference rather than the well

municipality receives from offshore production is equal to (FacingQuota x 0.22 x 0.05 x OutputField).

Finally, the second parcel of royalty rule also distributes 7.5% of royalty payments to municipalities which have facilities to support transportation to and from oil sites. But in this case, the distribution within this group considers the amount of oil transported by each facility.

B Oil Data

B.1 Oil output

The Brazilian Oil National Agency (ANP) is the main source of information on oil sector in Brazil. Since August 1998, it discloses monthly data on oil and gas production and prices by oil field. This information allows us to calculate oil output from 8/1998 to 12/2009 for each oil field by using the formula Output = OilPrice x OilProduction + GasPrice x GasProduction.

Data from the 1991 to 1997 were gathered at the December editions of Oil and Gas Journal. From 1991 to 1997, the magazine reported the average number of barrels of oil produced daily by each oil field. We measure the annual production by multiplying the average daily production by 365. However, this Journal does not provide information on prices, which are necessary to calculate production value. We rely on ANP (2001a) to calculate implicit prices by using the information on total royalty payments and total production. The price per barrel was obtained by using the formula: price=royalties /(0.05 x OutputBarrels). We did not compute prices from 1991 to 1993 since this was a high inflation period, what dramatically challenge the calculation of monetary values. We are confident about using this average price per year for the whole country because oil price was controlled by the state and did not fluctuate with exchange rate and international price before Oil Law was enacted in 1997. A final calculation was necessary to obtain 1998 annual production values since Oil and Gas Journal did not disclose information per oil field for that year. We rely on ANP information from August to December (the first ones available) to calculate 1998 production value as 12/5 x (OutputAugDec).

The next step was to associate oil fields with municipalities in order to obtain production values per municipality. We localized the onshore fields by using GIS information provided by ANP's Exploration and Production Database (Banco de Dados de Exploracão e Producão - BDEP). An onshore oil field is assigned to one municipality if its boundaries falls within a municipality border. In the case of oil fields whose boundaries cover more than one municipality, we distribute the production by considering the percentage of the area of the oil field located on the municipality. In the case of offshore production, we assigned oil fields to each municipality by using the list of facing quotas disclosed by ANP.The facing quotas are monthly disclosed by ANP at `http://www.anp.gov.br/?pg=14431` under the name 'Confrontação Month Year.pdf'.

We should note that we were not able to find the location of all oil fields listed on Oil and Gas Journal on DBEP or ANP database. The fields we didn't localize are responsible for less than 1 percent of total production in a given year and could not have their production assigned to a specific municipality only to the state.[1]

In order to double check our calculation, we added municipal oil output by state and year and compared these number to the ones disclosed at ANP (2001a). The series from 1994 to 1997 constructed based on data provided by Oil and Gas Journal are almost the same to the one informed by ANP at state level (correlation 0.9997), which support the quality of the data provided by the Journal. For the period from 1998 to 2008, our series also match almost perfect to the one disclosed by ANP (2001a).

B.2 Royalty payments

Data on royalty payments made to each municipality are disclosed monthly by ANP from 1999 to 2008 at `http://www.anp.gov.br/?pg=9080`. Data from 1994 to 1998 were calculated by us by following in detail the rule described in ANP (2001b) and relying on the information on production value per municipality (calculated as described above using data from Oil and Gas Journal).

Note that from 1994 to 1997, only the first 5% parcel of royalties was paid. The second parcel of royalties began to be paid on October 1998.[2] Hence, the main task to compute royalty payments for this period is to replicate the first parcel rule. We describe that first.

The computation of onshore oil royalties is the easiest part. By using GIS database provided by BDEP, we could match municipal borders with oil field borders and attribute to each municipality 0.2 x 0.05 x ShareFieldMunicipality

[1]The production of all non-localized fields represents 0.17 percent of total production in 1994, 0.83% in 1995, 0.67% in 1996, 0.15% in 1997. In most of the cases, they are small oil fields which should have been phased-out due to low production. The largest producing fields not identified are fields which are by the time in their early phases of production and therefore hadn't had a name but rather a code. We weren't able to match these codes with the new names.

[2]Although Oil Law was enacted in June 1997, decree 2.705/98 which detailed the rules for paying the second parcel was just enacted in August 1998. The second parcel of royalty payments was paid for the first time in October because royalties are due two months after production. This information was provided by ANP technicians.

x OutputField.[3].

For offshore oil royalties, the task is more cumbersome. In order to calculate royalties from 1994 to 1998, we need not only the information on producing municipalities but also the list of municipalities which have three or more oil plants (classified as being part of main producing zone), the ones crossed by pipelines (secondary zone), the neighboring municipalities and the ones from the same mesoregion to a municipality in the main producing zone.

Since no list was found for the 1990s, we rely on ANP (2001b) which provide information for 2000 and assume that the same municipalities were affected by oil output in the 1990s. According to ANP (2001b), eight municipalities are classified in the primary zone in 2000 because they have three or more producing plants. They are: São Sebastião do Passé (BA), Paracuru (CE), São Mateus (ES),Macaé (RJ), Guamaré (RN), Itajaí (SC), Aracaju (SE) e Cubatão (SP). We compose the list of main producing zone municipalities by listing these municipalities and the the ones facing oil fields under production during the 1990s, which are determined in accordance to 'facing quotas' list[4] Royalty payments to each municipality within this group were calculated using equation A-1, taking into account that Macaé (RJ) and Cubatão (SP) concentrated oil facilities and deserves at least 33 percent of royalty payments to main producing zone in their respective states.

ANP (2001b) also reports that there were ten municipalities in 2000 crossed by pipelines which compose the secondary zone: Fortaleza (CE), Cachoeiras de Macacu (RJ), Duque de Caxias (RJ), Guapimirim (RJ), Mage (RJ), Rio de Janeiro (RJ), Silva Jardim (RJ), Praia Grande (SP), São Paulo (SP), São Vicente (SP). The distribution of royalties to these municipalities also follows the population size rule[5] and equation (A-1).

The list of neighboring municipalities is determined by using mesoregion codes provided by IBGE. Based on this list, we distribute royalty payments within this group taking into account the population size rule and equation (A-1). Note that municipalities can receive royalties for more than one reason. For instance, a municipality can receive royalties because it has transportation

[3]This calculation requires a simplification because the law determines the payment according to oil well rather than the field. For fields entirely within one municipality border, that is not a problem. For fields which extend from more than one municipality, one may think the use of ShareFieldMunicipality as assessing the probability that the well is located within the municipal border.

[4]Note again that the law states that distribution should follow well location rather than the field, which is the unit of analysis in our dataset. We don't believe, however, that this is a major limitation since we can think about the use of these 'facing quotas' as assessing the probability that the well is located in front a specific municipality, which is equal to the share of that field in front of the municipality.

[5]The population size rule can be found at ANP (2001b).

facilities and because it is a neighboring municipality. Hence, we calculate all these quotas independently for each municipality and each year and then add them up.

Finally, we need to determine the list of municipalities with facilities which support transportation from and to oil sites. This again was extracted from ANP (2001b). In 2002, 57 municipalities had facilities which support onshore production and each of them receive (1/57)*0.1*0.05.ProductionValueOnshoreBrazil. In turn, 15 municipalities have transportation facilities to and from offshore site and each receive (1/15)*0.1*0.05.ProductionValueOffshoreBrazil (see ANP (2001b) for the list of municipalities).

After concluding the computation of the first parcel of royalties, we still need to input the second parcel of royalty payments for 1998. Onshore producing municipalities received additional 0.15 x 0.05 x ShareFieldMunicipality x 3/12 x ProductionValueField1998, while offshore producing municipalities received 0.22 x 0.05 x ShareFieldMunicipality x 3/12 x ProductionValueField1998, where 3/12 stands for three months in that year. We were not able to compute royalties relative to the second parcel for municipalities with transportation facilities. We didn't find information on the volume handled by each facility, which would be necessary to distribute royalties. We don't believe this is a major problem because we are losing just three months of payments.

B.3 Other data

Other variables used in this paper were gathered from different sources as following described.

Electoral information. We use Tribunal Superior Eleitoral (TSE) microdata for 1996, 2000, 2004 and 2008 local elections that is provided by TSE under request. TSE also sent us a list of candidates and parties elected in 1992, which allows us to construct 1996 party reelection variable.

Municipal finance. Data on public finance, including revenues and expenses, are available from Brazil's National Treasury through 'Finanças do Brasil' (FINBRA) database from 1997 to 2008 at `http://www.tesouro.fazenda.gov.br`. Some municipalities do not declare FINBRA every year and sometimes do not provide all the information requested. We use only data from municipalities which report most of revenues and expenses but we do not perform any correction for the years that municipalities did not declare. Hence, our analysis of municipal finance is based on an unbalanced panel.

Public employees. Data on the number of municipal public employees, their composition and wages were gathered from Registro Anual de Informações Sociais (RAIS), a database that comprises all formal workers in

Brazil. The Brazilian Ministry of Labor (MTE) collects that information and disclose it in Cd-Roms, which are available upon request.

Economic activity. RAIS provides information on private employees, total payroll and number of firms per sector. Municipal GDP is available from IBGE for 1999-2007 period at `http://www.ibge.gov.br/home/estatistica/economia/pibmunicipios/2006/default.shtm`.

Educational data. Educational outcomes are provided by Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira (INEP) at `http://www.inep.gov.br` from 1996 to 2006.

Health supply. The number of municipal health clinics and hospitals are available at DATASUS's site (See `http://www.datasus.gov.br`). Cadastros Extintos do SUS discloses information for 1998-2002 period, while Cadastro Nacional de Estabelecimentos de Saude (CNES) publish data for 2006-2008. We named health clinics the sum of 'unidades basicas de saude' and 'postos de saude'. Hospital units include 'Ambulatório de Unidade Hospitalar Geral' and 'Ambulatório de Unidade Hospitalar Especializada' in CNES database and 'Hospital Dia', 'Hospital Geral' and 'Hospital Especializado' in Cadastros Extintos do SUS database. We considered only health units managed by the local government.

Geographic characteristics. We gathered data on municipalities' geographic characteristics such as latitude, longitude, altitude and distance to the state capital at IPEADATA site (`http://www.ipeadata.gov.br`). IPEA also provides 1991 and 2000 population census variables such as population density, percentage of urban households and average years of schooling.

Population estimates. Inter-census population estimates are available at `http://www2.datasus.gov.br/DATASUS/index.php?area=0206`.

C Model Appendix

C.1 Posterior variance

$$Var(\tilde{a}) = \tilde{\sigma} = Var(\frac{g + r^e}{T}) = \frac{Var(g)}{T^2} = \frac{Var(a(T+\theta))}{T^2}$$

$$= \frac{Var(a)E(T+\theta)^2 + E(a)^2 Var(T+\theta) + Var(a)Var(T+\theta)}{T^2} = \sigma + \frac{\mu^2 v + \sigma v}{T^2}$$

C.2 Partial effects on rents:

Let

$$\frac{\partial p_I}{\partial r} = -\frac{\epsilon\sigma}{(\tilde{\sigma}+\sigma)T} = -\frac{\epsilon\sigma T}{\sigma(2T^2+v)+\mu^2 v} = -F$$

$$Signal\{\frac{\partial r}{\partial \rho}\} = -Signal\{\frac{\partial F}{\partial \rho}\} \ where \ \rho = \{\epsilon, v, \sigma, T\}$$

$$\frac{\partial F}{\partial \sigma} = \frac{\epsilon T\left[\sigma v + \mu^2 v\right]}{\left[\sigma v + \mu^2 v + 2\sigma T^2\right]^2} > 0$$

$$\frac{\partial F}{\partial T} = \frac{\epsilon\sigma\left[\sigma v + \mu^2 v - 2\sigma T^2\right]}{\left[\sigma v + \mu^2 v + 2\sigma T^2\right]^2} = \frac{-\epsilon\sigma\left[3\sigma - \tilde{\sigma}\right]}{\tilde{\sigma}+\sigma} < 0 \ \Leftrightarrow \ \tilde{\sigma} < 3\sigma$$

C.3 Partial effects on reelection probability

$$p_I^* = \frac{1}{2} + \frac{\epsilon\sigma}{\tilde{\sigma}+\sigma}\left[\frac{(a+\theta)T}{T} - \mu\right] = \frac{1}{2} + \frac{\epsilon\sigma T\left[(a-\mu)T + a\theta\right]}{\sigma(2T^2+v)+\mu^2 v}$$

$$\frac{\partial p_I^*}{\partial \sigma} = \frac{\epsilon T\left[(a-\mu)T + a\theta\right]\left[\sigma v + \mu^2 v\right]}{\left[\sigma(2T^2+v)+\mu^2 v\right]^2}$$

$$\frac{\partial p_I^*}{\partial \sigma} > 0 \Leftrightarrow \ a > \frac{\mu T}{T+\theta}$$

$$\frac{\partial p_I^*}{\partial T} = \frac{-\epsilon\sigma\frac{\partial\tilde{\sigma}}{\partial T}}{(\tilde{\sigma}+\sigma)^2}\left[\frac{a(T+\theta)}{T} - \mu\right] + \frac{\epsilon\sigma}{\tilde{\sigma}+\sigma}\left[\frac{aT - a(T+\theta)}{T^2}\right]$$

$$= \frac{-\epsilon\sigma}{\left[(\tilde{\sigma}+\sigma)^2 T^2\right]^2}\left[2T(\mu^2 v + \sigma v)(a-\mu) + a\theta(\tilde{\sigma} - 3\sigma)\right]$$

$$= \frac{-\epsilon\sigma}{\left[(\tilde{\sigma}+\sigma)^2T^2\right]^2}\left[2T(\mu^2 v+\sigma v)(a-\mu)+a\theta(\mu^2 v+\sigma v-2\sigma T^2)\right] > 0 \;\Leftrightarrow$$

$$2T(\mu^2 v+\sigma v)(a-\mu)+a\theta(\mu^2 v+\sigma v-2\sigma T^2) > 0$$

$$\Leftrightarrow\; \sigma < \frac{2T\mu^2 v\left[(a-\mu)+a\theta\right]}{a\theta(2T^2-v)-2Tv(a-\mu)}$$

D Coding Disque-Denuncia Reports

This appendix explains how we used Disque-Denúncia reports to construct violence indicators. We gathered from Disque-Denúncia (DD) all reports classified as 'gun fight between drug-gangs' (tiroteio entre facções) registered between 2004 and 2009 in the city of Rio de Janeiro. The content of each report varies a lot but in all cases it contains the date of the call, a location reference and a description of the event. Most of the reports are simple as the one below:

Inform that drug dealers from the referred slum are currently in a battle with rival drug dealers. The gunfight is intense and people are worried. Demand police intervention. Address provided: Morro da Mangueira.[1]

Other reports are incredibly rich, provide important information for the police (eg.the location of a drug dealer) and show how violent these events are:

Report that today (10/26/2005), at 7:00AM, there was a gunfight in front of the school Vicente Mariano between drug leaders from Timbau slum and Vila do Pinheiro slum. A man was killed and five children were shot. ... The traffic leader had intentionally shot in the school direction. This guy, whose nickname is Night, is currently located at rua Capivari, 55. Address provided: Maré slum.[2]

The two examples above also show that although DD always asks for the full address (street name, number and zip code), people do not always

[1]Original report: 'Relata que traficantes do morro citado se encontram nesse momento trocando tiros com traficantes rivais. Informa que a troca de tiros é intensa e os moradores estão preocupados. Sem mais, pede policiamento para o local."

[2]Original report: 'Informa que hoje (26/10/2005), as 07h, ocorreu um tiroteio na favela da Maré, em frente ao Brizolão Colégio Vicente Mariano, confronte entre o tráfico do morro do Timbau e Vila dos Pinheiros onde causou a mote de um adulto e o ferimento de cinco crianças (não identificados), estudantes do colégio supra citado, que encontram-se no hospital geral de Bonsucesso em estado grae. Relata que o chefe do tráfico do morro do Timbau, identificado como Night, foi o responsável pelos disparos, pois direcionou sua arma para o colégio atirando impiedosamente, provocando este acidente. Declara que Night pode ser encontrado neste exato momento, em uma casa, no alto do morro, na rua capivari, próximo ao numero 55, no local onde existe uma placa informando tratar-se do beco da escolinha. Sem mais, pede providências."

provide it in detail. In both cases, just the name of the slum was provided. The exact location of the second event was even harder to identify since the person mentioned Maré, which is the name of a slum complex. In order to deal with these issues, we relied on a combination of addresses provided, the name of the slum (when it was mentioned) and the content of each report to identify where the described event took place. Based on that information, we associated each report to a city slum by using the slum shape file provided by Instituto Pereira Passos (IPP). In some cases, this association was not straightforward due to three reasons. First, many times the name of a slum was not mentioned in any part of the report. In this case, we opened the slum shape file on Google Earth and added the address or other information provided in the report (for instance, in the second example, we added the address of school Vicente Mariano). In case the address was within a slum or close to its border, the report was associated with the respective slum. The addresses far away from a slum were classified as 'paved area' (asfalto) and were excluded from our sample. Another challenge is the fact that people use different names to refer to the same slum and the slum name used by IPP does not always match the one most used by the population. For instance, the slum popularly called Parada de Lucas or just Lucas is registered in IPP as 'Parque Jardim Beira Mar'. Fortunately, IPP also provides a list with alternative names for the same slum, which allows us to match the names used by population with the ones in IPP's shape file.[3] Finally, some reports mentioned that a gunfight occurred in places that are not officially slums but rather housing projects or irregular settlements, which are not marked in IPP's slum shape file. For instance, several reports mentioned a conflict in Conjunto Guaporé, Cidade Alta or Conjunto Fumacê, which are housing projects. To keep from losing that information, we used Google Earth and the addresses provided in the reports to draw borders for these areas and incorporated them in the slum shape file.[4]

In addition to standardizing the address, we read the content of each report to guarantee that each one indeed describes a gunfight that took place on the date and at the address registered. Hence, we marked the reports that mention the threat of a gunfight or the location of bodies and drug dealers but

[3] In the cases that the IPP list didn't have the slum name provided in the DD report, we used the address provided and Google Earth to make the match.

[4] We added 14 borders in IPP's slum shapefile which represents the following housing projects or irregular settlements (neighborhood indicated in parenthesis): Vila do Pinheiro (Maré), Vila do João (Maré), Conjunto Guaporé (Brás de Pina), Conjunto Alvorada (Santa Cruz), Conjunto Cezarão (Santa Cruz), Favela do Rola (Santa Cruz), Guandu II (Santa Cruz), Morro das Pedrinhas (Santa Cruz), Cidade Alta (Cordovil), Vila Alice (Laranjeiras), Cruzada São Sebastião (Leblon), Conjunto Mangariba (Paciência), Conjunto Cavalo de Aço (Senador Camará) e Conjunto Fumacê (Realengo)".

did not mention that a gunfight occurred at that place and date. We exclude these reports from our sample. In addition, some reports provide an address, but the content refers to a conflict that happened in another place. In this case, we corrected the address to guarantee that it informs where the event happened. For instance, the report below was registered as Baixa do Sapateiro, but the content led us to change it to 'Avenida Canal', which is the official name of Vila do Pinnheiro slum, and where the conflict took place according to the report.

Inform that drug dealers from the slum mentioned, which are part of Terceiro Comando gang, invaded Pinheiro slum, which is dominated by ADA. Both slums are located in Maré... Address: Baixa do Sapateiro.[5]

A similar adjustment was necessary for the dates. Sometimes people call and report that a gunfight occurred three days before and DD registers the call date. We corrected the dates to guarantee that they refer to when the event took place.

This procedure generated a slum list containing the dates on which a gunfight took place. We then aggregated the data per slum and year by counting the number of days that at least one report of armed conflict was registered in Disque-Denúncia. Table 3.2 provides the descriptive statistics of Disque-Denúncia reports.

Bellow, we give more examples of original reports and how we classified them in order to clarify our methodology.

Informs that this avenue is one of the access points to Morro do Cajueiro, which will be invaded today at night by people from Morro da Serrinha. These people want to revenge the death of three colleagues that were killed by the rival gang. The attempt to invade the slum has been planned since these guys began to steal cars in the neighborhood. Address: Avenida Ministro Edgard Romero. Date: 10/22/2004.

[5]Original report: 'Informa que traficantes (não identificados) da favela em questão, que pertencem a facção criminosa Terceiro Comando, invadiram a favela do Pinheiro, que pertence a facção ADA, ambas situadas no complexo da Maré, Afirma que a invasão ocorreu sábado a tarde, por volta as 18hs, com intuito dos traficantes assumirem os pontos de boca de fumo da favela rival. Menciona que a invasão aconteceu devido a retirada das viaturas que ficavam frequentemente na entrada da favela do Pinheiro, que tem acesso pela linha amarela. Segundo informações, traficantes da favela em questão, teriam pago aos policiais (no identificados) lotados no 22 BPM, para se retirarem do local para assim eles poderem invadir a favela rival com mais facilidade. Disse que ontem (09/11) todos os estabelecimentos da favela acima estavam com as portas fechadas com a ordem passada pelo tráfico, pois provalmente algum indivíduo teria sido morto pela guerra das facções. Pode que o policiamento retorne ao local."

Morro do Cajueiro is an alternative name for Morro do Sossego, which is the name in IPP's shape file. This report was not included in our sample because it mentions only the threat of a conflict.

Reports that in the mentioned road, close to the school Chiquinha Gonzaga, several drug dealers were seen yesterday around 10 pm with the possession of heavy guns and motorbikes. There was an intense gun fight and a car was severely shot. The gun fight took one hour and the group escaped to Vila Aliança, close to Beira Rio store (......) Demands police intervention in the region. Address: Estrada do Engenho, Bangu. Date: 10/31/2006.

We changed the date of this report to the day before (10/30/2006), when the conflict actually happened, but we ended up not using this report because it was not close to a slum.

Reports that in this street, which is the entrance to Favela Boogie Woogie, is the location of school Olga Benário, where it is possible to find several drug dealers from Terceiro Comando. One of them is known as 'Grilo' and he is the son of a school employee. Drugs are sold inside the schools during class breaks. Yesterday, at 4:30 pm, drug dealers from Comando Vermelho tried to invade the school. There was an intense gun fight. Address: Rua Dante Santoro, Cacuia, Ilha do Governador. Date: 8/22/2003. This report mentions the proximity to favela Boogie Woogie, whose official name is Bairro Nossa Senhora das Graças. Therefore, we associated this report to this last slum name. In addition, we changed the day of the report to the previous day (8/21/2003), when the event took place.

Report that in the mentioned street is the location of Guaporé housing project. A gun fight is taking place right now between drug dealers from rival gangs. A senior lady and a young boy were wounded. Address: Rua Carbonita, Brás de Pina. Date: 8/14/2004.

We drew the border of Guaporé housing project using Google Earth and added it to IPP's shape file in order to incorporate this and other reports in our analysis.

E Triggers of Drug Battles

This appendix provides more transcripts gathered from Plantão de Polícia and Casos de Polícia blogs. Our aim is to provide evidence that drug battles follow a unique dynamic that depends on betrayals, revanchism, the imprisonment or release of a gang leader and others.

Six bodies were found in Morro do Juramento. These people were killed in an 11 hour conflict that took place last Tuesday. CV drug dealers tried to reconquer the area, which is dominated by Terceiro Comando Puro (TCP). Last month TCP overthrew the area from ADA. (Source: Meia Hora, 9/20/2009)

...in July, Marcus Viniï¿½cius Martins Vidinhas Júnior, known as Palhaço, betrayed his father-in-law, Celsinho da Vila Vintém, who is in jail but is still the slum drug baron. Palhaço killed 13 drug gang members in order to control drug trade slots. Two days later, Celsinho allies deposed Palhaço, who ran away with guns and R$ 1 million. (Source: Meia Hora, 9/22/2009)

An intense gunfight took place yesterday night at Morro do Dendê. Chorrão (ADA) and Pixote attempted to conquer the slum, which is dominated by Fernandinho Guarabu (TCP). Pixote is a former member of Guarabu gang. (Source: Meia Hora, 10/11/2009)

In addition, several reports to Disque-Denúncia also provide examples on what trigger conflicts:

Informs that at the given address it is possible to find fugitives and drug dealers, who yesterday were involved in a gun conflict. Today, the mother of one of the boys was shot to death in the Estrada Porto Nacional. This group is part of Pipa's gang, who was recently murdered in jail. Pipa's death explains the attempt against his supporters. It concludes by mentioning that the school Piquet Carneiro received an order to close. Date: 3/26/2004 2:19 PM

Reports that the slum mentioned and Morro do Timbau, which are *controlled by Facão, were invaded today by more than 80 drug dealers. Some of them are known as 'Noquinha', 'Sassá', 'Alex Churrasquinho', 'Nelsinho', 'Daniel do Lava Jato', 'Ilton', (...). There are others from Morro do São Carlos. They are from ADA gang, are heavily armed, are led by Gan Gan and aim to kill Desviado, the leader of Baixa do Sapateiro, and the drug trade manager Tico. The gun fight began at noon and these drug dealers are still around the slum, shooting without a specific target and leaving slum inhabitants in panic. Date: 1/11/2004 5:20 PM*

Inform that in Parque Alegria slum a gun fight is taking place right now among drug dealers. Yesterday, during the day, the drug dealers Nêgo Dengo and Araketu killed a person and this is the reason for the current gun conflict. Drug dealers connected with the person who died invaded the slum to take revenge. Demands intervention because several people are being shot by stray bullets. Date: 12/12/2006 3:37 PM.