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From curse to blessing? institutional reform and resource booms in Colombia☆

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ABSTRACT

Is it possible to reverse 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 reverse this situation is scarce. In this paper, we exploit a component of an institutional reform that modified the allocation rule of oil royalties in Colombia and evaluate a set of effects of this reform on the living standards of Colombian households. Using international variations in the price of oil for identification, we find that the reform had important effects on several household welfare indicators. We find positive impacts on important dimensions, such as reductions in multidimensional poverty and improvements on income, employment, housing conditions, health, and education, among others. Results are mixed or null in other outcomes, such as formality or employment in the service sector. Importantly, we find no effects on monetary poverty but larger income effects for rich and urban households. We test for the different channels boosting these effects and provide tentative evidence that supports the role of fiscal and administrative capacity mechanisms.

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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

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[1] The 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.

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been enriched by better approaches dealing with causal effects in order to explain this paradoxical phenomenon.[2] This new literature represents an important progress in one of the most controversial topics in the development literature.[3]

One of the critical components of this new scholarship relates to the role of economic (Mehlun et al., 2006) and political institutions (Robinson et al., 2006; 2014) in explaining the resource curse. This theoretical scholarship shows that, when institutions are weak, economic agents are prone to rent-seeking and politicians are unconstrained in their intentions of remaining in power, causing a negative effect on 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 justified. 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 one critical element of a reform of the royalties system in Colombia: a change in the allocation rule for the case of producer municipalities to redistribute rents to non-producer ones.[6] To do so, we exploit spatial and time variation in rents allocation across municipalities in Colombia, before and after the reform.[7] 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, producer municipalities experienced a reduction in the share of royalties allocated from 72% to 10%. There is also 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. The combination of these sources of variation provides the justification for a differences in differences (DID) strategy combined with an instrumental variable (IV) design.[8]

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 compares the marginal effects of royalties on household welfare indicators, before and after 2011, year in which the reform on the royalties system was approved by the Colombian Congress. Although intuitive, this DID approach does not address the fact that royalties become more endogenous after the reform due to the introduction of the competitive mechanism described above, even after controlling for differential trends across producer and non-producer municipalities. Also, by allowing non-producer municipalities to compete for funds, the composition of treatment and control municipalities (endogenously) changes after the reform. Therefore, this strategy is improved with an IV approach where allocated rents are instrumented using international oil prices interacted with a measure of oil production in 1988.[9] This research design has the advantage of exploiting variation across producers, the group that

[2] Empirical approaches tend to use subnational variation to account for endogeneity (Caselli and Micheals, 2013; Brollo et al., 2013; Dube and Vargas, 2013), while theoretical work allows a clear understanding of the underlying mechanisms (Robinson et al., 2006, Caselli and Cunningham, 2009, Caselli, 2015).

[3] Scholarship exploiting within country variation show a more nuanced view regarding the resource curse (Allcott and Keniston, 2017; Aragon and Rud, 2013; Maldonado, 2017). The evidence tends to be mixed and a similar evaluation applies to the cross-country evidence (Van der Ploeg and Poelhekke, 2017).

[4] The predominant approach in studies about the institutional dimension of the resource curse is to treat institutions as exogenous, being Wiens (2014) a notable exception.

[5] See the discussion between "fast-moving" and "slow-moving" institutions in Roland (2004). Culture is an example of the latter whereas political institutions can be an example of the former. Our case is an example of change in a fast-moving institution.

[6] Indonesia, Ghana, Peru, Brazil, Bolivia, Canada, and Australia, for instance, 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 Jimenez (2012) for an overview.

[7] 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. Secondly, accountability mechanisms were incorporated via the introduction of monitoring and evaluation systems into the project cycles. Finally, access to royalties was extended beyond producer municipalities. We focus on this paper in this last element. A clean identification of all the aspects of the reform is difficult. Before the reform, most of the variation in rents was due to oil prices and quantities as allocation was mostly restricted to producers. After the reform, rents are also explained by endogenous differences in terms of the ability of designing project proposals, as any district can compete for funds. Evaluating the impact of the reform on the non-producer municipalities is of interest from a policy perspective, but we focus the analysis on the producer municipalities because they are the object of interest of the "resource curse" literature.

[8] In a standard DID design, the validity of the identification strategy depends on whether municipalities with high and low levels of royalties would have behaved similarly in the absence of the reform. However, it is possible that trends in pre-reform outcomes were different between producers and non-producers, as existing evidence on the negative impact of royalties suggests. Also, the fact that the reform includes a redistributive component implies that the standard parallel trends assumption is likely to be violated because members of the original control group (non-producer municipalities) become potential recipients of royalties after the reform. Therefore, non-producers may not longer be useful to recover the counterfactual of what would have happened with producer municipalities in the absence of the reform.

[9] A recent scholarship have used alternative ways to measure oil production (see, for instance, Van der Ploeg and Poelhekke, 2017 and Cust and Poelhekke, 2015 for an overview of these measurement issues). We make no particular claim regarding what the best measure is because we are aware of the limitations of all existing measures. Oil reserves and oil discoveries are endogenous to international prices (and potential anticipation effects) and depends

was affected the most by the reform in terms of changes in the level of royalties, and the ones who are expected to modify their behavior regarding the use of these royalties.

This design estimates a different parameter (a local average treatment effect—LATE) and exploits a source of variation that only affects producer municipalities. Whereas this alternative design does not allow us to incorporate the other components of the reform, it focuses the analysis on those municipalities who are expected to be negatively affected the most (in terms of revenue) by the changes in the allocation rule adopted with the reform.[10] This is important because, due to the progressive nature of the reform, most of the variation to be exploited in this study is due to changes in the allocation of royalties for producer municipalities.[11] 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. Although this strategy allow us to recover exogenous variation in the allocation of rents, it is important to emphasize that it does not fully exploit all the components modified by the reform. We rely on the interaction between a post-reform dummy and the instrumented royalties in this DID-IV design to take into account the aggregate effect of the reform across the producers.

We use different strategies to boost the credibility of our research design. We control for differential trends by including municipality and department-specific[12] linear trends in our main empirical specification.[13] We also rely on the exclusion restriction implied by this DID-IV strategy for identification along with providing suggestive evidence on the exogeneity of the reform. In the first case, the validity of the exclusion restriction is associated with the exogeneity of international oil prices. In the second one, a placebo test is implemented to evaluate whether the reform was designed with the goal of favoring members of the ruling political coalition. This is complemented with several robustness checks including controlling for migration, other sources of transfers from the Central government, non-linear effects of royalties, and whether the results are driven by municipalities that receive large amounts of royalties. 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 redistributive component 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. However, these effects seem to be larger for richer and urban households.

We map this improvement in well-being indicators to the provision of public goods and labor market externalities associated with the reform. After the reform was implemented, each additional COP invested in access to water, water quality and connection to an aqueduct has a higher marginal return for households than before the reform. We find similar results for social services, such as health and education, and also document labor market effects that suggest that the reform affected household economic opportunities beyond public good provision.

We also explore the role of state capacity as a mechanism that boosts the results described above. We find that in municipalities with higher levels of fiscal and administrative capacity, the effect of the redistributive component of the reform is stronger. We complement this result with some suggestive evidence that shows that higher levels of transparency and accountability do not explain the effects of the component of the reform under study.

We also run a set of placebo tests to evaluate the validity of our research design, described in full detail in the Online Appendix. We first analyze whether the reform was designed with the goal of benefiting municipalities where the local authorities belong to the same political coalition as the one in charge of designing and implementing it. We find no evidence on this regard. We also run a placebo test where other intergovernmental transfers are used instead of royalties to evaluate whether strategic responses from the Central Government were observed and find no evidence of impacts. The Online Appendix also includes alternative specifications with municipality and department specific linear trends, a distributive analysis of the impact of the reform and alternative estimators that are robust to the weak instruments problem. In the first case, we find that our main results are robust to relaxing the parallel trend assumption. In the second one, no evidence of distributive impacts. Finally, the basic results are maintained after implementing the LIML and Fuller's modified LIML with the alpha parameter equal to 1 and 4, suggesting that our results are robust to weak instruments.

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on institutional quality (Cust and Harding, 2017a). Current production is endogenous to prices and municipalities local economic and institutional characteristics. Although it is far from perfect, historic oil production is less troublesome because it cannot be affected by current institutional and socio-economic conditions as well as current oil prices. We will argue later why we believe this a credible source of exogenous variation in this setting.

[10] As we document below, there is a clear reduction in the amount of royalties allocated to oil producer municipalities. Royalties in oil municipalities went from COP 7650 millions on average before the reform, to COP 3510 millions on average after the reform. See Fig. 4 for details.

[11] Non-producer municipalities experienced a large increase in their budgets after the reform. As these windfalls are not related to the exploitation of natural resources in these municipalities, it is hard to interpret any of these effects under the umbrella of the "resource curse". Our identification strategy cannot shed light on the effects of the reform in these municipalities, but they are not relevant in the context of this paper.

[12] Departments is Colombia are analogous to States in the U.S.

[13] Standard solutions for the violation of the parallel trends assumption include the use of unit-specific trends or the use of matching on pre-treatment observables. Although both strategies have limitations (see Wolfers (2006) for the limitations of using trends and Chabe-Ferret (2017) on the limits of conditioning on pre-treatment outcomes), we choose the first one because implementing matching estimators with repeated cross-sections of household surveys is not straightforward.

Taking this evidence together, we find that these results are consistent with the idea that soft institutional reforms can contribute 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 sub-national variation. One strand of the literature has explored the impact of resource booms on the behavior of politicians with respect to electoral outcomes (Monteiro and Ferraz, 2012; Maldonado, 2017; and 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 (potentially) perverse economic and political effects of resource booms.[14]

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 exploitation takes place, as well as river and seaports through which production 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 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), to fulfill regional pension liabilities (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.[15] 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; and Bonet, 2007).[16]

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).

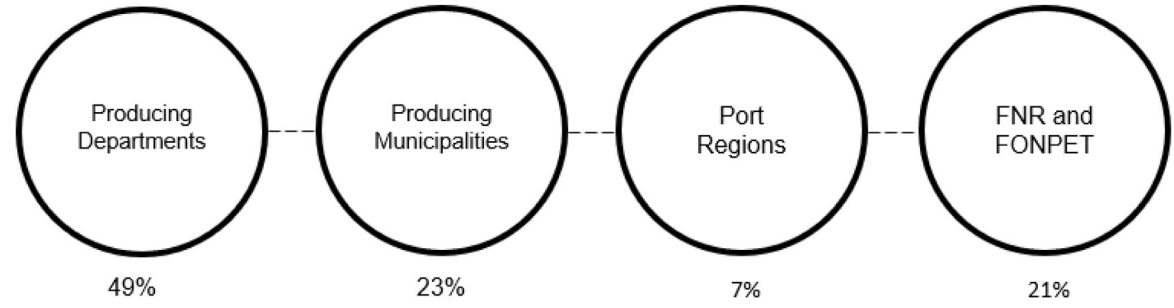
The reform introduced a set of new rules regarding the allocation of oil royalties. Several funds were created and competition based on project quality was introduced. All municipalities are able now to compete for funds, regardless of whether they are producers or not. Producer municipalities are still entitled to have access to direct royalties, but at a smaller share. Finally, a set of mechanisms to prevent malfeasance and monitoring projects were also incorporated. The organization of both the old and the new systems is depicted in Fig. 1.

The main element of the reform to be exploited in this paper is the reduction of the share of oil royalties of producer sub-national governments. Before the reform, departments and municipalities were oil extraction was taking place received 72% of total oil royalties. After the reform 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. As a consequence, the share of resources allocated to producer sub-national governments experienced a dramatic reduction.

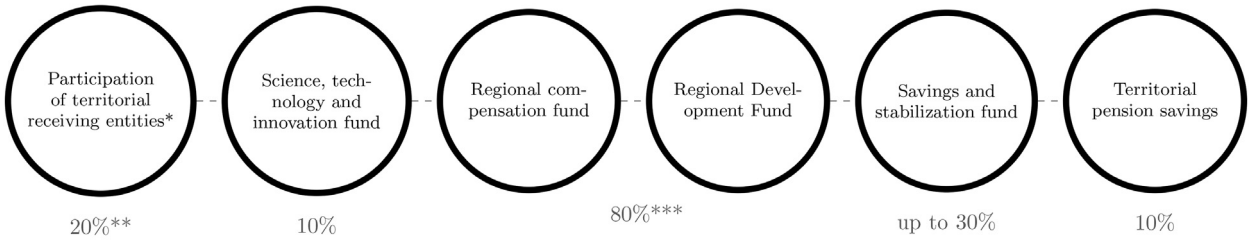
[14] Research has been conducted on the role of institutions in explaining the resource curse exploiting cross-country variation. Scholars have emphasized the role of political regimes, rule of law, quality of bureaucracies, and fiscal transparency and discipline (Van der Ploeg, 2011), among others.

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

[16] Examples 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.



(a) Before 2011



*The receiving entities (producers or with maritime or fluvial ports for the transport 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: Own elaboration based on Ministry of Finance and Public Credit data.

(b) After 2011

Fig. 1. Royalties System Before and After the 2011 Reform.

Fig. 2 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. On the other hand, Fig. 3 shows the evolution of oil production in Colombia and international oil prices for the 1997–2016 period, while Fig. 4 plots the evolution of royalties, non-competitive royalties, and potential revenues (historic production times international price) for oil-municipalities. It is noteworthy that, after 2011, there is a clear decrease in these three measures. For instance, royalties in oil municipalities went from COP 7650 millions on average before the reform, to COP 3510 millions on average after the reform.

In sum, the main component of the reform to be analyzed in this paper is the change in the allocation rule for producer municipalities. This change dramatically modified the amount of royalties these municipalities were entitled for. Fig. 5 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,[17] 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 below, the survey was originally launched in 1997 and includes several pre- and post- reform waves; second, the survey gathers data on important household-level welfare dimensions, such as education, health, housing conditions, transportation, labor, income, poverty,

[17] *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.

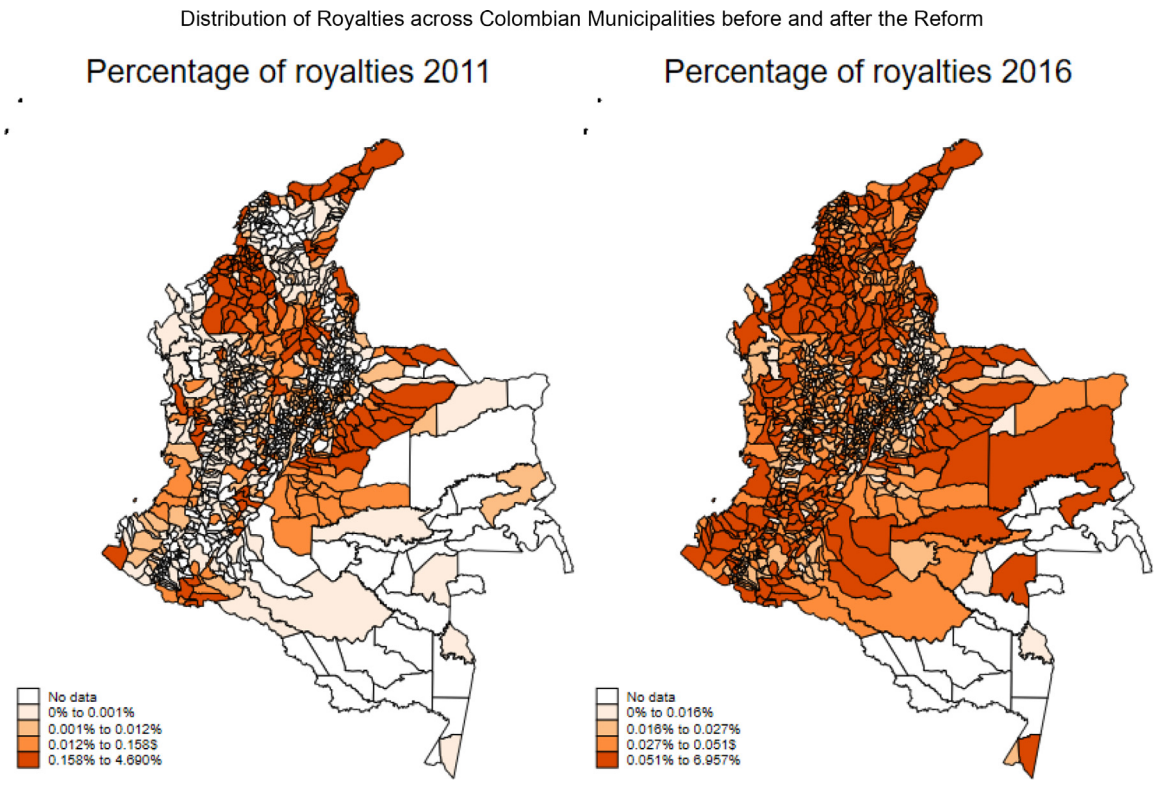


Fig. 2. This map shows the geographical distribution of royalties, measured as the percentage of total royalties received by the municipality in the corresponding year. The left map presents the distribution in 2011, just before the reform was approved. The right map shows this distribution in 2016, some years after its approval. Clearly, after the reform more municipalities receive royalties, no matter if they are producers or not.

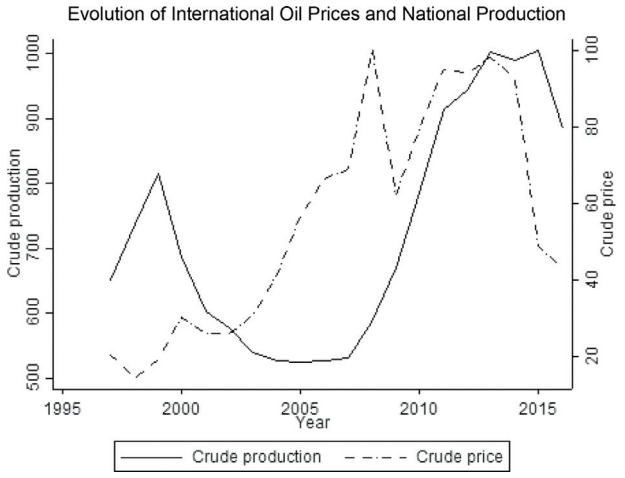


Fig. 3. This figure shows the evolution of oil production and international oil prices for the 1997–2016 period. Source: Authors' elaboration based on National Planning Department (DNP).

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 a combination of DID and IV research designs.

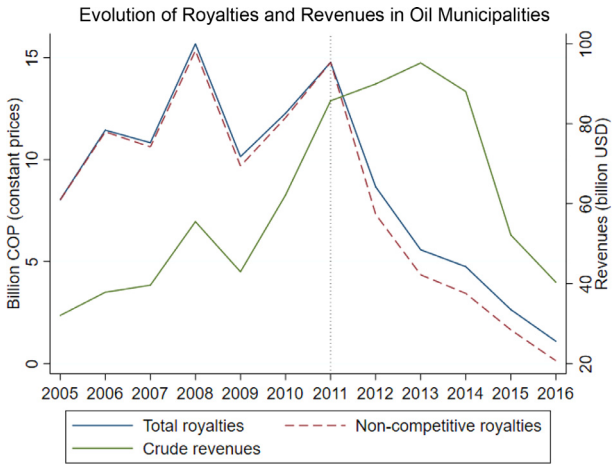


Fig. 4. This figure shows the evolution of total royalties, non-competitive royalties, and crude revenues for municipalities producing oil in 1988. Source: Authors' elaboration based on DNP.

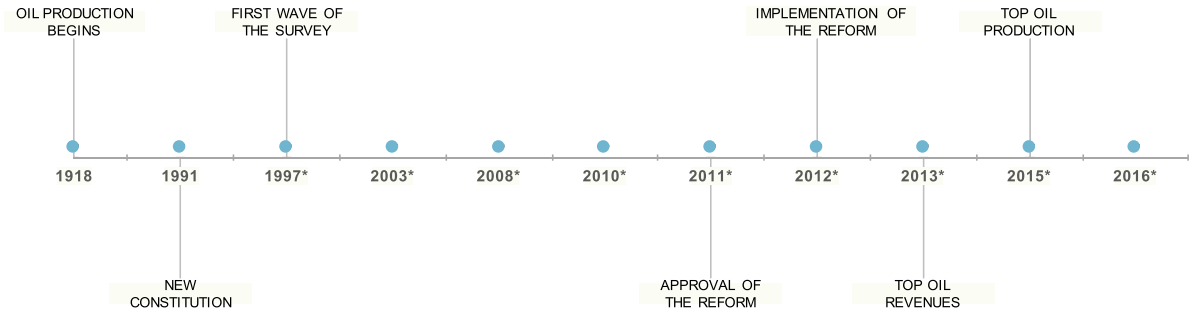


Fig. 5. Timeline of Events.

We accommodate both approaches in Two-Stage Least Squares (2SLS) estimations of the following form:

$$y_{imt} = \alpha_m + \beta_t + \widehat{Royalties}_{mt}\delta_1 + (\widehat{Royalties_{mt} \times Post2011_t})\delta_2 + \mathbf{X}_{imt}\phi + \mathbf{Z}_{mt}\eta + \varepsilon_{imt} \quad (1)$$

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 per capita royalties transfer, in hundred thousand Colombian Pesos (COP), to municipality m in year t;[18] $Post2011_t$ is a time dummy, indicating whether the observation corresponds to the post-reform period or not.[19] Note that in (1) we use estimations of royalties and its interaction with the time dummy, $\widehat{Royalties}_{mt}$ and $\widehat{Royalties_{mt} \times Post2011_t}$, 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 $\widehat{Royalties_{mt} \times Post2011_t}$, 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 change in the allocation rule for those municipalities that are affected by the exogenous variation in oil prices. This corresponds to a LATE for the producer municipalities. Positive and significant values of this coefficient mean that, compared to the pre-reform period, the marginal effect of each COP distributed in the form 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 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 o macroeconomic fluctuations. $\mathbf{X}_{imt}$ is a vector of household-level covariates, that include age and gender of the household head, household size, an

[18] All monetary values are expressed in 2010 Colombian Pesos.

[19] In 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.

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 DID-IV design in a 2SLS framework as $Royalties_{mt}$ might be an endogenous variable. This endogeneity is associated with production levels, prices and the allocation rules and the way these interact with economic and non-economic unobservables at municipality level. 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.[20] This variation is evident from Fig. 3. 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.[21] Therefore, in our basic specifications, the interaction between oil prices and the 1988 level of production constitutes our instrument for royalties. This structure is somewhat similar to the so-called Bartik instruments or shift-share designs, which have received a lot of attention in the econometric literature.[22] 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. Clearly, this interaction is not fully exogenous as the post-reform dummy can be endogenous as well. There is not an obvious solution to this issue as this dummy recovers the aggregate effect of the several changes that were implemented with the reform. We address this concern with a placebo test to analyze whether these changes were introduced with the purpose of benefiting municipalities with mayors from the same political coalition as the government that implemented the reform. Moreover, in the Online Appendix A.8, we discuss and relax the common trend assumption for our DID-IV especifications, using models that incorporate municipality and departament time trends.[23] Consequently, the first-stage of our model is of the form:

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

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

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 (Eq. 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 Eq. 1. Given the way we measure royalties, δ_1 represents the marginal effect on welfare of an additional COP 100,000 in royalties before the reform for municipalities that are affected by the exogenous variation in oil prices, while $\delta_1 + \delta_2$ is such effect after 2011. Hence, δ_2 represents the change in the marginal effect due to the reform for producer municipalities.

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 this problem, 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).[24] We also provide results for alternative estimators that have been

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

[21] Recent scholarship suggest that the location of oil production might be endogenous to institutions (Cust and Harding, 2017b). We are sympathetic with these arguments when it comes to using cross-country variation, but we are less convinced regarding the use of these arguments in research designs based on within-country variation where formal institutions do not change. Clearly, within-country variation in enforcement or municipality-level informal institutions can potentially play a role, but those are hard to measure dimensions. On the other hand, because oil exploration and exploitation depends on decisions being made by decision-makers at the national government, local decision makers are unlikely to play an important role in these decisions as well as the local institutions under which they operate.

[22] Recent scholarship has discussed the identification and inference issues surrounding this type of design. For the standard case where shares are used, scholars have shown that shares are required to be as good as randomly allocated conditional on the shocks and independent across municipalities (Goldsmith-Pinkham et al., 2018). Borusyak et al. (2018) also use a shift-share regressor as an instrumental variable whose validity depends on whether the set of shocks is as good as randomly assigned conditional on the shares. Finally, Adao et al. (2018) document that standard inference procedures tend to over-reject the null of no effect, given that regression residuals are correlated across municipalities with similar shares regardless of their geographical location. They propose an alternative inference approach that addresses this issue. We emphasize that these advances are consistent with our research design. The oil price shocks are exogenous to municipalities and production shares are mainly driven by geological factors because production decisions are motivated by costs considerations. This condition can be relaxed by requiring that, conditional on observables, production shares in 1988 are unrelated with unobservables that affect both the change in rents and outcomes after the reform.

[23] This approach has the advantage of allowing us to control for group dynamics (before and, more importantly, after treatment) that may be confounding the real impact of the reform through the inclusion of group-specific time trends (Mora and Reggio, 2019).

[24] We 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.

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 analysis.

4. Data

To evaluate the impact of change in the royalties' allocation rule 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 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.[25]

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. Table 1 presents descriptive statistics of the main variables to be used in this study, both before and after the reform.[26] Note that the number of observations varies for each variable, as some questions are omitted in specific waves of the survey. Additionally, Table A.9 in the Appendix reports summary statistics for royalties per capita and some selected outcomes, in the sample of households in oil-producing municipalities, both before and after the reform. As previously suggested by Fig. 4, there is a sharp (56%) reduction in royalties per capita for households in oil-producing municipalities after the reform. Fig. A.6, on the other hand, shows that most oil municipalities in the survey exhibit a decrease in royalties (the so-called *losers* of the reform), but that some places see an increase in this measure (the *winners*).[27]

The outcomes of interest for this paper come from this survey. We study the effect of the reform on the following variables: poverty, measured through the Multidimensional Poverty Index (MPI)[28], a self-reported dummy that indicates whether the family considers itself poor or not, and monetary poverty; household income; a housing deficit index;[29] 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 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. An inspection of Table 1 shows that the proportion of poor households, using the MPI, decreases substantially when we move from pre- to post-reform. Figs. A.6, A.7, and A.8 show similar patterns for other outcomes in oil-producing municipalities. The MPI is our preferred outcome, 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 well-being outcomes.

The data on royalties come from the Ministry of Finance and is published by 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.[30]

[25] It is important to clarify that the survey's sampling design is based on a descriptive approach, where one is interested in how the sample allows to recover a population parameter. The weights designed with this purpose are not relevant when the goal is to establish a causal relationship. In this scenario, scholars are interested in an analytical design, where the goal is to test a hypothesis regarding a difference in outcomes across groups defined by the treatment. Therefore, it is not recommended the use of descriptive weights in an analytical design, a practice we will follow in the rest of the paper.

[26] All these calculations are unweighted estimates. Due to the changes in the sampling design across years, we prefer this way to report the descriptive data.

[27] We collapse each measure at the municipality level and calculate the difference between averages before and after the reform.

[28] The MPI, developed by the Oxford Poverty and Human Development Initiative and the United Nations Development Program (UNDP), captures severe deprivations faced with respect to dimensions such as education, health, and living standards. Someone is poor if she is deprived in three or more of the ten dimensions aggregated in the index.

[29] We 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.

[30] While the source providing the information changes, the source producing it is the same. We acknowledge that this change may introduce measurement error. However, we believe that our empirical strategy deals with most of the problems that this change may introduce. On the one hand, the year fixed-effects that we include in our specifications should absorb measurement error under the assumption that it equally affects all municipalities. On the other

Table 1
Summary Statistics.

	Before the Reform					After the Reform				
Variable	Mean	Std. Dev.	Min.	Max.	N	Mean	Std. Dev.	Min.	Max.	N
Poverty Index	0.314	0.464	0	1	40,165	0.221	0.415	0	1	108987
Poverty Perception	0.565	0.496	0	1	51,285	0.462	0.499	0	1	86085
Household Income	1,374,161.193	2,248,575.32	0	94,216,664	40,165	1,422,647.559	2,444,388.623	0	224,427,168	108987
Monetary Poverty	0.31	0.462	0	1	76,725	0.295	0.456	0	1	86094
Housing Deficit Index	0.306	0.146	0.104	0.93	71,954	0.351	0.152	0.127	0.939	108968
Aqueduct Service	0.8	0.4	0	1	85,846	0.813	0.39	0	1	108987
Water Continuity	0.823	0.381	0	1	61,526	0.712	0.453	0	1	83768
Cellphone Service	0.699	0.459	0	1	85,846	0.941	0.236	0	1	108987
Computer at Home	0.258	0.437	0	1	58,017	0.311	0.463	0	1	108978
Internet Access	0.152	0.359	0	1	55,763	0.282	0.45	0	1	108967
Healthcare Access	0.875	0.331	0	1	85,813	0.972	0.166	0	1	108911
Illness	0.274	0.446	0	1	76,725	0.23	0.421	0	1	108987
Children Education	0.525	0.407	0	1	63,952	0.536	0.418	0	1	74704
Level of Education	3.578	1.489	1	8	83,087	3.644	1.414	1	8	105472
Years Approved	4.696	2.526	1	25	11,098	4.298	2.365	1	15	15960
Time to School	16.393	16.417	3	180	15,678	18.259	18.45	5	180	34868
Time to Work	26.336	30.563	0	600	57,574	22.919	27.066	0	240	71811
Security Perception	0.782	0.413	0	1	85,733	0.836	0.37	0	1	108967
Employment (HH Head)	0.825	0.38	0	1	85,846	0.789	0.408	0	1	102900
Work Contract	0.221	0.415	0	1	18,697	0.074	0.262	0	1	21065
Formal Job	0.207	0.405	0	1	70,835	0.251	0.433	0	1	64404
Construction Job	0.029	0.168	0	1	34,873	0.059	0.236	0	1	65198
Civil Work Job	0.003	0.051	0	1	34,873	0.001	0.027	0	1	65198
Agricultural Job	0.095	0.294	0	1	34,873	0	0.012	0	1	65198
Manufacturing Job	0.188	0.391	0	1	34,873	0.066	0.249	0	1	65198
Service Job	0.493	0.5	0	1	34,873	0.439	0.496	0	1	65198
Extreme Poverty	0.105	0.307	0	1	76,725	0.101	0.301	0	1	86094
Poverty Gap	379453.576	1462053.51	2.5	305,831,232	76,724	439942.932	1265361.814	8	149,766,464	86094
Age (HH Head)	47.58	15.621	11	104	85,846	49.137	16.005	12	104	108987
Gender (HH Head)	0.689	0.463	0	1	85,846	0.651	0.477	0	1	108987
Urban	0.646	0.478	0	1	85,846	0.616	0.486	0	1	108987
No. of Children	0.349	0.63	0	6	85,846	0.292	0.579	0	7	108987
Household Size	3.726	1.946	1	20	85,846	3.351	1.788	1	24	108987
Migration	0.543	0.498	0	1	70,804	0.682	0.466	0	1	108987
Royalties Per Capita (in 100,000 COP of 2010)	0.475	2.129	0	33.707	85,846	0.318	0.523	0	10.328	108987

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 are provided by the Administrative Department of National Statistics. Data on municipality investments by sector come from the National Planning Department, while mining and oil production data were provided by the Mines and Energy Ministry. The oil measure used to construct our instrumental variable corresponds to the "average number of barrels of crude oil produced per day in each municipality in 1988" as in Dube and Vargas (2013).[31]

5. Results

In this section, we present the main results of the empirical analysis based on the models described in Eq. 1. In each case, we run 2SLS regressions to determine the impact of the change in the allocation rule on a series of household-level indicators. Table 2 reports the results for a set of welfare outcomes, namely the poverty index,[32] a subjective measure of poverty,[33] monetary poverty, monthly household income, and an index of housing deficit, that measures the quality and

hand, endogeneity issues would arise if municipalities are differentially affected by the change, i.e. in such a way that measurement error is correlated with unobservable household characteristics. However, our IV approach should take care of these endogeneity issues.

[31] A total of 39 municipalities produced oil in Colombia in 1988.

[32] As 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.

[33] Respondents are asked whether they consider themselves poor or not.

Table 2
Effect of the Reform on Welfare Indicators (IV Estimations).

	(1) Poverty Index	(2) Poverty Index	(3) Poverty Perception	(4) Poverty Perception	(5) Household Income	(6) Household Income	(7) Monetary Poverty	(8) Monetary Poverty	(9) Housing Deficit Index	(10) Housing Deficit Index
Panel A										
Royalties	0.004	−0.005	−0.098***	−0.127***	14371.1	−80829.5	−0.005	−0.007	−0.023**	−0.018***
	(0.006)	(0.008)	(0.022)	(0.029)	(63898.7)	(105301.5)	(0.054)	(0.046)	(0.010)	(0.006)
Royalties × Post2011	−0.019***	−0.009***	−0.155***	−0.167***	−51924.7*	129054.3***	0.012	−0.001	−0.043**	−0.026***
	(0.005)	(0.003)	(0.031)	(0.036)	(27207.3)	(44651.1)	(0.073)	(0.060)	(0.018)	(0.009)
Post2011	−0.092***	−0.092***	−0.059***	−0.109***	−107705.5*	10362.6	−0.086***	−0.073***	0.140***	0.103***
	(0.011)	(0.012)	(0.020)	(0.027)	(61157.2)	(72513.4)	(0.018)	(0.014)	(0.015)	(0.012)
SW F-Stat 1	922.80***	1163.22***	26.55***	23.62***	922.80***	1163.22***	4.04**	3.61*	13.39***	9.85***
SW F-Stat 2	1588.87***	943.26***	52.46	58.33***	1588.87***	943.26***	7.30***	7.05***	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	0.867	0.991	0.017	0.007
Benjamini & Hochberg	0.1	0.15	0.05	0.06	0.19	0.14	0.25	0.25	0.16	0.14
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
N	127,769	112,968	103,831	88,942	127,769	112,968	114,350	132,466	147,383	132,466

Notes: Standard errors clustered at the municipality level are shown in parentheses. Year 2012 has been excluded from each estimation. *Royalties* is the amount of royalties, in hundreds of thousands of 2010 Colombian pesos, allocated to the municipality where the household lives. *Post*2011 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.

conditions of the respondent's house.[34] In each case, we estimate models with and without household-level covariates.[35] Municipality-level covariates and fixed effects, as well as year fixed effects, are included in all regressions.[36]

5.1. Welfare indicators: Poverty, income, health, and education

In columns 1 and 2 of Table 2, 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* × *Post*2011, which measures the change in the marginal effect of royalties when we move from the old allocation rule (pre-2011) to the new allocation rule (post-2011) for municipalities affected by the exogenous variation in oil prices. This coefficient, in both cases, is negative and significant. Hence, after the change in the allocation rule, every additional Peso of royalties allocated to the municipality where the household lives, reduces its probability of being poor. The coefficient in column 1, for instance, reveals that after 2011 for every additional COP 100,000 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 old allocation rule period, in producer municipalities.

In substantive terms, after the change in the allocation rule, the marginal effect of royalties on poverty improves for producer municipalities. 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 change in the allocation rule in producer municipalities, is quite small and statistically insignificant. Such result would support the claim that before the allocation rule change, natural resource rents were not instrumental in improving well-being.[37]

We also find significant effects if we use a subjective measure of poverty. Columns 3 and 4 reveal that the allocation rule change also had a negative effect on this variable. Column 3 suggests that after the allocation rule change, the effect of royalties on the poverty self-report measure is 16 percentage higher in absolute terms. Again, this is a LATE defined for municipalities with oil production. Columns 5 and 6 show that the results on income are mixed, although our preferred specification—the one that includes household-level controls—suggests that each marginal COP after the allocation

[34] The index was constructed using principal components analysis, using several traits of the house.

[35] All regressions are unweighted, following the common practice. The use of weights is an issue of debate. Following Solon et al. (2015), we find no reasons to use weights in our setting. These authors suggest to use weights to improve precision by correcting by heterokedasticity and the computation of partial average effects in the presence of treatment effect heterogeneity, none of them relevant here. Another reason is endogenous sampling, which is unlikely to play a role since the oversampling of socioeconomic groups was not based on factors related to oil production or producer districts' characteristics. As stressed by the authors, weighting in this case is even harmful because it reduces precision.

[36] Tables A32–A37 in Appendix report the first-stage results of the main specifications presented here.

[37] As detailed below, there is mixed evidence concerning the existence of the resource curse before the reform when restricting to the different outcomes we analyze. Tables A41–A43 in the Appendix compare oil and non-oil municipalities before the allocation rule change. Results show that, in general, producers were not better off, and in many cases fared worse in terms of welfare, than non-producers.

rule change is positively and significantly related with income. Hence, the change in the allocation rule had positive effects on these objective, subjective, and monetary measures of well-being.[38]

It is somehow surprising that, despite encountering a positive effect on income, the allocation rule change has a null effect on monetary poverty—which indicates if the household is below the poverty line. As shown in columns 7 and 8,[39] these results suggest that the effect on income is distributionally skewed. In Appendix Section A.10, we delve deeper into the reasons underlying this discrepancy. Three additional results support the hypothesis that some of the positive effects of the allocation rule change may be distributed unevenly between rich and poor households. First, Table A29 shows that the allocation rule change moves non-poor families further away from the poverty line, without closing the gap to this threshold for poor households. Second, Table A30 shows that the positive effect on income is mainly driven by rich households, namely those above the 75th percentile of the income distribution. Finally, Table A.39 reveals that the effect of the allocation rule change on multidimensional and self-reported poverty is higher in urban areas compared to in rural areas, where baseline poverty levels tend to be lower.

Statistically significant impacts are also found when examining housing conditions. Columns 7 and 8 of Table 2 show that the allocation rule change 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 allocation rule change, every additional COP 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 allocation rule change.

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 2 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.

Given that our models follow a DID-IV approach, we test for the plausibility of the common trend assumption. For this purpose, following Angrist and Pischke (2009) and Wing et al. (2018), we estimate models that incorporate either municipality or department time trends.[40] We rely on this approach, rather than using the more conventional models with time-varying treatment effects, due to multicollinearity issues when implementing the latter. We discuss and report the results of these models in the Online Appendix Section A.8. In general, Table A25 shows that our results are robust to the inclusion of these trends, which enhances the credibility of our identification strategy.[41]

In Table 3, we report the effects of the change in the allocation rule 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 allocation rule change the marginal effect of royalties on the probability of having access to the aqueduct service is higher. Every additional COP 100,000 per capita after the allocation rule change represents an 8 percentage point increase in this probability for the case of municipalities with oil production. 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.[42] Several of the new projects have enabled the construction of new water infrastructure and the improvement of existent aqueducts.

It is important to acknowledge that there are no effects on access to other public services, such as electricity or sewage (results available upon request). In the case of natural gas service, the effect is even negative, which is in line with the fact that several gas projects encountered implementation problems and certain regions presented strong price rises. Nonetheless, the allocation rule change has effects on other important privately provided services. For example, Columns 5 and 6 show that after the change in the allocation rule, the marginal positive effect of royalties on the probability of having a cell phone is about 7 percentage points higher. 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.

Table 4 presents the results for important welfare indicators associated with health and education. Columns 1 and 2 show that the change in the allocation rule has a positive effect on access to the healthcare system—the impact is of approximately 8 percentage points. This result is not surprising, as several projects aim to improve healthcare conditions. We also find effects on an another important health outcome. Columns 3 and 4 show that after the change in the allocation rule, every additional COP 100,000 per capita reduces the likelihood of self-reported illness by about 9 percentage points in producer

[38] Table A29 in the Appendix reports the results for alternative monetary measures of poverty.

[39] The same result is encountered for an equivalent monetary poverty measure—the indicator of whether the household is below the extreme poverty line. See Appendix Section A.10 for details.

[40] Autor (2003) and Besley and Burgess (2004) follow similar approaches.

[41] Tables A.34, A.35, A.36 report the results of non-IV DID versions of these models. However, these results should be interpreted with care as it is hard to give a causal interpretation to these estimates.

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

Table 3
Effect of the Reform on Housing Indicators (IV Estimations).

	(1) Aqueduct Service	(2) Aqueduct Service	(3) Water Continuity	(4) Water Continuity	(5) Cellphone Service	(6) Cellphone Service	(7) Computer at Home	(8) Computer at Home	(9) Internet Access	(10) Internet Access
Panel A										
Royalties	0.050**	0.040**	0.110***	0.105***	0.051***	0.050***	−0.034***	−0.042***	−0.011**	−0.017***
	(0.025)	(0.019)	(0.031)	(0.033)	(0.013)	(0.014)	(0.005)	(0.006)	(0.004)	(0.004)
Royalties × Post2011	0.077*	0.066**	0.272***	0.268***	0.066***	0.064***	0.009	0.032***	0.026**	0.045***
	(0.043)	(0.032)	(0.040)	(0.041)	(0.019)	(0.018)	(0.007)	(0.009)	(0.011)	(0.012)
Post2011	0.026	0.017	0.189***	0.199***	−0.039*	0.003	−0.446***	−0.333***	0.205***	0.230***
	(0.048)	(0.054)	(0.073)	(0.075)	(0.021)	(0.022)	(0.051)	(0.057)	(0.017)	(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	0.19	0.18	0.02	0.03	0.12	0.11	0.21	0.13	0.15	0.1
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	161,293	146,335	117,213	106,028	161,293	146,335	142,478	127,629	141,735	126,893

Notes: Standard errors clustered at the municipality level are shown in parentheses. Year 2012 has been excluded from each estimation. *Royalties* is the amount of royalties, in hundreds of thousands of 2010 Colombian pesos, allocated to the municipality where the household lives. *Post*2011 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 Health and Education Indicators (IV Estimations).

	(1) Healthcare Access	(2) Healthcare Access	(3) Illness	(4) Illness	(5) Children Education	(6) Children Education	(7) Level of Education	(8) Level of Education	(9) Years Approved	(10) 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	0.17	0.18	0.03	0.05	0.07	0.11	0.24	0.23	0.16	0.20
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
N	161,194	146,249	152,172	137,243	161,293	146,335	156,178	141,679	20,674	18,775

Notes: Standard errors clustered at the municipality level are shown in parentheses. Year 2012 has been excluded from each estimation. *Royalties* is the amount of royalties, in hundreds of thousands of 2010 Colombian pesos, allocated to the municipality where the household lives. *Post*2011 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.

municipalities. It is important to remember that some of the projects funded using royalties, after the allocation rule change, include the construction of new hospitals and the improvement of existent ones. Additionally, if the change in the allocation rule 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 for municipalities with oil production. 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

Table 5
Effect of the Reform on other Welfare Indicators (IV Estimations).

	(1) Time to School	(2) Time to School	(3) Time to Work	(4) Time to Work	(5) Security Perception	(6) Security Perception	(7) Employment (HH Head)	(8) Employment (HH Head)
Panel A								
Royalties	0.335**	0.296*	−2.524***	−1.753***	0.114***	0.095**	0.043***	0.059***
	(0.171)	(0.168)	(0.570)	(0.476)	(0.040)	(0.037)	(0.010)	(0.019)
Royalties × Post2011	−1.777***	−1.002*	−5.241***	−4.883***	0.223***	0.201***	0.021	0.046**
	(0.506)	(0.592)	(0.877)	(0.784)	(0.056)	(0.051)	(0.017)	(0.022)
Post2011	0.681	1.109*	−1.873	−1.154	0.117***	0.117***	0.257***	0.258***
	(0.509)	(0.568)	(1.512)	(1.543)	(0.038)	(0.034)	(0.019)	(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	0.12	0.2	0.04	0.04	0.08	0.08	0.22	0.17
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	50,546	49,331	107,795	97,844	161,161	146,243	155,206	140,248

Notes: Standard errors clustered at the municipality level are shown in parentheses. Year 2012 has been excluded from each estimation. *Royalties* is the amount of royalties, in hundreds of thousands of 2010 Colombian pesos, allocated to the municipality where the household lives. *Post*2011 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.

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 change in the allocation rule, the most popular dimension so far has been the construction of roads.[43] 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 recently, increasing communication among beneficiaries. Even though the survey 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 allocation rule change 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 5 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 change in the allocation rule. 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 each additional COP transferred to producer municipalities after the change in the allocation rule is related with 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. 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 change in the allocation rule on this outcome is huge: more than 20 percentage points for every additional COP 100,000 in royalties in municipalities with oil production. Finally, column 8—which represents our preferred specification as it includes household-level controls—reveals one of the most important results of the change in the allocation rule: the effect on employment is positive and significant, which implies that the allocation rule change contributed to the creation of new jobs.[44] We now analyze if there are any distributional effects on employment.

5.2. Labor indicators: Employment, formality, and development

Given the nature of the projects funded through these rents, one should expect labor shifts across different sectors. 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 6 report a positive and significant effect on the

[43] Transport represented 32% of total investments funded by the General System of Royalties for the period 2012–2015. See: https://www.sgr.gov.co/LinkClick.aspx?fileticket=-uDHxQYXLmo%3D&tabid=320.

[44] As before, we find that these results are robust to controlling for multiple comparisons.

Table 6
Effect of the Reform on Labor Indicators (IV Estimations).

	(1) Work Contract	(2) Work Contract	(3) Formal Job	(4) Formal Job	(5) Construction Job	(6) Construction Job	(7) Civil Work Job	(8) Civil Work Job
Panel A								
Royalties	−0.019***	−0.019**	0.136	0.127	−0.010***	−0.012***	0.001***	0.001***
	(0.007)	(0.008)	(0.135)	(0.135)	(0.002)	(0.002)	(0.000)	(0.000)
Royalties × Post2011	0.089***	0.126***	0.216	0.213	0.024***	0.023***	0.001***	0.001***
	(0.013)	(0.008)	(0.208)	(0.208)	(0.003)	(0.003)	(0.000)	(0.000)
Post2011	−0.026	−0.020	0.010	0.062	−0.007	−0.008	−0.002***	−0.002***
	(0.018)	(0.022)	(0.093)	(0.073)	(0.009)	(0.011)	(0.001)	(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	0.02	0.0	0.22	0.22	0.01	0.0	0.13	0.13
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
N	39,762	34,917	110,468	99,129	91,290	91,172	91,290	91,172

Notes: Standard errors clustered at the municipality level are shown in parentheses. Year 2012 has been excluded from each estimation. *Royalties* is the amount of royalties, in hundreds of thousands of 2010 Colombian pesos, allocated to the municipality where the household lives. *Post*2011 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.

likelihood of having a work contract. However, 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. Additionally, columns 5 and 6 (7 and 8) show that the effect of the change in the allocation rule on the probability of being employed in the construction (civil work) sector is positive and significant.

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. Our data 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. Columns 1 and 2 of Table 7 show that the effect of the change in the allocation rule on the probability of being employed in agriculture is negative and significant while the effect is positive and significant 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.[45]

5.3. Heterogeneous effects: Accountability, planning, and fiscal capacity

The institutional reform that changed the allocation rule of resource rents in Colombia focused on three important dimensions of governance: the way in which investments are monitored and held accountable, the incentives that local authorities have to plan and formulate their projects, and the access to royalties granted to different types of municipalities. Although we are not able to evaluate the role of these dimensions for the reasons discussed above, in this subsection we test whether pre-treatment levels of governance can shed some light about the positive effects of the change in the allocation rule. In other words, we evaluate whether the estimated effects are driven by dimensions of local state capacity.

Because the reform implied the introduction of several changes to the pre-reform allocation rules, it is hard to isolate the role of these three dimensions of governance in explaining the results described above. Moreover, data on project planning and accountability are not available for the pre-reform period. Therefore, in this section we provide some tentative results

[45] In fact, a recent reform to the system modifies the allocation of science and technology resources.

Table 7
Effect of the Reform on Labor across Sectors (IV Estimations).

	(1) Agricultural Sector	(2) Agricultural Sector	(3) Manufacturing Sector	(4) Manufacturing Sector	(5) Service Sector	(6) Service Sector
Panel A						
Royalties	−0.115***	−0.115***	0.039***	0.035***	0.053***	0.056***
	(0.024)	(0.023)	(0.008)	(0.008)	(0.019)	(0.019)
Royalties × Post2011	−0.137***	−0.137***	0.070***	0.072***	−0.022	−0.036
	(0.035)	(0.033)	(0.018)	(0.017)	(0.030)	(0.028)
Post2011	−0.064**	−0.063**	−0.300***	−0.264***	−0.389***	−0.445***
	(0.031)	(0.032)	(0.057)	(0.055)	(0.047)	(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	0.09	0.07	0.09	0.06	0.24	0.21
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
N	91,290	91,172	91,290	91,172	91,290	91,172

Notes: Standard errors clustered at the municipality level are shown in parentheses. Year 2012 has been excluded from each estimation. *Royalties* is the amount of royalties, in hundreds of thousands of 2010 Colombian pesos, allocated to the municipality where the household lives. *Post*2011 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.

for the mechanisms discussed above by looking at pre-treatment proxies of those dimensions emphasized by the reform.[46] For this purpose, we use pre-reform municipality-level proxies for state capacity, in general, and for transparency and accountability, administrative capacity to plan projects, and fiscal capacity, in particular. First, as a proxy for state capacity we use a municipality-level index that was created by the National Planning Department (DNP) in 2005. The Overall Performance Index (IDI, in Spanish)[47] captures municipalities' capacities on four important dimensions: Efficacy,[48] efficiency,[49] management,[50] and legal requirements.[51] This index has been used to rank municipalities in terms of state capacity. Therefore, we use the variable $Capacity_m$, which corresponds to the realization of this index for municipality m in 2005, as our pre-reform measure of state capacity.

Second, and in order to capture the accountability dimension, we use the Open Government Index (IGA, in Spanish),[52] constructed since 2010 by the Office of the Inspector General of Colombia,[53] to measure the fulfillment of norms and reports considered strategic in order to prevent corruption and inefficiency in public management. Consequently, $Accountability_m$ corresponds to municipality m's realization of the Open Government Index in 2010. Third, as a proxy for the planning quality of projects, we use one of the dimensions of the Overall Performance Index, namely the administrative capacity of municipalities. This dimension, measured by DNP, captures the disposition of human, physical, and technological resources that support processes and procedures of subnational entities, like the planning and formulation of projects. As a matter of fact, this index is composed of measures such as personnel stability and profesionnalization or the availability of technological tools within public organizations. We construct a variable called $Administrative_m$, which corresponds to the 2005 measure of administrative capacity in municipality m. Finally, to test the fiscal capacity hypothesis, we use the Fiscal Performance Index, created by the DNP to evaluate municipalities in several dimensions of fiscal capacity such as: the ability to raise taxes, investment levels, savings, financial solvency, among others. We construct the variable $Fiscal_m$, which corresponds to the 2005 measure of fiscal capacity in municipality m.

[46] Notice that this exercise is not meant to be an analysis of the mechanism of the reform because of data constraints and lack of proper sources of variation to identify the effects. Rather than evaluating how the components of the reform affected well-being, we explore how municipalities with different pre-treatment levels of dimensions related to the mechanisms of the reform responded to the treatment.

[47] Indice de Desempeño Integral in Spanish.

[48] This dimension measures the degree of fulfillment of development plans goals.

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

[50] Quantifies the effect of management and financial variables on efficacy and efficiency outcomes.

[51] Measures whether municipalities fulfill conditions and requirements imposed by formal rules.

[52] Indice de Gobierno Abierto.

[53] See http://www.anticorrupcion.gov.co/Paginas/indice-gobierno-abierto.aspx

Table 8
Heterogeneous Effects across Mechanisms of the Reform (IV Estimations and Selected Outcomes).

	(1) Poverty Index	(2) Poverty Index	(3) Poverty Perception	(4) Poverty Perception	(5) Household Income	(6) Household Income	(7) Housing Deficit Index	(8) Housing Deficit Index
Panel A: State Capacity								
Royalties × Post2011 × Capacity	−0.000380***	−0.000276*	−0.00375***	−0.00396***	−409.9	3539.7***	−0.00114***	−0.000663***
	(0.000134)	(0.000156)	(0.000833)	(0.000587)	(915.7)	(726.6)	(0.000217)	(0.000149)
Panel B: Fiscal Capacity								
Royalties × Post2011 × Fiscal	−0.000256***	−0.000144	−0.00299***	−0.00328***	−1576.1***	1511.9***	−0.000964***	−0.000508***
	(0.0000929)	(0.000111)	(0.000476)	(0.000369)	(610.6)	(531.8)	(0.000178)	(0.000124)
Panel C: Administrative Capacity								
Royalties × Post2011 × Administrative	−0.000405**	−0.000256	−0.00184***	−0.00185***	317.5	2963.7**	−0.000476**	−0.000261***
	(0.000162)	(0.000168)	(0.000444)	(0.000543)	(1356.0)	(1277.1)	(0.000192)	(0.0000812)
Panel D: Transparency and Accountability								
Royalties × Post2011 × Accountability	−0.000524*	−0.000544	−0.00150	−0.00119	2151.1	5668.7	−0.000336	−0.000158
	(0.000305)	(0.000410)	(0.00139)	(0.00253)	(2845.0)	(3881.4)	(0.000412)	(0.000281)
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	127,769	112,968	103,831	88,942	127,769	112,968	147,383	132,466

Notes: Standard errors clustered at the municipality level are shown in parentheses. Year 2012 has been excluded from each estimation. *Royalties* is the amount of royalties, in hundreds of thousands of 2010 Colombian pesos, allocated to the municipality where the household lives. *Post*2011 equals 1 for observations beyond year 2011 and 0 otherwise. *Capacity* is the 2005 Overall Performance Index. *Fiscal* is the 2005 Fiscal Performance Index. *Administrative* is the administrative capacity dimension of the 2005 Overall Performance Index. *Accountability* is the 2010 Open Government Index. 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.

With these measures in hand, we estimate 2SLS models of the form:

$$y_{imt} = \alpha_m + \beta_t + \widehat{Royalties}_{mt}\delta_1 + (\widehat{Royalties_{mt} \times Post2011_t})\delta_2 + (\widehat{Royalties_{mt}} \times Mec_m)\delta_3 + (Mec_m \times Post2011_t)\delta_4 + (Royalties_{mt} \times \widehat{Mec_m} \times Post2011_t)\delta_5 + \mathbf{X}_{imt}\phi + \mathbf{Z}_{mt}\eta + \varepsilon_{imt}$$

where Mec_m is any of the proxies used for our proposed mechanisms, $Capacity_m$, $Accountability_m$, $Administrative_m$, and $Fiscal_m$. Note that Mec_m is a pre-reform cross-sectional characteristic of the municipality where household i lives. Also, note that some of the constituent terms of the interactions in this equation are absorbed either by the time effects or the municipality fixed effects. In what follows, and for the sake of brevity, we restrict this analysis to the outcomes considered in Table 2.[54] In fact, the multidimensional poverty index results from the aggregation of several of the dimensions studied in this paper, and consequently, is our preferred outcome. The coefficient of interest in these specifications is δ_5, the parameter associated to the triple interaction between royalties, the post-reform dummy, and the mechanism at hand. δ_5 shows whether the effect of the reform on the marginal effect of royalties varies across the dimensions described above. One final caveat: every term that contains the variable $Royalties_{mt}$ is instrumented through $Oil_m^{1988} \times Price_t$ and its corresponding interaction. For instance, triple interactions of the form $Royalties_{mt} \times Mec_m \times Post2011_t$ are instrumented through the terms $Oil_m^{1988} \times Price_t \times Mec_m \times Post2011_t$.

We report the results of these specifications in Table 8. The coefficients of the constituent terms of the interactions are not included in order to ease the inspection of the table. Panel A reports the results of models in which we test for heterogenous treatment effects on state capacity, as measured by the Overall Performance Index, on the four welfare outcomes of interest. The results show that the positive effects of the change in the allocation rule on welfare are stronger in municipalities with higher pre-reform levels of state capacity. For instance, the decrease in the probability of being poor caused by every additional peso in royalties after the reform, is higher in places with stronger states.

Moreover, we are to able to disentangle these effects across the different dimensions of state capacity that were targeted by the reform, i.e. fiscal capacity, administrative capacity, and transparency and accountability. Panel B shows the existence of robust heterogenous effects for fiscal capacity. Heterogeneous effects are also found for administrative capacity. Finally, no effects are found in the case of transparency and accountability. Consequently, we conclude that the effects of the change in the allocation rule introduced by the reform were boosted by the pre-reform levels of fiscal and administrative capacity of Colombian municipalities. Despite the efforts to enhance the monitoring and accountability mechanisms of the system, it is somehow surprising that no differential effects are found on this dimension.

[54] I.e. the multidimensional poverty index, poverty perception, household income, and the housing deficit index. Results are consistent for the rest of outcomes and are available upon request.

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 on household well-being. 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), but it is not completely clear if these changes are a direct result of production or of the way rents are spent. For example, 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 Sections 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 change in the allocation rule 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 our story 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 Spanish[55]—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 re-estimate all of our models, but instead 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 change in the allocation rule that took place in 2011. If other factors—different to the reform—are affecting the revenues after 2011, or if the change in the allocation rule 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.

However, Tables A2–A7 in the Appendix show that this is not the case.[56] The results of the placebo tests reveal that, in general, there are no differential changes in the marginal effects of SGP transfers or municipalities' own revenues 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 their own income.

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.[57] 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 A8–A10 in the Appendix report the results of these estimations, for our set of outcomes. We do find significant heterogeneous effects for an important number of outcomes. However, in general, the sign of the 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.

[55] In Spanish, *Sistema General de Participaciones.*

[56] Tables A38–A40 in Appendix report the first-stage results of these models.

[57] This coalition included the following parties: Liberal, Conservador, U, and Cambio Radical.

We also test for distributional treatment effects among municipalities with high and low level of royalties, given that this trait may have also affected the approval timing of the reform. For this purpose, we construct a dummy variable called $HighRecipient_m$, 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 A11–A12 in the Online Appendix show that, in general, the change in the allocation rule has no differential effects across households living in producer municipalities with high and low levels of royalties. Similarly, Tables A14–A16 shows that there are no heterogeneous effects between households living in municipalities exhibiting an increase in royalties post-reform, compared to those exhibiting a decrease.

The former result is important as it helps us attenuate the concern that the positive effects of the change in the allocation rule are driven by the fact that oil municipalities receive less transfers after the reform and that royalties may have a non-linear effect on welfare. To further rule out this alternative mechanism, we estimate parametric models to test for non-linear effects of royalties, through quadratic, logarithmic, and inverse hyperbolic sine (IHS) specifications. If the positive effects on welfare were driven by the reduction in royalties for oil municipalities, we should see evidence for non-linear effects. Table A20 in the Appendix show that, in general, there is no evidence supporting this hypothesis. For the majority of our estimations, the coefficients for the quadratic, logarithmic and IHS terms are not significant.

Finally, we consider alternative estimators robust to the weak instrument problem. In particular, we estimate the LIML and the Fuller's modified LIML for alpha parameters of 1 and 4. These estimators have better finite sample properties, a problem with the standard IV estimator. Table A21 in the Online Appendix present the results using the poverty index as the outcome of interest. Results are unchanged. Tables A22, A23, and A24 present the results for alternative outcomes. Again, the result are maintained.

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 a change in the allocation rule implemented during a reform in the royalties' system 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 different measures of well-being. 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 the intended effects on sectors such as roads, education, healthcare, etc. In addition, after the reform, investments induce shifts on employment, both in terms of quality and distribution across sectors. Each additional COP after the change in the allocation rule has a positive impact on the probability of working with contract and on the shift from the agricultural to the manufacturing sector.

However, the reform is far from perfect. The economic effects of the change in the allocation rule are not necessarily large with respect to specific dimensions. In some other cases, they are null or negative. For instance, we observe that the impact 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.

Additionally, 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. Therefore, it is safe to conclude that the effects of the change in the allocation rule 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.

Declaration of Competing Interest

The authors declares that they have no relevant or material financial interests that relate to the research described in this paper.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2020.07.006.

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