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Blessing a Curse? Institutional Reform and Resource
Booms in Colombia

Jorge Gallego

Stanislao Maldonado

Lorena Trujillo

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Blessing a Curse?

Institutional Reform and Resource Booms in Colombia*

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Jorge Gallego Stanislao Maldonado
Universidad del Rosario Universidad del Rosario

Lorena Trujillo
Departamento Nacional de Planeación

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Abstract

Is it possible to revert the resource curse through institutional reform? Evidence suggests that there is a negative relationship between abundance of natural resources and economic growth, political stability, democracy, and peace. However, evidence illustrating how institutional reform can revert this situation is scarce. In this paper, we exploit an institutional reform that took place in Colombia in 2011. We evaluate the effects of the reform to the royalties system, that modified the allocation rule of these rents but also introduced important changes in terms of control and accountability, on the living standards of Colombian households. We instrument municipality-level allocations of royalties using international variations in the price of oil, and we find that the reform had important effects on several household welfare indicators. We find positive impacts on important dimensions, such as poverty, income, employment, housing conditions, health, and education, among others. Results are mixed or null in other areas, such as formality or employment in the service sector. We test for different channels explaining these effects, which include theories of state capacity, competition for resources, and increased control and accountability. Our evidence supports the state capacity mechanism.

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1. INTRODUCTION

Recent years have witnessed a renewed interest in understanding the role of natural resources in economic development.¹ The so-called “resource curse” literature (Sachs and Warner (1995), Karl (1997) and Ross (1999), among others) has been enriched by better approaches dealing with causal effects in order to explain this paradoxical phenomenon. These approaches also consider the use of subnational variation in order to account for sources of endogeneity that were prevalent in most of the existing papers based on cross-national variation (Caselli and Micheals (2013), Brollo et al. (2013), Dube and Vargas (2013)). It has been accompanied with relevant theoretical work that allows a clear understanding of the political and economic mechanisms that link resource abundance to poor economic and political outcomes (Mehlun et al. (2006), Robinson et al. (2006), Caselli and Cunningham (2009), Caselli (2015)). This new literature represents an important progress in one of the most controversial topics in the development literature.²

One of the critical components of this new scholarship relates to the role of economic and political institutions in explaining the resource curse. On the economic side, Mehlun et al. (2006) document the existence of different growth experiences in resource-rich countries and explain this difference using a theoretical model in which the quality of institutions matters.³ On the political side, Robinson et al. (2006) and Robinson et al. (2014) propose a model in which the effects of resource booms on income depend on whether the incumbents are able to buy off electoral support via clientelism, which in turn depends on institutional quality. Therefore, this theoretical scholarship shows that, when institutions are weak, economic agents are prone to rent-seeking and politicians are unconstrained in their intentions of

¹The literature about the resource curse is very large. Deacon (2011), Van der Ploeg (2011), Venables (2016), Van der Ploeg and Poelhekke (2017), and Badeeb et al. (2017) offer excellent overviews.

²Results from the recent scholarship that exploits within country variation show a more nuanced view regarding the effects of natural resources on development outcomes. Studies that emphasize the effects of natural resource exploitation typically finds positive effects regardless resources are extracted in developed or underdeveloped countries (Allcott and Keniston (2017) and Aragon and Rud (2013)). Other studies for developing countries have found a coexistence of positive results with negative externalities, for instance in the case of increases of employment opportunities that negatively affects human capital accumulation in children (Santos (2018)). On the other hand, studies that have analyzed the role of natural resource rents have shown mixed results. In some cases, the effects on well-being and public good provision are very modest or null (Caselli and Cunningham (2009)), but some evidence on non-monotonic effects have been found (Maldonado (2017)). In sum, the evidence about a natural resource curse is mixed. A similar evaluation applies to the cross-country evidence (Van der Ploeg and Poelhekke (2017)).

³To explain this result, they propose a theoretical model in which resource rents are distributed under different institutional arrangements depending on whether production and rent-seeking are complementary or competing activities. When these activities are competing, rents can be wasted in inefficient activities (due to corruption and weak rule of law), being the abundance of natural resources unable to attract entrepreneurial inputs, which in turn has a negative impact on growth.

remaining in power, causing a negative effect on both growth and well-being. On the other hand, resource-rich places with good institutions do not experience a resource curse.⁴

Because institutions tend to change slowly over time, a consequence of this scholarship is a pessimistic view regarding the ability of resource-rich countries with poor institutional quality to overcome the resource curse. However, it is not obvious whether this is necessarily the case because institutional change is not always slow (Roland, 2004). If large scale institutional reforms or particular critical junctures are needed to develop good quality institutions consistent with the transformation of natural resource wealth into citizens' well-being, then this pessimistic view would be valid. But, if similar outcomes can be obtained through specific policy changes or soft institutional reforms, then a more optimistic view can be defended.⁵ Unfortunately, this pessimistic view is so prevalent that it is not surprising the lack of interest among scholars in understanding whether alternative forms of institutional change can turn a curse into a blessing.

In this paper, we shed light on this issue by studying a reform of the royalties system in Colombia.⁶ This reform introduced a new scheme of incentives for the allocation of rents related with oil and minerals exploitation. It holds three main components. Firstly, competition was introduced as a mechanism to allocate public funds depending on the quality of public projects. In contrast to the previous institutional arrangements, municipalities are now required to develop proposals about specific interventions, which are evaluated based on their technical content by an external committee. Secondly, accountability mechanisms were incorporated via the introduction of monitoring and evaluation systems into the project cycles, so citizens now have access to better information regarding the use of fiscal resources. Finally, access to royalties was extended beyond producer municipalities,

⁴The predominant approach in theoretical and empirical studies about the institutional dimension of the resource curse is to treat institutions as exogenous. One exception is Wiens (2014), who proposed a theoretical model in which institutions are endogenous to resource abundance. In his model, the timing of resource dependence is critical to explain institutional quality in resource-rich countries. When institutions that constraint rulers are in place before resource dependence, resource abundance is a blessing because politicians have incentives in using resource rents to increase citizens' well-being. On the other hand, when poor institutional quality before resource dependence, resource abundance reinforce poor institutional quality, creating a resource curse. See Wiens (2014) for details.

⁵See, for instance, the discussion between "fast-moving" and "slow-moving" institutions in Roland (2004). According to the author, culture is an example of the latter whereas political institutions can be an example of the former because they can, in some occasions, change overnight. Our focus on the change of specific allocation rules within a complex fiscal system is understood as an example of change in a fast-moving institution.

⁶Indonesia, Ghana, Peru, Brazil, Bolivia, Canada, and Australia, to mention just a few countries, have implemented mechanisms that share some of the taxes and royalties paid by extractive companies with subnational governments. Most of these allocation rules are based on fixed proportions over taxation or production. See Brosio and (eds) (2012) for an overview.

contributing to a more equitable allocation of public funds across municipalities in Colombia.

We explore the impact of this reform on citizens' well-being. To do so, we exploit spatial and time variation in rents allocation across municipalities in Colombia, before and after the reform. This variation is caused by the change of the rules concerning the allocation of rents due to the reform along with variation in oil prices and quantities. Regarding the first source of variation, it is important to note that before the reform, the royalties system privileged producer areas, which received 72% of the allocated funds. After the reform, producer areas were granted 10% of the distributed royalties with the possibility of obtaining extra fiscal resources (up to 30%) via the competitive mechanism for fund allocation introduced by the reform. With respect to the second source of variation, we study the effects of an extraordinary increase in oil prices that occurred during the period under analysis due to the commodity boom associated with the Chinese industrialization process.

We construct a unique dataset of oil production, transfers from the central government and living standards for the period 1997-2016. To claim causality, our identification strategy follows the previous literature using an instrumental variable approach where allocated rents are instrumented using international oil prices interacted with a measure of oil reserves (a proxy of oil production potential). This approach is complemented by a discussion of the endogeneity of the reform. This is an important piece of the analysis since the reform could have been implemented with the goal of redistributing rents in favor of groups aligned with the political interests of those who designed it in the first place. This exercise is complemented by several robustness checks including controlling for migration, other sources of transfers from the Central government, and whether the results are driven by producer municipalities. We also evaluate whether the results are sensitive to the weak instrument problem by estimating alternative econometric models that are robust to such a problem. Finally, to address the multiplicity of outcomes, we perform inference by controlling for the false discovery rate following [Benjamini and Hochberg \(1995\)](#).

We find evidence of positive impacts of the reform on living standards. After the introduction of the reform, we find a clear drop in various poverty measures and an increase in income and employment. In the case of monthly income, for instance, we document an increase of COP130,000 for every additional COP100,000 in royalties per-capita. We also document important reductions in multidimensional and subjective indicators of poverty, among other measures of well-being.

We map this improvement in well-being indicators to the provision of public goods and labor market externalities associated with the reform. The reform induced changes in municipalities' investment patterns, which were forced to compete based on project quality. These projects, typically oriented to improving public infrastructure, had an important effect on the quality of public services. After the reform was implemented, access to water, water quality and connection to an aqueduct improved for households. Municipalities also invested in improving roads and expanding the access to technologies of information and communication, and we find important effects on these dimensions as well.

We find similar results for social services such as health and education. Access to the health system improved, and the likelihood of being sick decreased. In education, we find increases in enrollment among those in school age, as well as better educational outcomes among adults.

We also document labor market effects that suggest that the reform affected household economic opportunities beyond public good provision. After the reform, the positive effect of royalties on employment is higher. Results are mixed in terms of job formality, as we find effects on the probability of having a work contract but no effects on other traditional measures of job formality. Also, there is suggestive evidence of crowding-out effects concerning employment in the agricultural sector as more people start working in the manufacturing sector. No effects whatsoever are found in the service sector, despite the fact that investments in science, technology, and innovation are one of the main pillars of the reform.

Taking this evidence together, we find that these results are consistent with the idea that soft institutional reforms can manage to turn a resource curse into a blessing. This is a critical addition to the resource curse literature that has implicitly emphasized a more negative view on this issue. To the best of our knowledge, this is one of the first papers to provide credible evidence about the feasibility of implementing soft institutional reforms to avoid the resource curse.

This paper also contributes to the existing debate about the political economy of resource booms by exploiting subnational variation. One strand of the literature has explored the impact of resource booms on the behavior of politicians with respect to electoral outcomes such as reelection and political competition (Monteiro and Ferraz (2012), and Maldonado (2017), Carreri and Dube (2017)). Other scholars have explored the impact of resource booms on citizens' well-being via public good provision (Caselli and Micheals (2013), Loayza et al. (2013), and Maldonado (2017)) and demand for local inputs (Aragon and Rud, 2013).

Other dimensions explored by researchers include corruption (Brollo et al. (2013), Maldonado (2011) and Vicente (2010)), politician quality (Brollo et al., 2013), conflict (Angrist and Kluger (2008), Dube and Vargas (2013)), and citizens' confidence in political institutions and democracy (Maldonado, 2015). As stated before, we are not aware of previous research regarding institutional reforms designed to overcome the perverse economic and political effects of resource booms, although research has been conducted on the specific role of institutions in explaining the resource curse. Scholars have emphasized the role of political regimes, lack of rule of law, poor quality bureaucracies, and limited fiscal transparency and discipline (Van der Ploeg (2011)), just to mention some of the dimensions that have attracted scholars' interest.

The rest of the paper is organized as follows. Section 2 provides some basic details about the institutional setting. Section 3 introduces the empirical strategy, and Section 4 describes the data. Section 5 presents the empirical results. Section 6 concludes the paper.

2. BACKGROUND

2.1. The Old System

The 1991 Political Constitution of Colombia establishes that royalties are monetary compensations for the exploitation of non-renewable natural resources within the country's territory. It also establishes that such compensations must benefit departments and municipalities where the exploitation activities take place, as well as river and seaports through which production from that exploitation is transported. Another portion of royalties might be allocated to local entities through the National Royalties Fund (FNR), specifically aimed at projects promoting mining, environmental preservation, and regional development.

Given these rules, between 1994 and 2011 producing departments received 49% of total royalties in Colombia, producing municipalities 23%, while port regions received 7% of these resources. The remaining 20% was distributed between the FNR and the National Pension of Territorial Entities Fund (FONPET), in order to fulfill pension liabilities among regions (Echeverry et al. (2011)).

Under these conditions, royalties were highly concentrated in a few departments, mainly oil producers Casanare, Meta, and Arauca. About half of the resources went to these departments, even though they represent less than 4% of the country's total population. Furthermore, municipal allocation of resources was not linked to economic outcomes—

such as poverty, drinking water coverage, literacy, or child mortality.⁷ Moreover, among the ten departments where most parts of the resources were concentrated, royalties went to municipalities without any consideration for population size or economic needs. In addition, the system did not encourage local governments to use resources efficiently or to improve service delivery. Corruption and inefficiency were the common denominators of these projects (Viloria (2005); Sanchez et al. (2005); Gamarra (2005); and Bonet (2007)).⁸

2.2. Institutional Change

This background motivated the creation of the General Royalties System (SGR) in 2011, according to President Santos' government, to promote equality, savings for the future, regional competitiveness, and good governance. In the words of the former ministry of finance, who was in charge of designing the reform, the goal of the new system is to “contribute to the local development of the country, prioritizing the poorest regions (...) and promoting the improvement of management capacities of local authorities” (Echeverry et al. (2011)).

Under the new rules, five funds constitute the backbone of the system: Savings and Stabilization Fund—FAE; Science, Technology, and Innovation Fund—FCTI; Regional Compensation Fund—FCR; Regional Development Fund—FDR; and the Pensions Fund—FONPET. The organization of the new system is depicted in Figure 2.⁹ Direct royalties —those that are assigned directly to producing municipalities—were not eliminated, but their share is now much lower compared to the old system. Perhaps the main novelty of the new institutional framework is the method used to allocate rents: Municipalities and departments present investment projects, with approval being conditional on their relevance, feasibility, sustainability, impact, articulation, and good governance.

Approved projects are chosen by the Management and Decision Collegiate Bodies (OCADs),¹⁰ newly created committees whose main functions are to assess, facilitate, approve, and prioritize the convenience and opportunity of funded projects. The maximum number of votes

⁷See, for instance, Perry and Olivera (2009) for an overview of these issues.

⁸Examples of mismanagement and corruption under the old royalty system have been documented by the media. For instance, in Casanare—one of the richest departments in Colombia—politicians have been using oil royalties to fund private businesses, like the case of governor Witman Herney. Another governor, Raul Florez, was removed from office after evidence of misuse of royalty funds in 3 contracts for US\$4.5 millions. Anecdotal evidence suggests that corruption under the old system was widespread.

⁹Overall, the distribution of resources across funds is as follows: Up to 30% to FAE, 10% to FONPET, 10% to FCTI, at least 24% to FCR, and at least 16% to FDR.

¹⁰Órganos Colegiados de Administración y Decisión, in Spanish.

in an OCAD is three and projects are approved following a majority rule, i.e. with two favorable votes. Its composition varies depending on the fund they administer, but in essence, they always have a member of each of the three levels of government: central, departmental, and municipal. Therefore, after the reform, local governments must plan projects and present them for approval, and the final decision is made by a democratic body composed of different agents.¹¹

The law also defines the criteria to be considered by each OCAD when assessing projects. Resources from the FDR and the FCR (projects with a regional impact), must be allocated to departments as a function of population size, poverty, and unemployment. In the case of municipality projects, royalties are prioritized to poorer and less developed places. Other resources, such as those coming from FONPET, are distributed according to municipal and departmental quotas, prioritizing those with larger pension liabilities, and again, larger population and higher levels of poverty. FCTI's resources are allocated among departments following the same shares they have for FCR and FDR. Finally, departments receive a share of the FAE's resources that coincide with the share they have of the rest of resources.

In sum, after 2011 the allocation of resources depends less on whether a municipality or department produces oil and minerals, and more on its economic characteristics and its ability to propose projects meeting the criteria described above. Figure 1 depicts the distribution of royalties across Colombian municipalities in 2011, just before the reform, and in 2016, some years after it was approved. Clearly, there is a big change in the way these resources are allocated, and in turn, our aim is to study the effects of this shift on households' living standards.

2.3. Monitoring and Evaluation

One of the major changes introduced by the reform is the set of mechanisms used by the government to monitor projects and prevent malfeasance. First, each public organization appointed by the OCAD is responsible for the execution of the project. Second, the Monitoring, Follow-up, Control and Evaluation System (SMSCE), affiliated to the National Planning Department, was created, with the goal of tracking projects and ensuring that resources are used efficiently and effectively. This agency collects, consolidates, and analyses

¹¹One potential concern is that the reform was implemented to target political allies of the national government. We discard the possibility of political targeting of the reform in a robustness check in the Appendix.

information from projects and takes preventive actions whenever they find irregularities in the execution of an intervention.

Third, traditional anti-corruption agencies, like the Office of the Comptroller General, keep monitoring these public investments, as they did before the reform. And fourth, several bottom-up mechanisms for accountability were created, including public audits, monitoring web-pages and public webcams, among others, in order to involve the communities in the monitoring process. Hence, this combination of top-down and bottom-up accountability represents a big shift in relation to the old system, in terms of strategies for fighting corruption and inefficiencies associated with royalties projects. However, whether these strategies have been effective or not is an empirical question, and we tackle it in the results section.

In sum, the institutional change we analyze in this paper is composed of at least three major shifts: i) All municipalities can participate in the funding process, independently of whether they are producers or not; ii) For this purpose, they must plan and present projects whose range varies along the spectrum of feasible public investments; and iii) Several new mechanisms for accountability were introduced, while the existent ones were greatly improved. Figure 3 depicts the timeline of the events associated to the reform as well as that of the data we will use in the analysis.

3. EMPIRICAL STRATEGY

One of the objectives of this study is to determine if the institutional reform that took place in Colombia, and that led to the creation of the General Royalties System, had a positive effect on households' welfare and deterred, in some way, the resource curse that motivated the reform. The empirical strategy we use compares the marginal effects of royalties on several household welfare indicators before and after 2011, year in which the institutional change was approved by the Colombian Congress. To make this comparison, we construct a pooled cross-sectional database from information contained in the Quality of Life Survey,¹² a household level survey carried out by the National Statistics Department in Colombia.

Several reasons justify using this source of information: first, as it will be described in the Data section, the survey was originally launched in 1997 and includes several pre and post-reform waves; second, as it is also clear from the description of the information, the survey gathers data on important household-level welfare dimensions, such as education, health,

¹²*Encuesta de Calidad de Vida* in Spanish. Section 4 provides the information regarding the sampling design, levels of coverage and sample sizes of this survey.

housing conditions, transportation, labor, income, poverty, among others. Finally, we are able to determine the municipality where each household lives, and consequently match individual-level characteristics with aggregate-level variables, including royalties transferred to municipalities before and after the reform.

Rents transferred by the central government to a given municipality might be endogenous, as several difficult-to-measure economic and institutional characteristics might both affect households' welfare and the exploitation of natural resources. For this reason, the basic models that we estimate in this paper correspond to Two-Stage Least Squares (2SLS) estimations of the following form:

$$y_{imt} = \alpha_m + \beta_t + \widehat{Royalties}_{mt}\delta_1 + (\widehat{Royalties}_{mt} \times \widehat{Post2011}_t)\delta_2 + \mathbf{X}_{imt}\phi + \mathbf{Z}_{mt}\eta + \varepsilon_{imt} \quad (1)$$

where y_{imt} is a welfare indicator for household i , that lives in municipality m , in year t . Several household-level indicators will be used as outcome variables, as it will become clear below; $Royalties_{mt}$ represents the royalties transfer, in hundred thousand Colombian Pesos (COP), to municipality m in year t ,¹³ $Post2011_t$ is a time dummy, indicating whether the observation corresponds to the post-reform period or not,¹⁴ Note that in (1) we use estimations of royalties and its interaction with the time dummy, $\widehat{Royalties}_{mt}$ and $\widehat{Royalties}_{mt} \times \widehat{Post2011}_t$, which correspond to the predicted values of these variables after the first-stage estimation in our 2SLS identification strategy.

The variable of interest in this study is $\widehat{Royalties}_{mt} \times \widehat{Post2011}_t$, which corresponds to the interaction between royalties and the post-reform dummy. Consequently, the coefficient of interest for this paper is δ_2 , which measures the change in the marginal effect of royalties on households' welfare *caused* by the 2011 institutional reform. Positive and significant values of this coefficient mean that compared to the pre-reform period, the effect of royalties on the corresponding outcome increases.

Our specifications also include municipality and time fixed effects, as well as several household and municipality-level covariates. α_m are municipality level fixed effects that control

¹³All monetary values are expressed in 2010 Colombian Pesos.

¹⁴In the estimations we always exclude observations corresponding to year 2012. The reform was approved by the Congress in 2011 and started its implementation the following year. Hence, this year is hybrid, exhibiting a mix of pre and post-treatment characteristics. Additionally, the data on municipality-level royalties overlaps from different sources for this year.

for any time-invariant municipal characteristics that might affect welfare, such as geographic conditions or long-term institutional traits. β_t are time dummies, that control for yearly events that affect in the same way Colombian households, such as other national-level reforms or macroeconomic fluctuations. \mathbf{X}_{imt} is a vector of household-level covariates, that include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Finally, \mathbf{Z}_{mt} is a vector of municipality-level controls, including population (in logs), the proportion of rural population, and in some specifications, other central government transfers besides royalties. As previously stated, all specifications exclude the transition year 2012 and all standard errors are clustered at the municipality-level, to allow for serial correlation at this level.

We follow a 2SLS estimation as $Royalties_{mt}$ might be an endogenous variable. For instance, omitted variables such as institutional characteristics of municipalities can determine the size of transfers. This situation is particularly evident after the reform, as some of the newly created funds condition the allocation of resources on municipal traits such as poverty or population. Also, as municipalities now compete for resources, their success might depend on individual mayoral or institutional characteristics difficult to measure. Consequently, following similar approaches to the ones used by [Dube and Vargas \(2013\)](#), we instrument royalties exploiting the variation in the international price of oil.¹⁵ This variation is evident from [Figure 4](#). Given that Colombia is a price-taker in this market, it is safe to assume that international prices are orthogonal to Colombian production and to several other characteristics, such as households' welfare.

Yearly oil prices represent time variation under this strategy. To account for cross-sectional variation at the household-level, as in [Dube and Vargas \(2013\)](#), we use municipality-level oil production in 1988. Therefore, in our basic specifications, the interaction between oil prices and the 1988 level of production constitutes our instrument for royalties. We expect higher transfers to municipalities producing more when the price increases. Additionally, as the interaction between royalties and the reform time dummy might be endogenous as well, we instrument it with the triple interaction between price, oil production in 1988, and the post-reform time dummy. Consequently, the first-stage of our model is of the form:

$$Royalties_{mt} = \alpha_m + \beta_t + (Oil_m^{1988} \times Price_t)\rho_1 + (Oil_m^{1988} \times Price_t \times Post2011_t)\rho_2 + \mathbf{X}_{imt}\phi + \mathbf{Z}_{mt}\eta + \varepsilon_{imt}$$

¹⁵Other recent papers using a similar approach are [Carreri and Dube \(2017\)](#) and [Martinez \(2017\)](#).

$$\begin{aligned}
Royalties_{mt} \times Post2011_t &= \alpha_m + \beta_t + (Oil_m^{1988} \times Price_t)\mu_1 \\
&+ (Oil_m^{1988} \times Price_t \times Post2011_t)\mu_2 + \mathbf{X}_{imt}\phi + \mathbf{Z}_{mt}\eta + \varepsilon_{imt}
\end{aligned}$$

where Oil_m^{1988} is oil production in 1988 in municipality m and $Price_t$ is the international price of oil in year t . The predicted values of this first-stage model are used in the second stage (equation 1), to estimate the causal effect of the reform on household welfare. As it was mentioned above, the main coefficient of interest is δ_2 in equation 1. Given the way we measure royalties, δ_1 represents the marginal effect on welfare of an additional COP100,000 in royalties before the reform, while $\delta_1 + \delta_2$ is such effect after 2011. Hence, δ_2 represents the change in the marginal effect due to the reform.

As discussed in the econometric literature, instrumental variable models are very sensitive to specification issues in the presence of weak instruments (Bound et al. (1995)). To detect the presence of weak instruments, we compute the Sanderson-Windmeijer F Statistics for first stage tests of weak identification and evaluate whether we are able to reject the null hypothesis that the instrument is weak. (Sanderson and Windmeijer (2016)).¹⁶ We also provide results for alternative estimators that have proved to be robust to the weak instrument problem and have better finite sample properties (Andrews and Stock (2007)). In particular, we consider the Limited Information Maximum Likelihood (LIML) estimator introduced by Anderson and Rubin (1949) and the adaptation of LIML developed by Fuller (1977).

Due to the multiple outcomes analyzed in this study, we correct for multiple testing using the method of Benjamini and Hochberg (1995) to control for the false discovery rate. We apply this procedure to each family of outcomes under consideration.

4. DATA

To evaluate the impact of the reform on household welfare, we use a repeated cross-sectional database constructed from the annual Colombian Quality of Life Survey, for periods before and after the institutional shift. Starting in 1997, each cross-section is a representative

¹⁶We use these tests as diagnostics of whether a particular regressor is weakly identified. Given that we have multiple endogenous regressors (royalties and its interaction with the time dummy), this test is preferred over the typical F-Statistic of the first stage. Also, note that for every model we present two SW F-Statistics: one for each instrument.

sample of the country’s population. Additionally, from 2008 onwards, each survey is also representative of the population in urban and rural areas. For the whole dataset, there is representativeness of the main regions of the country: Antioquia, Valle del Cauca, Atlantica, Pacifica, Oriental, Central, Bogota, San Andres and Orinoquia-Amazonia. For some years, the sample is also representative of specific departments.

Starting in 2010, the survey is conducted annually. Before that, it is intermittent, and in fact, we have information for years 1997, 2003, 2008, and 2010-2016. This coverage allows us to compare both systems, as we have household-level welfare data from before and after the reform. By gathering cross sections from all these years, we end up with a dataset of 194,833 households located in 394 municipalities all over the country.

The purpose of this survey is to analyze a large set of welfare characteristics of Colombian households, including housing conditions, education, health, childcare, labor force, income, assets ownership, and life satisfaction across several members of the household. Tables A.1 and A.2 in the Appendix, present some descriptive statistics of the main variables to be used in this study, both before and after the reform. Note that the number of observations varies for each variable, as some questions are omitted in specific waves of the survey.

The outcomes of interest for this paper come from this survey. We study the effect of the institutional reform on the following variables: poverty, measured through the Multi-dimensional Poverty Index (MPI)¹⁷ and a self-reported dummy that indicates whether the family considers itself poor or not; household income; a housing deficit index;¹⁸ access to the aqueduct service and continuity in the provision of drinking water; cell phone service, having a computer at home, and internet access; health indicators, such as affiliation to the healthcare system and illness occurrence; educational outcomes, including whether a child in the household attends school, level of education and the number of years of education of the household head; travel times to school and to work; perception of security in the city where the respondent lives; employment status of the household head, whether he has a work contract, a formal job, and whether he works in the construction, civil work, agricultural, manufacturing or service sectors.

¹⁷The MPI, developed by the Oxford Poverty and Human Development Initiative and the UNDP, captures severe deprivations that a person faces with respect to dimensions such as education, health, and living standards. An individual is defined as poor if she is deprived in three or more of the ten dimensions aggregated in the index.

¹⁸We construct this index using Principal Components Analysis (PCA). For this purpose, we use several variables of the survey, that include characteristics of the house where the family lives, including the material of floors, ceilings, walls, sanitation conditions, among others.

A comparison of tables A.1 and A.2 shows that the proportion of poor households, using the MPI, decreases substantially when we move from the pre-reform period, to the post-reform one. From the long list of outcomes analyzed, this is our preferred one, for two reasons. First, increasing the effect of royalties to thwart poverty was one of the main motivations of the reform. And second, by construction, the MPI is a synthetic indicator aggregating several of the other outcomes.

The royalties data we use comes from two chronologically distinct sources. Before the reform, direct royalties were assigned by the collecting agencies as a function of oil and mining resources exploited in each region. Indirect resources were allocated through the FNR, which was in charge of managing information of both sources. As of today, the National Planning Department consolidates all the information from both direct and indirect allocations. The reform included the creation of a new system of information that collects detailed data on transfers from the national government to departments and municipalities, including royalties and other resources.

Both data sources reveal information on the distribution of royalties across different sectors. Before the reform, these resources were used to fund a small number of sectors, particularly energy, transportation, and water supply. Additionally, a considerable amount was necessarily allocated to energy, mining, and environmental projects. This changed after the creation of SGR as other types of projects are increasingly being funded such as those in education, healthcare, housing and most importantly in transport which is the sector receiving the most funding.

Finally, we also use in our analysis other municipality-level variables, that come from various sources. Population series and projections are provided by the Administrative Department of National Statistics. Data on municipality investments by sector comes from the National Planning Department, while mining and oil production data is provided by the Mines and Energy Ministry.

5. RESULTS

In this section, we present the main results of the empirical analysis based on the models described in equation 1. In each case, we run 2SLS regressions to determine the impact of the reform on a series of household-level indicators. Table 1 reports the results for a

set of welfare outcomes, namely the poverty index,¹⁹ a subjective measure of poverty,²⁰ monthly household income, and an index of housing deficit, that measures the quality and conditions of the respondent’s house.²¹ For each outcome variable, we estimate a model that does not include household-level covariates and one that does include them. In both cases, municipality-level covariates and fixed effects, as well as year fixed effects, are included in the regressions.

5.1. Welfare Indicators: Poverty, Income, Health, and Education

In Columns 1 and 2 of Table 1, we report the results for our objective measure of poverty, the MPI. In each case, we are interested in the coefficient of the interaction *Royalties* \times *Post2011*, which measures the change in the marginal effect of royalties when we move from the pre-reform to the post-reform period. The coefficient, in both cases, is negative and significant for this interaction. This means that after the reform, every additional Peso of royalties allocated to the municipality where the household lives, reduces the probability that this household is poor. The coefficient in Column 1, for instance, reveals that after the reform for every additional COP100,000 per capita (approximately US53 if we use the 2010 exchange rate), the probability of being poor is almost 2 percentage points lower, as compared to the pre-reform period.

In substantive terms, after the reform, the marginal effect of royalties on poverty improves. This result is robust to the inclusion of household-level covariates, as shown in column 2. It is interesting to note that δ_1 , the coefficient capturing the marginal effect of royalties on poverty before the reform, is quite small and statistically insignificant. Such result would support the claim that before the reform, a resource curse was taking place, as royalties had no reduction effect on poverty.²²

We also find significant effects if we use a subjective measure of poverty. Columns 3 and 4 reveal that the reform also had a negative effect on this variable. Column 3, for example, suggests that after the reform, the effect of royalties on the poverty self-report measure is 16

¹⁹As we explained above, we use the multidimensional poverty index. This is represented at the household level by a dummy variable equal to one if the household is classified as poor.

²⁰Respondents are asked whether they consider themselves poor or not.

²¹This index was constructed using principal components analysis, based on several characteristics of the house.

²²As it will become clear below, results concerning the existence of the resource curse before the reform, are mixed for the different outcomes we analyze. Nonetheless, remember that for our study the MPI is the most relevant outcome and it is quite eloquent that before the reform, royalties have a null effect on this variable.

percentage higher in absolute terms. Columns 5 and 6 show that the results on income are mixed, although our favorite specification—the one that includes household-level controls—suggests that there is a positive and significant effect of the reform on income. Therefore, it is safe to conclude that the reform had positive effects on objective, subjective, and monetary measures of poverty.

Significant impacts are also found when examining housing conditions. Columns 7 and 8 show that the reform has a negative effect on the housing deficit indicator. This index, created using principal component analysis based on housing characteristics, takes values between 0 and 1, with higher values indicating worse housing conditions. Hence, the negative and significant coefficient suggests that after the reform, every additional Peso in royalties has a higher positive effect on housing conditions. This may be explained by the larger funding of projects aimed at improving the quality of housing as well as access to public services, but it can also be the result of higher incomes after the reform.

To address the problem of multiplicity of outcomes, we apply the Benjamini and Hochberg’s (1995) correction for the false discovery rate. We restrict our attention to the coefficient of the interaction between royalties and the dummy for 2011. Panel B of Table 1 presents the results of this exercise. We estimate the BH factor and compare it to the standard p-values. In all the four outcomes of interest and two specifications, the BH factor is larger than the standard p-value, suggesting that the null hypothesis of no effect can be rejected after adjusting for multiplicity.

In table 2, we report the effects of the reform on access to several public and private services, which include aqueduct service, continuous drinking water service, cellphone service, having a computer at home, and internet access. Columns 1 and 2 reveal, for instance, that after the reform the marginal effect of royalties on the probability of having access to the aqueduct service is higher. Every additional COP100,000 per capita after the reform represents an 8 percentage point increase in this probability. The impact is higher in the case of continuous drinking water service, where the effect is of 27 percentage points. These two indicators are crucial, as access to the aqueduct service and continuous drinking water are essential in order to prevent gastrointestinal diseases, especially among children under five.²³ It should be clear that several projects funded through the royalties system have enabled the construction of new water infrastructure and the improvement of already existent aqueducts.

It is important to acknowledge, in fact, that there are no effects on access to other relevant public services, such as electricity or sewage (results available upon request). In the case

²³A large body of evidence support this claim. See [Waddington et al. \(2009\)](#) for an overview.

of natural gas service, the effect is even negative, which is in line with the fact that several projects related to this service encountered implementation problems and certain regions presented strong price rises. Nonetheless, the reform has effects on other important privately provided services, which could reflect higher levels of income and welfare of households. For example, Columns 5 and 6 show that after the reform, the marginal positive effect of royalties on the probability of having a cell phone is higher. The marginal effect is about 7 percentage points higher after the reform. After controlling for household-level covariates, we observe similar results for the probability of having a computer at home. Moreover, the effect is also positive and significant for the probability of having access to internet services. All of these results are robust to controlling for multiple outcomes using the Benjamini and Hochberg's (1995) correction for the false discovery rate. Naturally, all these indicators are relevant for the purpose of closing digital gaps in a developing country like Colombia.

Table 3 presents the results for important welfare indicators associated with health and education. Columns 1 and 2 show that the reform has a positive effect on access to the healthcare system—the impact is of approximately 8 percentage points. This result is not surprising, considering that several projects aim to improve healthcare conditions. We also find effects on another important health outcome. Columns 3 and 4 show that after the reform, every additional COP100,000 per capita reduces the likelihood of self-reported illness by about 9 percentage points. It is important to remember that some of the projects funded using royalties, after the reform, include the construction of new hospitals and the improvement of existent ones. Additionally, if the reform has effects on poverty and income, one may expect that households will have access to improved healthcare services.

In terms of education, our results are interesting as well. Columns 5 and 6 show that the interaction's coefficient is positive and significant when we estimate a model for the probability that at least one child in the household attends school. The change in the marginal effect of royalties is about 3 percentage points. The effects on adults are mixed. Columns 7 and 8 report that the effect is null for the highest level of education achieved by the household head. Nonetheless, Columns 9 and 10 report positive effects on the number of years of education of the household head. These results are not surprising, as the highest degree of education achieved by the household head is an outcome that varies in the middle or long run, while the number or years of education approved can change in the short run, if the reform has effects on drop-out rates.

Moreover, if we analyze the way in which royalties have been invested after the reform, it is clear that the most popular dimension so far has been the construction of roads ²⁴. Due to the territorial divide concerning this dimension, many municipalities and departments have presented projects that aim to catch up. A large amount of small and tertiary roads have been built in recent years, increasing communication and productivity among beneficiaries. Even though the survey we use for this study makes it difficult to measure the impact of this type of investments, some of the survey questions can be used to infer effects of the reform on the quality of the transport system. Respondents are asked about the time it takes for them to go to school or to work. Columns 1-4 in table 4 show that the effects on these variables are negative and strongly significant. Respondents take less time to school or to work if they live in places that get more money from royalties after the reform. Without hesitation, these results are important, especially in rural areas where children have to walk long distances to attend school.

But progress has also changed certain perceptions that might seem hard to modify. The positive and significant coefficients associated with the security perceptions, reported on columns 5 and 6, suggest that places receiving more royalties after the reform exhibit important improvements on this dimension. This result might be a consequence of income effects, as the proportion of projects directly related to security issues is modest.²⁵ Nonetheless, it is not surprising that in places where poverty levels decrease and income rise, rises, the perception of how safe the location is, increases as well. The effect of the reform on this outcome is huge: more than 20 percentage points for every additional COP100,000 in royalties. Finally, column 8—which represents our favorite specification as it includes household-level controls—reveals one of the most important results of this reform: the effect on employment is positive and significant, which implies that the reform contributed to the creation of new jobs ²⁶. We now analyze if there are any distributional effects on employment.

²⁴Transport represented 32% of total investments funded by the General System of Royalties for the period 2012-2015. See, for instance: <https://www.sgr.gov.co/LinkClick.aspx?fileticket=-uDhxQYXLmo%3D&tabid=320>.

²⁵Investments in security issues represented less than 1% of total investment under the new system for the period 2012-2015.

²⁶As in the case of the previous outcomes, we find that our results are robust to controlling for multiple comparisons.

5.2. Labor Indicators: Employment, Formality, and Development

It is not surprising that such big shocks on governments' budgets and public revenue have distributional effects on employment, or at least, on the type of sectors demanding workers. We have previously claimed that the reform has a positive effect on employment. Moreover, given the nature of the projects funded through these rents, one should expect labor shifts across different sectors. First of all, given that many of these projects are being executed directly by the government or by third parties contracted by the State, one may expect important effects on formality, especially if we consider that the rate of informality in Colombia is quite high. However, our results on this issue are mixed. Columns 1 and 2 of table 5 report the effects of the reform on the probability that the household head has a work contract. Column 2, for instance, shows that the reform has a positive and significant effect on the likelihood of having a work contract. This effect is quantified as an approximate 13 percentage points increase for every additional COP 100,000. Even though the effect is also positive and even higher when we estimate the effect on the probability of working in the formal sector, as seen in columns 3 and 4, the coefficients are not significantly different from zero.

As displayed in Table 5, we decompose the effects of the reform into different relevant economic sectors that are expected to vary as a result of the institutional change described in this study. As claimed previously, roads are by far the most popular project funded through royalties. Housing projects are also quite popular, as well as infrastructure interventions related to public service delivery, such as schools, hospitals, etc. Consequently, it is not surprising, as reported in columns 1 and 2, that the effect of the reform on the probability of being employed in the construction sector is positive and significant. The effect is about 2 percentage points for every additional COP100,000 per capita. Naturally, the construction sector is broad enough to include private and public projects. Columns 3 and 4 show that the effect is significant—albeit modest—in the case of public jobs, which includes public infrastructure investments. This result suggests that public employment is not the only mechanism explaining the positive effect on construction and that private projects are probably very important as well.²⁷

²⁷It can be argued that local governments can use these new funds in patronage, employing more people in order to maximize reelection prospects (Robinson et al. (2006)). News would be bad in such case, as this form of redistribution tends to be inefficient and could deepen the resource curse. Nonetheless, as some of the above results suggest, more positive forms of redistribution would be taking place as a consequence of the reform, as we find effects on incomes, poverty, and public goods provision. Whether higher public employment is a consequence of patronage or not remains an important open question.

Engel's law establishes that an increase in income, enhancing consumers' purchasing power, shifts demand from agricultural to non-agricultural goods (Murata, 2008). Moreover, Petty-Clark's law (Clark, 1940) states that as an economy develops, there should be a shift from the primary sector, based fundamentally on agriculture and extraction of raw materials, to secondary and tertiary sectors, based more on manufactures and services. The basic theory behind this claim is that technological progress reduces transportation costs, which in turn magnifies the size of industrial goods and services. Hence, a shift from agriculture to manufacture, and in a later stage, to services, should take place if the reform is bringing development to the country.²⁸ Fortunately, the survey provides information that enables us to examine whether the royalties reform is promoting this path for development in Colombia, as respondents are asked about their sector of employment.

In accordance with this theory, columns 1 and 2 of table 6 show that the effect of the reform on the probability of being employed in agriculture is negative and significant. The marginal effect of every additional COP100,000 per capita on the probability of working in the agricultural sector is approximately 14 percentage points lower after 2011. Interestingly, the effect is positive and significant—of 7 percentage points—on the probability of working in the manufacturing sector (Columns 3 and 4). Finally, Columns 5 and 6 show that the effect is null on the probability of being employed in the service sector. This result is quite relevant, as it suggests that projects funded through royalties, after the reform, are not entirely associated with the tertiary sector, which is considered a more advanced step towards development. This is quite disappointing, given that one of the main pillars of the reform is to promote investments in science, technology, and innovation. Nonetheless, the result is not surprising, given that a lot of criticism has been raised against the reform for not boosting properly such investments.²⁹

Moreover, the result that the reform has a positive effect on the probability of being employed on the manufacturing sector suggests that this form of institutional arrangement helps thwart some of the pervasive consequences of the Dutch disease. It is well known that resource booms promote deindustrialization as a consequence of the appreciation of the exchange rate making local production less profitable.³⁰ But if income associated with the boom is redistributed from producing regions to the rest of the country, in the form of labor-

²⁸It is important to notice that the reform is relatively recent, so we do not expect to be able to capture large effects on this regard. Therefore, results from this section are tentative.

²⁹In fact, a recent reform to the Royalties System modifies the way resources for science and technology are allocated.

³⁰Van der Ploeg (2011) summarizes a set of studies that find support for this claim regarding the role of resource windfalls on deindustrialization.

intensive projects, many of them dependent on the manufacturing sector, we should expect lower negative effects due to the Dutch disease. Naturally, one of the biggest challenges of this type of institutional arrangement is not only to stimulate production and employment in the secondary sector but also to boost the development of high-value services.

5.3. Mechanisms: Accountability, Planning, and State Capacity

The institutional reform that changed the allocation rule of resource rents in Colombia modified, at least, three important dimensions: the way in which investments are monitored and held accountable, the incentives local authorities have to plan their projects, and the access to royalties granted to different types of municipalities. First, new mechanisms for accountability of projects were introduced. The new system combines traditional top-down accountability strategies, such as audits by the National Planning Department and the Office of the Comptroller General, with bottom-up methods, that include public audits and web-based tools for control. More accountability aims to diminish corruption and inefficiencies, which in turn should increase the marginal impact of royalties on welfare.³¹

Second, under the new rules, municipalities have to present projects to a board of reviewers, composed by members of different levels of government, who collegially decide if they are approved or not. Consequently, local authorities are in the obligation of planning their projects beforehand. This might improve the quality of projects and subsequent investments, in comparison to the previous system, or even the number of beneficiaries, given the criteria utilized by these boards to make decisions. This contrasts with the previous institutional arrangement, as under the old rules, producing municipalities would receive rents no matter how well-planned the projects were.

Finally, after the reform, every municipality in Colombia has access to royalties, and not only producers, as it used to be the case before 2011. Hence, places with varying levels of state capacity start receiving different shares of these resources. It could be the case then, that for money funded to municipalities with higher levels of state capacity, the marginal effect on welfare is higher. In such a case, one should observe higher marginal effects of royalties on welfare in places with stronger state capacity.

In order to test the hypotheses derived by these three potential mechanisms, we exploit the fact that after the reform new sources of information and data became available. Under the new system, information on project characteristics, timing, monitoring, etc., is richer.

³¹Darby (2011) provides an overview of the issues regarding natural resource wealth and accountability.

Hence, in the tests that follow we restrict the analysis to the post-reform period—i.e. for years after 2012. First, in terms of accountability, using information from the Royalties Directorate at DNP, we are able to track which projects have been monitored by this office since the reform began. Hence, we construct a measure, called $Audits_{mt}$, which indicates the proportion of projects monitored in municipality m in year t . Projects are audited at different stages and not only at the end, so it is fair to assume that if this mechanism is effective, the impact of royalties after the reform should be higher in places where a larger proportion of projects were audited.³²

Second, as a proxy for the planning quality of projects, we exploit the fact that when municipalities submit proposals to the decision boards, they must specify the precise timing of the projects. Nonetheless, inadequate planning and other factors can lead to a discrepancy between proposed and actual times. Hence, once more, using information provided by the Royalties Directorate, we construct the variable $Planning_{mt}$, which corresponds to the average difference between the actual and the planned length of projects, in municipality m and year y . Municipalities planning better projects should exhibit lower levels of discrepancy between these two lengths. Under the hypothesis that planning makes a difference under the new system, places with projects that plan better should exhibit higher marginal effects of royalties on welfare.

Finally, to test the state-capacity hypothesis, we use a municipality-level index that has been created by the National Planning Department in 2005. The Overall Performance Index (IDI, in Spanish)³³ captures municipalities' capacities on four important dimensions: Efficacy,³⁴ efficiency,³⁵ management,³⁶ and legal requirements³⁷ (DNP, 2005). This index has been used historically to rank municipalities in terms of state capacity. Therefore, we use the variable $Capacity_{mt}$, which corresponds to the realization of this index for municipality m in year t , as our measure of state capacity. Naturally, one would expect endogeneity between some dimensions captured by IDI, and the allocation of royalties in Colombia, especially after the reform. Consequently, in the analysis that follows we have to

³²157 projects were audited in the period 2015-2016, most of them in the transport sector (51). During the same period, 198 public meetings were organized to foster citizens' participation in accountability. Understanding the role of these accountability mechanisms is an area of active research.

³³Indice de Desempeño Integral in Spanish.

³⁴This dimension measures the degree of fulfillment of development plans goals.

³⁵Determines if the municipality optimizes human, financial, and physical endowments in order to provide health, education, and water services.

³⁶Quantifies the effect of management and financial variables on efficacy and efficiency outcomes.

³⁷Measures whether municipalities fulfill conditions and requirements imposed by formal rules.

be careful to interpret all coefficients as correlations between variables and take the results as suggestive evidence of the underlying mechanisms.

With these three measures in hand, we estimate the following 2SLS models:

$$Poverty_{imt} = \alpha_m + \beta_t + \widehat{Royalties}_{mt}\kappa_1 + (\widehat{Royalties}_{mt} \times \widehat{Audits}_{mt})\kappa_2 + \mathbf{X}_{imt}\phi + \mathbf{Z}_{mt}\eta + \varepsilon_{imt} \quad (2)$$

$$Poverty_{imt} = \alpha_m + \beta_t + \widehat{Royalties}_{mt}\omega_1 + (\widehat{Royalties}_{mt} \times \widehat{Planning}_{mt})\omega_2 + \mathbf{X}_{imt}\phi + \mathbf{Z}_{mt}\eta + \varepsilon_{imt} \quad (3)$$

$$Poverty_{imt} = \alpha_m + \beta_t + \widehat{Royalties}_{mt}\psi_1 + (\widehat{Royalties}_{mt} \times \widehat{Capacity}_{mt})\psi_2 + \mathbf{X}_{imt}\phi + \mathbf{Z}_{mt}\eta + \varepsilon_{imt} \quad (4)$$

for $t = 2013 \dots 2016$ and where $Poverty_{imt}$ is a dummy variable for poverty using the Multidimensional Poverty Index. Note that, we restrict the mechanism analysis to this poverty index for the sake of clarity. The reason is that with so many outcomes, it is difficult to detect which effects prevail, so it makes more sense to use a unique indicator of welfare. The poverty index results from the aggregation of several of the dimensions studied in this paper, and consequently, is our preferred outcome. The coefficients of interest in these specifications are κ_2 , ω_2 , and ψ_2 , which correspond to the estimates of the interactions between royalties and our three mechanisms. Hence, for instance, if κ_2 were negative and significant, the negative effect of royalties on poverty after 2012 would be higher in municipalities that are monitored more by the authorities. Something similar for ω_2 in the case of planning, and for ψ_2 in the case of state capacity.

One final caveat: in each case, $Royalties_{mt}$ is instrumented through $Oil_m^{1988} \times Price_t$, while the three interactions of royalties and the mechanisms are instrumented through $(Oil_m^{1988} \times Price_t) \times Audits_{mt}$, $(Oil_m^{1988} \times Price_t) \times Planning_{mt}$, and $(Oil_m^{1988} \times Price_t) \times Capacity_{mt}$, respectively. As previously mentioned, even though we instrument royalties and its interactions using oil price shocks, our measures of accountability, planning, and state capacity might be endogenous, so the heterogeneous effects estimated with this method must be interpreted with caution.

We report the results of these specifications in Table 7. The coefficients of the constituent terms of the interactions are not included in order to ease the inspection of the table. Columns 1 and 2 report the results of models in which the mechanisms are not introduced, just to test the effect of royalties on poverty reduction after the reform. As expected, the sign of the coefficient is negative, implying that households living in places receiving more royalties exhibit lower probabilities of being poor. Columns 3-8 incorporate the interactions of our mechanisms and royalties. Columns 3 and 4 show that there are no differential effects of audits on the marginal effect of royalties. It is not necessarily true that households living

in places in which a higher proportion of projects were audited, exhibit a sharper decrease in the probability of being poor for every additional Peso received.

A similar result is reported in columns 5 and 6: the interaction between royalties and our planning measure is not significantly different from zero. Hence, we cannot conclude that people living in municipalities where projects are better planned—measured by a lower lag between the actual and the expected durations of projects, experience a higher decrease in the probability of being poor for every additional Peso received. Therefore, this evidence suggests that the effect of royalties on poverty is not mediated by accountability or planning. Hence, our last candidate is state capacity.

As a matter of fact, columns 7 and 8 of Table 7 show that the coefficients of the interaction between royalties and our measure of state capacity—the Overall Performance Index—is negative and significant. This result suggests that people living in municipalities with higher levels of state capacity, as measured by this index, exhibit higher decreases in the probability of being poor. Therefore, this tentative evidence suggests that the institutional reform has been successful in local governments with more efficient investment structures. Because our research design exploits variation in those municipalities that were producers before the reform, these results cannot be explained by a change in the composition of the recipients of royalties³⁸. Although producer municipalities are receiving less revenues in general, those with higher state capacity among them are using these revenues in a more effective way. In columns 9 and 10 we include the three interactions at the same time. The results, once more, support the state capacity mechanism, against the accountability and planning stories.

5.4. Robustness: Placebo Tests and Alternative Mechanisms

We have claimed in previous sections that in 2011 a soft institutional reform took place in Colombia, changing the rules of allocation of rents, and impacting in a considerable way household welfare and living conditions. However, other explanations might be consistent with the empirical findings presented in this study. For instance, it might be the case that after the reform, the change in the allocation rule has an effect on migration. It is well known that resource-rich regions tend to attract certain types of workers (Warner, 2015),

³⁸It is important to remember that the LATE estimator captures an effect for the subpopulation of compliers defined by the instrument. Because the distribution of royalties was concentrated among producer municipalities before the reform, our population of compliers are composed by these producers. As such, the effects found in this paper are not driven by non-producer municipalities.

but it is not completely clear if these changes are a direct result of production or of the way rents are spent. For example, as a function of their skills and abilities, certain families might prefer to move to places in which royalties are more likely to be invested, instead of staying in producing municipalities. These migrations might, in turn, affect economic variables such as income or poverty, confounding the direct effects of the reform with indirect effects that result from changes in incentives.

To account for the potential effects of varying migration patterns after 2011, all of the models presented in subsections 5.1 and 5.2 control for migration. In this case, we incorporate a dummy variable indicating if the family has always lived in the same municipality. We also use alternative measures of migration, such as how long the family has lived in the same place (results not shown). In any case, the results are the same. The reform has positive effects on the different welfare outcomes we use. Hence, it is not the case that after 2011 the marginal effect of royalties on welfare is higher simply because families are migrating to places with better conditions or because municipalities are receiving “richer” households.

An alternative mechanism that might fit the story presented in this paper brings into consideration other sources of local government revenue. It is well known that royalties are not the only source of revenue available for these governments (Martinez, 2017), and in fact, they are not the only transfer made by the central authority. In Colombia, the General System of Shareholdings—SGP for its acronym in Spanish³⁹—is the main instrument used by the central government in order to transfer resources to local government to fund investments in social services, such as education and healthcare. Also, revenues raised by local governments themselves, through different taxes such as the property tax, represent important complementary sources used to fund public service delivery. If the 2011 royalties reform induces transformations of the allocation patterns of SGP and territorial-specific income change after 2011, then the changes in these alternative sources of revenue may explain the effects found on household welfare. To account for these potential confounders, we estimate all the models reported in sections 5.1 and 5.2 including additional municipality-level control variables such as the time-varying amounts of SGP transfers and territorial-specific income raised in the municipality where the survey respondents live. The results of these specifications, available upon request, show that our original estimations are robust to the inclusion of these variables.

Moreover, we exploit these alternative sources of revenue to perform a series of placebo tests that corroborate the robustness of our results. We reestimate all of our models, but instead

³⁹In Spanish, *Sistema General de Participaciones*.

of using royalties as our treatment variable, we utilize SGP and territorial-specific income per capita in the municipality where the respondents live. The rationale underpinning these placebo tests rests on the fact that the marginal effect of royalties, understood as a source of revenue for local governments, changes as a result of the reform that took place in 2011. If other factors—different to the reform—are affecting the revenues of municipalities after 2011, or if the reform per se affects not only royalties but also other transfers made by the central government and taxes raised by local governments, all these elements may be confounding with the impact we aim to calculate.

However, Tables A.3-A.8 in the Appendix show that this is not the case. The results of the placebo tests reveal that, in general, there are no differential changes in the marginal effects of SGP transfers or revenues municipalities have raised themselves on household living standards. Hence, it seems to be the case that the reform is indeed changing the way municipalities spend royalties, as opposed to the incentives that the central government has to allocate other sources of income or the way in which local governments use the income raised on their own.

It can also be claimed that the timing of the reform was endogenous to the political process and that politicians belonging to President Santos' governmental coalition approved it in 2011 in order to favor their constituencies. If electoral motives guided the approval timing of the reform, we should find that the treatment effects are higher in municipalities governed by politicians of the coalition. To test this claim, we construct a dummy variable called $Coalition_m$, equal to 1 for households living in municipalities governed between 2007-2011 by a Mayor belonging to Santos' coalition in 2011.⁴⁰ To test for heterogeneous effects across this dimension, we estimate models that include the triple interaction between the allocation of royalties, the post-reform dummy, and the coalition dummy.

Tables A.9–A.11 in the Appendix report the results of these estimations, for our different outcome variables. We do find significant heterogeneous effect for an important number of outcomes. However, in general, the sign of the heterogeneous effect goes in the opposite direction of the sign of the main effect. Hence, in most cases the effect of the reform is lower for households living in places where Santos' coalition governed. This supports our argument, as it cannot be claimed that the governmental coalition—which had a majority in Congress—approved the reform in 2011 precisely to favor the constituencies where they had more political support.

⁴⁰This coalition included the following parties: Liberal, Conservador, U, and Cambio Radical.

Finally, we test for heterogeneous treatment effects among producing and non-producing municipalities, given that this trait may have also affected the approval timing of the reform. For this purpose, we construct a dummy variable called $Producer_m$, equal to 1 for households living in places above the 75th percentile in royalties allocation before the reform. Again, we include the triple interaction between royalties, the post-reform dummy, and the producer dummy. Tables A.12–A.13 show that, in general, the reform has no differential effects across households living in producing and non-producing municipalities.

6. CONCLUDING REMARKS

In this paper, we claim that soft institutional reforms might serve to counter the negative consequences of the resource curse. The literature showing that resource abundance entails poor economic and political outcomes is broad. But studies showing how to solve this puzzle, in the short or medium run, are remarkably scarce. We try to fill this gap by showing that the reform that took place in Colombia during 2011 had positive impacts on the marginal effects generated by resource rents on the well-being of households.

These positive effects are evident on measures of poverty, income, and housing conditions. We also find positive effects on different indicators related to the provision of social services and public goods, such as education, health, transportation, or security. At least two channels seem to explain these results. First, the direct purpose of projects seems to yield intended effects. Many of them relate to roads, education, healthcare, etc., and important effects on these dimensions are found. In addition, the evidence suggests that after the reform, investments induce shifts on employment, both in terms of quality and distribution across sectors. More people tend to have work contracts after the reform in places receiving more royalties, and they tend to shift from the agricultural to the manufacturing sector.

However, the reform is far from perfect. As our results reveal, the economic effects of the reform are not necessarily large—even though they are statistically significant—with respect to specific dimensions. In some other important cases, they are null or negative. For instance, we observe that the impact of the reform on employment in the service sector is negative. This is somehow surprising, given that at least 10% of total rents after the reform go to the Science, Technology, and Innovation Fund. Such results suggest that the reform was ill planned in this dimension and that certain institutional adjustments may yield the desired effects.

Additionally, even though new and innovative mechanisms for top-down and bottom-up accountability were introduced with the reform, corruption scandals and allegations of embezzlement are still present. In fact, as several judicial investigations have shown, some mayors and governors in different regions have used resources from the Science Technology, and Innovation Fund in an inappropriate way to enrich themselves. Therefore, it is safe to conclude that accountability mechanisms are far from perfect. Bottom-up techniques, such as public audits and web-based methods, tend to be underutilized. And top-down strategies, like audits by anti-corruption agencies, tend to be limited to a few number of projects. Consequently, it would be natural to conclude that the positive effects found in this paper are just a lower bound of the potential impacts that soft institutional reforms might have on the marginal effects of resource rents in developing countries.

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7. TABLES AND FIGURES

Distribution of Royalties across Colombian Municipalities before and after the Reform

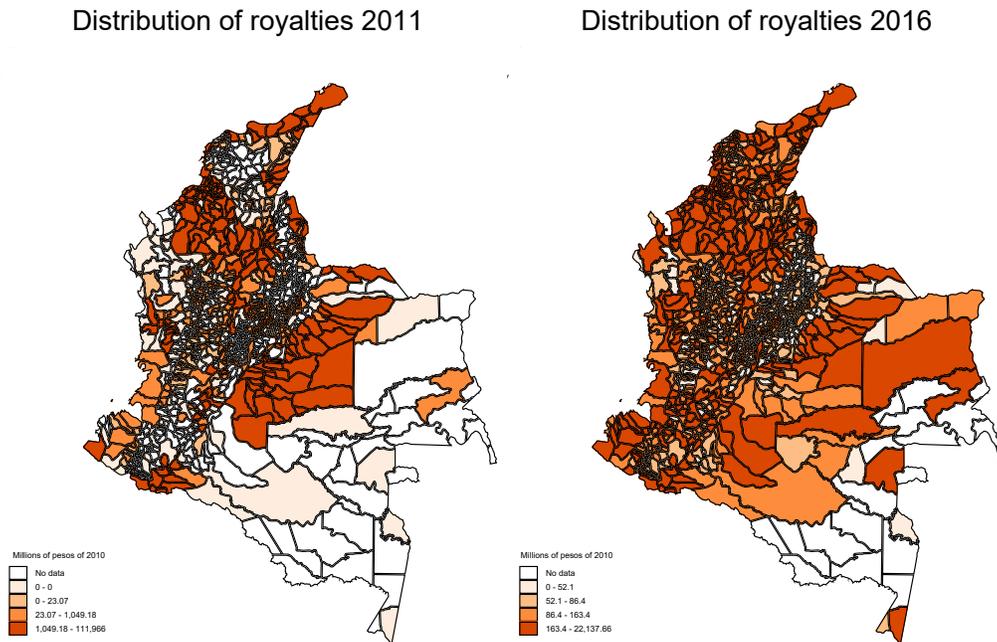
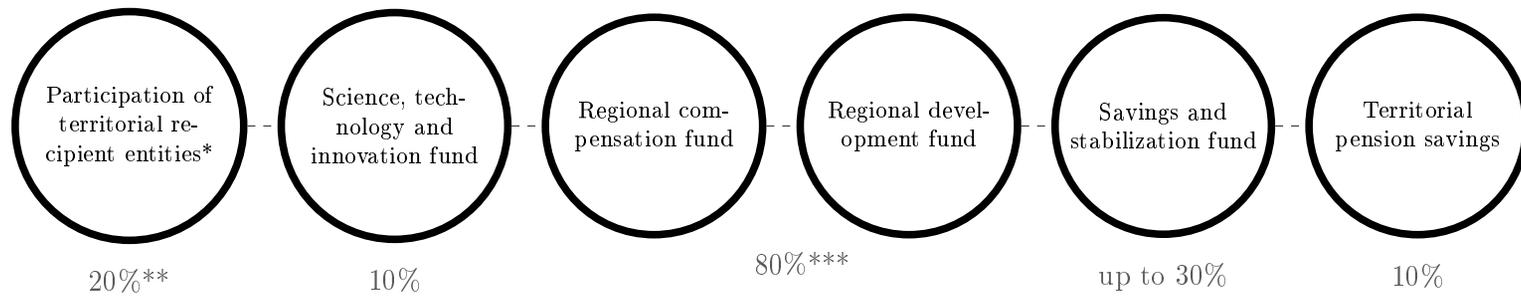


Figure 1: This map shows the geographical distribution of royalties, measured in millions of 2010 Colombian Pesos (COP), across Colombian municipalities. The left map presents the distribution in 2011, just before the reform was approved. The right map shows this distribution in 2016, some years after its approval. Clearly, after the reform more municipalities receive royalties, no matter if they are producers or not.

Figure 2: General Royalties System After 2011 Reform



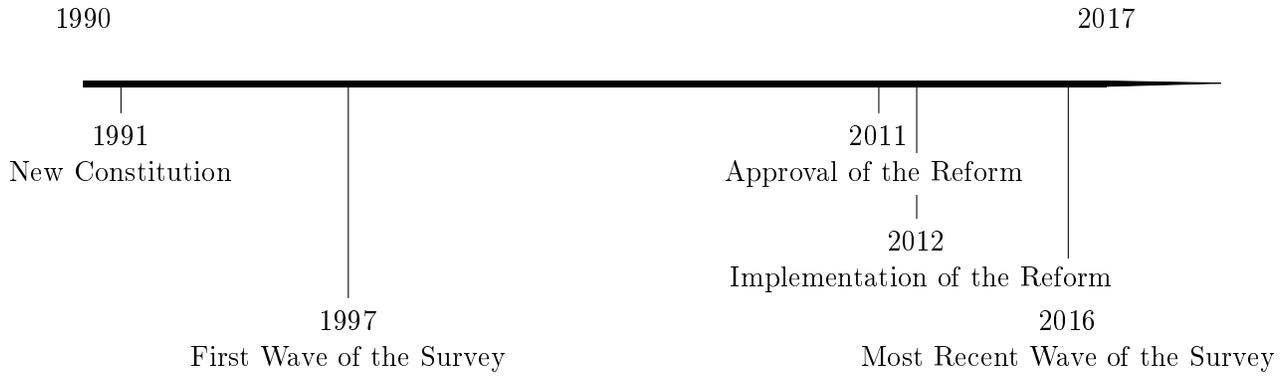
*The recipient entities (either producers or maritime/fluvial ports for transportation of non-renewable natural resources) participate in all the components of the General Royalty System.

**After discounting FCTI, FAE, FONPET.

***After discounting FCTI, FAE, FONPET.

Note: Author's elaboration based on Ministry of Finance and Public Credit data.

Figure 3: Timeline of Events



Evolution of International Oil Prices and National Production

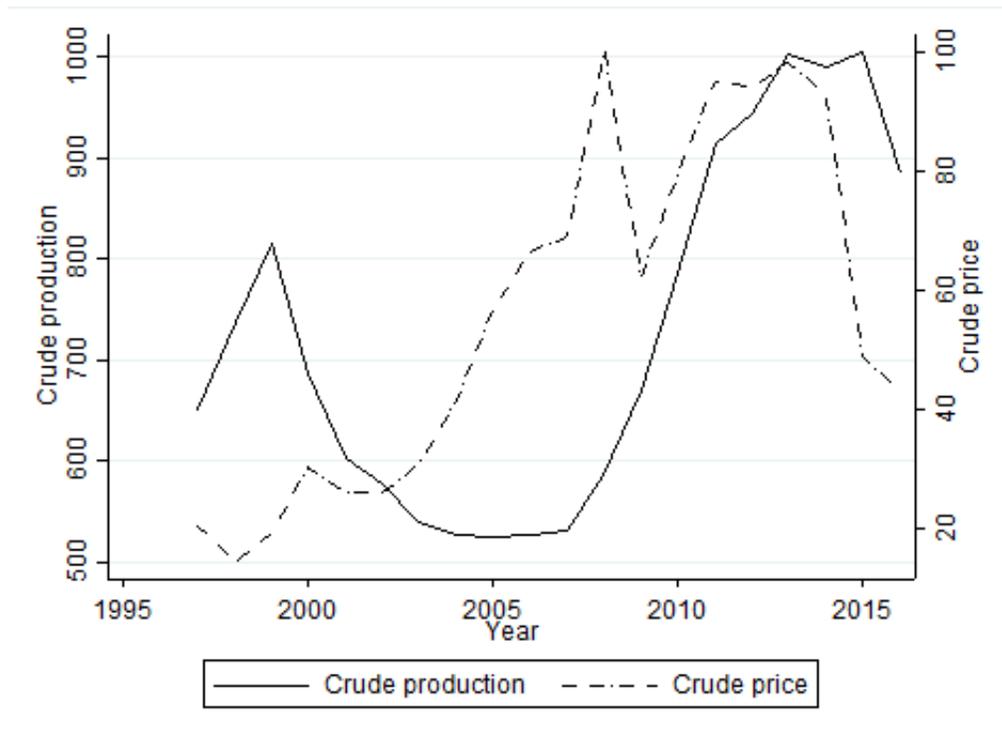


Figure 4: This figure shows the evolution of oil production and international oil prices for the 1997-2016 period. Source: Authors' elaboration based on National Planning Department.

Table 1: Effect of the Reform on Welfare Indicators

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Poverty Index	Poverty Index	Poverty Perception	Poverty Perception	Household Income	Household Income	Housing Deficit Index	Housing Deficit Index
Panel A								
Royalties	0.004 (0.006)	-0.005 (0.008)	-0.098*** (0.022)	-0.127*** (0.029)	14371.1 (63898.7)	-80829.5 (105301.5)	-0.023** (0.010)	-0.018*** (0.006)
Royalties×Post2011	-0.019*** (0.005)	-0.009*** (0.003)	-0.155*** (0.031)	-0.167*** (0.036)	-51924.7* (27207.3)	129054.3*** (44651.1)	-0.043** (0.018)	-0.026*** (0.009)
Post2011	-0.092*** (0.011)	-0.092*** (0.012)	-0.059*** (0.020)	-0.109*** (0.027)	-107705.5* (61157.2)	10362.6 (72513.4)	0.140*** (0.015)	0.103*** (0.012)
SW F-Stat 1	922.80***	1163.22***	26.55***	23.62***	922.80***	1163.22***	13.39***	9.85***
SW F-Stat 2	1588.87***	943.26***	52.46	58.33***	1588.87***	943.26***	38.78***	29.45***
Panel B								
Multiple comparison correction for Royalties×Post2011								
P-value	0.000	0.007	0.000	0.000	0.056	0.004	0.017	0.007
Benjamini&Hochberg	0.1	0.2	0.05	0.05	0.2	0.1	0.15	0.15
Reject of Ho	1	1	1	1	1	1	1	1
Household Controls	N	Y	N	Y	N	Y	N	Y
Mun. Controls	Y	Y	Y	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y	Y	Y	Y
N	127769	112968	103831	88942	127769	112968	147383	132466

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *Royalties* is the amount of royalties, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 2: Effect of the Reform on Housing Indicators

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Aqueduct Service	Aqueduct Service	Water Continuity	Water Continuity	Cellphone Service	Cellphone Service	Computer at Home	Computer at Home	Internet Access	Internet Access
Panel A										
Royalties	0.050** (0.025)	0.040** (0.019)	0.110*** (0.031)	0.105*** (0.033)	0.051*** (0.013)	0.050*** (0.014)	-0.034*** (0.005)	-0.042*** (0.006)	-0.011** (0.004)	-0.017*** (0.004)
Royalties×Post2011	0.077* (0.043)	0.066** (0.032)	0.272*** (0.040)	0.268*** (0.041)	0.066*** (0.019)	0.064*** (0.018)	0.009 (0.007)	0.032*** (0.009)	0.026** (0.011)	0.045*** (0.012)
Post2011	0.026 (0.048)	0.017 (0.054)	0.189*** (0.073)	0.199*** (0.075)	-0.039* (0.021)	0.003 (0.022)	-0.446*** (0.051)	-0.333*** (0.057)	0.205*** (0.017)	0.230*** (0.018)
SW F-Stat 1	9.60***	7.49***	10.65***	8.36***	9.60***	7.49***	750.93***	1059.42***	846.76***	1271.62***
SW F-Stat 2	32.51***	25.91***	35.18***	27.02***	32.51***	25.91***	1413.19***	1649.41***	1441.02***	1669.26***
Panel B										
Multiple comparison correction for Royalties×Post2011										
P-value	0.073	0.041	0.000	0.000	0.000	0.000	0.170	0.001	0.015	0.000
Benjamini&Hochberg	0.16	0.2	0.04	0.04	0.08	0.12	0.2	0.16	0.12	0.08
Reject of Ho	1	1	1	1	1	1	1	1	1	1
Household Controls	N	Y	N	Y	N	Y	N	Y	N	Y
Mun. Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	161293	146335	117213	106028	161293	146335	142478	127629	141735	126893

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *Royalties* is the amount of royalties, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 3: Effect of the Reform on Health and Education Indicators

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Healthcare Access	Healthcare Access	Illness	Illness	Children Education	Children Education	Level of Education	Level of Education	Years Approved	Years Approved
Panel A										
Royalties	0.045*	0.038*	-0.008	-0.026*	0.009**	0.011**	0.035	0.022	0.121**	0.106**
	(0.025)	(0.022)	(0.010)	(0.016)	(0.004)	(0.006)	(0.057)	(0.057)	(0.049)	(0.044)
Royalties×Post2011	0.081**	0.065**	-0.090***	-0.085***	0.024***	0.032***	0.068	0.097	0.212**	0.170*
	(0.039)	(0.033)	(0.014)	(0.017)	(0.006)	(0.009)	(0.126)	(0.119)	(0.099)	(0.099)
Post2011	0.294***	0.296***	-0.066***	-0.063***	0.043***	0.072***	0.458***	0.544***	-0.042	-0.053
	(0.040)	(0.041)	(0.017)	(0.017)	(0.014)	(0.013)	(0.083)	(0.083)	(0.128)	(0.130)
SW F-Stat 1	9.62***	7.51***	46.63***	37.86***	9.60***	7.49***	7.76***	6.13**	111.10***	82.17***
SW F-Stat 2	32.53***	25.92***	247.47***	257.41***	32.51***	25.91***	24.52***	19.48***	746.95***	781.24***
Panel B										
Multiple comparison correction for Royalties×Post2011										
P-value	0.036	0.049	0.000	0.000	0.000	0.000	0.592	0.418	0.031	0.086
Benjamini&Hochberg	0.16	0.12	0.04	0.04	0.08	0.08	0.2	0.2	0.12	0.16
Reject of Ho	1	1	1	1	1	1	0	0	1	1
Household Controls	N	Y	N	Y	N	Y	N	Y	N	Y
Mun. Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
<i>N</i>	161194	146249	152172	137243	161293	146335	156178	141679	20674	18775

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *Royalties* is the amount of royalties, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 4: Effect of the Reform on other Welfare Indicators

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Time to School	Time to School	Time to Work	Time to Work	Security Perception	Security Perception	Employment (HH Head)	Employment (HH Head)
Panel A								
Royalties	0.335** (0.171)	0.296* (0.168)	-2.524*** (0.570)	-1.753*** (0.476)	0.114*** (0.040)	0.095** (0.037)	0.043*** (0.010)	0.059*** (0.019)
Royalties×Post2011	-1.777*** (0.506)	-1.002* (0.592)	-5.241*** (0.877)	-4.883*** (0.784)	0.223*** (0.056)	0.201*** (0.051)	0.021 (0.017)	0.046** (0.022)
Post2011	0.681 (0.509)	1.109* (0.568)	-1.873 (1.512)	-1.154 (1.543)	0.117*** (0.038)	0.117*** (0.034)	0.257*** (0.019)	0.258*** (0.019)
SW F-Stat 1	642.28***	800.83***	11.53***	8.66***	9.60***	7.49***	6.54**	5.13**
SW F-Stat 2	271.84***	977.03***	39.63***	31.01***	32.60***	26.04***	18.49***	14.55***
Panel B								
Multiple comparison correction for Royalties×Post2011								
P-value	0.001	0.091	0.000	0.000	0.000	0.000	0.209	0.038
Benjamini&Hochberg	0.15	0.2	0.05	0.05	0.1	0.1	0.2	0.15
Reject of Ho	1	1	1	1	1	1	0	1
Household Controls	N	Y	N	Y	N	Y	N	Y
Mun. Controls	Y	Y	Y	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y	Y	Y	Y
<i>N</i>	50546	49331	107795	97844	161161	146243	155206	140248

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *Royalties* is the amount of royalties, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 5: Effect of the Reform on Labor Indicators

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Work Contract	Work Contract	Formal Job	Formal Job	Construction Job	Construction Job	Civil Work Job	Civil Work Job
Panel A								
Royalties	-0.019*** (0.007)	-0.019** (0.008)	0.136 (0.135)	0.127 (0.135)	-0.010*** (0.002)	-0.012*** (0.002)	0.001*** (0.000)	0.001*** (0.000)
Royalties×Post2011	0.089*** (0.013)	0.126*** (0.008)	0.216 (0.208)	0.213 (0.208)	0.024*** (0.003)	0.023*** (0.003)	0.001*** (0.000)	0.001*** (0.000)
Post2011	-0.026 (0.018)	-0.020 (0.022)	0.010 (0.093)	0.062 (0.073)	-0.007 (0.009)	-0.008 (0.011)	-0.002*** (0.001)	-0.002*** (0.001)
SW F-Stat 1	335.84***	405.86***	1.42	1.02	27.73***	28.17***	27.73***	28.17***
SW F-Stat 2	568.27***	687.38***	0.84	0.67	79.75***	79.64***	79.75***	79.64***
Panel B								
Multiple comparison correction for Royalties×Post2011								
P-value	0.000	0.000	0.298	0.305	0.000	0.000	0.001	0.001
Benjamini&Hochberg	0.1	0.1	0.2	0.2	0.05	0.05	0.15	0.15
Reject of Ho	1	1	0	0	1	1	1	1
Household Controls	N	Y	N	Y	N	Y	N	Y
Mun. Controls	Y	Y	Y	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y	Y	Y	Y
<i>N</i>	39762	34917	110468	99129	91290	91172	91290	91172

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *Royalties* is the amount of royalties, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 6: Effect of the Reform on Labor across Sectors

	(1)	(2)	(3)	(4)	(5)	(6)
	Agricultural Sector	Agricultural Sector	Manufacturing Sector	Manufacturing Sector	Service Sector	Service Sector
Panel A						
Royalties	-0.115*** (0.024)	-0.115*** (0.023)	0.039*** (0.008)	0.035*** (0.008)	0.053*** (0.019)	0.056*** (0.019)
Royalties×Post2011	-0.137*** (0.035)	-0.137*** (0.033)	0.070*** (0.018)	0.072*** (0.017)	-0.022 (0.030)	-0.036 (0.028)
Post2011	-0.064** (0.031)	-0.063** (0.032)	-0.300*** (0.057)	-0.264*** (0.055)	-0.389*** (0.047)	-0.445*** (0.046)
SW F-Stat 1	27.73***	28.17***	27.73***	28.17***	27.73***	28.17***
SW F-Stat 2	79.75***	79.64***	79.75***	79.64***	79.75***	79.64***
Panel B						
Multiple comparison correction for Royalties×Post2011						
P-value	0.000	0.000	0.000	0.000	(0.470	0.197
Benjamini&Hochberg	0.0666	0.1333	0.1333	0.0666	0.2	0.2
Reject of Ho	1	1	1	1	0	1
Household Controls	N	Y	N	Y	N	Y
Mun. Controls	Y	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y	Y
<i>N</i>	91290	91172	91290	91172	91290	91172

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *Royalties* is the amount of royalties, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 7: Mechanisms: Accountability, Planning, and State Capacity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Poverty Index	Poverty Index	Poverty Index	Poverty Index	Poverty Index	Poverty Index	Poverty Index	Poverty Index	Poverty Index	Poverty Index
Royalties	-0.0119 (0.00853)	-0.0138 (0.00951)	-0.160 (0.195)	-0.179 (0.219)	-0.0142 (0.00900)	-0.0159 (0.0101)	0.235*** (0.0320)	0.268*** (0.0373)	0.382** (0.183)	0.560** (0.274)
Royalties×Audits			0.00448 (0.00572)	0.00503 (0.00640)					0.00205 (0.00215)	0.00382 (0.00335)
Royalties×Planning					0.00848 (0.00617)	0.0110 (0.00785)			0.00164 (0.00382)	0.00266 (0.00512)
Royalties×Capacity							-0.00347*** (0.000487)	-0.00386*** (0.000610)	-0.00643* (0.00342)	-0.00957* (0.00521)
Household Controls	N	Y	N	Y	N	Y	N	Y	N	Y
Mun. Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
<i>N</i>	87604	87604	52215	52215	52215	52215	62917	62917	41641	41641

Notes: Standard errors clustered at the municipality level are shown in parentheses. All models use observations beyond 2012 only. *Royalties* is the amount of royalties, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Audits* is the proportion of projects audited by DNP in the municipality where the household lives. *Planning* is the average difference between the planned time and the real completion time of projects. *Capacity* is the Overall Performance Index for each municipality. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

A. APPENDIX FOR ONLINE PUBLICATION

A.1. Descriptive Statistics

Table A.1: Summary Statistics Before the Reform

Variable	Mean	Std. Dev.	Min.	Max.	N
Poverty Index	0.314	0.464	0	1	40165
Poverty Perception	0.565	0.496	0	1	51285
Household Income	1374161.193	2248575.32	0	94216664	40165
Housing Deficit Index	0.306	0.146	0.104	0.93	71954
Aqueduct Service	0.8	0.4	0	1	85846
Water Continuity	0.823	0.381	0	1	61526
Cellphone Service	0.699	0.459	0	1	85846
Computer at Home	0.258	0.437	0	1	58017
Internet Access	0.152	0.359	0	1	55763
Healthcare Access	0.875	0.331	0	1	85813
Illness	0.274	0.446	0	1	76725
Children Education	0.525	0.407	0	1	63952
Level of Education	3.578	1.489	1	8	83087
Years Approved	4.696	2.526	1	25	11098
Time to School	16.393	16.417	3	180	15678
Time to Work	26.336	30.563	0	600	57574
Security Perception	0.782	0.413	0	1	85733
Employment (HH Head)	0.825	0.38	0	1	85846
Work Contract	0.221	0.415	0	1	18697
Formal Job	0.207	0.405	0	1	70835
Construction Job	0.029	0.168	0	1	34873
Civil Work Job	0.003	0.051	0	1	34873
Agricultural Job	0.095	0.294	0	1	34873
Manufacturing Job	0.188	0.391	0	1	34873
Service Job	0.493	0.5	0	1	34873
Age (HH Head)	47.58	15.621	11	104	85846
Gender (HH Head)	0.689	0.463	0	1	85846
Urban	0.646	0.478	0	1	85846
No. of Children	0.349	0.63	0	6	85846
Household Size	3.726	1.946	1	20	85846
Migration	0.543	0.498	0	1	70804
Royalties Per Capita (in 100,000 COP of 2010)	0.475	2.129	0	33.707	85846

Table A.2: Summary Statistics After the Reform

Variable	Mean	Std. Dev.	Min.	Max.	N
Poverty Index	0.221	0.415	0	1	108987
Poverty Perception	0.462	0.499	0	1	86085
Household Income	1422647.559	2444388.623	0	224427168	108987
Housing Deficit Index	0.351	0.152	0.127	0.939	108968
Aqueduct Service	0.813	0.39	0	1	108987
Water Continuity	0.712	0.453	0	1	83768
Cellphone Service	0.941	0.236	0	1	108987
Computer at Home	0.311	0.463	0	1	108978
Internet Access	0.282	0.45	0	1	108967
Healthcare Access	0.972	0.166	0	1	108911
Illness	0.23	0.421	0	1	108987
Children Education	0.536	0.418	0	1	74704
Level of Education	3.644	1.414	1	8	105472
Years Approved	4.298	2.365	1	15	15960
Time to School	18.259	18.45	5	180	34868
Time to Work	22.919	27.066	0	240	71811
Security Perception	0.836	0.37	0	1	108967
Employment (HH Head)	0.789	0.408	0	1	102900
Work Contract	0.074	0.262	0	1	21065
Formal Job	0.251	0.433	0	1	64404
Construction Job	0.059	0.236	0	1	65198
Civil Work Job	0.001	0.027	0	1	65198
Agricultural Job	0	0.012	0	1	65198
Manufacturing Job	0.066	0.249	0	1	65198
Service Job	0.439	0.496	0	1	65198
Age (HH Head)	49.137	16.005	12	104	108987
Gender (HH Head)	0.651	0.477	0	1	108987
Urban	0.616	0.486	0	1	108987
No. of Children	0.292	0.579	0	7	108987
Household Size	3.351	1.788	1	24	108987
Migration	0.682	0.466	0	1	108987
Royalties Per Capita (in 100,000 COP of 2010)	0.318	0.523	0	10.328	108987

A.2. Placebo Test

In this placebo test we reestimate the models of the main text, but instead of using our royalties per capita measure (and its interactions), we use other sources of revenue for municipalities. First, we use the amount of SGP per capita disbursed by the central government to the municipality where the household lives. Transfers from the SGP (*Sistema General de Participaciones*) are resources allocated by the central government to departments, districts, and municipalities in Colombia to pay for the services they must provide, which include health and education, among other services. And second, we use the amount of income raised by municipalities themselves, which corresponds to income raised through different forms of local taxes.

The rationale underpinning these placebo tests is that because these transfers and additional sources of revenue are independent of the 2011 reform, there should be no differential effects on our welfare indicators. Tables A.3–A.8 show that in general, that is the case. Every specification includes household-level and municipality-level controls, as well as municipality and time effects. Standard errors are clustered at the municipality level. Other estimations (not presented, available upon request), show that when we estimate our original models (with our royalties measure), but controlling for SGP transfers and municipalities’ own income, results hold and are robust to such alternative specifications.

Table A.3: Placebo Test: Other Transfers from the Central Government

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Poverty Index	Poverty Perception	Household Income	Housing Deficit Index	Aqueduct Service	Water Continuity	Cellphone Service	Computer at Home	Internet Access
SGP	0.505*	-2.137	-1842857.1	0.449	-1.606	2.457	-0.0590	-1.265	-0.188
	(0.284)	(2.867)	(1642896.2)	(0.566)	(1.718)	(3.518)	(0.131)	(1.832)	(1.087)
SGP×Post2011	0.0236	0.244	-327561.7**	-0.0371	0.126	-0.228	0.0482**	-0.00631	-0.0699
	(0.0201)	(0.681)	(137193.8)	(0.0579)	(0.199)	(0.363)	(0.0233)	(0.198)	(0.0928)
Post2011	-0.331***	0.287	1784812.9***	0.0254	-0.0676	0.330	-0.108*	0.390	0.444***
	(0.124)	(1.019)	(634714.7)	(0.134)	(0.470)	(0.929)	(0.0630)	(0.413)	(0.107)
Household Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mun. Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
<i>N</i>	90075	78207	90075	97952	103615	73168	103615	103534	103534

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *SGP* is the amount of transfers from the *Sistema General de Participaciones*, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A.4: Placebo Test: Other Transfers from the Central Government (cont.)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Healthcare Access	Illness	Children Education	Level of Education	Years Approved	Time to School	Time to Work	Security Perception
SGP	-0.479 (0.555)	4.434 (4.892)	-3.563 (3.856)	-6.852 (6.754)	-4.894 (7.422)	109.0 (359.2)	-143.1 (186.9)	-3.083 (3.381)
SGP×Post2011	0.0467 (0.0613)	-0.249 (0.582)	0.241 (0.456)	0.460 (0.847)	0.252 (0.554)	7.232 (18.40)	14.32 (23.14)	0.178 (0.401)
Post2011	-0.0383 (0.141)	-0.161 (1.365)	-0.0184 (1.079)	0.146 (2.037)	-0.580 (1.783)	-60.69 (187.8)	-21.33 (54.19)	0.136 (0.938)
Household Controls	Y	Y	Y	Y	Y	Y	Y	Y
Mun. Controls	Y	Y	Y	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y	Y	Y	Y
<i>N</i>	103552	103615	103615	100250	13923	37459	69146	103534

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *SGP* is the amount of transfers from the *Sistema General de Participaciones*, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A.5: Placebo Test: Other Transfers from the Central Government (cont.)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employment (HH Head)	Work Contract	Formal Job	Construction Job	Civil Work Job	Agricultural Sector	Manufacturing Sector	Service Sector
SGP	0.0675 (0.476)	10.46 (35.05)	-3.363 (3.686)	1.086 (1.198)	0.000244 (0.00947)	0.103 (0.391)	-0.108 (0.175)	2.505 (3.219)
SGP×Post2011	0.0709 (0.0544)	-0.554 (1.224)	0.190 (0.451)	-0.274 (0.315)	0.000630 (0.00178)	-0.211* (0.126)	0.0515 (0.0351)	-0.236 (0.849)
Post2011	-0.507*** (0.0931)	-0.0813 (4.258)	0.178 (1.051)	0.388 (0.547)	-0.00148 (0.00121)	0.360 (0.239)	-0.121** (0.0564)	-0.595 (1.405)
Household Controls	Y	Y	Y	Y	Y	Y	Y	Y
Mun. Controls	Y	Y	Y	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y	Y	Y	Y
<i>N</i>	103615	27633	83757	58778	58778	58778	58778	58778

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *SGP* is the amount of transfers from the *Sistema General de Participaciones*, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A.6: Placebo Test: Municipalities' Own Income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Poverty Index	Poverty Perception	Household Income	Housing Deficit Index	Aqueduct Service	Water Continuity	Cellphone Service	Computer at Home	Internet Access
OwnIncome	1.233 (2.053)	-16.37 (25.07)	-14603600.6 (39792381.7)	-0.269 (0.271)	0.743 (1.001)	6.570* (3.471)	0.749*** (0.264)	-6.175 (12.47)	-3.589 (8.277)
OwnIncome×Post2011	-0.737 (1.939)	12.09 (23.01)	11006806.7 (34877943.4)	0.357 (0.319)	-0.532 (1.084)	-6.244 (13.33)	-1.000 (1.082)	4.590 (11.14)	2.929 (7.153)
Post2011	0.0673 (0.485)	-1.831 (4.165)	-2405831.3 (8439558.6)	-0.0971 (0.188)	0.0770 (0.172)	0.812 (5.831)	0.172 (0.436)	-0.652 (1.285)	-0.452 (1.529)
Household Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mun. Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
<i>N</i>	87259	86413	87259	106032	119885	87506	119885	101532	100834

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *OwnIncome* is the total amount of income per capita raised by each municipality through different types of taxes. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A.7: Placebo Test: Municipalities' Own Income (cont.)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Healthcare Access	Illness	Children Education	Level of Education	Years Approved	Time to School	Time to Work	Security Perception
OwnIncome	1.015 (1.071)	-7.333 (14.93)	0.914 (1.172)	3.607 (2.851)	38.27 (335.5)	131.8 (217.5)	-98.77 (133.2)	3.481 (4.582)
OwnIncome×Post2011	-0.787 (1.569)	3.795 (12.13)	-0.469 (1.123)	-1.967 (3.788)	-41.67 (247.0)	-91.30 (197.2)	35.92 (99.69)	-1.670 (4.438)
Post2011	0.349 (0.308)	-0.529 (2.337)	0.0423 (0.127)	0.308 (0.679)	12.17 (62.40)	21.35 (51.42)	10.48** (4.771)	-0.176 (0.515)
Household Controls	Y	Y	Y	Y	Y	Y	Y	Y
Mun. Controls	Y	Y	Y	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y	Y	Y	Y
<i>N</i>	119823	110793	119885	116061	14315	36374	80232	119814

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *OwnIncome* is the total amount of income per capita raised by each municipality through different types of taxes. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A.8: Placebo Test: Municipalities' Own Income (cont.)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employment (HH Head)	Work Contract	Formal Job	Construction Job	Civil Work Job	Agricultural Sector	Manufacturing Sector	Service Sector
OwnIncome	-0.173 (2.004)	-10.60 (10.12)	0.0498 (0.987)	0.539 (2.525)	0.0298** (0.0121)	-1.612 (4.282)	1.744*** (0.670)	-2.459 (7.516)
OwnIncome×Post2011	-0.871*** (0.255)	7.677 (10.59)	0.853* (0.502)	0.545 (0.899)	-0.0302 (0.0242)	3.484*** (0.773)	-1.502 (1.467)	-0.981 (3.490)
Post2011	0.410 (0.490)	-1.462 (2.376)	-0.204 (0.421)	-0.286 (0.300)	0.00320 (0.0106)	-1.017 (1.267)	-0.0542 (0.532)	1.182** (0.526)
Household Controls	Y	Y	Y	Y	Y	Y	Y	Y
Mun. Controls	Y	Y	Y	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y	Y	Y	Y
<i>N</i>	119885	26662	96050	72277	72277	72277	72277	72277

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *OwnIncome* is the total amount of income per capita raised by each municipality through different types of taxes. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

A.3. Heterogeneous Effects: Government Coalition

In this subsection we test if there are any heterogeneous treatment effects across households living in places politically aligned with President Santos' coalition. These effects are important, because it can be claimed that the timing of the reform—which was approved in 2011 with implementation starting in 2012—is endogenous to the political process. It might be the case that the coalition approved the reform because they anticipated that it would benefit more their constituencies. To test if the political affiliation of mayors may be associated with the incentives to approve the reform, we test for heterogeneous effects at this level.

Consequently, we construct a dummy variable called $Coalition_m$, which equals 1 for households living in municipalities governed in 2007-2011 by a political party that belongs to Santos' 2010 coalition. Hence, the dummy equals one if the mayor belongs to any of the following parties: Liberal, Conservador, de la U, or Cambio Radical, which make up the so called *Unidad Nacional*, during his first tenure. Using this variable, we estimate models that include the triple interaction between the allocation of royalties, the post-reform dummy and the coalition dummy. Tables A.9–A.11 present the results of these estimations.

The results show that for an important number of variables, there are significant heterogeneous treatment effects. However, the results run counter to the hypothesis that the reform was approved in order to benefit municipalities governed by the Santos' coalition. In general, the main effect and the heterogeneous effect go in opposite directions. This means that the positive effects of the reform on welfare are weaker in places governed by the coalition.

Table A.9: Heterogeneous Effects: Government Coalition

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Poverty Index	Poverty Perception	Household Income	Housing Deficit Index	Aqueduct Service	Water Continuity	Cellphone Service	Computer at Home	Internet Access
Royalties×Post2011×Coalition	-0.00232 (0.00945)	0.0912*** (0.0298)	385292.1*** (91126.1)	0.0114 (0.0123)	-0.0206 (0.0402)	-0.306*** (0.0923)	-0.0156 (0.0233)	0.0412*** (0.0140)	-0.0548** (0.0228)
Household Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mun. Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
<i>N</i>	110004	86413	110004	128565	142326	105423	142326	124269	123566

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *Royalties* is the amount of royalties, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. *Coalition* is a dummy that equals 1 for municipalities whose 2007-2011 Mayor belongs to the 2011 presidential coalition. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A.10: Heterogeneous Effects: Government Coalition (cont.)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Healthcare Access	Illness	Children Education	Level of Education	Years Approved	Time to School	Time to Work	Security Perception
Royalties×Post2011×Coalition	-0.148*	0.0921**	-0.0589***	-0.353***	-0.0885	7.896***	0.310	-0.160***
	(0.0835)	(0.0371)	(0.0135)	(0.121)	(0.283)	(0.777)	(2.134)	(0.0396)
Household Controls	Y	Y	Y	Y	Y	Y	Y	Y
Mun. Controls	Y	Y	Y	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y	Y	Y	Y
<i>N</i>	142241	133538	142326	137723	17873	48255	94750	142242

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *Royalties* is the amount of royalties, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. *Coalition* is a dummy that equals 1 for municipalities whose 2007-2011 Mayor belongs to the 2011 presidential coalition. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A.11: Heterogeneous Effects: Government Coalition (cont.) (cont.)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employment (HH Head)	Work Contract	Formal Job	Construction Job	Civil Work Job	Agricultural Sector	Manufacturing Sector	Service Sector
Royalties×Post2011×Coalition	-0.102*** (0.0186)	-0.00633 (0.0551)	-0.198 (0.223)	-0.0479*** (0.00975)	-0.000598 (0.000570)	0.161*** (0.0434)	0.0326 (0.0239)	-0.196*** (0.0615)
Household Controls	Y	Y	Y	Y	Y	Y	Y	Y
Mun. Controls	Y	Y	Y	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y	Y	Y	Y
<i>N</i>	136340	33951	96176	88364	88364	88364	88364	88364

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *Royalties* is the amount of royalties, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. *Coalition* is a dummy that equals 1 for municipalities whose 2007-2011 Mayor belongs to the 2011 presidential coalition. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

A.4. Distributive Effects: High vs. Low Royalties Recipient Municipalities

Municipalities differ in terms of the level of royalties they receive as a consequence of the reform. One possibility is that our results are driven by the effect of the reform on those municipalities that receive large amounts of royalties. In this section, we explore this possibility.

Tables A.12, A.13, and A.14 report the results of this analysis. For each case, we construct a dummy variable indicating whether the household lives in a municipality that receives large levels of royalties or not. High royalties recipient municipalities are defined as those above the 75th percentile of the distribution of royalties before 2011. We interact this dummy with $Royalties_{mt}$, $Post2011_t$, and the interaction of these two. Consequently, to determine if there is any distributive treatment effect at this level we should focus on the coefficient of the triple interaction between royalties, the post-reform dummy, and the high recipient dummy. To ease the interpretation of the results, in tables A.12, A.13, and A.14 we omit the rest of the coefficients. As mentioned before, all these models include household-level and municipality-level covariates, as well as municipality and time effects. Standard errors are clustered at the municipality level.

The results show that-in general-there are no distributive effects. The coefficient of the triple interaction is not significant in 21 out of the 25 estimated models. More importantly, there are null distributive effects for the most important variables, including poverty, income, housing conditions, employment, health, and education. Therefore, there seems to be little or no differential effects of the reform between high and low recipient municipalities. At least in marginal terms, the effects are the same. Admittedly, the problem for high recipients is that the share of resources allocated after the reform has fallen considerably. Because most high recipient municipalities have oil and mineral exploitation, this explains why in many of these municipalities citizens have voted to ban economic activities related to mining production.

Table A.12: Distributive Effects: High vs. Low Royalties Recipient Municipalities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Poverty Index	Poverty Perception	Household Income	Housing Deficit Index	Aqueduct Service	Water Continuity	Cellphone Service	Computer at Home	Internet Access
Royalties×Post2011×High-recipient	-0.0828 (0.516)	-0.0743 (0.216)	1526751.3 (13331549.0)	0.0757 (1.862)	-0.0373 (0.897)	0.515 (2.357)	-0.0466 (1.065)	0.226 (1.023)	0.226 (0.536)
Household Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mun. Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
<i>N</i>	112968	88942	112968	132466	146335	106028	146335	127629	126893

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *Royalties* is the amount of royalties, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. *Producer* is a dummy that equals 1 for municipalities above the 75th percentile in royalties before the reform. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A.13: Distributive Effects: High vs. Low Royalties Recipient Municipalities (cont.)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Healthcare Access	Illness	Children Education	Level of Education	Years Approved	Time to School	Time to Work	Security Perception
Royalties×Post2011×High-recipient	-0.0453 (0.638)	-0.00924 (0.214)	0.0685 (0.0912)	0.0705 (0.177)	1.153*** (0.204)	-1.307 (6.474)	-2.386 (8.270)	-0.000399 (1.495)
Household Controls	Y	Y	Y	Y	Y	Y	Y	Y
Mun. Controls	Y	Y	Y	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y	Y	Y	Y
<i>N</i>	146249	137243	146335	141679	18775	49331	97844	146243

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *Royalties* is the amount of royalties, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. *Producer* is a dummy that equals 1 for municipalities above the 75th percentile in royalties before the reform. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A.14: Distributive Effects: High vs. Low Royalties Recipient Municipalities (cont.)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employment (HH Head)	Work Contract	Formal Job	Construction Job	Civil Work Job	Agricultural Sector	Manufacturing Sector	Service Sector
Royalties×Post2011×High-recipient	-0.0325 (0.348)	0.342 (1.561)	-0.0347 (0.0491)	0.0333*** (0.00776)	0.00153*** (0.000521)	-0.130** (0.0635)	0.0265 (0.0205)	0.0421 (0.0378)
Household Controls	Y	Y	Y	Y	Y	Y	Y	Y
Mun. Controls	Y	Y	Y	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y	Y	Y	Y
<i>N</i>	140248	34917	99129	91172	91172	91172	91172	91172

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *Royalties* is the amount of royalties, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. *Producer* is a dummy that equals 1 for municipalities above the 75th percentile in royalties before the reform. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

A.5. Alternative estimators robust to the weak instruments problem

We consider alternative estimators to address the weak instruments problem. Besides the standard IV estimator, we consider the LIML and Fuller’s modified LIML. We briefly discuss why these alternative estimators provide a way to evaluate the robustness of our results.

Theoretical scholarship has shown that IV estimators are biased in finite samples. This opens the door for alternative estimators. The LIML estimator jointly estimates the main and the reduced-form equations by maximum likelihood assuming normality. Because the LIML estimator can be written in the classic IV form, it is asymptotically normal regardless of the normality of disturbances. However, this estimator is still sensitive to the problem of weak instruments.

Fuller’s modified LIML estimators are a consistent and asymptotically normal alternative to the standard IV and LIML estimators. This estimation method has the advantage of having better finite sample performance when instruments are weak. In particular, Fuller’s LIML estimator with the value of the alpha parameter equal to 1 is almost unbiased and has been suggested as a good choice. On the other hand, the estimator with a value of this parameter equal to 4 is approximately the minimum mean square error. [Hahn et al. \(2004\)](#) have shown in simulations that these estimators perform well in the presence of weak instruments and homoskedastic disturbances. [Hausman et al. \(2012\)](#) have derived a version of the Fuller’s LIML estimator that is robust to heteroskedasticity.

Table [A.15](#) presents the results for the proposed estimators when focusing on the poverty index. Columns 1 and 2 replicate the basic results for the standard IV estimator without and with controls. Column 3 presents the estimates for the LIML estimator. The coefficient of interest and its significance levels remain unaltered under this new estimator. Column 4 and 5 present the results for Fuller’s modified LIML with values of the alpha parameter equal to 1 and 4. The coefficients and levels of significance are similar to the ones obtained using the standard IV estimator. Overall, these results provide evidence that our estimates are robust to considering alternative estimators that are less sensitive to the weak instruments problem.

Tables [A.16](#), [A.17](#), and [A.18](#) presents the results for all the other outcomes considered in Table 1. Results follow the same pattern as the ones described in Table [A.9](#). The Online Appendix includes the results of this exercise for all the other outcomes analyzed in this study. Our main message holds as estimates based on alternative estimators are also very similar to those obtained using the standard IV estimator.

Table A.15: Alternative IV estimators for Poverty Index

	(1)	(2)	(3)	(4)	(5)
	Poverty Index	Poverty Index	Poverty Index LIML	Poverty Index Fuller(1)	Poverty Index Fuller(4)
Royalties	0.004 (0.006)	-0.005 (0.008)	-0.005 (0.008)	-0.005 (0.008)	-0.005 (0.008)
Royalties×Post2011	-0.019*** (0.005)	-0.009*** (0.003)	-0.009*** (0.003)	-0.009*** (0.003)	-0.009*** (0.003)
Post2011	-0.092*** (0.011)	-0.091*** (0.012)	-0.091*** (0.012)	-0.091*** (0.012)	-0.091*** (0.012)
Household Controls	N	Y	Y	Y	Y
Mun. Controls	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y
<i>N</i>	127769	112968	112968	112968	112968

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *Royalties* is the amount of royalties, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in columns 1 and 2. Column 3 estimates a LIML model and columns 4 and 5 a Fuller's modified LIML for the alpha parameter equal to 1 and 4. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A.16: Alternative IV estimators for Poverty Perception

	(1)	(2)	(3)	(4)	(5)
	Poverty Perception	Poverty Perception	Poverty Perception LIML	Poverty Perception Fuller(1)	Poverty Perception Fuller(4)
Royalties	-0.098*** (0.022)	-0.127*** (0.029)	-0.127*** (0.029)	-0.126*** (0.029)	-0.125*** (0.030)
Royalties×Post2011	-0.155*** (0.031)	-0.167*** (0.036)	-0.167*** (0.036)	-0.167*** (0.036)	-0.165*** (0.036)
Post2011	-0.059*** (0.020)	-0.109*** (0.026)	-0.109*** (0.026)	-0.109*** (0.026)	-0.110*** (0.026)
Household Controls	N	Y	Y	Y	Y
Mun. Controls	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y
<i>N</i>	103831	88942	88942	88942	88942

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *Royalties* is the amount of royalties, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in columns 1 and 2. Column 3 estimates a LIML model and columns 4 and 5 a Fuller's modified LIML for the alpha parameter equal to 1 and 4. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A.17: Alternative IV estimators for Household Income

	(1)	(2)	(3)	(4)	(5)
	Household Income	Household Income	Household Income LIML	Household Income Fuller(1)	Household Income Fuller(4)
Royalties	14,371.1 (63,898.8)	-80,829.5 (105,301.5)	-80,829.5 (105,301.5)	-80,733.8 (105,253.1)	-80,447.9 (105,108.3)
Royalties × Post2011	-51,924.7* (27,207.3)	129,054.3*** (44,651.1)	129,054.3*** (44,651.1)	129,034.6*** (44,639.9)	128,975.9*** (44,606.4)
Post2011	-107,705.5* (61,157.2)	10,362.6 (72,513.4)	10,362.6 (72,513.4)	10,350.6 (72,504.4)	10,314.5 (72,477.4)
Household Controls	N	Y	Y	Y	Y
Mun. Controls	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y
<i>N</i>	127769	112968	112968	112968	112968

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *Royalties* is the amount of royalties, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in columns 1 and 2. Column 3 estimates a LIML model and columns 4 and 5 a Fuller's modified LIML for the alpha parameter equal to 1 and 4. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A.18: Alternative IV estimators for Housing Deficit Index

	(1)	(2)	(3)	(4)	(5)
	Housing Deficit Index	Housing Deficit Index	Housing Deficit Index LIML	Housing Deficit Index Fuller(1)	Housing Deficit Index Fuller(4)
Royalties	-0,023** (0,010)	-0,018*** (0,006)	-0,018*** (0,006)	-0,018*** (0,006)	-0,018*** (0,006)
Royalties×Post2011	-0,043** (0,018)	-0,026*** (0,009)	-0,026*** (0,009)	-0,026*** (0,009)	-0,025*** (0,009)
Post2011	0,140*** (0,015)	0,103*** (0,012)	0,103*** (0,012)	0,103*** (0,011)	0,103*** (0,011)
Household Controls	N	Y	Y	Y	Y
Mun. Controls	Y	Y	Y	Y	Y
Mun. & Year Effects	Y	Y	Y	Y	Y
<i>N</i>	127769	112968	112968	112968	112968

Notes: Standard errors clustered at the municipality level are shown in parentheses. Years 2011 and 2012 have been excluded from each estimation. *Royalties* is the amount of royalties, in hundred thousand Colombian pesos, allocated to the municipality where the household lives. *Post2011* equals 1 for observations beyond year 2011 and 0 otherwise. Household-level controls include age and gender of the household head, household size, an urban dummy, number of children, and a migration dummy. Municipality-level controls are population (in logs) and the proportion of rural population. A 2SLS model is estimated in columns 1 and 2. Column 3 estimates a LIML model and columns 4 and 5 a Fuller's modified LIML for the alpha parameter equal to 1 and 4. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.