Corruption, Transparency, and Natural Resources

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Abstract

This paper studies how a formal institutional reform of the royalty resource allocation system can affect local politicians' and rent-seekers' corrupt behavior. Using a *difference-in-differences* strategy exploiting the timing of a 2011 reform to the royalty allocation system in Colombia and the cross-sectional variation of royalty allocation prior to the reform, we find that before the reform, the *producer* municipalities were more corrupt but reduced their corrupt behavior when the reform was implemented. We rule out that the results are explained by the *producers'* loss of resources and a problem with the control agencies' capacity and incentives to investigate the *net-losers*. We suggest that our main mechanism is transparency in the step-by-step process that local politicians must go through to give a contract to a rent-seeker. We find that, before the reform, illegal armed groups stole royalties through state capture and collaboration with politicians, but we found an indirect way to control it; the transparency component of the reform differentially reduced corruption in *producer* municipalities with the presence of illegal armed groups. Our estimates indicate that formal institutional reform that ensures transparent resource allocation through meritocratic processes can help reduce corruption and state capture by politicians and rent-seekers.

Keywords: corruption, transparency, natural resources, institutions, rent-seeking, political accountability.

JEL codes: D02, D72, D73, D74, P48.

1   Introduction

Assume that a country has a fixed number of administrative divisions — municipalities, for example — and that a percentage of the national budget is transferred to municipalities as royalties. There are two groups of municipalities: *producers* that receive royalties as a counter-benefit to exploiting natural resources in their territory; and a group of *non-producer* municipalities that do not receive royalties. Now, suppose that the national government reforms the resource allocation system because local politicians in the municipalities that receive royalties are corrupt and misspend the funds. The institutional reform gives access to both groups of municipalities, reduces discretion, and ensures transparent resource allocation through meritocratic processes. Does this reform reduce corruption?

The answer is not as trivial as it may seem. As in all redistribution, there are *net-losers* and *net-winners. Producer* municipalities lose because they have to share their resources, and *non-producer* municipalities win because the reform gives them access to royalty resources. Theoretically, the *net-losers* should reduce corruption because they have fewer resources that are less discretionary and have to face more-transparent meritocratic allocation processes. Although we should not expect the new resources to generate corrupt incentives for politicians in the *net-winners* municipalities because they face the same system of *producers*, ambiguous windfall effects have been found in the natural resource curse literature. The political process and the interaction of interest groups can lead to adverse effects such as rent-seeking (Tornell and Lane, 1999; Velasco, 1997) or even civil wars (Ross, 2006; Besley and Persson, 2008; Caselli and Coleman, 2013; Dube and Vargas, 2013). A windfall also can exacerbate the problem of political agency and deteriorate politicians' quality by, for example, attracting corrupt candidates (Brollo et al., 2013; Vogel, 2020). The literature has also found that a windfall can affect development and growth, even at local levels, by improving living standards through public goods provision (Caselli and Michaels, 2013; Gallego et al., 2020).

Using the reform of the royalty system implemented by the Colombian government in 2011, this paper studies how such a formal institutional reform can affect local politicians' and rent-seekers' corrupt behavior. The institutional arrangement reduces the discretionary use of resources (with mechanisms of transparency and accountability), the competitive allocation (based on merit), and the equitable access to royalties (all municipalities can access royalties). Using a novel database at the municipal and term-of-office level with information from the Attorney General's Office (PGN) news bulletins, we estimate the impact of the reform on the likelihood of local politicians being prosecuted, found guilty, or removed from office.

Using a *difference-in-differences* strategy, we exploit the reform's timing and the cross-sectional variation of royalty allocation in Colombia. We construct a measure of royalty exposure before the first year of our study period to deal with endogeneity concerns, to identify *producer* and

non-producer municipalities. Specifically, we use the average royalties received by a municipality between 1996 and 1999.

First, using a pre-reform version of the main strategy, we show how exposure to large discretionary resource budgets incentivizes corrupt behavior. Before the reform, local politicians in *producer* municipalities were 21% more likely than politicians in *non-producer* municipalities to have open prosecutions. Also, the probability of a mayor or senior government official being found guilty or removed from office was 30% and 45% higher, respectively. The results are robust to the inclusion of controls and fixed effects.

Once the central government implemented the reform, corrupt behavior changed, and opportunities to extract rents decreased. We show that after the reform, there was a differential drop in local politicians' corrupt behavior in the *producer* municipalities. The effects of the reform were economically larger: a one-standard-deviation increase in royalties received before the reform decreased the probability of a local politician being prosecuted after the reform by 17%, relative to local politicians' probability in *non-producer* municipalities. In the same sense, the likelihood of being found guilty and being removed from office fell by 33% and 46%, respectively. Given the high concentration of resources in a few municipalities before the reform, we show that our results are not driven by a specific small group of *producer* municipalities or municipalities belonging to the same department. Finally, to eliminate possible endogeneity concerns, we estimate a Two-Stage Least Squares (2SLS) and find that with a strong instrument, our main results are robust.

One direct potential mechanism could be the mechanical effect of the reform. *Net-losers* have fewer discretionary resources at their disposal. We show that the reform reduced the total royalties that *producer* municipalities have received in the years after the reform by an average of 30%. This suggests that our main findings on the drop in corrupt behavior should be explained, at least in part, by the loss of resources of the *producer* municipalities. But when we include the time-varying royalties in our main specification as a "bad control", our results do not change. Based on the loss of resources, it is natural to think that the control agencies would investigate the *net-losers* less because they have fewer resources. Also, under the assumption that the control agencies' capacity has remained constant after the reform, having to investigate all municipalities could limit their capacity. We rule this out as another possible mechanism because we find that, on average, there is no difference between the number of public audits that the control agencies performed on *producer* municipalities before and after the reform.

In addition, the reform contained two components that may generate heterogeneous differential effects: transparency and accountability. The step-by-step approach designed by the reform, which local politicians have to follow from project formulation to contract assignment, makes the processes more transparent and more visible to government agencies and citizens. This increases costs for politicians and rent-seekers. To determine who is differentially affected by transparency, we use a

measure of the presence of illegal armed groups before the reform that allows us to identify rent-seekers. We find that *producer* municipalities with the presence of illegal armed groups before the reform were more corrupt. With additional anecdotal evidence, we show how illegal armed groups have stolen royalties through extortion or collaboration with local politicians, state officials, and local social leaders. But, after the reform, we find that *producer* municipalities with the presence of illegal armed groups actually further reduce corruption—the transparency component is an indirect way to control rent-seekers' rent appropriation and regain state capture.

Finally, we use in our main specification an accountability indicator developed by PGN to look at the differential effect of the reform on strengthening accountability mechanisms and citizen participation in the monitoring, control, follow-up, and evaluation of projects financed with royalties. We did not find results that suggest a change in the strengthening of these mechanisms.

We make several contributions to both research and policy. The results found in this paper contribute to the literature on the natural resource curse in many ways. The presence of natural resources generates corrupt incentives for local politicians (Bhattacharyya and Hodler, 2010; Vicente, 2010; Van der Ploeg, 2011); attracts corrupt candidates seeking to extract rents (Brollo et al., 2013; Vogel, 2020); and creates conflict (Ross, 2004; Collier and Hoeffler, 2004; Fearon, 2005; Humphreys, 2005; Snyder and Bhavnani, 2005; Snyder, 2006; Ross, 2013; Dube and Vargas, 2013). Also, we show evidence of how natural resources attract armed groups that, in the quest for rent appropriation, capture local states and reduce governance levels through the weakening of institutions. To our knowledge, this is the first paper that empirically support the results found by Tornell and Lane (1999) at subnational level in a context of conflict.

In line with some authors' research, such as North et al. (1990), Snyder (2006) and Thorp et al. (2012), we empirically show how the corrupt behavior of rent-seekers can be changed through formal institutional arrangements for the allocation of resources that come from exploiting natural resources. Particularly, as a novel result, we find an indirect way — i.e., decreasing the availability of discretionary resources and greater transparency in the processes — to prevent armed groups from appropriating rents and capturing the state. We also contribute to the literature that studies the strengthening of accountability mechanisms to reduce corruption (Olken, 2007; Ferraz and Finan, 2011; Bobonis et al., 2016; Avis et al., 2018). Finally, we contribute to the extensive literature on the effectiveness of different anti-corruption policies (Olken, 2007; Dal Bó and Rossi, 2007; Ferraz and Finan, 2008; Olken and Pande, 2012; Ferraz and Finan, 2011; Bobonis et al., 2016; Avis et al., 2018).

The rest of the paper is organized as follows. Section 2 provides a Colombian context for the transfer system, corruption dynamics, and the reform. Section 3 presents a description of the data, and in Section 4, we develop the empirical strategy. Section 5 presents the main results and Section 6 the heterogeneous effects. Finally, Section 7 presents the conclusions.

2 Context

This section provides a detailed description of the functioning of transfers, the composition of subnational government revenues, and the dynamics of corruption in Colombia. We also provide a description of the functioning and problems of the old system of royalty resource allocation and, finally, a detailed description of the institutional reform's key changes.

Transfer system and corruption dynamics. Colombia is a decentralized country. Subnational governments are responsible for a large number of competencies, including education, health, public services, water, and basic sanitation, among others. Departments are responsible for co-financing some municipal expenditures, while municipalities are responsible for managing spending on items not covered by the departments and accounting for about 65% of subnational government expenditures. In total, subnational governments are responsible for one-third of non-financial public sector spending (which is about 9.6% of GDP), of which 50% goes to health and education (Ardanaz and Tolsa, 2015). On average, 70% of the municipalities' income comes from the Sistema General de Participaciones (General Participation System, SGP), 20% from royalties, and 10% from their own resources (tax and non-tax revenues).

For SGP transfers, before the reform, mayors had sector-specific spending guidelines,[1] while they had complete discretion over how they spent royalty payments and the municipalities' revenues. Local politicians decided the amount of royalties to spend and on which projects. That is, the local authorities had complete autonomy to decide on investments, and no one demanded verifiable results. The high degree of autonomy and discretion that mayors had over royalties opened opportunities for resources to be used for clientelistic politics rather than for performance-based policies.[2] Also, the concentration of many discretionary resources in a few municipalities and departments[3] attracted interest groups seeking appropriate revenues. The main ones were the illegal armed groups that have historically had a territorial presence in areas highly dependent on natural resources.[4]

Between 2009 and 2016 in Colombia, the corruption identified and sanctioned was mainly manifested through bribery, extortion, appropriation of public assets and assets for private use, and nepotism (Newman et al., 2017).[5] Most corruption cases were due to accepting or demanding

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[1]These guidelines did not change in the reform.

[2]A high degree of discretion in the use of resources from the exploitation of non-renewable natural resources, which, in the hands of the local people, translate into payments of political favors (CGR, 2017).

[3]Between 2002 and 2011, 70% of the resources were concentrated in seven departments, where only 14% of the country's population lived. An even more severe situation existed in Casanare and Meta, which, with only 2.5% of the population, received 34% of the royalties. For a more detailed discussion, see (Bonet et al., 2014).

[4]Rettberg and Prieto (2018) show how armed groups stole oil royalties through extortion or collaboration with politicians, state officials, and local social leaders.

[5]According to data from the Attorney General's Office (FGN), between 2009 and 2016, 3,966 cases were registered

money or other utility to either perform or omit an act appropriate to the position; executing an act contrary to their official duties; and retaining assets that were provided specifically for private purposes. Another reason for corruption cases was government officials giving favorable treatment, such as granting public positions, permits and licenses, to relatives or friends, without taking into account other merits.

Old system. In Colombia, only *producer* departments and municipalities received royalties[6] as consideration for exploiting natural resources in their territories. They received direct and indirect royalties. As their name indicates, direct royalties were transferred directly to the municipalities, which had complete discretion over their use, as long as they were projects framed in education, health and sanitation water areas. Indirect royalties required approval from the national government.[7] Royalties increased from 0.6% of GDP in 2002 to 1.66% in 2012, and 80% was in direct royalties and the rest in indirect royalties.

The availability of large amounts of discretionary royalties led to corrupt incentives[8]. On average, between 2001 and 2010, 66% of the reported irregularities in royalty projects were due to public procurement problems. Corruption and inefficiency in the use of resources and rent capture are some of the most common problems in the assignment of contracts and execution of projects financed with royalty resources.[9]

Institutional reform. Under the old resource allocation system, local politicians were more likely to be corrupt when a large fraction of resources were discretionary and lacked oversight and accountability. This is one reason that the Colombian government pushed for a reform of the system in 2011. The reform, the General Royalties System (SGR), made it possible to distribute the wealth from the exploitation of natural resources to all Colombian municipalities, both *producers* and *non-producers*, to make the system more equitable. Not only has the reform divided the pie into more pieces, but it also requires municipalities to compete for a piece of the pie. The implication is

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in the Oral Accusatory Penal System (SPOA), with at least one conviction for any of the crimes related to forms of corruption. For the same period, PGN registered in the Information System for the Registration of Sanctions and Causes of Ineligibility (SIRI) a total of 6,163 disciplinary sanctions, of which 60 percent were to governors or local officials, among which are mayors, councilors, secretaries, managers of public companies and ombudsmen. For a more detailed discussion, see Newman et al. (2017).

[6]Municipalities and departments where natural resources are exploited, and sea and river ports where resources are transported.

[7]Municipalities could use them under certain conditions involving the formulation of investment projects that were subsequently evaluated by a royalty advisory board that determined the feasibility of the project.

[8]According to Echeverry et al. (2011), the National Planning Department (DNP), through administrative and financial audits contracted by the National Royalties Commission between 2001 and 2004, identified 27,610 alleged irregularities in contracts, budgets, financial problems, documentation and royalty projects. The DNP, already under its functions, between 2005 and 2010, reported 21,681 irregularities to the different control agencies.

[9]Related results have been found by Benavides et al. (2000), Gaviria et al. (2002), Viloria-de-la Hoz (2002), Leal et al. (2004), Torres et al. (2005), Gamarra-Vergara (2005), Viloria (2005), Sánchez et al. (2005), Pearce (2005), Bonet (2007), Perry and Olivera (2009), Echeverry et al. (2011), Bonet et al. (2014) and Gallego et al. (2020).

that, now, the municipality's mere coincidence of geographic location and the exploitation of the resources are not enough. The reform requires them to compete for the resources and share them with all municipalities.

Consequently, corrupt local politicians and interest groups seeking appropriate natural resource rents now have fewer discretionary resources available. Furthermore, they have to go through meritocratic processes to gain access to royalties and face a set of changes to make resource allocation processes more transparent and to strengthen accountability mechanisms to execute projects financed with royalties. Figure 1 shows the flow of processes that a local politician must adhere to in the new resource allocation system. Box 1 of the diagram describes the step-by-step process for the approval and contracting of an investment project. From formulation to contracting, the processes make information more visible and ensure a meritocratic process, which we refer to as the transparency component of the reform. Box 2 describes the stages that projects must go through, which we call the reform's accountability component.

3 Data

In this section, we provide a detailed description of our data and their sources. We describe our measures of corruption analytically and offer some arguments that make our variable a good measure to capture wrongdoing and irregularities. We explain our measure of the presence of illegal armed groups, our measure of exposure to royalties, and the details of the accountability indicator we use to explore heterogeneous effects. Finally, we describe the sources of our control variables.

Corruption. We use the novel database constructed by Martinez (2019), based on PGN news bulletins, to measure corruption. With the database, it is possible to identify how many open disciplinary prosecutions local politicians have had, the process's status, and the process's outcome. Martinez (2019) argues that prosecutions are a good proxy for local politicians' misbehavior because, although he can observe only 63% of the prosecutions' outcomes, 95% of them end in sanctions and 53% with removal from office along with political disqualification for several years. A fact that supports the measure's quality is that 70% of the cases involve the municipalities' mayors, who make the most critical decisions on using royalty resources.

We construct three measures, at both the intensive and the extensive margin of municipality-term of office level. The data have measures for four periods of government between 2001 and 2015 — three periods before the reform and one period after the reform — for 1,075 municipalities. We create a measure that allows us to know the probability that a municipality's mayor or senior government official will have an open disciplinary prosecution during their term in office. Similarly,

we construct a measure for the probability that they will be found guilty and removed from office. We also build these measures at the extensive margin. Note that the latter two measures are conditional on the first; are the probabilities that they will be, found guilty or removed from office, given that they have an open disciplinary prosecution.

Figure 3 shows the proportion of municipalities in which at least one mayor or senior government official has had an open disciplinary prosecution process. We can differentiate the proportion of *producer* and *non-producer* municipalities before and after the reform in the figure. Before the reform, local politicians from *producer* municipalities were more likely to have open prosecutions (4% more than *non-producers*). Still, once the reform was implemented, the proportion of both municipalities' groups dropped, and they were almost equally likely. It seems that the windfall received by the *net-winners* did not incentivize corrupt behavior, and the mechanisms affected both groups. We observe the same pattern in the probability of being found guilty and of being removed from office because both measures are conditional on prosecutions. We can conclude the same for the measures at the extensive margin.

Presence of illegal armed groups. For this variable we use a database originally compiled by Restrepo et al. (2004) and updated by the Universidad del Rosario until 2014.[10] To measure the presence of illegal armed groups in the municipality, we follow Prem et al. (2020). We construct a dummy for armed groups' presence if there was at least one violent act committed by an illegal armed group[11] in the period between 2006 and 2010, before the reform and the equivalent of the duration of one government term. We do not use the measure for 2011 because it was an election year, and attacks tend to increase before elections.[12] Additionally, we construct individual measures of presence in the municipality for each illegal armed group.

According to our measure, in the period between 2006 and 2010, illegal armed groups were present in 567 of Colombia's 1,123 municipalities. Paramilitaries were present in 27% of the municipalities, FARC in 38%, and ELN in 13%. Our measure of *producer* municipalities is positively correlated with our measure of the presence of illegal armed groups. Figure A1 shows that the set of municipalities with interest groups' presence contains the set of municipalities that received royalties between 1996 and 1999. The correlation between the two groups is positive because illegal armed groups have historically stolen royalties in Colombia, as described in the previous section.

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[10]For a detailed discussion of the data, see Prem et al. (2020).

[11]We include the Revolutionary Armed Forces of Colombia (FARC), the National Liberation Army (ELN) and paramilitaries.

[12]See Condra et al. (2018).

Royalties. The royalty data come from different sources.[13] We use the royalty data to construct a measure that allows us to differentiate between *producers* and *non-producers*. Between 1996 and 1999, 297 municipalities received royalties - 35% of our sample. We use the average direct royalties received by a municipality between 1996 and 1999 to measure exposure to resource holdings with high discretionary levels. We are interested in capturing the effect on municipalities that stakeholders see as targets for high royalty resource holdings, where they believe they can grab a larger share of the pie. We use this period for three reasons:: i) it is just before our study period; ii) it is four years, which, again, is the length of a government term in Colombia; and iii)it allows us to deal with endogenous concerns.[14].

Accountability. We use the Open Government Indicator (IGA), which PGN developed.[15] The IGA is a composite indicator that determines the level of information reporting and the state of the progress in implementing some standards that seek to strengthen territorial public management. Its composition includes an accountability indicator that evaluates the mechanisms used by territorial authorities to facilitate citizens' and civil society organizations' involvement in the formulation, execution, control, and evaluation of public management and to render accounts on the management executed.[16] The indicator takes values from 0 to 100, where a higher value means greater exposure of the municipality to accountability mechanisms.

Other variables. The mayoral controls come from the work of Ruiz (2017), who builds them from image analysis of the ballots and a process of administrative data analysis. The measurement of political ideology comes from Fergusson et al. (2020). We control for a number of alternative explanations for the corruption outcomes, including general and socio-economic characteristics of the municipalities, good governance, public expenditures, and financial development. In addition, we include geographical controls associated with the tenure of natural resources. We use the CEDE panel of the Universidad de Los Andes as a source for all of the controls mentioned above.

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[13]The entities responsible for providing the royalty data have changed over time and as the system has been transformed. Prior to the institutional change, direct royalties were allocated by the oil and mining management entities in the absence of such change.

[14]We are concerned, for example, that mayors or interest groups in municipalities with high royalty amounts would anticipate the reform and change their corrupt behavior. We believe that ten years before the reform is enough time to determine that the royalty budget is exogenous.

[15]See https://www.procuraduria.gov.co/portal/que_es_IGA.page

[16]See https://www.procuraduria.gov.co/portal/media/file/IGAP.pdf

4   Empirical Strategy

In this section, we illustrate our identification strategy, which is based on a difference-in-differences estimator. We describe our main identification assumption and develop our strategy for testing it. Finally, we develop an augmented version of our main specification that we will use to explore possible heterogeneous effects of the reform.

Main specification.   Our identification strategy exploits the timing of the reform's implementation (in 2011) and the variation in exposure to receiving royalties among Colombian municipalities between 1996 and 1999. The main specification has the following form, where i is the subindex of the municipality and t the term of office:

$$y_{it} = \alpha_i + \gamma_t + \theta(Royalties_i \times PostReform_t) + \sum_{x \in X_i} \psi_x(x \times \gamma_t) + \varepsilon_{it,} \tag{1}$$

where y_{it} are the measures of corruption in municipality i for the term of office t. $Royalties_i$ is the average of the direct royalties received by municipality i between 1996 and 1999. $PostReform_t$ is an indicator variable that takes the value of 1 for the period of government after the reform and is the same for all municipalities, and of 0 otherwise. We include a complete set of fixed effects of municipality α_i to control for any source of heterogeneity among municipalities that does not change over time, as well as a complete set of fixed effects of time γ_t that capture any trend at the national level on the investigations being conducted by the prosecutor's office. The term $\sum_{x \in X_i} \psi_x(x \times \gamma_t)$ in equation (1) represents a complete set of time interactions (government period) with a complete set of pre-reform controls, which were described in Tables 1 y 2. Finally, ε_{it} is the clustered error term that allows correlation between municipalities.[17]

The coefficient of interest, θ, captures the average differential change in the corruption behavior of local politicians before and after the reform in *producer* municipalities, relative to *non-producer* municipalities. We focus on the intensive margin measures of corruption — on the probability that at least one mayor or senior government official of municipality i had a disciplinary prosecution brought against him/her during government period t. The do the same for our two remaining measures: the probability of being found guilty and of being removed from office.

Identifying assumption.   The main identifying assumption of our model is that in the absence of reform, local politicians' corrupt behavior in *producer* municipalities should follow the same trajectory as the corrupt behavior of local politicians in *non-producer* municipalities. To validate

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[17]See Abadie et al. (2017).

the assumption, the "parallel trends," we estimate the following equation:

$$\Delta y_{it} = \Theta Royalties_i + \sum_{x \in X_i} \psi_x(x \times \gamma_t) + v_{i,} \tag{2}$$

where Δy_{it} is the first difference in the corruption measures before reform. Since the first difference is taken, the first observation is lost, and t takes values for the government periods ending in 2007 and 2011. The other terms included in the equation are the same as those included in equation (1), taking into account the new values taken by t.

The coefficient of interest in this equation is Θ, and we expect it to be close to 0 and not statistically significant. This implies that changes in local politicians' corrupt behavior before the reform are not correlated with the exposure to royalties between 1996 and 1999 or, more broadly with being a *producer* or a *non-producer* municipality.

Heterogeneous effects. Other key changes of the reform could have additional effects on the corruption behavior of local politicians. We use an augmented version of the main specification, equation (1), to evaluate the heterogeneous effects. We estimate the following equation:

$$\begin{aligned} y_{it} = \alpha_i + \gamma_t + \theta_1(Royalties_i \times Z_i \times PostReform_t) + \theta_2(Royalties_i \times PostReform_t) \\ + \theta_3(Z_i \times PostReform_t) + \sum_{x \in X_i} \psi_x(x \times \gamma_t) + \mu_{it,} \end{aligned} \tag{3}$$

The coefficient of interest in this equation is θ_1, which captures the differential effect on the corrupt behavior of mayors or government officials in *producer* municipalities that also share characteristic Z_i. This is an exploratory exercise that seeks to understand the heterogeneous effects of the reform and does not attribute a causal relationship, so we will be cautious about reading these results. The following section presents the main results, the dynamic persistence of the effect that allows us to validate the assumption of parallel trends, and some robustness tests and alternative specifications.

5   Main results

In this section, we present the main results of the effect of the reform. First, we show that before the reform, *producer* municipalities were more corrupt. Second, we show that, after the reform, local

politicians' corruption behavior in *net-loser* municipalities municipalities decreased more than in the *net-winner* municipalities. We reach the same conclusion for our three measures of corruption, and the results are robust to the inclusion of controls and different levels of fixed effects. Third, we show evidence that our main identifying assumption — i.e. the parallel trends assumption — hold. Changes in local politicians' corrupt behavior before the reform were not correlated with the royalties received between 1996 and 1999. Fourth, we show that the results hold when we randomly exclude groups of ten *producer* municipalities and departments individually. Fifth, we develop a Two-Stage Least Squares (2SLS) to finish eliminating endogeneity concerns. The results are robust and allow us to reach the same conclusions as with the main specification results, and our instrument is powerful. Finally, to deal with the difficulty of defining what corruption is, we perform a Least Absolute Shrinkage and Selection Operator (LASSO) for select controls to rule out possible explanations of our main results by unobservable confounders. Results are robust to the inclusion of controls selected by the LASSO.

Corruption before the reform. Table 3 presents the pre-treatment version, before 2011, of equation (1). Panel A differs from panel B in that they are specifications that do not include controls. All columns include government period fixed effects, and columns 4-6 include additional department fixed effects. To avoid multicollinearity with our independent variable, none of the specifications have municipality fixed effects. The dependent variable in columns 1 and 4 is the indicator of at least the mayor or a senior official of a municipality m having had a disciplinary prosecution process opened against him or her during government period t. Columns 2 and 5 are the indicator of the local politician being found guilty, and columns 3 and 6 are the indicator of the local politician being removed from office. We find that, before the reform, politicians in *producer* municipalities were more corrupt. Focusing on column 1 of panel B, a one-standard-deviation increase in the average royalties that a municipality received before the reform increased the probability that a local politician had open prosecution proceedings by 3.3 percentage points (pp.), which represents an increase of 21% in the probability for local politicians in *non-producer* municipalities.

Before the reform, we can observe in columns 2 and 3 that the likelihood that a mayor or a senior government official of a producing municipality would be found guilty or be removed from office was 30% and 45%, respectively. The municipalities that received royalties attracted more-corrupt politicians who also committed more-serious irregularities that resulted in sanctions (even severe ones). Columns 4-6 show that the results are robust to controlling for fixed characteristics of the departments.

Corruption after the reform. When the Colombian government implemented the reform in 2011, corrupt behavior changed, and resource extraction opportunities decreased. After the reform,

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we see a differential drop in the probability of a local politician committing irregularities or being sanctioned in the *producer* municipalities. Table 4 presents our main specification results, which allow us to show the differential effect that the reform had on politicians' corrupt behavior in *producer* relative to *non-producer* municipalities. Regressions in panel A differ from panel B in that they are uncontrolled specifications. All regressions include municipality and government term fixed effects. The dependent variables in columns 1 - 3 correspond to our three measures of corruption. In column 1 of panel B, we can observe that a one-standard-deviation increase in royalties in *producer* municipalities reduced the probability that the mayor or a senior official would be prosecuted after the reform by 2.6 pp, relative to local politicians in *non-producer* municipalities. The drop was 17% relative to the probability of the governors of the *net-winners* municipalities being prosecuted. In the same vein, we can observe in columns 2 and 3 of panel B that the drop in the probability of being found guilty and removed from office fell 33 and 46 percent, respectively. For robustness, in Table A2 we report our main results that take into account the potential cross-sectional dependence in the error term, see Conley (1999, 2016).[18]

Identifying Assumption. We did not find differential trends before the reform between *producer* and *non-producer* municipalities. In Table 5, we present our results from equation (2). All specifications include government period fixed effects. For each outcome given in columns 1 to 3, we take its respective first difference and regress it against our measure of exposure to royalty receipt. In column 1 of panel B, we show that mayors or senior government officials having been exposed to receiving royalties before the reform is not correlated with changes in the probability of being prosecuted before the refom. We can conclude the same for the other two outcomes presented in columns 2 and 3. Results are also robust to the inclusion of mayor and municipal controls. Note that the coefficient is not significant and is very close to 0.

Traditional methods for testing the assumption of parallel trends have been highly questioned in the literature.[19] Following Rambachan and Roth (2019), we allow for three possible violations of the parallel trends assumption that result in the existence of pre-trends: i) deviation from linearity, ii) monotonic restrictions on differential trends, and iii) sign bias in the post-period. The authors suggest to i) allow M to take values from 0 to the standard deviation of the first post-reform government period's coefficient.[20] Where $M = 0$ is complete linearity and for all $M > 0$ is a deviation from linearity. For policies that sought to fight corruption before the reform, we assume for ii) a possible monotonic decreasing form in the pre-trends. Finally, since we estimate the effect

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[18]We also implement the standard errors suggested by Adao et al. (2019) that allow for any correlation structure of the regression residuals across geographical regions. For all columns we find the p-values smaller than 1% (not reported).

[19]Roth (2019), using data from 12 published papers in top journals, he finds that the magnitude of violations of parallel trends against which there is 50 percent and 80 percent power can be sizeable, and often comparable in size to the estimated treatment effect.

[20]When the number of periods pre-reform is equal to 1. If we take as a reference in an event study version of our main specification the period before the reform, there is only one period left.

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on observed corruption, we believe that we are in the lower-bound, so for iii) we believe that the bias direction is negative. Figure 4 shows that our results are robust to any proposed three violations of parallel trend assumption.

Robustness Tests and Alternative Specifications. In the context section, we pointed out the concentration of resources in a few municipalities and departments throughout the country. Concerned that our results are driven by a small group of municipalities, by a small group of *producer* municipalities, or by a specific department, we show, in Figures 5, 6 and 7, estimates of equation (1) by randomly drawing groups of ten *producer* municipalities and drawing all municipalities that belong to a department.[21] In Figure 5, each point represents the estimated coefficient of the reform's differential effect without the randomly drawn group of twenty-six municipalities, balanced in both treatment and controls. The bars are the 95% confidence intervals of the estimator. Panel A shows the probability of having open prosecutions; Panel B shows the probability of being found guilty; and Panel C shows the probability of being removed from office. In panel A, we can observe that our results are robust to the exclusion of different municipalities, and the point estimates are always, on average, similar to the one presented in column 1 of panel B of Table 4.[22] In panels B and C, we can also observe that our results for the other two corruption measures are robust to excluding random groups of municipalities. Figure 6 shows that the results are also robust to the random exclusion of groups of ten producer municipalities.

The departments also receive royalty resources and may co-finance investment projects with the municipalities. The high concentration of resources is also concentrated in a few departments. In Figure 7, we present our main specification estimates' results by removing departments one by one. Again, we can observe that our results are robust to excluding a group of municipalities belonging to the same department. With these results, we can rule out that the drop in corruption we observe is driven by a small group of *producer* municipalities or by specific department. The drop in corrupt behavior of local politicians is generalized, on average, across all *producer* municipalities and departments.

What would happen to our results if the treatment allocation — royalty exposure — were different? We randomly assigned the average royalties received between 1996 and 1999 to each municipality based on the current observed values with a randomization inference exercise. We repeated this procedure a thousand times and each time re-estimated our main specification in equation 1. In Figure A2 in the appendix, we plot the estimators' distribution resulting from the permutation test, alongside a vertical line on the actual true estimate. The probability of observing a similar effect to the ones presented in Table 4 is below 1% in all outcomes.

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[21]We do this successively for all 32 departments.

[22]Note that our result in column 1 of panel B of Table 4 is significant at a 90% confidence level and the confidence intervals plotted in Figure 5 are at 95%.

Even though our measure of royalty exposure from the main specification is sufficiently exogenous, we develop a 2SLS to supplant any residual concerns about endogeneity that may be generated by local politicians' possible anticipation of reform. Following Dube and Vargas (2013), we instrument our royalty exposure measure by exploiting variation in the international price of resources and municipal presence of the resources. Therefore, our instrument is the interaction between the monthly average of international price of the resources and municipal presence of the resource.[23] Specifically we use oil and gold. The instrument is relevant because the municipalities' royalty transfers increase when the international price of oil and gold rises. Although we cannot test the exclusion restriction, we know that Colombia is a price-accepting country. Its influence on the international oil and gold market is low, so there is no reason to think that corrupt local politicians can influence the international price to benefit themselves. Since the interaction between royalties and the post-reform dummy should be endogenous as well, we instrument the interaction with presence of oil and gold, oil and gold international prices, and the post-reform time dummy.[24]

The second stage follows the same form of equation (1), where the interaction that accompanies the parameter of interest is instrumented. Meanwhile, the first stage follows the following form:

$$
\widehat{Royalties_i \times PostReform_t} = \alpha_i + \gamma_t + \Theta(\boldsymbol{PriceZ} \times \boldsymbol{PresenceZ}_i \times PostReform_t) \\
+ \sum_{x \in X_i} \psi_x(x \times \gamma_t) + v_{it,} \tag{4}
$$

where $\boldsymbol{PriceZ}$ is a vector of the average monthly international price of resource Z in 1988. $\boldsymbol{PresenceZ}_i$ is a vector of measures of the presence of resource Z in municipality i. We use production in 1988 for oil and gold suitability constructed by Idrobo et al. (2014) for gold. We also, as in equation (1), include a complete set of municipality and period fixed effects, as well as controls interacted by dummies of time. Table A3 presents the results of our 2SLS. In column 1, we present the results of the first stage. In the even columns, we present our main specification results in panel B of Table 4 and the odd columns (3, 5, and 7) resulting from the second stage.[25] Focusing on column 1, we can observe that our instrument is relevant. In columns 2 and 3, we show that our results on the drop in the probability of being prosecuted are robust to the correction for possible remaining endogeneities. The same conclusion can be generalized for our other two corruption measures in columns 4 - 7. The magnitude of the 2SLS coefficients are similar to those of the OLS — on average, two times larger — and we gain significance in prosecutions. Also, with Kleibergen and Paap (2006) over-identification test, we find evidence that the instrument is powerful.

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[23]The instrument has been widely used in the literature; see, e.g., Carreri and Dube (2017) and Gallego et al. (2020).

[24]Gallego et al. (2020) follow a similar approach.

[25]We place both results together to facilitate the comparison of the estimators.

Identifying the correct model is a common problem in applied econometrics. When we talk about corruption, the problem becomes even more acute. Belloni et al. (2011, 2014) have made a significant effort to address this problem by adopting innovations in "data mining" and "machine learning." Using LASSO as a method for the estimation of coefficients of linear models is useful to obtain predictors that are strongly associated with outcome variables.

Following the authors, Table A4 presents the results of the main specification, including the controls that were selected by the LASSO method for each of the dependent variables. The controls of municipalities and mayors used in the main regressions and a set of 127 controls at the municipal level were included for the selection. Within the additional set of controls are variables that measure general and socio-economic characteristics of the municipalities, good governance, public expenditures, and financial development. Panel A presents the results before the reform and Panel B the results after the reform. It can be observed that the results are consistent with those presented in Tables 3 and 4. This means that the selection of the controls is not driving the results.

Finally, with the same set of controls used in the main specification and in the LASSO exercise, we calculate a propensity score for the probability of being a producer municipality. Following Crump et al. (2009), we estimate the optimal cut-off point of the propensity score to select a sub-sample for which the average treatment effect can be estimated more precisely. Table A5 presents the results of our main specification, excluding municipalities with a propensity score lower than 0.09 and higher than 0.91. We find that our results are robust and hold in this optimal sub-sample. The probability that a local politician in a *producer* municipality is corrupt falls after the reform relative to local politicians in *non-producer* municipalities.

Mechanisms. One direct potential mechanism that can explain our results is a mechanical consequence of the reform. After the reform, *producer* municipalities had fewer royalties because, now all, municipalities (*producers* and *non-producers*) could access the resources and compete for them through meritocratic processes. Table 6 presents the results of equation (1), changing the outcome by the total royalties received by the municipality during a year and a logarithmic transformation of the measure. Focusing on column 1, we observe that after the reform, the *producer* municipalities received, on average, 30% less than their total yearly royalties — this is why we call *producer* municipalities the *net-losers* of the reform. The results are robust to the inclusion of controls and logarithmic transformations and suggest that the drop in royalties from *net-losers* could explain the drop in corruption that we observed in our main result. In Table 7, we control our main specification for the royalties received by each municipality in each term-of-office, as a "bad control", and observe that our results do not change - we observe only that the coefficients grow a little. We can rule this out as a potential mechanism behind our results.

It is natural to think that local politicians in *producer* municipalities would be prosecuted less

after the reform, simply because they manage fewer resources. The assumption behind this would be that the probability of having an open prosecution process is linear, and the control agencies would look more closely at those who have more resources. Table 8 shows that the number of audits - social control exercises - carried out in *producer* municipalities did not change after the reform. Even under the assumption that after the reform, the control agencies had fewer incentives and less capacity[26] to investigate *producer* municipalities, we observe that they continued to audit them with the same intensity as before the reform. With this result, we can rule out this as a potential explanation for our results. The transparency and accountability components of the reform are the two potential ways in which local politicians in *net-losers* municipalities reduced their corrupt behavior and ceased to be attractive to rent-seekers. In the next section, we indirectly identify rent-seekers to explore possible heterogeneous effects of the reform's transparency component. We also explore possible heterogeneous effects of the accountability component of the reform.

6 Heterogeneous effects

The reform of the allocation of royalty resources can be grouped into two set groups of changes: i) transparency and ii) accountability. In this section, we present the results of two potential heterogeneous effects of the reform. First, for i), through an indirect way of identifying rent-seekers that were more affected by transparency, we find that *producer* municipalities with a presence of illegal armed groups were more corrupt before the reform and that, after the reform, they further reduced corrupt behavior. Second, for ii), with our main specification, we look at the reform's effect on an accountability indicator. The results suggest no differential effect of accountability mechanisms on *producer* municipalities. The transparency component led to the drop in corruption in the step-by-step formulation and allocation phase of investment projects.

Conflict and corruption. From the formulation to the project executor assignment, a series of processes that guarantee quality and make the information more visible must be overcome; this is what we call the transparency component of the reform. To determine who is differentially affected by transparency, we identify where the rent-seekers are concentrated in Colombia. As mentioned earlier in the context section, all illegal armed groups have stolen royalties through extortion or through collaboration with local politicians, state officials, and local social leaders.[27] From the dynamics of conventional corruption mechanisms, typical of the traditional political and economic elites, the Colombian conflict finds a "natural" development in a process of strategic corruption, directed not only to usufruct in the public sector for private benefit, but also to dispute the political power of the State. Elites and armed groups constructed mafia-state forms, in which the illegal

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[26]Because, after the reform, control agencies had to investigate *non-producers* as well.

[27]See Rettberg and Prieto (2018).

and the legal coexist and interact in a natural and complementary way in a growing privatization of the public by the double way of co-opted institutionality and directed criminality.[28]

In panel A of Table 9, we present the pre-reform version of equation (3). Note that the producer municipalities that had the presence of armed groups were more corrupt before the reform. The probability that a governor of a municipality would be prosecuted, found guilty, or removed from office was higher in producer municipalities with armed groups.[29] Additional support from anecdotal evidence helps us understand the *modus operandi* of illegal armed groups to extract rents. Consider, for example, the "Casanare Pact" between six mayors and paramilitaries:

The Public Prosecutor and the Attorney General's Office asked a judge in Cundinamarca to sentence six former mayors of municipalities in southern Casanare, for aggravated conspiracy to commit a crime after having armed the so-called Casanare Pact with the Casanare Peasant Self-Defense Forces (ACC) with which they committed to give the parami1itaries 50 percent of the municipal budget and 10 percent of the contracts (VerdadAbierta, 2009).

Another example is the way the "Domingo Laín" front of the ELN operated in the department of Arauca, which was very similar to the way the FARC was used:[30]

Candidates agreed on a social investment plan with the Domingo Laín front, assigned bureaucratic quotas, and appointed people recommended by the guerrillas as secretaries of the department. Fictitious contracts for works were also processed and the money was diverted to the ELN. In some cases, the governor's office contracted works with community action boards controlled by the guerrillas. The contracts were signed but the works were not carried out and the money was distributed among the contractors, the governor, and the ELN (Duque, 2017).

What are the implications of making information more visible? Suppose that a mayor wants to give a contract to an illegal armed group that financed his campaign[31] or with whom he has an agreement. After the reform, in addition to being formulated to suit the contractor, the project had to comply with certain technicalities; go through discussions with experts and representatives of different levels of government; be socially benefited or affected communities; get approval if it was aligned with the prioritization criteria; and, finally, assign the contract through a bidding process. With government entities and civil society interacting constantly and with greater access to information on the procedures, it sounds like an impossible mission for a local politician to deliver a contract to repay a political favor, and even more so if it is to an armed group that has to operate illegally. In panel B of Table 9, we present the results of our equation (3). Indeed, after

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[28]For a more detailed discussion, see Gallego (2010).

[29]Table A6 shows that the result may be driven by municipalities with paramilitaries and FARC.

[30]See, for example, Gallego (2010).

[31]Gulzar et al. (2021) show evidence that donations to the winner of an election in Colombia increase the probability of receiving contracts.

the reform, local politicians' corrupt behavior declined more in the producer municipalities with illegal armed groups.[32] Not only are they less likely to be prosecuted, but they are also less likely to be found guilty and less likely to be removed from office.

Accountability and corruption.   While the transparency component operates before awarding contracts, the accountability mechanisms operate mainly during the investment project's execution. The reform created the Monitoring, Follow-up, Control and Evaluation System (SMSCE), which is in charge of the DNP, to strengthen accountability mechanisms to supervise and coordinate activities related to royalties' administration and execution. One tool that was strengthened was the Public Audits. We use the accountability indicator[33] described in the data section to see if the reform had a differential effect on the mechanisms available to citizens for accountability. In Table 10, we present the results of equation (1), where the outcome on this occasion is the accountability indicator. We do not observe any differential effect of the reform on strengthening accountability mechanisms in *producer* municipalities.

7   Conclusions

This paper studies how institutional reform around the allocation of resources from natural resource exploitation can reduce corruption. The 2011 reform in Colombia gave access to royalty resources to all municipalities and, with a set of changes, strengthened transparency and accountability mechanisms. Using as a measure of exposure to royalty receipt before the reform to identify *producer* and *non-producer* municipalities, we show that local politicians in *producer* municipalities were more corrupt before the reform. Exposure to large discretionary resource budgets attracts corruption and incentivizes corrupt behavior by politicians. With the reform's more equitable distribution of resources, there are *net-winners* and *net-losers*. *Non-producer* municipalities win because they did not receive royalties before, and producers lose because they now have to share resources. Exploiting the timing of the reform, we show that corruption fell differentially in the *net-loser* municipalities. We rule out that the main mechanism underlying our main results is the mechanical effect of the reform; the *producer* municipalities' loss of resources does not explain the reduction in local politicians' corruption. We also rule out that fewer local politicians in *producer* municipalities are investigated due to the loss of resources and the possible limitation of the control agencies' capacity to investigate all municipalities after the reform.

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[32]Table A7 shows that the drop is driven mainly by the producer municipalities with paramilitary and FARC presence.

[33]This indicator evaluates the mechanisms that allow citizen participation, promoted by the entities or agencies of the public administration, where natural or legal persons and social organizations meet in a public act to exchange information, explanations, evaluations, and proposals on aspects related to the execution and evaluation of policies in charge of each entity, as well as on the management of resources to comply with such programs. See http://www.anticorrupcion.gov.co/Paginas/indice-gobierno-abierto.aspx in Annex 1.

Also, we show how illegal armed groups stole royalties before the reform through associations with local politicians. But we find that, once the Colombian government implemented the reform, transparency could be an indirect way to control resource appropriation and state capture by illegal armed groups. We suggest that the step-by-step transparency that local politicians must face from project formulation to contract allocation is the main mechanism by which the corrupt reduce their corrupt behavior and make it more difficult for rent-seekers to extract rents. Finally, we find no differential effect of the reform on strengthening accountability mechanisms.

Our results have important policy implications. Institutional reform that ensures equitable distribution and transparent allocation of resources through meritocratic processes can help in the fight against corruption. Also, we find an indirect way for the national government to control state capture and the appropriation of royalty resources in subnational governments.

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Figure 1: Step-by-step flow of the reform
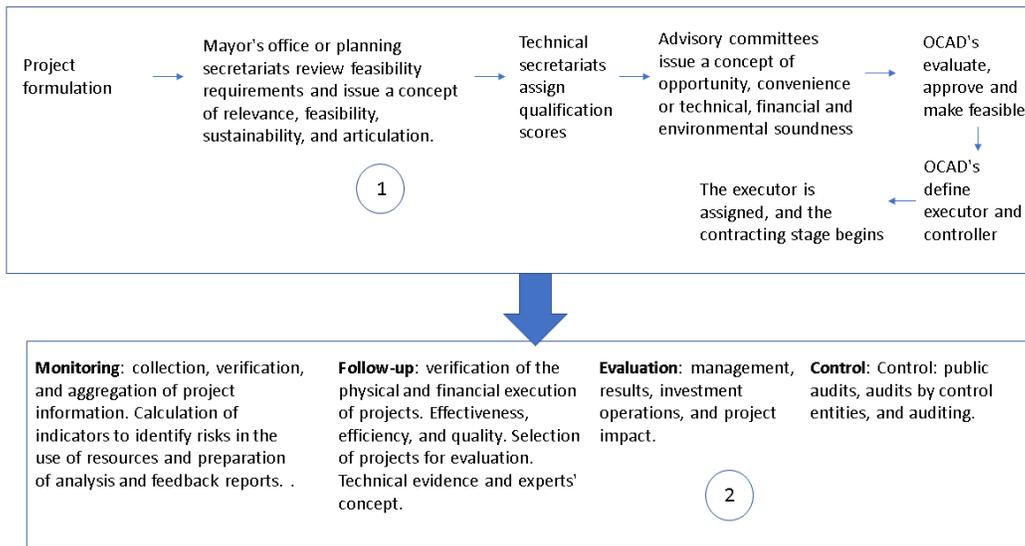
Notes: This figure illustrates the flow chart of the resource allocation system implemented by the reform in 2011. Box 1 is what we call the transparency component and box 2 is what we call the accountability component.

Figure 2: Royalties before the reform

Royalties

Mean 1996 - 1999 (millions of pesos)

- No data/No value
- 0 - 915
- 915 - 6483
- 6483 - 11820
- 11820 - 15623
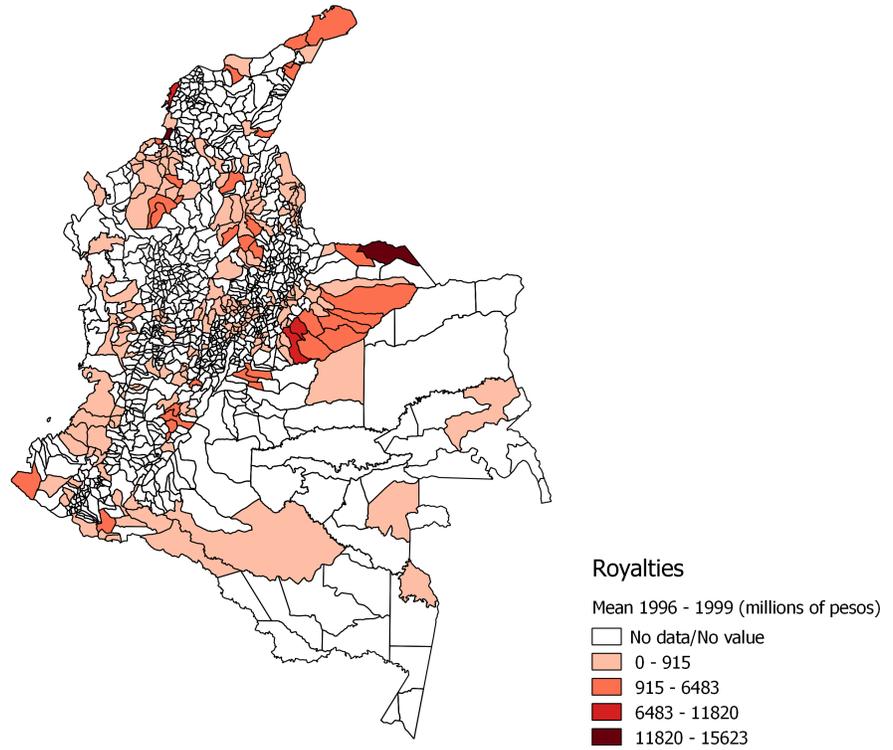
Notes: The map is the distribution of royalties between 1996 and 1999, which is the measure we use to identify *producer* and *non-producer* municipalities.

Figure 3: Prosecutions between producers and non-producers
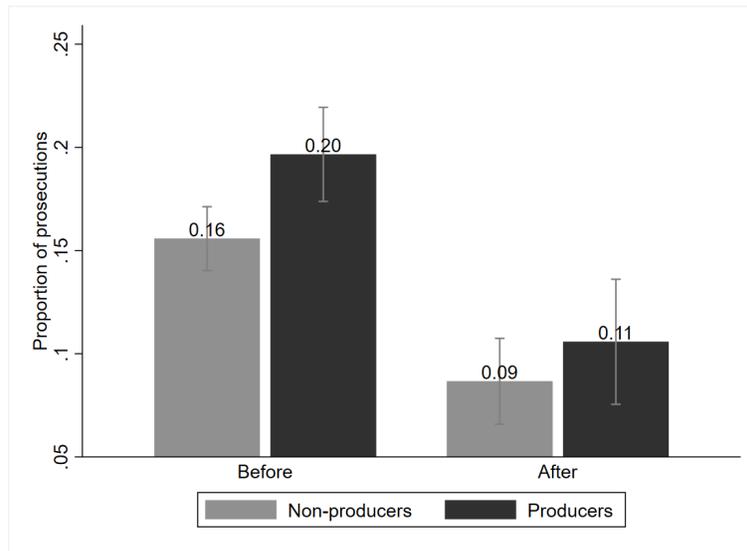
Notes: This figure shows the probability that a mayor or a senior official government had an open disciplinary process before and after the reform, disaggregated by *producer* and *non-producer* municipalities. Reported confidence intervals are at 95% confidence.

Figure 4: Violation of parallel trends assumption

(a) Prosecutions - linearity   (b) Prosecutions - monotonic   (c) Prosecutions - sign bias

(d) Guilty - linearity   (e) Guilty - monotonic   (f) Guilty - sign bias

(g) Dismissed - linearity   (h) Dismissed - monotonic   (i) Dismissed - sign bias
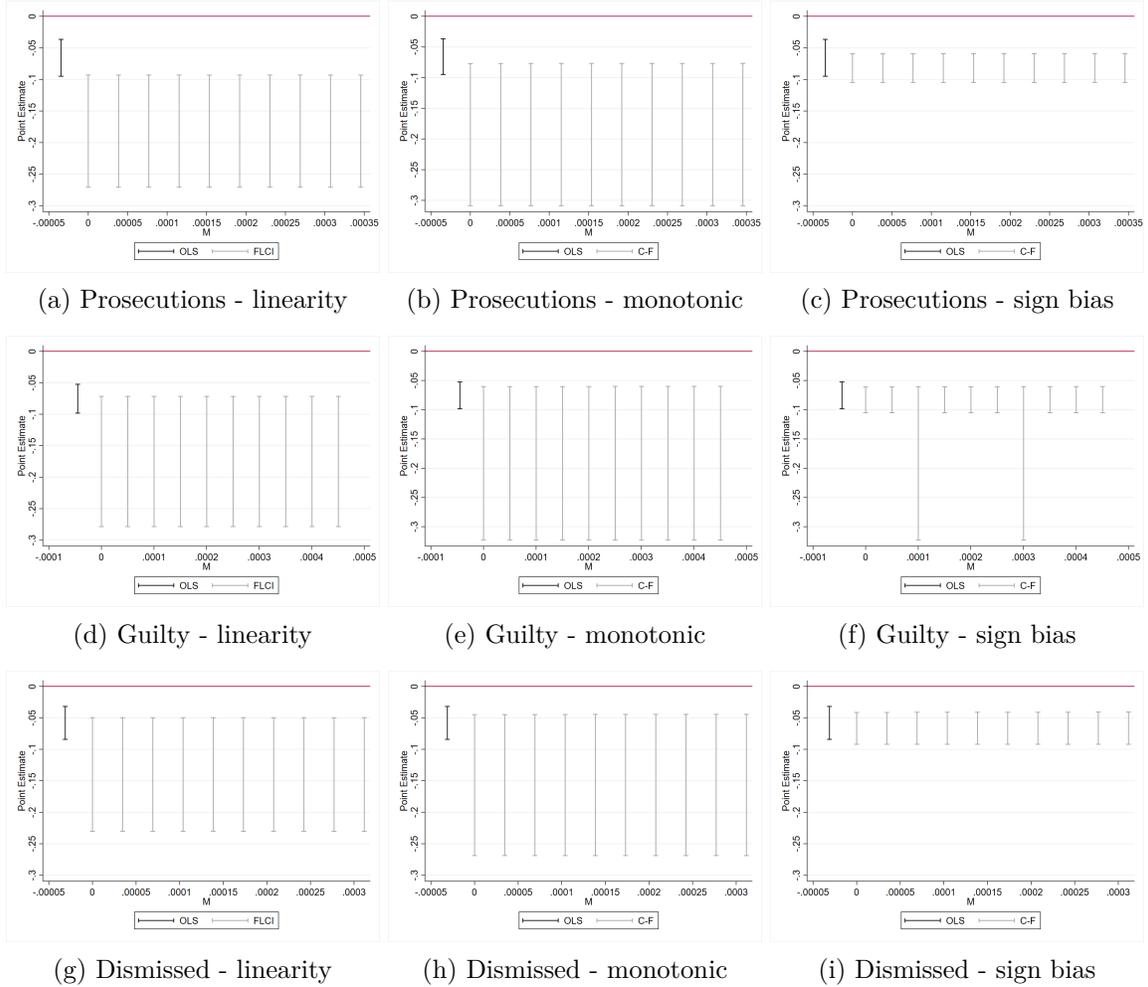
Notes: This figure presents the results of a sensitivity analysis of the effect of the reform on corrupt behavior, constructing robust confidence sets under varying assumptions on the class of possible violations of parallel trends. Row 1 corresponds to *Prosecutions* outcome, Row 2 to *Guilty* and Row 3 to *Dimissed*. In black we plot the OLS coefficient of an event study version of our main specification. In grey we plot optimal fixed length confidence intervals (FLCI) or conditional FLCI (C-F) depending on the violation, as Rambachan and Roth (2019) suggest. The first column of the figure corresponds to linearity, second column to monotonic decreasing, and third column to bias direction.

Figure 5: Dropout random groups of municipalities
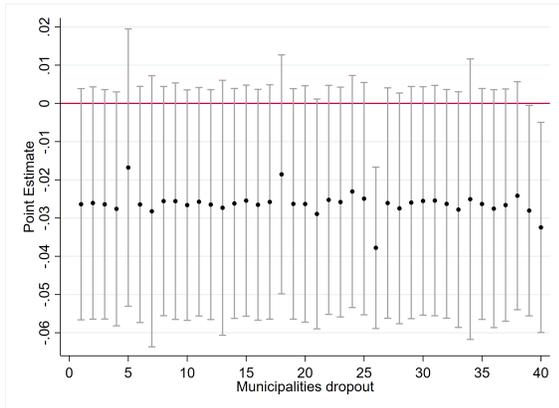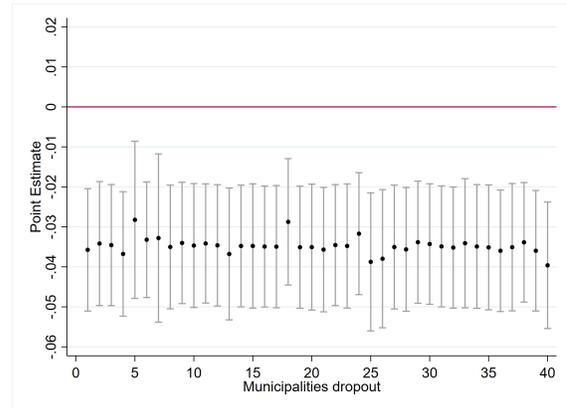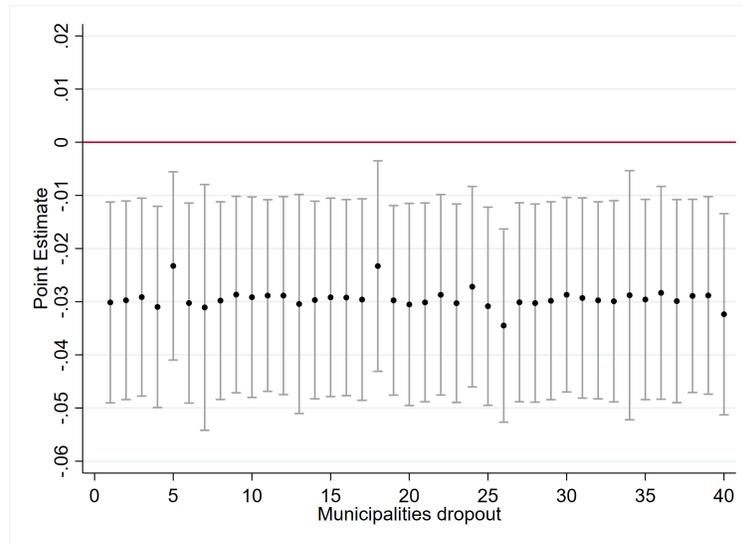
(a) Prosecutions

(b) Guilty

(c) Dismissed

Notes: This figure shows how the estimator of the main specification changes by taking out random groups of 26 municipalities. Each point estimate has its corresponding 95% confidence interval. Panel A corresponds to *Prosecutions* outcome, Panel B to *Guilty* and Panel C to *Dimissed*.

Figure 6: Dropout random groups of producer municipalities
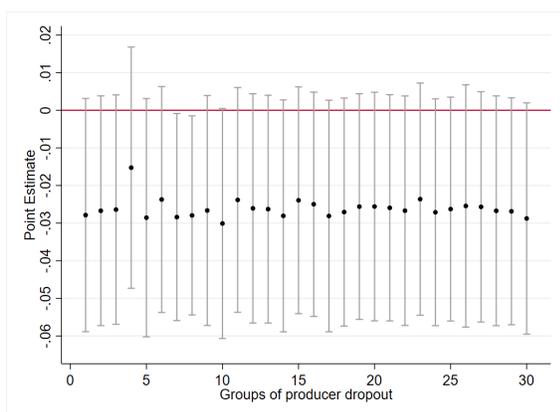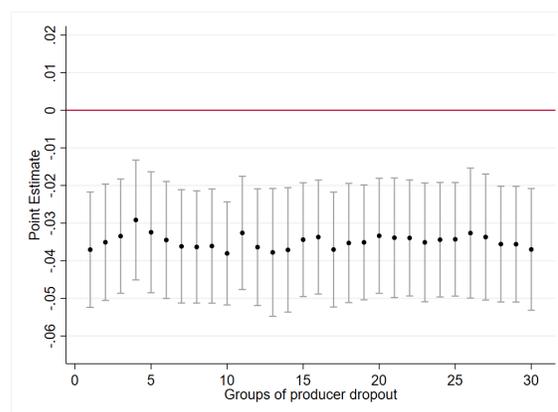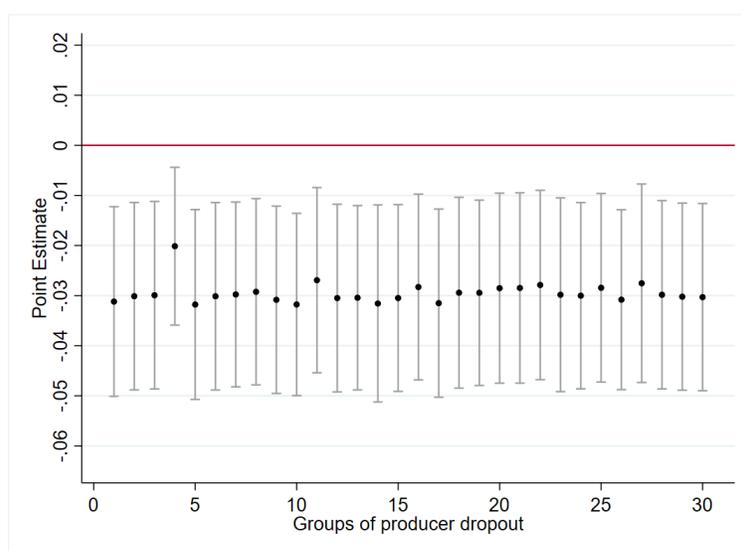
(a) Prosecutions

(b) Guilty

(c) Dismissed

Notes: This figure shows how the estimator of the main specification changes by taking out random groups of ten *producer* municipalities. Each point estimate has its corresponding 95% confidence interval. Panel A corresponds to *Prosecutions* outcome, Panel B to *Guilty* and Panel C to *Dimissed*.

Figure 7: Dropout departments
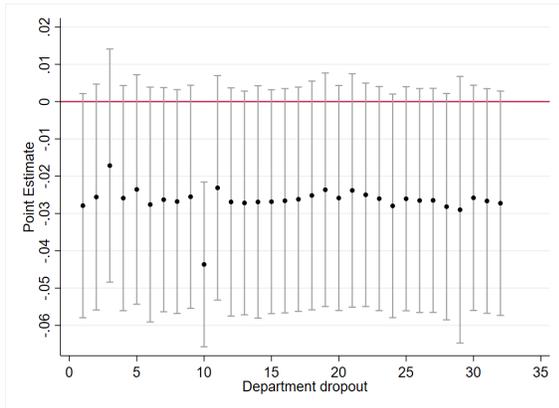
(a) Prosecutions
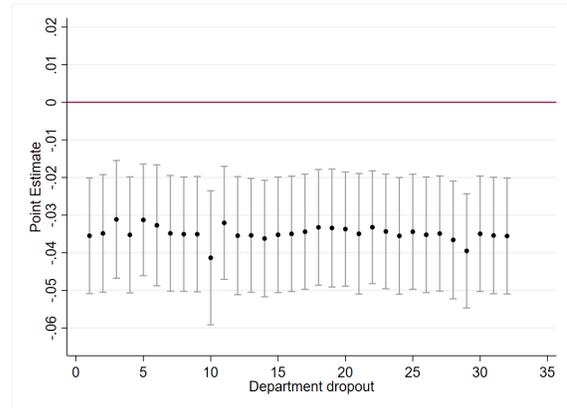
(b) Guilty
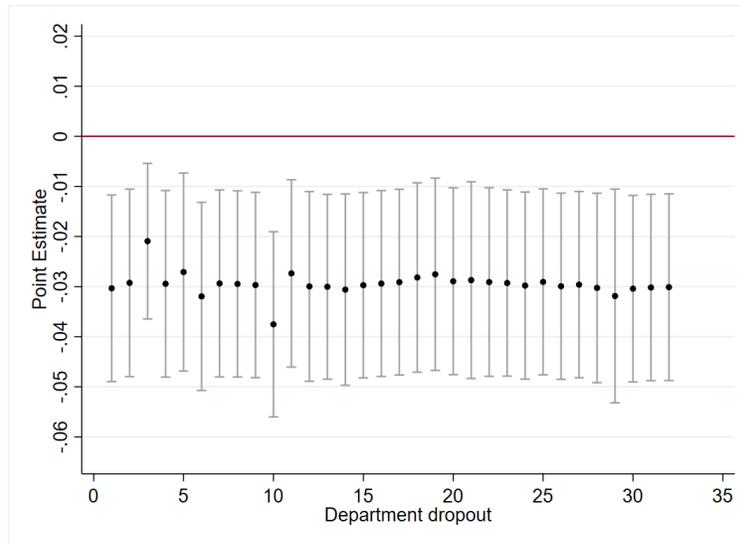
(c) Dismissed

**Notes:** This figure shows how the estimator of the main specification changes by taking out one department at a time. Each point estimate has its corresponding 95% confidence interval. Panel A corresponds to *Prosecutions* outcome, Panel B to *Guilty* and Panel C to *Dimissed*.

Table 1: Summary Statistics

	Obs.	Mean	S.D	Min	Max
Corruption					
Prosecutions	4,300	0.154	0.361	0	1
Guilty	4,300	0.096	0.294	0	1
Dismissed	4,300	0.055	0.227	0	1
N Prosecutions	4,300	0.257	0.792	0	15
N Guilty fee	4,300	0.132	0.491	0	10
N Dismissed	4,300	0.071	0.339	0	6
Mayor controls					
Women	4,300	0.098	0.297	0	1
Left party	4,300	0.028	0.165	0	1
Right-wing	4,300	0.240	0.427	0	1
Illegal Registration of ID	4,300	0.007	0.086	0	1
Has political experience	4,300	0.458	0.498	0	1
Has electoral experience	4,300	0.367	0.482	0	1
Municipal controls					
Rurality	4,300	0.566	0.239	0.001	0.983
Area (km^2)	4,300	880.602	2995.366	15	65674
Altitude	4,300	1163.519	1163.207	2	25221
Linear distance to the capital of the Department	4,300	78.711	56.090	0	376.118
Linear distance to Bogota	4,300	313.354	188.495	0	1270.850
NBI	4,300	44.321	20.218	5.360	100
Fiscal Performance Index	4,300	46.904	18.722	0	77.240
GINI Index	4,300	0.432	0.104	0	0.568
Population	4,300	41928.060	258320.900	1007	7467804

Notes: This table reports summary statistics at the municipality level for 1,075 municipalities. *Corruption* variables are for the period 2000-2015. *Prosecutions* is an indicator variable taking value 1 if the mayor or some high-level government staff member had an open disciplinary prosecution after the incumbency period, and 0 otherwise. *Guilty* is an indicator variable taking value 1 if the mayor or some high-level government staff member was found guilty after the start of the incumbency period, and 0 otherwise. *Dismissed* is an indicator variable taking value 1 if the mayor or some high-level government staff member was dismissed after the start of the incumbency period, and 0 otherwise. *N Prosecutions* is the number of disciplinary prosecutions of the mayor or some high-level staff after starting the incumbency period. *N Guilty* is the number of times a mayor or or some high-level staff was found guilty after the incumbency period. *N Dismissed* is the number of times a mayor or or some high-level staff was dismissed after the incumbency period. The controls, both of mayors and municipalities, are time-invariant measures before the reform.

Table 2: Balance of mayors and municipality controls

| | Mean | | | |
	Producers	Non-producers	Diff.	P-value
Mayor controls				
Women	0.099	0.094	0.005	0.789
Left party	0.028	0.027	0.001	0.891
Right-wing	0.241	0.237	0.004	0.876
Illegal Registration of ID	0.006	0.058	-0.053***	0.000
Has political experience	0.457	0.458	-0.001	0.979
Has electoral experience	0.368	0.367	0.001	0.966
Municipal controls				
Rurality	0.575	0.548	0.027	0.077
Area (km^2)	737.675	1151.817	-414.142*	0.031
Altitude	1250.205	999.027	251.178***	0.001
Linear distance to the capital of the Department	76.607	82.701	-6.094	0.090
Linear distance to Bogota	315.183	309.883	5.300	0.661
NBI	44.170	44.608	-0.438	0.736
Fiscal Performance Index	46.507	47.656	-1.149	0.339
GINI Index	0.437	0.422	0.015*	0.026
Population	3.5e+04	5.2e+04	-1.7e+04	0.278

Notes: This table reports controls balance between *producer* and *non-producer* municipalities. Mayor and municipal controls are pre-reform variables. Mayor controls are for the mayor in office before the reform. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3: Corruption before the reform

	(1) Prosecutions	(2) Guilty	(3) Dismissed	(4) Prosecutions	(5) Guilty	(6) Dismissed
Panel A - without controls						
$Royalties_i$	0.044***	0.040***	0.032***	0.043***	0.038***	0.032***
	(0.009)	(0.008)	(0.009)	(0.009)	(0.006)	(0.008)
R-squared	0.069	0.038	0.029	0.111	0.062	0.053
Panel B - with controls						
$Royalties_i$	0.033***	0.032***	0.029***	0.029***	0.026***	0.026***
	(0.008)	(0.007)	(0.010)	(0.006)	(0.005)	(0.009)
Observations	3,225	3,225	3,225	3,225	3,225	3,225
R-squared	0.113	0.073	0.049	0.158	0.108	0.079
Department FE	No	No	No	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var.(non - producers)	0.156	0.106	0.065	0.156	0.106	0.065
SD Dep Var.(non - producers)	0.363	0.307	0.246	0.363	0.307	0.246

Notes: This table illustrates the corrupt behavior of local politicians before the reform. The table reports coefficients obtained from the estimation of a pre-reform version of equation (1) presented in Section 4. The sample includes 1,075 municipalities and the period of study is 2000 - 2011. $Royalties_i$ is the standard deviation of the average direct royalties received during the period 1996-1999 for municipality i. *Prosecutions* is an indicator variable taking value 1 if the mayor or some high-level government had an open disciplinary prosecution after the incumbency period, and 0 otherwise. *Guilty* is an indicator variable taking value 1 if the mayor or some high-level government was found guilty after the start of the incumbency period, and 0 otherwise. *Dismissed* is an indicator variable taking value 1 if the mayor or some high-level government was dismissed after the start of the incumbency period, and 0 otherwise. *Mayor controls* included are sex, political inclination, illegal registration of ID, and previous political and electoral experience. *Municipal controls* included are rurality, area, altitude, distance to capital of the department, distance to capital of Colombia, NBI indicator, Fiscal Performance Index, GINI Index and population. *Avg Dep Var* and *SD Dep Var* values correspond to non-producer municipalities before the reform. Notice that all regressions include term of office fixed effects. Robust standard errors are clustered at the municipality level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Effects of the reform on corruption

	(1) Prosecutions	(2) Guilty	(3) Dismissed
Panel A - without controls			
$Royalties_i \times PostReform_t$	-0.029*	-0.042***	-0.032***
	(0.015)	(0.008)	(0.010)
R-squared	0.351	0.328	0.295
Panel B - with controls			
$Royalties_i \times PostReform_t$	-0.026*	-0.035***	-0.030***
	(0.015)	(0.008)	(0.009)
Observations	4,300	4,300	4,300
R-squared	0.378	0.353	0.314
Municipality FE	Yes	Yes	Yes
Period FE	Yes	Yes	Yes
Avg Dep Var.(non - producers)	0.156	0.106	0.065
SD Dep Var.(non - producers)	0.363	0.307	0.246

Notes: This table illustrates the differential effect of the reform on the corrupt behavior of local politicians. The table reports coefficients obtained from the estimation of equation (1) presented in Section 4. The sample includes 1,075 municipalities and the period of study is 2000 - 2015. $PostReform_t$ is an indicator variable taking value 1 for the period of government after the reform, and 0 otherwise. $Royalties_i$ is the standard deviation of the average direct royalties received during the period 1996-1999 for municipality i. *Prosecutions* is an indicator variable taking value 1 if the mayor or some high-level government had an open disciplinary prosecution after the incumbency period, and 0 otherwise. *Guilty* is an indicator variable taking value 1 if the mayor or some high-level government was found guilty after the start of the incumbency period, and 0 otherwise. *Dismissed* is an indicator variable taking value 1 if the mayor or some high-level government was dismissed after the start of the incumbency period, and 0 otherwise. *Mayor controls* included are sex, political inclination, illegal registration of ID, and previous political and electoral experience. *Municipal controls* included are rurality, area, altitude, distance to capital of the department, distance to capital of Colombia, NBI indicator, Fiscal Performance Index, GINI Index and population. *Avg Dep Var* and *SD Dep Var* values correspond to non-producer municipalities before the reform. Notice that all regressions include municipality and period fixed effects. Robust standard errors are clustered at the municipality level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Parallel trends

	(1) Δ Prosecutions	(2) Δ Guilty	(3) Δ Dismissed
Panel A - without controls			
$Royalties_i$	0.002	-0.001	0.001
	(0.007)	(0.005)	(0.005)
R-squared	0.001	0.012	0.014
Panel B - with controls			
$Royalties_i$	0.004	0.001	0.001
	(0.007)	(0.005)	(0.005)
Observations	2,150	2,150	2,150
R-squared	0.017	0.021	0.018
Period FE	Yes	Yes	Yes
Avg Dep Var.(non - producers)	0.106	0.049	0.026
SD Dep Var.(non - producers)	0.475	0.421	0.340

Notes: This table illustrates the test of our main identification assumption. The table reports coefficients obtained from the estimation of equation (2) presented in Section 4. The sample includes 1,075 munici-palities and the period of study is 2004 - 2011. $Royalties_i$ is the standard deviation of the average direct royalties received during the period 1996-1999 for each municipality. Δ means that it is the first difference of the variable. *Prosecutions* is an indicator variable taking value 1 if the mayor or some high-level gov-ernment had an open disciplinary prosecution after the incumbency period, and 0 otherwise. *Guilty* is an indicator variable taking value 1 if the mayor or some high-level government was found guilty after the start of the incumbency period, and 0 otherwise. *Dismissed* is an indicator variable taking value 1 if the mayor or some high-level government was dismissed after the start of the incumbency period, and 0 other-wise. *Mayor controls* included are sex, political inclination, illegal registration of ID, and previous political and electoral experience. *Municipal controls* included are rurality, area, altitude, distance to capital of the department, distance to capital of Colombia, NBI indicator, Fiscal Performance Index, GINI Index and population. *Avg Dep Var* and *SD Dep Var* values correspond to non-producer municipalities before the reform. Notice that all regressions include period fixed effects. Robust standard errors are clustered at the municipality level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Loss of producer municipalities

| | (1) | (2) | (3) | (4) |
	TotalRoyalties		Ln(TotalRoyalties + 1)	
$Royalties_i \times PostReform_t$	-1.708***	-1.683***	-0.089***	-0.084***
	(0.377)	(0.375)	(0.019)	(0.018)
Observations	14,411	14,411	14,411	14,411
R-squared	0.721	0.742	0.874	0.885
Mayor Controls	No	Yes	No	Yes
Municipal Controls	No	Yes	No	Yes
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Avg Dep Var.(producers)	1.259	1.259	0.279	0.279
SD Dep Var.(producers)	5.648	5.648	0.692	0.692

Notes: This table illustrates the differential effect of the reform on the total royalties received by the municipalities. The table reports coefficients obtained from the estimation of equation (1) presented in Section 4. The sample includes 1,075 municipalities and the period of study is 2000 - 2015. $PostReform_t$ is an indicator variable taking value 1 for the period of government after the reform, and 0 otherwise. *TotalRoyalties* is the total royalties received by a municipality in each year. *Ln(TotalRoyalties + 1)* is a logarithmic transformation of *TotalRoyalties*. *Mayor controls* included are sex, political inclination, illegal registration of ID, and previous political and electoral experience. *Municipal controls* included are rurality, area, altitude, distance to capital of the department, distance to capital of Colombia, NBI indicator, Fiscal Performance Index, GINI Index and population. *Avg Dep Var* and *SD Dep Var* values correspond to producer municipalities before the reform. Notice that all regressions include municipality and year fixed effects. Robust standard errors are clustered at the municipality level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Bad control

	(1) Prosecutions	(2) Guilty	(3) Dismissed
$Royalties_i \times PostReform_t$	-0.030*	-0.041***	-0.032***
	(0.015)	(0.008)	(0.010)
$TotalRoyalties_{it}$	-0.053	-0.096	-0.035
	(0.063)	(0.067)	(0.071)
Observations	4,300	4,300	4,300
R-squared	0.378	0.354	0.314
Mayor Controls	Yes	Yes	Yes
Municipal Controls	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Period FE	Yes	Yes	Yes
Avg Dep Var.(non - producers)	0.255	0.149	0.086
SD Dep Var.(non - producers)	0.780	0.519	0.375

Notes: This table illustrates the differential effect of the reform on the corrupt behavior of local politicians. The table reports coefficients obtained from the estimation of equation (1) presented in Section 4. The sample includes 1,075 municipalities and the period of study is 2000 - 2015. $PostReform_t$ is an indicator variable taking value 1 for the period of government after the reform, and 0 otherwise. $Royalties_i$ is the standard deviation of the average direct royalties received during the period 1996-1999 for municipality i. $TotalRoyalties_{it}$ is the total royalties received by municipality i in government period t. *Prosecutions* is an indicator variable taking value 1 if the mayor or some high-level government had an open disciplinary prosecution after the incumbency period, and 0 otherwise. *Guilty* is an indicator variable taking value 1 if the mayor or some high-level government was found guilty after the start of the incumbency period, and 0 otherwise. *Dismissed* is an indicator variable taking value 1 if the mayor or some high-level government was dismissed after the start of the incumbency period, and 0 otherwise. *Mayor controls* included are sex, political inclination, illegal registration of ID, and previous political and electoral experience. *Municipal controls* included are rurality, area, altitude, distance to capital of the department, distance to capital of Colombia, NBI indicator, Fiscal Performance Index, GINI Index and population. *Avg Dep Var* and *SD Dep Var* values correspond to non-producer municipalities before the reform. Notice that all regressions include municipality and period fixed effects. Robust standard errors are clustered at the municipality level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Public audits of producer municipalities

	(1)	(2)
	Number of audits	
$PostReform_t$	-0.026	-0.018
	(0.026)	(0.028)
Observations	1,567	1,484
R-squared	0.374	0.258
Mayor Controls	No	Yes
Municipal Controls	No	Yes
Municipality FE	Yes	No
Avg Dep Var.	0.096	0.096
SD Dep Var.	0.536	0.536

Notes: Notes: This table illustrates the changes in the number of public audits of producer municipalities after the reform. The sample includes 397 municipalities and the period of study is 2004 - 2015. $PostReform_t$ is an indicator variable taking value 1 for the period of government after the reform and it's the same for all municipalities, and 0 otherwise. *Mayor controls* included are sex, political inclination, illegal registration of ID, and previous political and electoral experience. *Municipal controls* included are rurality, area, altitude, distance to capital of the department, distance to capital of Colombia, NBI indicator, Fiscal Performance Index, GINI Index and population. *Avg Dep Var* and *SD Dep Var* values correspond to producer municipalities before the reform. Robust standard errors are clustered at the municipality level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Corruption and conflict

	(1) Prosecutions	(2) Guilty	(3) Dismissed
Panel A - before			
$Royalties_i \times ArmedGroups_i$	0.036***	0.025***	0.026**
	(0.009)	(0.008)	(0.013)
$Royalties_i$	0.009*	0.015***	0.011
	(0.005)	(0.004)	(0.008)
$ArmedGroups_i$	0.032**	0.025**	0.006
	(0.014)	(0.013)	(0.010)
Observations	3,225	3,225	3,225
R-squared	0.117	0.076	0.051
Mayor Controls	Yes	Yes	Yes
Municipal Controls	Yes	Yes	Yes
Period FE	Yes	Yes	Yes
Panel B - after			
$Royalties_i \times ArmedGroups_i \times PostReform_t$	-0.060**	-0.027***	-0.027**
	(0.027)	(0.009)	(0.013)
$Royalties_i \times PostReform_t$	0.014	-0.016***	-0.012
	(0.024)	(0.005)	(0.008)
$ArmedGroups_i \times PostReform_t$	-0.017	-0.009	-0.006
	(0.023)	(0.015)	(0.012)
Observations	4,300	4,300	4,300
R-squared	0.379	0.354	0.315
Mayor Controls	Yes	Yes	Yes
Municipal Controls	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Period FE	Yes	Yes	Yes
Avg Dep Var.(non - producers)	0.156	0.106	0.065
SD Dep Var.(non - producers)	0.363	0.307	0.246

Notes: This table illustrates the heterogeneous effect of the transparency component of the reform on local politicians and rent-seekers. The table reports coefficients obtained from the estimation of equation (3) presented in Section 4. The sample includes 1,075 municipalities and the period of study is 2000 - 2015. $PostReform_t$ is an indicator variable taking value 1 for the period of government after the reform, and 0 otherwise. $Royalties_i$ is the standard deviation of the average direct royalties received during the period 1996-1999 for each municipality i. $ArmedGroups_i$ is an indicator variable taking value 1 if at least one attack was attributed to an armed group (FARC, ELN or paramilitaries) in the period 2006-2010, as a measure of the presence of this groups. *Prosecutions* is an indicator variable taking value 1 if the mayor or some high-level government had an open disciplinary prosecution after the incumbency period, and 0 otherwise. *Guilty* is an indicator variable taking value 1 if the mayor or some high-level government was found guilty after the start of the incumbency period, and 0 otherwise. *Dismissed* is an indicator variable taking value 1 if the mayor or some high-level government was dismissed after the start of the incumbency period, and 0 otherwise. *Mayor controls* included are sex, political inclination, illegal registration of ID, and previous political and electoral experience. *Municipal controls* included are rurality, area, altitude, distance to capital of the department, distance to capital of Colombia, NBI indicator, Fiscal Performance Index, GINI Index and population. *Avg Dep Var* and *SD Dep Var* values correspond to non-producer municipalities before the reform. Notice that all regressions include municipality and period fixed effects. Robust standard errors are clustered at the municipality level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 10: Corruption and accountability

	(1)	(2)
	IGA Accountability	
$Royalties_i \times PostReform_t$	-0.460	-0.244
	(1.069)	(1.032)
Observations	4,300	4,300
R-squared	0.444	0.466
Mayor Controls	No	Yes
Municipal Controls	No	Yes
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Avg Dep Var.(non-producers)	43.433	43.433
SD Dep Var.(non-producers)	30.683	30.683

Notes: This table illustrates the differential effect of the reform on the strengthening of accountability mechanisms. The table reports coefficients obtained from the estimation of equation (1) presented in Section 4. The sample includes 1,075 municipalities and the period of study is 2010 - 2013. $PostReform_t$ is an indicator variable taking value 1 for the years after the reform, and 0 otherwise. $Royalties_i$ is the standard deviation of the average direct royalties received during the period 1996-1999 for municipality i. *IGA Accountability* is an indicator that takes values between 0 and 100. *Mayor controls* included are sex, political inclination, illegal registration of ID, and previous political and electoral experience. *Municipal controls* included are rurality, area, altitude, distance to capital of the department, distance to capital of Colombia, NBI indicator, Fiscal Performance Index, GINI Index and population. *Avg Dep Var* and *SD Dep Var* values correspond to non-producer municipalities before the reform. Notice that all regressions include municipality and year effects. Robust standard errors are clustered at the municipality level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

8 Appendix

Figure A1: Illegal armed group presence

Presence of armed groups

2006-2010
☐ No presence
▧ Presence
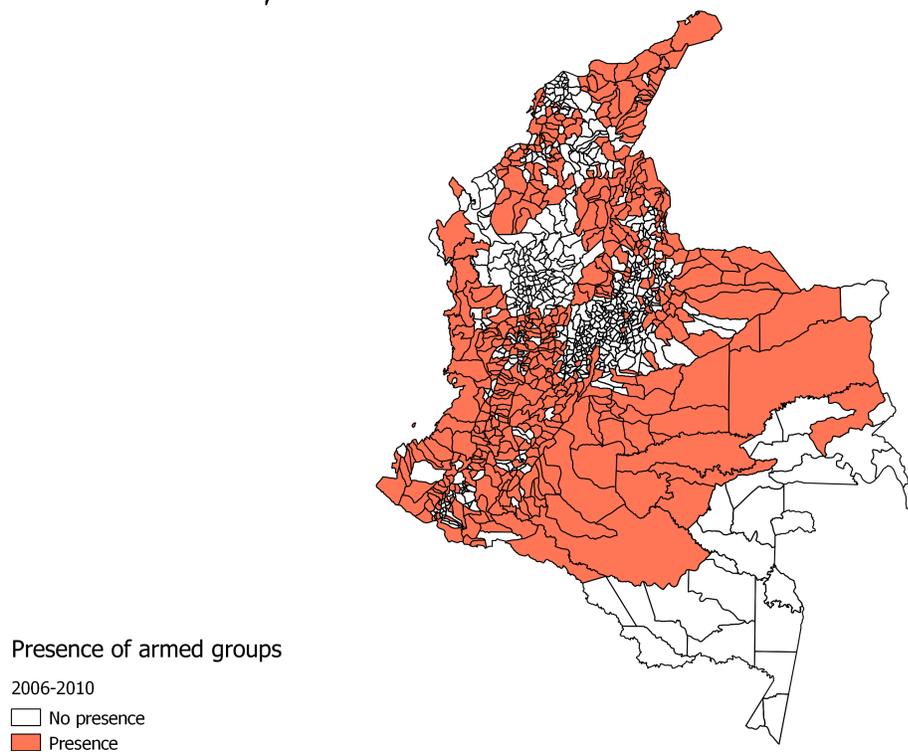
Notes: The map is our measure of the presence of armed groups between 2006 and 2010.

Figure A2: Permutation test
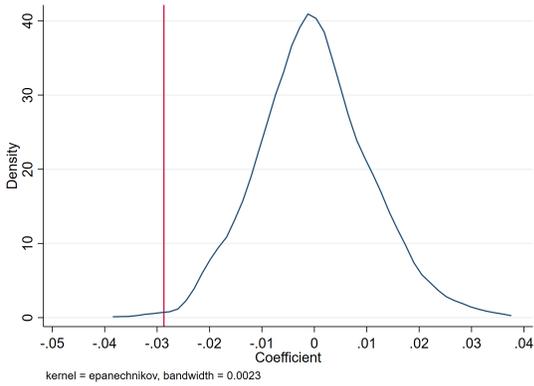
(a) Prosecutions
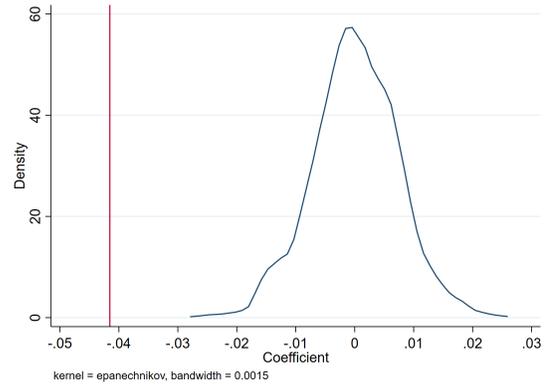
(b) Guilty
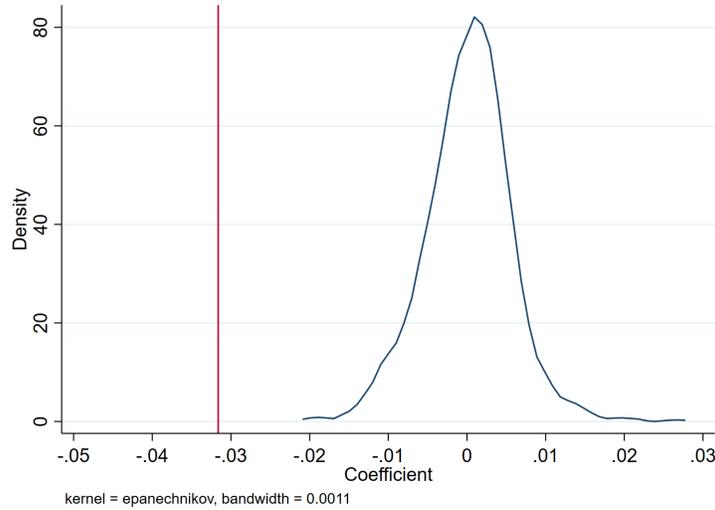
(c) Dismissed

Notes: This figure presents the distribution of our coefficient of interest $Royalties_i \times PostReform_t$ from equation (1), where we randomly assign the mean of royalties perceived between 1996 and 1999 to each municipality based on the actual values observed. We repeat this procedure 1000 times and plot the distribution of the estimates. We include the actual estimate in the vertical line. The probability of observing a similar effect to the ones presented in Table 4 is below 1% in all outcomes. Panel A corresponds to *Prosecutions* outcome, Panel B to *Guilty* and Panel C to *Dimissed*.

Table A1: Effects of the reform on corruption, extensive margin

	(1) N Prosecutions	(2) N Guilty	(3) N Dismissed
Panel A - without controls			
$Royalties_i \times PostReform_t$	-0.079*	-0.063***	-0.040***
	(0.047)	(0.021)	(0.014)
Mayor Controls	No	No	No
Municipal Controls	No	No	No
R-squared	0.360	0.315	0.288
Panel B			
$Royalties_i \times PostReform_t$	-0.062	-0.052***	-0.038***
	(0.045)	(0.020)	(0.014)
Observations	4,300	4,300	4,300
R-squared	0.396	0.345	0.314
Mayor Controls	Yes	Yes	Yes
Municipal Controls	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Period FE	Yes	Yes	Yes
Avg Dep Var.(non - producers)	0.255	0.149	0.086
SD Dep Var.(non - producers)	0.780	0.519	0.375

Notes: This table illustrates the differential effect of the reform on the corrupt behavior of local politicians. The table reports coefficients obtained from the estimation of equation (1) presented in Section 4. The sample includes 1,075 municipalities and the period of study is 2000 - 2015. $Post_{(2011)t}$ is an indicator variable taking value 1 for the period of government after the reform, and 0 otherwise. $Royalties_i$ is the standard deviation of the average direct royalties received during the period 1996-1999 for each municipality i. *N Prosecutions* is the number of disciplinary prosecutions of the mayor or high-level staff after starting the incumbency period. *N Guilty* is the number of times a mayor or or some high-level staff was found guilty after the incumbency period. *N Dismissed* is the number of times a mayor or or some high-level staff was dismissed after the incumbency period. *Mayor controls* included are sex, political inclination, illegal registration of ID, and previous political and electoral experience. *Municipal controls* included are rurality, area, altitude, distance to capital of the department, distance to capital of Colombia, NBI indicator, Fiscal Performance Index, GINI Index and population. *Avg Dep Var* and *SD Dep Var* values correspond to non-producer municipalities before the reform. Notice that all regressions include municipality and period fixed effects. Robust standard errors are clustered at the municipality level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A2: Spatial correlation correction

	(1) Prosecutions	(2) Guilty	(3) Dismissed
$Royalties_i \times PostReform_t$	-0.029**	-0.042***	-0.032***
	(0.014)	(0.011)	(0.010)
Observations	4,300	4,300	4,300
R-squared	0.002	0.006	0.005
Municipality FE	Yes	Yes	Yes
Period FE	Yes	Yes	Yes
Avg Dep Var.(non - producers)	0.156	0.106	0.065
SD Dep Var.(non - producers)	0.363	0.307	0.246

Notes: This table illustrates the differential effect of the reform on the corrupt behavior of local politicians. The table reports coefficients obtained from the estimation of equation (1) presented in Section 4. The sample includes 1,075 municipalities and the period of study is 2000 - 2015. $Post_{(2011)t}$ is an indicator variable taking value 1 for the period of government after the reform, and 0 otherwise. $Royalties_i$ is the standard deviation of the average direct royalties received during the period 1996-1999 for each municipality i. *Prosecutions* is an indicator variable taking value 1 if the mayor or some high-level government had an open disciplinary prosecution after the incumbency period, and 0 otherwise. *Guilty* is an indicator variable taking value 1 if the mayor or some high-level government was found guilty after the start of the incumbency period, and 0 otherwise. *Dismissed* is an indicator variable taking value 1 if the mayor or some high-level government was dismissed after the start of the incumbency period, and 0 otherwise. *Avg Dep Var* and *SD Dep Var* values correspond to non-producer municipalities before the reform. Notice that all regressions include municipality and period fixed effects. Standard errors in parentheses control for spatial and first-order time correlation (see Conley (1999) and Conley (2016)). We allow spatial correlation to extend to up to 420 km from each municipality's centroid to ensure that each municipality has at least one neighbor. *** p<0.01, ** p<0.05, * p<0.1.

Table A3: Endogenous Concerns

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Prosecutions		Guilty		Dismissed	
	FS	OLS	2SLS	OLS	2SLS	OLS	2SLS
$POil \times OilProd_i \times PostReform_t$	0.621***						
	(0.033)						
$PGold \times GoldSuitability_i \times PostReform_t$	-0.005						
	(0.004)						
$Royalties_i \times PostReform_t$		-0.026*	-0.059***	-0.035***	-0.050***	-0.030***	-0.055***
		(0.015)	(0.008)	(0.008)	(0.004)	(0.009)	(0.004)
Observations	4,300	4,300	4,300	4,300	4,300	4,300	4,300
R-squared		0.378	0.040	0.353	0.042	0.314	0.030
Mayor Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap rk Wald F statistic			194.6		194.6		194.6
Avg Dep Var.(non - producers)		0.156	0.156	0.106	0.106	0.065	0.065
SD Dep Var.(non - producers)		0.363	0.363	0.307	0.307	0.246	0.246

Notes: This table illustrates the differential effect of the reform on the corrupt behavior of local politicians. The table reports coefficients obtained from the estimation of equation (4) for the reduce form and in equation (1) for the 2SLS, presented in Sections 4 and 5 respectively. The sample includes 1,075 municipalities and the period of study is 2000 - 2015. $PostReform_t$ is an indicator variable taking value 1 for the period of government after the reform, and 0 otherwise. $Royalties_i$ is the standard deviation of the average direct royalties received during the period 1996-1999 for each municipality. *Prosecutions* is an indicator variable taking value 1 if the mayor or some high-level government had an open disciplinary prosecution after the incumbency period, and 0 otherwise. *Guilty* is an indicator variable taking value 1 if the mayor or some high-level government was found guilty after the start of the incumbency period, and 0 otherwise. *Dismissed* is an indicator variable taking value 1 if the mayor or some high-level government was dismissed after the start of the incumbency period, and 0 otherwise. $OilProd_i$ is the oil production in 1988 in the municipality i. $GoldSuitability_i$ is the suitability of gold constructed by Idrobo et al. (2014) in the municipality i. *POil* is the monthly average price of WTI oil in 1988. *PGold* is the monthly average international price of gold in 1988. *Mayor controls* included are sex, political inclination, illegal registration of ID, and previous political and electoral experience. *Municipal controls* included are rurality, area, altitude, distance to capital of the department, distance to capital of Colombia, NBI indicator, Fiscal Performance Index, GINI Index and population. *Avg Dep Var* and *SD Dep Var* values correspond to non-producer municipalities before the reform. Notice that all regressions include municipality and period fixed effects. Robust standard errors are clustered at the municipality level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A4: LASSO estimation

	(1) Prosecutions	(2) Guilty	(3) Dismissed
Panel A - before reform			
$Royalties_i$	0.048***	0.042***	0.032***
	(0.009)	(0.007)	(0.009)
Observations	3,225	3,225	3,225
R-squared	0.094	0.050	0.029
LASSO Controls	Yes	Yes	Yes
Period FE	Yes	Yes	Yes
Panel B - after reform			
$Royalties_i \times PostReform_t$	-0.031**	-0.036***	-0.028***
	(0.016)	(0.008)	(0.010)
Observations	4,300	4,300	4,300
R-squared	0.364	0.336	0.299
LASSO Controls	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Period FE	Yes	Yes	Yes
Avg Dep Var.(non - producers)	0.255	0.149	0.086
SD Dep Var.(non - producers)	0.780	0.519	0.375

Notes: This table illustrates the differential effect of the reform on the corrupt behavior of local politicians. The table reports coefficients obtained from the estimation of equation (1) presented in Section 4. The sample includes 1,075 municipalities and the period of study is 2000 - 2015. $PostReform_t$ is an indicator variable taking value 1 for the period of government after the reform, and 0 otherwise. $Royalties_i$ is the standard deviation of the average direct royalties received during the period 1996-1999 for each municipality i. *Prosecutions* is an indicator variable taking value 1 if the mayor or some high-level government had an open disciplinary prosecution after the incumbency period, and 0 otherwise. *Guilty* is an indicator variable taking value 1 if the mayor or some high-level government was found guilty after the start of the incumbency period, and 0 otherwise. *Dismissed* is an indicator variable taking value 1 if the mayor or some high-level government was dismissed after the start of the incumbency period, and 0 otherwise. *LASSO controls* included were selected by LASSO for each dependent variable. *Avg Dep Var* and *SD Dep Var* values correspond to non-producer municipalities before the reform. Notice that all regressions include municipality and period fixed effects. Robust standard errors are clustered at the municipality level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A5: Effect of the reform in the optimal sub-sample

	(1) Prosecutions	(2) Guilty	(3) Dismissed
$Royalties_i \times PostReform_t$	-0.020*	-0.020***	-0.022***
	(0.010)	(0.007)	(0.005)
Observations	3,432	3,432	3,432
R-squared	0.345	0.326	0.296
Municipality FE	Yes	Yes	Yes
Period FE	Yes	Yes	Yes
Avg Dep Var.(non - producers)	0.132	0.081	0.045
SD Dep Var.(non - producers)	0.339	0.273	0.208

Notes: This table illustrates the differential effect of the reform on the corrupt behavior of local politicians. The table reports coefficients obtained from the estimation of equation (1) presented in Section 4. The sample includes 858 municipalities and the period of study is 2000 - 2015. $Post_{(2011)t}$ is an indicator variable taking value 1 for the period of government after the reform, and 0 otherwise. $Royalties_i$ is the standard deviation of the average direct royalties received during the period 1996-1999 for each municipality i. *Prosecutions* is an indicator variable taking value 1 if the mayor or some high-level government had an open disciplinary prosecution after the incumbency period, and 0 otherwise. *Guilty* is an indicator variable taking value 1 if the mayor or some high-level government was found guilty after the start of the incumbency period, and 0 otherwise. *Dismissed* is an indicator variable taking value 1 if the mayor or some high-level government was dismissed after the start of the incumbency period, and 0 otherwise. *Avg Dep Var* and *SD Dep Var* values correspond to non-producer municipalities before the reform. Notice that all regressions include municipality and period fixed effects. Robust standard errors are clustered at the municipality level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Corruption and conflict before the reform, by armed group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Paramilitaries			FARC			ELN	
	Prosecutions	Guilty	Dismissed	Prosecutions	Guilty	Dismissed	Prosecutions	Guilty	Dismissed
$Royalties_i \times Z_{gi}$	0.031***	0.026***	0.029**	0.037***	0.031***	0.044***	0.006	0.009	0.026**
	(0.010)	(0.009)	(0.013)	(0.011)	(0.008)	(0.009)	(0.014)	(0.012)	(0.013)
$Royalties_i$	0.012**	0.014***	0.009	0.016*	0.018***	0.009	0.030***	0.028***	0.019**
	(0.006)	(0.004)	(0.008)	(0.009)	(0.005)	(0.007)	(0.011)	(0.008)	(0.009)
Z_{gi}	0.049***	0.040***	0.015	0.032	0.045**	0.029*	0.052***	0.034**	0.019*
	(0.015)	(0.013)	(0.010)	(0.022)	(0.020)	(0.015)	(0.016)	(0.015)	(0.011)
Observations	3,225	3,225	3,225	3,225	3,225	3,225	3,225	3,225	3,225
R-squared	0.117	0.076	0.052	0.119	0.078	0.057	0.114	0.075	0.053
Mayor Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var.(non - producers)	0.156	0.106	0.065	0.156	0.106	0.065	0.156	0.106	0.065
SD Dep Var.(non - producers)	0.363	0.307	0.246	0.363	0.307	0.246	0.363	0.307	0.246

Notes: This table illustrates the heterogeneous effect of the transparency component of the reform on local politicians and rent-seekers. The table reports coefficients obtained from the estimation of a pre-reform version of equation (3) presented in Section 4. The sample includes 1,075 municipalities and the period of study is 2000 - 2011. $Royalties_i$ is the standard deviation of the average direct royalties received during the period 1996-1999 for each municipality. Z_{gi} is an indicator variable taking value 1 if at least one attack was attributed to group g in the period 2006-2010 in the municipality i, and 0 otherwise. *Prosecutions* is an indicator variable taking value 1 if the mayor or some high-level government had an open disciplinary prosecution after the incumbency period, and 0 otherwise. *Guilty* is an indicator variable taking value 1 if the mayor or some high-level government was found guilty after the start of the incumbency period, and 0 otherwise. *Dismissed* is an indicator variable taking value 1 if the mayor or some high-level government was dismissed after the start of the incumbency period, and 0 otherwise. *Mayor controls* included are sex, political inclination, illegal registration of ID, and previous political and electoral experience. *Municipal controls* included are rurality, area, altitude, distance to capital of the department, distance to capital of Colombia, NBI indicator, Fiscal Performance Index, GINI Index and population. *Avg Dep Var* and *SD Dep Var* values correspond to non-producer municipalities before the reform. Notice that all regressions include period fixed effects. Robust standard errors are clustered at the municipality level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A7: Corruption and conflict, by armed group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Paramilitaries			FARC			ELN	
	Prosecutions	Guilty	Dismissed	Prosecutions	Guilty	Dismissed	Prosecutions	Guilty	Dismissed
$Royalties_i \times Z_{gi} \times PostReform_t$	-0.054**	-0.026***	-0.028**	-0.071***	-0.032***	-0.042***	-0.036	-0.007	-0.025*
	(0.026)	(0.009)	(0.013)	(0.019)	(0.010)	(0.009)	(0.022)	(0.013)	(0.013)
$Royalties_i \times PostReform_t$	0.010	-0.017***	-0.010	0.005	-0.021***	-0.011*	-0.012	-0.031***	-0.020**
	(0.023)	(0.005)	(0.008)	(0.016)	(0.007)	(0.006)	(0.019)	(0.009)	(0.009)
$Z_{gi} \times PostReform_t$	-0.035	-0.019	-0.021	-0.022	-0.009	-0.007	-0.060*	-0.055**	-0.039**
	(0.026)	(0.018)	(0.014)	(0.024)	(0.017)	(0.012)	(0.034)	(0.022)	(0.016)
Observations	4,300	4,300	4,300	4,300	4,300	4,300	4,300	4,300	4,300
R-squared	0.379	0.354	0.315	0.380	0.354	0.316	0.379	0.354	0.315
Mayor Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var.(non - producers)	0.156	0.106	0.065	0.156	0.106	0.065	0.156	0.106	0.065
SD Dep Var.(non - producers)	0.363	0.307	0.246	0.363	0.307	0.246	0.363	0.307	0.246

Notes: This table illustrates the heterogeneous effect of the transparency component of the reform on local politicians and rent-seekers. The table reports coefficients obtained from the estimation of equation (3) presented in Section 4. The sample includes 1,075 municipalities and the period of study is 2000 - 2015. $Post_{(2011)t}$ is an indicator variable taking value 1 for the period of government after the reform, and 0 otherwise. $Royalties_i$ is the standard deviation of the average direct royalties received during the period 1996-1999 for each municipality. Z_{gi} is an indicator variable taking value 1 if at least one attack was attributed to group g in the period 2006-2010 in the municipality i, and 0 otherwise. *Prosecutions* is an indicator variable taking value 1 if the mayor or some high-level government had an open disciplinary prosecution after the incumbency period, and 0 otherwise. *Guilty* is an indicator variable taking value 1 if the mayor or some high-level government was found guilty after the start of the incumbency period, and 0 otherwise. *Dismissed* is an indicator variable taking value 1 if the mayor or some high-level government was dismissed after the start of the incumbency period, and 0 otherwise. *Mayor controls* included are sex, political inclination, illegal registration of ID, and previous political and electoral experience. *Municipal controls* included are rurality, area, altitude, distance to capital of the department, distance to capital of Colombia, NBI indicator, Fiscal Performance Index, GINI Index and population. *Avg Dep Var* and *SD Dep Var* values correspond to non-producer municipalities before the reform. Notice that all regressions include municipality and period fixed effects. Robust standard errors are clustered at the municipality level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.