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RESEARCH ARTICLE

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Tax incentives, private investment and employment: Evidence from an Ecuadorian reform

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Abstract

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 or the creation of new employment for prioritised sectors compared to non-prioritised sectors. This matters from a policy perspective in a country with few private investments and a high share of people out of formal employment.

KEYWORDS
developing country, Ecuador, employment, investment, tax incentives

JEL CLASSIFICATION
H25, H32, H54

1 | INTRODUCTION

It is well known that fiscal incentives are a very common policy for attracting business investment and creating 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,

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domestic and foreign investment could increase, bolstering economic growth, and later may generate employment (Arnold et al., 2011; Jacob, 2022).

In this sense, the arrival of new investments could help the government reduce unemployment problems and budget deficits, among other concerns.[1] However, the use of tax incentives in developing countries is controversial as it usually comes with significant and sometimes overlooked costs (Klemm & Van Parys, 2012). Furthermore, there are several potentially serious adverse consequences from the widespread use of tax incentives (see, for an extensive justification, Klemm, 2010; Zee et al., 2002). On the other hand, it may happen that such a policy has no effect on attracting investment or job creation in developing countries because of major structural problems such as corruption, ease of doing business, macroeconomic and political instability, administrative barriers, transparency, a lack of rule of law, etcetera (see, for example, Dollar et al., 2005; Morisset & Pirnia, 2000; Rahman, 2014; Zee et al., 2002).

Nevertheless, in many developing countries, it has become very common to implement tax incentives focused on attracting investment and job creation. One reason for this behaviour might be the lack of empirical evidence that assesses the impact of tax incentives on business investment and formal employment. In addition, these types 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. To that end, I use two administrative data sets, one provided by the regulatory institution of firms on new investments, and the other by the social security institution on new contracts and the 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 on August 21st, 2018, as it was largely unexpected by firms and economic agents.[2] 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 rests on several pillars. First, it is very unlikely that firms would change from a non-prioritised sector to a prioritised sector since they must undergo drastic changes in capital and labour inputs in the short term.[3] Second, similar to Fuest et al. (2018), in this type of setting, general equilibrium effects on interest rates or consumer prices,[4] which may complicate measuring the incidence of tax incentives on investment and employment, are likely to be of minor importance. Finally, something remarkable about this incentive policy is that it is meant to attract additional national and foreign investments, which may or may not already be established in the country. Therefore, an increase in capital is also included in this policy, but not the transfer of assets from one already established firm to another, for the policy aims to attract purely new investments.

As a first insight into the results, I find no evidence that supports a significant effect of the law on either new employment or investments. In addition, I do not find any suspicious evidence 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 analysing the effect of a reduction in taxes or fiscal incentives on business investment and job creation in the literature has been that the

[1]Gechert and Heimberger (2022) argue that there is evidence for publication selectivity in favour of reporting growth-enhancing effects of corporate tax cuts. However, when they correct for this bias, they cannot reject the hypothesis of a zero effect of corporate taxes on growth.

[2]Here, the reader might think that this seems highly unlikely, as the law must have been in the making for a while and lobby efforts would likely have been intense to get specific sectors included. In Ecuador, to approve a law of this type, an absolute majority is required, which is half plus one of the votes of the National Assembly. This law was approved with 73 votes in favour, 28 against and 26 abstentions in the plenary session of the National Assembly, barely three additional votes to the absolute majority. In this sense, despite the previous discussion and socialisation of the law, it was not assured in the National Assembly since the president of the republic did not enjoy an absolute majority in the legislative function.

[3]However, it is probable that, in the presence of multi-sector firms and the ability to undertake profit-shifting, firms might switch from one sector to another to be a beneficiary of a policy incentive. There is also evidence that the adjustment of divestment for this type of company occurs in the long term (see, for example, Artuc et al., 2022). This is because of the high cost of moving fixed capital. In this sense, based on the Ecuadorian business characteristics, I conjecture that this does not occur, particularly because of the high costs of firing and bank loans.

[4]Interest rates in Ecuador are rigid, and the Ecuadorian Central Bank (BCE) is the institution that fixes the percentage.

studies use cross-country research designs, which make it difficult to fulfil the assumption of complete exogeneity of policy implementation, leading to non-causal evidence or at least severe suspicions of endogeneity. In this sense, this research overcomes this issue by exploiting exogeneity from a policy design, which allows me to construct a valid control group. Second, the research into 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 because of the unavailability of administrative data. This research analyses the effect of this type of policy in Ecuador, a developing economy, making use of administrative records to avoid common problems from survey data.

The literature on this topic is controversial. Authors like Zee et al. (2002) discuss the objectives, cost effectiveness and transparency of implementing tax incentives, which in some cases might attract business investments (see, for example, Chirinko & Wilson, 2010a, 2010b, 2017; Shah, 1995). In other cases, it is effective in attracting FDI (see, for example, De Mooij & Ederveen, 2003; Feld & Heckemeyer, 2011; Klemm & Van Parys, 2012; Mooij & Ederveen, 2008; Tung & Cho, 2000). Nevertheless, in some instances, this policy is ineffective in attracting new investments (see, for example, Klemm & Van Parys, 2012; Van Parys & James, 2010). In terms of the effect of this kind of policy on employment, the empirical evidence is also scarce.

Recent empirical evidence at the firm level finds a positive causal relationship between tax incentives and investment. For example, Liu and Mao (2019) analysed a major reform for capital taxation which was approved in China in 2004 and introduced permanent tax incentives for firms' investment in fixed assets. They found that the reform increased investment and productivity of the treated firms relative to the control firms by 38.4% and 8.9%, respectively. In the same line, Zhang et al. (2018), who analysed the same reform, found that the tax credit significantly increased fixed investment of eligible firms by 28% on average during the period 2004–2007 relative to 2001–2003. Furthermore, Chen et al. (2022) studied China's 2009 VAT reform, which lowered the tax cost of investment and reduced the price gap between new and used capital by using administrative tax data and a difference-in-differences design. The authors estimate a 36% investment increase. However, the authors suggest that policies that directly reduce the likelihood of firm inaction are more effective at stimulating investment. Guceri and Albinowski (2021) argue that under low uncertainty, tax incentives have strong positive effects on average investment. Nevertheless, the effects of tax incentives and investment might be heterogeneous. For example, Harju et al. (2022) studied the impact of corporate taxes on firm-level investments and business activity (in small firms) by exploiting a 6 percentage point reduction in the corporate tax rate in Finland from 2012 to 2014, but they did not find a significant average investment effect.

In addition, recent empirical evidence at the firm level supports a positive causal relationship between tax incentives and employment. 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 Auerbach (2018), Fuest et al. (2018) and Carbonnier et al. (2022), who analyse the effect on wages in the USA, Germany and France, respectively. Moreover, Curtis et al. (2021) study tax policies that lower the cost of capital impact investment and labour demand. The authors find that tax policies increased both investment and employment but did not stimulate wage or productivity growth. However, Tuzel and Zhang (2021) explore the states' adoption of a major federal tax incentive that accelerates the depreciation of equipment investments for eligible firms. They find that when states expand investment incentives, eligible firms immediately increase their equipment and skilled employees, but they reduce routine-task employees after a delay of up to two years. However, the authors suggest that there is an overall insignificant effect on the firms' total employment and shed light on the nuances of job creation through investment incentives. Criscuolo et al. (2019) exploit changes in area-specific eligibility criteria for a program to support jobs through investment subsidies and find that areas eligible for higher subsidies significantly increased jobs and reduced unemployment. Nevertheless, this effect exists solely for small firms: large companies accept subsidies without increasing activity. Obtaining a result similar to this finding, Garrett et al. (2020) studied the bonus depreciation policy established in the USA since 2002 and exploited cross-industry variations in policy generosity that interacted with

county-level industry location data. They found that places that experience larger decreases in investment costs see an increase in employment and earnings. In contrast, the policy does not have positive effects on earnings per worker. Overall, their findings suggest that federal corporate tax policy has large effects on local labour markets.

Despite this evidence, the empirical literature is inconclusive for various reasons, such as the data used, methodology, cross-country analysis, single country analysis, different outcomes analysed and period of study. Finally, as I show, the empirical literature has focused on developed countries, leaving aside the study of developing economies, except for China.

The rest of the paper is organised as follows. Section 2 describes the institutional setting of business taxation in Ecuador and discusses in more detail the 2018 tax incentive 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 developing, dollarised, commodity-dependent (mostly oil), small and open Latin American economy. Since the 1990s, oil and tax revenues have been the main source of financing (above the line) for public spending (Rańırez-Álvarez & Carrillo-Maldonado, 2020). Although few tax reforms were approved in Ecuador in the first years of dollarisation (since 2000), it was not until the enactment of the Tax Equity Reform Law of December 2007 when tax management was reorganised. This helped increase the tax contribution, which reached an average of 62% of the total income in the period 2007–2015 (Carpio & Carrasco, 2012; Rańırez-Álvarez & Carrillo-Maldonado, 2020).[5] However, since 2016, tax revenues have generated less and less with respect to total revenues, on average 39% between 2016 and 2019.[6]

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

The CIT rate structure to which Ecuadorian firms are subject is detailed in the "Ley Org'anica de Ŕegimen Tributario Interno (LORTI)" (Organic Law of the Internal Tax Regime) 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 non-deductible expenses covered by the law and its regulations (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 towards their tax obligation during the fiscal year (*anticipo del impuesto a la renta*) (Beverinotti et al., 2021; Deza et al., 2020). This amount is calculated as a percentage of total assets (0.2%), total costs and expenses deductible for income tax purposes (0.2%), total assets (0.4%) and 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

[5]For detailed tax reforms since dollarisation, see Cueva et al. (2018).
[6]Data obtained from the Central Bank of Ecuador (BCE).
[7]Data obtained from the Servicio de Rentas Internas (SRI).

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).[8]

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 rationalised, 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 substantial amount of 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

The project of the Organic Law for Productive Development, Attraction of Investments, Generation of Employment and Stability and Fiscal Balance was sent as an urgent economic law by President Lenin Moreno to the National Assembly on May 24th, 2018.[9] The project of the law was discussed in the first debate on June 12th, 2018 and approved in the second debate on June 21st, 2018 by the National Assembly. The law was approved with 73 votes in favour, 28 against and 26 abstentions from the assembly members, with some marginal reforms that do not alter the fundamental content of said law. Subsequently, it was partially objected by the Constitutional President of the Republic on July 18th, 2018. However, this law was approved by the Assembly without the necessary analysis and debate on the part of society. The disproportion recorded in the media was impressive, between the interviews and publications of analysts traditionally favourable to neoliberal thought compared to the silencing of critical analyses of these positions.[10]

On August 21st, 2018, the Organic Law for Productive Development, Attraction of Investments, Generation of Employment and Stability and Fiscal Balance went into effect in Ecuador. This law proposes a long-term economic stability plan and 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 and 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 reforms of various legal bodies. Finally, in other sections, several previous laws that have to do with macroeconomic stability are reformed.[11] Article 26 of the second chapter of the law mentions the exemption from CIT for new productive investments in prioritised sectors. It also mentions that the new productive investments.

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 eight years, and that for new firms, along with those already existing, this exemption will apply only to firms that generate net employment. 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 fulfil 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 eight-year exoneration when the new investments take place in the Quito and Guayaquil urban areas. Also, when the investment is made in border cantons, within the prioritised industrial, agro-industrial and

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

[9]In Ecuador, the projects of urgent economic laws must be debated by the National Assembly for a maximum period of 30 days. Otherwise, the law takes effect by ministry of law.

[10]The announcement and delivery of the draft of this law were made by the president of the republic on May 24th, 2018 in his first report to the nation. To see and listen to Lenin Moreno's first report to the nation of Lenin Moreno, go to https://www.youtube.com/watch?v=PZkISIk_o0Q.

[11]To read the law, go to https://www.gob.ec/sites/default/files/regulations/2018-09/Documento_Ley-Orgánica-Fomento-Productivo-Atracción-Inversiones.pdf.

agro-associative sectors, the CIT exemption will be 15 years. In both 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 law's taking effect, in which case the exemption will apply proportionally to the value of the new productive investments.[12]

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, in Spanish), based on the conditions of public finances and balance of payments, prior favourable 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, one provided 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 2016 to 2019, corresponding to two years before the policy implementation and one year after it.[13] Auerbach et al. (2010) argue that there is robust evidence that well-designed tax cuts can boost consumption and investment in the short run (see, for example, Harju et al., 2022; Zhang et al., 2018). The unit of observation is the firm for which there is information on the province-municipality of location, economic activity (at the four-digit ISIC level), the type of investment received, national or foreign and the total amount of capital received in US dollars. For the purpose of this study, I divided the analysis into two main effects: 1) micro-level and 2) macro-level. For the micro-level analysis, I use firm-level data in order to provide enough information on firms' real responses to tax policy change. To do this, I use 41,287 observations of firms that invest in Ecuador. For the macro-level analysis, I collapse the data set at the province, economic activity and quarter level, which results 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 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.[14] 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 and the workplace at the province level. There is also information on the wage (in US dollars), the gender and the 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 labour force constitution. The time horizon spans from 2016 to 2019. As in the investment data, I also collapse the social security records at the province, economic activity and quarter level. With this, I merge the two data sets, obtaining a balanced panel of provinces-economic activity at the quarter-year level.[15]

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 to whether or not the activity is included in the prioritised sectors as established in the Organic

[12]To read the specifications, go to https://www.sri.gob.ec/ley-organica-fomento-productivo and http://www.inteligenciaproductiva.gob.ec/archivos/beneficios_ley_organica_para_el_fomento_productivo.pdf.

[13]I use data up to 2019 to avoid 2020 because of the COVID-19 crisis and all the economic consequences arisen from it, (see, for example, Camino-Mogro, 2022; Camino-Mogro & Armijos, 2022; Camino-Mogro et al., 2022).

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

[15]It is important to mention that I do not have information at the firm-level, so I cannot merge this data set for the micro-level analysis.

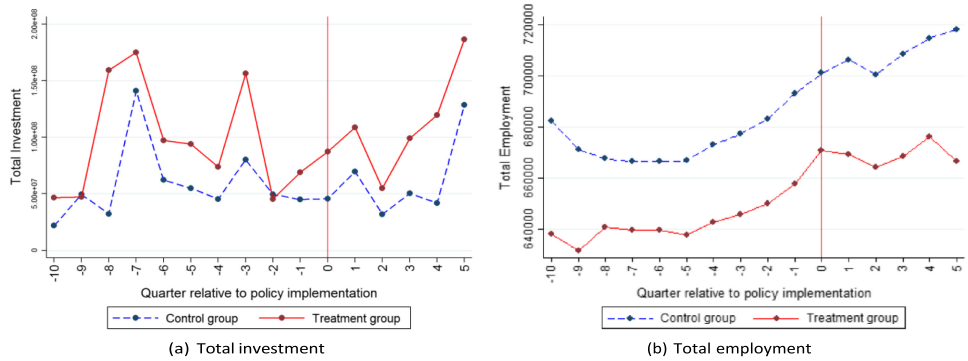


FIGURE 1 Total investment and employment in Ecuador. *Source*: Superintendencia de Compañías, Valores y Seguros and Instituto Ecuatoriano de Seguridad Social. *Elaboration*: the author.

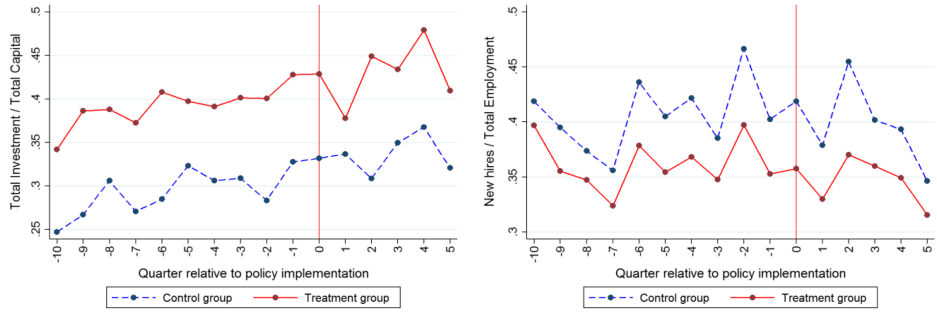
Law of the Internal Tax Regime (LORTI). Also, the law expands the list of prioritised sectors to 15, but allows some discretion of the Executive Power to include as beneficiaries of the incentives sectors of strategic import substitution and export promotion according to the recommendations issued by the Sectoral Council of Production (CEPAL and Oxfam, 2019). Because the classification of the prioritised sectors in the LORTI is ambiguous, and since there is no official classification at the ISIC level of the sectors that comprise it, in this paper it is grouped to one ISIC digit.[16] An extensive list of the treated economic activities is shown in Table A1. Furthermore, I follow the classification of the prioritised sectors made by CEPAL and Oxfam (2019).

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 look 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. The 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 the first descriptive evidence on trends before and after the new law.

As labour 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, 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 sample studied. I have split the sample into treated vs. control groups in the pre- and post-periods. For instance, Table 1 shows that the log of total (new) investment was 4.701 before the policy in the treated group and 5.389 after the reform. 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 analyse 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 are larger and more 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, Table 1 shows that there are no

[16]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.



(a) Total investment / Total capital (b) New hires / Total employment

FIGURE 2 Mean values of the ratio of total investment and ratio of new hires in Ecuador.
Source: Superintendencia de Compañías, Valores y Seguros and Instituto Ecuatoriano de Seguridad Social.
Elaboration: the author.

TABLE 1 Descriptive statistics.

	Treated group			Control group		
Variable	**Before**	**After**	**Difference**	**Before**	**After**	**Difference**
ln total investment	4.701 (0.102)	5.389 (0.1532)	0.687*** (0.184)	3.560 (0.091)	4.018 (0.139)	0.457*** (0.164)
Ln total employment	4.197 (0.049)	4.198 (0.073)	0.001 (0.088)	4.332 (0.042)	4.324 (0.064)	−0.008 (0.076)
Total investment/total capital	0.394 (0.009)	0.430 (0.013)	0.035** (0.016)	0.296 (0.008)	0.336 (0.012)	0.040*** (0.014)
New hires/total employment	0.361 (0.006)	0.344 (0.009)	−0.016 (0.012)	0.407 (0.006)	0.395 (0.009)	−0.012 (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 observations, respectively, of the main variables before and after the reform used in our analysis.
**$p < .05$.
***$p < .01$.

statistical differences in the new contract outcomes before or after the policy in either the treated or the control group.

4 | IDENTIFICATION AND EMPIRICAL STRATEGY

I start the empirical strategy by analysing the micro-level effects of the reform. To do this, 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 (total, national and FDI). I use six outcome variables: i) the log of total (new) investment, ii) the log of total (new) national investment, iii) the log of total (new) foreign direct investment, iv) the ratio of total (new) investment over total capital, v) the ratio between total (new) national investment over total capital and vi) the ratio of total (new) foreign direct investment over total capital; for each firm i in each economic sector s, province p, year t, quarterly q, yi,s,p,t,q.

In this line, I rely on a difference-in-differences (DiD) approach in which I compare the different types of investment created in the prioritised sectors vs. non-prioritised 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 investment outcomes relative to the pre-treatment period in an equation of the form:

$$y_{i,s,p,t,q} = \beta_0 + \beta_1 Treat_{i,s} + \beta_2 Post_{q,t} + \beta_3 Treat_{i,s} * Post_{q,t} + \theta_q + \tau_t + \delta_s + \rho_p + \epsilon_{i,s,p,t,q} \quad (1)$$

where $Treat_{i,s}$ is a dummy variable which takes the value of one for firms that operate in economic activities that are considered prioritised 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 coefficient of interest is β_3, which represents the impact of the policy on the new investment outcomes under prioritised sectors relative to non-prioritised sectors. I also include a set of fixed effects: (i) quarter θ_q and year τ_t fixed effects to account for potential common time shocks across units and (ii) economic sector δ_s and province fixed effects ρ_p to account for time-invariant heterogeneity in each investment. I cluster standard errors at the economic sector level since this is the level at which the effect takes place (Cameron et al., 2008; Wooldridge, 2003). As I have only 19 clusters, I use wild bootstrapped clustered standard errors, applying 999 replications.[17]

The DiD estimator intends to provide an unbiased estimate of the treatment in a situation in which, in the absence of the treatment, the outcomes in both treatment and control groups would have followed the same trend. This is more critical for the pre-treatment period; that is, one needs to add evidence that supports the parallel trend assumption before the implementation of the policy. In this sense, the empirical strategy used in this paper rests on 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 of this kind. Regarding this point, in the previous sections, I have mentioned anecdotal events that support this assumption. However, to support this hypothesis, I present an analysis of an event study design and placebo fake event of policy implementation. Second, 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 another to benefit from the policy. On this point, it is appropriate to mention that the policy was not announced long before its implementation, so changes and transfers of capital and labour from one sector to another are unlikely. Third, even though treated and non-treated economic activities might differ in terms of observable characteristics, I do not expect these differences to change as a result of the implementation of the law. In any case, I believe that by including economic activities and time 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 evidence of differential pre-trends, (ii) the results still hold after placebo tests and other additional checks and (iii) the results are very similar when I estimate macro-level effects.

A necessary condition for the validity of the DiD strategy is that pre-policy implementation trends for the prioritised sectors (treatment group) and non-prioritised sectors (control group) are parallel. For this reason, I evaluate possible differential pre-trends between the treatment and control groups, modifying the DiD specification of Equation (1) into an event study setting using 11 pre- and five post-policy implementation (quarter) periods. As suggested by the literature on event study designs, I set the reference category at one period (quarter) before the treatment (Freyaldenhoven et al., 2019; Fuest et al., 2018). Formally, I estimate an equation of the form:

$$y_{i,s,p,t,q} = \alpha + \beta_1 Treat_{i,s} + \sum_{j=-11}^{5} \beta_2^j Post_{q,t=j} + \sum_{j=-11}^{5} \beta_3^j Post_{q,t=j} * Treat_{i,s} + \theta_q + \tau_t + \delta_s + \rho_p + \epsilon_{i,s,p,t,q} \quad (2)$$

where the coefficients of interest are the set of β_3's that capture the effect of the leads and lags of the Organic Law for Productive Development, Attraction of Investments, Generation of Employment and Stability and Fiscal Balance

[17]Standard errors are clustered at the economic sector level (19 clusters) and computed using wild bootstrapped replications. For computation, I use the boottest Stata command (Roodman et al., 2019). MacKinnon (2019) argued that even though the disturbances for bootstrap samples generated by the wild cluster bootstrap are clustered in only one dimension, the wild cluster bootstrap can also be used in conjunction with two-way clustered standard errors. MacKinnon et al. (2021) show that the wild bootstrap often works well in this case and boottest makes computation easier.

on investment outcomes. Again, I cluster standard errors at the economic sector level since this is the level at which the effect takes place (Cameron et al., 2008; Wooldridge, 2003). As I have only 19 clusters, I use wild bootstrapped clustered standard errors, applying 999 replications.[18]

The specification in Equation (2) is a variant of the traditional DiD model with fixed effects. The identification of causal effects in such models requires common trends pre-treatment, that is, no statistically significant investment outcome responses preceding the policy reform (Fuest et al., 2018). While specification (2) is used to establish flat pre-trends, I rely on a DiD approach in which I compare investments and employment created in the prioritised sectors vs. non-prioritised sectors before and after the implementation of the law.

Finally, to show the robustness of the results at the micro-level (firm-level evidence), I present an analysis of the effects of the reform at the macro-level. For this analysis, 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 outcome variables: 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 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 investment and employment outcomes relative to the pre-treatment period in an equation of the form:

$$y_{s,p,t,q} = \beta_0 + \beta_1 Treat_s + \beta_2 Post_{q,t} + \beta_3 Treat_s * Post_{q,t} + \gamma risk_q + \theta_q + \tau_t + \delta_s + \rho_p + \epsilon_{s,p,t,q} \quad (3)$$

where I include the variable $risk_q$, which is the country risk at the end of each quarter q, in order to capture macroeconomic effects that might affect the decision to invest in Ecuador. Furthermore, here I include employment outcomes which might help in holistically understanding the effects of a reform of this type. Again, I cluster standard errors at the economic sector level since this is the level at which the effect takes place (Cameron et al., 2008; Wooldridge, 2003). As I have only 19 clusters, I use wild bootstrapped clustered standard errors, applying 999 replications.

Moreover, to validate the DiD strategy at the macro-level, I use an event study design (ESD) that features 11 pre- and 5 post-periods as it is a useful tool that makes it possible to obtain evidence that supports the parallel trends assumption by the inclusion of pre-policy variation. In addition, it also allows me to assess whether the effect is short-run or whether it persists over time. Formally, the equation reads:

$$y_{s,p,t,q} = \alpha + \beta_1 Treat_s + \sum_{j=-11}^{5} \beta_2^j Post_{q,t=j} + \sum_{j=-11}^{5} \beta_3^j Post_{q,t=j} * Treat_s + \gamma risk_q + \theta_q + \tau_t + \delta_s + \rho_p + \epsilon_{s,p,t,q} \quad (4)$$

Finally, the empirical strategy is supported by several exercises. First, I analyse the heterogeneous effects of the law on investment and employment. For this exercise, I estimate all the previously mentioned equations separated by Ecuadorian regions to assess heterogeneous effects on investment and employment outcomes. Second, in Ecuador, according to official data from the SCVS and the IESS, Guayas and Pichincha are the main provinces that receive new investments and create formal employment. Likewise, the agriculture and manufacturing sectors represent around 50% of new investments across all economic sectors. In this sense, one may wonder whether the results are driven by these two large provinces or economic activities. To show that this is not the case, I re-estimate all equations mentioned before excluding the provinces of Guayas and Pichincha and the agriculture and manufacturing sectors, separately. To support the empirical strategy, the results should still be similar to the main strategy. Third, I use an alternative treatment group (wholesale and retail trade sectors). I should not find an effect on the wholesale and retail trade sectors. Otherwise, the identification strategy would be challenged. Fourth, I perform a placebo test in which I change the timing of the policy implementation, assuming the event took place 1 to 10 quarters before the actual

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

policy implementation (Q3–2018). 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.

5 | RESULTS

5.1 | Baseline results

In order to provide information on firms' real responses to the application of the new law, I present the baseline results (micro-level effects) of the estimation of Equation (1) in Table 2. The results show the effect of the Organic Law for Productive Development, Attraction of Investments, Generation of Employment and Stability and Fiscal Balance over the period of analysis for the six outcomes of investment. From the results in column (1), it seems that the policy (law) does not have an effect on the log of total (new) investment for prioritised sectors compared to non-prioritised sectors over the last quarter of 2018 and 2019. In column (2), I present the results of the effect of the policy on the log of total (new) national investment. I find that the policy does not have an effect on this outcome for prioritised sectors compared to non-prioritised sectors over the last quarter of 2018 and 2019. In column (3), I present the results of the effect of the policy on the log of total (new) foreign direct investment; again, I find that the policy does not have an effect on this outcome for prioritised sectors compared to non-prioritised sectors over the last quarter of 2018 and 2019. Finally, in columns (4), (5) and (6), the results of the effect of the policy on the relative measures of investment are presented. Again, the results suggest that the reform does not have an effect on the attraction of new investments (of any kind) in prioritised sectors compared to non-prioritised sectors before vs. after the new law.

TABLE 2 Difference-in-differences estimates: Firm-level evidence.

Variable	Log total (new) investment (1)	Log total (new) national investment (2)	Log total (new) foreign direct investment (3)	Total (new) investment/ total capital (4)	Total (new) national investment/ total capital (5)	Total (new) foreign direct investment/ total capital (6)
Treat	0.028 (0.929)	−0.026 (0.925)	0.337 (0.439)	−0.031 (0.391)	−0.034 (0.372)	0.005 (0.448)
Post	−0.033 (0.748)	0.008 (0.944)	−0.439* (0.077)	−0.011 (0.515)	−0.003 (0.842)	−0.006 (0.186)
Treat*Post	**0.068** (0.423)	**0.028** (0.729)	**0.396** (0.149)	**0.017** (0.115)	**0.013** (0.205)	**0.003** (0.485)
Province FE	✓	✓	✓	✓	✓	✓
ISIC FE	✓	✓	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Trend	✓	✓	✓	✓	✓	✓
No. obs.	41,187	39,235	3,527	41,187	41,187	41,187
No. clusters	19	19	19	19	19	19
$_R2$	0.021	0.017	0.039	0.243	0.271	0.006

Notes: OLS estimates of Equation (1). *P*-values refer to standard errors clustered at the economic activity level in parentheses, calculated using wild cluster bootstraps with 999 replications.

*$p < .10$.

**$p < .05$.

***$p < .01$.

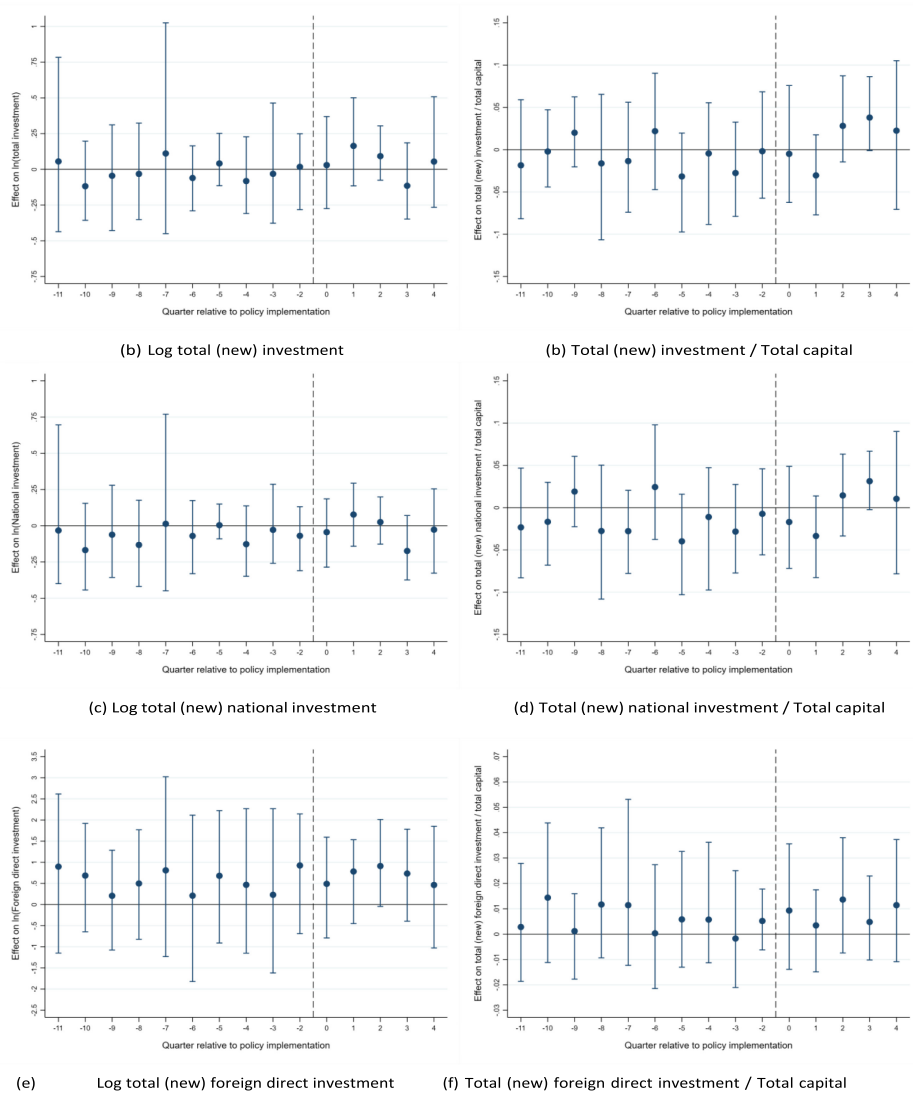


FIGURE 3 Event study of baseline estimates: Firm-level evidence. *Notes*: Figure plots the set of coefficients that correspond to the DiD interaction terms from the event study Equation (2). The reference period is the second quarter of 2018 and time periods are defined on a quarterly basis. Confidence intervals at the 95% level. *Source*: Superintendencia de Compañías, Valores y Seguros.

According to Zwick (2021), firms may fail to respond to tax policy because of tax complexity. However, when there is complexity in understanding a tax reform, it may be caused by the government, but firms may also not be so encouraged to invest in the country because of its characteristics. One conjecture is that a tax reform by itself does not generate enough incentives for new investments, local or foreign, to establish themselves in the country because of other structural problems such as the rigidity of the labour market, corruption, lack of access to credit or crime, among others.[19]

[19]I estimate the same equation by including the risk country variable. The results are very similar in magnitude and significance. The results of this exercise are available upon request.

TABLE 3 Difference-in-differences estimates: Investment and employment macro-level effects.

	Log total (new) investment	log employment stock	Total (new) investment/ total capital	New hires/total employment
Variable	**(1)**	**(2)**	**(3)**	**(4)**
Treat	6.562*** (0.001)	4.429*** (0.000)	0.499** (0.019)	0.099 (0.171)
Post	0.112 (0.628)	0.036 (0.361)	0.011 (0.634)	0.002 (0.836)
Treat*Post	**0.231** (0.347)	**0.010** (0.900)	**−0.005** (0.852)	**−0.005** (0.734)
risk	−0.000 (0.485)	−0.000 (0.888)	−0.000* (0.061)	−0.000 (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 (3). *P*-values refer to standard errors clustered at the economic activity level in parentheses, calculated using wild cluster bootstraps with 999 replications.

*$p < .10$.

**$p < .05$.

***$p < .01$.

To validate the results of the DiD estimator, it is necessary that pre-policy implementation trends for the prioritised sectors (treatment group) and non-prioritised sectors (control group) are parallel. Thus, I present the ESD, featuring 11 pre- and 5 post-periods by estimating Equation (2). Figure 3 shows the results for the six outcomes of interest. I find no evidence of differential pre-trends in the time (quarters) between the treated and the control group.[20] This evidence adds validity to the results of the DiD estimator.

Furthermore, I 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 by analysing the macro-level effects. In Table 3, I present the estimation results of Equation (3). From the results in column (1), it seems that the policy (law) does not have an effect on the log of total (new) investment for prioritised sectors compared to non-prioritised sectors over the last quarter of 2018 and 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 did not have an effect on this outcome for prioritised sectors compared to non-prioritised sectors over the last quarter of 2018 and 2019. Finally, in columns (3) and (4), the results of the effect of the policy on the relative measures of investment and employment, respectively, are 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 or on the creation of new employment in prioritised sectors compared to non-prioritised sectors before vs. after the new law.[21]

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

[21]I also estimate Equation (3) 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.

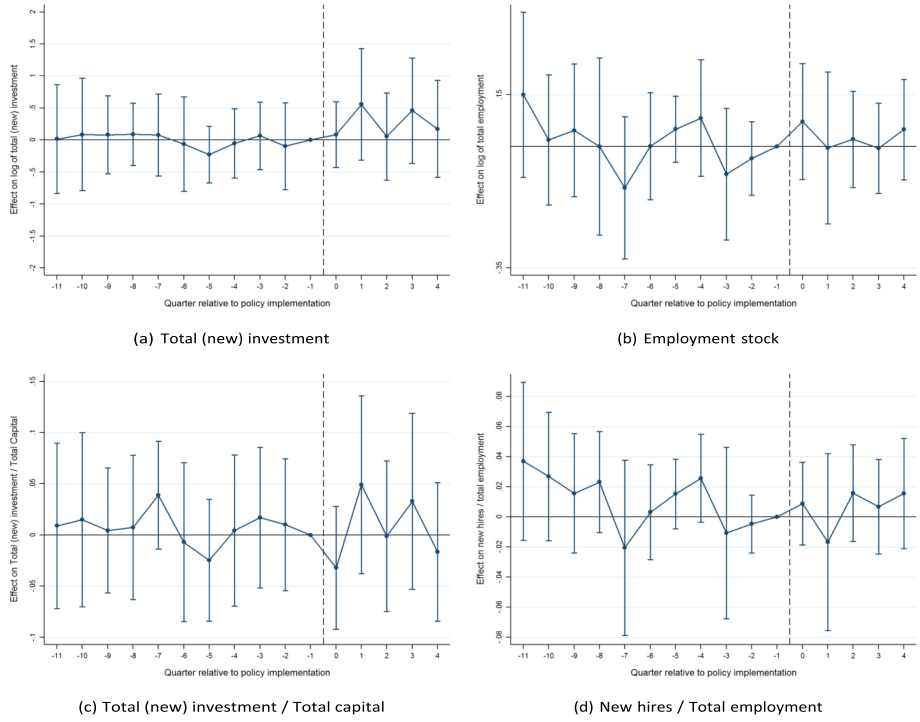


FIGURE 4 Event study of baseline estimates: macro-level evidence. *Notes*: Figure plots the set of coefficients that correspond to the DiD interaction terms from the event study Equation (4). The reference period is the second quarter of 2018 and time periods are defined on a quarterly basis. Confidence intervals at the 95% level. *Source*: Superintendencia de Compañías, Valores y Seguros and Instituto Ecuatoriano de Seguridad Social.

Similar to the analysis of the effect at the micro levels, it is necessary to validate the parallel trend assumption. Thus, I also do 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 (4) in Figure 4 for the four outcomes of interest. The results, depicted in Figure 4, show that the set of pre-reform coefficients (β_3) are statistically indistinguishable from zero for all four outcomes, which suggests a flat pre-trend. However, I do not find a positive and statistically significant effect of the policy reform on investment or employment.[22] These results support the parallel trend assumption and add validity to the identification strategy.

Overall, the results suggest that the policy does not seem to be effective in attracting investment or creating employment at either the micro-level or the macro-level. 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 other empirical studies. 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),

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

who argues 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 & James, 2010) and may not be enough to attract investments because of political and macroeconomic instability, lack of rule of law, corruption or administrative barriers (Morisset & Pirnia, 2000; Zee et al., 2002). 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 impact on employment. Also, the null effect of this policy is similar to Yagan (2015), who found that the 2003 dividend tax cut, one of the largest reforms ever to a US capital tax rate, caused zero change in corporate investment and employee compensation.

5.2 | Heterogeneous effects

In this section, I explore whether there are differences across geographical areas of the country. Again, I analyse whether there are micro-level and macro-level effects. Table A2 shows the results of the estimates of Equation (1), which capture the effect at the micro-level. The evidence obtained suggests 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 (national and FDI) for prioritised sectors compared to non-prioritised sectors before vs. after the implementation of the law in each Ecuadorian region. In addition, in Table A3, the macro-level effects are presented. I present the results of the estimates from Equation (3) split by regions. Overall, the results suggest that the new law does not have an effect on the attraction of new investments or the creation of new employment for prioritised sectors compared to non-prioritised sectors before vs. after the implementation of the law. This evidence adds validity to the empirical strategy because no region is affected differently by the law.

Furthermore, in Ecuador, according to the National Employment Survey (ENEMDU, in Spanish) for September 2018, the city of Guayaquil, located in Guayas province, accounted for approximately 24% of urban workers, and the capital, Quito (located in the province of Pichincha), registered 17.38% of urban workers (INEC, 2018). Moreover, Guayas and Pichincha are the provinces that have received the most private investment since 2014, with an annual average of 38% of the total private investment in the country (Camino-Mogro et al., 2018; SCVS, 2021). In addition, the economic sector that received the most private investment from 2016 to 2019 was the agriculture, livestock and fishing sector, followed by the manufacturing sector. On average, the agriculture, livestock and fishing sectors represent 23% of total investment in all economic sectors. It is followed by the manufacturing sector, which receives an average of 20% (Camino-Mogro et al., 2018; SCVS, 2021).

In this sense, one may still wonder whether there are provinces that have been affected, but 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 Equations (1) and (3) 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 be not significant distinguished from zero.

The results of this exercise are presented in Tables A4 and A5, for the micro-level and macro-level effects, respectively. I find that after excluding investments and employment spells in the Guayas and Pichincha provinces, and in the agriculture, livestock and fishing and manufacturing sectors separately, the effect continues to be not significant at standard levels. Thus, even if I do not consider 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 have an effect on the attraction of new investments or the creation of new employment for prioritised sectors compared to non-prioritised sectors over the last quarter of 2018 and 2019.

5.3 | Robustness checks

5.3.1 | Placebo event dates

I perform a placebo test in which I change the timing of the policy implementation, supposing that the event took place one to 10 quarters before the actual policy implementation (Q3-2018). To do this, I first drop all observations from the actual event date onwards and estimate a series of regressions of the form of Equation (1) for the micro-level evidence and regressions of the form of Equation (3) for the macro-level evidence.

This placebo exercise is done to add support to the parallel trends assumption, and I should not find any significant effect on the DID coefficients. Figure 5 plots the corresponding DiD coefficients along with their 95%

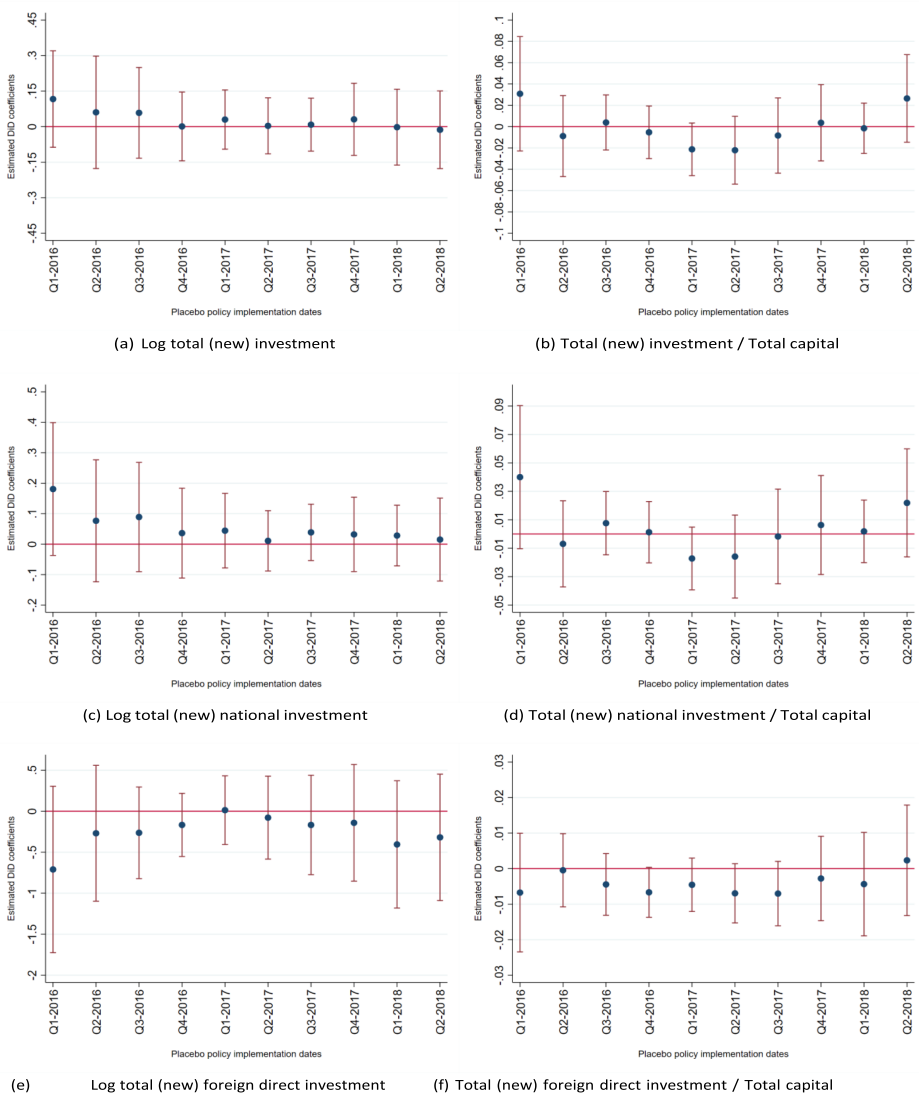


FIGURE 5 Robustness check - Placebo event dates: Firm-level evidence. *Notes*: Each point estimate (and its 95% confidence interval) represents a separate regression in which the event date is set according to the *X* axis. *Source*: Superintendencia de Compañías, Valores y Seguros.

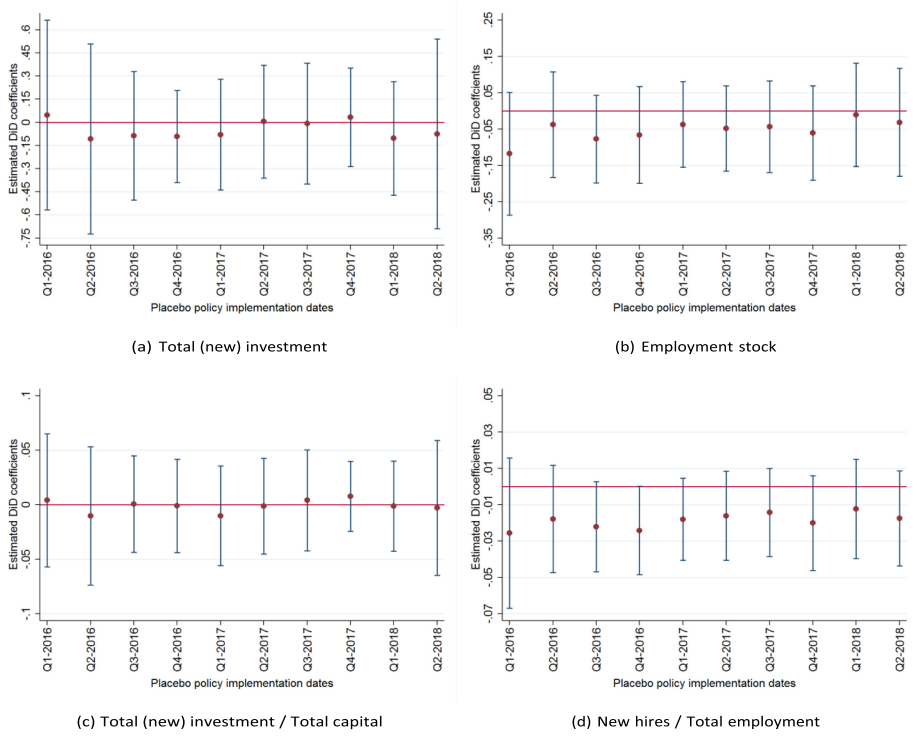


FIGURE 6 Robustness check - Placebo event dates: Macro-level evidence. *Notes*: Each point estimate (and its 95% confidence interval) represents a separate regression in which the event date is set according to the *X* axis. *Source*: Superintendencia de Compañías, Valores y Seguros and Instituto Ecuatoriano de Seguridad Social.

confidence intervals. Overall, I do not find any significant points estimate at the micro-level in any of the six investment outcomes. Moreover, Figure 6 plots the corresponding DiD coefficients along with their 95% confidence intervals for the macro-level evidence. Again, I do not find any significant points estimate in any of the four outcomes analysed. In this sense, this placebo exercise supports our identification strategy since I do not find substantial evidence of non-parallel trends before the implementation of the law.

Finally, the results of this exercise add evidence that supports the exogeneity of the Organic Law for Productive Development, Attraction of Investments, Generation of Employment, Stability, and Fiscal Balance introduced on August 21st, 2018, as it was largely unexpected by firms and economic agents. If firms might anticipate the implementation of the law, some quarters before the reform would be statistically significant; however, this is not the case.

5.3.2 | Placebo treatment group

In addition, I conducted a second placebo test to indirectly assess the common time trend assumption. I estimate Equations (1) and (3) using an alternative treatment group. I assign as placebo treatment units the wholesale and retail trade sectors and drop from the analysis observations that correspond to prioritised sectors (actual treated group). The control group remains the same: non-prioritised sectors. Because I expect the increase in total (new) investment and employment due to the policy implementation to affect mainly prioritised sectors, I should not find an effect on the wholesale and retail trade sectors; otherwise, our identification strategy would be challenged. Thus,

TABLE 4 Robustness check - Placebo treatment group: Firm-level evidence.

Variable	Log total (new) investment (1)	Log total (new) national investment (2)	Log total (new) foreign direct investment (3)	Total (new) investment/ total capital (4)	Total (new) national investment/ total capital (5)	Total (new) foreign direct investment/ total capital (6)
Treat	0.570 (0.375)	0.442 (0.395)	0.573 (0.257)	−0.106 (0.338)	−0.111 (0.217)	0.005 (0.269)
Post	−0.014 (0.900)	0.058 (0.592)	−0.719** (0.050)	−0.005 (0.844)	0.007 (0.756)	−0.010 (0.206)
Treat*Post	**−0.099** (0.397)	**−0.125** (0.360)	**0.091** (0.546)	**0.002** (0.798)	**−0.002** (0.747)	**0.003** (0.527)
Province FE	✓	✓	✓	✓	✓	✓
ISIC FE	✓	✓	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Trend	✓	✓	✓	✓	✓	✓
No. obs.	22,118	21,087	1,887	22,118	22,118	22,118
No. clusters	10	10	10	10	10	10
$_R2$	0.026	0.019	0.039	0.216	0.242	0.005

Notes: OLS estimates of Equation (1). *P*-values refer to standard errors clustered at the economic activity level in parentheses, calculated using wild cluster bootstraps with 999 replications.

*$p < .10$.

**$p < .05$.

***$p < .01$.

I re-estimate Equations (1) and (3) using the wholesale and retail trade sectors during the period of analysis as the treated group. The results of this exercise are shown in Tables 4 and 5.

In general, I find that none of the investment or 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 the wholesale and retail trade sectors, which supports the empirical strategy.

Finally, given flat pre-trends, this research design would still be invalid if local shocks systematically affected investments and employment measures. For this reason, I re-estimate Equation (4) using industry-GDP and 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 (3) by including industry-GDP and oil prices, as control variables, for 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.[23]

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

[23]The results of these exercises are available upon request.

TABLE 5 Robustness check - Placebo treatment group: Macro-level evidence.

	Log total (new) investment	log employment stock	Total (new) investment/ total capital	New hires/total employment
Variable	**(1)**	**(2)**	**(3)**	**(4)**
Treat	7.643*** (0.006)	5.500*** (0.000)	0.614*** (0.005)	0.286* (0.060)
Post	0.145 (0.497)	0.034 (0.340)	0.022 (0.331)	0.004 (0.738)
Treat*Post	**1.002** (0.266)	**0.064** (0.430)	**0.100** (0.290)	**−0.002** (0.836)
risk	−0.000 (0.910)	−0.000** (0.041)	−0.000 (0.520)	−0.000 (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 (3). *P*-values refer to standard errors clustered at the economic activity level in parentheses, calculated using wild cluster bootstraps with 999 replications.

*$p < .10$.

**$p < .05$.

***$p < .01$.

6 | FINAL REMARKS

This paper assesses the impact of the effects of tax incentives of an Ecuadorian reform in 2018 at the micro and macro-levels, on different outcomes such as i) the log of total (new) investment, ii) the log of total (new) national investment, iii) the log of total (new) foreign direct investment, iv) the ratio between total (new) investment over total capital, v) the ratio between total (new) national investment over total capital, vi) the ratio between total (new) foreign direct investment over total capital, vii) the log of total employment (employment stock) and viii) the ratio between total new hires over total employment. By using two novel, underexplored administrative data sets, I exploit the time, firm and industry variations in the implementation of this policy and provide evidence that suggests that fiscal incentives, by themselves, do not have a significant effect on private investment or formal employment.

I contribute to filling this empirical gap in the literature by examining the extent to which a program of tax incentives in prioritised sectors is not effective in increasing new investments or formal employment in Ecuador. I conjecture that tax incentives are unlikely to significantly affect the new investment or formal employment because there are other important factors that stop the decision to invest in Ecuador, such as corruption, quality of institutions, country risk, inequalities, country size, ease of doing business, etc. In this sense, Zee et al. (2002) and Klemm (2010) mention a non-exhaustive list of policy implications for an effective policy of tax incentives. For example, the main issue is to identify the sectors that benefit from the reduced tax rate so as to ensure transparency and avoid discretionary on the part of authorities. Furthermore, to stimulate investment, it is necessary for the country to follow international norms, and the regulatory environment must function well. Moreover, the implementation of fiscal

incentive programs aimed at increasing employment creation must be accompanied by other policies that target both productivity and the quality of employment (Garsous et al., 2017), which has not been the case in Ecuador.

Finally, an important limitation of this paper is that it focuses on formal employment and not on the informal labour force. Also, the paper does not analyse labour productivity, total factor productivity or entry and exit firms. Furthermore, I do not study the complexity of the tax reform, which may demotivate firms to adhere to the law. Additionally, in this paper, I analyse only short-term effects because of the effects of Covid-19 on firms' investment decisions. In this sense, these are issues for future research.

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CONFLICT OF INTEREST STATEMENT

The author declares that there is no conflict of interest.

DATA AVAILABILITY STATEMENT

The data that support the findings of this study can be requested from the Superintendencia de Compañías, Valores y Seguros and Instituto Ecuatoriano de Seguridad Social. Restrictions apply to the availability of these data, which were used under license for this study. Replication files are available upon request by e-mail to the corresponding author.

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APPENDIX A

TABLE A1 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.

English version downloadable at https://unstats.un.org/unsd/publication/seriesm/seriesm4rev4e.pdf.

TABLE A2 Heterogeneity impact on investment and employment by region: Firm-level evidence.

Variable	Log total (new) investment (1)	Log total (new) national investment (2)	Log total (new) foreign direct investment (3)	Total (new) investment/ total capital (4)	Total (new) national investment/ total capital (5)	Total (new) foreign direct investment/ total capital (6)
Panel A: Costa region						
Treat	0.081	0.041	0.649	−0.071	−0.071	0.001
	(0.822)	(0.893)	(0.138)	(0.109)	(0.126)	(0.872)
Post	−0.058	−0.048	0.211	−0.015	−0.008	−0.006
	(0.614)	(0.697)	(0.662)	(0.624)	(0.813)	(0.467)
Treat*Post	**0.031**	**0.018**	**−0.210**	**0.026**	**0.023**	**0.005**
	(0.765)	(0.843)	(0.638)	(0.250)	(0.252)	(0.456)
No. obs.	20,031	19,296	1,425	20,031	20,031	20,031
R^2	0.013	0.011	0.061	0.161	0.173	0.002
Panel B: Sierra region						
Treat	−0.018	−0.091	0.116	0.010	0.002	0.010
	(0.954)	(0.778)	(0.817)	(0.811)	(0.965)	(0.176)
Post	−0.040	0.051	−0.185	−0.003	0.007	−0.008
	(0.740)	(0.693)	(0.105)	(0.835)	(0.589)	(0.307)
Treat*Post	**0.145**	**0.079**	**0.250**	**0.003**	**−0.002**	**0.002**
	(0.167)	(0.470)	(0.122)	(0.881)	(0.899)	(0.811)
No. obs.	19,775	18,572	2,070	19,775	19,775	19,775
R^2	0.021	0.017	0.032	0.237	0.278	0.008
Panel C: Amazonia region						
Treat	−0.155	−0.201	1.215	−0.029	−0.038	0.007
	(0.511)	(0.354)	(0.545)	(0.334)	(0.330)	(0.673)
Post	0.276	0.016	2.957	−0.070	−0.110	0.031
	(0.336)	(0.938)	(0.579)	(0.548)	(0.202)	(0.363)
Treat*Post	**−0.108**	**−0.128**	**0.329**	**0.096**	**0.095**	**0.004**
	(0.651)	(0.553)	(0.874)	(0.154)	(0.202)	(0.875)
No. obs.	1,381	1,367	32	1,381	1,381	1,381
R^2	0.058	0.053	0.571	0.075	0.062	0.018
Province FE	✓	✓	✓	✓	✓	✓
ISIC FE	✓	✓	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Trend	✓	✓	✓	✓	✓	✓
No. clusters	19	19	19	19	19	19

Notes: OLS estimates of Equation (1). *P*-values refer to standard errors clustered at the economic activity level in parentheses, calculated using wild cluster bootstraps with 999 replications.

*$p < .10$.

**$p < .05$.

***$p < .01$.

TABLE A3 Heterogeneity impact on investment and employment by region: Macro-level evidence.

Variable	Log total (new) investment (1)	Log employment stock (2)	Total (new) investment/ total capital (3)	New hires/total employment (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 (3). *P*-values refer to standard errors clustered at the economic activity level in parentheses, calculated using wild cluster bootstraps with 999 replications.

*$p < .10$.

**$p < .05$.

***$p < .01$.

TABLE A4 Robustness check - excluding relevant provinces and economic sectors: Firm-level evidence.

Variable	Log total (new) investment (1)	Log total (new) national investment (2)	Log total (new) foreign direct investment (3)	Total (new) investment/ total capital (4)	Total (new) national investment/ total capital (5)	Total (new) foreign direct investment/ total capital (6)
Panel A: Excluding Guayas						
Treat	0.007	−0.051	0.172	−0.014	−0.020	0.006
	(0.983)	(0.872)	(0.701)	(0.204)	(0.707)	(0.423)
Post	−0.009	0.043	−0.189	0.018	0.022*	−0.002
	(0.945)	(0.721)	(0.111)	(0.785)	(0.100)	(0.670)
Treat*Post	**0.106**	**0.059**	**0.242**	**0.013**	**0.010**	**0.003**
	(0.337)	(0.563)	(0.118)	(0.406)	(0.546)	(0.597)
No. obs.	26,674	25,356	2,351	26,674	26,674	26,674
Panel B: Excluding Pichincha						
Treat	0.003	−0.036	0.535	−0.047	−0.047	0.001
	(0.990)	(0.896)	(0.234)	(0.301)	(0.331)	(0.837)
Post	0.016	0.005	−0.102	−0.023	−0.018	−0.005
	(0.872)	(0.961)	(0.807)	(0.484)	(0.512)	(0.455)
Treat*Post	**0.061**	**0.049**	**0.070**	**0.032***	**0.029**	**0.004**
	(0.578)	(0.629)	(0.847)	(0.070)	(0.100)	(0.415)
No. obs.	27,322	26,504	1,626	27,322	27,322	27,322
Panel C: Excluding agriculture, livestock and fishing sector						
Treat	−0.082	−0.125	0.292	−0.022	−0.026	0.004
	(0.792)	(0.646)	(0.602)	(0.590)	(0.550)	(0.582)
Post	−0.023	0.002	−0.443	−0.016	−0.008	−0.007
	(0.819)	(0.991)	(0.110)	(0.372)	(0.600)	(0.154)
Treat*Post	**0.071**	**0.036**	**0.308**	**0.015**	**0.012**	**0.003**
	(0.437)	(0.694)	(0.286)	(0.101)	(0.268)	(0.595)
No. obs.	38,330	36,509	3,227	38,330	38,330	38,330
Panel D: Excluding manufacturing sector						
Treat	−0.101	−0.142	0.064	−0.025	−0.028	0.003
	(0.715)	(0.577)	(0.868)	(0.547)	(0.522)	(0.699)
Post	0.013	0.069	−0.267*	−0.006	0.002	−0.006
	(0.876)	(0.405)	(0.094)	(0.707)	(0.894)	(0.239)
Treat*Post	**0.043**	**−0.004**	**0.231*****	**0.017**	**0.012**	**0.006**
	(0.624)	(0.960)	(0.006)	(0.106)	(0.321)	(0.151)
No. obs.	38,132	36,380	3,149	38,132	38,132	38,132
Province FE	✓	✓	✓	✓	✓	✓
ISIC FE	✓	✓	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Trend	✓	✓	✓	✓	✓	✓
No. clusters	19	19	19	19	19	19

Notes: OLS estimates of Equation (1). *P*-values refer to standard errors clustered at the economic activity level in parentheses, calculated using wild cluster bootstraps with 999 replications.

*$p < .10$.

**$p < .05$.

***$p < .01$.

TABLE A5 Robustness check - excluding relevant provinces and economic sectors: Macro-level evidence.

	log total (new) investment	log employment stock	total (new) investment/ total capital	New hires/total employment
Variable	**(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	✓	✓	✓	✓

TABLE A5 (Continued)

Variable	log total (new) investment (1)	log employment stock (2)	total (new) investment/ total capital (3)	New hires/total employment (4)
ISIC FE	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Trend	✓	✓	✓	✓
No. clusters	19	19	19	19

Notes: OLS estimates of Equation (3). *P*-values refer to standard errors clustered at the economic activity level in parentheses, calculated using wild cluster bootstraps with 999 replications. The number of observations used when I exclude Guayas and Pichincha is 6,992, and when I exclude the agriculture, livestock and fishing, and manufacturing sectors, it is 6,912.

*$p < .10$.

**$p < .05$.

***$p < .01$.