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Operating Flexibility and Capital Structure: Evidence from a Natural Experiment

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Abstract

This paper provides causal evidence of flexible operating strategies affecting capital structure of firms. I exploit the appealing setting of the European labor market to show that the use of employment contracts that provide firms with a greater operating flexibility in terms of less costly firing promotes debt financing. I build the identification strategy on the exogenous inter-temporal and cross-regional variation in government programs that discouraged the use of more flexible contracts by firms. The results of the paper highlight the importance of examining operating and organizational strategies as integral determinants of corporate financing policies.

Keywords: Capital Structure, Fixed-term Contracts, Operating Leverage, Operating Flexibility

JEL codes: D22, G32, J41

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1 Introduction

The seminal work of Modigliani and Miller (1958) established the irrelevance of capital structure in a perfect market – one of the most fundamental results in financial economics – that also implies that firms can make real and financial decisions independently. The introduction of market imperfections, such as bankruptcy costs of debt, however, opens up the scope for interactions between real corporate strategies that concern organizational structure and production processes of a firm, and financial strategy guarding the choice between debt and equity. How do operations *affect* financing decisions? What are the mechanisms and consequences of this interdependence? My paper uses a unique natural experiment to provide causal empirical evidence exploring these questions.

A large theoretical literature started by Van Horne (1977), Mandelker and Rhee (1984), Dotan and Ravid (1985), and Mauer and Triantis (1994) has argued that there exists a trade-off between operating leverage – the composition of fixed and variable operating costs – and financial leverage. In particular, having relatively less fixed and more variable obligations in the operating costs structure (lower operating leverage) makes firms less vulnerable to negative shocks in product demand and in overall business conditions by decreasing the probability of not being able to cover these costs. This reduces expected bankruptcy costs and promotes the use of another fixed-type obligation – debt – which may be advantageous for various reasons, such as tax shield considerations (DeAngelo and Masulis, 1980), the mitigation of free cash flow problems (Stulz, 1990), or other motives. Therefore, according to this leverage trade-off hypothesis, firms that adopt corporate strategies that can be characterized by lower operating leverage (and hence higher operating flexibility) should have higher debt capacity and enjoy more debt financing in their capital structure as a result.

One may think about numerous interesting examples of corporate strategies that provide operating flexibility. Some of them have been modeled in the framework of real options, such as use of flexible manufacturing systems that allow changing the product mix (as in He and Pindyck, 1992), the level of output (as in Brennan and Schwartz, 1985), or the operating "mode" (as in Kulatilaka and Trigeorgis, 2004). Other examples include the outsourcing of production; outsourcing of labor (the use of temp agencies); employing contingent workforce (e.g. part-time and seasonal labor, as in Hanka, 1998); adopting a defined contribution, rather than a defined benefit, pension plan (as in Petersen, 1994); and many others.

Given such a simple and long-established theoretical idea, it is surprising to see the virtual absence of actual empirical evidence that could be interpreted confidently as the effect of flexible

operating strategies on financial structure.[1] A potential reason for this scarcity is the difficulty in overcoming challenges that arise in testing such a relationship empirically. Specifically, firms tend to naturally choose their real and financial policies jointly as part of the value-maximization objective. Due to this inherent endogeneity of operations and financial leverage, observing a correlation between a flexible technology and capital structure neither reveals the direction of causality, nor it rules out the possibility of both being an outcome of some unobserved variables. Therefore, it cannot serve as a direct evidence of operating flexibility affecting financing let alone uncover the precise mechanism behind this relationship. My paper fills this gap in the literature by addressing this empirical challenge and providing evidence of flexible operating strategies causally affecting the financial structure of firms through reductions in operating leverage that promote greater financial leverage.

The European labor market turns out to be an appealing candidate for testing the effect of flexible operations on capital structure. In various European countries there naturally exist two types of employment contracts – temporary and permanent employment contracts – that differ dramatically in terms of the employment flexibility they provide for firms, i.e. in terms of the actual "fixity" of their labor expense. The distinguishing feature of temporary contracts is that they entail much lower firing costs compared to permanent contracts. When faced with exogenous adverse shocks to demand, a firm finds its average product of labor decreasing, and in the presence of high firing costs, it may be unable to simultaneously meet its wage and debt obligations (in the form of the interest payment or principal due), and go bankrupt.

Absent firing costs, however, the same firm could find it optimal to lay off some of the workers, thereby decreasing its next-period labor expense, increasing its profit on the margin and meeting its debt obligations. The absence of firing costs effectively makes total labor costs less fixed and more variable by allowing firms to match them to idiosyncratic shocks, especially on the downside where bankruptcy is a concern. Employing workers that can be fired less costly when needed – i.e. under temporary contracts – essentially corresponds to a corporate strategy characterized by higher operating flexibility and lower operating leverage. Therefore, all else equal, firms that employ a more flexible, temporary, labor force should use more debt financing.

I use a unique panel dataset of manufacturing firms to show that the use of more flexible contract arrangements with labor indeed increases debt financing. I build the identification strategy on the exogenous inter-temporal and cross-regional variation in government programs that aimed at

[1] A step in this direction is being undertaken by Reinartz and Schmid (2012), who explore the effect of electricity generation technology on capital structure.

increasing job security among workers by discouraging the use of more flexible, temporary, labor contracts by firms. This setting, akin to a natural experiment, allows me to identify the causal effect of employment flexibility on capital structure. I find a large economic magnitude of this effect: a thought experiment of completely prohibiting an average firm from using temporary employment contracts suggests that such a firm should reduce its indebtedness by 3.6 percentage points, which is about 6.3% of the average debt level across firms.

I proceed by complementing my main results with additional tests in order to show that the mechanism behind this causal effect is indeed the substitution of operating leverage by financial leverage. First, I provide micro-level evidence that temporary workers are used as a margin of labor force adjustment during adverse business conditions. This shows that temporary employment contracts are in fact more flexible arrangements than permanent contracts and provide for relatively more variable, rather than fixed, labor costs. Then, by exploring cross-sectional heterogeneity in the severity of bankruptcy costs, I show that the effect of flexible employment on capital structure is pronounced mostly for firms that would suffer in bankruptcy more, i.e. for firms that value a given reduction in the probability of bankruptcy, coming from employing flexible labor force, more.

Finally, I look at the data from yet another angle by providing some suggestive evidence that inability to adjust capital structure to changes in employment structure, i.e. to match changes in financial leverage to changes in operating leverage, is related to default. This finding is important in light of the recent literature documenting the existence of frictions and costs of adjusting capital structure (e.g. Leary and Roberts, 2005, and Faulkender et al., 2012, among others). In particular, one can argue that if firms cannot adjust debt levels immediately to the new optimal level, then an exogenous shock to their operating strategy (e.g. through specific incentives provided by the government) may leave them overlevered for a period of time and hence additionally affect their survival and long-term outcomes – indirectly through the link between operating strategy and financing. This is another reason why it is important to know whether such a link in principle exists.

Overall, the results of the paper taken together indicate that exogenous changes in operating leverage drive the structure of financial claims. Given that a considerable part of the variation in capital structure, both across and within firms, is yet unexplained (Lemmon, Roberts, and Zender, 2008; Frank and Goyal, 2009), this finding is also important in illustrating the use of operating strategy and organizational structure as integral determinants of financing decisions of firms.

My paper contributes to several other strands of literature. First, it relates to the research

on the interactions between corporate finance and labor economics that has recently attracted some attention (see a survey by Pagano and Volpin, 2008). A large strand of this literature has focused on the effects of labor policies, typically related to unionization, on firms' real decisions and outcomes, such as profitability and market values (Ruback and Zimmerman, 1984; Abowd, 1989; Hirsch, 1991; Lee and Mas, 2012), cost of equity (Chen, Kacperczyk and Ortiz-Molina, 2011), investment and economic growth (Besley and Burgess, 2004). The general conclusion of these papers is that pro-worker regulation affects firms adversely. As for capital structure, Bronars and Deere (1991), Dasgupta and Sengupta (1993), Perotti and Spier (1993), and Matsa (2010) have explored the strategic effect of debt financing when workers are unionized, suggesting that a firm may ex ante choose the level of debt in a such way as to preclude workers from bargaining over the remaining surplus ex post. Simintzi, Vig and Volpin (2010) explore the setting of the European employment protection legislature to argue that the direction of this effect depends on whether debt is renegotiable or not.

A subtle point in studying empirically the effects of labor policies on capital structure is that high levels of unionization or employment protection may correlate with debt financing for two conceptually different reasons. The first one – the bargaining channel – is the strategic use of debt by firms to preclude workers from bargaining over the wages ex post. The second one – the operating leverage channel – is the imposition of agreements that hinder firms from laying off workers, unrelated to wage concessions. This dichotomy is not so clear-cut, potentially because in many setups the two would not be empirically distinguishable; and while some authors implicitly refer to these different interpretations (e.g. Simintzi, Vig and Volpin, 2010), others may have attributed the effect of unionization or employment protection to either one or the other mechanism.

My paper contributes to this debate by exploring a unique institutional environment of Spain where one can effectively control for the bargaining channel and illustrate the mechanism of operating flexibility separately. In this environment, both temporary and permanent workers are equally covered by collective bargaining agreements, these agreements are generally set at the levels above a firm, thereby making them arguably exogenous for the majority of firms, and the law prohibits firms from wage discrimination based on worker contract type. Thus, firms characterized by different compositions of temporary and permanent contracts have different flexibility in terms of firing in bad times, but similar exposure to total labor cost and wage concessions ex post.[2]

[2]Notably, Benmelech, Bergman, and Enriquez (2012) have recently provided empirical evidence of firms renegotiating labor contracts to extract concessions from labor during times of financial distress. This evidence can be used to illustrate separately the complementary channel of bargaining.

This paper also relates to the empirical literature that has developed various measures of operating flexibility and illustrated their relation to financial structure. Because firms do not disclose costs on the basis of whether they are fixed or variable in their financial statements, operating leverage has been typically measured using more indirect approaches, such as estimating sensitivity of EBIT to sales (Mandelker and Rhee, 1984), growth of EBIT to growth of sales (O'Brien and Vanderheiden, 1987), growth of costs to growth of sales (Kahl, Lunn, and Nilsson, 2012), costs to negative sales growth (Chen, Hartford, and Kamara, 2013) or more fundamentally, calibrating elasticities of substitution between different inputs and different products, and shadow rents of buildings, machinery, and workforce (MacKay, 2003). In order to alleviate estimation errors in such regression analyses, Du, Liu, and Shen (2012) propose a characteristic measure based on the ratio of SG&A expenses to operating costs. My paper contributes to this literature in two ways. First, by using a unique dataset that has both financial data and employment data, I am able to effectively observe the degree of "fixity" of the labor expense, and use it as a direct measure of operating leverage, without having to rely on estimation or calibration from historic data. Second, and more importantly, I employ exogenous shocks to this fixity in order to make sure the results are not driven by omitted differences across and within firms over time, and establish a causal relationship between operating flexibility and financial leverage.

The paper proceeds as follows. Section 2 outlines the details of the institutional environment, builds in the identification strategy and describes the data. Section 3 presents the empirical results on the effect of employment flexibility on capital structure. Section 4 provides evidence on the precise mechanism of operating to financial leverage substitution by showing that temporary workers provide the margin of adjustment to negative shocks, by exploring the cross-sectional heterogeneity in bankruptcy costs, and by looking at firms in liquidation. Section 5 concludes.

2 Institutional Environment and Identification Strategy

This section presents a brief description of the institutional environment in Spain and the government policies that have been implemented there to promote the use of permanent employment contracts at the expense of temporary ones. It then discusses how the differential implementation of these policies is used as an exogenous source of variation in employment flexibility, both within and across firms, to test for the causal relationship between flexible employment and capital structure of a firm.

2.1 Dual Labor Market

A dual labor market consisting of workers who are characterized by different degrees of job security exists in virtually every country, either informally (with "under-the-table" payments) or formally (with different legal contractual arrangements with employees). One particular country that provides an excellent laboratory to study the effects of employment flexibility on financing decisions of firms is Spain. First of all, the labor market in Spain is a formal dual market, which enables one to accurately measure the composition of the labor force by the type of employment contract and the corresponding degree of job security. Second, temporary employment contracts are not an artifact and are commonly used in practice there. In fact, the level of temporary employment in Spain (currently at 24% of all salaried workers as of 2012) was the highest among the European countries for a long time and was only recently surpassed by Poland (OECD, 2012). It is still not too far from the European average of 15%, suggesting that temporary employment contracts are important building blocks of the labor market system in Europe. Finally, and most importantly, the only legal difference between the two types of contracts in Spain is the implicit firing cost associated with each.

Temporary employees (especially those hired under a particular type of temporary contract – fixed-term employment contract) often perform the same job within a firm as permanent employees. In Spain, all workers in a firm – irrespective of their contract type – are covered by collective bargaining agreements, and the agreement a given firm faces precludes it from discriminating between workers based on their contract type, e.g. by paying them different wages (Jimeno and Toharia, 1994). No worker can be excluded from the provisions of the collective bargaining agreement. Furthermore, the agreements for 85% of firms in manufacturing, and especially the smaller ones, are not made at the firm level, but rather at a more aggregated level (such as industry provincial or industry national, as reported by Izquierdo, Moral, and Urtasun, 2003). These agreements apply to all firms equally irrespective of whether they participated in the actual bargaining process or not, and given that many smaller firms, which I have in the sample, are generally not in the core of the bargaining process, those agreements are arguably exogenous for them. These legal and institutional arrangements suggest that workers within the same firm cannot be characterized by different degrees of bargaining over wages, employment conditions, etc.

On the other hand, the difference in firing costs between the two types of contracts is dramatic: on the side of the firm it is much easier to lay off workers on temporary contracts than it is to fire workers on permanent contracts. When a temporary worker is dismissed (or when a fixed-

term worker is not converted into a permanent employee at the end of the three-year maximum tenure) a firm pays 0 to 12 days' wages in severance payments as opposed to 33 to 45 days' wages for permanent workers (Jimeno and Toharia, 1994). Since both figures are per year of seniority, the effect is further amplified by the observation that a permanent worker is more likely to have worked in the firm for a longer time, given the three-year legal limit for workers on fixed-term and apprenticeship contracts and the short nature of contracts for temporary jobs. Therefore the cost differential in absolute terms is even larger. Moreover, firing a permanent worker would often involve a court procedure for "unfair dismissal" with substantial administrative costs, while a temporary worker does not have the right to sue her employer for lay-off. Finally, a firm may simply choose not to prolong the fixed-term contract upon expiration and anecdotal evidence suggests that fixed-term contracts with some employees get renewed *every week*, providing the firm with an option to adjust its labor force and total wage bill for the next period almost immediately.[3]

Ultimately, investigating the effects of employment flexibility on capital structure is very appealing in the framework of the Spanish institutional setup for several reasons. First, the difference across firms in the composition of employment contracts can fully characterize the difference in the degree of flexibility on the side of the firm in adjusting the labor force following negative shocks, keeping the effects of union-level bargaining constant. Second, the large difference in firing costs between the two types of contracts implies that firms operating with different contract compositions will be far apart in terms of employment flexibility, and this will make it easier to detect its effect on capital structure of the firm. Finally, the Spanish government has implemented a number of reforms providing a significant amount of plausibly exogenous variation that makes this setup attractive for an econometrician in terms of isolating the causal effect of interest.

2.2 Panel Framework

The main hypothesis of this paper is that composition of employment contracts affects capital structure of a firm, through changing its operating leverage, the corresponding probability of default, and expected bankruptcy costs. In particular, the use of more flexible employment, i.e. hiring workers that can be easily fired when needed, enables firms to take more debt. This relationship of interest can be operationalized in the following way:

[3]For the operating vs. financial leverage hypothesis to work it must be true that wages are senior to debt repayments, since otherwise they can be abandoned when bankruptcy becomes a concern, so that hiring temporary vs. permanent workers does not make a difference in terms of shifting the bankruptcy threshold. Indeed, in Spain wages are senior claims to non-collateralized debt.

$$D_{it} = \alpha_{rt} + \beta Temp_{it-1} + X'_{it-2}\gamma + \eta_i + \epsilon_{it}, \quad (1)$$

where D_{it} is the ratio of total debt to total assets of firm i in year t, α_{rt} are the region-year fixed effects, $Temp_{it-1}$ is the proportion of workers on temporary contracts in the prior year[4], X'_{it-2} are various firm-level control variables (taken with a two-period lag to make them predetermined[5]; included in some specifications to account for past firm-specific shocks), and η_i are firm fixed effects.

The panel structure of the dataset allows me to control for any time-invariant observed and unobserved differences that firms may have with respect to their capital structure. Some examples include whether the firm in general has a more variable cash flow, whether it is a small business with a distrust in credit and banking, whether its tasks generally require more human capital specificity, or whether its assets are on average more tangible. This provides an opportunity to explore what drives within-firm changes in financing decisions holding constant any time-invariant heterogeneity across firms.

In addition, including region-year fixed effects makes sure that the differences in leverage ratios are not explained by firms potentially having differential access to credit over time induced by their location in more or less credit-abundant regions, or macroeconomic effects driving the cost of financing. Moreover, if there is generally more pressure from the society against firing workers in regions with higher unemployment rates and firms take more conservative debt policies there, region-year fixed effects will also capture such differences.

It may also be the case that firms in certain regions are able to employ different proportions of temporary workers over time (e.g. due to worker migration) and at the same time raise debt at better terms. The correlation between the error term and the independent variable that could stem from this argument and bias the estimate of β can be eliminated by region-year fixed effects as well.

Although equation (1) already picks up a lot of endogeneity as outlined above, there is still a possibility for the time-varying unobserved component of the error term being correlated with the firm's choice of employment composition. In this case, estimating (1) using ordinary least squares would yield an inconsistent estimate of β. Below I provide several potential reasons for such a correlation, discuss the direction of the bias, and explain how I address endogeneity using a

[4]I have allowed for a one-year lag in the independent variable, because it may take time for the firm to change its capital structure policy upon changes in employment policy, given that these decisions are likely to be made by different divisions in the company. Empirically contemporaneous and lagged values of Temp are highly correlated, and the results are qualitatively similar to using contemporaneous values.

[5]This avoids the "bad control" problem: if contemporaneous values are not truly exogenous and can themselves be outcome variables, the estimate of β is biased (Angrist and Pischke, 2009)

natural-experiment framework.

2.3 Endogenous Choice of Employment Flexibility

One of the primary reasons for endogenous choice of employment flexibility is the (unobservable to econometrician) firm's investment opportunity set, which can change over time at the firm level and hence cannot be taken into account by saturating the model with firm and region-year fixed effects. In particular, when firms have a large range of uncertain projects that they can invest into and expect there to be a scope for ex post project substitution, they may prefer hiring workers under more flexible temporary arrangements ex ante. On the other hand, when investors rationally anticipate such risk-shifting behavior, they supply less debt. This would show up as a negative correlation between the error term and the independent variable, thereby biasing the OLS coefficient downwards. In order to identify the causal effect of flexible labor force one then needs to introduce a shock to this variable that would be orthogonal to a firm's investment opportunities.

Another idea, brought about by Caggese and Cuñat (2008) points out that financially constrained firms are more vulnerable to liquidity shocks and therefore generate a "demand for flexibility", hiring more temporary workers than do firms that are not financially constrained. In contrast to my paper, they look at the effect of financial constraints on flexible employment. Their argument, however, can be elaborated on to explain why OLS would underestimate the true effect of temporary employment on capital structure: if financially constrained firms indeed prefer more flexible employment and, being unable to get sufficient funds, are less levered, then the error term will be negatively correlated with the independent variable. Therefore, to uncover the effect of interest, the shock to flexible labor force should also be orthogonal to a firm's financial constraints.

These concerns suggest that if OLS estimation uncovers a positive relation between employment flexibility and capital structure, then the true causal effect is only larger. In this case OLS can be used to argue for the existence of the effect, while exogenous variation helps to quantify the true magnitude. However, there are also reasons why simple panel estimates can actually be biased upwards. In this case one needs the quasi-experimental setting to validate the very presence of the effect.

A potential reason for positive correlation between the error term and employment flexibility is a firm's desire to stimulate human capital investment. In particular, Jaggia and Thakor (1994) and Berk, Stanton, and Zechner (2010) argue that since firm-specific human capital is lost in bankruptcy, firms that wish to induce employees to invest in human capital can offer longer-term contracts and

precommit to more conservative debt policies. In this case, if the reason for offering permanent employment contracts is the (unobservable to econometrician) need for human capital investment, which also prescribes firms to take less debt, the OLS specification would show a spurious positive relationship between flexible shorter-term employment and debt. To the extent that the willingness of the firm to stimulate human capital investment is a fixed characteristic of each firm, this concern will be already taken care of by the introduction of firm fixed effects into the specification. If it is time-varying, however, one needs to use a shock to labor force composition that is orthogonal to the firm's current needs for human capital in order to find out whether there is a causal relationship between flexible labor force and capital structure.[6]

Finally, suppose that firms indeed adjust their labor force by laying off temporary workers in order to meet their debt obligations when faced with negative shocks – the very mechanism behind the operating to financial leverage substitution. Then, once in a while when a negative demand shock arrives, the firm will exercise its option of firing temporary workers, while at the same time its debt-to-assets ratio will increase, somewhat mechanically since equity in the denominator will be hurt by the same negative shock. This means that if the hypothesis of this paper is true, then estimating (1) by OLS will bias the coefficient of interest downwards, since part of the time firms will be changing the proportion of temporary workers endogenously due to the unobserved demand shocks, generating a spurious negative correlation between the variables of interest. Therefore, if one aims to estimate the true causal effect of flexible employment on capital structure, it is important to introduce shocks to the proportion of temporary workers that would be uncorrelated to firm-level demand shocks. In fact, finding that the magnitude of the panel OLS coefficient is lower than the true causal effect could provide some indirect evidence towards the mechanism of this paper.

All of the above concerns illustrate the importance of the use of exogenous variation in proportion of temporary employment in order to reveal the magnitude of the causal effect of flexible labor on debt financing, i.e. to make firms change their workforce compositions not endogenously – for example due to risk-shifting, financial constraints or demand for human capital investment – but rather due to exogenous reasons. In my paper, the exogenous incentives are provided by time-varying region-level government policies that promoted the use of permanent employment contracts at the expense of temporary employment contracts. Hence I proceed with a brief description of

[6] A similar concern arises if workers willing to invest into human capital self-select into permanent contracts only when firms carry relatively low debt levels. Given high unemployment rates in Spain during the period considered, it is unlikely that workers had much bargaining power in choosing the type of the contract under which they were employed. Still, even if they did, the instrumental variable approach that I use further on in the paper would eliminate this concern for reverse causality as well.

these policies and then discuss their implementation in the instrumental variable framework.

2.4 Region-Specific Government Labor Policies in Spain

Spain's dual labor market originated in the 1984 reform which recognized the need for flexibility in the labor market by largely extending the applicability of temporary employment contracts. As a result, their use quickly rose to 35% by 1995 (or about 29% in manufacturing industries).[7] Empirical evidence for some of the European countries[8] suggests that such dualism in the labor market may have negative effects on the economy. And indeed in the late 1990s the Spanish government partially reversed the employment liberalization policy by introducing subsidies to firms for converting temporary contracts with existing workers into permanent ones and for hiring new workers on permanent contracts.

The Spanish government has subsidized the creation of permanent contracts at both the national and regional levels. Since national reform affects all firms equally and at the same time, one would not be able to attribute within-firm changes in employment composition to the effect of the reform itself, rather than, for example, to some country-level macroeconomic shocks. On the other hand, the reforms at the regional level show much more variation due to the different timing of their implementation, distinct worker eligibility criteria (such as gender), and different amounts to be paid to firms in case of a new permanent contract creation.[9]

These regional subsidies were paid to the firm once at the time of conversion of an existing temporary contract into a permanent one (or creation of a new permanent contract), either as a direct transfer to the firm or as a reduction of payroll taxes, per each contract.[10] Although autonomous regions introduced subsidies both for creating new permanent contracts and for converting existing temporary contracts into permanent ones, the two types of subsidies were highly correlated with each other and in many region-years were exactly identical; therefore I opted to not differentiate across the two in my empirical analysis, so that I record one subsidy value for each region-year-gender – and the maximum across the two if they are different.

I summarize these maximum statutory subsidy amounts that a given firm could receive per

[7] *Encuesta de Población Activa* 1995.

[8] Blanchard and Landier (2002) for France; a survey by Dolado, García-Serrano and Jimeno (2002) for Spain.

[9] I would like to thank Ignacio García Pérez for sharing detailed data on subsidies that he and Yolanda Rebollo Sanz have assembled from multiple public sources. More information on these regional policies may be found in García Pérez and Rebollo Sanz (2009).

[10] The scope for manipulation on the part of the firm aimed at obtaining the subsidy without any real changes in employment is limited: only workers who have held a temporary contract within the same firm (or were unemployed) for a certain period of time, usually at least a year, are eligible for new permanent contract creations.

contract by region, year, and gender of the worker in Table I. [11] As can be seen from this table, the time profile of the policies is diverse: some regions, such as Andalucia, implemented these subsidies every year from 1997 onwards, some – only in certain years, while Catalonia did not introduce any regional-level subsidies at all during the sample period considered. One can also note a considerable variation in subsidy amounts across regions, years, and workers' genders that range from 1653 to more than 15000 Euros per contract.

2.5 Implementation of the Identification Strategy

In order to establish a causal relationship between employment flexibility and capital structure of a firm, one needs to hold other determinants of capital structure constant. I do so by estimating β in equation (1) through IV-2SLS using an exogenous source of variation coming from government labor policies. In this respect, Spain presents a unique opportunity to study this effect, because subsidization programs promoting permanent employment at the expense of temporary employment were introduced in various regions of the country in different years and amounts, depending on workers' exogenous characteristics (such as gender); such policy implementation helps me to construct a valid instrument.

Firms were affected by the policies differentially depending on both the statutory amount of the subsidy in their region and the number of eligible temporary workers these firms had according to that particular region's criteria. To exemplify the source of identification, let's consider, for example, a firm located in Baleares autonomous region. In 2000, such a firm was eligible to receive a one-time 1653 Euro subsidy for every female worker it converted from a temporary employment contract to a permanent contract. But if the firm did not employ women on temporary contracts in the first place, this subsidy would not affect its proportion of temporary workers.

The intuition behind the identification strategy can be further illustrated by the similarity with a difference-in-differences approach. A given increase in the statutory subsidy amount brings a larger increase in incentives to substitute away flexible contracts to firms that employ more workers that are eligible for subsidization (women in the above example). The effect of the reduction of flexible employment can then be estimated by comparing capital structures across firms that have high and low eligibility to substitute flexible contracts. Inasmuch as the cross-sectional variation in the

[11] Sometimes it was not clear what the maximum Euro value could be (e.g. Valencia in 1998-2000 offered subsidies as percentages of payroll tax). For these region-years I recorded a missing value. In my empirical analysis I also did a robustness check imputing values from total wage bill information that I have and the results were similar. Given that such imputation has to rely on additional assumptions, I opted to exclude such region-years from the main analysis.

proportion of eligible workers is driven by predetermined firm characteristics, their potential direct effect on capital structure can be controlled for with firm fixed effects. At the same time, region-year fixed effects capture all time-series variation in temporary employment within regions, which could be related to the relative size of regional budgets and corresponding governmental choices of subsidy amounts, as well as region unemployment rates and other macroeconomic conditions.

The identification assumption of such a test is that the remaining variation is not correlated with things such as a firm's investment opportunity set, financial constraints, or other firm-specific shocks. Importantly, this also means that the actual amounts of subsidies received by firms would not constitute a valid instrument, since firms may endogenously self-select into participating in the regional subsidy program depending on their current unobservable characteristics. The expected amount of subsidy that a given firm in a given region was eligible to receive in a given year, on the other hand, is by construction unrelated to firm's current conditions.[12] In other words, one can use the following expected subsidy amount to predict the shift in a firm's use of temporary labor:

$$ExpectedSubsidy_{it} = \sum_{g} w^{T}_{ig0} \cdot Subsidy_{grt} \ , \tag{2}$$

where $Subsidy_{grt}$ is the maximum statutory subsidy allowed by the government in region r in year t for a worker of gender $g \in \{\text{female; male}\}$[13], and w^{T}_{ig0} is the firm-specific proportion of temporary workers by gender (which is held constant at the year the firm enters the data to avoid any endogenous gender substitution; that year is subsequently dropped from the estimation)[14].

Ideally, I would like to use the firm-specific workforce composition by gender; however, the data only allow me to observe the overall proportion of temporary workers, which is already an improvement upon many available datasets. In order to overcome this data limitation, I use the industry-specific gender intensities to proxy for firm-specific gender intensities. These industry-specific gender intensities, as summarized in Table III, provide a considerable variation. For example, more than three quarters of all employees in the "Apparel" industry are female, while women constitute less than 5% of all workers in the "Other transport equipment" industry. These industry

[12] A similar dichotomy between expected and actual values is present in Paravisini (2008) who studies the effect of bank financial constraints on lending: although actual amounts of external bank financing are endogenous, the expected amounts can be used as a valid instrument for bank sources of capital.

[13] The maximum statutory subsidy amounts, $Subsidy_{grt}$, are listed in Table I and their summary statistics are provided in Table II under "Maximum Statutory Subsidy per Eligible Worker".

[14] Given that subsidies were also differentiated worker age in some regions, I have experimented with one more layer of worker heterogeneity – age: $ExpectedSubsidy_{it} = \sum_{ag} w^{T}_{iag0} \cdot Subsidy_{agrt}$, where workers are also characterized by their age cohort $a \in \{\text{less than 25; 25 to 30; 30 to 40; 40 to 45; 45 to 50; above 50}\}$. The results of the estimation were similar both qualitatively and quantitatively.

ratios are quite stable over time, but in order to mitigate any endogeneity concerns arising from the different eligibility criteria of subsidies and possible gender substitution within industries, in the empirical analysis they are kept fixed at the pre-sample, 1993, year. Furthermore, the potential differences in capital structures across industries due to their intrinsic gender compositions will be filtered out by the industry fixed effects (subsumed by the firm fixed effects in all specifications).

The final instrument for employment flexibility is given by the lagged value of

$$ExpectedSubsidy_{it} = \sum_{g} w_{i0}^{T} w_{sg0} \cdot Subsidy_{grt} \ , \tag{3}$$

where w_{sg0} is the industry-specific use of female and male employees, fixed at the pre-sample year, and w_{i0}^{T} is the firm-specific proportion of temporary workers at the year it enters the data (that year is subsequently dropped from the estimation).[15] Finally, I also express the subsidy amount in real 2006 Euros by deflating it using the industry-level producer price index.

This instrument calculates the expected total real Euro value of subsidies that a given firm would receive per employee if it converted all of its temporary workers into employment under permanent contracts. It can be further described as the expected wage bill reduction per employee. As summarized in Table II under "Expected Subsidy per Employee" this expected per-employee wage reduction amounted, on average, to 816 Euro. Although this variable may appear to implicitly assume that all eligible workers are converted, this does not have to be the actual case for the instrument to work, since the instrument can also be interpreted in the intention-to-treat framework. In fact, as mentioned above, it would not be possible to use actual subsidy amounts received as an instrument due to endogenous participation in the government program, potentially correlated with firm-specific shocks (e.g. related to financial health of the company). The expected subsidy, on the other hand, is arguably exogenous for the firm since it combines predetermined firm eligibility (defined by its pre-existing practices and the intrinsic characteristics of its industry; to be filtered out by firm fixed effects) with the variation in government interventions that is orthogonal to firms conditional on the region-year characteristics. This makes this variable uncorrelated to current firm-specific shocks and validates its use as an instrument.

The regional level variable, $Subsidy_{grt}$, has been used in the literature on temporary employment in other contexts to instrument for the worker's probability of being converted onto a permanent contract on the worker-level data (Barceló and Villanueva, 2010, Fernández-Kranz et al., 2013). To

[15] The subsidy is either received in the year of the actual conversion, or it reduces the tax burden paid in the next year. Therefore, there is no presumption on whether the lagged or contemporaneous value should be used. The lagged value, however, turns out to be more significant in the reduced form estimation.

the best of my knowledge, my paper is the first to construct the firm-level subsidy from regional data and use it as an instrument for the overall use of temporary contracts within a firm.

2.6 Data Description and Variables Definition

The results in this paper are based on three sets of data. I combine firm-level data, regional data on subsidies promoting permanent employment contracts, and industry-level data on the gender composition of the workforce.

The firm-level data come from the *Encuesta sobre Estrategias Empresariales* (ESEE) and span the years from 1994 to 2006. This is a panel dataset of Spanish manufacturing firms collected by the Fundación SEPI (a non-government organization) and the Spanish Ministry of Industry. The ESEE is designed to be representative of the population of Spanish manufacturing firms and includes on average about 1700 firms per year. The response rate in the survey is 80% to 100% annually, and when firms disappear over time due to attrition, new firms are re-sampled to ensure that the panel remains representative.[16]

The dataset contains information on both private and public firms. 14% of firms that enter the data with more than 200 employees will at some point trade on an exchange. Among smaller firms this percentage is less than 1%. Firms in the sample represent all 17 regions (autonomous communities) and 2-digit NACE industries.

Following the literature, I use the ratio of total debt to total assets as a measure of leverage. Total Debt is defined as the sum of short-term and long-term liabilities, while Total Assets is the book value of assets, also equal to the sum of Total Debt and Book Equity. As reported in Table II, around 57% of firm financing comes from debt. Although the survey is anonymous and the data cannot be matched to market values of equity, this does not pose a problem given that the vast majority of firms are private for whom such data would by definition not exist.

This is a unique dataset in that, on top of the basic balance sheet information and the total number of employees, it contains information concerning the composition of contract arrangements with employees. In particular, I can directly measure the firm-level flexibility of labor force over time with the proportion of workers employed on temporary contracts (measured at the end of the year). As shown by Table II, 269 employees work in an average firm, 24% of whom have temporary contracts in the year the firm enters the data (this percentage is lower in subsequent years, in par-

[16] Details on the survey characteristics and data access guidelines can be obtained at http://www.fundacionsepi.es/esee/sp/svariables/indice.asp.

ticular due to the implementation of government subsidies promoting permanent employment)[17]. Firm size, measured as the natural logarithm of a firm's real sales, is equal to 16, which corresponds to approximately 8.8 million in real 2006 Euros. Average profitability, measured by a firm's operating profit margin (which is defined as the ratio of sales net of purchases and labor expenses to sales), is equal to 23%. To proxy for growth opportunities, I also measure research and development intensity defined as the ratio of R&D expenditures over sales. These variables are typically found to be determinants of capital structure choice (Titman and Wessels, 1988, Rajan and Zingales, 1995, Frank and Goyal, 2009) and will be used as firm control variables in some of the analysis. Some specifications will also include tangibility, measured by the share of gross buildings and land in total assets[18], and average wage, measured as the total wage bill per employee in real 2006 Euros, as control variables.

All firms report the location of their industrial plants and I use the region of the first plant to merge firm-level data with the data on regional subsidies promoting permanent employment. Given that 85% of firms have just one plant and additional 6% of firms have two plants with the two main plants in the same region, this constitutes the exact merge for the majority of firms (the results are also robust to estimating everything using the sample of these firms). Table II also reports the average values of maximum statutory subsidy amounts per eligible worker (i.e. per each new permanent contract created), as well as expected subsidy per employee, which are equal to 3523 and 816, respectively. It should be noted that these averages correspond to all years from 1994 to 2006, so also include a period when regional subsidies were equal to zero. When conditioning on the period after 1997, these averages become 4288 and 994, respectively. This means that given the average yearly wage of about 29 thousand Euros, the one-time subsidy covers about $52 \cdot 4288/28790 = 8$ weeks of salary for the worker that was actually converted from temporary to permanent, roughly corresponding to the numbers reported in García Pérez and Rebollo Sanz (2009).

Finally, I use the data on intensities of the use of female and male employees in different manufacturing industries, as provided by the Encuesta de Población Activa, and merge them to the firm-level data at the industry level. These gender intensities, measured as of the 4th quarter of

[17] This percentage is naturally lower than the previously reported aggregate of 35%, since manufacturing employs less temporary labor force compared to other industries, such as agriculture and construction.

[18] Unfortunately, the survey does not measure the value of depreciation and amortization separately for different types of assets, but rather records their total value. This prevents me from constructing a somewhat neater measure of tangibility, such as net fixed assets over total assets. In specifications with tangibility I opted to define it in terms of buildings and land only since these assets typically do not lose their collateral value when depreciated and they are more readily redeployed than is equipment, which is essentially what matters in determining the amount of debt. The results in the paper are, however, robust to allocating all depreciation to all tangible assets, as well as proportionally to gross tangible and intangible assets.

1993, are listed in Table III and have been discussed in detail in Section 2.5.

3 The Effect of Employment Flexibility on Capital Structure

In this section I apply the estimation strategy outlined in Section 2 to study the effect of employment flexibility on capital structure. I defer evidence on the exact mechanism driving these results until Section 4.

3.1 Main Results

Before turning to formal analysis, I first use ESEE data to explore the relationship between the proportion of temporary employees and capital structure graphically. Figure 1 plots the averages of the two variables across different industries for the period from 1994 to 2006. As indicated by this figure, the industries that employ larger proportions of temporary workers, such as "Leather and Footwear" and "Timber", are also characterized by higher debt-to-assets ratios than are industries that employ relatively lower proportions of their labor force on temporary contracts, such as "Chemicals" and "Beverages".

Figure 2 plots the time-series relationship between the two variables, and again, a positive relationship can be deduced. A striking drop in the use of temporary labor force is noticeable starting around 1997. One of the possible explanations for this drop is the country-wide implementation of subsidies promoting the use of permanent employment contracts, as described in Section 2. Interestingly – and consistent with my hypothesis – the drop in temporary employment is also accompanied by a fall in the average debt-to-assets ratio.

Although these figures provide interesting visual correlations, there may be various unobserved characteristics of industries or common macroeconomic factors that may show up as a positive relationship between temporary labor force and debt financing either across industries or across years. Therefore, I now turn to a more systematic firm-level analysis by employing both the panel structure of the dataset and the exogenous variation arising from government labor programs to estimate the causal effect of employment flexibility on capital structure, using fixed-effects and instrumental variable approaches.

Table IV shows the OLS and IV-2SLS estimates of the coefficient of interest in different specifications. The standard errors throughout the paper are two-way clustered at the firm and region-year

levels, so that all statistics are robust to heteroskedasticity and arbitrary within-firm and within-region-year correlation (which could potentially arise from the same statutory subsidy amounts firms in general face in a given region-year).[19] The coefficient in column 1 means that a one percentage point change in the proportion of workers on temporary contracts is associated with a 0.06 percentage points higher leverage ratio. I have also calculated the average within-firm standard deviation of proportion of temporary workers, which equals 0.11 in my data. Therefore, when a given firm changes its proportion of temporary workers by 1 standard deviation, it also increases its leverage by 0.66 percentage points. This specification accounts for time-invariant firm heterogeneity and region-year fixed effects, so that the results illustrate within-firm differences in leverage and are not driven by region-specific variables, such as credit abundance across and within regions, or macroeconomic effects.

Column 2 adds several firm-level control variables that have been identified in the literature as determinants of capital structure: size (measured by the logarithm of a firm's real sales), average profitability, and share of R&D expenses over sales. Both the magnitude and the significance of the coefficient of interest stay similar, so that the observed differences in debt ratios cannot be explained by firms becoming larger and more profitable over time or changes in their growth opportunities.[20] Although these specifications already pick up both all time-invariant differences across firms as well as the effects of size, profitability and growth, employment composition and capital structure are still likely to be chosen endogenously due to within-firm changes in investment opportunity set, financial constraints or demand shocks. To identify the causal effect I exploit the exogenous variation induced by the differential implementation of government labor policies and report the IV-2SLS results in columns 3 to 6.[21]

Columns 3 and 5 report the results of regressing proportion of temporary employment on the expected subsidy instrument, firm and region-year fixed effects, and additional firm-level controls (in column 5). These regressions correspond to the first stage of the IV-2SLS estimation of (1) and are given by

[19] The two-way clustered standard errors, proposed by Cameron, Gelbach and Miller (2006) and Thompson (2011), were obtained using the Schaffer (2010) xtivreg2 command in STATA.

[20] I have also tried to include accumulated profits during the previous 3 years, since firms are likely to pay out debt when they have had a positive shock to their cash flow. The results were similar. I opted to exclude this variable from further analysis in order to keep more years of observations.

[21] Since I am able to directly observe the proportion of workers on temporary contracts and can use government subsidies to construct an instrumental variable, I do not have to rely on purely reduced-form estimation (e.g. debt on employment laws). For completeness, I report the reduced-form regression results (debt on subsidy) for all specifications from Tables IV and V in Table A.1. They all have predicted coefficient signs and are significant at conventional levels. The estimates suggest that an expected subsidy of 1000 Euro per-worker leads to 0.42-0.77 percentage point reduction in the debt-to-assets ratio.

$$Temp_{it} = \alpha_{rt} + \delta ExpectedSubsidy_{it-1} + X'_{it-1}\gamma + \eta_i + \epsilon_{it}, \quad (4)$$

The estimate of δ in column 3 is significant at 1% level and shows that an expected per-worker subsidy of 1000 Euro incentivizes a firm to reduce its proportion of temporary workers by 3.8 percentage points. Since the variation in the expected subsidy amount is reasonably exogenous conditional on the firm and region-year fixed effects, the change in employment flexibility can be entirely attributed to changes in the extent of government incentives to promote less flexible employment contracts. For each first-stage specification throughout the paper, I also report the weak-identification Kleibergen and Paap (2006) F-statistic. It exceeds the Stock and Yogo (2002) weak-identification critical value of 16.38 (for 5% maximal size distortion for 1 instrument and 1 endogenous regressor) in all specifications, suggesting that my instrument is also strong.[22]

Although the use of predetermined firm-level controls is not necessary with this identification strategy, it helps in corroborating the exclusion restriction and can add more power to the estimation. In column 5, I add a range of firm-level control variables to account for time-varying firm-specific shocks (the model is even further saturated in the robustness tests). The estimate of δ remains similar and is still significant at 1% level, suggesting that the instrument is uncorrelated with the range of included variables.

The coefficients in columns 4 and 6 report the corresponding second-stage IV-2SLS estimates of β. As outlined in Section 2.3, the presence of unobserved firm-specific shocks to investment opportunity set, financial constraints, or demand shocks that can be correlated with employment composition and choice of financing would bias the OLS estimate of β downwards. Indeed, I find that the magnitude of the IV-2SLS estimates is larger. The preferred estimate of β in column 4 (0.149 with a standard deviation of 0.0582) means that a one standard deviation increase in the proportion of flexible employment leads to a 1.64 percentage points higher leverage ratio. This result is economically and statistically significant. In particular, such magnitude suggests that prohibiting an average firm from hiring temporary employees (i.e. reducing its proportion from the average of 23.9% to 0%) would lead to a 3.6 percentage points reduction in debt level, or about 6.3% of the average. Finally, in column 6 I add firm control variables, and the resulting estimate is also statistically significant and similar in magnitude.

[22] Stock and Yogo (2002) critical values are derived under the assumption of homoskedasticity and no autocorrelation, so that their comparison to Kleibergen and Paap (2006) F-statistic, which is robust to heteroskedasticity and within-cluster correlation, should be interpreted with caution, as suggested by Baum, Schaffer, and Stillman (2007).

3.2 Robustness to Additional Specifications

In order to corroborate the exclusion restriction further and show robustness of the results, I saturate my empirical specification even more in Table V. Columns 1 and 2 introduce an additional control variable of tangibility, measured by the share of gross land and buildings in total assets. If the instrument were in fact picking up some of the firm-level time-varying shocks related to the nature of a firm's assets, then we would not observe a significant and large effect in the first stage of this specification. The coefficient in column 1 suggests that the first-stage results are similar, implying that the instrument is orthogonal to the set of included controls. Furthermore, the magnitude of the causal effect in column 2 is also unchanged. This means that the results are not driven by firms changing their capital structure as a response to changes in the nature of their assets over time.

Columns 3 and 4 include average wage, defined as gross wages and salaries, compensation, social security and supplementary pensions contributions, and other social spending, per employee, as an additional control variable. This specification allows me to rule out the possibility that firms respond to firm-specific time-varying shocks in wages (e.g. arising from a new bargaining agreement at the industry level) by changing contract composition and adjusting capital structure, and that the instrument accidentally picks up this variation. The results indicate that this is not the case either. Both the first-stage and second-stage results are the same in both magnitude and significance to the main results, providing evidence against wage effects.

In columns 5 and 6, I take yet another approach and saturate the model with region-industry-year (rather than simple region-year) fixed effects. This specification provides a very tight identification. In particular, it allows to control for potential time-varying industry-level lobby power that could affect the amounts and timing of subsidy introduction. It also captures the non-uniform distribution of industries across regions and allows for a separate non-parametric trend in capital structure for each industry-region combination. The coefficient of interest can still be identified because even within the same region-industry-year firms with higher original (predetermined) proportions of temporary workers on average benefit more from the same statutory level of subsidies. The coefficient of interest in this most saturated specification remains similar in magnitude and is significant at 5% level.

Finally, in columns 7 and 8 I explore the subset of firms that were present in the data in 1994. This allows me to hedge against the potential concern that the results are driven by firms that were sampled by ESEE in later years when the government policies have already been announced or implemented (in this case their measured original level of temporary labor force may have already

partially adjusted to the reform). The results are robust. I find that even for firms that determined their employment practices years in advance of government policies, the expected subsidy instrument is a good predictor of post-reform employment flexibility. Furthermore, employment flexibility significantly affects capital structure and the magnitude of the coefficient of interest is similar to those in previous specifications.

3.3 The Role of Cash

One important consideration to be analyzed is the observation that a subsidy promoting permanent employment does not only influence the composition of the labor force per se, but also provides the firm with a cash inflow (or a reduction of cash outflow). Firms may potentially use this cash to raise even more debt (Blanchard et al, 1994) or pay out the existing debt (Bates, 2005). In this respect, the exclusion restriction of the instrument would not be satisfied. Given that the estimated effect of proportion of temporary workers on debt levels is positive, we should be concerned only if cash from the subsidy is used to pay out debt (this will bias the estimated effect upwards; if cash is used to raise more debt instead, then the estimated effect is biased downwards, which means that the true effect of temporary employment on debt levels is even stronger).

Interestingly, one would not be able to refute these concerns by looking at net debt levels, i.e. (Total Debt - Cash) / Total Assets, as the dependent variable, because the cash from the subsidy would not necessarily have to stay on the balance sheet under the cash item, but could immediately be used to reduce the total debt value. However, there are several other ways to look at whether cash inflow plays a role in this particular situation and quantify its effect if it does.

First of all, I examine the effect of employment flexibility on capital structure for firms that can be considered relatively cash-abundant. For these firms, it is unlikely that a marginal increase in cash from the subsidy could trigger paying out debt, since they could have done so without receiving the subsidy if they wanted to. Therefore, finding a significant effect of the use of temporary contracts on capital structure for the subsample of firms that can be characterized by relatively high cash holdings would provide evidence against the cash hypothesis driving the results.

Although ESEE does not contain a separate entry for cash and cash equivalents, I can use several proxies for cash abundance based on the profit and loss items. I calculate an approximation to operating cash flow as sales plus other income (e.g. from leasing and services provided) less material, personnel, and other costs (e.g. advertising, R&D and external services), less 35% corporate tax rate, less net capital expenditures (which is equal to purchases minus sales of tangible assets). Then

I classify firms as being relatively cash-abundant based on this measure and estimate specification (1) for subsamples of these firms.

The results are reported in Table VI, where the even- and odd-numbered columns correspond to specifications with and without firm-level controls, respectively. In columns 1 and 2 I define firms as being rich in cash if their cash flow was above the industry median two years in advance. This corresponds to the time structure used throughout the paper when firms first receive the subsidy, then adjust their labor force, and then change their capital structure. Since firms are classified relative to the yearly industry median, the results are not driven by accidentally capturing whole industries that were positively affected by shocks in a particular year. The coefficient in column 1 suggests that among cash-abundant firms a ten-percentage-point decrease in the proportion of temporary workers leads to 2.54 percentage points lower debt ratio. This coefficient is statistically significant at 1% significance level and robust to including firm-level controls.

I perform a series of robustness checks by considering alternative definitions of cash-abundant firms. Columns 3 and 4 classify a firm as being rich in cash if its cash flow is above the industry median in the current year, i.e. when debt adjustment takes place. The results are very similar. In columns 5 and 6 I use the ratio of cash flow to total assets as the measure of cash abundance (redefining the industry median accordingly) to make sure that the results are not driven by larger or smaller firms overall. Finally, in columns 7 and 8 I classify firms as having relatively high cash holdings if their ratio of cash flows, accumulated over three years, over total assets is higher than the corresponding industry median. This mitigates the effects of transitory shocks and enables to look at firms which have performed better than their peers over several years. Again, the results are very similar across all specifications. Overall, they indicate that even among cash-abundant firms flexible labor force has a large and significant effect on capital structure, thereby providing evidence against firms simply using subsidies to pay out debt.

Another approach to looking at the direct effect of cash inflow is to compare the magnitudes of changes in debt to the magnitudes of cash inflow from subsidies. To do so, one would need to observe the actual subsidies received at the firm level; however, I am not aware of a dataset that would contain such information even at the region or industry level. Therefore, I do a back-of-the-envelope calculation of these magnitudes to quantify how much of the total change in debt levels, implied by my main results, can be attributed to purely repaying out using cash received from subsidies.

In my data, the average within-firm change in the percentage of temporary labor force is equal to 1.07 percentage points per year. Given the average size of the total labor force (269 from Table II)

and the maximum subsidy for each eligible worker (3158 from Table II), this amounts to receiving $0.0107 \cdot 269 \cdot 3158 = 9090$ Euro per year in subsidies[23]. At the same time, my preferred estimate of 0.149 (Table IV column 4) implies that such change in temporary labor force leads to 1.07*0.149 = 0.16 percentage points change in debt-to-assets ratio per year, or given the average total assets of 57.7 million Euro (from Table II), to $0.0107 \cdot 0.149 \cdot 57.7 \cdot 10^6 = 91991$ Euro average change in debt level per year. These two numbers suggest that about 9090/91991 = 9.9% of the found causal effect can be due to cash considerations, i.e. the true causal coefficient is not 0.149, but about 0.134.

As a robustness check I also perform this calculation with medians instead of means, as well as separately by region (weighted by the number of observations per region to obtain the overall mean). These results correspond to 6.5% and 10.3% of the total causal effect being due to cash considerations, respectively, indicating that the true magnitude of the causal effect of flexible labor force on capital structure is potentially only slightly lower than the one reported in the main results of the paper.

4 Flexible Employment Contracts Reduce Operating Leverage and Default Risk

The results so far provide evidence of a positive causal relationship between the use of temporary employment contracts and financial leverage, but the exact mechanism is not yet identified. In this section I use further analysis to first demonstrate that flexible employment reduces the operating leverage by providing the margin of adjustment when negative shocks arrive. Then I present a direct test of operating to financial leverage substitution by exploring the cross-sectional heterogeneity and comparing firms with different levels of ex ante bankruptcy costs, which implicitly define the value of using flexible employment in reducing default risk. Finally, I compare firms in a cross-section to illustrate that inability to adjust capital structure in response to shocks to employment flexibility is associated with default.

4.1 Temporary Employees as a Margin of Adjustment

The underlying assumption behind interpreting the causal effect of flexible employment on financial leverage as the substitution between operating and financial leverage is that temporary workers

[23]Instead of taking the product of the averages, another option would be to average the product of total labor force and maximum subsidy per eligible worker. This would amount to receiving 0.0107*839327 = 8981 Euro per year in subsidies.

lower the default risk of a firm by providing the margin of adjusting labor force when the firm faces negative shocks. Recent evidence across a range of European countries suggests that temporary workers absorb a higher share of the volatility of the output (Blanchard and Landier, 2002, for France; Alonso-Borrego et al., 2005, for Spain; Kugler and Pica, 2004, for Italy). My data allow to corroborate this assumption by providing micro-level evidence of firms adjusting their labor force by laying off temporary workers in response to negative shocks.

It should be noted that to test the assumption that temporary workers reduce operating leverage and default risk one could not simply use a measure of the overall risk of the company (e.g. volatility of cash flows or probability of going bankrupt) as the dependent variable in a regression similar to (1). The reason is simple: in such a test one needs to keep all other variables constant, and financial leverage in particular, since this assumption implies that flexible employment reduces operating leverage and probability of bankruptcy *for a given level of financial leverage*. In other words, the realized probability of default would also necessarily reflect the endogenous adjustment of capital structure, that has been shown to adjust in Section 3. In fact, if companies, as hypothesized, trade off operating and financial leverage, then empirically we should see no effect of flexible labor force on the realized probability of default (still under the restrictive assumptions of no effect of temporary workers on survival other than through capital structure, and perfect adjustment of debt to the optimal level).[24]

To overcome this challenge, I use another approach and test whether firms indeed fire temporary workers when faced with negative shocks. To do so I use ESEE to measure the current state of the firm's main product market, proxying for demand shocks to its product. In particular, every year firms report whether the market for their good is in expansion, stable, or in recession. Then I define a dummy variable ($NegativeShock_{it}$) that equals 1 if the firm reports that the market is in recession, and 0 otherwise. The idea of using this measure as a proxy for negative shocks relies on the observation that when a firm's product market is in recession, the average product of labor falls – so by firing some of its temporary workers a firm can save on labor costs and enjoy a higher profit than it could should it have kept these workers employed.

I estimate the following specification:

$$Temp_{it} = \alpha_{st} + \lambda NegativeShock_{it} + \eta_i + \epsilon_{it}, \tag{5}$$

[24]Formally, the optimal level of debt equates marginal benefit of debt (e.g. in the form of interest tax shield) and marginal cost of debt (in the form of expected bankruptcy costs comprising of probability of default and its severity). Since temporary workers affect neither marginal benefit of debt, no the severity of bankruptcy costs, the probability of default at the new optimum level of debt should be the same as before.

where α_{st} are the industry-year fixed effects, $NegativeShock_{it}$ is the indicator variable defined above, and η_i are firm fixed effects.

The inclusion of firm fixed effects captures potential average differences in assessing the state of the market across firms as well as any other unobserved time-invariant heterogeneity that could be related to the proportion of temporary workers. It is also important to include industry-year fixed effects in such specification in order to control non-parametrically for the state of the business cycle in a given industry. This implies that $NegativeShock_{it}$ measures the firm-specific demand shock over and above any industry-level shocks in the same year.

The results of estimating (5) are presented in Table VII column 1, while column 2 further saturates this specification with region-industry-year fixed effects. The latter identifies this correlation very tightly, because firm-specific demand shocks are now measured over and above any shocks to other firms in the same industry in the region where the firm is located. The coefficients in both specifications are highly statistically significant and imply that when the market for a firm's main product is in recession, it employs a lower proportion of temporary workers. In particular, during an average firm-specific negative demand condition, the proportion of workers employed on temporary contracts is 1.7 percentage points lower than it is during normal demand conditions; this roughly corresponds to firing about one tenth of the total flexible labor force. This result is robust to clustering standard errors at the industry level to account flexibly for the correlation of shocks within each industry, as well as to using a lagged indicator of firm-specific negative demand shock instead of a contemporaneous one.[25]

To the extent that firms may have several product markets, measuring firm-specific demand shock based only on the main market can introduce noise. Column 3 reports the results of estimating specification (5) for a subsample of firms with only one product. The results are robust. Although there is no quasi-experimental variation in the independent variable, including this large range of fixed effects (firm-level and region-industry-year) should capture a vast majority of potential omitted variables that could be correlated with the independent variable and bias the estimate of λ. In order to further minimize reverse causality concerns, I estimate specification (5) for a subset of firms that sell only one product and have a low share in that market (less than 5%). For these firms, the extent to which a given firm can affect the state of its product market is very limited, making the independent variable arguably exogenous for such firms. The results are again robust.[26]

[25] In addition, I also tried including a leading indicator of $NegativeShock_{it}$. This was not statistically different from zero, minimizing the concerns about reverse causality.

[26] In unreported results, I also find that total employment significantly falls when a firm suffers a negative demand shock, implying that the results for the proportion of temporary labor force, $Temp_{it}$, cannot be attributed to a rise in

Overall, the results presented in Table VII show unique micro-level evidence of temporary workers providing a margin of labor adjustment when negative shocks arrive, and thereby corroborate the assumption that underlies the mechanism of operating leverage behind flexible labor force affecting capital structure.

4.2 Cross-Sectional Heterogeneity in Bankruptcy Costs

If flexible labor force indeed reduces operating leverage and the incidence of bankruptcy, then firms with a relatively high *severity* of bankruptcy costs should value the option to fire workers more.[27] This means that a given change in the proportion of temporary workers and the corresponding change in the probability of bankruptcy involves a larger change in expected bankruptcy costs in firms with more severe bankruptcy costs. Since expected bankruptcy costs matter for financial leverage, we should find a larger causal effect of flexible labor on corporate financing for these firms.

In order to examine this prediction I estimate the following equation using the instrumental variables approach:

$$D_{it} = \alpha_{rt} + \beta^H High_{i0} \cdot Temp_{it-1} + \beta^L Low_{i0} \cdot Temp_{it-1} + X'_{it-2}\gamma + \eta_i + \epsilon_{it}, \tag{6}$$

where $High_{i0}$ and Low_{i0} are the indicator variables corresponding to firms with high and low levels of bankruptcy costs[28]. If temporary workers reduce operating leverage and the probability of bankruptcy, then the causal effect of flexible labor force on capital structure should be higher for firms with high levels of bankruptcy costs, i.e. β^H should be higher than β^L in the above specification. What remains is to identify the subsamples of firms that would incur high and low levels of bankruptcy costs if they were to go bankrupt.

Bankruptcy costs are typically modeled as the loss of value in liquidation, when keeping the firm alive would yield more. Williamson (1988) and Shleifer and Vishny (1992) have emphasized that the degree to which debt-holders can recover their assets in liquidation depends on the nature of these assets. In particular, when asset specificity is low and assets can be easily redeployed for other purposes, the loss of value from liquidation is low and so is the bankruptcy cost of debt. Given that

total employment (increase of the denominator), but rather to firing temporary workers when downsizing (decrease of the numerator more than the decrease of the denominator).

[27] Smith and Stulz (1985) present a similar argument on hedging: it reduces the variability of cash flow and the probability of bankruptcy, and these reductions are valued more by firms which would have suffered higher bankruptcy costs, had bankruptcy occured.

[28] The instruments for $High_{i0} \cdot Temp_{it}$ and $Low_{i0} \cdot Temp_{it}$ are $High_{i0} \cdot ExpectedSubsidy_{it-1}$and $Low_{i0} \cdot ExpectedSubsidy_{it-1}$.

some of the least specific assets are buildings and land, I first classify firms as having low levels of bankruptcy costs ($Low_{i0} = 1$) if they have buildings or land on their balance sheets – in the year they enter the data (to mitigate the concern of endogenous choice of assets specificity over time). Likewise, firms with no buildings and land are classified as having high levels of bankruptcy costs ($High_{i0} = 1$).

The results of these regressions are presented in Table VIII columns 1 to 4, with odd- and even-numbered columns corresponding to first- and second-stage results, respectively. Consistent with temporary workers reducing operating leverage and the corresponding probability of bankruptcy, the positive effect of having a flexible workforce is pronounced mostly within the high bankruptcy costs firms. The coefficient in column 2 means that for these firms a one standard deviation increase in the proportion of workers on temporary contracts leads to 3.3 percentage points higher debt ratio. Furthermore, the implied difference in the coefficients between high and low bankruptcy cost firms (0.223 with a standard error of 0.128) is large and statistically significant, suggesting that firms with high levels of bankruptcy costs are significantly more likely to adjust their capital structure in response to shocks to the flexibility of the labor force.

Importantly, column 1 indicates that both types of firms change their labor force composition in response to subsidies promoting less flexible contract arrangements with workers. However, only firms for whom this flexibility does matter (i.e. firms that would incur relatively severe bankruptcy costs) decrease their debt levels upon the reduction in the proportion of temporary workers. This cross-sectional comparison provides direct evidence that the mechanism behind the identified effect is the one of flexible labor reducing operating leverage by lowering probability of incurring bankruptcy costs of debt.

Columns 3 and 4 report the results of a similar specification with firm-level control variables. The results are very similar. As a robustness check, I also reestimate specification (6) for a different definition of high and low bankruptcy costs firms. In order to take into account potential differences across industries in their usage of buildings and land, I now classify firms as having high (low) level of bankruptcy costs if they have less (more) buildings and land than the industry median in the year they enter the data. The results of these specifications are reported in columns 5 to 8 of Table VIII, demonstrating similar patterns. Again, although both high and low bankruptcy cost firms respond to subsidies by lowering proportion of flexible labor force (columns 5 and 7), only the firms for which flexible labor force is most valuable in reducing the expected bankruptcy costs adjust their debt levels accordingly (columns 6 and 8).

4.3 Firms in Liquidation

The mechanism of operating to financial leverage substitution implies that if firms want to keep their overall level of risk constant, then they should match changes in flexible labor force to changes in indebtedness. Section 3 has shown that indeed on average firms reduce their debt levels following reductions in employment flexibility. It would be also interesting to explore what happens to firms that do not reduce leverage when substituting permanent workers for temporary ones. Theoretically, these firms should become riskier overall and hence be more likely to liquidate.

Although it is not possible to explicitly identify the subsample of firms that do not adjust their debt levels and then explore their outcomes (and exit rates in particular), one possible alternative is to compare the adjustment of debt in firms that liquidate to that in surviving firms. To do so I define an indicator variable $Exit_i$ that equals 1 if the firm exits the data by the end of the sample (due to liquidation or switching to non-manufacturing activity), and 0 otherwise. Similarly, $Stay_i$ is defined as an indicator variable that equals 1 if the firm is still in the data by the end of the sample (i.e. for each firm $Stay_i + Exit_i = 1$).

Then I estimate the following equation using the instrumental variables approach:

$$D_{it} = \alpha_{rt} + \beta^S Stay_i \cdot Temp_{it-1} + \beta^E Exit_i \cdot Temp_{it-1} + X'_{it-2}\gamma + \eta_i + \epsilon_{it}, \tag{7}$$

where $Stay_i$ and $Exit_i$ are the indicator variables as defined above[29].

The results are reported in Table IX, with odd- and even-numbered columns corresponding to first- and second-stage results, respectively. Interestingly, column 1 indicates that exiters and survivors respond equally to government subsidy program by reducing the proportion of workers on temporary contracts. However, as column 2 shows, the effect of employment flexibility on capital structure is only present among survivors. In other words, although firms that eventually liquidate do change the composition of labor force following subsidized contract conversions, they do not adjust their indebtedness – unlike firms that survive. The implied difference in the effect across the two subsets of firms is large and statistically significant[30]. The results are qualitatively and quantitatively similar to the results in columns 3 and 4 that include firm-level controls.

Given that firms that enter the data later in the sample are statistically more likely to survive by any given date (and by the end of 2006 as they are currently measured), as a robustness check, I

[29]The instruments for $Stay_i \cdot Temp_{it}$ and $Exit_i \cdot Temp_{it}$ are $Stay_i \cdot ExpectedSubsidy_{it-1}$and $Exit_i \cdot ExpectedSubsidy_{it-1}$.

[30]The non-significant effect of exiters (-0.0596 with a standard error of 0.120) is unlikely to be driven by the lack of power, since about one third of the firms are classified as exiters.

replicate the above analysis focusing on the subset of firms that are in the data in 1994. The results of these specifications are reported in columns 5 to 8 and are again very similar.

One certainly has to be very careful when interpreting these empirical results, and I do not claim that firms liquidate precisely *because of* not adjusting their financial leverage appropriately to the new optimal level that would correspond to employing workers on less flexible contracts. A more likely explanation is the existence of unobserved time-varying reasons that would be responsible for some firms not being able to adjust financial leverage, and that could at the same time correlate with the decision to liquidate. (The results of the previous sections are of course still valid irrespective of such reasons.) A stretch to this argument, however, also suggests that in the presence of frictions and adjustment costs of leverage that prevent firms from adjusting capital structure perfectly, real government policies may have both the desired direct effects on employment and operations, as well as additional unintended consequences through the link between operations and financing.

5 Concluding Remarks

Causal empirical evidence supporting a very intuitive and long-established theoretical idea of the substitution between operating and financial leverage is remarkably scarce. The lack of such evidence potentially arises from the difficulty of measuring operating flexibility accurately from available data as well as the need to recognize the joint nature of operating and financing decisions and account for the consequent endogeneity empirically. In this paper I address these challenges and provide such novel evidence.

By exploiting the attractive setting of the Spanish dual labor market, I am able to measure the actual "fixity" of each firm's labor expense, and correspondingly its operating flexibility, with the proportion of workers that it employs under flexible contracts. In addition, the inter-temporal and cross-regional variation in government policies allows me to evaluate the causal effect of a firm's employment flexibility on its capital structure in a natural-experiment setting. I show that a decrease in a firm's operating flexibility leads to a large decrease in its debt financing, and that this decrease is even higher for firms that would lose more if they were to go bankrupt.

The results of my paper emphasize the importance of further empirical and theoretical investigation of the interplay between different organizational strategies of a firm and its financing decisions. From the applied perspective, this interplay supports the complementarity of CEOs' and CFOs' decision-making. From the policy perspective, it suggests that exogenous changes in government

policies aimed at certain organizational changes in the firm would have indirect consequences for their financing decisions as well. Additionally, in a setting with imperfect capital structure adjustment, this may have implications for which firms are less likely to survive in the long run and for industry allocative efficiency; with quantitative predictions achieved through incorporating frictions into a joint model of a firm's organizational structure and corporate financing, potentially in a general equilibrium framework.

Finally, given that a large part of the variation in capital structure remains unexplained, we may seek to further explore the fundamental factors related to production processes, boundaries of the firm, and organizational structure as the essential drivers of corporate financing policies. As Zingales (2000) points out, 'Corporate finance is the study of the way *firms* are financed. Theory of the firm, thus, has a tremendous impact on the way we think about corporate finance, the way we do empirical research, the policy implications we derive, and the topics we choose to study.'

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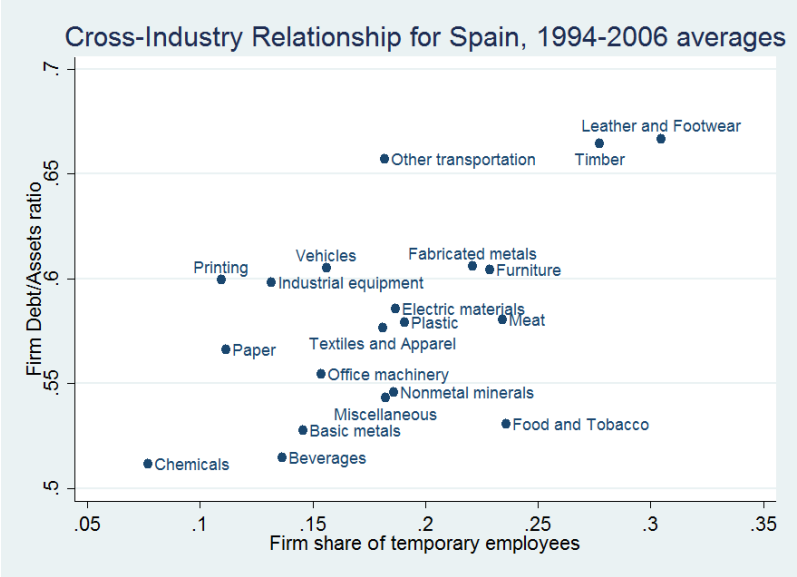
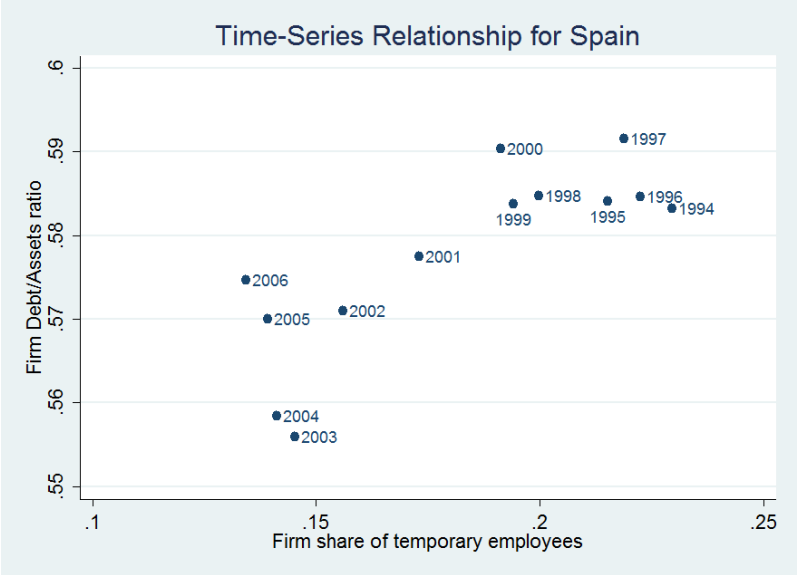
Figures and Tables

Figure 1



Note: This figure plots the relationship between average firm-level leverage (defined as the ratio of total debt to assets) and average firm-level share of temporary employees, computed for different industries across all firm-years in ESEE. The time period covers 1994-2006.

Figure 2



Note: This figure plots the relationship between average firm-level leverage (defined as the ratio of total debt to assets) and average firm-level share of temporary employees, computed for different years across all firms in ESEE.

Table I. Maximum Statutory Subsidies per Eligible Worker by Region, Year and Gender of the Worker

This table lists the maximum statutory amounts of region-specific subsidies for creating a permanent employment contract by region, year and gender of the worker, in current Euro amounts, excluding the special treatment provinces and disabled workers. The missing value indicates that the maximum amount is not available.

Year / Region	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006
Andalucia	0			4200					6012			4750	
Aragon	0				4200	5280			5500		3750 if male, 5280 if female	5280 if male, 4500 if female	4000 if male, 5280 if female
Asturias	0			4350	4500	0	4200				4500 if male, 5400 if female		
Baleares	0						0 if male, 1653 if female		0 if male, 1800 if female	0 if male, 4808 if female		0	3000
Canarias	0				3600		0						
Cantabria	0				3900	0	4507	4207 if male, 4808 if female					
Castilla-La Mancha	0				3000 if male, 3600 if female	0	3000 if male, 3600 if female				3000 if male, 4200 if female		
Castilla-Leon	0				5112	5115	4508				4000 if male, 4500 if female		
Catalonia	0												
Valencia	0				.			1875 if male, 2000 if female	1875 if male, 2250 if female	4400	2500 if male, 5000 if female	2000 if male, 4600 if female	4000 if male, 5000 if female
Extremadura	0			13402	14027	14028	4296 if male, 5217 if female	4455 if male, 5410 if female	6010	.	4500		
Galicia	0				4200	4207 if male, 4808 if female	4200 if male, 4808 if female	6000		3600 if male, 4200 if female	5400 if male, 6000 if female	3300 if male, 3900 if female	5000 if male, 7500 if female
Madrid	0				6000	7800	6600 if male, 9000 if female	12000	13824 if male, 15027 if female	12000	0 if male, 3000 if female	9100 if male, 10000 if female	7000 if male, 7800 if female
Murcia	0				6000		6000 if male, 9000 if female		4800 if male, 6000 if female	5400 if male, 6000 if female	5400		
Navarra	0				3000	4800						.	
Basque country	0				3600	7512 if male, 9014 if female					6000 if male, 7500 if female		
Rioja	0				4500	4491	6011					4508 if male, 5109 if female	

Table II. Descriptive Statistics

Notes: The sample includes all firms in the ESEE (1994-2006). Total Assets is book value of total assets of the firm. Total Debt / Total Assets is the ratio of total debt (which is the sum of short-term and long-term liabilities) to total assets. Total Employment is firm's total employment at the end of the year. Temp is the ratio of workers on temporary contracts relative to total employment. $Temp_0$ is the ratio of workers on temporary contracts relative to total employment in the first year the firm is in the data. Maximum Statutory Subsidy and Expected Subsidy per Employee are the maximum and expected subsidy amounts a firm is eligible to receive (defined in Section 2), in 2006 Euros. Size is the natural logarithm of firm's real sales, in 2006 Euros. All amounts are deflated using the industry-level producer price index – Indice de Precios Industriales. Profitability is the operating profit margin of the firm, which is defined as the ratio of sales net of purchases and labor expenses to sales. R&D is the ratio of total expenses on research and development to sales. Tangibility is the share of gross land and buildings in total assets. Average wage is the total wage bill per employee, in 2006 Euros. All firm-level control variables are winsorized at 1% tails.

Variable	Mean	Std. deviation	N
Capital Structure:			
Total Assets	57.7mln	255mln	18365
Total Debt / Total Assets (D_{it})	0.571	0.230	18365
Employment:			
Total Employment	269	783	18365
Temp ($Temp_{it}$)	0.174	0.210	18365
$Temp_0$	0.237	0.250	18364
Subsidies:			
Maximum Statutory Subsidy per Eligible Worker ($Subsidy_{grt}$)	3523	4011	17488
Expected Subsidy per Employee ($ExpectedSubsidy_{it}$)	816	1538	17488
Additional Variables:			
Size	16.013	2.014	18347
Profitability	0.225	0.134	18346
R&D	0.007	0.017	18246
Tangibility	0.139	0.152	18228
Average Wage	28790	12278	18365

Table III. Gender Distribution of Employees in Manufacturing Industries

Notes: This table lists total number of employees, in thousands of people, in different manufacturing industries and the corresponding proportion of women, measured as of the 4th quarter of 1993. The data come from Encuesta de Población Activa. *Petroleum refinery firms are not included in ESEE, but reported here for consistency.

	Total	Men	Women	% Women
Total in manufacturing	2105.4	1638.4	466.9	**28.5%**
Food and beverages	331.1	242.7	88.4	**26.7%**
Tabacco	9.4	5.0	4.4	**46.8%**
Textiles	105.4	62.1	43.3	**41.1%**
Apparel	119.2	29.8	89.5	**75.1%**
Leather and Footwear	64.0	43.2	20.8	**32.5%**
Timber	59.0	54.1	4.9	**8.3%**
Paper	39.6	32.4	7.1	**17.9%**
Printing and publishing	113.4	82.7	30.7	**27.1%**
Petroleum refinery*	12.2	10.6	1.6	**13.1%**
Chemicals	128.4	93.9	34.5	**26.9%**
Plastic and rubber products	82.1	68.3	13.8	**16.8%**
Other nonmetal mineral products	140.6	124.5	16.1	**11.5%**
Basic metal products	99.4	92.1	7.3	**7.3%**
Fabricated metal products	169.8	156.2	13.6	**8.0%**
Industrial and agricultural equipment	130.8	120.2	10.6	**8.1%**
Office machinery	12.3	9.4	2.9	**23.6%**
Electric materials and equipment	59.7	44.6	15.1	**25.3%**
Radio and TV equipment	36.3	26.8	9.5	**26.2%**
Medical equipment and precision instruments	25.6	15.3	10.3	**40.2%**
Vehicles and accessories	178.1	162.0	16.2	**9.1%**
Other transport equipment	57.9	55.1	2.8	**4.8%**
Furniture and other manufacturing	126.3	102.7	23.6	**18.7%**
Recycling	4.8	4.6	0.2	**4.2%**

Table IV. Capital Structure and Employment Flexibility: Main Results

This table reports the results of estimating the following specification using the OLS and IV-2SLS frameworks:

$$D_{it}=\alpha_{rt}+\beta Temp_{it-1}+X_{it-2}'\gamma+\eta_i+\varepsilon_{it}\ ,$$

where D_{it} is the leverage (ratio of total debt to assets) of firm *i* in year *t*, $Temp_{it-1}$ is its proportion of workers on temporary contracts, lagged one year, α_{rt} are region-year fixed effects, η_i are firm fixed effects, and X_{it-2} are firm-level controls (log of sales, operating profit margin, and R&D expenses over sales, all lagged two years; included in specifications 2, 5, and 6). Lagged2 Expected Subsidy is the expected subsidy amount, defined in Section 2, lagged two years and measured in thousand Euro. Standard errors are two-way clustered at the region-year and firm levels and are reported below the coefficients. The first year the firm appears in the sample is dropped from all regressions. The number of firms and observations excludes singletons. * indicates 10% significance; ** 5% significance; *** 1% significance.

	OLS		IV-2SLS			
			First Stage	Second Stage	First Stage	Second Stage
	1	2	3	4	5	6
Lagged Temp	0.0597***	0.0519***		0.149**		0.168**
	(0.0118)	(0.0132)		(0.0582)		(0.0766)
Lagged2 Expected Subsidy			-0.0377***		-0.0269***	
			(0.00589)		(0.00513)	
Firm-level controls	No	Yes	No	No	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	2244	2029	2226	2226	2013	2013
Observations	17679	15091	16889	16889	14348	14348
Within R^2	0.027	0.032	0.150		0.128	
F-statistic			41.06		27.56	

Table V. Capital Structure and Employment Flexibility: Robustness

This table reports the results of estimating the following specification using the IV-2SLS framework:

$$D_{it}=\alpha_{rt}+\beta Temp_{it-1}+X_{it-2}'\gamma+\eta_i+\varepsilon_{it},$$

where D_{it} is the leverage (ratio of total debt to assets) of firm *i* in year *t*, $Temp_{it-1}$ is its proportion of workers on temporary contracts, lagged one year, α_{rt} are region-year fixed effects, η_i are firm fixed effects, and X_{it-2} are firm-level controls (log of sales, operating profit margin, and R&D expenses over sales, all lagged two years; included in specifications 1 to 4). Lagged2 Expected Subsidy is the expected subsidy amount, defined in Section 2, lagged two years and measured in thousand Euro. Specifications 1 and 2 (3 and 4) additionally include firm-level control for tangibility (average wage). Specifications 5 and 6 additionally include region-year-industry fixed effects. Specifications 7 and 8 estimate the results using only the firms that are present in the sample in 1994. Standard errors are two-way clustered at the region-year and firm level and are reported below the coefficients. The first year the firm appears in the sample is dropped from all regressions. The number of firms and observations excludes singletons. * indicates 10% significance; ** 5% significance; *** 1% significance.

	IV-2SLS							
	First Stage	Second Stage	First Stage	Second Stage	First Stage	Second Stage	First Stage	Second Stage
	Add Tangibility		Add Average Wage		Additional FE		Originally-sampled firms	
	1	2	3	4	5	6	7	8
Lagged Temp		0.160*		0.168**		0.141**		0.188**
		(0.0819)		(0.0766)		(0.0648)		(0.0788)
Lagged2 Expected Subsidy	-0.0261***		-0.0269***		-0.0375***		-0.0408***	
	(0.00511)		(0.00513)		(0.00684)		(0.00634)	
Firm-level controls	Yes	Yes	Yes	Yes	No	No	No	No
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	Yes	No	No	Yes	Yes
Region-industry-year FE	No	No	No	No	Yes	Yes	No	No
Number of firms	1977	1977	2013	2013	2226	2226	1549	1549
Observations	13964	13964	14348	14348	16889	16889	12843	12843
Within R^2	0.128		0.128		0.311		0.151	
F-statistic	26.00		27.56		30.17		41.45	

Table VI. Capital Structure and Employment Flexibility: Cash-Abundant Firms

This table reports the results of estimating the following specification using the IV-2SLS framework for different subsamples of firms:

$$D_{it}=\alpha_{rt}+\beta Temp_{it-1}+X_{it-2}'\gamma+\eta_i+\varepsilon_{it}\ ,$$

where D_{it} is the leverage (ratio of total debt to assets) of firm *i* in year *t*, $Temp_{it-1}$ is its proportion of workers on temporary contracts, lagged one year, α_{rt} are region-year fixed effects, η_i are firm fixed effects, and X_{it-2} are firm-level controls (log of sales, operating profit margin, and R&D expenses over sales, all lagged two years; included in specifications 2, 4, 6, and 8). Specifications 1 and 2 (3 and 4) estimate the above specification for firms with cash flow above industry median in year t-2 (t). Specifications 5 and 6 estimate the above specification for firms with cash flow over total assets above industry median in year t-2. Specifications 7 and 8 estimate the above specification for firms with the ratio of cash flows, accumulated over three years, over total assets above industry median in year t. Standard errors are two-way clustered at the region-year and firm level and are reported below the coefficients. The first year the firm appears in the sample is dropped from all regressions. The number of firms and observations excludes singletons. * indicates 10% significance; ** 5% significance; *** 1% significance.

	IV-2SLS							
Sample	Operating Cash Flow above industry median in year t-2		Operating Cash Flow above industry median in year t		Operating Cash Flow / Assets above industry median in year t-2		Accumulated OCF / Assets above industry median in year t	
	1	2	3	4	5	6	7	8
Lagged Temp	0.254***	0.253***	0.222***	0.265**	0.276**	0.277**	0.265**	0.257*
	(0.0886)	(0.0892)	(0.0806)	(0.121)	(0.114)	(0.114)	(0.130)	(0.132)
Firm-level controls	No	Yes	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	1463	1457	1632	1467	1527	1520	1409	1403
Observations	7151	7093	8364	7082	6894	6847	6896	6847
1st stage F-statistic	27.39	27.86	46.79	29.98	19.26	19.46	17.77	18.58

Table VII. Flexible Labor Force and Firm-Specific Negative Shocks

This table reports the results of estimating the following specification using the OLS framework:

$$Temp_{it}=\alpha_{st}+\lambda NegativeShock_{it}+\eta_i+\varepsilon_{it},$$

where $Temp_{it}$ is the proportion of workers on temporary contracts, α_{st} are industry-year fixed effects, $NegativeShock_{it}$ is the indicator variable that equals 1 if the firm reports that the market for its main product is in recession, and 0 otherwise, and η_i are firm fixed effects. Specification 2 additionally includes region-industry-year fixed effects. Specification 3 estimates the results using firms with one product only. Specification 4 estimates the results using firms with one product only that have less than 5% of the market share in that product. Standard errors are two-way clustered at the region-year and firm level and are reported below the coefficients. The first year the firm appears in the sample is dropped from all regressions. The number of firms and observations excludes singletons. * indicates 10% significance; ** 5% significance; *** 1% significance.

	OLS			
Sample	All firms	All firms	Firms with only one product	Firms with only one product and a small share in it
	1	2	3	4
Negative Shock	-0.0172***	-0.0174***	-0.0206***	-0.0174*
	(0.00330)	(0.00370)	(0.00536)	(0.00889)
Firm FE	Yes	Yes	Yes	Yes
Industry-year FE	Yes	No	Yes	Yes
Region-industry-year FE	No	Yes	No	No
Number of firms	2290	2290	1140	338
Observations	18340	18340	8737	3346
Within R^2	0.111	0.253	0.108	0.135

Table VIII. Capital Structure and Employment Flexibility: Cross-Sectional Heterogeneity in Bankruptcy Costs

This table reports the results of estimating the following specification using the IV-2SLS framework:

$$D_{it}=\alpha_{rt}+\beta^{H}High_{i0}Temp_{it-1}+\beta^{L}Low_{i0}*Temp_{it-1}+X_{it-2}'\gamma+\eta_i+\varepsilon_{it},$$

where D_{it} is the leverage (ratio of total debt to assets) of firm *i* in year *t*, $Temp_{it-1}$ is its proportion of workers on temporary contracts, lagged one year, α_{rt} are region-year fixed effects, η_i are firm fixed effects, and X_{it-2} are firm-level controls (log of sales, operating profit margin, and R&D expenses over sales, all lagged two years; included in specifications 3, 4, 7 and 8). $High_{i0}$ is the dummy variable that equals 1 if the firm is classified as a high bankruptcy cost firm, defined as having no buildings and land on its balance sheet (columns 1 to 4) or as having less buildings and land that the industry median (columns 5 to 8), both in the year the firm enters the data, and 0 otherwise. Low_{i0} is equal to 1-$High_{i0}$. Lagged2 Expected Subsidy is the expected subsidy amount, defined in Section 2, lagged two years and measured in thousand Euro. Standard errors are two-way clustered at the region-year and firm level and are reported below the coefficients. The first year the firm appears in the sample is dropped from all regressions. The number of firms and observations excludes singletons. * indicates 10% significance; ** 5% significance; *** 1% significance.

	IV-2SLS							
	First Stage 1	Second Stage 2	First Stage 3	Second Stage 4	First Stage 5	Second Stage 6	First Stage 7	Second Stage 8
$High_{i0}$ * Lagged Temp		0.295**		0.378**		0.349***		0.445***
		(0.119)		(0.168)		(0.116)		(0.162)
Low_{i0} * Lagged Temp		0.0717		0.0664		0.0357		0.0138
		(0.0648)		(0.0851)		(0.0808)		(0.104)
$High_{i0}$ * Lagged2 Expected Subsidy	-0.0287***		-0.0193***		-0.0273***		-0.0183***	
	(0.00601)		(0.00542)		(0.00569)		(0.00494)	
Low_{i0} * Lagged2 Expected Subsidy	-0.0404***		-0.0312***		-0.0437***		-0.0338***	
	(0.00601)		(0.00544)		(0.00644)		(0.00560)	
Firm-level controls	No	No	Yes	Yes	No	No	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	2074	2074	1882	1882	1755	1755	1584	1584
Observations	15858	15858	13484	13484	13145	13145	11135	11135
F-statistic	12.27		7.07		11.95		7.23	
Implied estimate of the difference		0.223*		0.312*		0.314**		0.431**
standard deviation of the difference		(0.128)		(0.178)		(0.136)		(0.183)

Table IX. Capital Structure and Employment Flexibility: Firms in Liquidation

This table reports the results of estimating the following specification using the IV-2SLS framework:

$$D_{it}=\alpha_{rt}+\beta^{S}Stay_{i}Temp_{it-1}+\beta^{E}Exit_{i}*Temp_{it-1}+X_{it-2}'\gamma+\eta_{i}+\varepsilon_{it}\ ,$$

where D_{it} is the leverage (ratio of total debt to assets) of firm *i* in year *t*, $Temp_{it-1}$ is its proportion of workers on temporary contracts, lagged one year, α_{rt} are region-year fixed effects, η_i are firm fixed effects, and X_{it-2} are firm-level controls (log of sales, operating profit margin, and R&D expenses over sales, all lagged two years; included in specifications 3, 4, 7 and 8). $Exit_i$ is the dummy variable that equals 1 if the firm exits the data by the end of the sample (due to liquidation or switching to non-manufacturing activity), and 0 otherwise. $Stay_i$ is equal to 1-$Exit_i$. Lagged2 Expected Subsidy is the expected subsidy amount, defined in Section 2, lagged two years and measured in thousand Euro. Specifications 5 to 8 estimate the results using only the firms that are present in the sample in 1994. Standard errors are two-way clustered at the region-year and firm level and are reported below the coefficients. The first year the firm appears in the sample is dropped from all regressions. The number of firms and observations excludes singletons. * indicates 10% significance; ** 5% significance; *** 1% significance.

	IV-2SLS							
	First Stage	Second Stage	First Stage	Second Stage	First Stage	Second Stage	First Stage	Second Stage
	All firms				Originally-sampled firms			
	1	2	3	4	5	6	7	8
$Stay_i$ * Lagged Temp		0.204***		0.220***		0.255***		0.300***
		(0.0589)		(0.0797)		(0.0824)		(0.105)
$Exit_i$ * Lagged Temp		-0.0596		-0.0316		-0.0773		-0.0730
		(0.120)		(0.141)		(0.154)		(0.177)
$Stay_i$ * Lagged2 Expected Subsidy	-0.0376***		-0.0268***		-0.0397***		-0.0297***	
	(0.00653)		(0.00583)		(0.00668)		(0.00581)	
$Exit_i$ * Lagged2 Expected Subsidy	-0.0298***		-0.0242***		-0.0336***		-0.0260***	
	(0.00573)		(0.00570)		(0.00725)		(0.00724)	
Firm-level controls	No	No	Yes	Yes	No	No	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	2226	2226	2013	2013	1755	1755	1584	1584
Observations	16889	16889	14348	14348	12843	12843	11061	11061
F-statistic	14.60		14.21		11.34		8.06	
Implied estimate of the difference		0.264**		0.252*		0.333**		0.373**
standard deviation of the difference		(0.122)		(0.144)		(0.161)		(0.182)

Table A.1. Capital Structure and Subsidies: Reduced Form Analysis

This table reports the results of the following specification:

$$D_{it}=\alpha_{rt}+\beta ExpectedSubsidy_{it-2}+X_{it-2}'\gamma+\eta_i+\varepsilon_{it},$$

where D_{it} is the leverage (ratio of total debt to assets) of firm *i* in year *t*, Expected $Subsidy_{it-2}$ is the expected subsidy amount, defined in Section 2, lagged two years and measured in thousand Euro, α_{rt} are region-year fixed effects, η_i are firm fixed effects, and X_{it-2} are firm-level controls (log of sales, operating profit margin, and R&D expenses over sales, all lagged two years; included in specifications Table 4 Col 6 and Table 5 Col 2 and Col 4). Specification Table 5 Col 2 (Col 4) additionally includes firm-level control for tangibility (average wage). Specification Table 5 Col 6 includes region-year-industry fixed effects. Specification Table 5 Col 8 estimates the results using only the firms that are present in the sample in 1994. Standard errors are two-way clustered at the region-year and firm level and are reported below the coefficients. The first year the firm appears in the sample is dropped from all regressions. The number of firms and observations excludes singletons. * indicates 10% significance; ** 5% significance; *** 1% significance.

	Table4		Table5			
	Col 4	Col 6	Col 2	Col 4	Col 6	Col 8
Lagged2 Expected Subsidy	-0.00563**	-0.00453*	-0.00418*	-0.00452*	-0.00528*	-0.00768**
	(0.00241)	(0.00230)	(0.00238)	(0.00230)	(0.00296)	(0.00331)
Firm-level controls	No	Yes	Yes	Yes	No	No
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	Yes	No	Yes
Region-year-industry FE	No	No	No	No	Yes	No
Number of firms	2226	2013	1977	2013	2226	1549
Observations	16889	14348	13964	14348	16889	12843
Within R^2	0.0251	0.0306	0.0311	0.0306	0.192	0.0281