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The labor market worsening effects of a resource bust: Evidence from the crude oil price shock in Ecuador

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The Labor Market Worsening Effects of a Resource Bust: Evidence from the Crude Oil Price Shock in Ecuador*

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Abstract

To assess the effects of an oil price bust on individual labor market outcomes, we leverage the 2015 exogenous decline in international oil prices with geographical variation in oil-dependency in Ecuador. To account for propagation mechanisms, we also test the causal effect of the oil price bust on public transfers to local autonomous governments. Reduced form results suggest a moderate oil price pass-through channel on wages and nonlabor earnings but not on labor supply and participation. Public transfers play an amplification role, as a one percentage point decrease in these funds implies workers in oil-dependent areas to experience a wage reduction of 1.5%. Spillover effects to nonextractive industries, with reduced economic activity at the firm level, seem to be the transmission channels explaining the drop in individual earnings during the oil price bust.

Keywords: Resource bust, wages, employment, oil, public transfers, Ecuador.

JEL Codes: J21, J22, J30, H41, H72, I31

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1 Introduction

To what extent price busts of nonrenewable natural resources and other commodities deteriorate labor market outcomes in developing economies is a concern that steadily has gained attention from academics and policymakers alike. Importantly, accounting for such effects is urgent as similar price busts are likely to repeat in the near future. Regarding crude oil and their derivatives as an example, it is expected that at least a third of global oil reserves should remain unused between 2010 to 2050, in order to keep global warming below a 2°C pre-industrial times threshold (McGlade and Ekins, 2015; Welsby et al., 2021). Given the worldwide, inevitable tendency to replace fossil-originated sources of energy for renewable, environmentally sustainable alternatives, it is of great importance to evaluate how a significant decline in international crude oil prices affect labor market outcomes and how such impacts differ across demographic groups, industry sectors and occupations. This is particularly relevant for developing countries, as their fiscal capacity and public goods provision has strongly depended on revenues from the oil industry.

In order to assess the impact of a negative crude oil price shock on local labor market outcomes, this paper seeks to provide causal evidence on two dimensions. First, we present reduced form estimates of the crude oil price pass-through effect into wages, earnings, labor supply, and labor market participation. Second, we provide estimates on the effects of a decline in crude oil proceeds on labor market outcomes at the local level. Specifically, we examine whether exogenous changes in the redistribution of oil royalties impact labor market outcomes of those individuals residing in local labor markets with a high dependency on such revenues.

This study focuses on Ecuador during the period 2011-2014, as it provides a great opportunity to evaluate our research question. Historically, oil production has taken place in cantons localized in the Amazon region.¹By a constitutional mandate in 2011, the central government dictated that royalties will be collected from oil production, at a rate of US\$1 per barrel produced. This proceeds fund, known as “Fondo para el Ecodesarrollo Regional Amazonico”, or *Ley 010*, also establishes that cantons located in oil producing areas are entitled to receive 58% of such revenues. Royalties from *Ley 010* have also a specific destination, as they can only be used to finance the construction of local infrastructure, to enhance public goods provision, and to foster local economic development. As each canton’s eligibility to receive *Ley 010* funds is linked to oil extraction and the latter depends (mostly) on predetermined geological factors, this gives us the chance to classify cantons into treatment and control groups, in line with the impact evaluation lit-

¹Ecuador is divided into three regions (Amazon, Andean, Coast), and 24 provinces, containing 221 cantons. Cantons are considered as the second-level administrative subdivisions of the country, equivalent to municipalities or counties in other countries.

erature. We interact this predetermined cross-section variation in oil royalty dependency across cantons, with the exogenous time series variation in international crude oil prices, exploiting their sharp decline in 2015 from the preceding resource boom.

Contrary to the majority of the recent literature on this topic, this paper will emphasize on the identification of causal effects at the individual level, rather than on local markets or places. For this, we combine nationally representative quarterly labor market surveys with data on oil prices, central government transfers, and other characteristics at the canton level from multiple administrative sources of information. The final dataset then comprises a repeated cross-section of more than 200,000 observations, with the unit of observation being defined as a working-age individual (ages 18-65) in a particular canton-year.

To document the oil price pass-through effect to labor market outcomes, we adopt a difference-in-differences (henceforth, DiD) empirical strategy, in which we include a large set of household and individual-level controls, fixed effects by canton and year, and a set of linear and quadratic canton-specific time trends in the most conservative specifications. In this setting, the regressor of interest is the interaction between the WTI real oil price with a binary variable taking value of one if a canton is entitled to receive oil royalties according to *Ley 010*, and zero otherwise. A particular concern in the implementation of this estimation strategy is the violation of the parallel trend assumption between oil-producing cantons which receive royalties from the central government, and the remaining cantons serving as the control group. We resort to the new developments of the DiD and event studies literature, by incorporating a proxy variable for the time-varying confounder responsible for the creation of diverse trends in outcomes between cantons, to then instrument such proxy with the first lead of the DiD regressor of interest ([Freyaldenhoven et al., 2019](#)). It is expected that, under plausible identification assumptions (discussed in the text), we can consistently estimate the oil price pass-through effect that is robust to the presence of labor-demand induced diverging trends on outcomes between all cantons.

Reduced form estimates suggest that oil prices do not affect, at least directly, labor market outcomes at the extensive margin. We observe that, irrespective of the specification and outcome considered, price elasticities remain very close to zero and statistically insignificant. In contrast, estimates on the effects of the oil price bust on outcomes at the intensive margin are modest in magnitude, but highly significant. For instance, the price elasticity of wages do not exceed 1, in fact being very close to 0.5, suggesting wages will be reduced by half the percentage point decline in oil prices. The price elasticity of total income and wages are virtually similar, suggesting that total earnings declined as a consequence of the drop in labor income.

We continue our investigation by estimating the causal effect of a central government transfers decline on labor market outcomes as a byproduct of the oil price bust. The

literature has well documented the potential endogeneity bias from traditional OLS fixed effects regressions, as oil producing cantons may be positively selected into better labor market outcomes than the rest of the country. Another source of endogeneity arises when other economic and institutional attributes that are unobserved, or difficult to measure, are simultaneously affecting labor market outcomes and the amount of royalties received from oil production. To overcome such concerns, we implement an instrumental variables-two stages least squares (henceforth, IV-2SLS) strategy in which the excluded instrument is the interaction of *Ley 010* eligibility status with the longitudinal variation in WTI real oil prices. First stage regressions confirm that this shift-share instrument is relevant and statistically robust in predicting the differential decline in per capita public transfers to oil producing cantons. The exclusion restriction needed for this instrument to work is sustained by the fact that cross-sectional variation at the canton level in the *Ley 010* eligibility status is predetermined by geological factors that can be assumed to be constant across time. In addition, the magnitudes of oil production in Ecuador hardly give the country an edge in fixing the price in the international oil market. We provide several robustness checks and falsification tests that confirm that the excluded instrument we implement satisfies the usual IV assumptions.

Findings from this exercise indicate the negative oil price shock significantly affected labor market outcomes through reduced transfers as the main amplification mechanism. A one percentage point decrease in per capita public transfers received by an oil producing canton implies wages to be reduced by 1.6%, according to the most conservative (lower bound) estimates. The elasticity of labor supply is very close to one, suggesting that given a negative transfer shock labor supply adjust in a similar proportion. Contrary to other studies, we cannot identify heterogeneous effects across socio-demographic divides. However, we observe the effect of reduced per capita transfers to be more salient on occupation categories in the lower tail of the skill distribution. Also, modest spillover effects to non-extractive industries and to *Ley 010* non-eligible cantons are observed. We confirm that oil royalties are the main transmission channel by which public transfers reduced in oil-producing areas, and that such reduced transfers triggered demand-sided response from firms in terms of lower sales revenues, lower average compensations per worker, and modest drops in productivity.

This paper is related to at least two strands of the literature. First, this study contributes to a significant body of research testing the consequences of resource booms on labor market outcomes and wellbeing indicators, with an special focus on resource-endowed communities. Frequently, these papers use counties, regions, or any other relevant administrative subdivision of the country as the main unit of analysis. The large majority of this research has been conducted in industrialized countries at the subnational level,

with mixed results.² Studies documenting positive effects on wages and labor market participation suggest the presence of spillover mechanisms in which the businesses and jobs created within the extractive sectors have a multiplicative effect spreading to other industries not directly related with the resource boom (e.g. [Black et al., 2005a](#); [Michaels, 2011](#); [Marchand, 2012](#); [Feyrer et al., 2017](#); [Allcott and Keniston, 2018](#); [Bartik et al., 2019](#)). These effects are not necessarily bounded to the labor market, as resource booms seem to have increased housing and rental prices without major negative consequences for individual wellbeing ([Jacobsen, 2019](#)), and even may have induced fertility booms at resource-abundant areas ([Kearney and Wilson, 2018](#)).

Other research present more skepticism about such positive effects. Examples of studies documenting negative outcomes of a resource boom include negative selection of migrants into resource-rich markets (?), deteriorating living conditions ([Bartik et al., 2019](#)), and lower human capital accumulation ([Black et al., 2005b](#); [Agüero et al., 2021](#)). Since resource booms are far from being a permanent phenomenon, there is crucial evidence suggesting that communities would have been better off if such boom had not occurred, as the subsequent resource bust wiped out income gains obtained during prime working-age years ([Jacobsen et al., 2021](#)).

Where most of the studies are addressing the effects of resource booms, our paper sheds lights on the reverse hypothesis that resource busts effects are more salient in magnitude than gains from an oil boom. Related research has already establish the asymmetric nature of both resource booms and busts shocks on economic activity (e.g, [Black et al., 2005a](#)).

Little is known about the evolution of individual labor market outcomes at low and middle-income countries after an oil price bust. Our findings are informative on how labor supply and earnings change within markets with weak institutions, inefficient public goods provision, and a large informal sector. Our estimates also inform on the short run effect of an oil price bust, with some degree of extrapolation, they are valid for emerging economies whose fiscal capacity has been strongly built upon non-renewable resources availability.

The second strand of the literature to which this paper is related assesses the extent to which price commodity shocks have long- lasting effects on labor market outcomes and wellbeing indicators. The usual empirical strategy implemented in this research is to leverage exogenous jumps in commodity prices that induce differential effects at resource-dependent communities. The magnitude and direction of the effects largely depend on the type of commodity analyzed and the labor market structure. Thus, regarding evidence on crude oil, previous research suggests oil price booms to be a significant force behind the increase in real wages in the US (e.g. [Acemoglu et al., 2013](#)). However, oil price

²See [Marchand and Weber \(2018\)](#) for a complete literature review.

booms are also associated with more civil conflict-related violence and reduced electoral competition (Dube and Vargas, 2013; Carreri and Dube, 2017). These harmful effects are not restricted to the oil industry, as some papers document the detrimental impacts of mining booms on child labor, school attendance, school proficiency, and long-term human capital accumulation (Santos, 2018; Bonilla, 2020). Regarding agricultural commodity price shocks, evidence is provided on the procyclical nature of child deaths in coffee-producing areas (Miller and Urdinola, 2010). Researchers have also identified detrimental effects on wages and labor market participation for women and children in agricultural commodities booms, when producing farms exhibit monopsonic labor market power and implement labor coercive practices (Danzer and Grundke, 2020). Our contribution to this literature is to present lower bound estimates of the crude oil price pass-through into labor market participation, labor supply, wages, and other human capital indicators. Contrasting with the aforementioned literature, our estimates are free from civil conflict and political unrest confounders that challenges the identification of a clear propagation mechanism to other industry sectors and occupations. Our paper also documents the labor demand-sided response of such oil price bust, showing that payroll expenses and the average compensation per worker are more likely to adapt, while labor demand and employability remains fairly constant.

Finally, our paper is closely related to the scholarship assessing the impacts of resource-boom related fiscal windfalls, and how their redistribution affect economic outcomes at the geographical level. Within this branch of research, the skepticism about positive effects of increased transfers is explained by the fact that, while local revenues and public goods expenditures rise, that does not necessarily translate into improvements in wellbeing for resource-endowed communities (Caselli and Michaels, 2013). However, on a more optimistic note, some empirical evidence suggests that such increased transfers have positive effects on relaxing budget constraints and taxation (James, 2015). They and may also allow to obtain non-negligible gains in terms of better school infrastructure, and improved educational learning (Agüero et al., 2021). Some authors even suggest that a better institutional arrangement and a performance-based redistribution policy is all that is needed to avoid the Dutch Disease threat at oil-rich areas (Gallego et al., 2020). We contribute to this literature by documenting the sizable effects of an oil price bust on labor market outcomes. Given the particularities of how oil royalties are allocated across Ecuadorian cantons, our empirical strategy allows us to explore the potential mechanisms from the labor supply and demand sides that mediate between central government fiscal capacity and labor market outcomes at the individual level.

The remainder of the paper is organized as follows. Section 2 provides background information on oil dependency and oil-related transfers in Ecuador. Section 3 describes the data. Section 4 discuss the empirical strategies implemented in this paper. Section 5

presents our findings. Section 6 concludes.

2 Background Information

Ecuador is a dollarized emerging economy, that has traditionally being dependent on exports of certain commodities, and proceedings from rich natural endowments. It experienced substantial economic growth with improved wellbeing conditions during the 2001-2014 period. This growth has been attributed to favorable external conditions in terms of trade, the country's participation in regional economic integration agreements, and a 17 years long oil price boom that increased government revenues since 1998, with several accompanying fiscal and monetary reforms fostering private demand since early 2000s. In 2011, the government renegotiated oil contracts with the private sector, resulting in an increase of royalties from crude oil production but also in the expansion of public demand through increased government spending. Most of the additional resources were used to fund public investments, social infrastructure, and wages of a continuously expanding public sector.

While some welfare gains intensified after 2007 through labor income and public transfers, the decline in oil prices after 2014 exposed how vulnerable the country was to external negative shocks. With the absence of fiscal buffers, and an increasing public debt, the country could neither adopt a countercyclical response to mitigate the impacts of the oil price bust, nor depreciate its currency. Moreover, the private sector was not able to quickly compensate for the drop in public investment and oil exports. As a result, capital outflows rocketed, international reserves and economic growth sharply declined, with the subsequent stagnation in poverty reduction.

The extent and the timing to which an oil price bust affects public financing at the local level largely depends of the types of central government transfers each local government receives. According with the public finance ordinance sanctioned in 2011, local governments, at all levels, are entitled to receive 21% of permanent revenues and 10% of non permanent revenues of the central government budget. Provinces, cantons, and parishes, receive 27%, 67%, and 6% of such amount, respectively. Each local government receives two types of transfers. The first type (class A) corresponds to the funds required by local governments to function, and its quantity is fixed by law since 2010. The second type (class B) corresponds to those variable transfers with their amounts largely depending on socio-demographic indicators, as well as on fiscal performance indicators per local government.

Oil production determines the proceedings that are distributed among local governments at all levels. According to the law sanctioned to distribute these resources, known as *Ley 010*, oil producing companies need to return proceedings from extraction, at a rate

of one dollar per barrel produced. From these resources, cantons are entitled to receive 58% of those funds. These funds also have a specific destination, since by law they can be only used to improve public goods provision and access (e.g. access to sanitation services, tap water, education, health), as well as to enhance public local infrastructure and to foster local economic development. As only oil-producing cantons in the Amazon region are eligible to obtain these funds, we conjecture that the oil price bust affected disproportionately these areas in at least two ways. First, by a reduction of Class A and B transfers, as the central government budget shrunk during the resource bust affecting all local governments, and second, by affecting oil producing cantons as they received less resources that were directly tied to oil production.

3 Data Sources and Sample Construction

We construct three different datasets. First we merge administrative information on public transfers, oil prices, and canton socio-demographic characteristics from the last available population census, to obtain a balanced panel dataset of cantons. This dataset is combined with i) the repeated cross-sections obtained from processing the nationally representative employment survey, and ii) the balanced panel of economically active firms from the directorate of firm and businesses. canton and firm panels will cover seven years of data (years 2011-2017), while the repeated cross-section data of individuals will cover four years in order to avoid endogenous sampling and other problems. Construction of these datasets, and their descriptive statistics, are explained in the following subsections.

3.1 Measuring Crude Oil Price Changes and Central Government Transfers

We start by defining years 2011-2014 and 2015-2017 as the oil price boom and bust periods, respectively. The time series evolution of crude oil prices (USD per barrel) is measured by using the average annual global price of West Texas Intermediate (WTI) crude, available at the Federal Reserve Economic Data Portal (FRED). Regarding public finance information, we resort to administrative records on revenue transfers to locally decentralized governments, according to the redistributive public finances plan set in place and approved by congress in 2011, from the national secretary of economic planning and development (SENPLADES, in its spanish acronym).

Irrespective of their oil-producing status, all Ecuadorian cantons receive two types of transfers. The first type includes those funds fixed by constitutional law, usually devoted to government functioning (class A). The second type includes those variable funds, with their size depending on canton socio-demographic attributes such as total population, percentage of households with unsatisfied basic needs, life expectancy indicators, and prior

fiscal capacity (class B). In addition to central government transfers, SENPLADES keeps records of oil royalties that, according to *Ley 010*, only oil producing areas are allowed to (and currently) receive. Using population projections from the national statistical office INEC, we construct per capita measures for all types of transfers. Then, oil prices and per capita transfers are expressed in 2007 dollars, using the Ecuadorian GDP price deflator.

Figure 1 displays the joint evolution of crude oil real prices and national production (Panel a), and per capita central government total transfers, as an aggregate of class A, B, and *Ley 010* oil royalties (Panel b). While crude oil production remained relatively stable from 2014 to 2017, crude oil prices suffered a sharp decline from USD93 to USD43 per barrel, implying a 54% drop. Per capita central government transfers also experienced a decline during the price bust period, reaching a minimum of USD73 from an average of near USD100 in 2015. While the previous plots suggest a staggered response of public transfers as a consequence of the oil price shock, it is evident that *Ley 010* related transfers experience their sharpest decline in 2015 (Panel c). This fact indicates an immediate response from local governments, internalizing the price bust to reduce the risk of receiving less funds due to low budget execution (Maldonado and Ardanaz, [ming](#)).

Figure 2 presents the geographic distribution of central government per capita cumulative transfers, during the 2011-2017 period. The large majority of cantons localized in the amazon region have received the largest per capita transfers, relative to the remaining cantons in the country. Still, there is significant variation among cantons in the per capita transfers they received during the whole period of analysis, ranging from USD335 to USD4,050 (Panel (a)).

Once we account for the participation of *Ley 010* transfers in the total (cumulative) per capita amount each canton received, it is evident the dichotomous classification between cantons benefited with proceedings from oil extraction, with cantons in the andean and coastal regions receiving zero transfers for such concept (Panel (b)). Hence, we define treated cantons as those deemed eligible to receive (and actually receiving) *Ley 010* oil royalties, and the control group as all remaining cantons in the country.

The heterogeneity among treated cantons in their oil royalties dependency is far from being negligible, ranging from 25% to 72% of total per capita transfers. This spatial heterogeneity in oil dependency exposure might lead local economies in oil-producing cantons to experience spillover effects from the extractive sector into other non-extractive industries. It is also plausible that control cantons in the close neighborhood of oil producing areas will experience spillover effects in their local economic activity as a byproduct of the oil price decline. These hypotheses are tested in subsequent sections.

3.2 Socioeconomic Information by Canton

We combine information on oil prices and central government transfers with socioeconomic indicators at the canton level obtained from the 2010 national Census, administered by the National Statistical Office (INEC). In particular, we collected information on poverty, occupation, and unemployment rates, as well as the percentage of active workers in extractive industries, and the proportion of current residents who lived in a different province (or country) in the last five years.

Table 1 presents differences in means between treated and control cantons in these socioeconomic indicators. Cantons deemed eligible under *Ley 010* are, on average, less populated, slightly poorer, and with a larger presence of former migrants than cantons in the control group. Not surprisingly, treated cantons presented lower (higher) rates of unemployment (employment), and a significantly larger participation in extractive-related industries in 2010. As estimates in columns (4)-(6) suggest, these differences between treated and control cantons are fairly robust, even if we restrict the sample to only those cantons serving as sampling units in the labor market survey we combine information from. This final canton-level cross-section dataset is merged with information on prices and public transfers to create a balanced panel dataset, in which the unit of observation is a canton-year combination. This data are used to provide first stage effects of the oil price bust on public transfers. Also, this dataset is combined with information at the individual and firm levels. The details on these additional sources of information are as follows.

3.3 Measuring Labor Market Outcomes

The individual-level analysis on labor market outcomes is based on the nationally representative, quarterly labor market survey for Ecuador (ENEMDU). This survey collects information on household members and their characteristics, sources of income, labor participation, employment status, educational achievements, and other human capital indicators. Through the period 2007-2017, the survey is statistically representative at the national and province levels. For this research, we are going to only consider the survey from the last quarter of each year (December), as it comprises the most detail information on labor market outcomes and socio-demographic characteristics of respondents.

We identify two advantages of using this dataset for this study. First, it tracks detailed information on labor market outcomes at the individual level, with household, year, and canton of residence identifiers. This allows us to link individuals to canton-level characteristics, crude oil prices, and oil royalties at the local level. Second, besides collecting information on all family members aged five years old or above, the survey includes inverse probability weights specific to each household member. These weights can be used

in the estimation process to improve the representativity of the causal estimates, as well as to account for potential imbalances in the survey sampling between *Ley 010* eligible and non-eligible cantons across all years.

A serious limitation of the ENEMDU survey is that, because of logistic restrictions, not all cantons within a particular province are surveyed each year, with a significant misrepresentation of individuals in oil producing remote areas. From 2014 onward, this problem was accounted for. Sample sizes in oil-producing cantons substantially increased, to the extent that in some of these areas the number of surveyed individuals are up to seven times larger as the sample sizes during the price boom period.³

This study implements several strategies to improve the statistical representativity of our results. First, while some descriptive plots consider information for period 2011-2017, our individual-level analysis focuses on the subset of cantons that were always surveyed in the last year of the price boom (i.e. 2014) and during the following three years associated to the price bust period (2015-2017). We also exclude individuals in all cantons located in the provinces of Santa Elena, Santo Domingo and Galapagos, as data for these provinces are only available after 2015, with no available information during the price boom period. The cantons of Quito and Guayaquil are also excluded from the analysis to avoid the influence of such outliers, given that these cantons together received more than 40% of total central government transfers. Finally, as hinted before, all our regressions and descriptive analyses will use inverse probability weighting to control for potential endogenous sampling.⁴

A second limitation of the ENEMDU survey is that it does not constitute a longitudinal panel dataset of individuals and households. In an ideal setting, using within-individual variation would allow us to control for household or individual-specific confounders, isolating time-invariant factors influencing labor market outcomes, even in the absence of a price bust. However, as the main unit of observation for our main regressions will be an individual-year combination, we can append the different waves of ENEMDU to construct a repeated cross-section dataset, allowing us to include canton of residence and year fixed effects, without seriously compromising the identification of the parameters of interest (Heckman and Robb Jr, 1985).

In addition to the canton selection earlier described, we define our population of interest as all adults aged 18-65 years old, for which data on individual characteristics

³The sampling methodology within and across administrative units dramatically changed after 2018, implying additional challenges regarding the comparability of labor market indicators by province, and for lower aggregation levels. Also, the length of the questionnaire was dramatically reduced, excluding questions that were surveyed in previous waves.

⁴A particular concern regarding this survey is endogenous (under)sampling for most of the treated cantons localized in the amazon region. From 2014 onward, this problem was corrected, considerably increasing sample sizes for these administrative units. As relevant literature suggests, the use of inverse probability weights is justified to account for such sampling imbalances, at the expense of lower precision in our estimates (Solon et al., 2015).

and labor market outcomes are available. Once trimming observations to exclude the top and bottom 1% percentiles of the household total income distribution to avoid outliers influence, we merge this selected sample with oil prices, central government transfers, and canton socio-demographic indicators from 2010 census. This final individual-level database leads to a repeated cross-section of up to 189,061 individuals, covering up to 177 cantons, of which up to 32 received *Ley 010* transfers.

Table 2 reports summary statistics for our final individual-level dataset, and differences in means between individuals living in *Ley 010* eligible (treated) and non-eligible (control) cantons. Regarding labor market indicators at the extensive margin, 71% of individuals report to be employed, versus a 4% who report being unemployed. At the intensive margin, the average worker reports working 39 hours per week, earning USD238 and USD2 per month and by the hour, respectively.⁵ Individuals in our sample are, on average, 37 years old, and almost a third of that lifespan has been devoted to human capital accumulation (10 years). Roughly half of the population are women, and 39% of respondents live in rural areas. One out of five individuals self-identifies as a member of an ethnic minority, and 6% reported to have some migration experience over the last five years. House and land ownership is seldom, with 13% and 24%, respectively. Access to electricity is almost universal, while access to tap water and sewage services is more limited.

With regards to mean differences between treated and control cantons, the former seem to present higher rates of labor market participation, with working population in these areas exhibiting more informality and previous migration experience. Adult population living in *Ley 010* eligible cantons are, on average, older, with a lower participation of female workers but with no significant differences in their educational attainments. Relative to cantons in the control group, households in oil producing areas seem to have better public goods coverage, in terms of electricity, tap water, and sanitation services access. This fact is not necessarily surprising, as one of the specific destination of *Ley 010* resources is to improve the access and quality of infrastructure and public services provision.

3.4 Measuring Firm-Level Economic Performance

To provide evidence on labor-demand side mechanisms, we resort to the INEC firm laboratory (Directorio de empresas y establecimientos, in spanish). This publicly available dataset comprehends annual information about the universe of firms and businesses, reporting economic activity and social security contributions. The major advantage of this source of information is the detailed information on sales, payroll costs, and number of

⁵All wages and earning figures are also expressed in 2007 dollars, by using the Ecuadorian GDP price deflator.

workers currently employed. In contrast, the publicly accessible version of these data does not include information on assets, liabilities, and other operational costs such as material and capital costs. Another disadvantage of this dataset is the large attrition of firms across all years the information is available (2006-2019). This attrition occurs as some firms may interrupt activities but do declare bankruptcy, because of the latter prohibiting costs.

At the expense to lose some external validity, we consider only the balanced panel of firms in period 2011-2017. A firm is included in this final dataset as long as it has reported positive sales in each year, irrespective of the amount. This sample selection leads to obtain 168,651 observations (24,093 firms), in which an observation is defined as a firm-year combination. Since we can identify each firm’s location, this dataset is merged with the aforementioned information on prices and transfers at the canton level. We report summary statistics and mean differences for firms located in treated and control cantons (Table 3). In our balanced panel only 537 firms are located in *Ley 010* eligible cantons. Besides total sales, payroll expenses, and firm’s employment, we built indicators of average compensation, and two raw measures of sales productivity. Not surprisingly, firms localized in control cantons exhibit higher sales and employment, but also significantly higher payroll costs. However, firms in treated controls seem to be, on average, more productive than their counterparts in the control group with 7.46 dollars per dollar spent on payroll and 10% more productive per additional worker.

4 Empirical Strategy

4.1 Evidence on the Oil Price Pass-Through Effect: Reduced Form Model

To identify the reduced form effects of the oil price bust on labor market outcomes of individual i , living in canton c at survey year t , we suggest the following specification:

$$\log(y_{ict}) = \mu_c + \eta_t + \beta [Treated_c * \log(Price_t)] + \gamma w_{ct} + x'_{ict} \delta + f(c, t) + u_{ict}, \quad (1)$$

where y denotes the outcome of interest, such as labor market participation, number of hours worked per week, or monthly real wages, etc. μ and η denote fixed effects by canton and year, respectively. These are included to capture time invariant differences in outcomes between cantons, and time trends simultaneously affecting all cantons. Preferred specifications also include a set of individual and household attributes, x_{ict} , and a set of canton-specific linear and quadratic time trends, denoted as $f(c, t)$. Residuals in this

model are denoted by u , and robust standard errors are clustered at the canton level.

The regressor of interest is the interaction of two sources of variation. First, let $Treated$ be a binary variable taking value of one if a particular canton is eligible to receive oil royalties due to the implementation of *Ley 010*, and zero otherwise. $Price$ is the real price of WTI crude oil. Thus, model (1) can be considered as a traditional difference-in-differences (DiD) specification, in which its coefficient β captures the percentage point difference in a particular labor market outcome between treated and control cantons due to a one percentage point increase in oil prices.⁶

In this general setting, however, it is safe to assume that the common trend condition between treated and control cantons does not necessarily hold. This can be explained by the existence of other time-varying, labor-demand side confounding factors, leading treated and control cantons to exhibit different trends in labor market outcomes, even in the absence of an oil price change. This problem may prevail even after including proxy variables of such confounder, or by controlling for canton-specific time trends.

To circumvent this concern, we rely on recent developments in the event study and difference-in-differences (DiD) literature to identify the effect of interest even with the presence of diverging trends in outcomes between treated and control groups (Freyaldenhoven et al., 2019). A potential solution to this identification problem is to include a proxy variable that is also correlated with the labor-demand confounder, but that is not contemporaneously affected by the price shock. The proxy variable selected for this study measures migration flows into oil producing areas. The relationship between labor demand and migration inflows has been fairly documented in the economics of migration literature, and there is increasing evidence suggesting that commodity price shocks do not affect contemporaneous migration patterns. To enforce a reallocation decision, prospective migrants may want to discern whether a price shock is transitory or permanent. Even assuming a permanent price shock, instantaneous migration is not likely to occur as households have to embrace in non-negligible reallocation costs. Hence, the identification assumption required for the reduced form effects to be credible is that the current oil price shock will affect future but not contemporaneous migration flows into oil producing areas.⁷

Thus, the proxy variable w_{ct} is constructed and included as an additional control in equation (1):

⁶Since we are interested in reporting effects for an oil price drop, estimated coefficients will be reported with negative sign in all tables.

⁷Evidence from SVAR estimates suggests labor demand shocks to be more important than migration innovations in determining fluctuations in labor market participation at the local level (Partridge and Rickman, 2003). Due to barriers in obtaining information on job opportunities, and the fact that resource booms occur at remote locations, it is expected that the migration response to a resource boom is delayed up to three years (Allcott and Keniston, 2018).

$$w_{ct} = out_{c,2010} \times \log(ecumale_t),$$

where $out_{c,2010}$ denotes the percentage of people reporting in the 2010 census to have lived outside canton c at some point in time during the last five years, and $ecumale_t$ is the total number of male return-migrants entering the country by land, sea, or river in year t . It is expected that w_{ct} , as a shift-share variable, does not accurately measure migration flows into oil producing areas. However, as long as the identification assumption holds, this proxy variable is expected to be correlated with the first lead of our main regressor. Thus, we can use $Treated_c * \log(Price_{t+1})$ to instrument w_{ct} , to then replace w_{ct} by its first-stage prediction in model (1), recovering the β parameter using the two-stage least squares estimator (IV-2SLS):

$$y_{ict} = \mu_c + \eta_t + \beta [Treated_c * \log(Price_t)] + \gamma \widehat{w}_{ct} + x'_{ict} \delta + f(c, t) + e_{ict}. \quad (2)$$

Identification of β relies on the assumption that the noise in the proxy variable (as it is an imperfect measure of the true confounder) is conditionally mean-independent from the regressor of interest. Thus, as long as the oil price shock does not affect labor market outcomes in oil producing areas indefinitely, it is possible to use leads of the DiD regressor to leverage exogenous variation in the confounder, netting out the divergent trends in outcomes between treated and control cantons. In practice, this implies to assume that prospective migrants cannot anticipate future oil price shocks.

4.2 The Causal Effects of Central Government Transfers on Local Labor Market Outcomes

To estimate the amplifying mechanisms of central government transfers on labor market outcomes as a byproduct of the crude oil price negative shock, we suggest the following structural specification:

$$y_{ict} = \mu_c + \eta_t + \beta \log(T_{ct}) + x'_{ict} \delta + f(c, t) + u_{ict}, \quad (3)$$

in which variables are defined as in the reduced form described in equation (1). In this new model, the regressor of interest is the natural logarithm of real per capita transfers from the central government to canton c at year t , denoted as T_{ct} . The interpretation of β will depend on whether the outcome of interest is measured in levels or in logarithms. In the first case, a one percentage point decrease in per capita transfers will imply a decline of $\beta/100$ units of y_{ict} . In the second case, the interpretation of β is straightforward as it should approximate the corresponding price elasticity.

A particular concern arises when interpreting the parameter β as a causal estimate. It is expected that, as discussed in the oil price pass-through section, labor-demand confounders may be significantly correlated with the level of transfers the central government is willing to allocate between local autonomous governments. Another potential source of endogeneity arises when difficult-to-measure economic and institutional attributes may simultaneously affect labor market outcomes and resource extraction (Gallego et al., 2020). In this particular setting, prior descriptive evidence suggests that cantons in the treatment group are positively selected on labor market outcomes, since extractive industries located in these areas are labor intensive. Hence, naive estimates of such elasticities will be attenuated (downward biased).

To tackle these identification challenges, we resort to an instrumental variable framework in which the excluded instrument is the interaction between *Ley 010* eligibility to receive oil royalties ($Treated_c$) and the real price of WTI crude oil in logarithms. Thus, we implement an IV-2SLS estimator, in which the first and second stage regressions are equations (4) and (5), respectively:

$$\log(T_{ct}) = \mu_c^f + \eta_t^f + \beta^f [Treated_c \times \log(Price_t)] + x'_{ict} \delta^f + g_f(c, t) + u_{ict}^f, \quad (4)$$

$$y_{ict} = \mu_c^s + \eta_t^s + \beta^s \widehat{\log(T_{ct})} + x'_{ict} \delta^s + g_s(c, t) + u_{ict}^s. \quad (5)$$

The identification assumptions required for this empirical strategy to hold are the traditional ones in the IV literature. First, the instrument should be relevant to predict the difference in central government transfers between oil-dependent (treated) and non-dependent (control) cantons. Second, the instrument should be exogenous. This last requirement, while not possible to test directly, is guaranteed by the fact that Ecuador acts as a price-taker in the crude oil market. Hence, this oil price variation, and how it affects government transfers, is neither controlled nor foreseen by individuals working in oil dependent areas. Moreover, the canton variation in oil dependency has been determined by geological factors prior to the price bust that, conditional on the inclusion of canton-specific fixed effects and time trends, should not affect current labor market outcomes except through oil production and/or oil royalties reception. It follows that the interpretation of the causal effect under this setting is of local nature, as we expect eligible cantons under *Ley 010* to be more susceptible to suffer the negative consequences of a price bust than the remaining cantons in the country.

5 Results

5.1 Baseline Results

In this section, we present two sets of results. First, we document the reduced form, crude oil price pass-through reducing effect into wages and labor supply, showing no effects on labor market outcomes at the extensive margin whatsoever. Second, we present IV-2SLS estimates on the impacts of the per capita public transfers reduction induced by the oil price decline on labor market outcomes and human capital indicators. Findings from this body of evidence suggest wages to be highly sensitive to the public transfer reduction, suggesting public finances as a major amplifying mechanism by which individual wellbeing is affected by a resource bust.

5.1.1 Reduced Form Evidence on the Oil Price Pass-Through Channel

Table 4 reports estimates from the reduced form model (1) with the individual-level 2014-2017 sample. While all specifications include household/individual attributes, and fixed effects by canton and year, columns (1) and (3) report coefficients from models without including canton-specific time trends. Columns (3) y (4) display parameters in which the IV-2SLS method on the proxy variable to account for divergent trends is implemented, as suggested by the recent DiD literature ([Freyaldenhoven et al., 2019](#)). The coefficients reported correspond to the DiD regressor, and should be interpreted as the percentage point drop in labor market outcomes at treated cantons, implied by a one percentage point decline in the crude oil price. All regressions use inverse probability weights to adjust for endogenous sampling, and robust standard errors are clustered at the canton level.

Results from this empirical exercise suggest that a one percentage point decline in oil prices has a negligible impact on labor market participation and supply (Panel A). Irrespective of the specification and estimation method used, effects on the probability of working or being unemployed are very close to zero and imprecisely estimated. Taking results about the probability of work as an example, coefficients are positive but not statistically significant, even after including canton-specific linear trends (columns (1) and (2)). After instrumenting the proxy variable, the coefficient of interest turns negative, implying that a one percentage point (p.p.) decline in oil prices cause a 0.02 p.p. drop in the probability of work (column (4)). However, this estimate is also imprecise. A similar conclusion can be drawn when observing causal effects on the probability of experiencing unemployment.

Regarding labor supply results at the intensive margin, under the most demanding specifications we find price elasticities up to 0.6 (columns (1) to (3)). However, statistical significance is lost after controlling diverging trends between treated and control cantons.

Even without paying much attention on statistical significance, the elasticity we obtain implies that a 10 percentage point decrease in public transfers leads to only a 1.2 p.p. reduction in the total number of hours worked per week. These estimates taken together suggest that, relative to cantons in the control group, the oil price negative shock does not affect labor supply and participation in oil producing areas.

Interesting results emerge when assessing the oil price pass-through on wages and earnings (Panel B). Traditional DiD specifications report that a 10 p.p. drop in oil prices implies a 2 p.p. reduction in monthly wages (column (1)). Point estimates remain virtually the same once canton-specific time trends are included (column (2)). Once the proxy is instrumented with the lead of the treatment variable, coefficients remain relatively constant but their precision worsens (column (3)). When repeating the same exercise, but including canton-specific time trends, estimates become more precise, suggesting a drop in 7.4 p.p. in monthly wages for a 10% oil price decline. While this elasticity is, in absolute value, less than one, it represents a 83% of a standard deviation (SD) change, and explains up to 14% of the total decline in wages during the period 2011-2017. A similar conclusion can be drawn when examining results for the wage rate, where we identify a price elasticity of 0.5, representing a 7.5% of the total drop in the hourly wage during the same period.

Finally, in order to test whether the oil price decline affected other sources of income, we report the same results in which we estimate the price elasticity of total income. As observed, such elasticity suggest a 4.5% decline in total income explained by a 10 percentage point drop in oil prices. Since this estimate is included in the 95% confidence interval of the price elasticity of monthly wages coefficient, it is safe to infer that most of the decline in total income can be attributed to the decrease of labor income. Taken together, these results suggest an oil price pass-through that significantly affects wages but to a lesser extent labor supply and participation.

We replicate the same estimation process to document effects on human capital indicators in Appendix Table A1. While not significant effects on social security contributions and migration experience can be identified, an almost zero effect on human capital accumulation is recovered. According to the coefficients reported in column (4), a ten percentage point decrease in oil prices explains only a 0.05% increase in school enrollment at any level. With regards to marital decisions, we confirm the direction of earlier findings from the resource bust literature (e.g. [Dorn et al., 2019](#)), as we observe a decline in marriage and cohabitation rates as a consequence of the oil price decline.

5.1.2 Evidence on the Causal Effects of Central Government Transfers on Labor Market Outcomes

Having established that the oil price bust had negative effects on wages and earnings, we turn to the estimation of the causal effects of reduced public transfers on labor market outcomes. Table 6 presents results from the IV-2SLS setting in which we assess the causal effect of a one percentage point decrease in central government per capita transfers on labor market outcomes. As in the reduced form section earlier, we present different specifications with and without the inclusion of canton-specific time trends. OLS estimates are also reported to ease comparisons between plausibly biased versus consistent estimates. Kleibergen-Paap F statistics in IV-2SLS models provide evidence of strong first stage effects, a fact that will be further discussed in the robustness checks section.

Findings on the extensive margin suggest that the decline in per capita transfers did not affect labor market participation nor the probability of being unemployed (Panel A). In the case of labor supply, one percentage point decrease in per capita transfers implies a reduction of weekly working hours up to 0.9 percentage points (column (4)). This last estimated effect, recovered from an IV-2SLS with canton-specific time trends, is highly significant and sharply contrasts with the attenuated coefficients from OLS regressions (columns (1) and (2)).

We obtain salient IV-2SLS estimates of the per capita transfers elasticity of wages and earnings, in contrast with the null results recovered from OLS regressions (Panel B). Considering the specification without canton-specific time trends, a one percentage point decrease in per capita public transfers imply a 4.3 percent drop in monthly wages. Similar estimates can be obtained for the wage rate and total monthly income, with elasticities of 4.3, and 5.1 percentage points, respectively (column (3)). Once time trends by canton are included, these estimates drop in absolute value, but still remain both statistically and economically meaningful. Considering monthly wages as an illustrative example, a one percentage point drop in per capita public transfers explains a 1.6 percent decline in this outcome (column (4)). Wages per hour exhibit an almost unitary elasticity, a finding possibly explained by the simultaneous drop in labor supply documented earlier. The elasticity recovered in the case of total monthly income is virtually similar to the one obtained for monthly wages, suggesting that the drop in total earnings is almost fully explained by the simultaneous decline in monthly wages.

We consider elasticities reported in the last column of Table 6 as the lower bound estimates of the true effect of per capita public transfers reduction on labor market outcomes, as the inclusion of canton-specific time trends are helping us to i) net out unobserved variation in local labor markets, and ii) improve precision in our estimates. In Appendix Table A2 we report results on human capital indicators, with no discernible effects to be identified with regards to marriage decisions, educational enrollment, and

migration patterns. These findings taken together suggest that the amplifying effects of the oil price bust through public transfers, are mostly explained by a decline in economic activity with a direct impact in labor market outcomes, a transmission mechanism that will be further explored in subsequent sections.

5.2 Robustness Checks

In this section, we present different tests to provide evidence on the statistical robustness of the exogenous source of variation chosen to predict per capita public transfers. We first present compelling evidence that oil prices are the major source of variation influencing changes in public transfers, irrespective of the functional form and unit of measure used. Second, we show that, relative to other relevant commodity prices in the country, Oil prices are still the most relevant source of variation in explaining the public transfers' decline. Finally, we implement tests on the geographical distribution of oil royalty reception. As argued, the spatial distribution of treated and control cantons depends on predetermined geological factors linked to oil production. Hence, eligibility to receive Ley 010 transfers only depend on the capability of each canton to contribute with a fraction of total national oil production. We implement falsification tests in which we show that the significant effects obtained in first stage regressions are due to the original, true classification of cantons in treated and control groups, and not due to statistical randomness.

5.2.1 First Stage Results

We first report first stage results at both the canton-level and repeated cross-sections datasets, to show that the excluded instrument suggested in this study is statistically relevant, irrespective of the scale chosen. in Appendix Table A3 we present estimates using three versions of the same instrument. Namely, we interact the treated canton indicator variable with a dummy variable *After2015*, taking value of one if the observation belongs to the price bust period (2015-2017) and zero otherwise, and with real oil prices in 10USD levels and logarithms. In canton-level regressions we also interacted some predetermined canton's characteristics according to the 2010 population census with the price bust period indicator variable. Regressions at the individual level include the same set of household and individual's attributes as of baseline specifications reported in previous sections.

Estimates from canton-level regressions show that per capita public transfers in *Ley 010* eligible cantons were, on average, 7.2 percent lower than in comparable control cantons after 2015 (Panel A, column (1)). Focusing on the oil price pass-through effect into public transfers, a 10USD increase in price implies 1.9 percent higher per capita transfers in oil producing cantons, relative to the remaining cantons in the country. Given

that, in real terms, oil prices declined 30USD, this implies the oil price bust to explain up to 80% of the total decline in per capita transfers in oil producing cantons (column (2)). When focused on the price elasticity of transfers, a 10% price decline in crude oil implies a 5.6% drop in per capita public transfers. Since oil prices experience a 46% drop, this fact suggests their total percentage variation to explain up to 36% of the total decline in per capita public transfers in treated cantons after 2015. Kleibergen-Paap first stage F statistics reduce in size, but are always above modern critical values, allowing to reject the null hypothesis that the instrument is weak (Stock and Yogo, 2002).

Estimates from individual-level regressions are strongly significant, although their magnitudes differ drastically to the canton-level effects previously reported (Panel B). This is due to the fact that we are only considering cantons in which the ENEMDU survey was conducted throughout the entire period of interest. Also, it is important to remind that observations from Quito and Guayaquil were excluded. Nonetheless, effects are higher in magnitude but strongly relevant when checking first stage F statistics, whose values are above traditional threshold levels. Irrespective of the database used, and the scale of the excluded instrument, it is evident the existence of a first stage to instrument per capita public transfers.

5.2.2 Other Commodity Prices as Alternative Instruments

It is plausible that other commodity prices changed during the period of study, affecting public transfers to local governments. We test the hypothesis that the decline in public transfers previously documented by our first stage estimates is only due to the oil price bust. In Appendix Table A4 estimates of this exercise are reported, as we interact the *Ley 010* eligibility variable with the log prices of the most representative commodities in the Ecuadorian economy. Each column display the differential effect in outcomes between oil-producing and non-producing cantons that can be attributed to the oil price decline, and the influence of each commodity price, one at a time. If an alternative commodity price turns out to be the "true" source of variation behind the decline in per capita public transfers, the coefficient associated with our original DiD regressor may weaken its statistical relevance.

According with coefficients from canton-level regressions, it is evident that the oil price bust remains the main source of variation behind the per capita public transfers decline in oil producing areas (Panel A). Kleibergen-Paap first stage F statistics remain fairly high, with the notable exception of results in column (2), in which variation in per capita transfers is explained by changes in oil and copper prices. While this F statistic is above the critical value of 10. it is still below modern threshold values of the weak instrument literature (Stock and Yogo, 2002). This can be possible since copper mines are located in areas where oil is also extracted. In Panel B, we report estimates from the individual-

level regressions, in which these concerns fade away as not only the coefficients of the DiD regressor remain highly significant. It is also possible to reject the null hypothesis of weak instruments since first stage F statistics of all specifications remain above traditional critical values. These results, taken together, suggest that the most important source of variation to explain the drop in per capita public transfers reception at oil-producing areas is, precisely, the 2015 oil price bust.

5.2.3 Falsification Tests

To provide indirect evidence on the exclusion restriction, a series of falsification tests are implemented in which we interact the times series of oil prices with an incorrect treated group indicator, by choosing 43 cantons (the same number of *Ley 010* eligible cantons) at random. It is expected that this permutation exercise does not produce non-zero effects, provided we control for canton and year fixed effects, canton-specific time trends, and no remaining source of unobserved variation is behind our findings. In Appendix Figure A1, we display the distributions for the three versions of these placebo instruments, obtained from 1,000 permutations in which we estimate the canton-level benchmark model. As expected, in all panels we clearly observe the incorrect *Ley 010* eligibility status produced estimates concentrated around zero, with the top and bottom tails of such distributions to be, in absolute value, below the first stage effects obtained with the original data. These findings provide additional evidence of the validity of the exogenous sources of variations exploited in our empirical strategy.

5.3 Heterogeneity Effects Analysis

5.3.1 Heterogeneous Effects by Socio-Demographic Groups

In Table 6 we report regressions in which we explore heterogeneous effects among socio-demographic divides. Regarding labor market supply and participation we observe no significant estimates in all population subgroups on the probability of working or being unemployed. At the intensive margin, we show that the decline in weekly working hours is partially explained in the subset of rural workers, young adults, female workers and members of ethnic minorities. However, no differences in coefficients are statistically significant across all these dimensions.

Focusing on labor market wages and earnings, a similar conclusion can be drawn when quantifying effects on monthly wages and the wage rate. Interestingly, we are able to document a highly significant difference in total income between young and older adults, with the former having a decline 3.1 percent higher than the latter (column (6)). Appendix Table A5 displays the same heterogeneity effects analysis for human capital indicators, in which we cannot identify a single significant coefficient difference among the

socio-demographic groups considered. These results taken together imply that, with the notable exception of age-driven effects on total earnings, the worsening of labor market outcomes as a byproduct of the oil price bust is rather homogeneous.

5.3.2 Heterogeneous Effects by Occupation Type

While no significant heterogeneity on outcomes between socio-demographic groups was identified, it is expected that certain occupations are more susceptible to be exposed to the consequences of an oil price bust. In Table 7 we document results from regressions in which we classified employed individuals in the nine occupation categories according to the International Standard Classification of Occupations (ISCO-08). In column (1) we report baseline results for the entire sample to ease comparisons. Regarding the decline in weekly labor hours, higher-than-average effects can be identified for individuals working in elementary occupations, skilled agriculture positions, and plant and ensemble operators (columns (7), (9), (10)). These occupations also seem to have experienced the larger declines on monthly wages and earnings. For instance, workers in skilled agriculture positions have exhibited a drop in 19% and 23% in their monthly wages and total monthly earnings, because of a 10% reduction in per capita public transfers, respectively. Without a doubt, low skilled occupations have been the most affected, with declines in total earnings of at least 2.1 percent for each 1 p.p. decline in per capita public transfers.

5.4 Evidence on Spillover Effects

5.4.1 Spillover Effects on Non-Extractive Industries

In Table 8, we report the differences in outcomes between *Ley 010* eligible and non-eligible cantons for the five top non-extractive industry sectors that, combined, explain more than 50% of the total economic activity in Ecuador. Not surprisingly, the agriculture sector is the one driving most of the variation as weekly working hours and monthly wages decline 1.8 and 2.6 percent for a 1 percent drop in per capita public transfers, respectively. The effect on total monthly income present the same magnitude as in monthly wages, suggesting that the decline in total income occurs because labor earnings decreased. Other sectors where spillover effects can be identified are retail and trade, with monthly wages in these sectors dropping 1.7 and 3.4 percent for the same 1 percent decline in per capita transfers. the remaining sectors, while exhibiting negative coefficients indicating a decline in labor market outcomes for the employed population, seem to be unaffected as parameters are not statistically significant. As the non-extractive, economically dominant sector, workers in the agricultural industry have been harmed by the oil price bust and its subsequent decline in public transfers.

5.4.2 Geographic Spillovers

To contribute in the economics of geographic spillovers literature, we document the potential propagation effects of the reduced public funding in neighboring areas that, while not eligible to receive *Ley 010* related transfers, may be affected by two distinct channels. First, as proceedings from oil production decrease, this may affect the amounts of Class A and B transfers that each canton receive. Second, the economic consequences of the oil price boom and its subsequent bust may have affected nearby markets, in terms of an increased demand for goods and services during the buoyant times, and a shrinkage of the local demand afterwards.

Following the framework proposed by [Aragón and Rud \(2013\)](#), we suggest the following reduced form specification:

$$Y_{imt} = \mu_c + \eta_t + \beta [MinDist_{km} * \log(OilTr_{kt})] + \gamma w_{ct} + x'_{ict} \delta + f(c, t) + u_{ict} \quad (6)$$

in which all variables are similar to previous models already discussed, with the exception of the DiD regressor. This variable is the interaction of two sources of variation. The first source corresponds to changes in *Ley 010* related transfers, denoted by *OilTr*, in the closest oil producing canton *k* from non producing canton *m*. The second source of variation, denoted as *MinDist*, is the distance between control canton *m*, and the nearest *Ley 010* eligible canton *k*. The identification assumption is that spillover effects are decreasing with the distance to the nearest canton receiving oil royalties. For ease comparison, we are grouping control cantons in 50km distance bins, to then run specification (6) for each of these groups, with the baseline comparison group be comprised of all control cantons located at least 200km far away from the nearest treated canton.

Results from this analysis are displayed in Appendix Figure A#. Positive non-monotonic effects on labor supply at the intensive margin are obtained. On average, workers in control cantons located up to 150km away from oil producing areas experienced a 0.7 percentage point increase in their weekly working hours, given a 1 p.p. decrease in *Ley 010* public transfers. With regards to monthly wages in control cantons, we observe a inverted u-shape pattern, in which neither closer cantons nor cantons too far away from oil producing areas present significant spillover effects. Cantons located between 100-150km away present a 11% increase in monthly wages as a consequence of a 10 percent reduction in *Ley 010* related transfers at oil-producing cantons. Taken these results together, with the fact that the wage rate remains unaffected for all distance bins considered, allows us to conclude that spillover effects, while modest, still exist and exhibit a countercyclical tendency.

5.5 Mechanisms Analysis

In this section we explore two transmission mechanisms. First, we decompose the effect of per capita total transfers into those that have a fixed component, and those that either directly or indirectly depend on oil production and prices. Results from this exercise suggest that oil royalties are, indeed, the main channel by which local public finances are harmed, affecting the labor market outcomes and economic activity. Second, we run reduced form and IV-2SLS regressions using the balanced panel of firm dataset, to identify demand-sided responses to the resource bust. Results on this specific analysis confirm wages and sales to decrease, but no effects on employment whatsoever.

5.5.1 Different types of Public Transfers

In this subsection we explore which public transfers are driving our main results. In principle, since *Ley 010* related transfers are tied to oil production, with the latter depending on the oil price movements, it is logical to infer that this transmission channel is the most relevant in explaining the labor market outcome decline after 2015. However, oil royalties are also indirectly tied to the central government budget, which in turn can modify public transfers to all cantons in the country, as a consequence of the oil price bust.

To test these transmission mechanisms, in Table 9 we present a second version of our first stage regression, in which we decompose the per capita total transfers that are received by canton, in their main components. Column (1) reproduce the baseline first stage coefficients in order to ease comparisons. Columns (2) and (3) correspond to first stage regressions in which the dependent variable are the per capita class A and class B transfers, respectively. In the last column, we run the same first stage, but now the dependent variable is the (inverse hyperbolic sin transformed) per capita *Ley 010* related transfers.⁸ Estimates suggest oil royalties and class B transfers to be the main sources behind the total public transfers decline as the oil price but occurred. On average, 60 percent of the total reducing effect on per capital public transfers can be associated to the decline in oil royalties, with the remaining percentage to be attributed to class B transfers which, although they do not directly depend on oil production, can be affected by i) the central government budgeting and ii) the behavioral responses of consumers and producers in oil and non-oil producing areas. Not surprisingly, those transfers that are fixed by constitutional law remained intact, with no significant response to the oil price bust to be reported (column (2)).

⁸We resort to this transformation to avoid missing observations in which *Ley 010* transfers are zero.

5.5.2 Labor Supply-Sided Response to the Oil Price Bust: Firm level Results

In previous sections we have documented the labor market responses of individuals while facing an oil price bust and a subsequent reduction in per capita public transfers. To complete the picture, we present evidence on the demand-sided response to the resource bust, focusing on how firms respond in terms of adjusting their employment needs, payroll expenses, production, and productivity. In Table 10 we present reduced form and structural results using the IV-2SLS estimator. In columns (1) to (3), the coefficients reported should be interpreted as the price elasticity. Estimates reported in columns (4)-(6) are the percentage point effect on the firm's performance indicators, implied by a 1 p.p. decline in per capita public transfers. We present different specifications in which non-extractive firms are only considered, and when we exclude firms localized in Quito and Guayaquil, the two main economic centers in Ecuador.

Reduced form results suggest a contraction of economic activity. Sales decreased at least 18% for a 10% decline in oil prices. While payroll costs had a 10% decline for the same oil price variation, the parameter is only significant at the 90% confidence level. Average compensation per worker, as a proxy of mean wages, also experience a minimum decline of 3% per 10 percent drop in oil prices. This decline in average compensation is in line with a loss in our proxy measures of productivity, as productivity per dollar spent and per worker diminish at least 9% and 13% in treated cantons. Interestingly, it seems that the oil price pass-through did not affect employability as the average number of workers per firm remained constant.

We observe dramatic labor demand sided responses when public transfers decline. In particular, sales in treated cantons are 1.6% smaller per each percentage point drop in per capita transfers. On average, average compensation reduced 0.3 percentage points for the same oil price variation. The average worker per firm seem to be less productive, since at least 1% decline in per capita public transfers is associated with a reduction of its average productivity by the same percentage point variation.

An interesting aspect of this analysis is the fact that both, reduced form and IV-2SLS results are fairly robust when we focus on non-extractive firms, or when we exclude firms located in big economic hubs. These results taken together suggest that the demand side of the employment relationship adjusted payroll costs, compensation, but do not adjusted employment, even after experience a reduction in sales. These findings are informative in the extent to which an oil price bust, at least in this context, did not have any effect on employment.

6 Conclusion

This paper presents evidence on the worsening effects of a resource bust in local labor markets, with a special focus on individuals living and working in resource-rich areas. In the particular case of crude oil in Ecuador, we have documented the 2015 oil price decline to have had a moderate, direct effect on reducing wages and labor supply. However, it also affected local governments' fiscal capacity in such a way that the reduced transfers, otherwise funding public infrastructure projects employing local resources, served as an amplification mechanism by which the worsening effects of the resource bust were far more severe than expected.

The elasticities presented in these studies evidence that, under commodity price volatility, wages and earnings are the first indicators to adapt, while labor market participation at the extensive margin remained unaffected. This may be partially due to the fact that the Ecuadorian labor market is very rigid, forcing employers and workers to find alternative ways to cope with the oil price bust. Our findings allow us to conclude that households' wages and earnings in resource-dependent areas are going to be more than proportionally reduced after an oil price bust. Avoiding the concomitant effects on well-being that such drop in earnings might cause, depends on the central government's fiscal capacity to counteract and implement income alleviation programs, such as conditional cash transfers, or credit lines under soft financial constraints, to name a few initiatives.

This study is not free from limitations. The sampling restrictions of the national employment survey in oil-producing cantons forced us to only discuss short-term effects. Although the recovering of treatment effects under repeated cross-section samples is well perceived in the econometrics literature, we consider an individual balanced panel as a better dataset to shed light on the individuals' dynamic responses to the oil price bust. Yet, we believe this paper provides an informed analysis that can be extrapolated to contexts in which fiscal capacity and provision of local public goods is heavily tied to oil production. The extent to which the oil price bust affects short and long-term human capital accumulation decisions is a question that we will pursue in future research.

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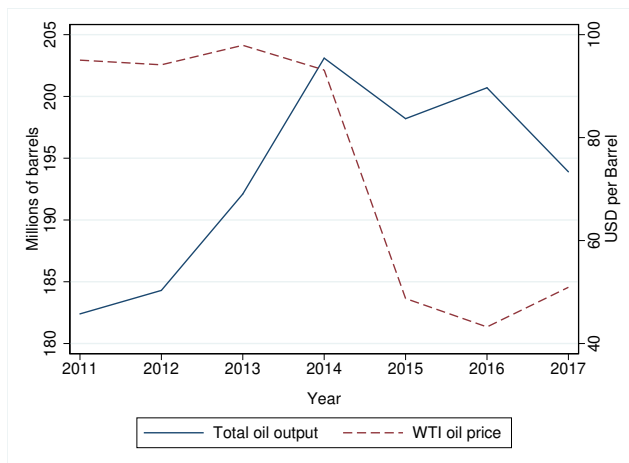
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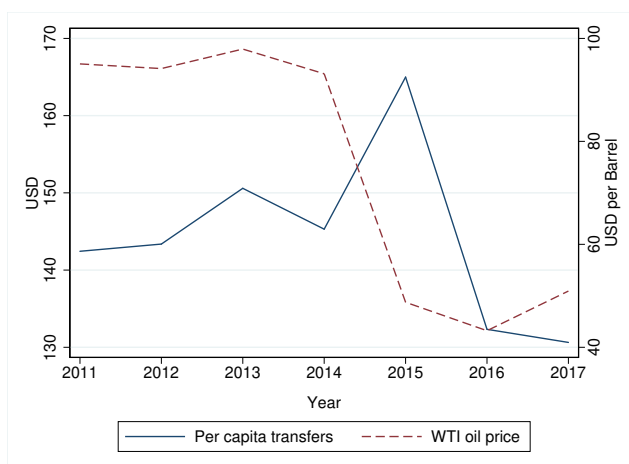
Figures and Tables

Figure 1: Evolution of International Crude Oil Prices and Public Transfers

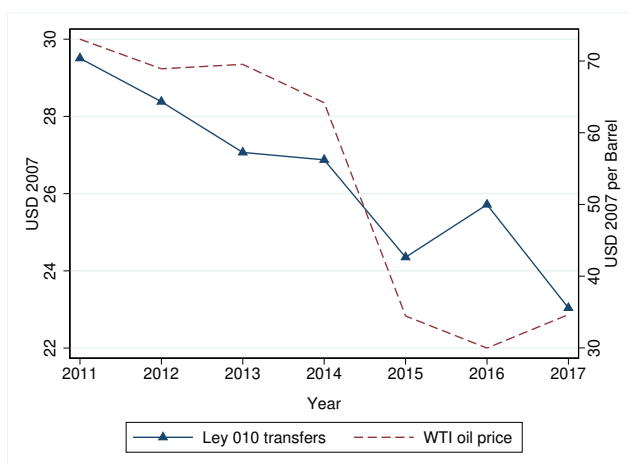
(a) Oil Prices vs. Oil Production



(b) Oil Prices vs. Central Government Transfers



(c) Oil Prices vs. Ley 010 Transfers

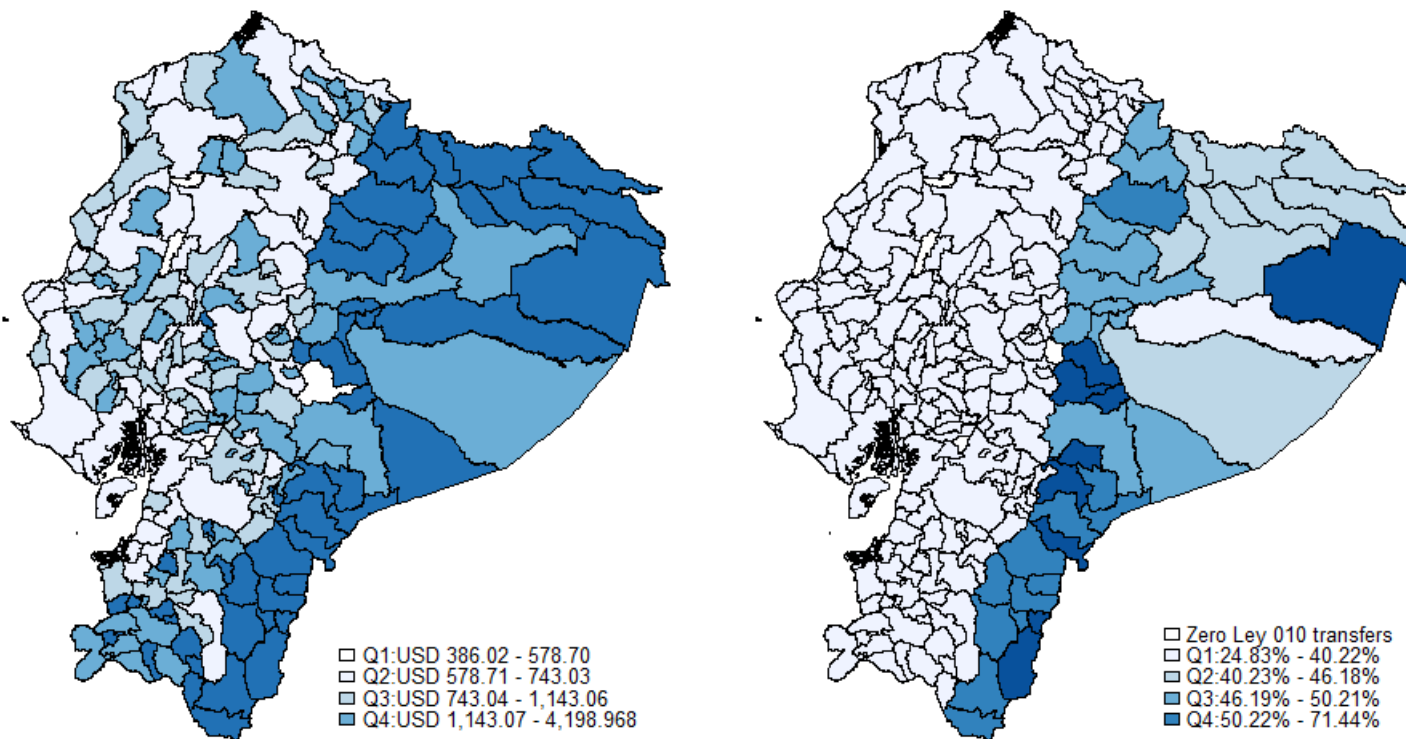


Notes: This figure shows the time series evolution of per capita public transfers, oil national production, and international West Texas Intermediate (WTI) oil prices. *Source:* Own elaboration. Oil production provided by Petroecuador. Public transfers information available from SENPLADES. Oil prices retrieved from the Federal Reserve Economic Data Portal (FRED).

Figure 2: Spatial Distribution of Central Government Transfers in Continental Ecuador (Cumulative 2011-2017)

(a) Per capita central government transfers (2007 USD)

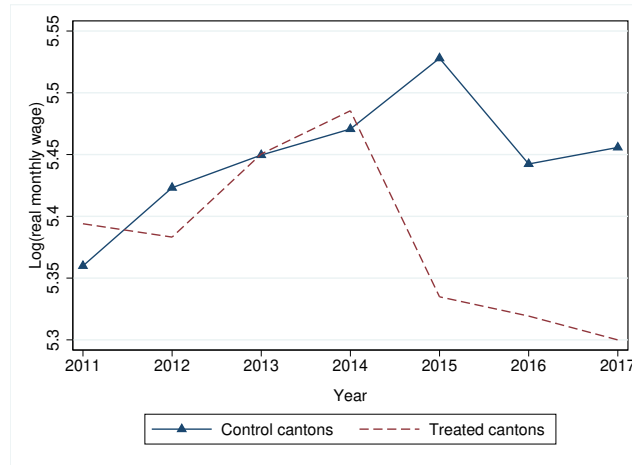
(b) Participation of oil-related *Ley 010* transfers (%)



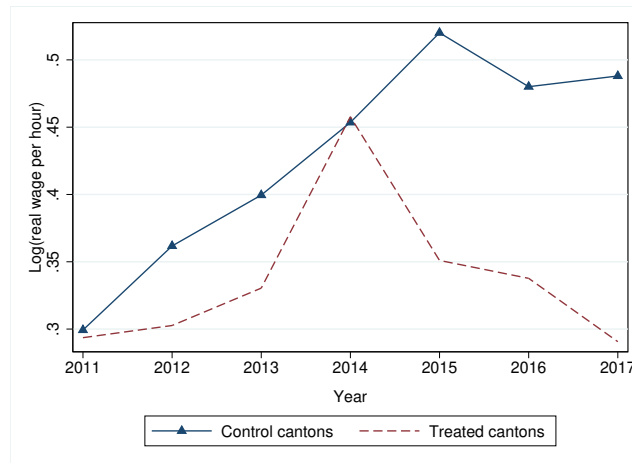
Notes: These maps show the geographical distribution of per capita public transfers (Panel a), and the participation of Ley 010 related transfers in such total (Panel b). Insular cantons not included. *Source:* Own elaboration with Public transfers information retrieved from SENPLADES.

Figure 3: Parallel Trends on Labor Market Outcomes

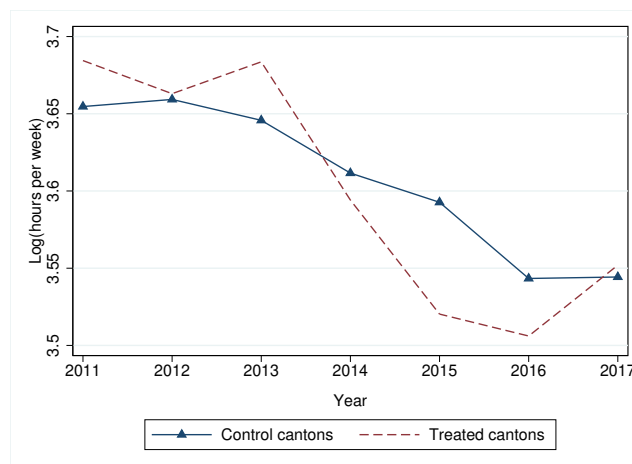
(a) Monthly Wages (log)



(b) Hourly Wages (log)



(c) Weekly working hours (log)



Notes: These figures show the time trend evolution of some labor market outcomes between treated and control cantons. Treated cantons are defined as those oil-producing areas entitled to receive Ley 010 royalties. Control cantons are defined as all remaining cantons in the country. *Source:* Own elaboration with information retrieved from the quarterly national employment survey ENEMDU, conducted by Ecuador's National Bureau of Statistics and Censuses (INEC).

Table 1: Summary statistics: Differences in means from 2010 population census

Canton attributes (Census 2010)	All Cantons			Cantons in ENEMDU Sample		
	Treated (1)	Control (2)	(Treated-Control) (3)	Treated (4)	Control (5)	(Treated-Control) (6)
Total population (10,000) s.e.	17.40 (3.06)	76.31 (18.42)	-58.91 (18.70)***	25.61 (5.34)	81.59 (20.51)	-55.98 (21.22)***
Poverty rate (UBN) s.e.	0.79 (0.02)	0.75 (0.01)	0.04 (0.02)*	0.78 (0.02)	0.76 (0.01)	0.02 (0.03)
Settled less than 5 years (%) s.e.	9.64 (0.66)	5.18 (0.24)	4.45 (0.70)***	10.18 (1.10)	4.92 (0.21)	5.26 (1.10)***
Working in extractive industries (%) s.e.	2.46 (0.71)	0.93 (0.31)	1.53 (0.77)**	1.20 (0.26)	0.79 (0.27)	0.41 (0.37)
Occupation rate (%) s.e.	72.13 (0.66)	66.42 (0.52)	5.71 (0.84)***	71.46 (0.91)	65.65 (0.52)	5.81 (1.03)***
Unemployment rate (%) s.e.	1.64 (0.10)	2.11 (0.08)	-0.47 (0.13)***	1.69 (0.12)	2.20 (0.09)	-0.51 (0.15)***
<i>N</i>	43	177	220	22	158	180

Notes: This table reports differences in means between treated and control cantons on some labor market outcomes according to 2010's population census. Columns (1)-(3) display statistics for all cantons in the country, while columns (4)-(6) present figures only for those cantons in which the labor market household survey (ENEMDU) was successfully conducted throughout the 2011-2017 period. Treated cantons are defined as those eligible to receive oil royalties in accordance with Ley 010, and the control group is defined as all remaining non-eligible cantons. *Source:* Own elaboration with information from Ecuador's National Bureau of Statistics and Censuses (INEC). * p-value < 0.1 ** p-value < 0.05 *** p-value < 0.01.

Table 2: Descriptive Statistics: Individual level sample 2011-2017

	Full sample					Treated		Control		Treated - Control	
	N (1)	Mean (2)	SD (3)	Min (4)	Max (5)	N (6)	Mean (7)	N (8)	Mean (9)	Diff. (10)	s.e. (11)
Panel A: Outcomes											
Working (1=yes)	189,061	0.71	0.45	0.00	1.00	37,760	0.77	151,301	0.71	0.06	0.02***
Unemployed (1=yes)	144,331	0.04	0.21	0.00	1.00	30,384	0.04	113,947	0.04	-0.00	0.01
Working hours (weekly total)	138,204	38.63	14.84	1.00	140.00	29,100	38.27	109,104	38.65	-0.38	0.66
Monthly wage (USD 2007)	118,001	305.53	292.67	0.00	4,456.77	22,954	322.51	95,047	304.37	18.14	13.37
Hourly wage (USD 2007)	118,001	2.00	2.17	0.00	104.04	22,954	2.08	95,047	2.00	0.08	0.08
Monthly total income (USD 2007)	189,061	237.56	350.19	0.00	4,454.72	37,760	219.48	151,301	238.85	-19.37	12.69
No social security (1=yes)	189,061	0.65	0.48	0.00	1.00	37,760	0.68	151,301	0.64	0.03	0.02**
Former migrant (1=yes)	189,061	0.33	0.47	0.00	1.00	37,760	0.50	151,301	0.32	0.18	0.04***
Settled <=5 years (1=yes)	189,061	0.07	0.25	0.00	1.00	37,760	0.10	151,301	0.07	0.03	0.01***
Currently attends education (1=yes)	189,061	0.09	0.28	0.00	1.00	37,760	0.08	151,301	0.09	-0.01	0.01
Married or cohabit (1=yes)	189,061	0.63	0.48	0.00	1.00	37,760	0.69	151,301	0.63	0.06	0.01***
Panel B: Household and individual's attributes											
Age	189,061	37.21	13.10	18.00	65.00	37,760	35.81	151,301	37.31	-1.50	0.16***
Female (1=yes)	189,061	0.52	0.50	0.00	1.00	37,760	0.50	151,301	0.52	-0.01	0.00***
Ethnic minority (1=yes)	189,061	0.19	0.40	0.00	1.00	37,760	0.39	151,301	0.18	0.21	0.06***
Years of education	189,061	10.35	4.61	0.00	23.00	37,760	10.53	151,301	10.34	0.19	0.24
Rural Household (1=yes)	189,061	0.39	0.49	0.00	1.00	37,760	0.60	151,301	0.37	0.23	0.06***
Household receives BDH (1=yes)	189,061	0.21	0.41	0.00	1.00	37,760	0.29	151,301	0.20	0.09	0.03***
Home ownership (1=yes)	189,061	0.13	0.34	0.00	1.00	37,760	0.15	151,301	0.13	0.01	0.02
Land ownership (1=yes)	189,061	0.24	0.43	0.00	1.00	37,760	0.46	151,301	0.22	0.23	0.05***
Bedrooms per person	189,061	0.63	0.36	0.00	8.00	37,760	0.60	151,301	0.63	-0.03	0.02**
Access to electricity (1=yes)	189,061	0.98	0.15	0.00	1.00	37,760	0.91	151,301	0.98	-0.07	0.02***
Access to sewage (1=yes)	189,061	0.54	0.50	0.00	1.00	37,760	0.45	151,301	0.55	-0.10	0.05**
Access to tap water (1=yes)	189,061	0.74	0.44	0.00	1.00	37,760	0.58	151,301	0.75	-0.17	0.04***
Washing machines per household	189,061	0.44	0.51	0.00	10.00	37,760	0.29	151,301	0.45	-0.15	0.03***
Automobiles per household	189,061	0.25	0.50	0.00	5.00	37,760	0.18	151,301	0.26	-0.07	0.03**

Notes: This table reports summary statistics on labor market outcomes (Panel A) and socio-demographic characteristics (Panel B). Columns (1)-(5) pertain to the full sample period of interest (2014-2017), while the remaining columns of the table present differences in means tests between treated and control groups. *Source:* Own elaboration with information from the quarterly labor market household survey ENEMDU conducted by Ecuador's National Bureau of Statistics and Censuses (INEC).

* p-value < 0.1 ** p-value < 0.05 *** p-value < 0.01.

Table 3: Descriptive statistics: Differences in Means Between Active Firms (2011-2017)

Firms outcomes (average 2011-2017)	All firms				Treated	Control	Treated-Control	
	Mean (2)	SD (3)	Min (4)	Max (5)	Mean (7)	Mean (9)	Diff. (10)	s.e. (11)
Total sales (100,000 USD 2007)	14.39	36.92	0.15	429.16	7.75	14.55	-6.80	1.21***
Payroll costs (100,000 USD 2007)	1.69	5.56	0.01	202.80	0.60	1.72	-1.11	0.21***
Employment (# workers)	30.36	93.61	1.00	4,081.43	12.89	30.77	-17.88	2.48***
Average compensation (1,000 USD 2007)	5.01	3.63	0.85	146.36	3.77	5.03	-1.27	0.39***
Productivity per payroll dollar (USD 2007)	16.95	97.79	0.03	14,187.47	24.25	16.78	7.46	2.39***
Productivity per worker (100,000 USD 2007)	1.01	2.86	0.00	272.50	1.11	1.01	0.10	0.05**
N			23,611		537	23,074		23,611

Notes: This table reports summary statistics and differences in means for the balanced panel of firms (2011-2017). This panel only includes firms that reported positive sales in all years. *Source:* Own elaboration with information from the enterprise laboratory dataset from Ecuador's National Bureau of Statistics and Censuses (INEC). * p-value < 0.1 ** p-value < 0.05 *** p-value < 0.01.

Table 4: Effect of the oil price bust on labor market outcomes: Reduced form results

Outcomes	OLS (1)	OLS (2)	OLS (3)	OLS (4)
Panel A: Labor market supply and participation				
Working (1=yes)	0.0191 (0.0180)	0.0375 (0.0269)	0.1107 (0.1231)	-0.0222 (0.0755)
R^2	0.16	0.16	0.16	0.16
N	189,061	189,061	189,061	189,061
Mean of outcome (baseline)	0.69	0.69	0.69	0.69
SD of outcome (baseline)	0.46	0.46	0.46	0.46
KP F-stat	-	-	29.39	23.37
Unemployed (1=yes)	0.0003 (0.0079)	-0.0002 (0.0117)	0.0032 (0.0550)	-0.0006 (0.0441)
R^2	0.03	0.03	0.03	0.03
N	144,331	144,331	144,331	144,331
Mean of outcome (baseline)	0.04	0.04	0.04	0.04
SD of outcome (baseline)	0.20	0.20	0.20	0.20
KP F-stat	-	-	25.45	20.85
Log weekly working hours	-0.0177 (0.0211)	-0.1274 (0.0352)***	-0.6320 (0.2053)***	-0.1281 (0.1117)
R^2	0.08	0.08	0.06	0.08
N	138,204	138,204	138,204	138,204
Mean of outcome (baseline)	3.58	3.58	3.58	3.58
SD of outcome (baseline)	0.51	0.51	0.51	0.51
KP F-stat	-	-	24.53	19.82
Panel B: Wages and earnings				
Log monthly wage	-0.2296 (0.0316)***	-0.2338 (0.0630)***	-0.3527 (0.3389)	-0.7420 (0.2342)***
R^2	0.24	0.24	0.24	0.24
N	116,242	116,242	116,242	116,242
Mean of outcome (baseline)	5.36	5.36	5.36	5.36
SD of outcome (baseline)	0.89	0.89	0.89	0.89
KP F-stat	-	-	31.32	23.81
Log hourly wage	-0.2196 (0.0294)***	-0.1393 (0.0532)***	0.1155 (0.2677)	-0.5608 (0.1902)***
R^2	0.19	0.19	0.19	0.18
N	116,242	116,242	116,242	116,242
Mean of outcome (baseline)	0.37	0.37	0.37	0.37
SD of outcome (baseline)	0.77	0.77	0.77	0.77
KP F-stat	-	-	31.32	23.81
Log monthly total income	-0.2632 (0.0271)***	-0.2187 (0.0461)***	-0.1191 (0.2248)	-0.4518 (0.2179)**
R^2	0.29	0.28	0.29	0.28
N	135,191	135,191	135,191	135,191
Mean of outcome (baseline)	5.35	5.35	5.35	5.35
SD of outcome (baseline)	0.95	0.95	0.95	0.95
KP F-stat	-	-	31.25	24.02
Canton-specific time trends	no	yes	no	yes
IV-2SLS on included confounder	no	no	yes	yes

Notes: This table reports reduced form results on labor market outcomes from the repeated cross-section dataset covering the 2014-2017 period, according to equations (1) and (2) in the main text. All specifications include fixed effects by canton and year, and the set of household and individual attributes displayed in Table 2 (Panel B). In columns (3) and (4), we implement the procedure suggested by Freyaldenhoven et al. (2019), to correct for potential divergent trends between treated and control cantons. Robust standard errors are clustered at the canton level and reported in parentheses. *Source:* Authors' calculations with information from the quarterly national employment survey ENEMDU, conducted by Ecuador's National Bureau of Statistics and Censuses (INEC). * p-value < 0.1 ** p-value < 0.05 *** p-value < 0.01.

Table 5: Effects of public transfers on labor market outcomes: Instrumental variables estimates

Outcomes	OLS (1)	OLS (2)	IV-2SLS (3)	IV-2SLS (4)
Panel A: Labor market supply and participation				
Working (1=yes)	-0.013 (0.068)	-0.164 (0.079)**	0.373 (0.358)	0.261 (0.189)
R^2	0.16	0.16	0.16	0.16
N	189,061	189,061	189,061	189,061
KP F-stat	-	-	17.63	59.19
Unemployed (1=yes)	0.008 (0.043)	0.002 (0.054)	0.007 (0.159)	-0.001 (0.084)
R^2	0.03	0.03	0.03	0.03
N	144,331	144,331	144,331	144,331
KP F-stat	-	-	15.77	56.08
Log weekly working hours	-0.003 (0.079)	-0.038 (0.098)	-0.358 (0.447)	-0.916 (0.271)***
R^2	0.08	0.08	0.08	0.08
N	138,204	138,204	138,204	138,204
KP F-stat	-	-	15.41	56.56
Panel B: Wages and earnings				
Log monthly wage	-0.003 (0.107)	-0.131 (0.171)	-4.394 (1.385)***	-1.622 (0.492)***
R^2	0.24	0.24	0.22	0.24
N	116,242	116,242	116,242	116,242
KP F-stat	-	-	17.02	57.94
Log hourly wage	-0.034 (0.099)	-0.111 (0.136)	-4.203 (1.324)***	-0.967 (0.417)**
R^2	0.19	0.19	0.16	0.19
N	116,242	116,242	116,242	116,242
KP F-stat	-	-	17.02	57.94
Log monthly total income	0.004 (0.122)	-0.228 (0.156)	-5.113 (1.431)***	-1.519 (0.350)***
R^2	0.28	0.28	0.26	0.28
N	135,191	135,191	135,191	135,191
KP F-stat	-	-	16.95	58.51
Fixed effects included	yes	yes	yes	yes
Covariates included	yes	yes	yes	yes
Canton-specific time trends	no	yes	no	yes

Notes: This table reports IV-2SLS effects of per capita public transfers on labor market outcomes from the repeated cross-section dataset covering the 2014-2017 period, according to equations (4) and (5) in the main text. All specifications include fixed effects by canton and year, and the set of household and individual attributes displayed in Table 2 (Panel B). Robust standard errors are clustered at the canton level and reported in parentheses. *Source:* Authors' calculations with information from the quarterly national employment survey ENEMDU, conducted by Ecuador's National Bureau of Statistics and Censuses (INEC). * p-value < 0.1 ** p-value < 0.05 *** p-value < 0.01.

Table 6: Heterogeneity analysis by socio-demographic groups: Effects on labor market outcomes

Outcomes	Rural			Young Adults			Female			Ethnic Minority		
	Yes (1)	No (2)	(Yes-No) (3)	Yes (4)	No (5)	(Yes-No) (6)	Yes (7)	No (8)	(Yes-No) (9)	Yes (10)	No (11)	(Yes-No) (12)
Working (1=yes)	0.231 (0.292)	0.103 (0.292)	0.125 (0.427)	0.352 (0.380)	0.095 (0.195)	0.261 (0.342)	0.209 (0.337)	0.237 (0.189)	-0.027 (0.344)	0.316 (0.315)	0.205 (0.243)	0.112 (0.366)
R^2	0.20	0.14	0.16	0.13	0.16	0.15	0.04	0.06	0.05	0.17	0.16	0.16
N	79,813	109,248	189,061	40,451	148,610	189,061	97,830	91,231	189,061	41,787	147,269	189,056
Unemployed (1=yes)	0.016 (0.118)	0.041 (0.199)	-0.024 (0.235)	-0.164 (0.387)	0.069 (0.095)	-0.233 (0.346)	0.140 (0.199)	-0.085 (0.128)	0.226 (0.220)	-0.010 (0.194)	0.024 (0.134)	-0.034 (0.192)
R^2	0.03	0.03	0.03	0.03	0.01	0.02	0.04	0.02	0.03	0.03	0.03	0.03
N	63,695	80,636	144,331	24,108	120,223	144,331	62,081	82,250	144,331	33,678	110,645	144,323
Log weekly working hours	-1.206 (0.486)**	-0.571 (0.489)	-0.632 (0.674)	-1.387 (0.796)*	-0.843 (0.323)***	-0.551 (0.646)	-0.750 (0.552)	-0.857 (0.325)***	0.102 (0.467)	-1.099 (0.571)*	-0.786 (0.401)*	-0.308 (0.597)
R^2	0.10	0.07	0.08	0.04	0.09	0.08	0.03	0.01	0.03	0.09	0.08	0.08
N	62,266	75,938	138,204	21,618	116,586	138,204	58,837	79,367	138,204	32,483	105,713	138,196
Log monthly wage	-1.854 (0.779)**	-1.072 (0.705)	-0.763 (0.962)	-1.931 (1.116)*	-1.427 (0.588)**	-0.505 (0.986)	-1.064 (0.895)	-1.547 (0.609)**	0.487 (0.899)	-1.696 (0.961)*	-1.135 (0.592)*	-0.570 (0.895)
R^2	0.22	0.23	0.23	0.09	0.27	0.25	0.25	0.17	0.21	0.21	0.24	0.23
N	47,283	68,959	116,242	15,246	100,996	116,242	44,122	72,120	116,242	23,968	92,267	116,235
Log hourly wage	-0.977 (0.680)	-0.676 (0.485)	-0.289 (0.740)	-1.186 (0.876)	-0.818 (0.499)	-0.353 (0.897)	-0.643 (0.729)	-0.900 (0.494)*	0.264 (0.741)	-0.971 (0.782)	-0.627 (0.508)	-0.353 (0.820)
R^2	0.14	0.19	0.17	0.07	0.20	0.19	0.22	0.15	0.18	0.14	0.19	0.18
N	47,283	68,959	116,242	15,246	100,996	116,242	44,122	72,120	116,242	23,968	92,267	116,235
Log monthly total income	-1.994 (0.586)***	-1.279 (0.591)**	-0.704 (0.752)	-4.273 (1.232)***	-1.179 (0.493)**	-3.101 (1.239)**	-0.728 (0.671)	-2.087 (0.641)***	1.355 (0.899)	-1.500 (0.841)*	-1.215 (0.513)**	-0.289 (0.869)
R^2	0.30	0.23	0.26	0.11	0.30	0.28	0.26	0.18	0.23	0.30	0.26	0.27
N	56,488	78,703	135,191	18,299	116,892	135,191	59,094	76,097	135,191	29,254	105,930	135,184

Notes: This table reports IV-2SLS effects of per capita public transfers on labor market outcomes by socio-demographic divides, from the repeated cross-section dataset covering the 2014-2017 period, according to equations (4) and (5) in the main text. All specifications include fixed effects by canton and year, and the set of household and individual attributes displayed in Table 2 (Panel B). Robust standard errors are clustered at the canton level and reported in parentheses. *Source:* Authors' calculations with information from the quarterly national employment survey ENEMDU, conducted by Ecuador's National Bureau of Statistics and Censuses (INEC). * p-value < 0.1 ** p-value < 0.05 *** p-value < 0.01.

Table 7: Heterogeneity analysis by ISCO-08 Occupation: Effects on labor market outcomes

Outcome	Baseline	Managerial	Professionals	Technicians	Clerical Work	Retail and Trade	Skilled Agriculture	Craft and Trades	Operators	Elementary Work
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Log weekly working hours	-0.916 (0.271)***	0.080 (1.117)	-0.353 (0.324)	-0.031 (0.491)	0.105 (0.450)	0.189 (0.376)	-1.539 (0.517)***	-0.641 (0.443)	-0.843 (0.382)**	-1.638 (0.572)***
R^2	0.08	0.10	0.03	0.04	0.04	0.07	0.13	0.09	0.02	0.07
N	138,204	1,236	11,608	4,971	4,877	27,172	29,198	15,986	9,624	32,904
KP F-stat	56.56	14.84	29.82	36.32	49.72	49.51	47.89	71.23	41.00	44.25
Log monthly wage	-1.622 (0.492)***	-1.372 (3.826)	0.070 (0.778)	0.048 (0.700)	-0.597 (0.870)	-0.427 (0.480)	-1.921 (0.952)**	-0.748 (0.655)	-2.585 (1.000)**	-2.180 (0.944)**
R^2	0.24	0.18	0.20	0.19	0.15	0.18	0.13	0.20	0.04	0.12
N	116,242	1,194	11,415	4,821	4,603	23,307	26,827	15,145	9,377	18,925
KP F-stat	57.94	13.65	30.41	37.34	46.80	46.14	42.27	70.09	41.17	52.62
Log hourly wage	-0.967 (0.417)**	-1.090 (4.407)	0.456 (0.842)	-0.095 (0.647)	-0.693 (0.905)	-0.735 (0.692)	-0.571 (0.868)	-0.445 (0.648)	-1.558 (0.941)*	-1.564 (0.842)*
R^2	0.19	0.17	0.17	0.15	0.16	0.10	0.05	0.11	0.03	0.05
N	116,242	1,194	11,415	4,821	4,603	23,307	26,827	15,145	9,377	18,925
KP F-stat	57.94	13.65	30.41	37.34	46.80	46.14	42.27	70.09	41.17	52.62
Log monthly total income	-1.519 (0.350)***	-6.190 (3.226)*	-0.714 (0.911)	0.823 (1.046)	-0.014 (0.763)	-0.730 (0.644)	-2.339 (1.013)**	-0.861 (0.614)	-2.418 (0.848)***	-2.241 (0.904)**
R^2	0.28	0.11	0.19	0.14	0.15	0.15	0.12	0.17	0.07	0.19
N	135,191	1,226	11,577	4,928	4,757	24,777	26,742	15,597	9,569	23,322
KP F-stat	58.51	13.35	30.49	38.54	47.51	46.39	47.95	67.47	40.11	52.58

Notes: This table reports IV-2SLS effects of per capita public transfers on labor market outcomes by ISCO-08 occupation category, from the repeated cross-section dataset covering the 2014-2017 period, according to equations (4) and (5) in the main text. All specifications include fixed effects by canton and year, and the set of household and individual attributes displayed in Table 2 (Panel B). Robust standard errors are clustered at the canton level and reported in parentheses. *Source:* Authors' calculations with information from the quarterly national employment survey ENEMDU, conducted by Ecuador's National Bureau of Statistics and Censuses (INEC). * p-value < 0.1 ** p-value < 0.05 *** p-value < 0.01.

Table 8: Spillover Effects in Non-Extractive Industries: Effects for the Top 5 Sectors

Outcome	Agriculture (1)	Manufacturing (2)	Retail (3)	Trade (4)	Transportation (5)
Log weekly working hours	-1.829 (0.647)***	-1.010 (0.796)	-0.373 (0.481)	-0.185 (0.573)	-0.256 (0.326)
R^2	0.10	0.08	0.07	0.04	0.01
N	47,851	11,471	20,722	7,235	9,178
KP F-stat	43.12	34.42	42.04	32.38	73.79
Log monthly wage	-2.620 (0.921)***	-1.622 (0.941)*	-1.723 (0.752)**	-3.414 (1.590)**	-0.373 (0.857)
R^2	0.14	0.22	0.20	0.05	0.07
N	33,461	9,956	17,393	6,990	8,904
KP F-stat	46.59	32.90	37.76	32.41	72.39
Log hourly wage	-1.103 (0.772)	-0.618 (0.778)	-1.348 (0.706)*	-3.255 (1.667)*	-0.252 (0.793)
R^2	0.06	0.16	0.12	0.06	0.07
N	33,461	9,956	17,393	6,990	8,904
KP F-stat	46.59	32.90	37.76	32.41	72.39
Log monthly total income	-2.666 (0.865)***	-0.868 (1.237)	-1.832 (1.020)*	-2.101 (1.481)	-0.667 (0.872)
R^2	0.18	0.22	0.16	0.13	0.08
N	37,009	10,278	18,411	7,076	8,974
KP F-stat	46.55	33.22	38.39	32.28	73.07

Notes: This table reports IV-2SLS effects of per capita public transfers on labor market outcomes by the top five industry sectors, from the repeated cross-section dataset covering the 2014-2017 period, according to equations (4) and (5) in the main text. All specifications include fixed effects by canton and year, and the set of household and individual attributes displayed in Table 2 (Panel B). Robust standard errors are clustered at the canton level and reported in parentheses. *Source:* Authors' calculations with information from the quarterly national employment survey ENEMDU, conducted by Ecuador's National Bureau of Statistics and Censuses (INEC). * p-value < 0.1 ** p-value < 0.05 *** p-value < 0.01.

Table 9: Mechanisms: Role of different types of transfers

	Total Transfers (1)	Transfers Class A (2)	Transfers Class B (3)	Ley 010 Transfers (4)
$Treated_m * After2015_t$	-0.1494 (0.0139)***	0.0002 (0.0005)	-0.0537 (0.0201)***	-0.0871 (0.0240)***
R^2	0.99	0.99	0.99	0.99
N	189,061	189,061	189,061	189,061
$Treated_m * Price_t$ (10USD)	-0.1436 (0.0187)***	-0.0004 (0.0003)	-0.0668 (0.0254)***	-0.0855 (0.0427)**
R^2	0.99	0.99	0.99	0.99
N	189,061	189,061	189,061	189,061
$Treated_m * \log(Price_t)$	-0.0364 (0.0042)***	-0.0001 (0.0002)	-0.0154 (0.0058)***	-0.0215 (0.0089)**
R^2	0.99	0.99	0.99	0.99
N	189,061	189,061	189,061	189,061

Notes: This table reports first stage effects of the decline of oil prices on per capita public transfers, according to equation (4) in the main text. All specifications include fixed effects by canton and year, canton-specific linear and quadratic time trends, and the set of household and individual attributes displayed in Table 2 (Panel B). Robust standard errors are clustered at the canton level and reported in parentheses. *Source:* Authors' calculations with information from the quarterly national employment survey ENEMDU, conducted by Ecuador's National Bureau of Statistics and Censuses (INEC). Information on public transfers comes from SENPLADES.
 * p-value < 0.1 ** p-value < 0.05 *** p-value < 0.01.

Table 10: Mechanisms: Labor demand adjustments to the oil price bust

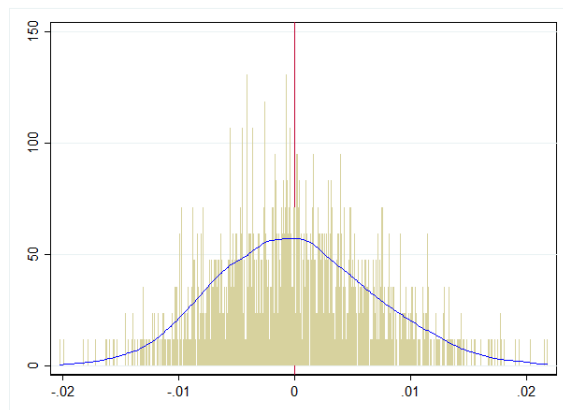
Outcomes	Reduced Form			IV-2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
Log total sales	-0.178 (0.070)**	-0.177 (0.068)**	-0.193 (0.070)***	-1.943 (0.885)**	-1.948 (0.893)**	-1.613 (0.687)**
R^2	0.00	0.00	0.00	-0.03	-0.03	-0.02
N	165,277	164,486	67,970	165,277	164,486	67,970
KP F Stat	-	-	-	38.20	33.67	73.06
Log payroll costs	-0.075 (0.053)	-0.080 (0.053)	-0.104 (0.054)*	-0.820 (0.586)	-0.886 (0.601)	-0.871 (0.473)*
R^2	0.00	0.00	0.00	-0.01	-0.01	-0.01
N	165,277	164,486	67,970	165,277	164,486	67,970
KP F Stat	-	-	-	38.20	33.67	73.06
Log employment	-0.045 (0.044)	-0.047 (0.044)	-0.063 (0.045)	-0.496 (0.478)	-0.522 (0.489)	-0.525 (0.389)
R^2	0.00	0.00	0.00	-0.00	-0.00	-0.00
N	165,277	164,486	67,970	165,277	164,486	67,970
KP F Stat	-	-	-	38.20	33.67	73.07
Log average compensation	-0.030 (0.014)**	-0.033 (0.014)**	-0.041 (0.014)***	-0.325 (0.162)**	-0.364 (0.170)**	-0.346 (0.128)***
R^2	0.00	0.00	0.00	-0.00	-0.00	-0.00
N	165,277	164,486	67,970	165,277	164,486	67,970
KP F Stat	-	-	-	38.20	33.67	73.06
Log productivity per dollar spent	-0.103 (0.049)**	-0.096 (0.046)**	-0.089 (0.048)*	-1.123 (0.619)*	-1.062 (0.593)*	-0.742 (0.445)*
R^2	0.00	0.00	0.00	-0.01	-0.01	-0.00
N	165,277	164,486	67,970	165,277	164,486	67,970
KP F Stat	-	-	-	38.20	33.67	73.06
Log productivity per worker	-0.132 (0.050)***	-0.129 (0.048)***	-0.130 (0.050)***	-1.447 (0.669)**	-1.425 (0.659)**	-1.088 (0.489)**
R^2	0.00	0.00	0.00	-0.02	-0.02	-0.01
N	165,277	164,486	67,970	165,277	164,486	67,970
KP F Stat	-	-	-	38.20	33.67	73.06
All firms	yes	no	no	yes	no	no
Excluding firms in extractive industries	no	yes	yes	no	yes	yes
Excluding firms in Quito and Guayaquil	no	no	yes	no	no	yes

Notes: This table reports reduced form and IV-2SLS effects of per capita public transfers on firms' economic performance indicators, for the period 2011-2017. All specifications include fixed effects by firm and year, linear and quadratic time trends per branch of industry (ISIC4), and a set of interactions between canton's 2010 population census characteristics with an indicator variable taking value one if the observation belongs to the price bust period. Characteristics from the census include: proportion of workers in extractive industries, employment and unemployment rates, and proportions of new jobs created. Robust standard errors are clustered at the canton level and reported in parentheses. *Source:* Own elaboration with information from the enterprise laboratory panel dataset from Ecuador's National Bureau of Statistics and Censuses (INEC). * p-value < 0.1 ** p-value < 0.05 *** p-value < 0.01.

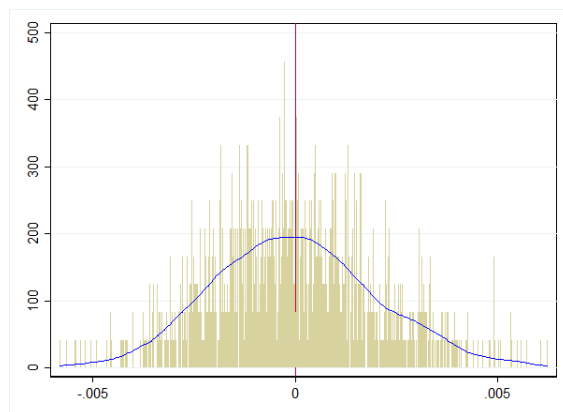
Appendix: Figures

Figure A1: Public Transfers Falsification Tests

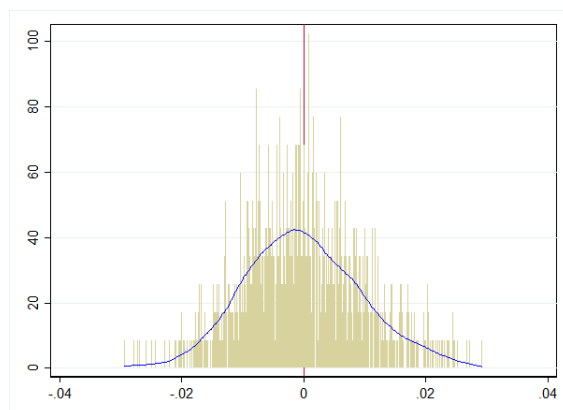
(a) Kernel density estimate for coefficient of $After_{2015}$



(b) Kernel density estimate for coefficient of $Price_t$



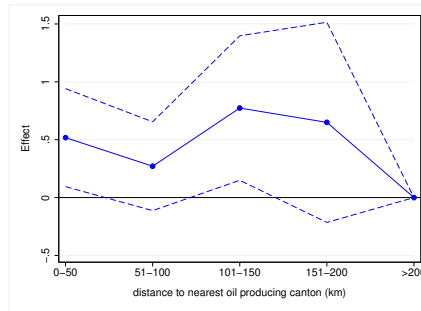
(c) Kernel density estimate for coefficient of $LogPrice_t$



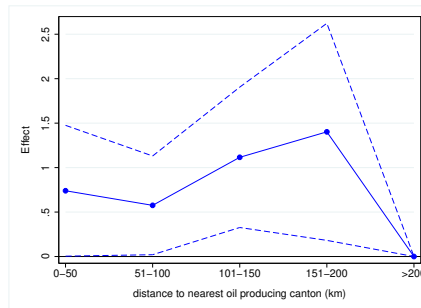
Notes: These figures show the distribution of 1,000 first stage falsification tests, in which canton-level regression coefficients are plotted. All specifications include fixed effects per canton and year, a set of canton-specific linear and quadratic time trends, and a set of interactions between canton's 2010 population census characteristics with an indicator variable taking value one if the observation belongs to the price bust period. Characteristics from the census include: proportion of workers in extractive industries, employment and unemployment rates, and proportions of new jobs created. *Source:* Own elaboration. Public transfers information available from SENPLADES. Oil prices retrieved from the Federal Reserve Economic Data Portal (FRED).

Figure A2: Geographic Spillovers: Reduced Form Results

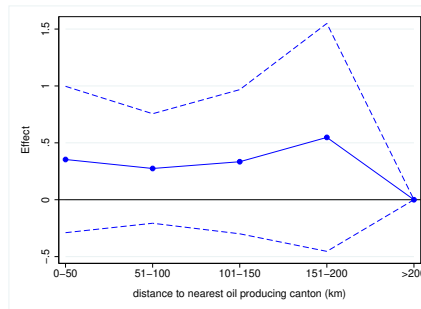
(a) Weekly working hours (log)



(b) Monthly Wages (log)



(c) Hourly wages (log)



Notes: These figures plot reduced form estimates and 95 percent confidence intervals of geographic spillover effects per distance bins of 50km from oil producing areas. All specifications include fixed effects per canton and year, a set of canton-specific linear and quadratic time trends, and the set of household and individual attributes presented in Table 2 (Panel B). *Source:* Own elaboration. Public transfers information available from SENPLADES. Individual information on labor market indicators comes from the quarterly national employment survey ENEMDU, conducted by the National Bureau of Statistics Office (INEC).

Appendix: Tables

Table A1: Effect of the oil price bust on human capital indicators: Reduced form results

Outcomes	(1)	(2)	(3)	(4)
1=no social security	0.0535 (0.0183)***	0.0138 (0.0308)	-0.1487 (0.1257)	0.0843 (0.0555)
R^2	0.13	0.13	0.13	0.13
N	189,061	189,061	189,061	189,061
Mean of Y (baseline)	0.64	0.64	0.64	0.64
SD of Y (baseline)	0.48	0.48	0.48	0.48
KP F-stat	-	-	29.39	23.37
Former migrant (1=yes)	-0.0169 (0.0197)	0.0201 (0.0405)	0.1396 (0.2036)	-0.1001 (0.0904)
R^2	0.06	0.06	0.06	0.06
N	189,061	189,061	189,061	189,061
Mean of Y (baseline)	0.32	0.32	0.32	0.32
SD of Y (baseline)	0.47	0.47	0.47	0.47
KP F-stat	-	-	29.39	23.37
Settled ≤ 5 years (1=yes)	-0.0093 (0.0121)	0.0084 (0.0123)	0.0604 (0.0807)	0.0232 (0.0536)
R^2	0.05	0.05	0.05	0.05
N	189,061	189,061	189,061	189,061
Mean of Y (baseline)	0.06	0.06	0.06	0.06
SD of Y (baseline)	0.24	0.24	0.24	0.24
KP F-stat	-	-	29.39	23.37
Attends education (1=yes)	0.0088 (0.0069)	0.0028 (0.0127)	0.0017 (0.0586)	0.0548 (0.0230)**
R^2	0.13	0.13	0.13	0.13
N	189,061	189,061	189,061	189,061
Mean of Y (baseline)	0.09	0.09	0.09	0.09
SD of Y (baseline)	0.29	0.29	0.29	0.29
KP F-stat	-	-	29.39	23.37
Married or cohabit (1=yes)	0.0055 (0.0128)	-0.0075 (0.0227)	-0.1156 (0.0965)	-0.1679 (0.0608)***
R^2	0.12	0.12	0.12	0.12
N	189,061	189,061	189,061	189,061
Mean of Y (baseline)	0.63	0.63	0.63	0.63
SD of Y (baseline)	0.48	0.48	0.48	0.48
KP F-stat	-	-	29.39	23.37
Covariates included	yes	yes	yes	yes
Canton-specific time trends	no	yes	no	yes
IV-2SLS on included confounder	no	no	yes	yes

Notes: This table reports reduced form results on human capital indicators from the repeated cross-section dataset covering the 2014-2017 period, according to equations (1) and (2) in the main text. All specifications include fixed effects by canton and year, and the set of household and individual attributes displayed in Table 2 (Panel B). In columns (3) and (4), we implement the procedure suggested by Freyaldenhoven et al. (2019), to correct for potential divergent trends between treated and control cantons. Robust standard errors are clustered at the canton level and reported in parentheses. *Source:* Authors' calculations with information from the quarterly national employment survey ENEMDU, conducted by Ecuador's National Bureau of Statistics and Censuses (INEC). * p-value < 0.1 ** p-value < 0.05 *** p-value < 0.01.

Table A2: Effects of public transfers on human capital indicators: Instrumental variables estimates

Outcomes	OLS (1)	OLS (2)	IV-2SLS (3)	IV-2SLS (4)
1=no social security	0.085 (0.072)	0.089 (0.109)	1.043 (0.423)**	0.096 (0.218)
R^2	0.13	0.13	0.12	0.13
N	189,061	189,061	189,061	189,061
KP F-stat	-	-	17.63	59.19
Former migrant (1=yes)	-0.008 (0.081)	-0.136 (0.073)*	-0.329 (0.396)	0.140 (0.286)
R^2	0.06	0.06	0.06	0.06
N	189,061	189,061	189,061	189,061
KP F-stat	-	-	17.63	59.19
Settled <=5 years (1=yes)	0.028 (0.043)	-0.015 (0.051)	-0.181 (0.251)	0.058 (0.084)
R^2	0.05	0.05	0.05	0.05
N	189,061	189,061	189,061	189,061
KP F-stat	-	-	17.63	59.19
Attends education (1=yes)	-0.003 (0.024)	0.029 (0.031)	0.171 (0.134)	0.019 (0.089)
R^2	0.13	0.13	0.13	0.13
N	189,061	189,061	189,061	189,061
KP F-stat	-	-	17.63	59.19
Married or cohabit (1=yes)	-0.018 (0.037)	-0.017 (0.045)	0.106 (0.253)	-0.052 (0.160)
R^2	0.12	0.12	0.12	0.12
N	189,061	189,061	189,061	189,061
KP F-stat	-	-	17.63	59.19
Fixed effects included	yes	yes	yes	yes
Covariates included	yes	yes	yes	yes
Canton-specific time trends	no	yes	no	yes

Notes: This table reports IV-2SLS effects of per capita public transfers on some human capital indicators from the repeated cross-section dataset covering the 2014-2017 period, according to equations (4) and (5) in the main text. All specifications include fixed effects by canton and year, and the set of household and individual attributes displayed in Table 2 (Panel B). Robust standard errors are clustered at the canton level and reported in parentheses. *Source:* Authors' calculations with information from the quarterly national employment survey ENEMDU, conducted by Ecuador's National Bureau of Statistics and Censuses (INEC). * p-value < 0.1 ** p-value < 0.05 *** p-value < 0.01.

Table A3: Effect of the oil price bust on public transfers: First stage results

	(1)	(2)	(3)
Panel A: Canton level analysis (2011-2017)			
$Treated_m * After2015_t$	-0.072 (0.007)***		
$Treated_m * Price_t$ (10USD)		0.019 (0.003)***	
$Treated_m * \log(Price_t)$			0.056 (0.014)***
R^2	0.99	0.99	0.99
N	1,540	1,540	1,540
KP F-statistic	94.05	57.69	16.84
Panel B: Individual level analysis (Sample 2014-2017)			
$Treated_m * After2015_t$	-0.149 (0.014)***		
$Treated_m * Price_t$ (10USD)		0.036 (0.004)***	
$Treated_m * \log(Price_t)$			0.144 (0.019)***
R^2	0.99	0.99	0.99
N	189,061	189,061	189,061
KP F-statistic	115.99	76.69	59.19

Notes: This table reports first stage results on per capita public transfers. Panel A reports coefficients from canton-level regressions. Panel B present coefficients from the individual cross-section dataset for period 2014-2017. All specifications include fixed effects by canton and year, a set of canton-specific linear and quadratic time trends, and the set of household and individual attributes displayed in Table 2 (Panel B). Robust standard errors are clustered at the canton level and reported in parentheses. *Source:* Authors' calculations with information from the quarterly national employment survey ENEMDU, conducted by Ecuador's National Bureau of Statistics and Censuses (INEC). * p-value < 0.1 ** p-value < 0.05 *** p-value < 0.01.

Table A4: Effect of the oil price bust on public transfers: First stage results

	(1)	(2)	(3)	(4)	(5)
Panel A: Canton level analysis (2011-2017)					
$Treated_m * \log(Price_t)$	-0.115 (0.012)***	-0.045 (0.014)***	-0.077 (0.012)***	-0.312 (0.019)***	-0.098 (0.012)***
$Treated_m * \log(PriceCopper_t)$		-0.224 (0.028)***			
$Treated_m * \log(PriceGold_t)$			-0.473 (0.032)***		
$Treated_m * \log(PriceBanana_t)$				-0.780 (0.070)***	
$Treated_m * \log(PriceShrimp_t)$					0.219 (0.016)***
R^2	0.99	0.99	0.99	0.99	0.99
N	1,526	1,526	1,526	1,526	1,526
KP 1st stage F-statistic	94.00	10.28	40.84	258.64	71.30
Panel B: Individual level analysis (Sample 2014-2017)					
$Treated_m * \log(Price_t)$	-0.144 (0.019)***	-0.643 (0.052)***	-0.212 (0.020)***	-2.129 (0.190)***	-0.781 (0.064)***
$Treated_m * \log(PriceCopper_t)$		1.242 (0.118)***			
$Treated_m * \log(PriceGold_t)$			-1.662 (0.158)***		
$Treated_m * \log(PriceBanana_t)$				-16.374 (1.551)***	
$Treated_m * \log(PriceShrimp_t)$					1.511 (0.144)***
R^2	0.99	0.99	0.99	0.99	0.99
N	189,061	189,061	189,061	189,061	189,061
KP 1st stage F-statistic	59.19	153.19	112.34	125.55	147.16

Notes: This table reports first stage results on per capita public transfers, in which we test the robustness of other commodity prices as competing instruments. Panel A reports coefficients from canton-level regressions. Panel B present coefficients from the individual cross-section dataset for period 2014-2017. All specifications include fixed effects by canton and year, a set of canton-specific linear and quadratic time trends, and the set of household and individual attributes displayed in Table 2 (Panel B). Robust standard errors are clustered at the canton level and reported in parentheses. *Source:* Authors' calculations with information from the quarterly national employment survey ENEMDU, conducted by Ecuador's National Bureau of Statistics and Censuses (INEC). Oil prices information comes from the Federal Reserve Economic Data Portal (FRED). Information on commodity prices comes from the World Bank. * p-value < 0.1 ** p-value < 0.05 *** p-value < 0.01.

Table A5: Heterogeneity analysis by socio-demographic groups: Effects on human capital indicators

Outcomes	Rural			Young Adults			Female			Ethnic Minority		
	Yes (1)	No (2)	(Yes-No) (3)	Yes (4)	No (5)	(Yes-No) (6)	Yes (7)	No (8)	(Yes-No) (9)	Yes (10)	No (11)	(Yes-No) (12)
1=no social security	0.379 (0.468)	0.054 (0.308)	0.328 (0.552)	-0.075 (0.382)	0.241 (0.288)	-0.313 (0.389)	0.219 (0.286)	0.062 (0.310)	0.164 (0.258)	0.485 (0.403)	-0.134 (0.286)	0.616 (0.434)
<i>R</i> ²	0.08	0.16	0.13	0.03	0.12	0.10	0.14	0.11	0.12	0.10	0.13	0.12
<i>N</i>	79,813	109,248	189,061	40,451	148,610	189,061	97,830	91,231	189,061	41,787	147,269	189,056
Former migrant (1=yes)	0.198 (0.439)	0.647 (0.497)	-0.462 (0.620)	0.694 (0.415)*	0.176 (0.401)	0.519 (0.462)	0.391 (0.408)	0.391 (0.392)	-0.004 (0.356)	0.420 (0.523)	0.661 (0.405)	-0.235 (0.525)
<i>R</i> ²	0.06	0.06	0.06	0.06	0.03	0.04	0.06	0.07	0.06	0.06	0.06	0.06
<i>N</i>	79,813	109,248	189,061	40,451	148,610	189,061	97,830	91,231	189,061	41,787	147,269	189,056
Settled <=5 years (1=yes)	0.143 (0.299)	0.187 (0.182)	-0.055 (0.350)	0.270 (0.279)	0.099 (0.158)	0.168 (0.276)	0.055 (0.166)	0.240 (0.180)	-0.190 (0.154)	0.239 (0.234)	0.131 (0.191)	0.105 (0.287)
<i>R</i> ²	0.05	0.05	0.05	0.06	0.04	0.05	0.06	0.05	0.05	0.05	0.05	0.05
<i>N</i>	79,813	109,248	189,061	40,451	148,610	189,061	97,830	91,231	189,061	41,787	147,269	189,056
Attends education (1=yes)	0.064 (0.130)	0.015 (0.176)	0.053 (0.218)	0.082 (0.318)	0.045 (0.074)	0.040 (0.330)	0.040 (0.141)	-0.001 (0.142)	0.041 (0.179)	0.078 (0.167)	0.006 (0.146)	0.074 (0.232)
<i>R</i> ²	0.09	0.15	0.13	0.09	0.02	0.07	0.12	0.13	0.13	0.10	0.13	0.13
<i>N</i>	79,813	109,248	189,061	40,451	148,610	189,061	97,830	91,231	189,061	41,787	147,269	189,056
Married or cohabit (1=yes)	0.140 (0.332)	-0.249 (0.343)	0.386 (0.507)	-0.518 (0.411)	0.076 (0.232)	-0.596 (0.490)	-0.029 (0.285)	0.169 (0.235)	-0.194 (0.300)	0.114 (0.385)	-0.065 (0.281)	0.175 (0.473)
<i>R</i> ²	0.12	0.13	0.12	0.10	0.07	0.08	0.08	0.20	0.14	0.11	0.13	0.12
<i>N</i>	79,813	109,248	189,061	40,451	148,610	189,061	97,830	91,231	189,061	41,787	147,269	189,056

Notes: This table reports IV-2SLS effects of per capita public transfers on some human capital indicators by socio-demographic divides, from the repeated cross-section dataset covering the 2014-2017 period, according to equations (4) and (5) in the main text. All specifications include fixed effects by canton and year, and the set of household and individual attributes displayed in Table 2 (Panel B). Robust standard errors are clustered at the canton level and reported in parentheses. *Source:* Authors' calculations with information from the quarterly national employment survey ENEMDU, conducted by Ecuador's National Bureau of Statistics and Censuses (INEC). * p-value < 0.1 ** p-value < 0.05 *** p-value < 0.01.