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Abstract

This paper uses data from Spanish manufacturing firms to analyze the effect of a labor market reform on firms' innovation, growth and exporting. The reform provided additional flexibility to firms with less than 50 employees by enabling them to hire workers on a permanent basis with an extended trial period, and thus effectively reducing their firing costs. Exploiting this natural experiment in a difference-in-differences framework, we find that the reform increased the product innovations of the affected firms. We also provide evidence that the reform induced upgrading of product quality and enabled firms to grow faster and to enter new markets. The effects are concentrated in industries where flexible adjustment to unexpected shocks is likely to be important like industries with high R&D intensity and high levels of volatility. Our results suggest that a reduction of employment protection legislation (EPL) increases innovation in firms operating in environments that require high flexibility to produce because the reduction of EPL decreases labor adjustment costs.

Keywords: Innovation, New products, Productivity, Labor market reform, EPL

JEL Codes:D22, J3, O31, G31

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1 Introduction

The importance of innovation within theories of long run growth and development has generated considerable interest among economists and policy makers in understanding how innovation is affected by various features of the policy environment.[1] Traditionally, the focus of innovation policy has been on the role of intellectual property right protection, along with various tax and R&D subsidy schemes (Bransstetter, Fisman, and Foley, 2006; Hall and Rosenberg, 2010; Bloom, Van Reenen, and Williams, 2019). More recently, it has been recognized that as innovation investments are risky, these decisions are also sensitive to the adjustment costs incurred when altering the scale of production. Under such circumstances, changes in policies affecting these adjustment costs, such as those around employment, can increase firms' incentive to invest in innovation (Aghion, Bergeaud, and Van Reenen, 2019; Acharya, Baghai, and Subramanian, 2014; Griffith and Macartney, 2014; Saint-Paul, 1997).

Employment protection legislation (EPL) are key policy instruments for governments prescribed to increase labor flexibility and influence workers' welfare and living standards.[2] They have also been shown to affect innovation. Theoretical research suggests that if the demand for new products is uncertain and there are firing costs, firms may be less willing to take the risk of introducing innovative products to the market, if they are unable to downsize their labor force quickly if the demand of the new products is lower than expected. (Saint-Paul, 1997; Samariego, 2006; Bartelsman, Gautier, and De Wind, 2016; Mukoyama and Osotimehin, 2019). Therefore, in the presence of firing costs and uncertain demand, firms might be reluctant to innovate because they want to avoid the cost of discharging workers in the future.[3]

An alternative possibility is that job security increases workers' productivity and thus firm productivity and the returns of innovation. For example, there is evidence that suggests that workers increase their training and investments in firm-specific skills when there is high EPL (Kahn, 2007; Belot, Boone, and van Ours, 2007; Boeri, Garibaldi, and Moen, 2017). Consequently, EPL and in particular firing costs, by rising job protection might increase firm productivity and thereby firms' incentives to innovate.

[1] See among others Romer (1990), Grossman and Helpman (1991), Aghion and Howitt (1992), Hall (2007).

[2] EPLs are a multi-faceted set of regulations that seek to alter the relative balance of power between firms and workers within the labor market and help to fix real, or perceived market imperfections.

[3] According to Saint-Paul (1997) "to avoid paying the firing costs, the country with a rigid labor market will tend to produce goods with a relatively stable demand, at a late stage of their product life cycle, such as refrigerators".

In this paper, we empirically investigate whether a change in employment protection legislation (EPL) increases firms' incentive to invest in innovation. This contributes to our understanding of the policy determinants of innovation by providing evidence on the innovation effects of a change in EPL — a decline in firing costs— targeted at small firms. Small changes in EPL can often represent large changes in adjustment costs for small firms, raising the possibility that these firms will respond particularly strongly. In addition, the motivation for innovation can differ in important ways for small, compared to the more commonly studied large firms. This is important because as Akcigit and Kerr (2018) show small firms are more likely to undertake exploration R&D in order to develop new products, whereas large firms are more likely to conduct exploitation R&D that seeks to improve the product lines they already serve. Therefore, understanding policies that influence innovation for small firms is particularly relevant to induce long-term economic growth.

Our study is distinct in that we provide, to our knowledge, the first causal evidence of links between an EPL reform and innovation at the firm level. To capture the causal effect of changes in EPL on new product innovations, we leverage a period of EPL liberalization in Spain that included different provisions at specific firm size thresholds. This reform allowed firms with fewer than 50 employees to hire workers on permanent contracts under an extended trial period of up to one year, compared to the pre-reform period of two months in the case of unskilled-workers and six months for skill-workers.[4] This new contract increased flexibility in employment by reducing firing costs for the treated firms. That the reform encouraged the employment of high skilled workers may suggest a strong response to innovation outcomes. We exploit these differences in the size threshold of the reform in a difference-in-differences regression, using the pre-treatment size of the firm to identify treated firms allowing us to capture intention-to-treat (ITT) effects.[5] To estimate local average treatment effects (LATE), we also conduct instrumental variable (IV) regressions. In order to identify the mechanism behind the relationship between EPL and innovation, we study the heterogeneity in the outcome across firms according to their industry. By doing so, we test the idea that new product innovations are more sensitive to EPL reforms in uncertain environments where flexibility is very important.

The main finding of the paper is that a reduction in EPL encourages new product innovations

[4] Gamberoni, Gradeva, and Weber (2016) analyze employment effects of this liberalization period. However, they focus on a different aspect of the reform; employment subsidies.

[5] Reform-specific size thresholds are often exploited within a regression discontinuity design (RDD). However, in our application firms just below the size threshold are unlikely to benefit significantly from the reform. For instance, a firm with 49 employees would only be able to hire one additional worker with the new type of contract.

by small and medium-sized firms. Our estimates of intention-to-treat effect indicate an increase in the number of product innovations between 0.25 and 0.3 per year amongst treated firms (defined by pre-reform size) relative to a control group in the post-reform period. These effects are economically important as the average rate of product innovation per year is about 0.7. These results are robust to the addition of controls, including allowing for different exposures to geographic and sectoral shocks across treatment and control group.

Our difference-in-differences (DiD) analysis allows us to detect the negative average effect of EPL on innovation. In order to identify the mechanism behind this relationship, particularly whether it is due to the increase in firm flexibility owing to the ability to adjust employment, we explore treatment effect heterogeneity. We find that the effects of the labor market reform are concentrated in R&D intensive industries. We also find that the effects are most pronounced in industries with high demand volatility, suggesting that the reform induced innovation due to an increase in flexibility and a reduction in adjustment costs, rather than for example through the improved screening of workers. We also find that these effects are strong for firms that did not employ many temporary workers, indicating that skill levels also explains part of the effectiveness of this particular EPL reform on production innovation. Further investigation of the data indicates that this EPL reform also encouraged investment in capital equipment to produce new products and upgrading of product quality. We find no evidence of a significant effect on physical TFP, indicating that the capital investment induced by the reform has been directed towards introducing new products and improving existing products rather than reducing production costs. Moreover, we find that treated firms grow faster and enter new international markets. All these evidence suggest that the reform reduced firms' adjustment costs, which induced firms to innovate.

Our study relates to the literature on labor market institutions and economic performance (Freeman (2005)). There is large empirical literature on the effects of EPL on employment.[6] This literature suggests that EPL reduces job fluidity although the specific effects from the reforms largely depend on type of workers and the specific EPL reform. Another strand of the literature studies the effects of EPL on capital investments. For instance, Cingano, Leonardi, Messina, and Pica (2010) finds that firms' adjustments to EPL reforms depend on credit constraints. Some studies have focussed on the effect of EPL on labor productivity. For example, Autor, Kerr, and Kugler (2007), using the adoption of wrongful-discharge protection by state courts in the US, find that there is a

[6]For a summary of this literature see Boeri, Cahuc, and Zylberberg (2015).

negative relationship between EPL and total factor productivity (TFP). Similarly, Bassanini, Nunziata, and Venn (2009) find a negative effect at the industry level of EPL on TFP for OECD countries. More recently, Bjuggren (2018), who study the effect of a Swedish EPL reform that differentially affected firms of different sizes and increased firm flexibility, lead to a raise on labor productivity. In addition to our focus on innovation outcomes, our contribution to this literature is through the ability to separate any changes that occured to prices and physical quantities allowing us to study physical TFP for example. The increase in prices and costs of materials that we find after the reform supports models that relate increases in quality and within firm effects after a reduction in firing costs (Mukoyama and Osotimehin, 2019; Da-Rocha, Restuccia, and Tavares, 2019).

We also contribute to the literature on the innovation effects of EPL and labor regulations. Aghion, Bergeaud, and Van Reenen (2019) analyze how size thresholds related to employment regulations affect innovation incentives during periods of demand volatility. They focus on how such a threshold reduces incentives for firms to innovate since firms are reluctant to grow above a size threshold where labor laws become stricter. In contrast, our paper analyzes how changes in employment protection affect innovation and we exploit the size threshold to identify the effect of changes in EPL on innovation via adjustment costs.

Using industry-level data across countries and within the US, Acharya, Baghai, and Subramanian (2014) find that stronger dismissal laws had a positive impact on innovation intensity and led to relatively more innovation in innovation-intensive industries. Barbosa and Faria (2011) finds for a broader measure of EPL the opposite effect, again using country-industry data. The use of country or industry level data to answer this question has the disadvantage that it is difficult to account for unobservable characteristics of countries, industries and firms that determine innovation, undermining claims of causation (Griffith and Macartney, 2014).

To deal with these endogeneity problems, a more recent approach has been to turn to the use of firm level data. Pioneering work here includes Griffith and Macartney (2014) and Acharya, Baghai, and Subramanian (2014) who focus on innovation by large firms. Griffith and Macartney (2014) explore the relationship between EPL and innovation behavior within subsidiaries of multinationals (MNEs) spread across different countries. They argue that the relationship between EPL and innovation depends on whether innovation is radical or incremental. Their results indicate that MNEs locate more innovative activity in countries with high EPL, but more radical innovation activity in countries with low EPL. That their measure of EPL also has no time variation that can be exploited for identification compares to our paper and with Acharya, Baghai, and Subramanian (2014), who focus on EPL within a single country. Exploiting variation in the adoption of wrongful dismissal legislation across US states along with data on the granting of patents and citations, Acharya, Baghai, and Subramanian (2014) find that tougher legislation led to increase innovation compared to other large firms in states when this legislation had not been enacted. A disadvantage of their approach is that the strongest effects occur from differences in the timing of EPL provisions by 13 states over a 30 year period. Our contribution relative to this literature is that we provide evidence on a particular form of EPL, that on probation periods and that we are able to study the effects of an unanticipated policy change that occurs in just a single year. Our study has therefore the advantage that it is the first to combine changes in innovation with an EPL reform for a large sample of firms. This enables us to provide new, compelling evidence on the relationship between innovation and EPL.

The rest of the paper is organized as follows. In section 2, we describe the Spanish labor market and the reforms that occurred in 2012. Section 3 describes the data and the empirical strategy that we use in the paper. We describe the empirical results in Section 4 and we perform a large battery of robustness checks. In section 5, we study whether the main channel that drives our results is the increase flexibility in the labor market. In Section 6, we show the effect of the reform for alternative outcome variables. Section 7 concludes.

2 Institutional background and labor market reform

2.1 The Spanish labor market and its dismissal costs

Before turning to the details of the labor market reform, we summarize the main features of the Spanish labor market that are relevant for our study. On average, the costs of dismissal in Spain are high (OECD, 2013a). For example, an employee with 20 years of tenure in his job would receive 30 months of wages in case of unfair dismissal (12 monthly wages in case of fair dismissal).[7] This compares with 13.7 months of wages in a case of unfair dismissal for an employee with the same

[7]For contracts signed before 12 February 2012, contracts have a severance payment of 45 days of salary per year of job tenure with a maximum of 42 months in case of unfair dismissal. After that date, the severance payment for contracts under unfair dismissal is equal to 33 days with a maximum of 24 months. In case of fair dismissal, the severance payment is 20 days of salary per year with a maximum of 12 months. Firms with less than 25 employees have to pay 60 percent of severance payment in case of fair dismissal. The other 40 percent is paid by a Governmental Wage Guarantee Fund (OECD, 2013b).

tenure for the average OECD country. Employees on a fixed-term contract have a severance payment which is equal to 12 days of salary per year of service at the end of his contract or when the task for which they have been hired finishes. In case of unfair dismissal, fixed term contract workers receive the same severance payment as workers with permanent contracts.[8]

An important and differential characteristic of the firing costs of the Spanish labor market is that the large majority of firms in Spain dismiss workers by declaring the dismissal as unfair. The main reason is to avoid legal costs if workers sue the firm (typically these legal costs are paid by the firm, see García-Pérez, Marinescu, and Vall Castello (2018)). Furthermore, Spanish labor courts rule in three-quarters of cases that the dismissals was unfair (Bentolila, Cahuc, Dolado, and Le Barbanchon, 2012). This implies that, before the reform, employees with one year of tenure typically would have received 45 days of salary as severance payment, independently on their type of contract. Note that very few OECD countries have any severance payment for contracts shorter than one year. In summary, dismissal costs are very high in Spain for both temporary and fixed-term contracts as compared to other OECD countries.

2.2 The reform

The Great Recession beginning in 2008, soon followed by the Spanish sovereign debt crisis in 2010, affected the Spanish economy badly. In 2010, GDP per capita fell by 4.9% while the average unemployment rate rose to 20.1% (for the young population this rate was 41.5%). As a response to these economic woes, the Spanish Government approved an unexpected and deep labor reform in February 2012, with the intention to reduce the rate of job destruction and to generate employment. The economic logic behind the reform was to increase the internal flexibility of employment within firms so that they could adjust to the recession (Bentolila, Cahuc, Dolado, and Le Barbanchon, 2012). There is a general consensus that the details of the reform were not anticipated by firms (OECD, 2013a). The reform occurred as the result of a change in government in November 2011 and was not discussed during the political campaign. The reform was instead first mentioned in the inaugural address of the Prime Minister at the end of December 2011.

[8]The Spanish labor market has two main types of contracts: permanent and fixed-term contracts. In 2010, the first year of our main estimation sample, the share of fixed-term contracts was 24.7 percent. This large number is partly due to the large size of the service sector and in particular the importance of the tourist industry in Spain, which is very seasonal (Source: Spanish National Institute of Statistics). The share of fixed-term contracts is much smaller for the manufacturing sector. For example, in our representative sample of the manufacturing sector, the share of fixed-term workers is 9.4 percent for the year 2010.

The key element of the reform for our study was the creation of a new type of contract called *contrato de emprendedores*. This contract was introduced in July 2012 for firms with less than 50 employees and with no unfair or collective dismissals actions in the preceding 6 months. The new contract allowed firms to hire workers on permanent contracts with a trial period of one year. This extended the pre-reform trial period of two months in the case of unskilled-workers and six months in the case of skilled workers.[9] Note that firms could only hire workers using the new type of contract until they reached the size threshold of 50, even if their pre-reform size was below that threshold. The reform created the longest trial period within civil-law OECD countries (OECD, 2013b).[10]

The new contract increased employment flexibility as it could be used as a one-year contract without severance payments following dismissal. It also provided companies with an annual subsidy of 1100 euros on average per worker over a period of three years.[11] The usage of the contract was limited in the first year. Governmental statistics report that, for firms with less than 50 employees in the manufacturing sector, "contrato de emprendedores" represented 2.1% of all new contracts of 2012.[12] From 2012 to 2015, it represented 15.6% of all new fixed-term contracts. By occupation, 20% of the contracts were for scientists and high-skilled technicians, 33% of the contracts were for skilled-construction and production occupations, 31% for machinery operators and low-skilled production occupations, and the rest of the contracts were for other occupations (such as administrative staff). The contract was used uninterruptedly from July 2012 to January 2019, until the unemployment rate fell below 15%.

Other elements of the 2012 reform applied to all firms as they included no specific size thresholds. These changes included the following: a) decentralization of collective bargaining agreements;[13] b) new definition of the causes for fair dismissal; and c) a reduction of severance payment in case of

[9]There are two exceptions to the probation time explained above. The first exception is for companies with less than 25 employees that have a probation time of three months. The second exception is for temporary contracts with a duration of less than six months that have a probation time of one month (unless specified in the collective bargaining agreement).

[10]Civil-law OECD countries include those with French civil-law: Belgium, France, Greece, Italy, Luxemburg, Mexico, the Netherlands, Portugal, Spain, Turkey; countries with German civil-law: Austria, the Czech Republic, Estonia, Germany, Hungary, Japan, Korea, Poland, the Slovak Republic, Slovenia, and Switzerland; and countries with Nordic civil-law: Denmark, Finland, Iceland, Norway, and Sweden (OECD, 2013c).

[11]Gamberoni, Gradeva, and Weber (2016) analyze the impact of this subsidy on employment growth but they do not find any significant effects.

[12]This information is available at https://www.sepe.es/HomeSepe/que-es-el-sepe/estadisticas/contratos/emprendedores.html and at https://www.sepe.es/HomeSepe/que-es-el-sepe/estadisticas/contratos/estadisticas-nuevas.html

[13]The reform gave priority to the collective bargain agreements at the firm level over those at the industry or regional level. In addition, it also made easier for firms to opt-out of higher-order agreements. However, the use of this opt-out option was low in the recent post-reform period. For example, Izquierdo and Jimeno (2015) report that only 3.4% of firms opt-out from collective agreements in 2013.

unfair dismissal for permanent contracts, with the intention of decreasing the EPL gap between permanent and fixed-term contracts.

3 Data, variables and empirical strategy

3.1 The data and the main variables

In this section, we describe the dataset and the main variables that we use for our empirical analysis. Further details are in the following sections and in Table 1 where we present descriptive statistics and definitions of the main variables by treatment status. The data we use is from the Encuesta Sobre Estrategias Empresariales (ESEE).[14] This dataset, funded by the Spanish Ministry of Industry, is a representative survey of Spanish firms in the manufacturing sector. Our sample is an unbalanced panel of 1,766 firms from 2010 to 2015, with an average of five observations for each firm. In the survey, firms provide information on sales, number of employees, changes in prices of inputs and final goods and information on innovation outputs along with information on other indicators of product upgrading.

Our principal measure of innovation output is the number of product innovations that a company has obtained in a given year. Product innovations are defined in the survey as: *"Completely new products, or with such modifications that they are different from those produced earlier."* The number of new products as measure of innovation output has been previously used in the innovation literature (e.g., Guadalupe and Wulf, 2010; Raymond, Mohnen, Palm, and Van Der Loeff, 2010; Harrison, Jaumandreu, Mairesse, and Peters, 2014; Fernandes and Paunov, 2012). Guadalupe, Kuzmina, and Thomas (2012) also consider that product innovations per year can be interpreted as the change in a firm's product innovation stock. In our context, product innovations also link with the theory of Saint-Paul (1997), where product innovations (or introduction of new goods) are likely to be sensitive to demand uncertainty and thus to firing costs. Another advantage of focussing on product innovations in our application is that they are extensively used by both small and large firms. We use the number of product innovations for our baseline specifications and as alternative outcomes, in Section 6, we also use measures of quality upgrading such as imports of technology and investments

[14] Details on EESE dataset and data access guidelines can be obtained at: http://www.fundacionsepi.es/investigacion/esee/en/spresentacion.asp. Recent papers using this dataset include Doraszelski and Jaumandreu (2013), Doraszelski and Jaumandreu (2018) and Guadalupe and Wulf (2010) among others.

in capital for product improvement.

On average, firms introduce an average of 0.72 new products a year over the sample period. On average small tend to innovate less than larger firms. For firms with fewer than 50 employees, the average number of product innovations for the sample period is equal to 0.32 per year, while larger firms introduce an average of 1.06 new products per year. In the pre-reform period (2010 and 2011), the number of product innovations increased by 0.86 per year and in the post-reform period (2012 to 2015) by 0.64. This suggests an overall decline in innovation after the reform which may have arisen as a consequence of the broader economic conditions in Spain during this time period.

We consider the impact of the reform for alternative variables in Section 6. Our data also provide information about other firm outputs that might face high demand uncertainty. In particular, we know the number of geographical markets where a firm sells its products and the volume of exports. We also consider traditional measures of firm outputs such as sales and quantity growth. Our data also provide information about prices for final goods and materials and we construct measures of physical total factor productivity (TFP), which we also use in our analysis. The descriptive statistics in Table 1 show that firms with fewer than 50 employees differ from larger firms along several dimensions including: lower physical capital investments, lower export share, smaller number of geographical markets, and lower productivity. We describe the construciton of physical TFP later in the paper.

Before explaining our empirical methodology and in order to confirm that the EPL reform of interest affected employment decisions, we report the difference-in-difference of means between firms with fewer than 50 employees in 2011 (the year before the reform) and larger firms before and after the reform for several employment related variables. We present these results in Table 2. In the table we include the natural logarithm of the number of employees, employment growth, the natural logarithm of the number of hours worked (and its growth) and reported overtime (and its growth). For all variables, there is a statistically significant increase in the treatment group relative to control firms after the reform. For instance, the average yearly employment growth rate of the treated firms increases by about 2.2 percent relative to unaffected firms and the working hours growth rate increases by about 1.3 percent. These numbers suggests that firms modified their hiring behaviour after the policy change.

3.2 Empirical strategy

To study the effect of a reduction on EPL on firm innovativeness we estimate a difference-in-differences (DID) model, where we take advantage of the size threshold of the reform. The differences in within-firm changes in innovation between treated and control firms can be expressed as follows:

$$y_{it} = \alpha_0 + \beta\, T_i + \theta\, T_i \times Post_t + \delta_t + \varepsilon_{it}. \tag{1}$$

In equation (1), the variable y_{it} represents the innovation outcome of firm i in period t, α_0 is a constant term, T_i is a time-invariant treatment indicator which takes value one if employment in 2011, the year before the reform, was below 50. $Post_t$ is a dummy variable that takes the value of one in all post-reform periods (2012-2015), δ_t includes a full set of time dummies and ε_{it} is an error term. Our term of interest is the interaction term $T_i \times Post_t$, with the estimated θ coefficient, which is the DiD parameter of interest and measures the intention-to-treat (ITT) effect. If the estimated θ coefficient is positive, it would indicate that a reduction in EPL (through a decline in firing costs) increases innovation. Standard errors are adjusted for clustering at the firm level. We restrict our main estimation sample to the years 2010 to 2015 to focus on a relatively narrow time window around the reform. However, we also report some additional results for earlier years.

Our identification assumption is that the timing of the reform is unrelated to the potential outcomes of the firm. We argue that this effect is identified because the introduction of "contrato de emprendedores" was not anticipated before the start of the year 2011. In the following sections, we also provide evidence in support of similar pre-reform parallel trends between treatment and control group in our main outcome variables of interest.

Since the main goal of our analysis is to investigate the effect of the labor market reform on innovation and how this effect varies across different types of firms and industries, we extend (1) to allow for firm-specific unobserved heterogeneity (α_i), and add in some specifications a vector of firm characteristics including initial size, age and size growth, as in the following equation:

$$y_{it} = \alpha_i + \theta\, T_i \times Post_t + \beta\, X_{it} + \delta_t + \varepsilon_{it} \tag{2}$$

A potential concern is that θ might be capturing differential trends between treated and control groups. Controlling for this issue is particular relevant in our case, because it is possible that the treated group, which are small firms, grew faster than the control group independently of the reform. For this reason, we conduct placebo tests for different thresholds of firm size, and in addition to present results where we restrict our sample to firms of similar pre-reform size, we include in our model firm-specific growth paths as in a correlated random trend model (e.g., Bøler, Moxnes, and Ulltveit-Moe, 2015):

$$y_{it} = \alpha_i + \theta\, T_i \times Post_t + g_i \times trend_t + \beta\, X_{it} + \delta_t + u_{it} \tag{3}$$

where $trend_t$ denotes a linear time trend. We take first differences of equation (3). This yields the following equation, which we estimate by fixed effects:

$$\Delta y_{it} = \theta\, \Delta(T_i \times Post_t) + g_i + \beta\, \Delta X_{it} + \Delta\delta_t + \Delta u_{it} \tag{4}$$

We use equation (4) to analyse changes in the product innovation stock across time. Thus, we use the number of new product innovations in a particular year as our measure of Δy_{it} in equation (4).

It might be tempting to exploit the size threshold of the reform using a regression discontinuity design. However, in our case, regression discontinuity is not a useful methodology because firms just below the size threshold benefit little from the policy change as they can only exploit the change induced by the reform until they cross the size threshold. For example, a firm with 49 employees could only hire one additional worker using a "contrato de emprendedores". However, as we discuss in the results section, our results are robust to limiting the estimation sample to firms with initial size that is not too far away from the threshold, i.e. firms with more than 30 and/or below 70 employees in the pre-reform year.

4 The effects of the reduction of EPL on innovation

4.1 Baseline results

In this section, we turn to the analysis of the reduction of EPL protection on innovation. Table 3 presents our core results of the effect of the reform on product innovation. Across the table, we estimate equation (4) adding different combinations of firm fixed effects, year dummies, industry-year and region-year fixed effects and additional controls that include initial size, age and size growth. The results suggest a strong and positive effect of a reduction of EPL protection on new product innovation. In all columns, the interaction coefficient is positive and statistically significant at standard levels. This implies that there is a significant increase in the number of product innovations for the treated firms in the post-reform period as compared to the control group. The estimated coefficients range from 0.26 to 0.3 per year in the different estimations. Since the average number of product innovations per year is 0.7, the effects are quantitatively important. They imply an increase in the innovation rate for the treated group by approximately 37%.

Comparing columns (1) and (2) to those in columns (3) to (6), the results are robust to controlling for firm-specific trends in the innovation stock. There is also little change in the results when we control for industry-year and region-year fixed effects in columns (2) and (4), which account for potentially different exposure to geographic and sectoral shocks across treatment and control group. In most of our regressions, we cluster standard errors at the level of the firm. Our results remain statistically significant for the alternatives regularly used for DiD analyses, including clustering by initial firm size, which determines treatment status, bootstrapping and collapsing the data to the treatment-group-year level (see columns 5 to 7, respectively). The latter account for potential serial correlation in the error term that may lead the standard errors to be under-estimated.

4.2 Alternative specifications and robustness checks

We next turn to establishing the robustness of these main findings taking into account the following potential biases: First, we consider pre-reform trends; second, we construct placebo tests; third, we consider pre-sample estimations; fourth, we test for potential anticipation effects; fifth, we estimate local average treatment effects (LATE).

4.2.1 Pre- and post-reform trends

The DiD results are only valid if treatment and control group have similar pre-reform trends. To test whether this is the case, we re-estimate equation (4) to estimate time-varying treatment effects for all years including pre-reform changes between 2011 and 2010 (the change between 2010 and

2009 serves as the omitted base category) as in the following specification:

$$\Delta y_{it} = \theta\,\Delta(T_i \times \delta_t) + g_i + \beta\,\Delta X_{it} + \Delta\delta_t + \Delta u_{it} \tag{5}$$

The results are summarized in Figure 1. As it is evident from the figure, in the pre-reform period and in the year the reform was implemented (the year 2012), the differences in product innovation between the treatment and control group are small and statistically insignificant as compared to the omitted category (the change between 2010 to 2009). The difference between the treatment and the control group increases and becomes statistically significant in the years subsequent to the reform. Importantly, this indicates that differences in the slope of innovation trends between treatment and control group materialize after, rather than before, the reform was implemented. These results point towards a causal interpretation of the effect from the reform on new product innovation among the treatment group.

A related concern is that the labor market reform was implemented a few years after the peak of the global financial crisis. If small firms were more affected by the crisis than large firms and in subsequent years are recovering more quickly, we might falsely attribute this recovery process to the policy change. To check whether this could explain our results, we run DiD estimates for the pre-sample period 2005 to 2009 using product innovations, sales growth and employment growth as outcome variables. Year-specific estimates depicted in Figures 2 to 4 show that there is little evidence that the growth of those small firms that form the treatment group in our main estimation sample differed from non-treatment firms during the financial crisis. Therefore, differences in the effect of and subsequent recovery from the global financial crisis do not appear to confound our results.

4.2.2 Placebo tests

The period of Spanish EPL reforms that we focus on was, of course, coincident with a period of macroeconomic instability in Spain as the Global Financial Crisis gave way to the Sovereign Debt crisis that affected many Southern European countries. A potential concern is that, due to heterogeneous responses to macroeconomic shocks, innovation in firms of different size might evolve differently in post-reform period and that it is this effect that we capture. However, if that were the case, macroeconomic shocks are unlikely to matter solely around the threshold of 50 employees and we should thus see differential changes in innovation outcomes for different size thresholds as well. Put differently, if our identification strategy is valid, we should not estimate any significant treatment effects for arbitrary size thresholds. To test whether this is the case, in Table 4, we conduct placebo regressions for deliberately false treatment thresholds of 75 (column 1), 100 (column 2) and 150 employees (column 3). In these regressions, we exclude firms with less than 50 employees in 2011. The results from this exercise indicate the expected result of no statistically significant differences between the placebo and control group after the reform for any of the arbitrarily chosen thresholds. This supports the view that our baseline results capture the effect of the EPL reform rather spurious differences in the rate of product innovation across firms of different sizes.

4.2.3 Further robustness checks

In Table 5, we show results of regressions where we exclude different subsamples of firms. Our main specification is based on all firms with available data, irrespective of their initial size. This has the advantage that we use a sample of firms independently on their past innovation success, which may have determined firm growth—and thus size—in the pre-reform period. A disadvantage of using the full sample of firms is that treatment and control group are arguably different in terms of their pre-reform characteristics. Although in previous regressions, we control for firm characteristics and unobserved heterogeneity and obtain plausible results from placebo tests, we re-run our DiD analysis keeping only firms with a rather similar size category, measured as the number of employees, in the pre-reform year 2011. We consider the following samples of firms: those with more than 30 employees (column 1); with less than 70 employees (column 2); and between 30 and 70 employees (column 3). The results documented in columns (1) to (3) are in all cases positive and statistically significant at standard levels. The estimated effects even increase slightly as compared to the baseline estimates.[15] This result suggests that our estimated effects on the number of product innovations are unlikely to be driven by unobservable time-varying firm characteristics that are correlated with initial size.

As discussed above, the package of EPL reforms was not previously announced by politicians and they were not included in the manifesto of the government prior to the election. Therefore, it was unlikely that firms anticipated the introduction of "contrato de emprendedores" and adjusted

[15]However, although the effects are statistically significant at standard levels, due to the smaller sample size, the coefficients are less precisely estimated. As mentioned before, due to the design of the reform, firms could only hire workers using the newly introduced contracts until they reached the size threshold of 50 employees. Therefore, a too narrow bandwidth around the threshold is unlikely to be informative about the effect of the reform.

their size in advance to benefit from the policy change. Nonetheless, we use column (4) in Table 5 to report the result from a DiD regression where we exclude firms that fell below the size threshold of 50 employees during the 3 years before the reform. The results from this estimation indicate that rather than attenuating the treatment effect, the estimated ITT effects increase as compared to the baseline regression. This suggests that anticipation effects are unlikely to explain our results.

The EPL reform of interest increased the flexibility of workers on permanent contracts, which are typically higher skilled than those on temporary contracts. The dual labor market within Spain already offered firms flexibility through temporary workers. To consider if the skill level was an important feature of the effects we capture, we examine if they are driven by firms that mainly employed a temporary workforce before the reform. A workforce made up of temporary workers could already be easily adjusted if the firm needs to downsize. One approach to the study of the effect of EPL reforms in a dual-labor market setting such as in Spain has been to use the pre-reform share of temporary versus permanent workers (e.g., Dolado, Ortigueira, and Stucchi, 2016). To test whether this is the case, we exclude firms with a share of temporary workers in the total workforce of 50% or more in the year before the reform. We present the results from this estimation in column (5). The estimated effects remain statistically significant at standard levels and, in fact, the estimated magnitude increases as compared to the baseline regression. This result is consistent with the limited role that temporary workers play in our sample, where the average share of temporary workers in the total firm employment for this period is below 10%.

A potential explanation for the increase in innovation by small firms after the reform, which is unrelated to labor market flexibility, is a relaxation of credit constraints due to the financial incentives associated with the policy. As a reminder, the reform also provided an employment subsidy to the treatment firms. To investigate whether this is a likely explanation, we exploit a question from the ESEE survey, which asks firms whether, in a given year, they have unsuccessfully searched for external financing of innovation. If the labor market reform induced innovation acts solely through a reduction in financing constraints, we would expect our findings so far to be driven purely by firms that previously reported these financing problems. However, the results in column (6) of Table 5, where we exclude firms that reported such problems (within the 5 years before the start of our main sample period), indicate that this is not the case. We find instead that the results are very similar to the baseline effect reported in Table 3. This result, together with the fact that only around 12% of innovating firms in both treatment and control group report external financing problems, suggest that a reduction of financial constraints is not the channel by which the reform affected innovation.[16]

Finally, a requirement of the contract was that firms had not incurred in unfair or collective dismissals in the preceding 6 months. In Spain, the minimum threshold in the case of collective dismissals for firms with less than 100 workers is 10 employees. In order to account for this issue, we drop from our sample those treated firms with a decrease of at least 10 employees in a given year (from the year before the reform). We report the results from this estimation in column (7). The results are again consistent with previous evidence showing a positive and significant effect of the reform for the treated firms on product innovation.

4.2.4 Local average treatment effects

The results presented so far measure ITT effects. To estimate local average treatment effects (LATE), we conduct instrumental variable (IV) regressions similar to Bjuggren (2018). In these regressions, we instrument actual treatment status ($T_{it} \times Post_t$) using time invariant treatment status based on pre-reform size ($T_{i,2011} \times Post_t$), i.e. the same variable used to estimate ITT effects in Table 3. The exclusion restriction for this instrument is likely to hold since, as we have discussed, the reform and the corresponding size threshold was not anticipated. Further, the previous analysis of pre-reform parallel trends indicates that firms (and therefore the instrument $T_{i,2011} \times Post_t$) were unlikely to be affected before the reform.

The results of the LATE using IV regressions are reported in Table 6. The estimated LATE parameters identify the effects on compliers, i.e. firms that have fewer than 50 employees in a specific year and can thus benefit from the reform in that year due to their initial size in 2011. Since some of the firms cross the threshold of 50 employees after the reform, LATE effects are per construction larger than ITT effects. In columns (1) and (2), we average the effect of the reform across all post-reform periods and in columns (3) to (5) we estimate interactions of treatment status (based on current employment) with year dummies for 2012-2015 to study how they vary on a year-by-year basis. The coefficient estimates in columns (1) and (2) vary between 0.35 and 0.30, depending on whether we control for year fixed effects (column 1), or firm and year fixed effects (column 2). Either way this indicates that the number of new product innovations increases for those firms that

[16] Consistent with the limited role of the employment subsidy in our sample, Gamberoni, Gradeva, and Weber (2016) show that there is little evidence that this subsidy induced employment growth.

remained in the treatment group in the post-reform period. The estimated parameters consistently indicate an increase in innovation that starts one to two years after the reform.

5 The mechanism behind the effect of the reform on innovation

Having established the robustness of our main findings from several threats to identification, in this section we investigate treatment effect heterogeneity in several industry characteristics for which labor market flexibility is likely to be of particular importance. We distinguish across two different dimensions: R&D sectoral intensity and sectoral market uncertainty.

First, we analyse heterogeneous effects across industries' R&D intensity. As previously argued by Bartelsman, Gautier, and De Wind (2016) and Akcigit and Kerr (2018) innovations are often characterized as having higher expected, but more uncertain, returns. This uncertainty is likely to increase in R&D intensive sectors due to the importance of innovations and rapid rate of technological change. Moreover, R&D intensive sectors are typically very dynamic and in need to introduce aggressively new products and scale up (or down) quickly in order to take advantage of economies of scale (Pavitt, 1984; Dosi, 1982; Jansen, Bosch, Frans, and Volberda, 2006, among others). We thus expect the reform to have larger effects on innovation incentives in these industries. For this analysis, we stratify the sample by distinguishing between firms that operate in sectors with R&D intensity (defined as total R&D expenditures over sales) above the median and below the median. Our sectoral measure of R&D intensity is the average firm-level R&D intensity across in a sector in the pre-reform year 2011 or alternatively in 2007, one year before the start of the financial crisis.

We show results from these sample splits in columns (1) to (4) of Table 7, where we distinguish between R&D intensive sectors (column 1) and non-R&D intensive sectors (column 2). The results indicate that the effects of the labor market reform are clearly concentrated in industries with pre-reform R&D intensity above the median. The estimated effect of the EPL changes for these groups of firms are larger than those in the baseline estimates in Table 3. For firms in non-R&D intensive industries, the estimated coefficients are subtantially smaller and statistically insignificant. Moreover, the standard errors in these regressions are of a similar size to those reported previously, suggesting that these near zero effects are well identified.

Along with firms that operate in R&D intensive industries, we also expect that new product innovations are affected more in industries characterized by greater market uncertainty. In industries where uncertainty is high, the reform might have had a greater impact on incentives to innovate since unforeseen events, that require adjustment of the labor force, occur with higher frequency. To investigate the role of uncertainty, we follow Czarnitzki and Toole (2011) and compute a measure of product market uncertainty at the firm-level as follows:

$$UNC_i = \frac{\sqrt{\frac{1}{T}\sum_{t=1}^{T}\left[S_{it} - \left(\frac{1}{T}\sum_{t=1}^{T} S_{it}\right)\right]^2}}{\frac{1}{T}\sum_{t=1}^{T} S_{it}}, \tag{6}$$

where S_{it} denotes sales per employee. This uncertainty measure is based on the standard deviation of sales per employee and thus measures the variability of sales per worker across years. We divide the standard deviation by the average level of sales per employee to obtain a measure that is comparable across firms of different size. Our industry-level measure of uncertainty is simply the average of the firm-level measure across all firms in an industry. To reduce endogeneity problems, we compute the uncertainty measure over pre-reform years only.

In Table 8, we show results of sample splits based on the median of uncertainty across industries. We experiment with different time periods. For the definition in columns (1) and (2), sample splits are based on all years from 1990 until 2007, in order to exclude the period of the financial crisis when uncertainty might have been exceptionally high. In columns (3) and (4), we use the years 2003 to 2007 only. While this results in a measure that is based on a much smaller number of observations per firm, it accounts for the fact that more recent periods might be of higher importance to predict future uncertainty. In columns (5) and (6), we include the time periods of the financial crisis in the calculation of the uncertainty measure. The results of all sample splits suggest that the effects on innovation are most pronounced in industries with high volatility, indicating that the reform, at least partially, induced innovation due to a reduction in adjustment costs. The results from the different data stratifications suggest that the reform encouraged firms to innovate by offering greater flexibility to scale up or down quickly in the event of successful, or unsuccessful innovation.

6 Further outcome variables

The previous sections show a robust effect of the labor market reform on the number of product innovations, which is the innovation output measure that is the most directly related to the presumed mechanism. In Table 9, we show results using alternative innovation indicators including a binary indicator for product innovation (column 1), purchases of capital goods for product improvement (column 2) and the corresponding purchasing value in logs (column 3). Further, we investigate outcome variables such as whether the firm has introduced new methods for organizing the work or organizational labor innovations (column 4), change in the number of markets where the firm operates (column 5), a dummy variable for imports of technology (column 6) and its value in logs (column 7).

All in all, the results confirm the positive effect of the reform on different innovation outcomes. We find strong significant effects on the log of investments in capital goods for product innovations in column (3), the dummy for technology imports in column (6) and the log value of technology imports in column (7). For some variables, the results are only weakly statistically significant, including the product innovation dummy in column (1), or are insignificant, see columns (2), (4) and (5). We investigate this point further in Table 10, where we repeat the analysis from Table 9, and interact treatment status with year dummies. As before, treatment status is based on employment in 2011. The results in Table 10 show that the weakly significant (or insignificant outcomes) in Table 9, can largely be explained by treatment effects that are concentrated in certain post-reform years. The results indicate that all of the innovation-related outcomes increase significantly in at least one of the post-reform periods but the timing differs across the various measures.

Our results for the innovation related variables indicate that the labor market reform induced investment by enabling firms to better adjust to uncertain events. Since export market entry can be interpreted as an investment with high sunk costs and uncertain returns, a hypothesis consistent with our previous results is that firms in the treatment group are more likely to start exporting after the reform (Lileeva and Trefler, 2010).

In Table 11, we explore the effect of the labor market reform on the incidence of exporting. Results in column (1), where we pool over all post-reform years, are positive but only weakly significant. However, as shown in column (2), where we distinguish across years, this is mainly due to time-heterogeneous treatment effects. In columns (3) and (4), we investigate treatment effect heterogeneity by industry-level uncertainty. In contrast to the sample split for product innovations, we use export sales instead of total sales to measure sales per employee, which should be more relevant for export decisions. The results indicate that the reform induced exporting in high-volatility industries but not in low-volatility industries, consistent with our results for product innovation. This result is in line with Cuñat and Melitz (2012), who find that countries with higher labor market flexibility export relatively more in high-volatility industries.

Finally, we analyse additional outcome variables such as sales revenue growth, growth in the physical quantity sold, growth in the prices of final goods, and growth in the price of materials and physical total factor productivity (TFP). The construction of the different variables that we use for this part of our analysis is as follows: The variable sales growth is measured in yearly log changes. The growth of the physical quantity index is obtained by deflating sales using a firm-specific output price deflator. The companies report information about specific price indices of outputs and material inputs, which we consider in logarithms. TFP is measured as the residual from a production function. To estimate the production function, we relate sales to materials, labor input (measured by hours worked), and the capital stock -constructed from fixed assets. To account for pricing heterogeneity across firms and industries, we deflate sales using firm-specific price deflator to obtain a measure of physical output. Further, materials are deflated using a firm-specific input price index and capital is deflated using industry-level capital prices from EU Klems. Production functions are estimated separately by 2-digit industry using the method suggested by Ackerberg, Caves, and Frazer (2015). We also account for measurement error in capital following Collard-Wexler and De Loecker (2016).

The results are presented in Table 12, where we show the average effect, and in Table 13, where we present time-heterogeneous effects. The results in both tables indicate that the reform induced sales revenue growth (column 1). As can be seen in columns 2 and 3 respectively, the rise in sales growth is mainly due to higher quantities sold and to a lesser extent due to higher prices charged. The result of higher quantities besides higher prices is consistent with quality upgrading. This is also in line with the rise of material prices, shown in column 4, under the common assumption that high quality outputs require inputs of high quality, which is reflected in input prices (Kugler and Verhoogen, 2011). We present results on TFP in column 5. The fact that physical TFP is not significantly affected indicates that investment induced by the reform has been directed towards introducing new products and improving existing products rather than reducing production costs. The overall conclusion from these estimations is that after the reform, the treated firms increase their sales and that average product quality has increased. These results support again the idea of an increase in firm innovativeness.

7 Summary and concluding remarks

In this paper, we study the effects of a reduction of EPL on innovation. This is important for understanding the links between labor market regulations and long-term economic growth. We study the effect of a labor market reform that provided additional flexibility to firms with less than 50 employees by enabling them to hire workers on a permanent basis with an extended trial period. Thereby, decreasing the effective firing costs for these firms.

The labor reform took place in Spain in the year 2012 and our analysis uses data of a representative sample of Spanish manufacturing firms for the period 2010 to 2015. We explore the natural experiment in a difference-in-differences framework and find that the reform increased product innovations, product quality, enabled firms to enter new markets (including export markets) and grow faster. We show that these effects are concentrated among firms that operate in industries with high R&D intensity and high volatility.

Taking together, our empirical results support theories which predict that a decrease in EPL through a decline in firing costs can reduce adjustment costs of employment to changes in demand, increasing the incentives to invest in innovation (Saint-Paul, 1997). More generally, our study also supports theoretical arguments that consider that a more-flexible labor market might lead to aggregate growth through firm investment in risky innovations (Saint-Paul, 1997; Samariego, 2006; Bartelsman, Gautier, and De Wind, 2016; Mukoyama and Osotimehin, 2019) and to induce comparative advantage in more volatile innovative sectors (Cuñat and Melitz, 2012).

Our results indicate that changes in EPL for small firms can have important consequences for firm innovation patters in times of economic distress. However, these effects are largely concentrated in firms that operate in environments that require high flexibility. Moreover, the innovative behaviour of large firms might be different than the one of small firms when they face a reduction in adjustment costs. As it is well-known since Williamson (1985) (chapter 6), large firms might less creative and innovative than small firms due to their high organizational bureaucracy, the routinization of their R&D investments (Baumol, 2002), and their focus on exploitation R&D (Akcigit and Kerr, 2018).

This paper also contributes to the understanding of the effects of EPL at a broader level. Our findings highlight the importance of labor market reforms to contribute to firm innovation, but also the quality of new products and firm growth. Overall, our findings suggest that when policy makers are looking for policies to promote innovation and local growth, they should also consider labor reforms that aim to reduce adjustment costs of firms operating in volatile environments.

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Tables

Table 1: Descriptive statistics

Variable	Firms with employees<50 Mean	Std. Dev.	Obs.	Firms with employees ≥50 Mean	Std. Dev.	Obs.
Number of employees	21.86	14.43	4,320	333.09	872.75	5,149
Sales (in millions of euros)	3.25	5.87	4,320	122.00	432.00	5,149
Physical capital (in logs)	13.83	1.33	4,302	16.63	1.68	5,149
Number of products innovations	0.33	1.81	4,320	1.06	3.56	5,149
Exports share	0.12	0.23	4,320	0.34	0.31	5,149
Capital goods for product innovation (in logs)	10.95	1.59	399	13.12	1.86	1,110
Organizational labor innovation	0.12	0.32	4,320	0.24	0.43	5,149
Number of markets	1.77	1.01	4,320	2.05	1.15	5,149
Imports of technology (in logs)	1.67	1.32	793	0.68	4.30	1,168
Growth of prices of final output	0.00	0.04	4,250	0.00	0.05	4,880
Growth of prices of intermediate inputs	0.03	0.05	3,984	0.02	0.06	4,608
TFP	0.05	0.94	4,053	0.13	1.22	4,657

Table 2: Difference-in-differences of means for employment measures

	Change in variable	Observations
Employees (logs)	0.139***	9469
	(0.039)	
Employees growth	0.020***	9469
	(0.007)	
Hours worked (logs)	0.125***	9439
	(0.039)	
Hours worked growth	0.013*	9428
	(0.007)	
Overtime	2.141**	9458
	(0.866)	
Overtime growth	1.803**	9452
	(0.722)	

See Table 1 for variable definitions.
Standard errors are in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Change in product innovation stock

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Post \times T$	0.295**	0.284**	0.275**	0.263**	0.275**	0.275**	0.300**
	(0.115)	(0.121)	(0.121)	(0.122)	(0.122)	(0.114)	(0.107)
Firm fixed effects	No	No	Yes	Yes	Yes	Yes	No
Year dummies	Yes	No	Yes	Yes	No	Yes	Yes
Industry- and region-year fixed effects	No	Yes	No	Yes	No	No	No
Additional controls	No	Yes	No	No	No	No	No
N	9469	8728	9469	8728	9236	9469	12

Dependent variable is the number of product innovations

Standard errors in parentheses

Standard errors clustered at the firm-level in (1)-(3) and (5)

(1) shows estimates controlling for time invariant treatment indicator and year dummies

(2) controls for initial size, size, growth, age, industry-year and region-year FE

(3), (5) controls for firm and year FE

(4) controls for firm FE, industry-year and region-year FE

(5) clusters standard errors by initial size

(6) computes bootstrapped standard errors

(7) collapses data by treatment status and year

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Placebo regressions for product innovations

	(1)	(2)	(3)
Size Threshold	75	100	150
$Post \times T75$	0.098		
	(0.224)		
$Post \times T100$		0.124	
		(0.200)	
$Post \times T150$			0.119
			(0.218)
N	5149	5149	5149

Dependent variable is the number of product innovations

All regressions contain firm fixed effects and year dummies

Standard errors in parentheses

Standard errors clustered at the firm-level

Sample includes firms with at least 50 employees in 2011

(1) Threshold: 75 employees

(2) Threshold: 100 employees

(3) Threshold: 150 employees

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Further robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Post \times T$	0.333**	0.337*	0.394*	0.304**	0.364**	0.260**	0.274**
	(0.159)	(0.204)	(0.228)	(0.124)	(0.147)	(0.132)	(0.126)
N	6135	4986	1652	8505	8718	7600	9387

Dependent variable: number of product innovations

Regressions contain firm and year fixed effects

Standard errors in parentheses

Standard errors clustered at the firm-level

(1): exclude firms from treatment group with $\leq$ 30 employees in 2011

(2): exclude firms from control group with $\geq$ 70 employees in 2011

(3): only keep firms with 30 $<$ size in 2011 $<$ 70

(4): exclude firms from treatment group with $\geq$ 50 employees between 2008-2010

(5): exclude firms with share of temporary workers of 50% or more

(6): exclude firms that reported financing constraints for innovation

(7): exclude firms with decline in employees by 10 or more from 2011

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Local average treatment effects (LATE) for product innovations from IV regressions

	(1)	(2)	(3)	(4)	(5)
$Post \times T_{it}$	0.349**	0.298**			
	(0.136)	(0.136)			
$2012 \times T_{it}$			0.105	0.115	0.086
			(0.118)	(0.126)	(0.126)
$2013 \times T_{it}$			0.374**	0.229	0.232
			(0.171)	(0.157)	(0.151)
$2014 \times T_{it}$			0.500**	0.460**	0.458**
			(0.199)	(0.194)	(0.199)
$2015 \times T_{it}$			0.495**	0.495**	0.496**
			(0.203)	(0.193)	(0.201)
N	9469	9236	9469	9236	9236

Dependent variable is the number of product innovations

Standard errors in parentheses

Standard errors clustered at the firm-level

(1) and (3) control for time invariant treatment status and year FE

(2) and (4) control for firm and year FE

(5) controls for firm FE, industry-year and region-year FE

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Heterogeneous effects, by industry-level R&D intensity

Subsample	(1) R&D intensive	(2) Non-R&D intensive	(3) R&D intensive	(4) Non-R&D intensive
$Post \times T$	0.387**	0.140	0.461**	0.067
	(0.191)	(0.159)	(0.188)	(0.155)
N	4467	5002	4611	4858

Dependent variable is the number of product innovations

All regressions contain firm fixed effects and year dummies

Standard errors in parentheses

Standard errors clustered at the firm-level

(1) firms in industries with R&D intensity above the median in 2007

(2) firms in industries with R&D intensity below the median in 2007

(3) firms in industries with R&D intensity above the median in 2011

(4) firms in industries with R&D intensity below the median in 2011

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Heterogeneous effects, industry-level volatility

Industry-level uncertainty	(1) High	(2) Low	(3) High	(4) Low	(5) High	(6) Low
$Post \times T$	0.392**	0.109	0.392**	0.109	0.351*	0.142
	(0.181)	(0.164)	(0.181)	(0.164)	(0.181)	(0.157)
N	4920	4549	4920	4549	5022	4447

Dependent variable is the number of product innovations

All regressions contain firm fixed effects and year dummies

Standard errors in parentheses

Standard errors clustered at the firm-level

(1) firms in industries with volatility above median based on years before 2008

(2) firms in industries with volatility below median based on years before 2008

(3) firms in industries with volatility above median based on years 2003-2007

(4) firms in industries with volatility below median based on years 2003-2007

(5) firms in industries with volatility above median based on all pre-reform years

(6) firms in industries with volatility below median based on all pre-reform years

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Alternative innovation indicators

	(1) Prod.inno. dummy	(2) Prod. improvement	(3) Invest prod. improvement	(4) Organizational innovation	(5) Δnumber of markets	(6) Imports technology	(7) Import exp. technology
$Post \times T$	0.025*	0.015	0.490***	0.021	0.041	0.035***	1.422**
	(0.015)	(0.016)	(0.185)	(0.016)	(0.025)	(0.008)	(0.655)
N	9469	9469	1509	9469	9469	9456	1961

All regressions contain firm fixed effects and year dummies
Standard errors in parentheses
Standard errors clustered at the firm-level
(1) Product innovation dummy
(2) Dummy capital goods for product improvement
(3) Log investment capital goods for product improvement (excluding zero values)
(4) Dummy organizational labor innovation
(5) Change in number of markets
(6) Dummy imports of technology
(7) Imports of technology, log amount (excluding zero values)
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Alternative innovation indicators, treatment effects by year

	(1) Prod.inno. dummy	(2) Prod. improvement	(3) Invest prod. improvement	(4) Organizational innovation	(5) Δnumber of markets	(6) Imports technology	(7) Import exp. technology
$2011 \times T$	0.029*	0.030	0.006	-0.012	0.049	0.006	-0.495
	(0.017)	(0.020)	(0.314)	(0.019)	(0.045)	(0.005)	(0.505)
$2012 \times T$	0.020	0.030	0.253	0.059***	0.024	0.099***	2.303***
	(0.020)	(0.022)	(0.324)	(0.022)	(0.040)	(0.012)	(0.640)
$2013 \times T$	0.033	0.025	0.359	-0.015	0.113***	0.013	-0.079
	(0.021)	(0.023)	(0.336)	(0.024)	(0.039)	(0.009)	(0.423)
$2014 \times T$	0.049**	0.029	0.590*	0.008	0.083**	0.016	0.103
	(0.023)	(0.024)	(0.339)	(0.025)	(0.039)	(0.010)	(0.461)
$2015 \times T$	0.071***	0.043*	0.856**	-0.006	0.056	0.003	0.233
	(0.023)	(0.025)	(0.353)	(0.026)	(0.040)	(0.010)	(0.388)
N	9469	9469	1509	9469	9469	9456	1961

All regressions contain firm fixed effects and year dummies
Standard errors in parentheses
Standard errors clustered at the firm-level
(1) Product innovation dummy
(2) Dummy capital goods for product improvement
(3) Log investment capital goods for product improvement (excluding zero values)
(4) Dummy organizational labor innovation
(5) Change in number of markets
(6) Dummy imports of technology
(7) Imports of technology, log amount (excluding zero values)
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Effects on Exports

Industries	(1) all	(2) all	(3) high uncertainty	(4) low uncertainty
$Post \times T$	0.015*		0.028**	-0.007
	(0.009)		(0.013)	(0.013)
$2011 \times T$		0.006		
		(0.017)		
$2012 \times T$		0.025*		
		(0.015)		
$2013 \times T$		0.000		
		(0.016)		
$2014 \times T$		0.032**		
		(0.016)		
$2015 \times T$		0.013		
		(0.015)		
N	9469	9469	5020	4449

Dependent variable is the change in export status
All regressions contain firm fixed effects and year dummies
Standard errors in parentheses
Standard errors clustered at the firm-level
(1) and (2): Full sample
(3): Industries with export sales volatility above median
(4): Industries with export sales volatility below median
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Effects on sales, prices and TFP

	(1) Sales growth	(2) Quantity growth	(3) Price growth	(4) Material price growth	(5) TFP growth
$Post \times T$	0.036***	0.027***	0.009***	0.011***	0.009
	(0.010)	(0.010)	(0.003)	(0.003)	(0.009)
N	8352	8352	8352	8352	8352

All regressions contain firm fixed effects and year dummies
Standard errors in parentheses
Standard errors clustered at the firm-level
Dependent variables:
(1): Change in sales
(2): Change in index of physical output (using firm-specific output price deflator)
(3): Change in log firm-specific output price index
(3): Change in log firm-specific material price index
(5): Change in physical TFP (using ACF algorithm and firm-specific deflators)
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 13: Effects on sales, prices and TFP

	(1) Sales growth	(2) Quantity growth	(3) Price growth	(4) Material price growth	(5) TFP growth
$2011 \times T$	-0.016	-0.016	-0.001	0.002	-0.017
	(0.016)	(0.016)	(0.003)	(0.004)	(0.021)
$2012 \times T$	0.013	0.005	0.008**	0.010**	0.003
	(0.016)	(0.016)	(0.004)	(0.005)	(0.014)
$2013 \times T$	0.021	0.013	0.007**	0.013***	-0.002
	(0.016)	(0.016)	(0.004)	(0.005)	(0.014)
$2014 \times T$	0.046***	0.037**	0.010***	0.014***	-0.000
	(0.017)	(0.017)	(0.004)	(0.005)	(0.013)
$2015 \times T$	0.038**	0.029*	0.009***	0.013***	-0.000
	(0.017)	(0.017)	(0.004)	(0.005)	(0.013)
N	8352	8352	8352	8352	8352

All regressions contain firm fixed effects and year dummies

Standard errors in parentheses

Standard errors clustered at the firm-level

Dependent variables:

(1): Change in sales

(2): Change in index of physical output (using firm-specific output price deflator)

(3): Change in log firm-specific output price index

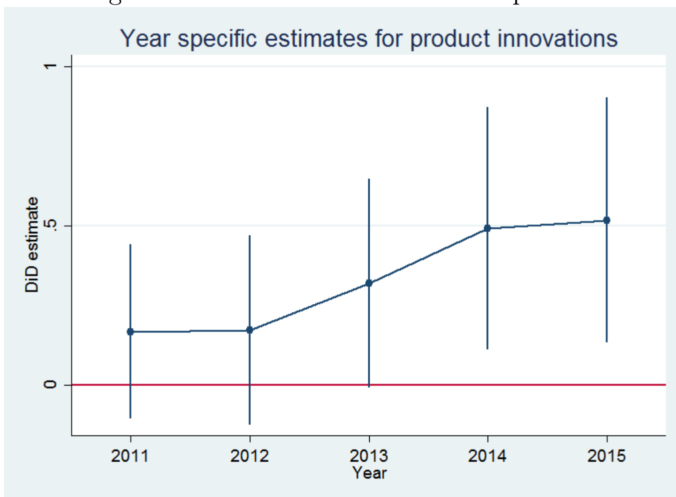
(3): Change in log firm-specific material price index

(5): Change in physical TFP (using ACF algorithm and firm-specific deflators)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

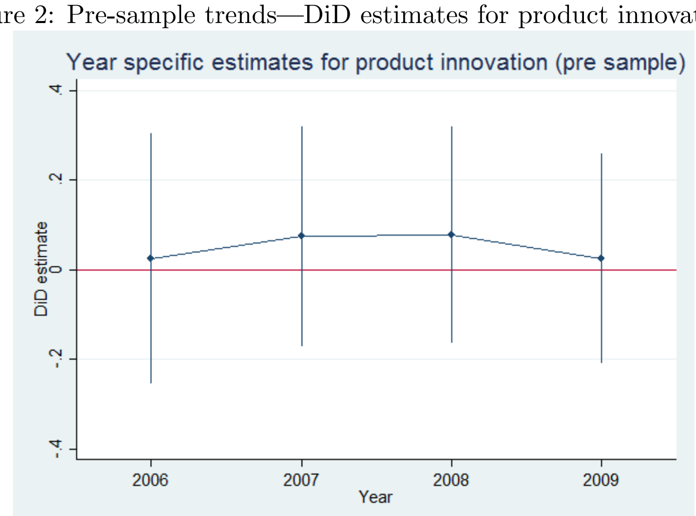
Figures

Figure 1: DiD estimates across time periods.



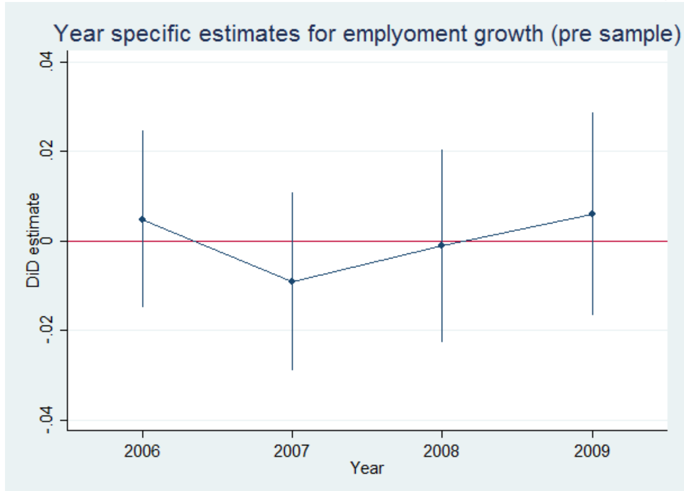
Dots denote point estimates, vertical lines indicate 95% confidence intervals.

Figure 2: Pre-sample trends—DiD estimates for product innovations.



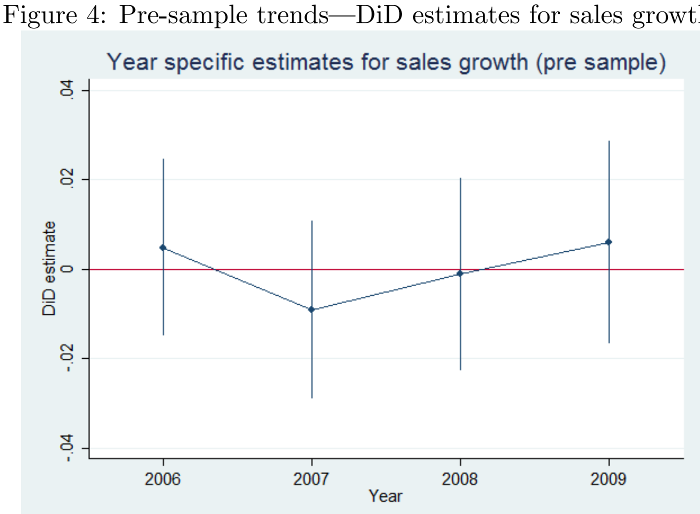
Dots denote point estimates, vertical lines indicate 95% confidence intervals.

Figure 3: Pre-sample trends—DiD estimates for employment growth.



Dots denote point estimates, vertical lines indicate 95% confidence intervals.

Figure 4: Pre-sample trends—DiD estimates for sales growth.



Dots denote point estimates, vertical lines indicate 95% confidence intervals.