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Octavio Bertín, Guillermo Cruces, Fabián  
Enrique González, Ignacio Lunghi y Azul  
Menduiña

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# Effects of Hiring Credits on the Argentine Labor Market\*

Octavio Bertín, *CEDLAS-IIE-UNLP*

Guillermo Cruces, *CEDLAS-IIE-UNLP & University of Nottingham*

Fabián Enrique González, *CEDLAS-IIE-UNLP*

Ignacio Lunghi, *CEDLAS-IIE-UNLP*

Azul Menduiña, *CEDLAS-IIE-UNLP*

## Abstract

This study assesses the impact of a hiring credit policy introduced in Argentina in 2014 on various labor outcomes of firms. The reform established differentiated labor cost reductions in the employers contributions to be paid for their new employees, according to the size that firms had at a date prior to the announcement of the policy. Using a differences-in-differences approach and employer-employee administrative data, we analyze the intervention's effects. Our results show a significant 4.1 percentage point increase in employment growth rates for small firms compared to medium-sized ones, persisting for several years post-reform. This paper also explores the relationship between the intervention's effects and sector labor informality; we find a significant 6.2 percentage point increase in employment growth rates for firms in high-informality sector, whereas no significant effect is found for firms in low-informality sectors.

JEL Codes: C31, J08, J23, O17

Keywords: hiring credits, employment, wages.

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## 1. Introduction

In recent years, governments have increasingly adopted hiring credits as a strategy to stimulate job creation. Unlike broad payroll tax reductions that apply to all employees, hiring credits specifically target new hires, potentially offering a more cost-effective approach by directly subsidizing the labor costs associated with expanding the workforce. This targeted mechanism allows firms to increase employment more efficiently, particularly in sectors with wage rigidity. For instance, the French hiring credit implemented during the Great Recession had significant positive employment effects without affecting wages (Cahuc *et al.*, 2019). Similarly, research indicates that certain designs, such as those targeting the unemployed, can effectively boost job growth (Neumark & Grijalva, 2017). However, empirical evidence on the broader impacts of hiring credits remains limited and is primarily concentrated in developed countries during recessionary periods. This underscores the need for further research to evaluate their effectiveness across diverse economic contexts.

In the absence of conclusions that can be extrapolated to the context of developing countries, extending the evidence to Argentina provides a valuable contribution, both from an academic perspective and for the formulation of public policies. The latter becomes particularly relevant if we consider the stagnation that Argentine formal employment has suffered during the last decade. Between 2010 and 2019, registered jobs in the private sector grew by only 7% , while the unemployment rate increased by 35%. In turn, in Argentina, as well as in the rest of Latin American countries, the interest in analyzing the functioning of this type of policies responds to an additional concern: labor informality. High levels of informal employment have been a persistent problem for governments in the region. In addition to hindering tax collection and generating inefficiencies in the allocation of social welfare spending, informality is associated with poorer quality jobs, as they tend to offer lower wages, less stability and lower labor benefits (Camacho *et al.*, 2014; Gerard & Gonzaga, 2016; Lauletta, 2023). In this context it is crucial to generate solid evidence on the effectiveness of certain policies in order to extract valuable lessons that contribute to the design of public policies.

With the aforementioned purposes, our paper analyzes a reform that took place in Argentina in August 2014, which modified the tax benefits that firms could obtain up to that time, moving from a system of reductions in employer contributions for new employees, homogeneous for all firms, to another in which the reduction depended on their size, measured according to the number of employees. The smallest firms benefited the most from the new system. The

objective of this paper is to evaluate the impact of these increased tax benefits on employment. We also examine the different effects across sectors according to their level of labor formality.

Given that the policy design established an allocation of benefits according to firm size at a date prior to the announcement of the policy, this study uses that allocation to treatment as an exogenous variation introduced by the new regulation. Taking advantage of this quasi-experiment, a difference-in-differences (DiD) approach is employed to estimate the causal effect of the policy. For this purpose, employer-employee administrative records of the Argentine Integrated Pension System (SIPA, for its Spanish acronym) and administrative records of the Federal Administration of Public Income (AFIP, for its Spanish acronym) registration certificate are used.

The results of the analysis lead to important implications regarding the impact of the policy on the labor market. The policy proved to have a tangible effect, resulting in a statistically significant increase of 4.1 percentage points in the employment growth rates of small firms compared to their medium-sized counterparts. This impact remained relatively constant throughout the post-reform years (2015, 2016 and 2017), corroborating the stability of the observed effect over time. Although this effect is larger than that found in other papers, such as that of Cahuc *et al.* (2019), it is in line with the new theoretical literature that has emerged recently, such as the work of Graves (2023), which highlights that subsidies that encourage job creation are less effective in recessions.

Our findings also show that, within the “small firms” group, the influence of the policy was most pronounced in the smaller subset of firms. Firms with a size from 6 to 8 employees recorded an increase in the employment growth rate of over 5.06 percentage points. For firms with 9 to 11 and 12 to 14 employees, the impact on the employment growth rate was 3.28 and 2.59 percentage points, respectively (an statistically significant difference when compared to the smaller firms). This evidences the differential effect of the policy depending on the size of the firm.

Finally, comparing small and medium-sized companies, an increase of 0.4 p.p. was observed in the probability of firms hiring employees previously classified as self-employed (or *monotributo*, according to Argentine Law, often used by firms to under-report the total number of workers). Furthermore, when analyzing the heterogeneity between productive sectors, we found that our effect was driven by those sectors which previously had the highest level of informality.<sup>1</sup>

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<sup>1</sup> We classified productive sectors into low or high informality using the Argentine National 2010 Census, depending

As mentioned previously, most of the existing literature on hiring credits is focused on developed countries and recessionary contexts. For France, Cahuc *et al.* (2019) examined a hiring credit policy targeting low-income employees of small firms that took place during the Great Recession, using firm-level data and a difference-in-differences (DiD) approach. Their findings indicated positive effects on employment growth, while no significant impacts on wage growth were observed. Batut (2021) extended the analysis using a Dynamic Difference in Differences to look at the long-term impact. In turn, Neumark and Grijalva (2017) exploited cross-state variability in state hiring credits in the United States and found moderate positive effects on employment growth. Within the different types of hiring credits provided by states, the authors found that only certain types were effective, such as those which targeted unemployed. On the other hand, an interesting effect was found in the d'Agostino *et al.* (2020) study for Italy, who observe an unexpected negative impact on the wages of employees who benefited from the tax cut, which was more pronounced for women. Finally, some studies also point out the cost-ineffectiveness of these type of programs (Fenizia *et al.*, 2024).

More generally, this paper also links to the literature examining the effects of payroll tax cuts. Saez *et al.* (2019) employed administrative data to analyze a substantial and long-lasting reduction in the payroll tax for young workers in Sweden that was implemented starting in 2007. Their findings revealed a null effect on the net-of-tax wages of young employees and a positive effect on their employment rate, evidencing a 2-3 percentage point increase in youth employment compared to the control group. These results are consistent with the findings of previous research examining the same reform. One of these is the work of Egebark and Kaunitz (2014), which found small effects of the policy on both employment and wages, suggesting an elasticity of labor demand for young employees of approximately  $-0.31$ . Considering that the tax reduction also applied to workers already employed, the cost associated with each new job created turned out to be considerably high, evidencing the inefficiency of the policy. For the specific case of Argentina, Cruces *et al.* (2010) investigated the effect on wages and employment of payroll tax changes introduced during the 1995-2001 period, using administrative data and exploiting the geographic and temporal variability of tax rates. Their results suggest that changes in payroll tax rates were only partially passed through to wages, and point to the absence of significant employment effects. Also for Argentina, Lauletta (2023) found that changes in payroll taxes reduced informal employment, albeit slightly, while having no effect on wages. Finally, Lobel (2024) observed a rise in employment, particularly notable among small

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on whether their informality rate is above or below the median informality rate of all sectors.

firms, alongside an increase in wages, which contrasts with the majority of existing literature.

As we can see, the emerging evidence on hiring credits is not conclusive and is only available for developed countries. Our study makes a significant contribution to the hiring credits literature by carrying out a specific analysis for a developing country, offering evidence on the impact not only on employment, but also on the differential effects of the reform across sectors of low and high (previous) informality. It is also highlighted that, unlike the policies analyzed in previous research, this reform took place in a non-recessionary context, considering that in the period 2010-2018 Argentina had a cycle of GDP falls in even-numbered years and GDP growth in odd-numbered years. In this way, this paper provides valuable evidence of the functioning of hiring credits in unexplored scenarios.

The paper is structured as follows. Section 2 describes the reform analyzed, while Section 3 discusses the data and methodology employed in this paper. The main results are presented in Section 4, followed by several robustness tests in Section 5. Finally, Section 6 concludes.

## **2. The Reform**

Prior to 2014 Argentina had a system of hiring credits for payroll taxes in which all employers had to pay 50% of the contributions for their new permanent employees during the first 12 months and 75% of the contributions during the second 12 months. In May 2014, the Argentine government, through Law 26940, promoted a reform on these credits (which was announced on April 2014). Unlike the previous system, the reform established different benefits according to the size of the firm in a period prior to the announcement of the reform: March 2014. For small firms (up to 15 permanent employees in March 2014), for each new permanent employee hired, the firms did not have to pay contributions during the first 12 months, while during the following 12 months the credit covered 75% of the contributions. In the case of medium-sized firms (from 16 to 80 permanent employees in March 2014), the firms had to pay 50% of the contributions during the first 12 months and 50% during the following 12 months. Finally, firms with more than 80 permanent workers in March 2014 had no benefit. Both systems are summarized in Table 1. Also, it is worth mentioning that employers were able to access the benefits as of August 2014, and that, initially, the benefit was to be available until August 2015, but through successive decrees it was extended until December 2017.

It should be noted that the possibility of obtaining a reduction in employer contributions for new workers was not unlimited. If a firm exceeded the limit of employees that defined its

group (always taking into account only permanent workers), then the firm would have become part of the next higher group of beneficiaries, despite not losing the benefits of the previous workers.<sup>2</sup> Note that, in this way, in the case of companies that had precisely 15 employees in March 2014, even though they were theoretically considered small companies, they could not access the tax benefits of such group. Therefore, for the purposes of the analysis, these companies will be included within the group of medium-sized companies.

The possibilities of carrying out strategic actions to obtain extra benefits were limited. On the one hand, as previously mentioned, the size that was considered to evaluate a firm's eligibility for the program was that corresponding to the month prior to the announcement, which prevented pervasive incentives for firms such as manipulating their size in order to obtain greater benefits. In turn, evidence suggests that the policy was not anticipated by agents either, in the sense that prior to the announcement they were unaware of the existence of the measure. Figure 1 shows that Google searches for the law began only in the period after the April 2014 announcement. On the other hand, the law also established that employers could enjoy the corresponding benefit for each new worker hired, as long as such additional worker produced an increase in the total number of permanent workers with respect to the reference period. In this way, firms were not able to choose, for example, to lay off workers in order to replace them or to rehire them under the new modality.

Finally, to illustrate the importance of the reform for the firm, Figure 2 shows the effects of the hiring credits on the labor costs of new permanent employees, on a hypothetical first stage. In the first year, the credit reduces labor cost by 23% for small firms, while in the second year, the policy reduces labor costs by 17.25% for them.

### **3. Data and methodology**

#### **3.1. Data**

We mainly use employer-employee administrative data from the Argentine Integrated Pension System (SIPA), which is a matched database of employers and employees used by the pension system to calculate individual pensions and record the contributions made by firms for their

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<sup>2</sup> For example, if a company had 12 permanent workers in March 2014 (small company) and, as a result of the reform, increases its plant to 17 permanent workers (becoming a medium-sized company), then it could obtain: (a) for the first 3 employees hired, a 100% reduction in the contributions of the first year and 75% in those of the second year, and (b) for the following 2 additional workers, only a 50% reduction in the contributions of both years.

employees<sup>3</sup>. From it, a monthly panel of private firms is constructed, in which the main variable included is the number of permanent employees in the firms (those who have been hired during the last year). SIPA data were also unified with data from the records of the Federal Administration of Public Income (AFIP), which has information on taxpayers registered in the *monotributo* regime.<sup>4</sup>

The period covered by the database extends from July 2010 to July 2017, with monthly frequency. The reason for excluding years after 2017 is due to the enactment of Law 27430 in December of that year, which legally invalidates the articles of Law 26940 that supported the reform analyzed. Although it might be tempting to assess the effects of the elimination of the reform, it is important to note that with Law 27430 several other regulations were modified simultaneously, which would make it impossible to ensure that the effect found is exclusively the result of the elimination of the interest intervention.

### 3.2. Methodology

As discussed in Section 3, the reform generated exogenous variations in firms' tax benefits, thus creating a valuable opportunity to analyze the effects of temporary hiring credits on various employment outcomes. In this section, we present the identification strategy used, which exploits the difference between two groups experiencing different levels of benefits: small and medium-sized firms.

A difference-in-differences approach is used<sup>5</sup>, where the treatment group consists of firms with between 6 and 14 employees in March 2014 (small firms), which qualified for the highest tax breaks of the policy, and the control group consists of firms with between 15 and 23 employees in March 2014 (medium-sized firms), which only had a small reduction. Given that the control group also receives some treatment, the effects found in this paper should be interpreted as lower bounds.<sup>6</sup>

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<sup>3</sup> To preserve the confidentiality of the data, the Center of Studies for Production XXI (CEP XXI, for its Spanish acronym) was the think tank in charge of processing the databases. Consequently, the data used for the development of the research were completely anonymised in order to guarantee statistical confidentiality.

<sup>4</sup> Also, those firms that at some point were found in the REPSAL register, a register of firms that failed to comply with labor laws, were excluded.

<sup>5</sup> Although at first sight it may seem obvious to use a regression discontinuity approach, there is an imperfect treatment assignment that prevents the correct implementation of the methodology, considering that firms' accountants' records may not be excessively controlled by AFIP, as shown in Figure 3. This situation determines the absence of a discontinuity in the treated firms around the number of employees of 15.

<sup>6</sup> The reason for not using the group of firms with more than 80 permanent employees as a control group, for which benefits were entirely eliminated, is due to the log-normal behaviour of the size distribution of firms. Given the small number of large firms, their use as a comparison group is discarded because of the threat to the estimates. On the other hand, companies with less than 6 permanent employees were eliminated from the analysis because other articles of the same law gave them additional benefits.



The main outcome variable of the regressions is the growth rate of permanent employment of firms.<sup>7</sup>

Since there is a single common treatment date for all firms, we first estimate the following specification:

$$Y_{it} = \alpha + \beta(\text{Treat}_i \times \text{Post}_t) + \phi_i + \gamma_t + \varepsilon_{it} \quad (1)$$

Where  $Y_{it}$  is the outcome of firm  $i$  in period  $t$ ,  $\phi_i$  and  $\gamma_t$  are firm-level and time fixed effects, respectively,  $\text{Treat}_i$  is a dummy variable indicating whether firm  $i$  had between 6 and 14 employees in March 2014 (ie., if it is part of the treatment group) and  $\text{Post}_t$  indicates whether  $t$  is a period after July 2014 (that is, if it is after the implementation of the reform). Finally,  $\varepsilon_{it}$  is the error term. Standard errors are clustered at the firm level.

Assuming parallel trends, the parameter  $\beta$  represents the average causal effect of the reform on  $Y_{it}$  for small firms. Since it captures the effects regardless of whether firms use the benefits or not, it should be interpreted as an intention-to-treat effect (ITT).

An extension of the previous specification is also estimated to allow for size heterogeneities within the group of small firms.

$$Y_{it} = \alpha + \beta_1 I(\text{Size} \in [6, 8]) \times \text{Post} + \beta_2 I(\text{Size} \in [9, 11]) \times \text{Post} + \beta_3 I(\text{Size} \in [12, 14]) \times \text{Post} + \phi_i + \gamma_t + \varepsilon_{it} \quad (2)$$

Where  $\beta_1, \beta_2$  and  $\beta_3$  represent the differential effects of firms according to their size in March 2014. The estimation of these parameters allows us to test for differential effects within the treated firms.

Additionally, we estimate a dynamic specification of the equation 1.

$$Y_{it} = \alpha + \sum_{k=1q2010}^{1q2014} \beta_k [\text{Treat}_i \times I(t = k)] + \sum_{k=3q2014}^{4q2017} \beta_k [\text{Treat}_i \times I(t = k)] + \phi_i + \gamma_t + \varepsilon_{it} \quad (3)$$

Estimations are performed quarterly, so here  $I(t = k)$  indicates that the period  $t$  (a quarter) is  $k = 1q2010, \dots, 4q2017$ . The omitted period is the second quarter of 2014, which corresponds

<sup>7</sup> The reason for using growth rates instead of level variables can be found in Cahuc *et al.* (2019). To illustrate their point, consider a simple static model, where the production function is  $F(L)$ , labor cost is equal to the net wage  $w$  and firms are perfect competitors. The optimal level of employment satisfies  $F'(L) = w$ . This equation implies that a one per cent change in labor cost induces a change in the level of employment that is proportional to the firm's initial level of employment. This means that, even in the absence of a hiring credit, a common shock that also affects the labor costs of the two groups of firms will have a different effect on employment.

to the quarter prior to the implementation of the reform (readers should keep in mind that our annual data always correspond to the month of July). The other variables definitions are inherited from our previous specifications.

With equation (3) it is possible to assess the dynamic evolution of the treatment effect, since the parameters  $\beta_k$  for  $k = 3q2014, \dots, 4q2017$  measure, for each quarter, the average causal effect of the reform on  $Y_{it}$  for small firms. Additionally, the estimation of the  $\beta_k$  for  $k = 1q2010, \dots, 2q2014$  allows us to observe whether the reform had effects before its implementation, that is, it allows us to test for the presence of pre-trends. The presence of differential trends between groups in the period prior to the introduction of the reform would constitute a threat to the internal validity of the results. Thus, in order to validly interpret our findings from a causal approach, it is essential that the estimated coefficients for the pre-treatment period are not statistically different from zero.

We also seek to evaluate whether the reform had different effects on different sectors according to their labor informality. To do this, we estimate the same static and dynamic regressions as those of equations 1 and 3 respectively but for low and high informality sectors separately. We calculate the informality level of each economic activity using Argentine census data from 2010. Subsequently, we identify sectors with high informality (above the median) and re-estimate equation 1 using separate samples.

Finally, we estimate the effect of the reform on firms hiring employees who six months prior were self-employed. We use workers in the labor category of *monotributo*, Argentina's labor regime for self-employed workers, often used by firms to under-report workers. For this purpose, both the SIPA database and the AFIP registration certificates are used. Our outcome for this effect indicates whether the firm decided to employ in the period of analysis an individual who six months earlier was only registered in *monotributo* (and not in SIPA)<sup>8</sup>.

The regressions in this case are as follows:

$$F_{it} = \alpha + \beta(\text{Treat}_i \times \text{Post}_t) + \phi_i + \gamma_t + \varepsilon_{it} \quad (4)$$

$$F_{it} = \alpha + \sum_{k \neq \text{Aug}2012} \beta_k [\text{Treat}_i \times \mathbb{I}(t = k)] + \phi_i + \gamma_t + \varepsilon_{it} \quad (5)$$

Which are similar to equations (1) and (3), but only include, considering the data restrictions of AFIP records, February 2012, August 2012, August 2015, July 2016 and November 2017.  $F_{it}$

<sup>8</sup> The reason for using a dichotomous variable at the firm level and not a continuous variable of the number of *ex-monotributistas* formally hired by firms is the low number of firms that actually hire this type of workers.

is the probability (in percentage) of becoming an employer who decided to hire an individual who was previously registered as a self-employed.

It should be noted that all estimations presented in this section are performed using an unbalanced panel of firms, that is, without requiring the firm to appear in all years of the sample (however, regressions using a balanced panel of firms are included in the robustness section).

## 4. Results

### 4.1. Employment growth rate

According to Table 2, the reform had a statistically significant impact on employment growth rates, resulting in an increase of 4.102 percentage points for small firms compared to their medium-sized counterparts. Figure 4 shows the heterogeneity of the effects within small firms, following equation (2). The results suggest that the impact of the policy was notably significant for the smallest firms (6-8 employees in March 2014), manifesting itself in a 4.1 percentage point increase in their employment growth rates. For firms with 9 to 11 employees, the impact was 2.3 percentage points, while for firms with 12 to 14 the effect increased slightly to 2.5, thus revealing significant heterogeneity between these two groups and the subset of smaller firms.

The dynamic estimation of the effects on permanent employment growth is presented in Figure 5. We find that the effect remained constant over between the third quarter of 2014 and the last quarter of 2017, suggesting that the influence of the policy persisted beyond its initial implementation. This findings goes beyond the results found in previous studies, such as that of Cahuc *et al.* (2019), by showing a more substantial effect on employment growth rates in a developing country context and in a non-recessionary scenario. Another reason why the effects identified in this study significantly outweigh those found in Cahuc *et al.* (2019) could be linked to the design of the policy itself: while the French hiring credit targeted a specific group of workers (those with lower wages), in the Argentine reform no conditionality of any kind was introduced.

Additionally, the regression was estimated using the growth rate of the average salary of the firm's new employees as the dependent variable. The results can be consulted in Table 2 and Figure A.1. However, these results should be taken with caution since they are not hourly wages.

## 4.2. Heterogeneity by previous informality of the productive sector

The results are presented in Table 3. We estimate the effect for firms from low informality sectors to be 1.86 percentage points, which is not significant at conventional levels. However, we find a statistically significant ITT of 6.2 percentage points for high informality sectors. This evidence suggests that the causal effect on the growth of permanent formal employment is concentrated in firms from high informality sectors.

## 4.3. Characterization of new employees: hiring self-employed workers

A positive effect is observed in the number of firms hiring employees who were previously registered as self-employed. Specifically, we find a 0.4 percentage point increase in the probability of hiring a previously self-employed worker, as can be seen in Figure 6. While this could potentially be interpreted as evidence of formalization particularly the transition from the *monotributo* regime (a system commonly used in Argentina to reduce labor costs) to registered employment we cannot rule out the alternative explanation that treated firms are simply more likely to hire self-employed workers.

## 5. Robustness tests

In this section we analyze the robustness of our results. The presence of pre-trends has already been tested with the specification (3) estimated in the previous section. Next, two further robustness tests are carried out to validate the result found for employment growth.

### 5.1. Bandwidth change (dynamic)

As a robustness check, equation (3) is re-estimated restricting the sample to firms between 10 and 20 employees in 2014. Figure A.2 in the appendix illustrates that the results are substantially unchanged from those found in Section 4.

### 5.2. Bandwidth change (static)

Equation (2) is also re-estimated changing the upper bound of the control group by one permanent employee from 20 to 30. Figure A.3 shows that the results are essentially the same regardless of the upper bound for the control group.

## 6. Conclusions

Our study provides a comprehensive assessment of the effects of a policy of temporary hiring credits in Argentina, highlighting its impact on different employment outcomes. The introduction of this policy was intended to encourage formal employment, especially in the context of a high rate of informality in the Argentine labor market. Through an analysis using a difference-in-differences approach, convincing evidence is found in favour of the effectiveness of the policy in promoting formal employment growth of smaller firms. The significant impact, reflected in a 4.1 percentage point increase in employment growth rates of small firms, persists for several years after the reform. Moreover, our results highlight the differential effect of the policy depending on firm size: smaller firms experience a more pronounced impact in terms of employment growth rates.

The reform and its effects hold a close relationship with labor informality. The employment growth effect found is mainly driven by highly informal sectors, increasing the employment growth rate by around 6.2 percentage points, while no significant effect can be found for the formal sectors. This gives us a reason to believe that formalization could have been an important issue around the decision to take the credit, something particularly relevant in the context of the high levels of informality prevailing in Latin American countries.

Overall, our research contributes crucial insights to the current debate on hiring credits and their effectiveness in promoting formal labor market growth, especially in contexts characterized by high informality. By addressing the lack of empirical evidence on the impact of these policies, especially for developing countries and in non-recessionary settings, this work provides valuable information for policy makers seeking targeted interventions to foster job creation.

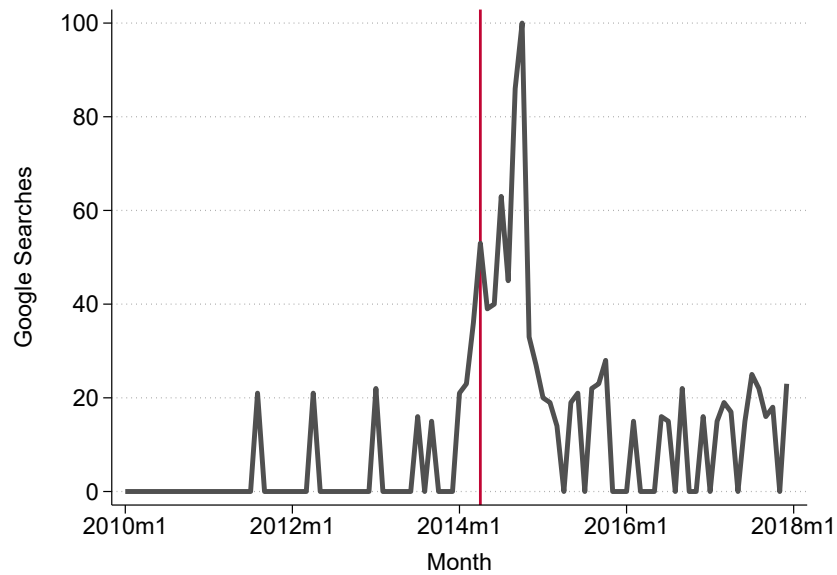
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## 7. Graphs and Tables

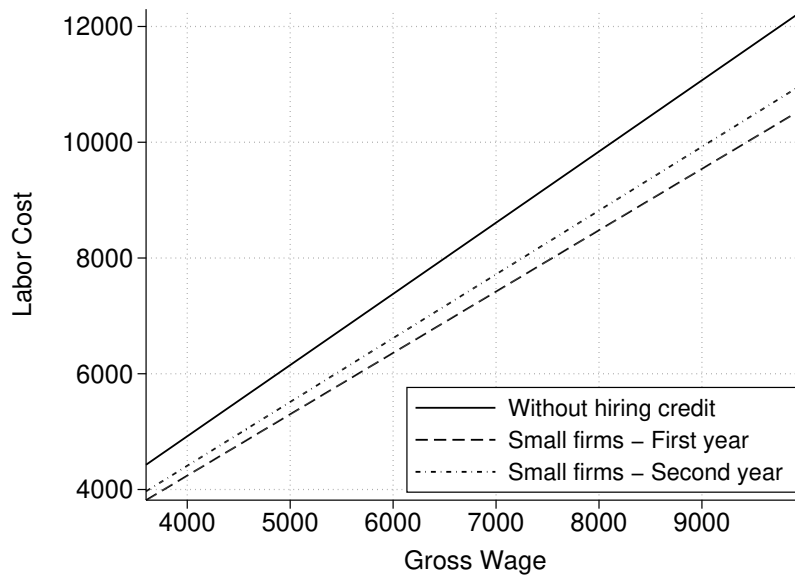
### 7.1. Graphs

Figure 1: Google searches (2010-2017): “reduccion contribuciones”.



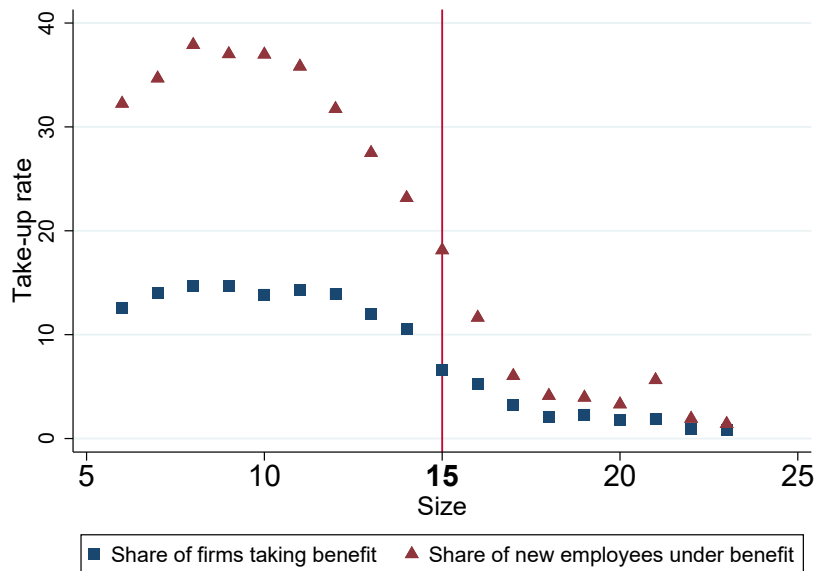
Notes: The figure reports an index reflecting the intensity of Google searches for the phrase “reduccion contribuciones” (reduction of payroll taxes) during the analysis period. The date of the announcement is the date marked by the vertical line.

Figure 2: Incidence of the hiring credit on labor costs



Notes: The horizontal axis reports the monthly salary (in pesos) net of employer social contributions of a permanent full-time employee. The vertical axis reports the monthly labor cost. The estimates are made assuming an amount of employer contributions of 23%, the "base" amount according to the law (we do not take into account other types of specific reductions linked to, for example, the sector of activity). The minimum wage in Argentina in 2015 was ARS 4,716.

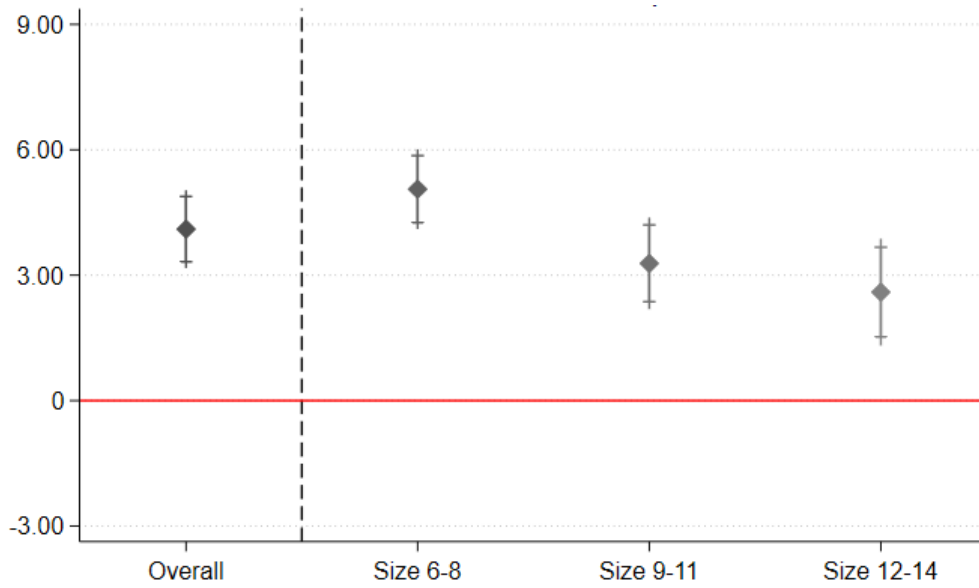
Figure 3: Take-up rate of hiring credits by firm size



Notes: The horizontal axis shows the firm size in March 2014. The left axis reports the share of firms that decided to use the hiring subsidy at least once in the period 2014-2017 (i.e. hired at least one employee under the modality) and the share of employees hired by the hiring credit, among those hired after the reform. The vertical line indicates the legal cut-off for claiming the benefit.



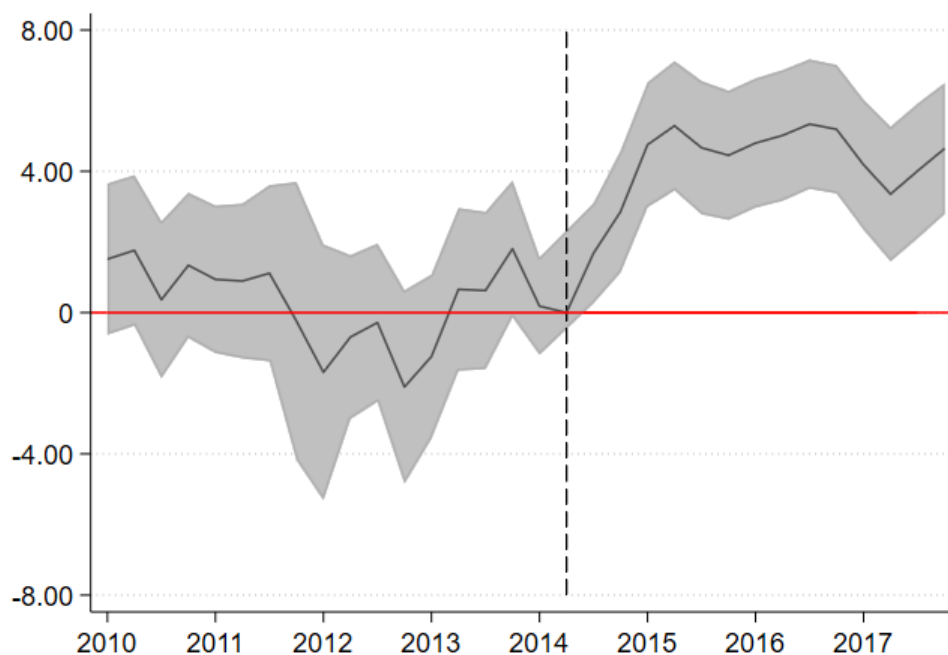
Figure 4: Heterogeneous effect on employment growth rate



- [6-8] coefficients different from [9-11] and [12-14] (at the 1% level).
- [9-11] and [12-14] coefficients not statistically different.

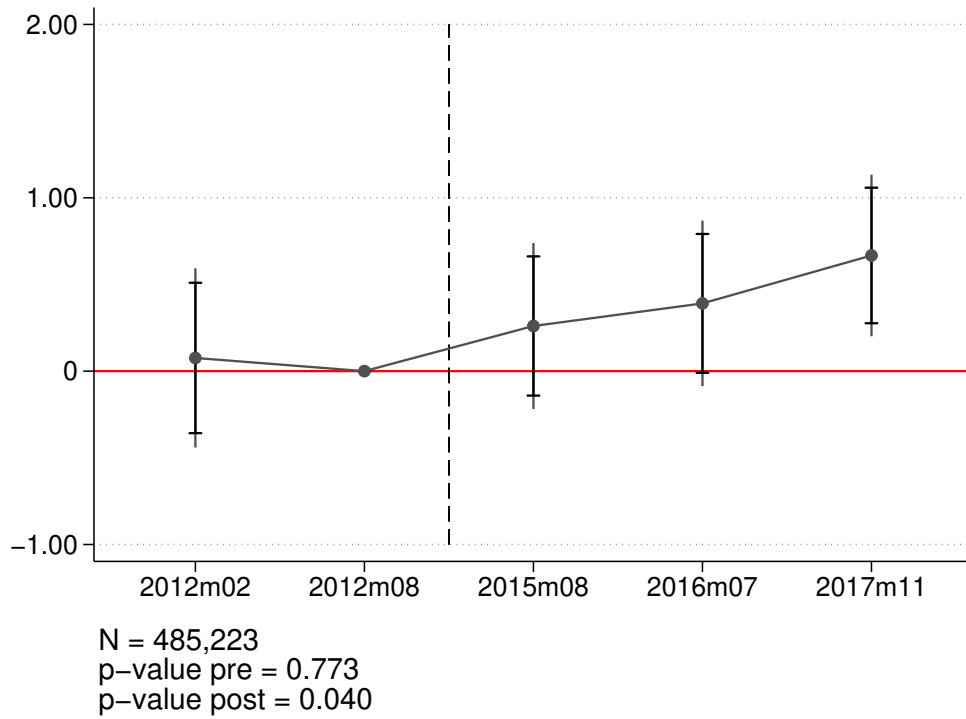
Notes: The horizontal axis shows the estimation of the coefficient  $\beta$  of equation 1 and the coefficients  $\beta_1$ ,  $\beta_2$ , and  $\beta_3$  of equation 2, using the employment growth rate as the dependent variable. All estimates are made using an unbalanced panel. Standard errors clustered at the firm level. Individual and time fixed effects included. Confidence intervals of 90% (shorter line) and 95% (longer line) are presented.

Figure 5: Dynamic effect on employment growth rate



Notes: The horizontal axis shows the analysis year, while the vertical axis reports the coefficients of the dynamic DiD estimations of equation 3 using employment growth rate as the dependent variable. The omitted period is July 2014 (immediately preceding the implementation of the reform). All estimates are made using an unbalanced panel. Standard errors clustered at the firm level. Individual and time fixed effects are included. Confidence intervals of 95% are presented.

Figure 6: Dynamic effect on the probability of formalization



Notes: The horizontal axis shows the analysis year, while the vertical axis reports the coefficients of the dynamic DiD estimations of equation 5 using the formalization dummy variable (multiplied by 100) as the dependent variable. The omitted period is August 2012. All estimates are made using an unbalanced panel. Standard errors clustered at the firm level. Individual and time fixed effects are included. Confidence intervals of 90% (shorter line) and 95% (longer line) are presented.

## 7.2. Tables

Table 1: *Hiring credits* for new permanent employees, before and after the reform

	Pre-reform		Post-reform	
	Year 1	Year 2	Year 1	Year 2
Up to 15 permanent employees (small firms)	50%	25%	100%	75%
16-80 permanent employees (medium-sized firms)	50%	25%	50%	50%
More than 80 permanent employees	50%	25%	0%	0%

Table 2: Estimated effects with static models

	Outcome variables	
	Employment growth rate	New employees' wages growth rate
Treat x Post	4.102*** (0.475)	-1.386 (2.306)
Firm fixed effects	Yes	Yes
Time fixed effects	Yes	Yes
Control Mean	12.63	56.77
	Employment level	New employees' wages
Treatment Mean	8.796	10538.6
Control Mean	20.108	13184.9

\*\*\*Statistically significant at the 99%, \*\*95% and \*90% levels. Standard errors clustered at the firm level in parenthesis.

Notes: \*\*\*Statistically significant at 99%, \*\*at 95%, \*at 90%. Robust standard errors in parentheses. All estimates are made using an unbalanced panel. Standard errors clustered at the firm level. Individual and time fixed effects included. The formalizing signature *dummy* is multiplied by 100 to facilitate the interpretation of the results.

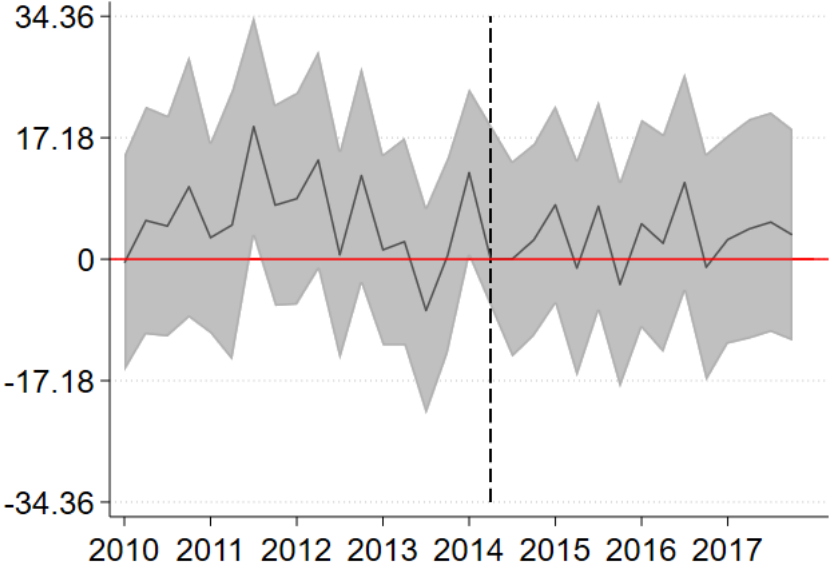
Table 3: Estimated effects with static models - Heterogeneity by informality

Variables	Employment growth rate	
	Low informality	High informality
<i>Treat * Post</i>	1.862 (0.695)	6.181*** (0.651)
Treatment Mean (Pre)	14.34	17.14
Observations	3,869,726	4,091,134

Notes: \*\*\*Statistically significant at 99%, \*\*at 95%, \*at 90%. Robust standard errors in parentheses. All estimates are made using an unbalanced panel. Standard errors clustered at the firm level. Individual and time fixed effects included.

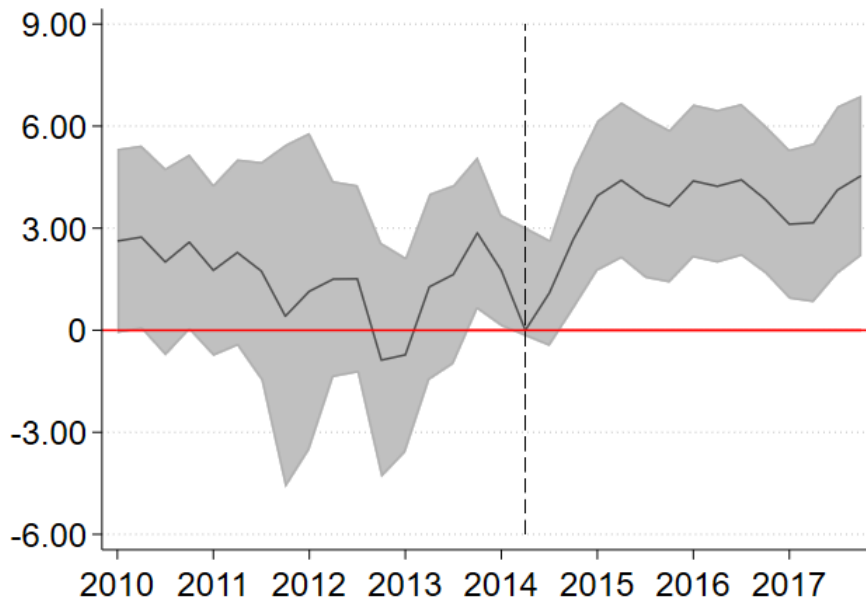
# A. Appendix

Figure A.1: Dynamic effect on the growth rate of average wages for new employees



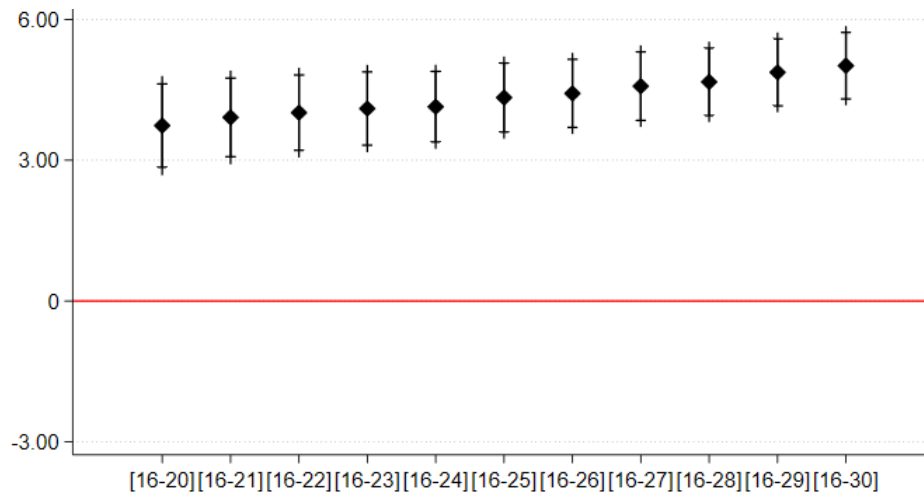
Notes: The horizontal axis shows the analysis year, while the vertical axis reports the coefficients of the dynamic DiD estimations of equation 3 using the growth rate of average wages for new employees as the dependent variable. The omitted period is July 2014 (immediately preceding the implementation of the reform). All estimates are made using an unbalanced panel. Standard errors clustered at the firm level. Individual and time fixed effects are included. Confidence intervals of 90% (shorter line) and 95% (longer line) are presented.

Figure A.2: Robustness test: bandwidth change (dynamic)



Notes: The horizontal axis shows the analysis year, while the vertical axis reports the coefficients of the dynamic DiD estimations of equation 3 using employment growth rate as the dependent variable. Only firms with between 10 and 20 permanent employees in March 2014 are included in the analysis. The omitted period is July 2014 (immediately preceding the implementation of the reform). All estimates are made using an unbalanced panel. Standard errors clustered at the firm level. Individual and time fixed effects are included. Confidence intervals of 95% are presented.

Figure A.3: Robustness test: bandwidth change (static)



Notes: The horizontal axis shows the bandwidth of the control group, while the vertical axis reports the coefficients of the static DiD estimations of equation 1 using employment growth rate as the dependent variable. Only firms with between 10 and 20 permanent employees in March 2014 are included in the analysis. The omitted period is July 2014 (immediately preceding the implementation of the reform). All estimates are made using an unbalanced panel. Standard errors clustered at the firm level. Individual and time fixed effects are included. Confidence intervals of 95% are presented.