Tax incentives, Private Investment and Employment: Evidence from an Ecuadorian reform

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Tax incentives, Private Investment and Employment: Evidence from an Ecuadorian reform*

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Abstract

Tax incentives are a common policy to attract investment and create formal employment in developing countries. However, scarce evidence is available that study the impact of such reforms. This paper estimates the effect of the Organic Law for Productive Development, Attraction of Investments, Generation of Employment and Stability and Fiscal Balance on new investment and employment applied in august 2018 in Ecuador. Using event study designs and difference-in-differences models, I find that the policy implementation does not have an effect on the attraction of new investments and creation of new employment for prioritized sectors compared to non-prioritized sectors over the last quarter of 2018 and the 2019. Also, I provide several robustness checks to support the results. This matters from a policy perspective in a country with a low private investments and high share of people out of formal employment.

Keywords: Tax incentives; employment; investment; developing country; Ecuador
JEL Codes: H25, H32, H54

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1 Introduction

It is well known that fiscal incentives are a very common policy to attract business investment and create formal employment, especially in developing countries. The core idea behind this hypothesis is that through changes in the corporate income tax (CIT), via temporary exemptions (tax holidays), or simply by the reduction of the CIT rate, domestic and foreign investment could increase, bolstering economic growth, and later may generate employment (Arnold et al., 2011).

In this sense, the arrival of new investments could help the government to reduce unemployment problems, budget deficits, among others. However, the use of tax incentives in developing countries is controversial, as they usually come with significant, and sometimes overlooked, costs (Klemm and Van Parys, 2012). Furthermore, there are several potentially serious adverse consequences from the widespread use of tax incentives (see, for an extensive justification, Zee et al., 2002; Klemm, 2010). On the other hand, it may happen that such a policy has no effect on attracting investment and job creation in developing countries due to major structural problems such as corruption, ease of doing business, macroeconomic and political instability, administrative barriers, transparency, lack of rule of law, among others (see, for example, Morisset and Pirnia, 2000; Zee et al., 2002; Dollar et al., 2005; Rahman, 2014).

Despite this, in many developing countries it has become very common to implement tax-incentives public policies focused on attracting investment and job creation. A plausible reason justifying this behavior might be the lack of empirical evidence assessing the impact of tax incentives on business investment and formal employment. In addition, these type of policies are often seen as a "development" tool by policy makers in an attempt to compensate for structural weaknesses of the country (Klemm, 2010). In this sense, this paper seeks to fill that gap by providing robust empirical evidence on the effect of tax incentives on business investment and employment, since it is still scarce in developing economies.

The aim of this paper, therefore, is to evaluate the impact of a tax incentive program introduced by the Ecuadorian government on formal employment and business investment. For this, I use two administrative data sets, one provided by the regulatory institution of firms on new investments, and the second one by the social security institution on new contracts and total number of formal employees. With this, I exploit the exogeneity of the Organic Law for Productive Development, Attraction of Investments, Generation of Employment, Stability, and Fiscal Balance introduced in august 21, 2018, as it was largely unpredicted by firms and economic agents. Formally, I use a difference-in-differences (DiD) strategy taking advantage of the design of the policy as it was targeted to benefit a specific group of economic activities. Thus, I compare affected vs. non-affected economic sectors before and after the policy implementation. In addition, the econometric strategy is based on several pillars, first, it is very unlikely for firms to change from a non-prioritized sector to a prioritized sector since they must incur in drastic changes in capital and labor in the short-term. Second, similar to Fuest et al. (2018), in this type setting, general equilibrium effects on interest rates or consumer prices[1], which may complicate measuring the incidence of the tax incentives on investment and employment, are likely to be of minor importance. Finally, something remarkable about this incentive policy is that it is given to new national and foreign investments, already established or not in the country. Therefore, the increase in capital is also included in this policy, but not the transfer of assets from an already established firm to another, allowing to capture purely new investments.

As a first insight of the results, I find no evidence supporting a significant effect of the law on neither new employment nor investments. In addition, I do not find any suspicious of pre-trends as shown in an event study setting. Moreover, the results are very similar after performing several robustness tests such as placebo analysis.

I contribute to the empirical literature in several ways. First, one of the main problems in analyzing the effect of a reduction in taxes or fiscal incentives on business investment and job creation in the literature has been that they use cross-country research designs, which makes it difficult to fulfill the assumption of complete exogeneity of policy implementation, leading to non-causal evidence, or at least severe suspicions. In this sense, this research

[1]Interest rates in Ecuador are rigid and the Ecuadorian Central Bank (BCE) is the one that fixed the percentage.

overcomes this issue exploiting exogeneity from a policy design with allows to construct a valid control group. Second, the research of the effect of tax incentives has focused on their impact on foreign direct investment (FDI), leaving aside the effect on business investment (FDI and local) and job creation. This paper covers this gap in the literature. Third, although having fiscal incentive policies is very popular in developing countries, very little is known about their effects in these countries, perhaps due to the lack of availability of administrative data. This research analyzes the effect of this type of policy in Ecuador, a developing economy making use of administrative records avoiding common problems from survey data.

The literature on this topic is controversial. Authors like Zee et al. (2002) discusses the objectives, cost effectiveness, and transparency of implementing tax incentives, which in some cases might attract business investments (see, for example, Shah, 1995; Chirinko and Wilson, 2010a,b, 2017), in other cases is effective in attracting FDI (see, for example, Tung and Cho, 2000; Klemm and Van Parys, 2012; De Mooij and Ederveen, 2003; Mooij and Ederveen, 2008; Feld and Heckemeyer, 2011). Nevertheless, in some cases this policy is found ineffective on attracting new investments (see, for example, Klemm and Van Parys, 2012; Van Parys and James, 2010). In terms of the effect of this kind of policies on employment, the empirical evidence is also scarce. For instance, Garsous et al. (2017) find a positive effect of a tax incentive policy on employment in a particular area in the tourism sector in Brazil; something similar is found by Fuest et al. (2018) analyzing the effect on wages in Germany. In general, the empirical literature is inconclusive for various reasons, such as data used, methodology, cross-country analysis, single country analysis, different outcomes and period of analysis.

The rest of the paper is organized as follows. Section 2 describes the institutional setting of business taxation in Ecuador and discusses in more details the 2018 tax incentives program. Section 3 introduces the main data sources used and describes the treatment and control groups. Section 4 presents the identification and empirical strategy. Section 5 shows the main results of the effect of the policy implementation on new investments and formal employment. Finally, Section 6 concludes.

2 Institutional Background

2.1 Business Taxation in Ecuador

Ecuador is a Latin American, developing, dollarized, commodity depending (mostly oil depending), small and open economy. Since the 1990s, oil and tax revenues have been the main source of financing (above the line) for public spending (Ramírez-Álvarez and Carrillo-Maldonado, 2020). Although, the first years of dollarization (since 2000) were approved few tax reforms in Ecuador, it is not until the enactment of the Tax Equity Reform Law of December 2007 where tax management is reorganized and that helped the tax contribution to increasing, reaching an average of 62% of total income in the period 2007-2015 (Ramírez-Álvarez and Carrillo-Maldonado, 2020; Carpio and Carrasco, 2012).[2] However, since 2016, tax revenues have been representing less and less with respect to total revenues, on average 39% between 2016 and 2019.[3]

Furthermore, since 2016 tax revenues represents around 14% of Gross Domestic Product (GDP), 0.7 percentage points less than the four pre-years period, on average. Being the value added tax (VAT) and the income tax the most important. In particular, corporate income tax (CIT) collection during the 2016-2019 period has had a 7% share, on average, of gross tax collection. However, the collection of this tax has been decreasing in recent years.[4] Moreover, the CIT represents less than 4% of the GDP (Deza et al., 2020).

The CIT rate structure to which Ecuadorian firms are subject to is detailed in the "Ley Orgánica de Régimen Tributario Interno (LORTI)" (Organic Law of the Regime of Internal Tax) and its regulations. Under this regime, firms must declare their revenues and costs to calculate and declare their profits. Profits or earnings before taxes are then adjusted by exempt income, deductions, and nondeductible expenses covered by the law and its regulations

[2]For a detailed tax reforms since dollarization, see, Cueva et al. (2018)
[3]Data obtained from Central Bank of Ecuador (BCE).
[4]Data obtained from Servicio de Rentas Internas (SRI).

(Beverinotti et al., 2021). Currently, the nominal CIT for firms in Ecuador stands at 22%; Until 2010, the rate amounted to 25% and then (since 2011) it decreased by 1 percentage point each year to remain at the current rate as of 2013. This modification was generated within the framework of the Organic Code of Production, Trade and Investments (COPCI) (Deza et al., 2020). One particularity in the Ecuadorian tax regime is that, until 2019, firms had to pay a mandatory minimum advance payment toward their tax obligation during the fiscal year (anticipo del impuesto a la renta) (Beverinotti et al., 2021; Deza et al., 2020). This amount amount is calculated as a percentage of the total assets (0.2%), of the total costs and expenses deductible for income tax purposes (0.2%), of the total assets (0.4%), and of the Total taxable income for income tax purposes (0.4%), calculated in the annual declaration of the previous fiscal year. As of 2011 and until the end of 2017, the advance was a minimum and final payment, not subject to reimbursement in the event that it was greater than the tax due (see, for details, Beverinotti et al., 2021; Deza et al., 2020).[5]

Deza et al. (2020) mention that tax incentives and benefits in the CIT regime in Ecuador amounted to about 1.2% of GDP in 2016. Moreover, the authors argue that the reduction or elimination of tax incentives has the potential to generate important fiscal space for the Ecuadorian economy. If tax incentives are reduced or rationalized, the deficit could be reduced by up to 1.2 percentage points in a static manner, or these resources could be allocated to counter-cyclical public policies that reactivate aggregate demand, as shown by a vast literature on the multiplier tax in times of recession.

2.2 The reform: Organic Law for Productive Development, Attraction of Investments, Generation of Employment and Stability and Fiscal Balance

On August 21, 2018, the Organic Law for Productive Development, Attraction of Investments, Generation of Employment, and Stability and Fiscal Balance entered into effect in Ecuador. This law proposes a long-term economic stability plan, as well as incentives to attract new investments to the country, both internal and external, promoting employment and boosting production and the economy. In this sense, the law has several chapters where the incentives are established. For example, Chapter 1 mentions the remittance of interest, fines and surcharges of tax, fiscal and customs obligations; Chapter 2 (the one of interest in this analysis) mentions the specific incentives for attracting private investment; Chapter 3 mentions the incentives for Social Interest housing; Chapter 4 mentions various reforms to various legal bodies; Finally, in other sections several previous laws that have to do with macroeconomic stability are reformed.[6]

Article 26 of the second chapter of the law mentions the exemption from CIT for new productive investments in prioritized sectors. It also mentions that the new productive investments, according to the definitions established in sub-paragraphs a) and b) of article 13 of the Organic Code of Production, Commerce and Investments, which start from the effective date of this Law, in the established prioritized sectors in article 9.1 of the Internal Tax Regime Law (LORTI, in Spanish), they will be entitled to exemption from CIT, and its advance payment, for 12 years, counted from the first year in which income attributable directly and solely to the new investment, and that they are outside the urban jurisdictions of the cantons of Quito and Guayaquil.

In addition, the Law mentions that the investments made in these sectors in the urban areas of Quito and Guayaquil may benefit from the same exemption for 8 years and that for new firms, as well as for those already existing, this exemption will apply only in firms that generate net employment, for which the regulations to this Law will establish the conditions and procedures for the application and verification of this requirement, in view of the size of the companies that want to access it.

In particular, the exemption from CIT and its advance payment for current firms and new 12-year firms will be given when the new investments take place in cities throughout the country except Quito and Guayaquil urban areas. They will have an 8-year exoneration when the new investments take place in the Quito and Guayaquil urban

[5]For an extensive details of positive, negative and neutral changes for tax collection, see Carpio and Carrasco (2012).

[6]To read the Law, go to the next url: `https://www.gob.ec/sites/default/files/regulations/2018-09/Documento_Ley-Org%C3%A1nica-Fomento-Productivo-Atracci%C3%B3n-Inversiones.pdf`.

areas. Also, when the investment is made in border cantons, within the prioritized industrial, agro-industrial and agro-associative sectors, the CIT exemption will be 15 years. In new and existing firms, the incentive will apply only if net employment is generated, with the conditions and procedures established by the Regulation, considering the size of the firm. Said incentives can also be applied by firms constituted prior to the entry into force of this law, in which case the exemption will apply proportionally to the value of the new productive investments.[7]

In general, this law sought to promote a clear legal framework that encourages private activity and employment, by eliminating the minimum advance payment of Income Tax, the gradual reduction of the foreign exchange outflow tax (ISD), based on in the conditions of public finances and balance of payments, prior favorable opinion of the governing body of public finances and maintaining the tax credit for ISD paid on the import of inputs, raw materials and capital goods.

3 Data

3.1 Main data sources

I use two main administrative data sets provided one by the Superintendencia de Compañías, Valores y Seguros (SCVS) and the other by the Social Security Administration of Ecuador (IESS, in Spanish). Regarding private investment data, I use administrative data provided by the SCVS, which is the regulatory institution of firms in Ecuador, on the universe of total private investments allocated to each firm from years 2016 to 2019, corresponding to 2 years before the policy implementation and one year after.[8] The unit of observation is the firm for which there is have information on the province-municipality of location, economic activity (at the four-digit ISIC level), the type of investment received that can be national of foreign, and the total amount of capital received, in u.s dollars. For the purpose of this analysis, I collapse this data set at the province, economic activity and quarter level resulting in 24 provinces, 19 economic activities, and 16 quarter-year categories, with a total of 7,296 observations.

Regarding inflows to formal employment, that is, new contracts registered and employment stock in the social security, I use administrative records provided by the IESS on the universe of new employment spells for individuals working in the formal private sector and employment stock.[9] In this data set, the unit of observation is the employment spell for which there is information on the date of the contract, the economic activity of the position, the workplace at the province level. There is also information on the wage (in American dollars), the gender and age of the employee. Moreover, I also have information on the total number of individuals working in the formal sector to account for any change in labor force constitution. The time horizon spans from year 2016 to 2019. As in the investment data, I also collapse this social security records at the province, economic activity and quarter level. With this, I merge the two datasets obtaining a balance panel of provinces-economic activity at the quarter-year level.

3.2 Control and treatment group definition

I define treatment and control groups based on economic activities at the one-digit ISIC level (ISIC Rev.4 classification) according on whether the activity is included or not in the prioritized sectors as established in the Organic law of the Internal Tax Regime (LORTI), respectively. Due to the fact that the classification of the prioritized sectors in the LORTI is ambiguous, and since there is no official classification at the ISIC level of which sectors comprise it, in this paper it is grouped to one ISIC digit.[10] An extensive list with the treated economic activities is shown in Table 6 in the appendix.

[7] To read the specifications, go to the next url: `https://www.sri.gob.ec/ley-organica-fomento-productivo#:~:text=La%20Ley%20Org%C3%A1nica%20para%20el,el%20empleo%20y%20dinamizando%20la` and this url: `http://www.inteligenciaproductiva.gob.ec/archivos/beneficios_ley_organica_para_el_fomento_productivo.pdf`.

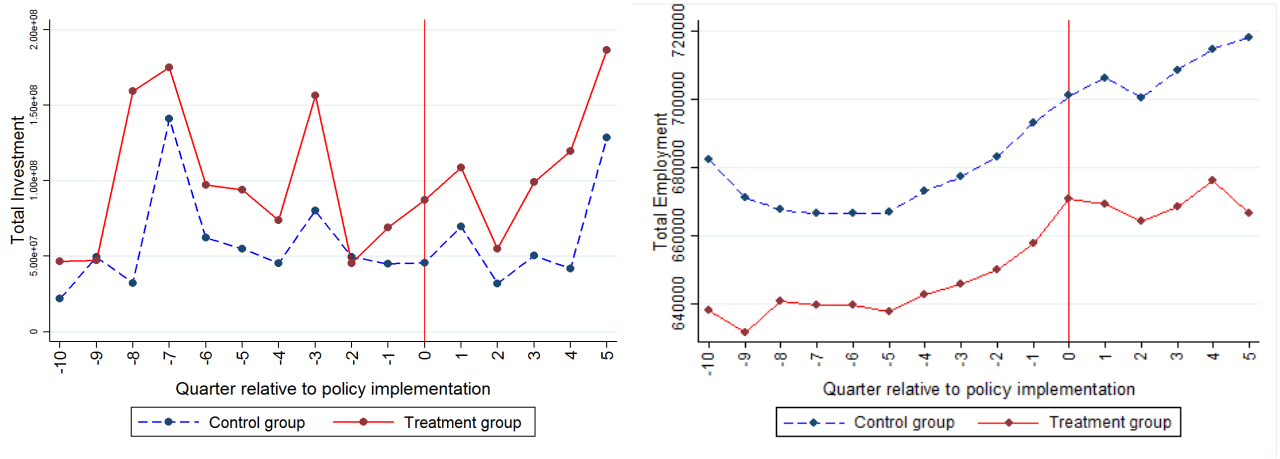
[8] We use data until year 2019 to avoid year 2020 due to the COVID-19 pandemic and all the economic consequences it has risen.

[9] I refer to employment stock to the stock of social security contributors.

[10] One of the main reasons for making this classification is because at two, four and six levels of the ISIC there are many sectors that do not receive any national or foreign investment and this could bias the results downward.

In Figure 1, I present the evolution of the total (new) investment and the total employment stock in Ecuador, before and after the policy implementation for the treatment and the control groups. Panel (a) shows that before the policy the trends in total (new) investment in formal firms looks very similar for both groups, which might support no differential pre-trends. After the policy implementation, the outcome of interest also evolves in the same trend, though, with a more pronounced slope for the treated group. Panel (b) shows the evolution for the second outcome, the employment stock, again before the policy, trends look very similar for the treated and control groups, though for the post-period it does not seem that the treated group experienced a sharper increase in employment stock. In any case, this is first descriptive evidence on trends before and after the new law.

Figure 1: Total investment and employment in Ecuador

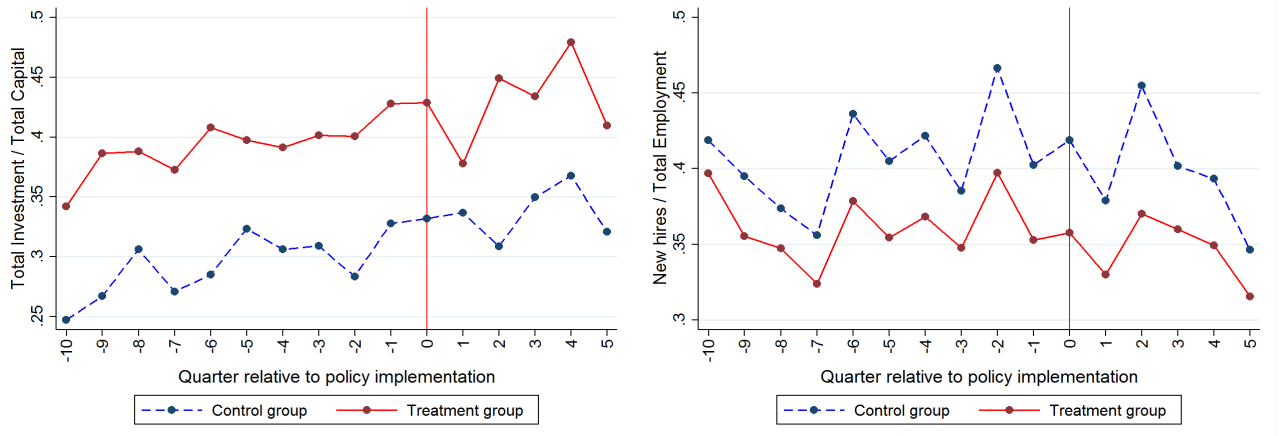


(a) Total investment

(b) Total employment

Source: Superintendencia de Compañías, Valores y Seguros and Instituto Ecuatoriano de Seguridad Social.
Elaboration: The author.

Figure 2: Mean values of the ratio of total investment and ratio of new hires in Ecuador



(a) Total investment / Total capital

(b) New hires / Total employment

Source: Superintendencia de Compañías, Valores y Seguros and Instituto Ecuatoriano de Seguridad Social.
Elaboration: The author.

As labor and economic markets may be different across industries, provinces, and time, it is better to work with a relative measure instead of an absolute. In this sense, for the first outcome, I construct the ratio between total investment over total capital, and for the second outcome, the total new contracts over total number of workers (employment stock). In Figure 2, I show the mean values of these two new measures before and after the policy date, for the treated and control groups. Both Panels (a) and (b) suggest very similar trends before the policy implementation, but for the post-policy period the two outcomes do not seem to show an increase. As with the

absolute measure, these graphical results are preliminary descriptive evidence that needs to be further explored in a regression analysis.

In Table 1, I provide basic descriptive statistics for the studied sample. I have split the sample by treated vs control groups, in the pre- and post-periods. For instance, Table 1 shows that, the log of total (new) investment before the policy in the treated group was 4.701 and after the reform was 5.389, this shows that the reform might be associated with an increase in the log of total (new) investments since the difference column is positive and statistically significant. Something similar happens when I analyze the ratio of total investment over total capital. However, the log of total (new) investment and the ratio of total (new) investment over total capital after the policy in the control group is larger and statistically significant than before the policy in the same group. This suggests that the reform might be associated with an increase for the control group. Furthermore, the Table 1 shows that there is no statistically differences in the new contract outcomes before and after the policy neither in the treated nor in the control group.

Table 1: Descriptive statistics

	Treated group			Control group		
Variable	**Before**	**After**	**Difference**	**Before**	**After**	**Difference**
ln Total investment	4.701	5.389	0.687***	3.560	4.018	0.457***
	(0.102)	(0.1532)	(0.184)	(0.091)	(0.139)	(0.164)
Ln Total employment	4.197	4.198	0.001	4.332	4.324	-0.008
	(0.049)	(0.073)	(0.088)	(0.042)	(0.064)	(0.076)
Total investment / Total capital	0.394	0.430	0.035**	0.296	0.336	0.040***
	(0.009)	(0.013)	(0.016)	(0.008)	(0.012)	(0.014)
New hires / Total employment	0.361	0.344	-0.016	0.407	0.395	-0.012
	(0.006)	(0.009)	(0.012)	(0.006)	(0.009)	(0.011)
Observations	2,376	1,080	3,456	2,640	1,200	3,840

Notes: The Table presents the mean, standard errors in parentheses and the number of observation, respectively, of the main variables before and after the SAS reform used in our analysis. K is the probability of starting a new formal firm with less than 400 USD.
$^{***}p < .01$.

4 Identification and Empirical Strategy

I use different empirical models to estimate the causal effect of the Organic Law for Productive Development, Attraction of Investments, Generation of Employment and Stability and Fiscal Balance on investments and employment. I use four outcomes variables which are the i) the log of total (new) investment, ii) the log of total employment (employment stock), iii) the ratio between total (new) investment over total capital and iv) the ratio between total new hires over total employment; in each economic sector s, province p, year t, quarterly q, $y_{s,p,t,q}$.

I start the analysis using an event study design (ESD) featuring 11 pre and 5 post periods as it is an useful tool that allows to get evidence supporting the parallel trends assumption by the inclusion of pre-policy variation. In addition, it also allows to assess whether the effect is short run or whether it persists over time. As suggested by the literature on event studies designs and in applied microeconomics, I set the reference category at one period (quarter) before the policy implementation (Freyaldenhoven et al., 2019; Fuest et al., 2018; Schmidheiny and Siegloch, 2020). Formally, the equation reads:

$$(1)\quad y_{s,p,t,q} = \alpha + \beta_1 Treat_s + \sum_{j=-11}^{5} \beta_2^j Post_{q,t=j} + \sum_{j=-11}^{5} \beta_3^j Treat_s * Post_{q,t=j} + \gamma risk_q + \theta_q + \tau_t + \delta_s + \rho_p + \epsilon_{s,p,t,q}$$

where $Treat_s$ is a dummy taking the value of one for economic activities that are considered as prioritized sectors according to the LORTI, and zero otherwise, $Post_{q,t}$ is a dummy variable that takes the value of one after the implementation of the policy and zero otherwise. The coefficients of interest are the set of β_3's which capture the

effect of leads and lags of the Organic Law for Productive Development, Attraction of Investments, Generation of Employment and Stability and Fiscal Balance on investments and employment outcomes. I also include the variable $risk_q$ which is the country risk at the end of each quarter q, and a set of fixed effects: (i) quarter θ_q and year τ_t fixed effects to account for potential common time shocks across units, (ii) economic sector δ_s, and, province fixed effects ρ_p accounting for time invariant heterogeneity in each investment and labor market. I cluster standard errors at the economic sector level, since this is the level at which the effect takes place (Wooldridge, 2003; Cameron et al., 2008). As I only have nineteen clusters, I use wild bootstrapped clustered standard errors applying 999 replications.[11]

The specification in equation (1) is a variant of the traditional differences-in-differences (DiD) model with fixed effects. A necessary condition for the validity of the DiD strategy is that pre-policy implementation time trends for the treatment group and the control group are parallel. In this sense, the identification of causal effects in such models requires common trends pre-treatment: that is, no statistically significant investments and employment outcomes responses preceding the policy reform (Fuest et al., 2018). While specification (1) is used to establish flat pre-trends, I rely on a DiD approach in which I compare investments and employment created in the prioritized sectors vs. non-prioritized sectors, before and after the implementation of the law. More formally, I estimate the average effect of a change in the Organic Law for Productive Development, Attraction of Investments, Generation of Employment and Stability and Fiscal Balance on investments and employment outcomes relative to the pre-treatment period, in a equation of the form:

$$y_{s,p,t,q} = \beta_0 + \beta_1 Treat_s + \beta_2 Post_{q,t} + \beta_3 Treat_s * Post_{q,t} + +\gamma risk_q + \theta_q + \tau_t + \delta_s + \rho_p + \epsilon_{s,p,t,q} \tag{2}$$

where β_3 is the parameter of interest, which represents the impact of the policy on the new investments and employment outcomes under prioritized sectors relative to non-prioritized sectors. Again, I cluster standard errors at the economic sector level, since this is the level at which the effect takes place and I use wild bootstrapped clustered standard errors applying 999 replications.

The DiD estimator intends to provide an unbiased estimate of the treatment in a situation in which in the absence of the treatment, the outcome in the both treatment and control groups would have follow the same trend. This is more critical for the pre-treatment period, that is, one needs to add evidence supporting the parallel trend assumption before the implementation of the policy. In this sense, the empirical strategy used in this paper is supported by several pillars. First, I take advantage of the exogeneity of the Law implemented, which was not previously announced, in addition, economic agents were not expecting a tax incentive as this. It is also very unlikely to switch between treatment and control groups, for instance, it is not economically justifiable for a firm to change from one economic activity to other benefited by the policy. Second, even though treated economic activities and non-treated might differ in terms of observable characteristics, I do not expect these differences to change as a result of the the Law implementation, in any case, I believe that by including economic activities fixed effects I am properly controlling for any existing difference. Finally, to support the econometric strategy, I perform several robustness checks. For instance, (i) the event study design does not seem to show suspicious of differential pre-trends, and (ii) the results still hold after placebo tests and other additional checks.

5 Results

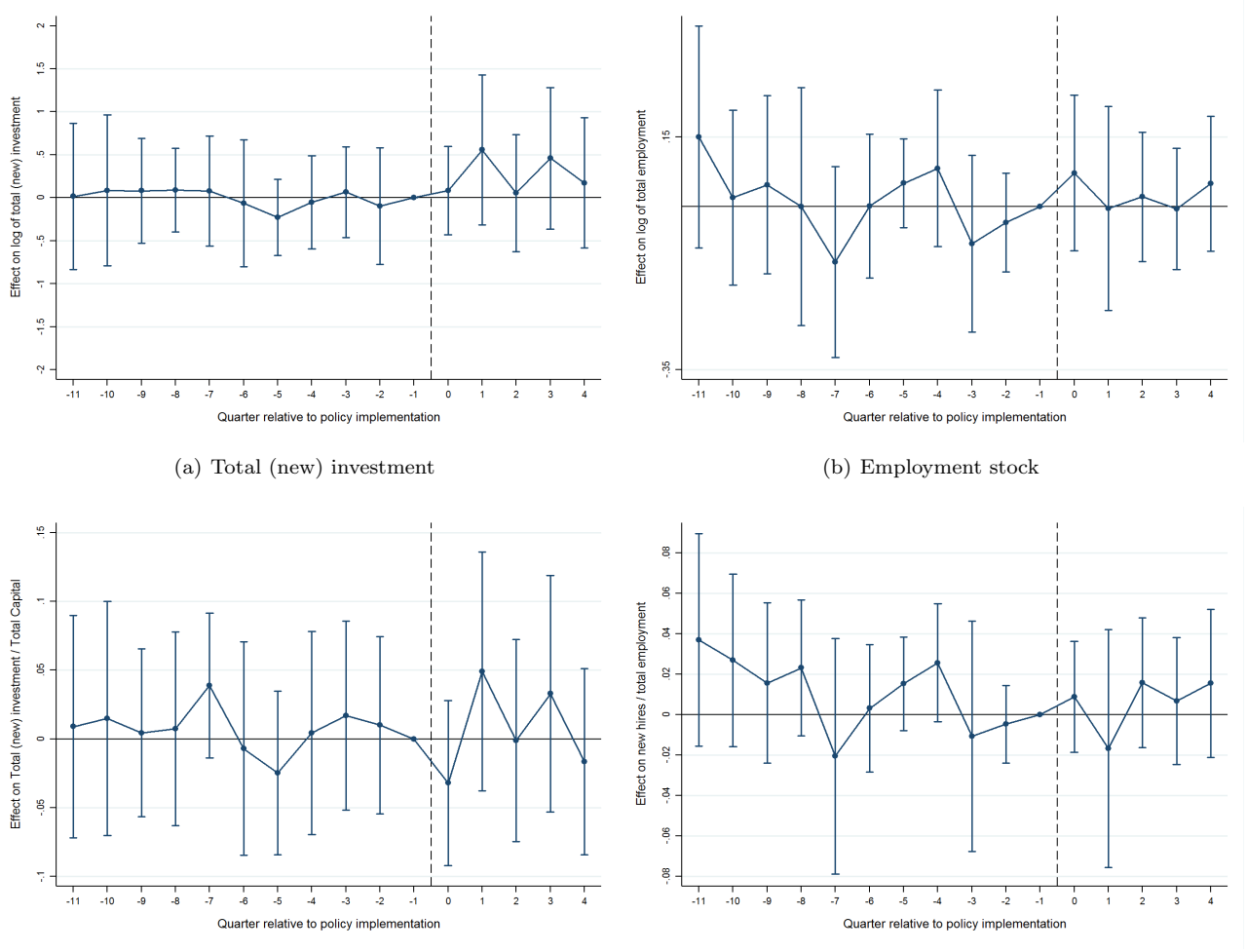
5.1 Baseline results

I start the analysis of the investment and employment effects of the Organic Law for Productive Development, Attraction of Investments, Generation of Employment and Stability and Fiscal Balance by plotting the event study estimates from equation (1) in Figure 3 for the four outcomes of interest. The results, depicted in Figure 3, show

[11] For computation, I use the boottest Stata command developed by Roodman et al. (2019).

that the set of pre-reform coefficients (β_3) are statistically indistinguishable from zero for all the four outcomes, suggesting a flat pre-trend. However, we do not find a positive and statistically significant effect of the policy reform on investment and employment.[12] These results support the parallel trend assumption and add validity to the identification strategy.

Figure 3: Event study of baseline estimates



(a) Total (new) investment

(b) Employment stock

(c) Total (new) investment / Total capital

(d) New hires / Total employment

Notes: Figure plots the set of coefficients corresponding to the DiD interaction terms from the event study equation (1). The reference period is the 2nd Quarterly 2018 and time periods are defined on a quarter basis. Confidence intervals at the 95 percent level.
Source: Superintendencia de Compañías, Valores y Seguros and Instituto Ecuatoriano de Seguridad Social.

I then estimate the average effect of the Organic Law for Productive Development, Attraction of Investments, Generation of Employment and Stability and Fiscal Balance over the period of analysis. In Table 2, I present the estimation results of equation (2). From results in column (1), it seems that the policy (law) does not has an effect on the log of total (new) investment for prioritized sectors compared to non-prioritized sectors over the last quarter of 2018 and the 2019. In column (2), I present the results of the effect of the policy on the log of total employment (employment stock), I find that the policy does not have an effect on this outcome for prioritized sectors compared to non-prioritized sectors over the last quarter of 2018 and the 2019. Finally, in columns (3) and (4) the results of the effect of the policy on the relative measures of investment and employment, respectively, is presented. Again, the results suggest that the Organic Law for Productive Development, Attraction of Investments, Generation of Employment and Stability and Fiscal Balance does not have an effect on the attraction of new investments nor on the creation of new employment in prioritized sectors compared to non-prioritized sectors before vs. after the

[12]The table results of the ESD estimates are available upon request.

new law.[13]

Table 2: Difference-in-Difference estimates: Baseline Investment and employment effects

Variable	**log total (new) investment**	**log employment stock**	**total (new) investment / total capital**	**New hires / total employment**
	(1)	**(2)**	**(3)**	**(4)**
Treat	6.562***	4.429***	0.499**	0.099
	(0.001)	(0.000)	(0.019)	(0.171)
Post	0.112	0.036	0.011	0.002
	(0.628)	(0.361)	(0.634)	(0.836)
Treat*Post	**0.231**	**0.010**	**-0.005**	**-0.005**
	(0.347)	(0.900)	(0.852)	(0.734)
risk	-0.000	-0.000	-0.000*	-0.000
	(0.485)	(0.888)	(0.061)	(0.439)
Province FE	✓	✓	✓	✓
ISIC FE	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Trend	✓	✓	✓	✓
No. Obs.	7,296	7,296	7,296	7,296
No. Clusters	19	19	19	19
R^2	0.239	0.504	0.247	0.358

Notes: OLS estimates of equation (2). P-values referring to standard errors clustered at the economic activity level in parentheses, calculated using wild cluster bootstrap with 999 replications.
*$p < .10$, **$p < .05$, ***$p < .01$.

Overall, the results suggest that the policy does not seem to be effective on attracting investment and creation of employment. Zee et al. (2002) mention that there are several potentially serious adverse consequences from the widespread use of tax incentives. First and foremost, they erode the tax base, either because many investments (especially highly profitable ones) would have taken place even without them, or because they are given to investments not eligible to receive them through abuse of provisions in the relevant laws and regulations by either officials or investors, or both. Second, tax incentives distort resource allocation, as some activities are encouraged over others not because they are necessarily more economically productive, but because they have been given a tax advantage.

The results are in concordance with others in the literature. For example, Klemm and Van Parys (2012) and Van Parys and James (2010) found that none of the tax incentives in developing countries are effective in boosting gross private fixed capital formation (private investment). This might be for the reasons proposed by Edgerton (2010) who argue that tax incentives have the smallest impact on investment exactly when they are most likely to be put in place — during downturns in economic activity when cash flows are low. Also, fiscal incentives might be less effective in countries with low endowments of public goods (Van Parys and James, 2010), and may not be enough to attract investments because of political and macroeconomic instability, lack rule of law, corruption, and administrative barriers (Zee et al., 2002; Morisset and Pirnia, 2000). Finally, the effect of tax incentives on job creation has been less explored and the results are mixed. This evidence is close to the results obtained by Thom (2019) who found that a combination of corporate tax incentives and other services had no statistically significant employment impact.

5.2 Heterogeneous effects

In this section, I explore whether there are differences across geographical areas of the country. In Table 3, I show the results of the estimates from equation (2) split by regions. Overall, the results suggest that the Organic Law for Productive Development, Attraction of Investments, Generation of Employment and Stability and Fiscal Balance

[13]I also estimate equation (2) by using the oil prices in each quarter q of year t instead of country risk. The results are very similar and are available upon request.

does not have an effect on the attraction of new investments and creation of new employment for prioritized sectors compared to non-prioritized sectors before vs. after the implementation of the law.

Table 3: Heterogeneity impact on Investment and employment by region

Variable	**log total (new) investment**	**log employment stock**	**total (new) investment / total capital**	**New hires / total employment**
	(1)	**(2)**	**(3)**	**(4)**
		Panel A: Costa region		
Treat	11.202***	5.461***	0.832***	-0.063
	(0.000)	(0.000)	(0.001)	(0.260)
Post	-0.107	-0.026	0.003	0.001
	(0.744)	(0.776)	(0.936)	(0.942)
Treat*Post	**0.473**	**0.104**	**-0.014**	**0.002**
	(0.107)	(0.537)	(0.660)	(0.946)
risk	-0.001	0.000	-0.000*	-0.000
	(0.117)	(0.838)	(0.091)	(0.843)
No. Obs.	2,128	2,128	2,128	2,128
R^2	0.318	0.506	0.280	0.302
		Panel B: Sierra region		
Treat	6.332***	4.603***	0.501**	0.032
	(0.000)	(0.000)	(0.018)	(0.315)
Post	0.319	0.068	0.015	0.018
	(0.178)	(0.148)	(0.413)	(0.157)
Treat*Post	**0.039**	**-0.040**	**-0.011**	**-0.022**
	(0.854)	(0.553)	(0.676)	(0.321)
risk	0.000	0.000	-0.000	-0.000
	(0.631)	(0.393)	(0.485)	(0.937)
No. Obs.	3,344	3,344	3,344	3,344
R^2	0.262	0.558	0.274	0.457
		Panel C: Amazonia region		
Treat	1.573	2.905***	0.109	0.410*
	(0.248)	(0.000)	(0.257)	(0.052)
Post	-0.013	0.049	0.013	-0.028
	(0.983)	(0.547)	(0.842)	(0.123)
Treat*Post	**0.296**	**-0.009**	**0.016**	**0.020**
	(0.528)	(0.919)	(0.748)	(0.448)
risk	0.000	-0.000	-0.000	-0.000
	(0.954)	(0.104)	(0.955)	(0.147)
No. Obs.	1,824	1,824	1,824	1,824
R^2	0.370	0.762	0.407	0.468
Province FE	✓	✓	✓	✓
ISIC FE	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Trend	✓	✓	✓	✓
No. Clusters	19	19	19	19

Notes: OLS estimates of equation (2). P-values referring to standard errors clustered at the economic activity level in parentheses, calculated using wild cluster bootstrap with 999 replications.
*$p < .10$, **$p < .05$, ***$p < .01$.

Furthermore, in Ecuador, according to the National Employment Survey (ENEMDU in Spanish) for September 2018, the city of Guayaquil, located in the Guayas province, accounted for approximately the 24% of the urban workers; and the capital Quito (located in the province of Pichincha) registered the 17.38% of urban workers (INEC, 2018). Moreover, Guayas and Pichincha are the provinces that receive the most private investment since 2014 with an annual average of 38% of the total private investment in the country (SCVS, 2021; Camino-Mogro et al., 2018). In addition, the economic sector that has received the most private investment from 2016 to 2019 is the Agriculture, Livestock and Fishing sector, followed by the manufacturing sector. On average, the Agriculture,

Livestock and Fishing sector represents 23% of total investment in all economic sectors. It is followed by the manufacturing sector sector with an average of 20%(SCVS, 2021; Camino-Mogro et al., 2018).

Table 4: Robustness check - excluding relevant provinces and economic sectors

Variable	**log total (new) investment**	**log employment stock**	**total (new) investment / total capital**	**New hires / total employment**
	(1)	**(2)**	**(3)**	**(4)**
		Panel A: Excluding Guayas		
Treat	6.138***	4.386***	0.475**	0.138
	(0.003)	(0.000)	(0.033)	(0.124)
Post	0.116	0.037	0.016	0.002
	(0.614)	(0.367)	(0.500)	(0.863)
Treat*Post	**0.251**	**0.006**	**0.001**	**-0.005**
	(0.329)	(0.942)	(0.958)	(0.748)
risk	-0.000	-0.000	-0.000*	-0.000
	(0.417)	(0.876)	(0.072)	(0.447)
R^2	0.264	0.543	0.275	0.375
		Panel B: Excluding Pichincha		
Treat	6.255***	4.368***	0.477**	0.131
	(0.001)	(0.000)	(0.027)	(0.127)
Post	0.129	0.034	0.013	0.001
	(0.601)	(0.417)	(0.566)	(0.906)
Treat*Post	**0.241**	**0.011**	**-0.003**	**-0.004**
	(0.345)	(0.885)	(0.917)	(0.778)
risk	-0.000	-0.000	-0.000**	-0.000
	(0.362)	(0.905)	(0.039)	(0.480)
R^2	0.264	0.551	0.271	0.372
		Panel C: Excluding Agriculture, Livestock and Fishing sector		
Treat	2.783*	1.764***	0.203	-0.015
	(0.085)	(0.001)	(0.159)	(0.262)
Post	0.118	0.033	0.011	0.001
	(0.642)	(0.394)	(0.657)	(0.936)
Treat*Post	**0.280**	**0.020**	**-0.000**	**-0.002**
	(0.255)	(0.773)	(0.994)	(0.893)
risk	-0.000	-0.000	-0.000*	-0.000
	(0.599)	(0.930)	(0.100)	(0.490)
R^2	0.247	0.518	0.257	0.378
		Panel D: Excluding Manufacturing sector		
Treat	6.592***	4.425***	0.502***	0.097***
	(0.000)	(0.000)	(0.010)	(0.160)
Post	0.202	0.036	0.016	0.001
	(0.371)	(0.393)	(0.491)	(0.883)
Treat*Post	**0.134**	**0.023**	**-0.015**	**0.001**
	(0.545)	(0.747)	(0.585)	(0.948)
risk	-0.000	-0.000	-0.000**	-0.000
	(0.435)	(0.783)	(0.045)	(0.386)
R^2	0.245	0.508	0.258	0.374
Province FE	✓	✓	✓	✓
ISIC FE	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Trend	✓	✓	✓	✓
No. Clusters	19	19	19	19

Notes: OLS estimates of equation (2). P-values referring to standard errors clustered at the economic activity level in parentheses, calculated using wild cluster bootstrap with 999 replications. The number of observations used when I exclude Guayas and Pichincha, respectively, is 6,992; and when I exclude Agriculture, Livestock and Fishing, and Manufacturing sectors, respectively, is 6,912.
*$p < .10$, **$p < .05$, ***$p < .01$.

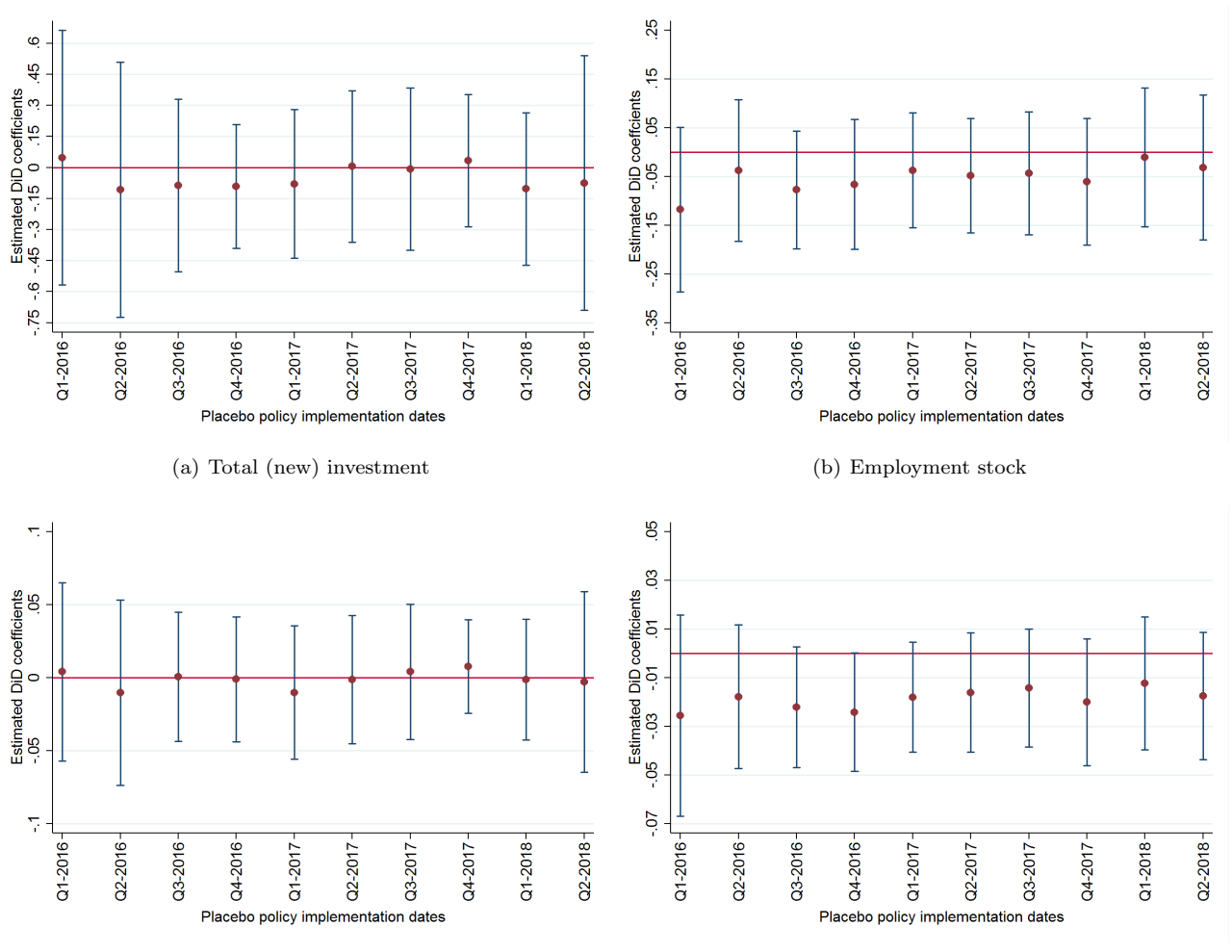
In this sense, one may still wonder whether there are provinces that have been affected but that the effect may have been washed out by other groups with null effects. To examine, whether most relevant provinces experienced an effect, I re-estimate equation (2) only for the Guayas and Pichincha provinces and for the Agriculture, Livestock and Fishing and manufacturing sectors, separately.

To support the empirical strategy, the results should still find not significant distinguished from zero. The results of this exercise is presented in Table 4. I find that after excluding investments and employment spells occurred in the Guayas and Pichincha provinces, and in the Agriculture, Livestock and Fishing and manufacturing sectors, separately, the effect continuous to be not significant at standard levels. Thus, even if I do not considered the biggest provinces and economic sectors, I still find that the Organic Law for Productive Development, Attraction of Investments, Generation of Employment and Stability and Fiscal Balance does not has an effect on the attraction of new investments and creation of new employment for prioritized sectors compared to non-prioritized sectors over the last quarter of 2018 and the 2019.

5.3 Robustness check

5.3.1 Placebo event dates

Figure 4: Robustness check - Placebo event dates



(a) Total (new) investment

(b) Employment stock

(c) Total (new) investment / Total capital

(d) New hires / Total employment

Notes: Each point estimate (and their 95 percent confidence intervals) represents a separate regression in which the event date is set according to the X axis.
Source: Superintendencia de Compañías, Valores y Seguros and Instituto Ecuatoriano de Seguridad Social.

I perform a placebo test in which I change the timing of the policy implementation assuming the event took

place one, to ten quarters before the actual policy implementation (Q3-2018). For this, I first drop all observations from the actual event date onwards, and estimate a series of regressions of the form of equation (2).

This placebo exercise is done to add support to the parallel trends assumption, and I should not find any significant effect in the DID coefficients. Figure 4 plots the corresponding DID coefficients along with their 95 percent confidence intervals. Overall, I do not find any significant points estimate, in none of the four outcomes. In this sense, this placebo exercise supports our identification strategy since I do not find serious suspicions of non-parallel trends.

5.3.2 Placebo treatment group

In addition, I conduct a second placebo test to indirectly assess the common time trend assumption. I estimate equation (2) using an alternative treatment group. I assign as placebo treatment units the wholesale and retail trade sector and drop from the analysis those observations corresponding to prioritized sectors (actual treated group). The control group remains the same; non-prioritized sectors. Because I expect that the increase in total (new) investment and employment due to the policy implementation affects mainly prioritized sectors, I should not find an effect on the wholesale and retail trade sector, otherwise our identification strategy would be challenged. Thus, we re-estimate equation (2) using the the wholesale and retail trade sector during the period of analysis as the treated group. Results of this exercise are shown in Table 5.

In general, I find that none of the investment and employment outcomes are statistically significant. Thus, I conclude that there is no evidence of differential effects of the Organic Law for Productive Development, Attraction of Investments, Generation of Employment and Stability and Fiscal Balance on wholesale and retail trade sector, which supports the empirical strategy.

Table 5: Robustness check - Placebo treatment group

Variable	**log total (new) investment**	**log employment stock**	**total (new) investment / total capital**	**New hires / total employment**
	(1)	**(2)**	**(3)**	**(4)**
Treat	7.643***	5.500***	0.614***	0.286*
	(0.006)	(0.000)	(0.005)	(0.060)
Post	0.145	0.034	0.022	0.004
	(0.497)	(0.340)	(0.331)	(0.738)
Treat*Post	**1.002**	**0.064**	**0.100**	**-0.002**
	(0.266)	(0.430)	(0.290)	(0.836)
risk	-0.000	-0.000**	-0.000	-0.000
	(0.910)	(0.041)	(0.520)	(0.219)
Province FE	✓	✓	✓	✓
ISIC FE	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Trend	✓	✓	✓	✓
No. Obs.	3,840	3,840	3,840	3,840
No. Clusters	10	10	10	10
R^2	0.264	0.505	0.252	0.350

Notes: OLS estimates of equation (2). P-values referring to standard errors clustered at the economic activity level in parentheses, calculated using wild cluster bootstrap with 999 replications.
*$p < .10$, **$p < .05$, ***$p < .01$.

Finally, given flat pre-trends, this research design would still be invalid if local shocks systematically affected investments and employment measures. In this sense, I re-estimate equation (1) using industry-GDP, oil prices in each quarter q of year t as outcome variables, similar to Fuest et al. (2018). Significant pre-treatment trends for these outcomes would hint at local shocks and cast doubt on the identification assumption. I find no local shocks to the business cycle prior to the policy implementation. Moreover, I re-estimate equation (2) by including industry-GDP and oil prices, as control variables, by each quarter q of year t, similar to Garsous et al. (2017).

The results show that this variable is not statistically significant and the DiD coefficient is similar to the baseline results.[14]

Altogether, I find evidence supporting the robustness of my findings. For instance, the results, do not show suspicious of pre-trends (seen in the event study), when excluding the most populated provinces and industries, separately. They are also robust to two placebo test, in which I change the timing of the policy implementation and the treatment group, separately.

6 Final Remarks

This paper assesses the impact of of tax incentives of an Ecuadorian reform in 2018 on four different outcomes: i) the log of total (new) investment, ii) the log of total employment (employment stock), iii) the ratio between total (new) investment over total capital and iv) the ratio between total new hires over total employment. By using two novel, under-explored administrative data sets, I exploit the time and industry variation in the implementation of this policy and provide evidence supporting that fiscal incentives, by themselves, do not have a significant effect on private investment and formal employment.

I contribute to filling this empirical gap in the literature by examining the extend to which a program of tax incentives on prioritized sectors have not been effective in increasing new investments and formal employment in Ecuador. I conjecture that tax incentives are unlikely to affect significantly the new investment and formal employment because there are other important factors that stop the decision to invest in Ecuador such as: corruption, institution quality, country risk, inequalities, country size, easy to do business, etc. Zee et al. (2002) and Klemm (2010) mention a non-exhaustive list of policy implications for effective policy of tax incentives. For example, the main issue is to identify the sectors benefiting form the reduced tax rate, so this ensure transparency and avoid discretion on the part of authorities. Furthermore, to stimulate investment is necessary that the country us international norms, and the regulatory environment functions well. Moreover, the implementation of fiscal incentives programs aimed at increasing employment creation must be accompanied by other policies targeting both productivity and the quality of employment Garsous et al. (2017), which was not the case in Ecuador.

Finally, an important limitation of this paper is that it focus on formal employment and not on informal labor force. Also, the paper does not analyze labor productivity, total factor productivity or on entry and exit firms. In this sense, these are issues for future research.

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[14]Results of these exercises are available upon request.

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7 Annexes

Table 6: Treated economic activities

ISIC description	**Treated**
A - Agriculture, Livestock and Fishing	Yes
B - Exploitation of mines and quarries	Yes
C - Manufacturing Industry	Yes
D - Supply of electricity, gas, steam and air conditioning	Yes
E - Water distribution: Sewerage, waste management and sanitation activities	Yes
F - Construction	Yes
G - Wholesale and retail trade	No
H - Transportation and storage	Yes
I - Accommodation and food service	Yes
J - Information and commun.	Yes
K - Financial and Insurance activities	Yes
L - Real estate activities	No
M - Professional, scientific and technical activities	No
N - Administrative and support service activities	No
P - Education	No
Q- Human health care and social assistance activities	No
R - Arts, entertainment and recreation	No
S - Other service activities	No
T - Activities of households as employers	No

Elaboration: The Author.

Source: National Institute of Statistics of Ecuador (INEC, 2020).

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