“The Effect of Labor Market Rigidities on Firms’ R&D Behavior”

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Abstract

A few recent theoretical papers argue that firms that operate in flexible labor markets will be more innovative. They argue that a flexible labor market should increase a firm’s R&D spending because of the lower cost of adjusting the labor force to successful innovations. In this paper I test this hypothesis by estimating the effect on firm’s R&D of the Spanish labor market reform of 1997, which significantly reduced firing costs. I use a differences-in-differences model comparing the effects of the reform on the R&D spending of different groups of firms. I show that the firms more affected by the reform invested significantly more on R&D than less affected firms after the reform.

Keywords: R&D**,** Firing Costs, Differences-in-Differences, Innovation, Labor Reform
JEL Codes: J3, K31, L0, O3

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1. Introduction

A few recent theoretical papers argue that firms that operate in flexible labor markets will be more innovative. They argue that a flexible labor market should increase a firm's R&D spending because of the lower cost of adjusting the labor force to successful innovations. The purpose of this paper is to test this hypothesis using data on Spanish manufacturing firms.

Previous empirical studies on the relation between labor rigidities and firms' innovative activities are scarce and provide only weak support to the hypothesis of a positive link between labor flexibility and innovation effort. In addition they use cross-country variation and aggregated data in which it is difficult to control for institutional differences, which makes the causal effect difficult to identify. I use instead micro data from a single country - Spain- and find that the increased flexibility after the labor market reform of 1997 had a strong and significant positive effect on firms R&D spending.

Spain provides a good study case because it undertook a major labor market reform in 1997 that significantly reduced the costs of hiring and firing workers under permanent contracts. I take advantage of the fact that the reform affected firms differently and estimate a differences-in-differences model comparing the effects of the reform on the R&D spending of different groups of firms. Large firms benefited from the reform relative to small firms. I show that firms with more than 25 workers invested around 20 percent more on R&D after the reform. A second distinction arises from workers type, as the reform only affected permanent workers. I show that firms with a high proportion of permanent workers invested around 10 percent more on R&D after the reform compared to firms with a high rate of temporary workers. The results are robust to many different specifications and controls.

The policy implications of the results are clear. The rigidity of Spanish labor market may explain in part the low technological effort of Spanish economy and the pattern of R&D spending predominant in this country where innovations are scarce, and usually non-drastic and not patented. In addition the results provide insights on the effects of labor market reforms on firms' behavior that should be taken into account when evaluating the success of both labor and R&D policies. Finally, although the usual caveat of the of the narrow scope of identification strategies based on a natural experiment applies to my results, they may provide an explanation to the sometimes puzzling low innovative activity of European firms even in those countries with substantial R&D spending: firms might not find profitable to invest on drastic innovations because the costs of adjusting the workforce to the new product or process are too high.

The rest of the paper is organized as follows. Section 2 discusses the relevant literature. Section 3 discusses the theoretical hypothesis to be tested. Section 4 summarizes the main aspects of the labor reform in Spain. Section 5 presents the data. Section 6 discusses the empirical model. Section 7 provides the main results. Section 8 is devoted to extensions and additional robustness checks. Finally, Section 9 concludes.

2. Related literature

I now discuss the relation of this paper with the previous theoretical and empirical literature. As a summary, while a few recent theoretical models study the effect of labor market rigidities on R&D activities, there is very little empirical evidence on the topic.

2.1 Theoretical papers

From the theoretical perspective, the paper relates to three different but related research agendas. Broadly speaking the paper relates to the literature on employment and investment dynamics that shows that costs of adjusting factors may reduce long term productivity and growth. More closely, the paper relates to the literature on the effects of employment protection on productivity and innovation. Lastly, the paper also relates to the research agenda on the effects of unionization and workers' bargaining power on innovation.

The literature on investment and employment dynamics of firms shows that hiring and firing costs have a significant impact on firm's behavior. Bentolilla and Bertola (1990) present a partial equilibrium model of labor demand to study how firing costs affect labor demand. They suggest that uncertainty regarding the future plays an important role on firms' employment decisions. In an environment of high firing and hiring costs typical of European economies, hiring workers is risky. Therefore uncertainty regarding the future is a crucial aspect to take into account on firms' maximization problem (Bentolilla and Bertola (1990:398). They conclude that labor demand is more stable under high labor market protection. In their model Bentolilla and Bertola (1990) assume that investment is fixed and exogenous. Risager and Sorensen (1994) endogenize investment and study the relation between job protection and investment. They show that when product demand is very elastic to prices, hiring costs have a large negative impact on firms' employment and investment. They argue that increased costs lead to quantity adjustments and to lower profitability which leads to less investment. Dixit (1997) and Eberly and Mieghem (1997) extend Risager and Sørensen (1994) by considering more flexible ways in which investment decisions can be endogenous but still assume that technological shocks are exogenous. In particular Dixit (1997:18) concludes that unanticipated technological shocks may leave the firm with a non-optimal proportion of capital and labor. Depending on the relative costs of labor and capital adjustment, firms' strategies may differ. This could lead to firms in countries with low firing costs to adjust via labor reduction while in other countries where capital adjustment costs are similar but firing costs are high, firms may prefer to adjust reducing their investment. This could explain differences between European and American firm's adjustment patterns to unexpected shocks.

The above papers focus on the effects of labor rigidities on employment and growth but do not deal explicitly with the R&D decision of firms. Saint-Paul (2002) shows that apart from the effect on employment, labor protection affects productivity by changing the pattern of specialization of firms and their innovation strategies. He develops an open economy model to explain the effect of labor protection on the incentives to innovate. He argues that countries with rigid labor markets specialize in the production of mature goods whose demand is very stable to avoid paying the costs of adjusting the labor force. Countries with flexible labor markets specialize in new goods and only when the demand for these goods stabilizes do the countries with rigid labor market start producing them. This pattern can explain why European countries invest less on R&D compared to the U.S. and engage mainly in incremental and usually non patented innovations (e.g. small improvements on mature goods). The model could also explain why the U.S. engages in the introduction of drastic innovations that lead to new products and patents and require workforce adjustments. In the same line, Samaniego (2006) develops a dynamic general equilibrium model in which technological change leads to a greater job turnover and concludes that high firing costs may lead to less companies operating in sectors where embodied technological change is high. Similar arguments appear also in Boone (2000).

From a different perspective, the literature about the effects of unionization on innovation also concludes that labor market rigidities affect firm's innovation patterns. The basic argument is that strong union bargaining power leads to workers appropriating a greater part of the returns from the innovation, which in turn reduces firms' R&D effort. This basic conclusion is reached by Grout (1984) using a simple model including one firm negotiating with one union. More recent models like Ulph and Ulph (1994, 1998, 2001) or Haucap and Wey (2004) qualify the conclusion in Grout (1984) by looking at more sophisticated models that include strategic interactions among different firms and unions. These recent papers conclude that the hold up effect due to workers capturing some innovation rents might be counterbalanced by other effects when the strategic interactions are taken into account. Specifically, modeling innovation in the context of patent races may lead to cases in which a firm wins a patent race when unions exists and loses it otherwise (see Ulph and Ulph (1994) for details). Which effect dominates depends on the type of unionization and other assumptions of the models.

2.2 Empirical papers

The above theoretical papers conclude that labor market policies should affect firms' innovative behavior. The empirical evidence is scarce but provides some support to that conclusion. To my knowledge there are only two papers –Bassani and Ernst (2002) and Scarpetta and Tressel (2004)- that specifically study the relation between increased labor flexibility and innovation. I will also discuss three other lines of research that, although less closely related to the approach taken in this paper, also deal with the relation between labor markets and innovation.

Bassani and Ernst (2002) studies the relationship between Employment Protection Legislation and innovation using a cross-section of 18 manufacturing industries in 18 OECD countries. The dependent variable is the ratio of business performed R&D expenditures to sales and independent variables include an index of Employment Protection Legislation constructed by the OECD. They only find partial support for the claim that job protection leads to less R&D. Support for the hypothesis depends on the industrial relations regime and on the technological intensity of the industry. Scarpetta and Tressel (2004), also using aggregated data on OECD countries focus on the determinants of multifactor productivity growth. They also conclude that depending on the industrial relations regime and the technological intensity of the industry, high adjustment costs (proxied by employment protection indexes) may have a significant negative impact on productivity growth. Consistent with Dixit (1997), this result suggests that firms in different countries follow different strategies to adjust their factors to technological shocks.

Apart from these two papers three other lines of empirical research are also related to the topics addressed in this paper. Firstly, the empirical research on the impact of unions on innovation suggests that firms investment incentives decrease with union bargaining power as proposed in the theoretical model of Grout (1984) –see for example Van Reenen (1986) or Bronnars et al (1994). Second, the research on R&D management uses survey and interview data to study the obstacles that R&D managers find more important to carry innovative activities – see Delmas (2002) and Michie and Sheenan (2001). Both regulation and the lack of qualified workers appear as significant barriers for innovation. Finally, there is a growing literature on the effects of innovation on employment that shows that firms that implement innovations actually readjust their workforce afterwards[2]–see e.g. Vivarelli et al. (1996) or Garcia et al. (2002).

The evidence provided in these papers suggests that more attention should be paid to the effects of labor protection on innovation in order to fully understand both the effects of labor regulation and firms innovative behavior. I attempt to contribute to this literature by providing what appears to be the first study of the effects of a labor reform on firm's R&D behavior.

3. Testable Hypothesis

The purpose of this paper is to study the effect on firm's R&D spending of the labor reform carried in Spain in 1997. The reform significantly reduced firing and hiring costs. Its institutional details are discussed in the next section. In this section I intend to motivate, based on the theoretical papers discussed above, what are the expected effects of such a reform.

The basic argument derived from the above literature is that maximizing firms take into account adjustment costs when deciding how much effort to devote to R&D. Firms predict that a high technological effort is likely to result in innovations (new products or processes) that may require different workers' skills. The greater the costs of adapting the workforce to the new technologies, the lower the incentive to invest on R&D. This idea can be summarized by the model represented in Figures 1 and 2. Profit maximizing firms will invest on R&D until the point where an additional unit of R&D spending yields no extra gain. Or in other words the equilibrium level of R&D spending is given by the point where marginal gains equal marginal costs of R&D.

[2] For a review of the theoretical arguments on the effect of innovations on employment, see Pianta (2004). On the one hand, innovation could reduce firms' employment because the new technology replaces labor. On the other hand if the new technology leads to a demand increase, innovations can spur employment. Which effect dominates is an empirical question addressed among others in Vivarelli et al. or Garcia et al. (2002).

Figure 1 *Effects of a Decrease in Firing Costs on R&D spending*

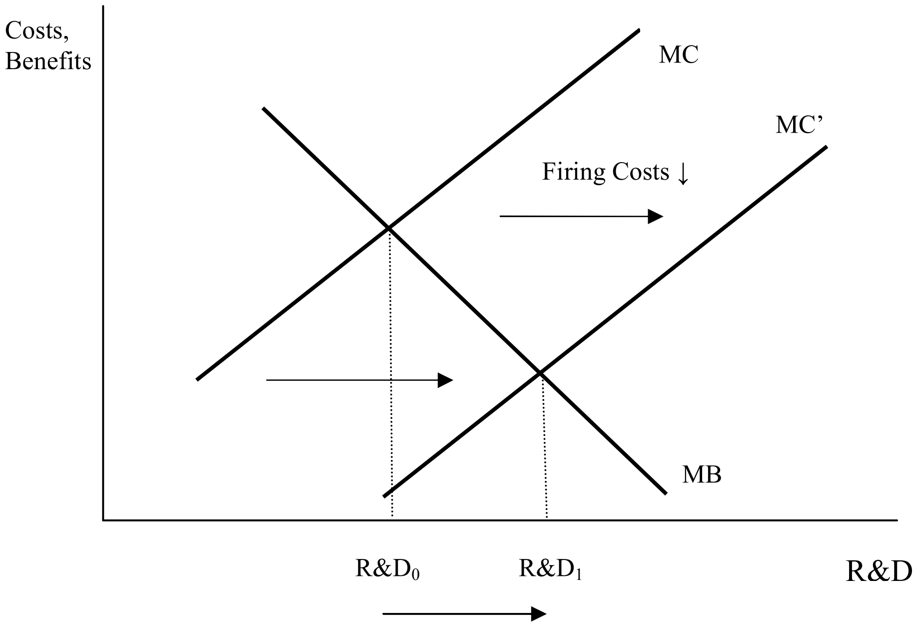
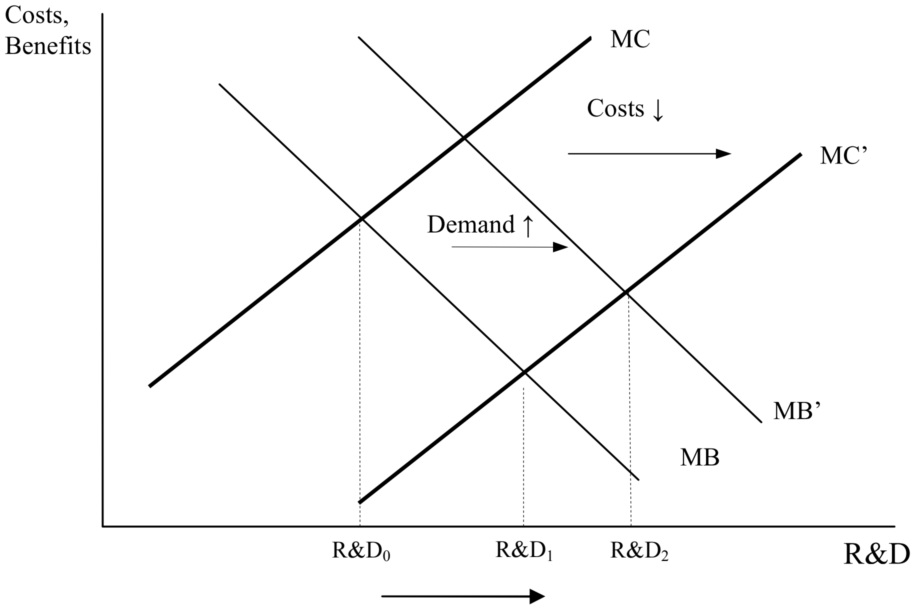


Figure 2 *Equilibrium levels of R&D spending*



The benefits derived from R&D are increasing in R&D spending because the more R&D the more the cost reduction due to process innovations or the greater the increase in sales of new products derived from product innovations. I assume that R&D projects are chosen so that benefits exhibit diminishing returns. Following the literature on the determinants of R&D spending, the marginal benefit curve shifts with demand shocks, changes in technological opportunities, improvements in appropriability conditions (an increase in the monopoly rights granted by intellectual property rights), or changes in market structure (e.g. increases in competition) [3]. For empirical purposes it is important to think carefully about which factors are likely to shift the marginal benefit curve because they represent the variables that should be included as controls in the empirical specification. A lack to account for the relevant covariates would not allow us to properly measure the change in R&D due a shift on the cost curve because it could be confounded with a change in other omitted relevant variables. This is illustrated in Figure 2, where the equilibrium R&D spending after a shift in the cost curve is attenuated or exaggerated in the presence of other contemporaneous shifts in the marginal benefit curve (e.g. demand shocks).

The marginal costs from R&D are assumed to be increasing with R&D spending but at an increasing rate (increasing marginal cost of R&D). A possible way to motivate this assumption is that the first units of R&D spending can be financed with internal funds but extra R&D spending requires external funds (bank loans) which are costly. The marginal cost curve shifts with labor protection to reflect as suggested by the theoretical literature discussed above that hiring and firing costs increase the cost of adjusting the labor force to the new technology. A decrease in labor protection (due for example to a reduction of firing or hiring costs) would increase the expected returns from R&D spending because if the innovative effort is successful it will now be less costly for the firm to hire workers with the necessary skills and fire workers whose skills do not match the new technology. In Figures 1 and 2 a labor reform that reduces employment protection shifts down the marginal cost

[3] See for example Cohen et. al (1987) or Levin et al (1985) or Gilbert (2006).

curve, ceteris paribus. The new equilibrium is at a greater level of R&D spending. Therefore the following testable hypotheses are implied by the model[4]:

Hypothesis 1: Labor market reforms that decrease firing and hiring costs should increase firms R&D spending.

Hypothesis 2: The greater the reduction in firing or hiring costs, the greater the increase in R&D spending.

In the following sections I test these hypotheses using data on Spanish firms before and after the labor reform in Spain in 1997.

4. Institutional Background

The Spanish Labor Market Reform of 1997

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[4] The model can be seen as a simple extension of the well known model by Cohen and Klepper (1996) on the determinants on R&D spending extended to account for labour rigidities. The extension is similar to the one proposed in Eicher and Schreiber (2006). In particular, consider that innovative firms solve the following maximization problem:

$$\max_{r} \pi_i = a \cdot q \cdot PC(r) - r(1+\lambda)$$

Where *a* can be interpreted as the number of periods that a given firms enjoys positive price-cost margins from a given product (it reflects appropriability conditions in the market or how easy it is to copy the product to other firms); *q* is the quantity of the product sold by the firm; *r* is R&D spending and *PC(r)* is the product's price margin of the firm, which depends on R&D spending. If R&D spending is on process innovations a greater *r* decreases the average cost of the firm and therefore increases *PC(r);* if R&D spending is on product innovations then *PC(r)* also increases with *r* because of the introduction of new product features that increase the willingness of consumers to pay for the product. Therefore in both cases we assume $PC'(r)>0$. Furthermore let assume that $PC''(r)<0$ to reflect that more R&D spending increases price margins but at a decreasing rate. For simplicity assume that the marginal cost of R&D is increasing at a constant rate (the conclusions hold if we use an increasing marginal cost as in Figure 1). Finally, λ is a parameter measuring the strictness of labour market protection, for example firing costs. If we assume as in Cohen and Klepper (1996) that $PC(r) = f \ln r$, then the first order condition for local maximum is:

$$r^* = \frac{a \cdot q \cdot f}{(1+\lambda)}$$

Which implies that a decrease in the parameter λ, that reflects firing costs, increases the equilibrium level of R&D spending.

Since the early 1980s Spanish labor market regulation has experienced substantial changes. In 1980 the Parliament approved the *Workers Statute* (*Estatuto de los Trabajadores*) to adapt the labor market regulation to the recently reestablished democracy. The approval of this law gave rise to one of the most rigid Employment Protection Legislations (EPL) in Europe. The Workers Statute established that workers could be dismissed for either "objective" or "economic" reasons. "Objective reasons" refers to workers incompetence or absenteeism. It is the firm's duty to prove that they exist. "Economic reasons" refers to organizational, technical or productive causes. The firm has to prove in this case that it needs to restructure the workforce. In both cases dismissed workers can appeal to court. The 1980 Statute established that if the dismissal was considered "unfair" the firm had to pay to the worker 45 days of wages per year of seniority up to a maximum of 42 months. If the dismissal was considered "fair", the firm had to pay 20 days of wages per year of seniority up to a maximum of 12 months. In the majority of cases the court considered dismissals to be unfair and therefore the higher severance payments were the rule[5]. In the majority of cases the court considered dismissals to be unfair and therefore the higher severance payments were the rule. In any case, firms with fewer than 25 workers had to pay only 60 per cent of these costs, while the other 40 per cent was paid by the government.

During the 80s Spanish unemployment rose to more than 20 per cent. In an attempt to reduce unemployment, a new labor market reform in 1984 liberalized the use of fixed term contracts. Fixed term contracts face much less severance payments and their termination cannot be appealed to labor courts. As a result, during the decade following the 1984 reform around 95 per cent of all the new labor contracts were temporary and as a consequence the share of workers under temporary contracts increased drastically from 10 per cent to more than 30 per cent of the total labor force (Kugler et al. (2003:5)).

In 1994 a new labor market reform tried to limit the use of fixed term contracts by widening the cases in which "economic reasons" could be alleged by employers to dismiss workers under permanent contracts. The effect of this reform was small, and temporary contracts continued to be widely used -see Kugler et al. (2003:6) or Dolado (2002:274).

[5] See Boletin de Estadísticas Laborales, Ministerio de Trabajo y Asuntos Sociales, in www.mtas.es.

The lack of success of the reform of 1994, leaded to passage of a major labor reform in May of 1997. The reform was aimed at reducing the share of temporary contracts and at decreasing unemployment. In order to achieve these goals the reform established a significant reduction in the costs of firing and hiring new workers under permanent contracts for most population groups (in practice everyone except men between 30 and 44 years of age). The cost of hiring new workers under permanent contracts decreased because payroll taxes were reduced between 40 and 90 per cent (depending on the population group). The cost of firing workers decreased by more than 25 per cent because severance payments for the new contracts were reduced to 33 days of wages per year of seniority up to a maximum of 24 months (recall that before 1997 severance payments were 45 days of wages up to a maximum of 42 months) in the case of unfair dismissals (this cost sets the standard for parties agreements). The reform established that the new hiring and firing costs would be effective for a period of four years (until 2001) after which the effects of the reform would be evaluated. In 2001 the provisions of the 1997 reform were extended indefinitely[6].

The reform affected all types of firms regardless their size. Therefore, firms with less than 25 workers faced also a reduction of their firing costs. However, given that these firms were already enjoying a 40 per cent reduction of their firing costs compared to bigger companies, the reform affected them less. For example, before the reform firms with less than 25 workers had to pay 27 days of wages per year of seniority in the case of unfair dismissal[7]. After the reform they had to pay 19.8 days of wages per year of seniority. This means that a firm with less than 25 workers faced a expected reduction per worker of 7 days of wages (from 27 to 20). On the other hand a firm with more than 25 workers

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[6] The effects of the 1997 reform on labor market outcomes have been extensively analyzed in recent years. Kugler et al. (2002) show that the reform had a significant effect on unemployment outcomes of the population groups mainly benefited by the new contracts. In addition, Dolado et al (2002) show that the reform reduced the incidence of temporary employment by about 4 percentage points.

[7] This example, together with a legal discussion in English about the provisions of the 1997 Spanish Labor Reform can be found in Alarcón Caracuel (1997): "The 1997 labour reform in Spain: the April agreements", available on line at the European Industrial Relations Observatory: http://www.eurofound.europa.eu/eiro/1997/06/feature/es9706211f.html .

experienced an expected reduction of firing costs per worker of 12 days of wages per year of seniority (from 45 to 33 days of wages per year) and 18 months of wages reduction in the maximum amount to pay (from 42 to 24 months).

The institutional details of the reform just outlined provide two sources of variance that I exploit in the estimation section below. On the one hand firms with less than 25 workers are expected to be less affected by the reform and therefore according to hypotheses 1 and 2, we would expect the change in their level of R&D investment to be smaller than that of large firms, *ceteris paribus*. On the other hand, the reform affected only permanent contracts and therefore firms with a greater propensity to hire permanent workers should also invest more on R&D after the reform compared to other firms that employ mainly temporary workers. While this latter source of variance is problematic because the aim of the reform was to change the ratio of permanent workers to total number of workers, I provide details below on how one can still take advantage of this variation in the estimation.

Spanish R&D Policy

Spain constitutes an interesting case of study not only because of the characteristics of the 1997 labor reform but also because of the low R&D effort of Spanish firms during the period and the absence of substantial policy changes that could significantly distort the interpretation of the effects of the labor reform. During the nineties Spain has been among the lowest performers of R&D among European countries. According to the OECD (2006) Spanish business R&D spending as a percentage of GDP was in 1995 of 0.35% while in 2001 the percentage was 0.48%. This percentage is significantly lower than in other European countries (0.88% is the mean for the EU-25 in 1995) and very far from that of the US or Japan (1.51% and 1.91% respectively in 1995). Figure 1 shows that during the first half of the nineties, Spanish firms' aggregated R&D spending was stable at around 2000 million euros but starting in 1997 the aggregated R&D spending grew every year and reached around 4000 million euros in 2001.

One of the main public policies implemented during the period that could explain the increase on business' R&D spending in the second half of the nineties (other than the labor reform) was the tax subsidies offered to innovative firms after the passage of the Business Tax Statute (Ley del Impuesto de Sociedades) in 1995 and extended in 1999. The rate of tax subsidies increased from 0.25 cents per every dollar spent in R&D in 1990 to 0.30 in 1995 and 0.44 from 1999 onwards (see OECD (2006:242)). This rate means that for example in 1990 each dollar spent on R&D yielded 25 cents of tax relief before taxes. The rates are the same for small and big firms. However, as with the labor reform, small firms are subject to a lower general tax rate (30 per cent) than big firms (35 per cent). This could create problems identifying the causal effect of the labor reform on R&D because the differential effect of the labor reform to be expected on firms with more than 25 workers compared to smaller ones could be due to the changes in the tax incentives during this period. However, the threshold to be considered small firm in the tax statute during this period is to have sales of less than 3 million euros which is different from the threshold to be exempted from paying the firing costs (less than 25 workers). While most firms with less than 25 workers are subject to the reduced business tax, many other firms with more than 25 workers enjoy the reduced tax too. This allows me to construct comparison groups that face the same tax incentives during the whole period of study and therefore to identify separately the effects of the labor reform. In particular in some of the specifications I will estimate the effect of the labor reform using only the sub-sample of firms whose sales are less than 3 million euros, that therefore faced identical tax incentives during the period.

It is however possible that even if all the firms used in the estimation faced the same tax incentives *ex ante*, not all of them were equally able to take advantage of the incentives to invest on R&D introduced in 1999. This would create an identification problem if bigger firms were able to benefit more from the tax credits introduced in 1999 compared to small firms. While it is not clear why this should be the case, I account for this possibility in some of the specifications by including an interaction term of firms with more than 25 workers and the years after the tax reform. The results of these estimations show that the tax incentives do not account for the differential investment in R&D between big and small companies observed after the labor reform.

5. Data

I use micro data of Spanish manufacturing firms from the Spanish Survey of Entrepreneurial Activities (ESEE, Encuesta de Estrategias Empresariales). The database is sponsored by the Spanish Ministry of Industry and maintained by the SEPI Foundation. The Survey is an unbalanced panel of manufacturing firms from 1994 to 2002. Around 1800 firms are included every year, of which about one third perform R&D activities. Only firms with more than ten workers are sampled. The survey is exhaustive for firms with more than 200 workers (all firms were asked to participate in the survey at the beginning of the period and around 60% of them agreed). The rest of the firms are randomly sampled by industry and size interval. A balanced panel of approximately 700 firms is included in the database. Other firms enter and drop from the survey due to attrition or exit from the market. Newly created firms are also included every year to preserve the inference properties of the sample. The final composition of the survey is considered to be representative of the whole population of Spanish manufacturing firms. The database has been extensively used in empirical research and policy analysis in recent years. In fact, the web page from the SEPI Foundation cites more than a hundred published papers in the last decade that use this database[8]. The papers by Martinez-Ros (2000), Beneito (2003), or Huergo and Jaumandreu (2004), among others, provide more details on the data and present applications to the study of innovative behavior of firms.

The database contains very detailed information on innovative activities by firms, on accounting data, on the number of employees and their type of contract (permanent or fixed term) and on several other relevant observable characteristics. In table 1 I provide summary statistics of the variables used in the estimation for the whole sample of innovative firms (5253 observations) and several other relevant sub-samples. A detailed definition of each of the variables is included in the appendix.

[8] See www.funep.es for details. The database can be purchased from this website.

6. Empirical Model

The summary statistics of Table 1 show that the average R&D spending in the sample is considerably higher after 1997 compared to previous years (around a 20 percent increase for all firms). This increase in R&D after 1997 (also see Figure 1) might be, in part, due to the causal effect of the reduction in hiring and firing costs. However it is also possible that the greater observed R&D is due to other observed or unobserved factors that were changing at the same time of the reform and could also have affected firms' R&D spending. A simple regression controlling for the main observable characteristics fails to account for the many other unobserved (to the econometrician) characteristics that may explain the change in R&D spending.

For this reason in order to isolate the causal effect of the reform, I take advantage of the institutional details and follow a differences-in-differences approach that allows me to control for observed and unobserved characteristics. In particular, as discussed above, companies with less than 25 workers were less affected by the reform (faced a smaller reduction of firing costs) but it seems reasonable to assume that were affected in the same way than bigger companies by other unobserved characteristics. Therefore I can look at what happened with companies with less than 25 workers (control group) after 1997 and compare it with the results of the rest of companies (treatment group). If the reform had an effect on R&D, I would expect to see that the group more affected by the reform increased R&D spending disproportionately, controlling for differences between big and small companies and other observable characteristics. In the simplest specification, I can estimate a regression of the form:

$$logRD_{it}= b_0+ b_1More25_{it}+ b_2After_t+b_3\, After_t*More25_{it}+b_4\, X_{it}+b_5\, Industry_i+e_{it} \qquad [1]$$

where $logRD_{it}$ is the logarithm of R&D spending of the firm *i* in period *t*[9]. *More25* is a dummy that takes value 1 if the firm has more than 25 workers and its coefficient, b_1, captures the invariant differences associated with the treatment group. *After* is a dummy that takes value 1 for the period after the reform and its coefficient, b_2, captures the secular changes in R&D common to all firms, X_{ij} is a vector of observable characteristics to be discussed below and $Industry_j$ is a set of twenty industry dummies that capture invariant differences among industries such as technological opportunities. The coefficient of interest is b_3, which measures the differential effect (in percentage) of the reform on firms with more than 25 workers ("more treated" firms) compared to smaller firms. Positive and significant estimates of b_3 would support the claim that the reform had a greater effect on R&D spending of the more treated firms, as predicted by hypotheses 1 and 2 above.

The key identification assumption of the model is that b_3 would have been zero in the absence of a differential effect of the reform on firms with more than 25 workers, or in other words the identification assumption is that $E[e_{ij}| After=1, More25=1]=0$. If this is the case an unbiased estimate of b_3 would be capturing the effect of the reform on firms with more than 25 workers compared to the effect on smaller firms:

$$b_3=E[logRD|More25=1,After=1]-E[logRD|More25=1,After=0]-E[logRD|More25=0,After=1]-E[logRD|More25=0,After=0] \quad [2]$$

This research design has become increasingly popular in the economics literature as a tool to measure the effect of policy changes on different outcomes. While most of the differences-in-differences papers come from the literature in labor economics and policy analysis, this approach has also been used in industrial organization (see for example Milyo and Waldfogel (1999) or Glazer (1983)). Meyer (1995) provides a good overview of this type of estimators. The influential paper by Bertrand et al. (2004:252) reports 92 published

[9] Another possibility is to use a standarized measure of R&D spending such as R&D to sales or R&D to workers. The problem with R&D to sales is that often times the changes in the numerator cancel with the changes in the denominator making difficult to identify the changes in R&D. Using R&D to workers is more appropriate because the number of workers is more stable. In addition using this standarized measure could help alleviate the possible heteroscedasticity problem due to the differences in the size of the companies. In any case the results using R&D to sales are very similar to the ones reported in tables 2 and 3. This results are available upon request from the author.

papers that use differences in differences estimators in recent years. Both Meyer (1995) and Bertrand et al. (2004) as well as several other papers point out several potential threats to the validity of the inferences drawn from differences in differences estimators. I now discuss a few of these threats and the way I address them in my estimation.

A first potential problem is that the heterogeneity in the control and treatment groups casts doubts to the hypothesis that the control group represents what would have happened to bigger companies should they had been subject to the same reduction of firing costs than small firms. In my basic specification, I use as a treatment group all firms with more than 25 workers and as a control group firms with less than 25 workers. According to the summary statistics of Table 1 both groups differ substantially and therefore a few adjustments and robustness checks are in place. First, I mitigate the problem by including in the regressions observable characteristics that control for size differences among firms (e.g. number of workers, sales, and the wage bill) and the dummy that accounts for other unobserved differences (*More 25*). In addition I ran several other robustness tests using more homogeneous groups. Specifically, I ran several specifications using as a treatment group other small companies (less than 40, 50 and 60 workers). I also ran additional specifications using companies whose total sales are small (e.g. smaller than 3 million euros). None of these specifications yielded significantly different results compared to the benchmark model.

Another possible specification problem arises from the way in which shocks that are common to all firms are accounted. In the specification of equation (1) secular trends common to all firms are controlled using only a dummy variable (*After*) for before and after periods. In order to allow for more flexible ways to control for time trends I estimated several models including industry-specific trends in addition to the *After* variable or including year and year-industry dummies in place of it. As with the size controls, the results are very robust to these changes.

In the most basic specification I only include as controls the variables that account for size and time. The literature on the determinants of R&D spending (see for example Cohen et.

al (1987) or Levin et al (1985)) has pointed out however that technological opportunities, appropriability in the market, and demand conditions should at the minimum be accounted for. In terms of the discussion of Section 3 above, these variables can potentially shift the marginal benefit curve as in Figure 2 and therefore should be included in the regression to isolate the effect of the reform as in Figure 1. The industry dummies (twenty industries) capture technological opportunities. I control for appropriability of the R&D effort by including a variable used among others in Beneito (2003) that is constructed as the total number of patents in a given industry over the total number of companies that patent in that industry. Demand conditions are taken into account by the measure of yearly sales. Finally, in some of the specifications I also include a control for financial constraints (short run debt) and several measures of competition (price-cost margins and concentration ratios) and export intensity to account for the effects of different market structures. While these latter variables are suspect to be endogenous to the model, I argue that the simultaneity problem is small and not likely to change the results. I find support for this claim in the fact that the results with and without the market structure variables remain similar and the goodness of fit of the model is almost unchanged after the inclusion of these variables. In addition the fact that the panel is short (I only observe firms on an average of 4 periods) and the market structure is unlikely to change in the short-run, alleviates the possible endogeneity bias.

Finally, a few comments are in place regarding the error structure. First, I take advantage of the panel data characteristics of my database and estimate the models including a random effects error term, which I assume is not correlated with the other covariates. I performed a set of Hausman specification tests to test the validity of this assumption. The fixed effects models estimated in order to compute the Hausman test, which do not rely on this assumption, lead to similar coefficients but less precise estimates. The Hausman tests favored the use of the random effects model in the more inclusive specifications while rejected it in the simplest, less inclusive ones (due to the omitted variables). I take this as an indication of the appropriateness of the random effects error structure. In addition, I also clustered the errors at the firm level to avoid potential autocorrelation problems. Such a problem here is likely to be small due to the fact that the panel is short and wide. My estimates of the errors are, as expected, slightly greater when clustering, but they are qualitatively equivalent to those using only random effects. For the shake of brevity I adopt the most conservative approach and report only the former.

7. Results

Table 2 shows the results of the benchmark model and several other specifications. In column 1 I present the simple regression in which I control for common secular shocks just with the dummy for the periods after the reform, and I include as covariates the number of workers and the sales of the firm. The coefficient of interest in column 1 (the interaction between *After* and *More25*) is positive and strongly significant, as expected. The coefficient has a value of 0.39, which implies that firms with more than 25 workers invested around a 39% more on R&D after the reform compared to smaller firms[10]. This magnitude is quite large but taking into account the very low level of R&D spending in Spain, the effect is still small in absolute terms. In the specifications of column 2 and 3 in which I allow for more flexible ways to control for time shocks, the coefficient decreases but is still positive and significant at the 99 percent level. Adding more controls, as in regressions 4, 5, and 6, leads to the same conclusions and the positive differential effect of the reform on big firms remains at around 30 percent. The R-squared in all the regressions is around 0.60, showing a good fit of the model to the data. In addition, an additional regression not reported in the table, in which only year and industry dummies were used as regressors, lead to an R-squared of 0.16, which shows that a significant part of the variance explained by the models of Table 2 is not due just to industry and year effects.

In column 7, I show the results of the model in which the effect of the reform is disaggregated by year. All the relevant interactions (*year***More25*) have the expected signs

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[10] Interpreting the coefficients of the dummy variables of Table 2 and 3 as percentages is just an approximation. The exact estimate of the percentage impact of a dummy variable on the dependent variable can be computed using the following formula:

$$\text{Effect in Percentage} = 100\,(\exp(b - V(b)/2) - 1)$$

Where is the coefficient and V is its variance. See Halvorsen and Palmquist (1980) or Kennedy (1981) for details.

and are statistically significant at conventional levels except for the interaction with year 2000 (which has the expected sign). The effect is larger in years 2001 and 2002. This is interesting because it suggests that it is relevant, in terms of policy implications, whether a given reform is permanent or not. As it was explained above, the labor reform of 1997 was initially approved for a period of four years after which its effects were evaluated again. In 2001, the government decided to extend the reform indefinitely. Given that R&D activities are risky and usually imply a long run effort by the firm, it seems reasonable that the reform had a greater effect after it was made permanent. However it also possible that the magnitude of the coefficient in years 2001 and 2002 is due to other shocks that I am not aware of and that differentially affected larger firms[11]. For this reason, in column 8 I re-estimate the model excluding the years 2001 and 2002. The effect is still positive and significant although the coefficient decreases to show that during the four years after the reform, firms with more than 25 workers invested around 20 percent more on R&D compared to smaller firms.

In Table 3 I conduct a series of checks to test the robustness of the model to possible validity threats[12]. In column 1 I estimate the model using only companies with less than 50 workers, which are more similar to companies with less than 25 workers than the whole population of firms included in Table 1. Note also that the database includes only firms with more than 10 workers. The result of this regression is very similar in magnitude (the coefficient is in this case 0.38) to the results of table 1, showing that even in this more similar group of firms, the reform had a differential effect.

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[11] For example, during 2001 and 2002, the international stock market suffered a crisis due to the crash of technology stocks. In addition the terrorist attacks of September 11th also lead to a decrease in the value of companies in the stock market. This affected the capacity of firms to fund their R&D through bank loans. To the extent that small technology companies were more affected, this could be reflected in the high coefficients or regression 7. In the case of Spain however, it is not very likely that this is the case. First the database includes only manufacturing firms and therefore, firms whose only purpose is the production of technology are excluded from the database. Second, none of the companies with less than 25 workers in the database trade their shares in the Spanish stock market. Thirdly, the effect of the international crisis is more likely to affect big manufacturing companies that operate in international markets, instead of small companies that usually do not.

[12] In Table 3 I use the specification with the industry-specific time trend and I exclude years 2001 and 2002. I prefer to use the specification with industry-specific trend instead of industry-year interactions to gain degrees of freedom.

In columns 2, 3, and 4 of Table 3 I perform several falsification exercises on the original control and treatment groups. I intend to see if there is a true threshold effect at 25 workers or if, in fact, the effect I am capturing is produced by a different threshold that may exist at some other level. In column 2 I use only firms with more than 25 workers (that is, I exclude the control group from the estimation) and create an arbitrary treatment group dummy with firms with less than 50 workers. The idea behind this test is that if there is a true effect in the labor reform, there should not be a differential effect between firms with more than 50 workers and firms between 25 and 50 workers because they were subject to the same treatment. The results of column 2 confirm this idea. The coefficient of the interaction variable is now non-significant and even negative. Other regressions using firms with less than 40 and 60 employees as control groups, yielded similar results.

In column 3 I perform a similar test but in this case using only firms with less than 25 workers. I create an arbitrary control group at the median of this sample which happens to be at 17 workers. If all firms with less than 25 workers were subject to the same reduction in firing cost we would not expect to see statistically significant differences in the impact of the reform among these firms. The regression of column 3 in table 3 is consistent with this reasoning. The coefficient of the interaction term is still positive but is small in magnitude and non-significant. I ran similar regressions using different arbitrary control groups. I found that in some cases, using different arbitrary thresholds greater than 17 workers, yielded results that were close to statistically significant levels (in one case the interaction was statistically significant at the 80 percent level). For this reason I performed several other tests to confirm that the threshold is indeed at 25 employees. In particular, in column 4 I show that even when I use companies with more than 17 workers but less than 25 as control group (that is excluding the very small companies that are suspect to be driving the results) there is still a large and significant impact of the reform.

In columns 5 and 6 of Table 3 I address the possibility that the differential effect might be due to changes in the tax incentives to invest on R&D, that were smaller for small firms. In column 5 I estimate the model using only the sub-sample of small and medium firms for which the tax incentives were the same during the period (companies with less than 3 million in sales in a given year). Again in this case the interaction term has the expected sign and is statistically significant at the 90 percent level. The magnitude of the coefficient is now smaller (0.215) but is still within the 95 percent confidence interval of most of the specifications of Table 2, and therefore we cannot reject the hypothesis that they are equal.

In column 6 I estimate the model including an interaction term between firms with more than 25 workers and the years after or equal 1999 in which the incentive to R&D activities was implemented. Including this interaction does not change the magnitude nor the significance level of the coefficient that captures the effect of the labor reform. In addition the coefficient of the new interaction term is non-significant and small in magnitude showing that the tax reform does not explain the differential effect on bigger firms' R&D after the labor reform.

Similar results are obtained in the regression of column 7 in which I only use a balanced sub-sample. The interaction coefficient is in this case 0.246, showing that the effect is not due to the different compositions of the before and after samples.

Finally in column 8 I use data on a single industry (electrical and mechanical machinery) to see to what extent the results might be driven by technological and industry specific factors that are difficult to control even after the inclusion of industry dummies. The results of column 7 are very similar to the pooled models of the previous columns in which all the industries are included[13].

As a whole the results show that the labor reform of 1997 had an impact on R&D spending in Spanish firms. Firms with more than 25 workers invested around 20 percent more (see column 8 of table 2) on R&D than smaller, less affected firms, during the four years following the reform, and even more after the indefinite approval of the reform in 2001.This result supports hypothesis 1 and 2 above, regarding the effects of a labor reform on innovative activities, and suggests that a possible reason for the extremely low levels of

[13] I estimated the model for several other technologically intense industries (e.g. chemicals), obtaining also significant results. I report the results on electrical and mechanical machinery because it is the group with more observations among the top-five most technological industries.

innovative activity in Spain could be the rigidities that Spanish firms face in order to appropriate the results of their innovation.

8. Extensions and Additional Robustness checks

In this section I complement the results of the previous section with a different identification strategy to measure the effects of the reform. The reform affected only new workers under permanent contracts. This suggests that firms with a greater propensity to hire workers under permanent contracts should be more affected by the reform than other firms that due to the characteristics of their production process require only of temporary workers. An example would be companies that produce on a seasonal basis, that hire workers only for the months with a demand peak (e.g. some food and beverages). These firms are not likely to hire permanent workers instead of temporary workers even in the presence of a substantial reduction in firing or hiring costs. Therefore a reform such as the Spanish one should have a positive differential effect on those firms that hire mainly permanent workers compared to those that have a structural propensity to hire temporary workers.

The problem is identifying which firms have a structural need for either temporary or permanent workers. A possible way to take this reasoning to the data would be to follow a differences in differences approach using a continuous measure of the ratio of permanent workers to the total number of workers as the relevant variable to be interacted with the years after the reform. A positive and significant sign in the interaction would give some support to the above reasoning. There are however obvious limitations with this approach that prevent against its use. On the one hand, the ratio of permanent workers is clearly endogenous because the reform was aimed precisely at increasing the ratio of permanent workers. Secondly, it is not clear that the ratio identifies those companies that have a greater structural need for either temporary or permanent workers. There is no reason to expect that for intermediate levels of the ratio of permanent workers a company that has to change its workforce to adapt to new products or processes resulting from innovations has more propensity to hire permanent workers than a company with slightly smaller ratio.

I follow a different approach that avoids the endogeneity problem while captures better those companies with a greater potential need to hire temporary or permanent workers. I rank the companies according to their average ratio of permanent workers before the reform. Then I drop from the sample those companies that are between the 25^{th} and the 75^{th} percentile according to their ratio. Then I create a dummy for the remaining companies, that takes the value 1 if the company was in the top 75 percentile of the ratio of permanent workers before the reform and 0 if it was in the 25^{th} percentile. I assume then that this dummy captures companies with a greater structural need for either temporary (bottom 25th) or permanent workers (top 75th) and estimate the following differences in differences model:

$$logRD_{it}= b_0+ b_1 Top75_{it}+ b_2 After_t+b_3 After_t*Top75_{it}+b_4 Z_{it}+e_{it} \quad [3]$$

in which the relevant interaction is now *After*Top75* and Z_{it} includes observable characteristics as well as industry and time controls. A positive and significant interaction term would give additional support to the idea that the reform increased proportionately more R&D spending of more benefited firms –in this case firms in the top 75^{th} percentile.

In table 4 I show the estimates of the model of equation (3). The results show that after the reform companies with a greater propensity to hire permanent workers invested a 17 percent more than companies with a greater propensity to hire temporary workers. Several other specifications using different controls for time as in column 2 yield similar coefficient estimates and significance results (slightly below the 90 per cent significance level). The results are also robust to the estimation of the model with the balanced sub-sample (column 3) or including only a single industry (e.g. electrical and mechanical machinery or chemicals) as in column 4.

Several caveats however should be mentioned to close this section. First, using the top and bottom percentiles of the ratio of permanent workers as the relevant independent variable is a very noisy and imperfect measure of the beneficiaries of the reform. A very strong assumption (the top and bottom percentile represent companies with a greater structural need for one or the other type of worker) is required to identify the effect of the reform. This assumption is only partially supported by the data because while the average ratio of permanent workers among companies in the top 75th percentile is more than 0.9, for companies in the bottom 25th percent is still as high as 0.6. Lastly, another possible concern is that the estimates of table 4 are measuring differences among big and small companies as in previous sections. This would be the case if most companies in the top 75th percentile of permanent workers have more than 25 workers while most of the companies in the bottom 25th percentile have less than 25 workers. This is not the case in our sample (most companies in both the top 75th and the bottom 25th percentiles have more than 25 workers) but estimating the model without companies with less than 25 workers reduces both the coefficient estimates and the significance levels.

These caveats cast some doubt on the estimates of this section. Overall however, the results of table 4 while not valid as an independent test of hypotheses 1 and 2, are still useful as a complementary check that provides additional support to the conclusions obtained in the previous sections regarding the positive effect of the labor reform on firms' R&D spending.

9. Concluding Remarks

To date there is little evidence on the relationship between labor market and innovation. From the theoretical perspective there are reasons to believe that a more rigid labor market would lead to a lower innovative effort from firms. A possible reason is that firms anticipate that R&D spending will result in new products or processes that may require of adjustments in the workforce. If it is very difficult to hire workers with the necessary skills to use the new technology and fire workers with outdated knowledge, firms prefer not to spend on R&D and keep using the old technology.

In this paper I have provided empirical evidence supporting the above reasoning. I have analyzed the effects of the Spanish labor reform of 1997 on R&D spending of Spanish firms. The reform reduced significantly the costs of hiring and firing new workers under permanent contracts. My econometric model shows that the reform had an important effect on R&D spending of firms. Firms more affected by the reform, increased significantly more their R&D spending after the reform than less affected firms. An interesting policy implication of the results is that labor reforms can contribute to attain innovation policy goals.

To my knowledge, this is the first analysis of the effects of a labor reform on R&D outcomes. It is just a limited contribution to the research agenda on innovation and labor markets but it shows that there is both need and room for more studies in this unexplored area.

10. Appendix 1. Variable Definition

After: Dummy variable that takes the value 1 for the years 1997 and after, and 0 otherwise. It is arguable if *After* should include 1997 or not because the reform was passed on may of 1997. I prefer to include 1997 to capture the possible anticipation effects of the reform.

Appropriability: total number of patents over the total number of firms in the sector.

Concentration Ratio: sum of market share of main four firms in the product markets were the company operates, weighted by the share of the sales in this markets on total sales of the company. This variable has a high rate of missing values in the database. I imputed the missing values assigning to each missing observation the mean of the concentration ratio of those firms within each industry that report the same number of competitors and operate in the same geographical market (national, regional, or international). This procedure was used among others by Beneito (2003). I also estimated the models following the more accurate multiple imputation procedure used among others in Artés (2006) or Steimetz and Brownstone (2005). Given that the coefficients remain very similar under both procedures, I prefer to report only the results using the simpler imputation procedure.

Export intensity: total exports over total sales.

Log of R&D Spending: log of R&D spending in constant euros in a given year.

Log of Sales: Logarithm of total sales of the firm in constant euros in a given year.

More 17: Dummy that takes the value 1 if the firm has more than 17 workers, and 0 otherwise.

More 25: Dummy that takes the value 1 if the firm has more than 25 workers, and 0 otherwise.

More 50: Dummy that takes the value 1 if the firm has more than 50 workers, and 0 otherwise.

Number of workers: total number of workers at the end of each year.

Price Margin: total sales plus stock variation plus other revenues minus intermediate consumption minus labor costs.

Short run debt: ratio of short run debt over total debt.

Top75: dummy that takes the value 1 if the company was in the top 75 percentile of the ratio of permanent workers before the reform and 0 if it was in the 25^{th} percentile.

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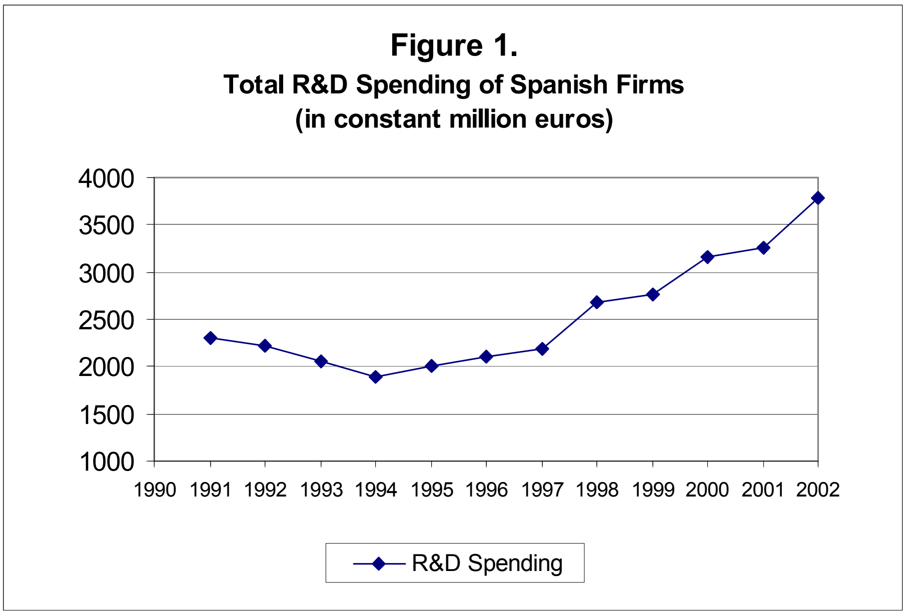

Source: INE, several years.

Table 1 Summary Statistics. Mean /Standard Deviation

	All Innovative Firms			Less than 25 workers		Between 25 & 40 workers		Sales Less than 3M €		Top 75th ratio of permanent workers		Bottom 25th ratio of permanent workers	
	Whole Sample	Before	After	Before	After	Before	After	Before	After	Before	After	Before	After
Log of R&D	12.19	12.05	12.25	9.29	9.36	10.24	10.60	11.17	11.28	12.50	12.78	11.18	11.50
	2.09	2.05	2.11	1.56	1.61	1.44	1.49	1.47	1.51	1.92	2.03	1.95	1.94
CR-4	39.75	40.65	39.33	38.46	38.76	37.85	36.84	38.62	36.97	41.47	41.37	37.39	36.57
	15.07	15.24	14.98	15.12	16.38	14.23	15.26	14.92	15.56	15.61	14.76	14.52	15.19
Export Intensity	0.30	0.27	0.31	0.09	0.10	0.18	0.21	0.27	0.29	0.28	0.34	0.23	0.29
	0.27	0.26	0.28	0.19	0.21	0.22	0.26	0.26	0.28	0.26	0.28	0.26	0.28
Short-run Debt	0.44	0.46	0.44	0.47	0.46	0.46	0.46	0.46	0.46	0.44	0.43	0.48	0.43
	0.19	0.19	0.19	0.22	0.22	0.20	0.19	0.20	0.19	0.19	0.19	0.20	0.18
Price Margin	0.11	0.10	0.11	0.09	0.09	0.12	0.11	0.09	0.10	0.10	0.11	0.11	0.11
	0.10	0.11	0.10	0.13	0.11	0.11	0.10	0.11	0.10	0.11	0.10	0.10	0.11
Log of Wages	15.65	15.34	15.79	12.35	12.84	13.39	13.87	14.56	14.77	15.78	16.35	14.14	14.99
	1.71	1.60	1.74	0.56	0.86	0.50	0.88	0.89	1.09	1.39	1.60	1.63	1.70
Log of Number of Workers	5.30	5.35	5.28	2.74	2.78	3.60	3.59	4.73	4.47	5.67	5.65	4.47	4.69
	1.36	1.39	1.35	0.40	0.32	0.19	0.21	0.81	0.78	1.26	1.27	1.50	1.39
Log of Sales	17.03	16.84	17.12	13.81	14.09	14.94	15.23	15.87	15.97	17.27	17.65	15.81	16.41
	1.72	1.72	1.71	0.84	0.73	0.75	0.70	0.86	0.83	1.55	1.59	1.74	1.67
Log of Appropriability	-1.03	-0.90	-1.10	-0.94	-1.15	-0.92	-1.11	-1.06	-1.16	-0.80	-0.97	-1.07	-1.26
	1.11	1.03	1.13	1.01	1.04	0.93	1.08	0.96	1.02	1.03	1.19	1.05	1.13
Number of Observations	5064	1740	3624	184	322	141	378	560	1153	931	1318	403	566

Table 2. Random Effects Estimation

Dependent variable: log of R&D spending

	Column 1	Column 2	Column 3	Column 4	Column 5	Column 6	Column 7	Column 8
	Benchmark model	Industry specific trend	Industry/year dummies interaction	Adding controls to 1	Adding Controls to 2	Adding controls to 3	After/year interactions	Only before 2001
After96	0.184***	0.137***		0.144***	0.126***			
	(0.039)	(0.050)		(0.039)	(0.052)			
More25	-0.345***	-0.365***	-0.350***	-0.349***	-0.356***	-0.365***	-0.361***	-0.350***
	(0.114)	(0.118)	(0.118)	(0.119)	(0.122)	(0.123)	(0.119)	(0.119)
After*More25	0.394***	0.378***	0.341***	0.376***	0.349***	0.324***		0.209**
	(0.113)	(0.112)	(0.115)	(0.115)	(0.115)	(0.118)		(0.107)
1997*More25							0.342**	
							(0.138)	
1998*More25							0.242*	
							(0.126)	
1999*More25							0.290*	
							(0.158)	
2000*More25							0.199	
							(0.166)	
2001*More25							0.644***	
							(0.211)	
2002*More25							0.629***	
							(0.197)	
Log of Number of Workers	0.461***	0.490***	0.494***	0.415***	0.431***	0.169*	0.166*	0.136
	(0.071)	(0.074)	(0.073)	(0.070)	(0.078)	(0.094)	(0.089)	(0.089)
Log of Sales	0.511***	0.487***	0.486***	0.510***	0.502***	0.370***	0.372***	0.349***
	(0.057)	(0.060)	(0.059)	(0.064)	(0.065)	(0.073)	(0.077)	(0.076)
Concentration Ratio				-0.002	-0.002	-0.002	-0.002	-0.004
				(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Export Intensity				0.213*	0.224**	0.245**	0.209*	0.313**
				(0.113)	(0.109)	(0.109)	(0.114)	(0.125)
Short-Run Debt				-0.251*	-0.278**	-0.215	-0.191	-0.202
				(0.135)	(0.134)	(0.135)	(0.137)	(0.140)
Price Margin				-0.402**	-0.349*	-0.223	-0.287	-0.093
				(0.200)	(0.201)	(0.200)	(0.205)	(0.243)
Log of Wages				0.037	0.033	0.413***	0.410***	0.460***
				(0.024)	(0.027)	(0.103)	(0.105)	(0.110)
Log Appropriability				-0.012	-0.023	-0.156	-0.011	-0.152
				(0.021)	(0.021)	(0.106)	(0.022)	(0.111)
Constant	0.219	78.117		0.146	-81.512	-2.275**	-2.462**	
	(0.695)	(85.207)		(0.710)	(135.275)	(1.120)	(0.958)	
Year dummies	No	No	Yes	No	No	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry specific trend	No	Yes	No	No	Yes	No	No	No
Year /Industry interactions	No	No	Yes	No	No	Yes	No	Yes
R^2	0.58	0.59	0.60	0.60	0.60	0.61	0.60	0.62
Observations	5253	5253	5253	5064	5064	5064	5064	3958

Standard errors are in parentheses; statistical significance levels: *** = 1 %, ** = 5 %, * = 10%

Errors are clustered at the firm level

Table 3. Random Effects Estimation
Dependent variable: log of R&D spending

	Column 1	Column 2	Column 3	Column 4	Column 5	Column 6	Column 7	Column 8
	Sample of less than 40 workers	Sample of more than 25	Sample of less than 25	Sample of more than 17 & less 40	Sample of sales less than 3M	Controlling for tax effects after 1999	Balanced subpanel	Mechanical & Electrical Machinery Only
After	0.206	0.087	-0.152	0.192	0.118	0.094	0.107	
	(0.243)	(0.055)	(0.181)	(0.261)	(0.079)	(0.058)	(0.075)	
More25	-0.505**			-0.493*	-0.207	-0.352***	-0.310*	0.470**
	(0.229)			(0.268)	(0.146)	(0.119)	(0.172)	(0.222)
After*More25	0.443**			0.390*	0.237**	0.266***	0.278**	0.299*
	(0.217)			(0.229)	(0.113)	(0.101)	(0.131)	(0.162)
More50		-0.091						
		(0.159)						
After*More50		-0.131						
		(0.137)						
More17			0.160					
			(0.241)					
After*More17			0.109					
			(0.211)					
After99						0.009		
						(0.058)		
After99*More25						0.119		
						(0.122)		
Log of Number of Workers	0.452***	0.116	0.306	0.559	0.202	0.124***	0.413***	-0.209
	(0.146)	(0.134)	(0.217)	(0.461)	(0.137)	(0.085)	(0.124)	(0.212)
Log of Sales	0.789***	0.291***	0.706***	0.770***	0.482***	0.359***	0.551***	0.340**
	(0.114)	(0.078)	(0.143)	(0.149)	(0.088)	(0.077)	(0.097)	(0.142)
Concentration Ratio	-0.012*	-0.002	-0.017*	-0.015*	-0.004	-0.004*	-0.004	0.002
	(0.006)	(0.002)	(0.010)	(0.008)	(0.003)	(0.002)	(0.003)	(0.002)
Export Intensity	0.987***	0.273**	1.333***	0.788**	0.497***	0.302**	0.142	0.257
	(0.273)	(0.129)	(0.471)	(0.327)	(0.169)	(0.124)	(0.173)	(0.183)
Short-Run Debt	-0.681**	-0.149	-0.787**	-0.536	-0.527***	-0.203	-0.393*	-0.218
	(0.310)	(0.144)	(0.382)	(0.370)	(0.199)	(0.140)	(0.206)	(0.215)
Price Margins	-0.932**	0.002	-0.845	-0.698	-0.554*	-0.09	-0.316	-0.149
	(0.461)	(0.269)	(0.589)	(0.596)	(0.286)	(0.247)	(0.280)	(0.386)
Log of Wages	-0.292*	0.579***	-0.208	-0.427*	0.099	0.46***	0.023	0.796***
	(0.153)	(0.144)	(0.183)	(0.257)	(0.144)	(0.11)	(0.042)	(0.218)
Log of Appropriability	-0.071	-0.002	0.005	-0.120	-0.021	-0.0068	-0.077***	-0.105
	(0.070)	(0.028)	(0.100)	(0.089)	(0.035)	(0.026)	(0.029)	(0.115)
Constant	-341.010	279.849	-308.750	-203.424	-122.667	-177.39	229.694	-5.966**
	(315.104)	(193.962)	(336.713)	(373.602)	(235.834)	(223.983)	(164.497)	(2.332)
Year Dummies	No	No	No	No	No	No	No	Yes
Industry Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Industry specific trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Year /Industry interactions	No	No	No	No	No	No	No	No
R^2	0.36	0.54	0.37	0.34	0.44	0.60	0.56	0.56
Rho	0.63	0.68	0.61	0.64	0.71	0.66	0.65	0.70
Observations	654	3548	410	476	1718	5064	2255	1098

Standard errors are in parentheses; statistical significance levels: *** = 1 %, ** = 5 %, * = 10%
Errors are clustered at the firm level

Table 4 Random Effects Estimation

Dependent Variable: log of R&D spending

	Column1	Column 2	Column 3	Column 4
	Industry & Year interactions	Industry Specific Trend	Balanced Subpanel	Single Industry
Top75	-0.189	-0.025	-0.118	-0.299
	(0.140)	(0.122)	(0.132)	(0.332)
Top75*After	0.179	0.102	0.108	0.356**
	(0.119)	(0.065)	(0.072)	(0.173)
Log of Number of Workers	0.155	0.413***	0.363***	0.423
	(0.117)	(0.099)	(0.111)	(0.268)
Log of Sales	0.357***	0.48***	0.552***	0.357**
	(0.088)	(0.081)	(0.085)	(0.088)
Concentration Ratio	-0.034	-0.029	0.02	0.04
	(0.118)	(0.110)	(0.111)	(0.214)
Export Intensity	0.295**	0.264**	0.233	0.299
	(0.132)	(0.134)	(0.145)	(0.211)
Short-Run Debt	-0.027	-0.102	-0.086	-0.122
	(0.163)	(0.165)	(0.18)	(0.25)
Price Margins	-0.182	-0.378	-0.337	-0.233
	(0.237)	(0.231)	(0.228)	(0.473)
Log of Wages	0.429***	0.047	0.034	0.932***
	(0.137)	(0.035)	(0.036)	(0.302)
Log of Appropriability	-0.036	-0.043*	-0.043	-0.141**
	(0.148)	(0.025)	(0.028)	(0.136)
Constant	-3.063**	290.549***	297.464***	-7.638
	(1.528)	(37.41)	(37.976)	(3.095)
Year Dummies	Yes	No	No	Yes
Industry Dummies	Yes	Yes	Yes	No
Industry specific trend	No	Yes	Yes	No
Year /Industry interactions	Yes	No	No	No
R^2	0.63	0.62	0.61	0.59
Rho	0.57	0.58	0.62	0.68
Observations	3218	3218	2712	749

Statistical significance levels: *** = 1 %, ** = 5 %, * = 10%
Errors are clustered at the firm level