DISCUSSION PAPER SERIES

DP15819

Employment Flexibility and Capital Structure: Evidence from a Natural Experiment

Olga Kuzmina

FINANCIAL ECONOMICS

LABOUR ECONOMICS

CEPR

ISSN 0265-8003

Employment Flexibility and Capital Structure: Evidence from a Natural Experiment

Olga Kuzmina

Discussion Paper DP15819
Published 16 February 2021
Submitted 12 February 2021

Centre for Economic Policy Research
33 Great Sutton Street, London EC1V 0DX, UK
Tel: +44 (0)20 7183 8801
www.cepr.org

Employment Flexibility and Capital Structure: Evidence from a Natural Experiment

Abstract

Exploiting the variation in labor-market programs in Spain, I show that the use of flexible (shorter and cheaper-to-fire) employment contracts increases a firm's debt capacity. A thought experiment of prohibiting an average firm from hiring workers on flexible contracts suggests that such a firm should reduce its debt-to-capital ratio by 7%. I further nail down the employment flexibility mechanism behind this effect, which works through reductions of the firm's operating leverage and the fixity of its costs. I use specific institutional features to separate this explanation from differences in wages or labor bargaining power. I show that the effects are stronger for firms suffering most in bankruptcy and that in downturns, firms downsize using flexible labor, implying that the employment-contract structure is a significant component of operating flexibility and expected default costs. Finally, employment flexibility increases firm value for firms that benefit most from operating leverage reductions and depend more on external financing. The results demonstrate how management can use heterogenous labor contracts to improve firm outcomes. Most broadly, the results emphasize the importance of studying operating strategy and organizational structure as integral determinants of the financing decisions of firms and highlight the complementarities between CEOs' and CFOs' decision-making.

JEL Classification: D22, G32, J41

Keywords: Capital Structure, Fixed-term contracts, Operating flexibility, Operating Leverage

Olga Kuzmina - okuzmina@nes.ru
New Economic School and CEPR

Employment Flexibility and Capital Structure: Evidence from a Natural Experiment*

Olga Kuzmina
New Economic School and CEPR

This version: January 31, 2021

Abstract

Exploiting the variation in labor-market programs in Spain, I show that the use of flexible (shorter and cheaper-to-fire) employment contracts increases a firm's debt capacity. A thought experiment of prohibiting an average firm from hiring workers on flexible contracts suggests that such a firm should reduce its debt-to-capital ratio by 7%. I further nail down the employment flexibility mechanism behind this effect, which works through reductions of the firm's operating leverage and the fixity of its costs. I use specific institutional features to separate this explanation from differences in wages or labor bargaining power. I show that the effects are stronger for firms suffering most in bankruptcy and that in downturns, firms downsize using flexible labor, implying that the employment-contract structure is a significant component of operating flexibility and expected default costs. Finally, employment flexibility increases firm value for firms that benefit most from operating leverage reductions and depend more on external financing. The results demonstrate how management can use heterogenous labor contracts to improve firm outcomes. Most broadly, the results emphasize the importance of studying operating strategy and organizational structure as integral determinants of the financing decisions of firms and highlight the complementarities between CEOs' and CFOs' decision-making.

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

JEL codes: D22, G32, J41

---

* This paper draws from Chapter 2 of my doctoral dissertation at Columbia GSB, also circulated under the title "Operating Flexibility and Capital Structure: Evidence from a Natural Experiment". I would like to thank Ignacio García-Pérez for sharing data on subsidies. I am also obliged for many conversations and suggestions to Kenneth Ahern (discussant), Sven Arnold, Efraim Benmelech (discussant), Patrick Bolton, Vicente Cunat, Juan Dolado, Andrew Ellul, Guido Friebel, Maria Guadalupe, Irina Ivashkovskaya (discussant), Wei Jiang, David Matsa (discussant), Thomas Noe, Sarmistha Pal, Daniel Paravisini, Francisco Perez-Gonzalez (discussant), Michael Roberts, Bernard Salanié, Carsten Sprenger, Sergey Stepanov, Catherine Thomas, Vikrant Vig, Daniel Wolfenzon, to seminar participants at Columbia, Goethe University Frankfurt, New Economic School, University of Maryland, University of Surrey, University of Cambridge, CERGE-EI, and to participants of the 2014 American Finance Association meeting in Philadelphia, 1st CSEF Conference on Finance and Labor, 2013 Financial Intermediation Research Society meeting in Dubrovnik, 2012 December International Paris Finance meeting, 2012 International Finance and Banking Society meeting in Valencia, and the NES 20th Anniversary Conference. All remaining errors are my own. Email for correspondence: okuzmina@nes.ru; http://pages.nes.ru/okuzmina. Address: 45 Skolkovskoe shosse, Moscow 121353, Russia.

Employment Flexibility and Capital Structure: Evidence from a Natural Experiment

This version: January 31, 2021

Abstract

Exploiting the variation in labor-market programs in Spain, I show that the use of flexible (shorter and cheaper-to-fire) employment contracts increases a firm's debt capacity. A thought experiment of prohibiting an average firm from hiring workers on flexible contracts suggests that such a firm should reduce its debt-to-capital ratio by 7%. I further nail down the employment flexibility mechanism behind this effect, which works through reductions of the firm's operating leverage and the fixity of its costs. I use specific institutional features to separate this explanation from differences in wages or labor bargaining power. I show that the effects are stronger for firms suffering most in bankruptcy and that in downturns, firms downsize using flexible labor, implying that the employment-contract structure is a significant component of operating flexibility and expected default costs. Finally, employment flexibility increases firm value for firms that benefit most from operating leverage reductions and depend more on external financing. The results demonstrate how management can use heterogenous labor contracts to improve firm outcomes. Most broadly, the results emphasize the importance of studying operating strategy and organizational structure as integral determinants of the financing decisions of firms and highlight the complementarities between CEOs' and CFOs' decision-making.

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

JEL codes: D22, G32, J41

1 Introduction

The seminal work of Modigliani and Miller (1958) established the irrelevance of financing structure in a perfect market, implying that firms can make operational and financial decisions independently. The introduction of market imperfections, such as the bankruptcy costs of debt, opens up the scope for the interdependence between the organizational structure of a firm and its financial strategy. Given that about 60% of output pertains to labor compensation (the so called "labor share"), it is important to understand how the structure of labor contracts affects the financing decisions of firms, and what the value implications of this interdependence are. My paper uses a unique natural experiment to causally show that firms that employ workers on more flexible (shorter and cheaper-to-terminate) contracts use more debt financing. I further nail down the employment flexibility mechanism behind this effect, which works through reductions of the firm's operating leverage and the fixity of its costs, which in turn increase debt capacity.[1]

A large literature has explored the interactions between labor and corporate finance in general (see a survey by Pagano and Volpin, 2008), and capital structure in particular (see the most recent survey by Matsa, 2018). However, isolating the pure employment flexibility effect of any labor arrangement or regulation is not trivial. The first and more obvious reason is that firms choose real and financial policies jointly as part of their value-maximization objective, so that one always has to tackle the endogeneity problem. The second and less obvious reason is that, besides employment flexibility, there are at least two other competing mechanisms that can *causally* link labor arrangements to capital structure.[2] In the context of employment protection and firing costs, provided by the different types of labor contracts, these two alternative mechanisms are reductions in operating leverage due to total labor cost (rather than its fixed-to-variable composition), and changes in the bargaining power of labor that make firms adjust debt due to strategic considerations. By addressing both of these empirical challenges, my paper is the first to quantify the effect of employment flexibility on capital structure, to isolate it from other competing mechanisms, and to demonstrate

[1] See Van Horne (1977), Dotan and Ravid (1985), and Mauer and Triantis (1994) for seminal contributions on this operating-financial leverage substitution hypothesis.

[2] For example, a parallel strand of the unionization literature demonstrates that exogenous shocks to unionization affect both the operating flexibility of firms (as in e.g. Chen et al., 2011) and the bargaining power of labor (Matsa, 2010).

its value implications for firms.

Specifically, using a panel dataset of manufacturing firms in Spain and exogenous variation provided by active labor market programs, I show that firms that employ workers on more flexible (shorter and cheaper-to-terminate) contracts use more debt financing in their capital structure. A thought experiment of prohibiting an average firm from hiring workers on flexible contracts (i.e. reducing their proportion from the unconditional average of 23.9% to 0%) suggests that such a firm should reduce its debt-to-capital ratio by 4 percentage points, which corresponds to about 7% of the average debt-to-capital ratio across firms, on top of the usual leverage correlates. Furthermore, flexible employment explains about one-sixth of the variability in debt-to-capital ratio, in terms of within-firm standard deviations.

I further show that this effect is explained by flexible labor contracts reducing a firm's operating leverage (the fixity of its costs) and making its wage claim more variable and contingent on the realization of demand shocks. First, I provide micro-level evidence that workers on temporary contracts are used as a margin of the labor force adjustment during adverse business conditions, showing that these contracts are not only de jure, but also de facto more flexible employment arrangements in practice. Then, by exploring cross-sectional heterogeneity in the severity of bankruptcy costs, I show that the effect of flexible employment contracts on capital structure is most pronounced for firms that would suffer the most in case of financial distress. These two pieces of evidence show that the lack of flexibility afforded to firms by their labor contract composition is an economically important component of expected default costs, and also of operating leverage, which in turn crowds out financial leverage.

To paint a broader picture of the effects of flexible labor contracts on firms' financial policies, I explore how several other variables related to financing are affected by labor contract composition. I find that an increase in the share of temporary contracts in a firm leads to both a significantly higher net flow of credit and a slightly higher net flow of equity (rather than, for instance, firms swapping debt for equity), consistent with the interpretation that flexible employment contracts also relax some of the firms' financing constraints. I also find suggestive evidence that employment flexibility also allows firms to depend less on external financing, since empirically they are less likely to be part of a group of companies and no more likely to be traded on an exchange.

Finally, I demonstrate the value implications of flexible employment contracts that stem from their ability to reduce operating leverage. I show that while the average effect of having flexible contracts is statistically zero, it is positive for firms that should value this channel the most (the high-bankruptcy-cost firms and firms that depend more on external financing), but not the others. This heterogeneity demonstrates a novel channel of how flexible contracts can improve firm outcomes – the one through reducing operating leverage and helping to overcome financing constraints.

Illustrating these causal links between employment flexibility, capital structure, and value, is important both for company management and for policy makers. For company management, they highlight the complementarity between CEOs' and CFOs' decision-making. They also show how firms can use heterogenous labor contracts to positively affect firm outcomes. For policy makers, they show that employment protection policies that affect the employment flexibility of firms have additional unintended effects through their connection with the financing side of the firm: even if the average firm doesn't lose when hiring workers on longer contracts, the most financially constrained firms do, potentially affecting the industry composition and hurting allocative efficiency.

The European labor market turns out to be a particularly good candidate for testing the effect of employment flexibility on capital structure. Partly compensating for rigid labor markets, various European countries naturally use two types of employment contracts – permanent (regular indefinite) and temporary (fixed-term) contracts – that differ dramatically in terms of the employment flexibility that they provide for firms. In particular, temporary employment contracts have a shorter duration and are much less costly to terminate than permanent contracts. This dual nature of the labor market in principle allows for the measurement of contract composition at the firm level, and I use a unique dataset that contains this information. While many countries have a dual labor market of this or a similar sort, I focus on Spain because several of its institutional features allow me to cleanly estimate the effect of a flexible employment strategy on the financing decisions of firms, and overcome the two main empirical challenges that arise in this context.

The first challenge that arises whenever one looks at the effects of any operating strategy (and employment structure, in particular) on financing decisions is that firms choose their real and financial policies jointly as part of their value-maximization objective. I overcome this endogeneity problem by using the first institutional feature: the Spanish government has implemented a series

of reforms since 1997 that provided incentives to firms to convert workers on the more flexible (temporary) contracts into the less flexible (permanent) contracts. These incentives were different over time, across regions, and across worker characteristics (such as gender).

Using all these detailed layers of variation, I construct an instrumental variable that measures the time-varying incentives to employ a less flexible contract mix for each firm, based on the combination of its location, industry and pre-reform employment patterns. I instrument the firm-level share of flexible employment contracts with it. Importantly, because my instrument picks conversions of contracts with the *same* employees, rather than a change in the composition of workers, I can ensure that other structural characteristics of the labor force (such as female composition or skill intensity) do not change in this experiment.

Such a detailed level of variation also allows me to include a battery of fixed effects, most of which are impossible in studies that use aggregate (cross-country or cross-industry) variation. Specifically, besides firm fixed effects (capturing time-invariant firm heterogeneity), I also include region-year fixed effects (capturing macroeconomic effects, region-specific fiscal budgets, unemployment rates, and cost of financing, etc.), industry-year fixed effects (capturing unobserved industry shocks that could affect both the financing and labor policies of firms), and even region-industry-year fixed effects in my tightest specifications (capturing region-industry-specific lobby power, differential trends in female employment, and many other possible confounders).

The second challenge is that the employment flexibility effect that works through reductions in operating leverage is not the only mechanism of how an employment protection regulation or changes in firing costs can affect capital structure. The other two that have been broadly discussed in the literature are wage differentials and strategic bargaining considerations. Specifically, if workers need to be compensated for the higher unemployment risk associated with more flexible contracts, then this higher wage bill would decrease the debt capacity of firms (Agrawal and Matsa, 2013). On the other hand, if less protected workers, instead of a compensating differential, receive lower wages (as predicted by insider-outsider labor models, going back to Lindbeck and Snower, 1984, or because they invest less in human capital, as in Wasmer, 2006), then firms can increase debt ratios to capture more tax benefits. Given that the wage differential can spur the direct effect of employment flexibility, I use another institutional feature of Spanish labor law. It explicitly

prohibits any discrimination of workers based on their contract type, so firms with different shares of flexible contracts will face the same average wage, and this channel of impact is effectively shut down.[3]

The second potential channel works through the strategic use of debt. In this theory, firms issue debt to make it harder for employees to capture parts of the surplus in the form of wages or job security, and a large literature has found evidence of such behavior.[4] The most natural prediction of this theory is that firms should issue less debt for strategic purposes when the bargaining power of labor is low (e.g. when workers are employed on flexible contracts). However, as Simintzi, Vig, and Volpin (2010) show mathematically, the opposite conclusion arises once debt contracts are renegotiable: the lower bargaining power of labor makes firms issue *more* debt. Thus, a prediction of their model is that firms with more flexible contracts (as well as firms that face less employment protection or lower firing costs) can have more debt due to strategic bargaining considerations. I unbundle this channel by using another institutional feature of Spain. In Spain, collective bargaining agreements are generally set at levels above the firm and equally cover all firms and all workers irrespective of the contract within these firms. This makes the bargaining power of labor at the firm level orthogonal to the firm's contract composition, effectively minimizing any strategic-debt effects.

My paper contributes to three strands of the literature. The first contribution is to use a novel firm-level contract-based flexibility measure and show that employment flexibility increases financial leverage. As such, I build on the recently growing literature on the effects of employment protection on capital structure. Specifically, Simintzi, Vig, and Volpin (2015) explore the changes in employment protection legislation in a panel of European countries, while Serfling (2016) uses the earlier methodology of Autor, Kerr, and Kugler (2007) to explore the passage of the "good-faith exception" law in the U.S. They show that firms decrease leverage in countries and states

[3] I additionally test for the absence of wage effects in my data, and use this variable as a control.

[4] Within this paradigm, debt has been shown to reduce the bargaining power of labor by lowering the available cash flow (e.g. Matsa, 2010, Agrawal and Matsa, 2013, Ellul and Pagano, 2017, and earlier contributions by Bronars and Deere, 1991, Dasgupta and Sengupta, 1993, Perotti and Spier, 1993), and to discipline firms' relations with workers by allowing for more frequent dismissals and reliance on part-time and seasonal labor (Hanka, 1998). Additionally, Benmelech et al. (2012) have recently provided empirical evidence of firms renegotiating labor contracts to extract concessions from labor during times of financial distress. Their evidence can be used to illustrate this labor bargaining channel.

that pass employment protection laws. I build on this literature and make three new important findings. First, by applying a natural experiment to the firm-level composition of contracts, I can estimate the quantitative magnitude of the effect of interest, rather than just the sign. This means that my estimates can be used to quantitatively predict the effect on capital structure through the channel of employment flexibility, of *any* future labor-related policy that affects the share of flexible contracts. From a pure identification perspective, it also means that I do not have to rely on any assumptions between aggregate regulation and a firm's actual employment choices, because I observe these choices directly and see if they are affected by the regulation or not.[5] Second, by using the specific institutional features of Spain, I can isolate the employment flexibility effect from strategic bargaining considerations or the effects of total labor costs. And third, I demonstrate that this employment flexibility channel increases firm value through its ability to reduce operating leverage and help firms to overcome financial constraints. From a much broader perspective, my paper is thus part of a larger literature that has focused on the effects of labor policies on firms' real decisions and outcomes.[6]

My paper also contributes to the literature that has modeled various management strategies that a firm can undertake to actively increase its operating flexibility, and their implications for the risk of the firm and its financial policies (including capital structure and hedging behavior). Some interesting examples include flexible production (e.g. in the form of choosing technology with lower costs of shutting down and reopening a plant, as in Mauer and Triantis, 1994, or Reinartz and Schmid, 2016), flexible pension funding (in the form of adopting a defined contribution, rather than a defined benefit, pension plan, as in Petersen, 1994), flexible pricing (D'Acunto et al., 2018), as well as using various operational hedges (e.g. in the form of geographically dispersed production, as in Allayannis et al., 2001, or matching destinations of imports and exports, as in Kuzmina and Kuznetsova, 2018). The persistence of such management strategies implies that it is often hard to find exogenous shocks to firm-level flexibility for identification purposes. My paper adds to

[5] In my setup, firms with a higher proportion of flexible contracts will have unambiguously higher employment flexibility both by definition of these contracts, and also empirically. This is in contrast to aggregate measures of employment protection that, although often used in the literature, do not necessarily represent this particular type of employer costs (Myant and Brandhuber, 2017).

[6] Examples include: 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).

this literature by considering flexibility related to a firm's choice of employment contracts, and by using firm-level exogenous shocks to trace the causal effects. Additionally, by exploring the value implications, I emphasize how management strategies that promote operating flexibility can improve firm outcomes.

Finally, I extend prior work that has developed quantitative measures of operating leverage and operating flexibility and illustrated how these measures relate to financial structure. Because firms do not disclose costs on the basis of whether they are fixed or variable in their financial statements, they have been typically quantified using more indirect approaches, such as estimating the sensitivity of EBIT to sales (Mandelker and Rhee, 1984), growth of costs to growth of sales (Kahl et al, 2012), the share of SG&A expenses to assets (Chen et al, 2017), or to operating costs (Du et al, 2012), or more structurally, by calibrating the elasticities of substitution between different inputs and different products, and shadow rents of buildings, machinery, and workforce (MacKay, 2003). My paper contributes to this literature by using the firm-level mix of temporary and permanent contracts, which maps almost axiomatically to the notions of variable and fixed costs in the firm's cost structure. I also provide greater resolution to these studies by showing that the structure of employment contracts is an economically important firm fundamental that explains about 17% of the sensitivity of EBIT to sales.

From the broadest perspective, I relate to the literature studying the determinants of capital structure choice (e.g. Titman and Wessels, 1988, Rajan and Zingales, 1995), especially in the framework of natural experiments (e.g. Vig, 2013, among others). 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), my findings highlight the importance of studying operating strategy and organizational structure as integral determinants of the financing decisions of firms.

Although there are clear benefits of using the institutional setup of Spain in terms of precisely identifying the effect of interest, one may be concerned about the external validity of my results. While the U.S. labor market is mostly characterized by "employment at will", continental European countries, including Spain, lie at the other end of the spectrum and have much more stringent employment protection. In order to counterbalance these laws and achieve flexibility "at the margin",

firms in these countries hire non-trivial shares of workers on temporary contracts, implying that the employment flexibility channel described in my paper is relevant to them as well.[7] However, as long as other institutional features related to collective bargaining and wage differentials are different in countries other than Spain, hiring workers on temporary contracts may have additional effects on top of the pure flexibility effect there, affecting the generalizability of the magnitudes. Finally, the original models of a two-tier labor market were developed in the U.S. context (see Saint-Paul, 1996, for an overview). In these models, which largely contrast "good jobs" versus "bad jobs", the upper tier is characterized by high wages and high employment security, and the lower tier by low wages and low employment security. Within this broader view of a two-tier market, the main message of my paper could in principle apply to any other composition of employment arrangements that provide different degrees of flexibility, such as formal versus informal labor markets, with the latter accounting for a substantial 20% even in a country like the U.S. (Bracha and Burke, 2016).

The rest of the paper proceeds as follows: Section 2 outlines the details of the institutional environment; Section 3 builds on the identification strategy and describes the data; Section 4 presents the empirical results of the effect of employment flexibility on capital structure; Section 5 provides evidence of the operating-financial leverage mechanism behind these results; Section 6 analyzes the value implications of employment flexibility; Section 7 concludes and discusses policy implications.

2 Institutional Environment

2.1 Dual Labor Market, Collective Bargaining, and Firing Costs

A dual labor market consisting of workers who are characterized by different degrees of job security exists in many countries, either informally (with "under-the-table" payments) or formally (with different legal contractual arrangements with employees).[8] Spain provides an excellent laboratory

[7] Similarly to Spain, Italy, the Netherlands, Belgium, Portugal, and Sweden liberalized the use of temporary contracts in 1980s-1990s. The Netherlands and Portugal, as well as Poland, have very high temporary employment rates similar to those in Spain (above 20%, see OECD, 2017), and even the largest EU economies – Germany, France and Italy – each express significant shares of temporary employment (13%, 16%, and 14% respectively).

[8] Although there is a debate on whether the U.S. has a dual labor market (see Saint-Paul, 1996, for an overview), the original models of the two tiers within regular employment were developed in the context of the U.S. As for

to study the effects of the structure of labor contracts on the financing decisions of firms for multiple reasons. Here I present those that are the most relevant for the channel of operating flexibility, while I refer the reader to Jimeno and Toharia (1994) and Dolado et al. (2002) for a more extensive overview of the institutional details.

First of all, Spain has a formal dual labor market, which allows one to accurately measure the composition of the labor force in terms of employment contracts. Second, temporary contracts are not an artifact and are commonly used in practice there. In fact, the level of temporary employment in Spain (at 26% of all salaried workers as of 2017) has been the highest among the European countries for a long time, followed by Poland and the Netherlands (OECD, 2017). It is still not too far from the European average of 14.2%, suggesting that temporary employment contracts are important building blocks of the labor market system in Europe.

Finally, and more importantly, particularly in Spain, these two types of contracts are similar in all dimensions except for the associated implicit firing cost. First, there are no de facto restrictions on which jobs can be performed by workers on different types of contracts (Bentolila et al, 2012). Second, all workers have a constitutional right to be covered by collective bargaining agreements, independently of union affiliation. These agreements preclude firms from discriminating based on the contract type, stipulating that workers of the same grade are to be paid the same wage and to have the same employment conditions (Jimeno and Toharia, 1994, Izquerido et al, 2003).[9] And third, these agreements are arguably exogenous at the firm level.

This happens for two complementary reasons. The first is the empirical fact that more than 85% of all collective bargaining agreements are set at levels higher than the firm, such as industry provincial or industry national levels (Izquerido et al., 2003), and firm-level policies cannot overrule them. And the second is the principle of the "automatic general applicability" of any collective

non-regular employment, at least 6% of those employed in the U.S. can be classified as "temporary" workers (see Farber, 1999, and Addison and Surfield, 2006). These include agency and direct-hire temporaries, on-call workers, day laborers, and contract employees, and are on top of regular and self-employed workers. Informal employment can be also thought of as having even less (virtually absent) job security, but it is harder to estimate accurately, and it is characterized by even more confounding differences from regular employment besides job security.

[9] There is some empirical evidence that, despite the law, workers on temporary contracts receive lower wages on average (Dolado et al, 2002). However, Davia and Hernanz (2004) demonstrate that such average pay gaps are fully explained by the selection bias of different workers into different types of contracts. My setting is free of such selection bias, and empirically I do not find any difference in average wage (results reported in Internet Appendix Table 8).

agreement settled at a level higher than the company. It means that once the agreement is set, it applies to all firms and all workers equally, irrespective of whether they actually participated in the bargaining process or not (Bentolila et al, 2012). Thus, while it is entirely possible that there are differences in labor bargaining power at the macro level (across industries or over time), the above features imply this is not the case at the micro level of the firm.[10] Hence, the extent to which firm-level changes in the proportion of temporary contracts may affect the bargaining power of labor within a firm, and the firm's subsequent strategic choice of leverage, are severely limited in this institutional setting. This is an important distinction from previous setups, as it makes sure that my empirical findings will not accidentally pick up the effects of labor on capital structure that work through strategic labor bargaining channels.

The difference in firing costs between the two types of contracts is, on the other hand, very large. Permanent contracts are indefinite and are subject to an advance notice period of 1 month in case of dismissal. They entitle an employee to severance pay of 20 to 45 days of wages, depending on whether the dismissal is for economic, personal (incompetence), or "unfair" reasons. Due to this right of permanent workers to appeal to the court upon dismissal, and since judges would rule dismissals as unfair in the majority of cases, the effective severance payment was 45 days of wages per year of seniority, with a maximum of 42 months of salary, before 1997, and reduced to 33 days with a maximum of 24 months, afterwards (Kugler et al, 2003).

Temporary contracts, on the other hand, are short-term in nature. When they are terminated at term, a firm pays only 0 to 12 days of wages in severance payments (depending on the type of temporary contract), which cannot be appealed in labor courts.[11] For the earlier years, García-Pérez and Rebollo-Sanz (2009) report that a high share of temporary workers sign a new contract *every week*. The most recent evidence shows that up to 25% of temporary contracts last less than one week, and an additional 10-15% last less than one month (Felgueroso et al., 2018). Thus, by

[10] One situation when the agreement may be considered less exogenous to the firm is when a firm is particularly large in a given industry (and as reported by Izquerido et al, 2003, the largest firms are also more likely to sign firm-level agreements). However, as I show in Internet Appendix Table 1, my results fully hold for subsamples of smaller firms – precisely those that are most likely to be outside of the bargaining process. Hence, it is not the big firms with possibly endogenous bargaining agreements that drive any of my results.

[11] As Dolado et al. (2002) point out, even though severance payments for dismissals of workers under temporary contracts before the termination of the contract have been similar to those applying to permanent contracts, these firing costs are not relevant, since most temporary contracts have very short duration.

using such temporary contracts, firms obtain exceptional flexibility to downsize almost immediately if needed. Alonso-Borrego et al.(2005) show that temporary workers absorb a higher share of the volatility of output, and in Section 5.1, I provide additional micro-level evidence that firms do lay off temporary workers in response to negative shocks. Taken together, Spain is characterized by a dual labor market, with highly protected permanent workers, and highly unprotected temporary workers that provide firms with firing flexibility at the margin (Bentolila et al, 2012).

So why do firms have a mix of employment contracts, given that one type of contract should not in principle dominate another in an equilibrium? Spain's dual labor market originated in the 1984 reform which recognized the need for flexibility in the labor market by extending the applicability of temporary employment contracts. As a result, their use quickly rose to 35% (29% in manufacturing) by 1995. After the reform, almost *all* new hires were in fact on temporary contracts (Guell and Petrongolo, 2007), suggesting that on the margin, firms may have considered one type of contract to dominate the other – at least at the moment of choosing which contract to offer to a new hire.[12] Together with the pre-1984 variation in hiring cycles across firms (e.g. due to the retirement of existing workers), this defined the time-series history-dependent evolution of the share of temporary workers that each firm achieved by the mid-1990s, when the new reforms, reversing the employment liberalization policy, were introduced.

In the long-run, however, the equilibrium share of temporary workers is likely to be determined by the trade-off between firing flexibility on the temporary contract side and higher productivity on the permanent contract side (Blanchard and Landier, 2002), with a few corresponding correlates identified in the literature. In particular, and firms that have a higher demand for flexibility from the product side (e.g. those that are subject to more volatile product demand, as in Abraham and Taylor, 1996, or more competition as in Aparicio-Fenoll, 2015), firms that have less need for firm-specific human capital investment, which requires employment protection (as in Jaggia and Thakor, 1994, or Wasmer, 2006), are likely to have a higher proportion of fixed-term workers on average.

Ultimately, investigating the effects of the composition of labor contracts on the financing de-

[12] Eventually temporary workers have to be converted to permanent contracts (or dismissed), so temporary employment is also hypothesized to work as a "stepping stone" towards regular employment – not only in Spain, but also in the U.S. (Farber, 1999) and the U.K. (Booth et al, 2002).

cisions of firms 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 employment flexibility on the side of the firm, keeping other labor market effects, such as labor bargaining power, 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 their employment flexibility, giving more statistical power in identifying the effect. Finally, the Spanish government has implemented a number of reforms that will help in terms of causal identification.

2.2 Region-Specific Government Labor Policies in Spain

Such labor-market dualism has been shown to have negative effects on the economy (Blanchard and Landier, 2002, for France; Dolado, García-Serrano and Jimeno, 2002, for Spain). And indeed in the late 1990s the Spanish government partially reversed the employment liberalization policy of 1984, and started subsidizing firms for creating permanent contracts, both at the national and regional levels. Since national reform affects all firms equally and at the same time, one would not be able to credibly attribute within-firm changes in employment composition to the effect of the reform, rather than, for example, to some country-level macroeconomic shocks. On the other hand, the reforms at the regional level provide 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.[13]

These regional subsidies were paid to the firm once at the time when new permanent contracts were created, either as a direct transfer to the firm or as a reduction of payroll taxes, per each contract.[14] While the subsidy could be given for both converting existing temporary contracts with

[13] In terms of the regional composition, Spain is one of the most decentralized countries in the OECD: there are 17 autonomous communities, each having its own executive, legislative, and judicial powers; with tax devolution in the country similar to the U.S. (OECD, 2016). The regions are quite diverse, with Catalonia, Madrid, and Andalusia being the largest in terms of both population (about 6-8 mln people each) and GRP (comparable to e.g. Greece), and Cantabria and La Rioja being the smallest (0.3-0.5 mln people each and GRP comparable to e.g. Paraguay).

[14] The scope for manipulation on the part of the firm aimed at obtaining the subsidy without any real changes in employment is limited by "conditionalities", as normally put by any Active Labor Market Program. In the Spanish regional program, 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 subsidized permanent contract creations. Additionally, the law explicitly conditions on maintaining the subsidized worker for at least 2 years (sometimes up to 3 years, depending on the region), and if the firm can't for any reason, then they need to replace her with another (non-

existing workers into permanent ones and for hiring new workers from the unemployed on permanent contracts, in most region-years the two types of subsidies were exactly identical. Therefore, I do not differentiate across the two in my empirical analysis and record the maximum available subsidy value for each region-year-gender. Also, as Guell and Petrongolo (2007) mention, over 90% of the new permanent contracts in 1994-2002 in Spain were conversions from existing temporary contracts, rather than new hires from the unemployed. More precisely, Kugler et al. (2003) find that subsidies were effective in boosting conversions rather than hires from the unemployed. This implies that in my paper I will mostly pick changes in the terms of a contract with the *same* employee, rather than of a change in the composition of employees. This is a cleaner experiment, as it automatically ensures that other structural characteristics of the labor force (such as female composition or skill intensity) do not change in this type of experiment.

I summarize these maximum statutory subsidy amounts that a given firm could receive per contract by region, year, and gender of the worker in Table I.[15] These data were assembled directly from the laws by García-Pérez and Rebollo-Sanz (2009). As can be seen from this table, the time profile of the policies is quite diverse: some regions, such as Andalucia, implemented these subsidies every year from 1997 onwards, some – only in certain years, while Catalonia, for example, 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 just 1653 Euros in the Baleares community to more than 15000 Euros in Madrid per contract. These differences are largely explained by the sizes of regional budgets, different situations in the labor market, and different political orientation of regional governments.

2.3 Debt Market

Putting in a perspective, although the U.S. has a more flexible formal debt market (both ex-ante in the form of better firm-creditor matching, and ex-post in the form of flexible enforcement of covenants), its more regulated nature in Europe (and in Spain in particular) is to a significant

subsidized) permanent contact.

[15] Sometimes it was not clear what this maximum value in Euro 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 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.

extent counterbalanced by firms through, for instance, a wider use of trade credit as a means of financing. In particular, accounts payable constitute about 22-26% of assets in manufacturing firms in countries like France, Spain, Belgium – as calculated by e.g. Garcia-Teruel and Martinez-Solano (2010). Importantly, similar to financial debt, trade credit also involves substantial costs of financial distress that are priced both ex-post (e.g. as termination of a valuable long-term buyer-supplier relation and seizure of collateralized goods, as in Petersen and Rajan, 1997), and ex-ante (e.g. as a high implicit interest rate compensating for the default premium, as in Cuñat, 2007).

The existence of trade credit also makes the overall market for credit more flexible. Specifically, with many firms and suppliers, a better ex-ante buyer-supplier matching would result, while ex-post long-term buyer-supplier relationships would make enforcement of trade credit even more flexible than it would be in a competitive debt market (Wilner, 2000). Accordingly, including trade credit in the measure of leverage would make the Spanish setting more comparable to the U.S. in terms of the debt market flexibility. This follows the approach of Rajan and Zingales (1995), who argue that "in countries, or specific classes of firms which use trade credit as a means of financing, accounts payable should be included in measures of leverage," and this is what I do in the paper.

3 Data and Empirical Strategy

3.1 Data Description and Variables Definition

The results in this paper are based on three sets of data. I combine firm-level data on employment and financials, region-level data on subsidies, 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.

I use the ratio of total debt to total capital as a measure of leverage. It is defined as the sum of total short- and long-term debts (including bonds, debts with financial institutions, debts with affiliated companies, and accounts payable) over total capital (defined as the sum of these debts and book equity).[17] As reported in Table II, around 57% of firm financing comes from debt.[18] Although the survey is anonymous and the data cannot be matched to market values of equity, this does not pose a problem given that most firms are private anyway.

This is a unique dataset in that, on top of the basic balance sheet information and the total number of employees, it contains information on their contracts. In particular, I can directly measure the fraction of workers employed on temporary contracts at an annual basis. As shown in Table II, 269 employees work in an average firm, 24% of whom have temporary contracts in the year the firm enters the data.

Firm size, measured as the natural logarithm of the 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 (defined as the ratio of sales net of purchases and labor expenses, to sales), equals 23%. To proxy for growth opportunities, I also measure research and development intensity defined as the ratio of R&D expenditures over sales. Finally, I use the modified Altman z-score to account for the financial distress status of the firm.[19] These variables are typically found

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

[17] This measure specifically excludes pensions, deferred taxes and other provisions. It does, however, include trade credit, consistent with the argument in Rajan and Zingales (1995), as discussed in Section 2.3. Yet, due to data limitations, it is not possible to separate it out empirically to see which component of debt is potentially more important and sensitive to labor contract composition.

[18] With accounts payable included this number exactly corresponds to the one for the U.S. reported in Rajan and Zingales (1995).

[19] The modified Altman's z-score is $3.3\frac{EBIT}{Total\ Assets} + 1.0\frac{Sales}{Total\ Assets} + 1.2\frac{Working\ Capital}{Total\ Assets} + 1.4\frac{\text{Re}tained\ Earnings}{Total\ Assets}$ (Altman, 1984, MacKie-Mason, 1990). Because ESEE does not separately provide retained earnings, I use the first three components of the z-score as a control. I capture the forth component in the regressions by using various sets of fixed effects. For example, using industry-year fixed effects assumes that this ratio varies similarly across firms in the same industry over time.

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)[20] and average wage (defined as gross wages and salaries, compensation, social security and supplementary pensions contributions, and other social spending, 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 largest plant in terms of employment to merge firm-level data with the data on regional subsidies. Given that 85% of firms have just one plant and an additional 6% of firms have two plants, both in the same region, this constitutes the exact merge for the majority of firms (with the results 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 the expected subsidy per employee (the instrument, defined below in Section 3.4), which are equal to 3523 and 816 Euros, respectively. Given the average yearly wage of about 29 thousand Euros, this corresponds to a one-time subsidy covering about 8 weeks of salary for a worker who was actually converted from temporary to permanent, broadly corresponding to the numbers reported in García-Pérez and Rebollo-Sanz (2009).[21]

Finally, I use the data on the proportion of women and men across all types of workers by industry, as provided by the Spanish Labor Force Survey. These gender intensities will be used to construct the instrument in Section 3.4. They are measured as of the 4th quarter of 1993 and are listed in Table III, showing a considerable cross-industry 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 ratios are quite stable over time, but in order to mitigate endogeneity concerns, they are kept fixed at the pre-sample year

[20] As ESEE records only the total value of depreciation and amortization across all types of assets, I cannot construct a more common measure of tangbility, such as e.g. net fixed assets over total assets. 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 equipment, which is essentially what matters in determining the amount of debt. The results in the paper are, however, robust to allocating all accumulated D&A to tangible assets, as well as proportionally to gross tangible and intangible assets.

[21] The averages reported in Table II correspond to all years from 1994 to 2006, so also include a period when regional subsidies were equal to zero. Conditioning on the period after 1997, these averages become 4288 and 994, respectively. The one-time subsidy thus covers $52 \cdot 4288/28790 = 8$ weeks of salary .

in the analysis.

3.2 Panel Framework

The main hypothesis of the paper is that a more flexible composition of employment contracts increases the debt capacity of a firm, through changing its operating leverage, the corresponding probability of default, and expected bankruptcy costs. Hence I estimate the following relationship:

$$D_{it} = \alpha_{rt} + \alpha_{st} + \beta Temp_{it-1} + X'_{it}\gamma + \eta_i + \epsilon_{it}, \tag{1}$$

where D_{it} is the ratio of total debt to total capital of firm i in year t, α_{rt} are the region-year fixed effects, α_{st} are the industry-year fixed effects, $Temp_{it-1}$ is the proportion of workers on temporary contracts in the prior year[22], X'_{it} are various firm-level control variables included in some specifications to account for firm-specific shocks, and η_i are firm fixed effects.

The panel structure of the dataset allows me to explore what drives within-firm changes in financing decisions by holding constant time-invariant heterogeneity across firms. Some examples include whether the firm in general has a more variable cash flow, whether it is a small business with a distrust of credit and banking, or whether its tasks generally require more human capital specificity.

In addition, including region-year and industry-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, macroeconomic effects driving the cost of financing, or any cross-industry variations over time. Also, 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.

Although the specification in equation (1) accounts for many sources of confounding variation, 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. Below I provide several potential reasons

[22] 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. The results are similar when using contemporaneous values.

for such a correlation, discuss the direction of the bias in the panel OLS estimate, and explain how I address this endogeneity.

3.3 Endogenous Choice of Employment Flexibility

There are many reasons why firms may choose the composition of employment contracts endogenously. One of them is the firm's investment opportunity set, whereby firms that have a large range of uncertain projects can prefer to hire workers under more flexible contract arrangements. At the same time, if investors rationally anticipate potential project substitution, they will supply less debt. Another idea, brought about by Caggese and Cuñat (2008) points out that financially constrained firms may hire more temporary workers, thereby generating a "demand for flexibility". If such firms are also less levered, this would also show up as a spuriously low correlation between flexible labor contracts and debt.

Another important unobserved factor could be the firm's time-varying desire to stimulate human capital investment. Jaggia and Thakor (1994) 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.[23] If the reason for offering permanent employment contracts is the need for firm-specific human capital investment and firms take less debt, we would observe a spurious positive correlation between flexible employment and debt.[24]

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-financial leverage substitution. Then, when they do so in response to bad economic conditions, the debt-to-capital ratio will increase mechanically, since equity in the denominator will be hurt by the same negative shock. This implies that if the hypothesis of this paper is true, then estimating (1) by OLS will bias the coefficient of interest downwards. To this extent, finding that

[23] A similar conclusion is reached by Graham et al (2014), who show that corporate bankruptcy leads to significant earnings losses. Wasmer (2006) also models the idea that that employment protection in the form of longer-term contracts stimulates investment in firm-specific human capital.

[24] 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 alleviate this concern as well.

the magnitude of the panel OLS coefficient is lower than the true effect provides some indirect evidence towards the mechanism of this paper.

3.4 Implementation of the Identification Strategy

To tackle this endogeneity problem, I estimate β in equation (1) through IV-2SLS using government labor policies as the source of variation. Firms were affected by these 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 their region's criteria.

To exemplify the source of identification, consider 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 on temporary contracts in the above example). Notably, this heterogeneity in incentives by gender and contract type exactly mimics the empirical finding from previous studies that statutory subsidies were effective in promoting permanent employment only in the most affected groups, but not in the others, implying that any simple average would hide this substantial heterogeneity. These most affected groups are precisely (middle-aged) women in García-Pérez and Rebollo-Sanz (2009) and all temporary contract conversions (rather than hires from the unemployed) in Kugler et al. (2003).

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 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, political orientation and the 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 the firm's current conditions.[25] In other words, one can use the following expected subsidy amount to predict the shift in a firm's use of temporary labor:

$$ExpectedSubsidy_{it} = \sum_{g} w_{i0}^{T,g} \cdot Subsidy_{grt} \ , \tag{2}$$

where $Subsidy_{grt}$ is the maximum statutory subsidy allowed by the government in region r in year t for a worker of gender $g \in \{\text{female; male}\}$ (as listed in Table I), and $w_{i0}^{T,g}$ 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).[26][27] I also express the subsidy amount in real 2006 Euros by deflating it using the industry-level producer price index. Since the subsidy often reduces the payroll tax to be paid next year, I use its lagged value as the instrument, but the results are robust to using contemporaneous

[25] 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.

[26] ESEE allows me to observe only the overall proportion of temporary workers (which is already an improvement upon other datasets). Therefore, I use industry-level gender composition (assuming it is the same as the firm's) to predict how many female and male temporary workers there are in a firm: $w_{i0}^{T,g} = w_{i0}^{T} \cdot w_{s0}^{g}$, where w_{i0}^{T} is the firm-specific proportion of temporary workers at the year it enters the data and w_{s0}^{g} is the industry-specific use of female and male employees as of pre-sample 1993 year. Even if there is a measurement error involved in this assumption, as long as it is uncorrelated with the error term of the main equation and the first stage is strong, the inference is consistent.

[27] Given that subsidies were also differentiated by worker age in some regions, I have experimented with one more layer of worker heterogeneity – age: $ExpectedSubsidy_{it} = \sum_{ag} w_{iag0}^{T} \cdot Subsidy_{agrt}$, 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.

values of the instrument as well.

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 contracts into permanent contracts. It can be further described as the expected wage bill reduction per employee.[28] As summarized in Table II under "Expected Subsidy per Employee" this expected per-employee wage reduction amounted, on average, to 816 Euros. Although this variable may appear to implicitly assume that all eligible workers are converted, this does not have to be the case for the instrument to work, since it can also be interpreted in the intention-to-treat framework. The expected subsidy plausibly constitutes a valid instrument 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.[29]

4 The Effect of Employment Flexibility on Capital Structure

4.1 Main Results

Before turning to formal analysis, I first use ESEE data to explore the relationship between the fraction of temporary contracts 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-capital ratios than are industries that have a lower fraction of flexible contracts, such as "Chemicals" and "Beverages".

[28] One can argue that firms that are more labor-intensive (e.g. have a higher employment-to-assets ratio) are more likely to be affected in the aggregate, as they can receive a higher total value of the subsidy per dollar of assets. My instrument captures this idea fully, as long as these firms convert proportionately more workers than the less labor-intensive firms, as a result of these higher incentives. Additionally, I find that the effects are also similar when using the dollar value of subsidy per dollar of assets (rather than per employee) as an instrument, or both instruments at the same time (the results are reported in Internet Appendix Table 3).

[29] Given that firms with different predetermined eligibility may still differ along other dimensions that may lead to divergence in firms' behavior over time, I explicitly address these possible violations of the exclusion restriction in Section 4.2.

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 2.2. Interestingly – and consistent with my hypothesis – the drop in temporary employment is also accompanied by a fall in the average debt-to-capital ratio. Although these figures provide interesting suggestive correlations, I now turn to a more systematic regression analysis.

Table IV shows the OLS (columns 1 and 2) and IV-2SLS (columns 3 to 6) 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. All specifications account for time-invariant firm heterogeneity, region-year, and industry-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, macroeconomic effects, or industry shocks over time.

While the OLS coefficients in columns 1 and 2 are positive and statistically significant, I report them for completeness only and move directly to the interpretation of the IV-2SLS estimates in columns 3 to 6.[30]

Columns 3 and 5 report the results of regressing the proportion of temporary employment on the expected subsidy instrument, firm, region-year, and industry-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

$$Temp_{it} = \alpha_{rt} + \alpha_{st} + \delta ExpectedSubsidy_{it-1} + X'_{it}\gamma + \eta_i + \epsilon_{it}, \tag{3}$$

The estimate of δ in column 3 is significant at the 1% level and shows that an expected per-worker subsidy of 1000 Euro incentivizes a firm to reduce its proportion of temporary workers by 3.6pp. To corroborate the exclusion restriction and add more power to the estimation, in column 5,

[30] The corresponding reduced-form regression results (debt on subsidy) for all specifications from Tables IV and V are available 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 0.51-0.87 percentage point reduction in the debt-to-capital ratio.

I add a range of firm-level control variables (the model is even further saturated in the robustness tests). The estimate of δ remains similar and is still significant at the 1% level, suggesting that the instrument is uncorrelated with the range of included variables.

Given that the subsidy instrument is central to the paper, let me briefly discuss its economic magnitude and how it compares to the literature. First, an average 816-Euro subsidy from Table II, constituting about 1/2 of its standard deviation, predicts a 2.9pp change in the percentage of temporary workers, which is about 1/7 of the standard deviation of this variable. For a starting average of about 20pp, this means a 15% relative decrease. García-Pérez and Rebollo-Sanz (2009) mention that the average economic effect of regional subsidies across all types of workers is rather small. However, as mentioned above, both their paper, as well as Kugler et al. (2003) show empirically that subsidies promote permanent employment only in the most affected groups, but not in the others. I take this heterogeneity into account and do a back-of-the-envelope calculation based on the full transition matrix between different types of contracts and the unemployed, available from Guell and Petrongolo (2003). I find that the estimates in García-Pérez and Rebollo-Sanz (2009) imply a 2.1pp decrease (or a 10% relative decrease) in the share of temporary workers for the average size of the subsidy, meaning that my first-stage estimates are in fact of the same order of magnitude.[31]

The coefficients in columns 4 and 6 report the corresponding second-stage IV-2SLS estimates of β. Consistent with the downward bias in the OLS estimate of β discussed in detail in Section 3.3, I find that the magnitude of the IV-2SLS estimates is larger. The preferred estimate of β in column 6 (0.167 with a standard deviation of 0.0584) means that a one within-firm standard deviation increase in the proportion of flexible employment (10.78% in my data) leads to a 1.8pp higher leverage ratio (i.e. an increase of a 1/6 of within-firm standard deviation of debt-to-capital). This result is statistically significant at the 1% level. Its economic magnitude means that prohibiting

[31] To be more precise, I use quarterly transition matrix from Guell and Petrongolo (2003) Table 1, that is based on the Spanish Labor Force Survey for 1987-2002. According to this matrix, which I take as the base scenario, temporary workers have a 5.7% probability of exiting to permanent contract. García-Pérez and Rebollo-Sanz (2009) report a 67% relative increase in the probability of conversion for the affected women, and no change for men. In my data, the average proportion of women is 29%. Thus, in the "subsidy-affected" scenario, temporary workers have a 6.79% = 5.70%*(1+0.67*0.29) probability of exiting to permanent contract. Starting from 20% of workers on temporary contracts, and accruing quarterly transitions into an annual figure, the base scenario results in 21.74% of temporary contracts after one year, while the "subsidy-affected" scenario results in 19.60% temporary workers. The difference between these scenarios is 2.14pp or 10% in relative terms.

an average firm from hiring temporary employees (i.e. reducing their proportion from the average of 23.9% to 0%) would lead to a 4pp reduction in debt level, or about 7% of the average. This suggests that employment flexibility is an important component of default costs.

Finally, under the assumption that temporary workers represent variable costs, and permanent workers represent fixed costs, I calculate that the degree of operating leverage (i.e. the sensitivity of EBIT to sales) of an average firm that employs temporary workers would increase by about 17% if it were prohibited from hiring flexible labor force.[32] This suggests that the ability to hire workers on temporary contracts constitutes a significant part of a firm's operating flexibility.

4.2 Robustness: Additional Specifications

To show robustness of the results, I further saturate my empirical specification in Table V. Columns 1 and 2 introduce additional control variables of tangibility and average wage. 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 or wages (e.g. arising from skill improvements under a different contract), then we would not observe a significant and large effect in the first stage of this specification. Both the first-stage and second-stage results are the same in both magnitude and significance to the main results, providing evidence against tangibility and wage effects.[33]

In columns 3 and 4, I take another approach and saturate the model with region-industry-year (rather than separate region-year and industry-year) fixed effects. This specification provides a very tight identification. In particular, it allows me to control for industry-region-specific variation in female employment over time (which can affect the conservancy of financial decisions, as in Sapienza et al, 2009), as well as time-region-specific industry-level lobby power (which could affect the amounts and timing of subsidy introduction), as well as captures the non-uniform distribution of industries across regions. The coefficient of interest can still be identified because even within the same region-industry-year firms with higher pre-determined proportions of temporary workers

[32] I arrive at this figure by assuming that the only other variable costs are purchases of materials, and then taking the average of the percentage change in the degree of operating leverage ($\frac{DOL_0 - DOL_{Temp}}{DOL_{Temp}} = \frac{Temp \cdot TC_{labor}}{Sales - Purchases - Temp \cdot TC_{labor}}$, where $DOL_{Temp} = \frac{Sales - Purchases - Temp \cdot TC_{labor}}{Sales - Purchases - TC_{labor}}$), across firms that employ at least one temporary worker. If other costs, besides purchases, are considered variable, the effect on the DOL would be only larger.

[33] In Internet Appendix Table 8 I additionally show that differences in contract composition do not affect average wages in firms.

on average benefit more from the same statutory level of subsidies. It remains similar in magnitude and is significant at the 5% level.

In columns 5 and 6 I combine the previous two approaches by having both the additional control variables and the additional fixed effects in the specification. In this most saturated specification the coefficient of interest is significant at the 1% level.

In columns 7 and 8 I explore the subset of firms that were present in the data in 1994. This refutes a potential concern that the results are driven by firms that were sampled by ESEE in later years when the government policies had already been announced or implemented. The results are robust. I find that even for firms that determined their employment practices years in advance of government policies, the instrument is a good predictor of post-reform employment flexibility, and the magnitude of the coefficient of interest is similar to those in previous specifications.

One may still worry that the instrument is not fully exogenous since it includes a firm-specific pre-determined term w_{i0}^T.[34] As is also discussed in Section 2.1, some firms are likely to have a higher proportion of temporary workers to start with (such as those that are subject to more volatile product demand, as in Abraham and Taylor, 1996, or more competition as in Aparicio-Fenoll, 2015), or firms that have less need for firm-specific human capital investment, which requires employment protection (as in Jaggia and Thakor, 1994, or Wasmer, 2006). If these fluctuations are mostly industry- (or industry-region-) based, then industry-year (industry-region-year) fixed effects take care of all such effects flexibly. If they are not, however, then having captured the average differences across firms by firm fixed effects, one may still be concerned about the remaining interacted variation of other correlates of w_{i0}^T with the subsidy, which potentially violates the exclusion restriction. For example, firms with more temporary workers may be smaller, less profitable, or more levered to start with, and the effect of a given statutory subsidy may be different for these firms for reasons unrelated to subsidy eligibility. Thus, as a robustness check, I include interactions with all of the abovementioned variables, as well as of average size, profitability, and leverage, with the subsidy, in the IV specifications from Tables IV and V, as controls. The results are reported in Internet

[34] Part of this variation is still likely to be close to random due to the variation in pre-1984-reform hiring cycles across firms. In particular, as almost all of new hires were on temporary contracts, the higher the exogenous separation rate from a firm was (e.g. due to the retirement of previously existing workers or other reasons), the more temporary workers it would have by a certain date.

Appendix Table 2, and they are fully robust. The identification assumptions in these tests are much weaker than in the main specifications, since they assume that the remaining interacted variation of w_{i0}^T with subsidies – after all correlates of w_{i0}^T from the literature as well as cross-sectional correlates from my data have been accounted for – is orthogonal to firm-specific shocks. Moreover, since the coefficient of interest does not change while adding the interactions of these observable correlates of w_{i0}^T with the subsidy, as controls, this also implies that the interactions with the *un*observable correlates are unlikely to bias my results (Altonji et al, 2005).

Finally, in Internet Appendix Table 5 Panel A, I reestimate these specifications with a different dependent variable – the net flow of credit – defined as the change in debt, normalized by lagged total capital. The results are also robust, suggesting that firms indeed respond with changes in financial policy – by issuing/retiring debt – and not that I am picking some fluctuations in asset values. They are also large in terms of the economic magnitude: a one within-firm standard deviation increase in the proportion of flexible employment leads to about 3pp higher flow, which equals about half of the average of this variable or 1/8 of its standard deviation.

4.3 Robustness: The Role of Cash

One important consideration to be analyzed is that a subsidy not only influences the composition of labor contracts, but also provides the firm with a cash inflow. Firms may potentially use this cash to issue even more debt (Blanchard et al, 1994) or to retire the existing debt (Bates, 2005). In this respect, the exclusion restriction of the instrument would not be satisfied. Given that the estimated effect of flexible contracts on debt is positive, we should be concerned mostly about its upward bias, i.e. about the situation when cash from the subsidy is used to retire debt.

The first 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. 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 paying off debt using the cash received from subsidies.

In my data, the average within-firm change in the percentage of the temporary labor force is equal to 1.08 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 (3523 from Table II), this amounts to receiving $0.01078 \cdot 269 \cdot 3523 = 10206$ Euro per year in subsidies. At the same time, the preferred estimate of 0.167 (Table IV column 6) implies that such a change in the temporary labor force leads to $1.078 \cdot 0.167 = 0.18$ percentage points change in debt-to-capital ratio per year, or given the average total assets of 64.4 million Euro (from Table II), to $0.01078 \cdot 0.167 \cdot 66.4 \cdot 10^6 = 119674$ Euro average change in debt level per year. These two numbers suggest that about $10206/119674 = 8.5\%$ of the found effect can be due to cash considerations, i.e. the true causal coefficient is not 0.167, but about 0.153.

Because the overall averages may hide differential incentives for the firms to change their employment structure, I also perform a similar calculation at the individual firm level. In particular, for each firm I calculate how much of the actual change in debt level can be mechanically explained by the received subsidies. Only for 2% of firms, this implied maximum subsidy amount is comparable in magnitude to the change in debt levels. For more than 90% of firms, direct cash received from subsidies can explain at most 10% of the change in debt levels, which is consistent with the mean comparison. These results indicate that the absolute amount of money received from the subsidy is largely irrelevant as a means of debt retirement.

Another approach is to compare the effect of employment flexibility on capital structure for firms that can be considered relatively more and less cash-abundant. While firms hold optimal amounts of cash due to many reasons, if we see evidence of short-of-cash firms having much larger effects on debt, then it could be interpreted as evidence towards the violation of exclusion restriction and cash inflow helping them to retire debt. In Internet Appendix Table 4, I estimate specification (1) for subsamples of high- and low-cash firms, using several definitions.[35] As we observe, there is no statistical difference between the effects for high- and low-cash firms. If any it is the opposite: it is the high-cash firms that have (insignificantly) higher effects than low-cash firms in most specifications. This again shows that it is unlikely that the direct money effect of the subsidy can spur the relationship

[35] Since ESEE does not contain a separate entry for cash and cash equivalents, I use a proxy based on profit and loss items. Specifically, I calculate an approximation of 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 the 35% corporate tax rate, and less net capital expenditures. Then I classify firms as being relatively cash-abundant relative to their industry peers based on this measure (at the moment of receiving the subsidy, at the moment of debt conversion, or accumulated over three years).

between employment flexibility and leverage.

4.4 Other Margins of Financing

To paint a broader picture of the way labor contract structure affects firms' financial policies, I also explore several other variables related to sources of funding and report the results in Internet Appendix Table 5 Panels B, C, and D.

First, I want to test whether the increase in the net flow of debt (Panel A) happens as a result of swapping equity for debt, or alternatively, both equity and debt financing increase (which would be the case if flexible contracts also help relaxing firms' financial constraints). To do that, I consider the net flow of equity in Panel B and test whether the coefficient at the share of temporary workers is negative (as it would be had there been a swap) or not, using a one-sided test. As we see, the magnitude in all specifications is instead positive, suggesting that a swap is not likely,[36] and the alternative story may be the case. Consistent with this interpretation, I also find that firms are no more likely to become traded on an exchange when using more flexible labor contracts (Panel C).

Finally, firms that use more flexible contracts are also less likely to be part of a group of companies. This result is significant at the 5% level in several specifications, with economic magnitudes of 2-4pp increase in probability (about 1/10 of the mean) per each 10pp increase in temporary labor. The sign of this coefficient, similar to other variables, also supports the interpretation that flexible contracts may allow firms to depend less on external funds in general.

5 Flexible Employment Contracts Reduce Operating Leverage

The results so far provide evidence of a positive effect of flexible employment contracts on financial leverage, but the exact mechanism is not yet identified. In this section I use further analysis to first demonstrate that flexible employment reduces operating leverage by providing a margin of adjustment when economic conditions turn bad. Then I present the most direct test by exploring the

[36] I cannot make a very strong conclusion here, since the p-values range from 1% up to 14% in the tightest specifications with industry-region-year fixed effects.

cross-sectional heterogeneity and comparing firms with different magnitudes of ex ante bankruptcy costs.

5.1 Temporary Employees as a Margin of Adjustment

The underlying assumption behind interpreting the effect of flexible employment as operating-financial leverage substitution is that a flexible labor force lowers default risk by providing a margin for adjusting labor when the firm faces negative shocks.[37] 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; Holmlund and Storrie, 2002, for Sweden; Alonso-Borrego et al., 2005, for Spain; Kugler and Pica, 2004, for Italy). My data can corroborate this assumption by providing micro-level evidence of firms laying off temporary workers in response to negative shocks.[38]

To test this assumption, 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 behind this measure is 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 have if it had kept these workers employed.

I estimate the following specification:

$$Temp_{it} = \alpha_{st} + \lambda NegativeShock_{it} + \eta_i + \epsilon_{it}. \tag{4}$$

Inclusion of industry-year fixed effects α_{st} implies that $NegativeShock_{it}$ measures the firm-specific demand shock over and above any industry-level shocks in the same year, while firm fixed

[37] As Bentolila et al (2012) note: "[E]mployees under very flexible contracts are better thought of as being part of the outsiders because, given that their role is precisely to bear the brunt of employment adjustments, their attachment to the job is fragile."

[38] Notably, to test this assumption 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), because of endogenous leverage adjustment. 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.

effects η_i capture time-invariant heterogeneity across firms.

The results of estimating (4) are presented in Table VI 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 1.8pp lower than it is during normal demand conditions; this roughly corresponds to firing about one tenth of the total flexible labor force. [39]

To the extent that firms may have several product markets, measuring firm-specific demand shock based only on the main market may be noisy. Column 3 estimates specification (4) for a subsample of firms with only one product, while column 4 does so for a subset of firms that sell only one product and have a low share in that market (less than 5%). For these latter firms, the extent to which a given firm can affect the state of its product market is very limited, making it reasonably exogenous for them. The results are robust. Overall, the micro-level evidence presented in Table VI is consistent with the prevailing view of the literature that temporary workers provide a margin of labor adjustment to negative demand shocks. This corroborates the assumption behind the mechanism of why flexible labor force affects capital structure.

5.2 Cross-Sectional Heterogeneity in Bankruptcy Costs

First, it should be noted that to test the assumption that temporary workers reduce operating leverage one cannot 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 (including financial leverage), since this assumption implies that flexible employment reduces operating leverage and

[39] This result is robust to clustering standard errors at the industry level, as well as to using a lagged, rather than contemporaneous, indicator of firm-specific negative demand shock. 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.

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 4. In fact, if companies, as hypothesized, trade off operating and financial leverage, then empirically we should see no effect of the flexible labor force on the realized probability of default (under the assumptions of no effect of temporary workers on survival other than through capital structure, and perfect adjustment of debt to the optimal level).[40]

To overcome this empirical challenge, I use a different approach. If a flexible labor force indeed reduces operating leverage and the probability of default, then firms with a relatively high *severity* of bankruptcy costs should value the option of firing workers more, because for them the corresponding change in expected costs of default would be higher. In this case, we would expect to find a larger effect of flexible labor on corporate financing for these firms.

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

$$D_{it} = \alpha_{rt} + \alpha_{st} + \beta^H High_{i0} \cdot Temp_{it-1} + \beta^L Low_{i0} \cdot Temp_{it-1} + X'_{it}\gamma + \eta_i + \epsilon_{it}, \tag{5}$$

where $High_{i0}$ and Low_{i0} are the indicator variables corresponding to firms with high and low levels of severity of bankruptcy costs[41]. Based on the above prediction, β^H should be higher than β^L in the above specification.

What remains is to identify the subsamples of firms that would incur higher and lower bankruptcy costs if they were to go bankrupt. 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: when assets can be easily redeployed for other purposes, the loss of value from liquidation is low, and so is the cost of default. Based on the balance sheet data that I have, the least specific assets appear to be buildings and land (as compared to e.g. work-in-progess, in-

[40] 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 default 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.

[41] Each of $High_{i0} \cdot Temp_{it}$ and $Low_{i0} \cdot Temp_{it}$ variables is intrumented with both $High_{i0} \cdot ExpectedSubsidy_{it-1}$ and $Low_{i0} \cdot ExpectedSubsidy_{it-1}$; the first-stage coefficients for each equation are reported in Internet Appendix Table 6.

ventory, and specific machinery). I therefore first classify firms as having lower 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 endogenous asset substitution over time). Likewise, firms with no buildings and land are classified as having higher levels of bankruptcy costs ($High_{i0} = 1$).

The results of the second-stage regressions are presented in Table VII columns 1 and 2, where the latter includes firm-level control variables. Consistent with temporary workers reducing operating leverage and the probability of default, the positive effect of a flexible workforce is most pronounced within the higher 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 4.1pp higher debt ratio. Furthermore, the implied difference in the coefficients between high and low bankruptcy cost firms (0.314 with a standard error of 0.122) is large and statistically significant at the 5% level, suggesting that firms with higher levels of bankruptcy costs are significantly more likely to adjust their capital structure in response to labor flexibility shocks.

Importantly, the first-stage results (reported in Internet Appendix Table 6) indicate that both types of firms change their composition of labor contracts in response to subsidy incentives. However, as can be seen from the second stage, only firms for whom this flexibility does matter retire debt upon a reduction in the proportion of temporary workers. This cross-sectional comparison provides direct evidence that the mechanism behind the effect of interest goes through operating leverage.

As a robustness check, I also reestimate specification (5) 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 3 and 4 of Table VII, demonstrating similar patterns.[42]

[42] This heterogeneity could be explained by other variables correlated with bankruptcy costs. In Internet Appendix Table 7 I also control for additional cross-sectional splits by size, profitability, and competition. The difference between firms with high and low bankruptcy costs is still significant in all specifications.

6 Value Effects of Flexible Employment Contracts

I now explore the value implications of hiring more workers on temporary contracts.[43] While there is a large literature on how employment protection affects productivity (a typical dependent variable in this setup), it has provided somewhat mixed results. For example, Autor et al., 2007, find negative effects of WDL-induced employment protection on productivity, while Capellari et al., 2012, and Aguirregabiria and Alonso-Borrego, 2014, find positive effects of employment protection provided by permanent contracts. These mixed results largely mirror the main theoretical trade-off discussed in the literature – the one between the greater effort put forth by the less protected (temporary) workers vs the higher on-the-job training and investment in firm-specific capital as well as the fewer work-related accidents of the more protected (permanent) workers (see Jaggia and Thakor, 1994, Booth et al., 2002, Guadalupe, 2003, Wasmer, 2006, among others).

In this section I propose that employment flexibility can also increase firm value and performance through an additional channel related to the reductions in operating leverage, which is the primary focus of my study. To the best of my knowledge, this is a novel channel that has not been explored in the literature.[44] Besides focusing on this new channel, my setting is also superior to previous studies in two important dimensions. First, I look at firm-level consequences, rather than at individual worker-level changes in contracts and subsequent worker behavior. Measuring firm-level effects allows me to capture the possible within-team equilibrium response of different types of workers, such as changes in effort provision by permanent workers depending on the number of temporary workers. And second, my identification strategy ensures that the results are not driven by the selection of different types of firms into different contract compositions, or by omitted factors driving both.

In Table VIII I report the results of estimating specifications similar to (1), with labor productivity (Panel A) and profitability (Panel B), as dependent variables.[45] For completeness, Column 1

[43] I thank an anonymous referee for suggesting this important discussion.

[44] While Serfling (2015) demonstrates negative abnormal returns upon adoption of employment protection laws, no average effect can be attributed solely to operating leverage, precisely because employment protection affects both the individual-worker effort provision, and also the composition of workers (Autor et al, 2007), both of which affect value and productivity negatively.

[45] Since most firms are private and do not have a market-based measure of performance, these measures are the next best alternatives and are also commonly used in the literature (see e.g. in Guadalupe et al, 2012, besides the references above).

reports the average effect of hiring workers on flexible contracts on these performance measures. As we see, this average effect is generally zero (slightly positive, but not statistically significant, with p-values of 10% and 22%, respectively). This is expected, since there are opposing mechanisms by which workers on flexible contracts may affect productivity and performance. Importantly, it also means that one cannot trace the effect of employment flexibility that works through reductions in operating leverage solely from such average regressions. To nail down this specific channel, I hence look at the heterogeneity of the effect of interest and test whether the effect is positive in subsamples of firms which would benefit most from this ability of flexible contracts to reduce operating leverage.

First, as shown in Table VII, employment flexibility reduces the expected costs of bankruptcy most for high-bankruptcy-cost firms (but not for low-bankruptcy-cost firms). These are precisely the firms that would benefit most from reductions in operating leverage provided by employment flexibility, and hence the first prediction that I test is whether employment flexibility increases firm value and performance for the high-bankruptcy-cost firms. Column 2 reports the results of estimating (1) for this subsample of firms. As we see, hiring workers on flexible contracts is highly value-enhancing for these firms. Both labor productivity and profitability significantly increase with the proportion of flexible contracts. The economic magnitude implies that for every 10pp of flexible workers, labor productivity and operating profit margin increase by 4.5 percent and 1.2 percentage points, respectively. On the other hand, as indicated in column 5, flexible contracts do not affect the productivity and performance of the firms that should not value this aspect of flexibility as much, i.e. of the low-bankruptcy-cost firms.

The second idea is inspired by Caggese and Cuñat (2008), who propose that by reducing the fixed component of a firm's costs, flexible employment helps overcome financial constraints. If so, then firms that are most dependent on external finance should increase their value most from the same shock to flexible contracts. I use the methodology of D'Acunto et al.(2018) to test this second prediction. In column 3, I estimate specification (1) for firms in the top tercile of cash-flow-to-assets firms (in column 6 – for other firms), and in column 4 – for firms in the top tercile of the external finance gap (in column 7 – for other firms). As in D'Acunto et al.(2018), I also follow Demirgüç-Kunt and Maksimovic (2002) to calculate the external finance need of firms, using the average sales growth over the last three years, and subtracting the earnings available to finance that

growth internally. Then the difference is scaled by total assets to arrive at the external finance gap. Consistent with the above prediction, we see that the effect of flexible contracts on firm productivity and profits is most pronounced for the firms with low cash-flow-to-assets ratios and high external finance gaps (columns 3 and 4), rather than the other firms (columns 6 and 7).

Taken together, these results indicate that employment flexibility increases the value of firms through the channel of reducing operating leverage and helping firms to overcome financial constraints. This has two implications. For firms, this highlights a specific management practice that can help at least some of them to increase performance. And for the policy makers, this shows the additional unintended negative effects of promoting permanent employment: even if the average firm doesn't lose with hiring more permanent labor, the most financially constrained firms do, affecting the industry composition and hurting allocative efficiency.

7 Concluding Remarks

In this paper, I explore how the structure of employment contracts affects the financing decisions of firms. The institutional features of the Spanish dual labor market allow me to measure the actual "fixity" of each firm's labor expenses by the proportion of workers that it employs under temporary contracts, and isolate the employment flexibility channel from differences in wages or worker bargaining power.

While I focus on the duration and firing cost aspects of different types of labor contracts (which fully explain the difference in the Spanish case), other components of labor compensation structure, such as wage seniority profile, currency of compensation, the existence and the structure of incentive pay, and others, may have different effects on capital structure choice (if any), and the economic mechanisms behind them. Exploring them is beyond the scope of my paper and is left for future research.

More generally, the results of my paper emphasize the interdependence between the different organizational strategies of a firm and its financing decisions. From the management perspective, it illustrates the complementarity of CEOs' and CFOs' decision-making. From the policy perspective, it reflects the indirect consequences of exogenous changes in government policies that originally

aim at certain organizational changes in firms. 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 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.'

References

[1] Abowd, John M., 1989, The Effect of Wage Bargains on the Stock Market Value of the Firm, *American Economic Review* 79(4), 774-800.

[2] Abraham Katharine G., and Susan K. Taylor, 1996, Firms' Use of Outside Contractors: Theory and Evidence, *Journal of Labor Economics*, 14 (3), 394-424.

[3] Addison John T., and Christopher J. Surfield, 2006, The use of alternative work arrangements by the jobless: Evidence from the CAEAS/CPS, *Journal of Labor Research* 27(2), 149-162.

[4] Aguirregabiria, Victor, and César Alonso-Borrego, 2014, Labor contracts and flexibility: Evidence from a labor market reform in Spain, *Economic Inquiry,* 52(2), 930–957

[5] Agrawal Ashwini K., and David A. Matsa, 2013, Labor Unemployment Risk and Corporate Financing Decisions, *Journal of Financial Economics* 108 (2), 449-470.

[6] Allayannis, G., Ihrig, J., Weston, J.P., 2001. Exchange-rate hedging: financial versus operational strategies. *American Economic Review* 91 (2), 391–395.

[7] Alonso-Borrego, César, Jesús Fernández-Villaverde, and José E. Galdón-Sánchez, 2005, Evaluating Labor Market Reforms: A General Equilibrium Approach, *NBER Working Paper No. 11519.*

[8] Altman, Edward, 1984, A further empirical investigation of the bankruptcy cost question, *Journal of Finance* 39, 1067–1089.

[9] Altonji, Joseph G., Todd E. Elder, and Christopher R. Taber, 2005, Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools, *Journal of Political Economy* 113 (1), 151-184.

[10] Aparicio-Fenoll, Ainhoa, 2015, The effect of product market competition on job security, *Labour Economics* 35, 145-159.

[11] Autor, David H., William R. Kerr, and Adriana D. Kugler, 2007, Does employment protection reduce productivity? Evidence from U.S. states, *Economic Journal* 117, F189–F217.

[12] Bates, Thomas W., 2005, Asset Sales, Investment Opportunities, and the Use of Proceeds, *Journal of Finance* 60(1), 105-135.

[13] Benmelech, Efraim, Nittai K. Bergman and Ricardo J. Enriquez, 2012, Negotiating with Labor under Financial Distress, *Review of Corporate Finance Studies* 1(1), 28-67.

[14] Bentolila, Samuel, Juan J. Dolado, and Juan F. Jimeno, 2012, Reforming an insider-outsider labor market: the Spanish experience, *IZA Journal of European Labor Studies* 2012, 1:4.

[15] Bentolila, Samuel, Pierre Cahuc, Juan J. Dolado, and Thomas Le Barbanchon, 2012, Two-Tier Labour Markets in the Great Recession: France Versus Spain, *Economic Journal* 122, F155-F187.

[16] Besley, Timothy, and Robin Burgess, 2004, Can Labor Regulation Hinder Economic Performance? Evidence from India, *Quarterly Journal of Economics* 119, 91-134.

[17] Blanchard, Olivier, and Augustin Landier, 2002, The perverse effects of partial labour market reform: fixed-term contracts in France, *Economic Journal* 112(480), F214–F244.

[18] Blanchard, Olivier, Florencio Lopez-de-SiIanes, and Andrei Shleifer,1994, What do firms do with cash windfalls?, *Journal of Financial Economics* 36(3), 337-360.

[19] Booth, A.L., Francesconi, M., Frank, J., 2002, Temporary Jobs: Stepping Stones or Dead Ends? *Economic Journal Features: Symposium on Temporary Work,* F189 – F213.

[20] Bracha, Anat, and Mary A. Burke, 2016, Who Counts as Employed? Informal Work, Employment Status, and Labor Market Slack. *Federal Reserve Bank of Boston Working Paper* No. 16-29.

[21] Bronars, Stephen G., and Donald R. Deere, 1991, The Threat of Unionization, the Use of Debt, and the Preservation of Shareholder Wealth, *Quarterly Journal of Economics* 106(1), 231-254.

[22] Caggese, Andrea, and Vicente Cuñat, 2008, Financing Constraints and Fixed-Term Employment Contracts, *Economic Journal* 118, 2013–2046.

[23] Cappellari, Lorenzo, Carlo Dell'Aringa, Marco Leonardi, 2012, Temporary Employment, Job Flows and Productivity: A Tale of Two Reforms, *Economic Journal*, 122(562), F188–F215.

[24] Chen, Zhiyao, Jarrad Harford, and Avraham Kamara, 2017, Operating Leverage, Profitability and Capital Structure, *working paper.*

[25] Chen, Huafeng, Marcin Kacperczyk, and Hernan Ortiz-Molina, 2011, Labor unions, operating flexibility, and the cost of equity, *Journal of Financial and Quantitative Analysis* 46, 25–58.

[26] Cuñat, Vicente, 2007, Trade credit: Suppliers as debt collectors and insurance providers, *Review of Financial Studies* 20, 491–527.

[27] D'Acunto, Francesco, Ryan Liu, Carolin Pflueger, and Michael Weber, 2018, Flexible prices and leverage, *Journal of Financial Economics* 129(1), 46–68.

[28] Dasgupta, Sudipto, and Kunal Sengupta, 1993, Sunk Investment, Bargaining, and Choice of Capital Structure, *International Economic Review* 34(1), 203–220.

[29] Davia Maria A. and Virginia Hernanz, 2004, Temporary employment and segmentation in the Spanish labour market: An empirical analysis through the study of wage differentials, *Spanish Economic Review* 6(4), 291-318.

[30] Demirgüç-Kunt, Aslı, and Vojislav Maksimovic, 2002, Funding growth in bank-based and market-based financial systems: evidence from firm-level data, *Journal of Financial Economics* 65(3), 337-363.

[31] Dolado, Juan J. , Carlos García-Serrano, and Juan F. Jimeno, 2002, Drawing lessons from the boom of temporary jobs in Spain, *Economic Journal* 112(480), F270–F295.

[32] Dotan, Amihud, and S. Abraham Ravid, 1985, On the Interaction of Real and Financial Decisions of the Firm Under Uncertainty, *Journal of Finance* 40(2), 501-517.

[33] Du, QianQian, Laura Xiaolie Liu, and Rui Shen, 2012, Cost inflexibility and capital structure, *working paper*.

[34] Ellul Andrew and Marco Pagano, 2017, Corporate leverage and employees' rights in bankruptcy, *working paper*.

[35] García-Pérez, J. Ignacio, and Yolanda F. Rebollo-Sanz, 2009, Do wage subsidies affect the subsequent employment stability of permanent workers?: the case of Spain, *Moneda y Crédit,* 228, 65-103.

[36] Garcia-Teruel, Pedro Juan, and Pedro Martinez-Solano, 2010, Determinants of trade credit: A comparative study of European SMEs, *International Small Business Journal* 28 (3), 215-233.

[37] Graham John, Hyunseob Kim, Si Li, and Jiaping Qiu, 2014, Employee Costs of Corporate Bankruptcy, with John Graham, Si Li, and Jiaping Qiu, *AFA 2014 Philadelphia Meetings Paper*

[38] Guadalupe, Maria, 2003, The hidden costs of fixed term contracts: The impact on work accidents, *Labour Economics* 10(3), 338–358.

[39] Guell, Maia, and Barbara Petrongolo, 2007, How binding are legal limits? Transitions from temporary to permanent work in Spain, *Labour Economics* 14, 153–183.

[40] Farber, H.S., 1999, Alternative and Part-Time Employment Arrangements as a Response to Job Loss, *Journal of Labor Economics* 17, S142 – S169.

[41] Felgueroso, Florentino, J. Ignacio García-Pérez, Marcel Jansen, and David Troncoso-Ponce, 2018, The Surge in Short-Duration Contracts in Spain, *De Economist* 166, 503–534.

[42] Frank, Murray Z., and Vidhan K. Goyal, 2009, Capital Structure Decisions: Which Factors Are Reliably Important?, *Financial Management* 38(1), 1-37.

[43] Izquierdo Mario, Esther Moral, and Alberto Urtasun, 2003, Collective bargaining in Spain: An individual data analysis, *Banco de Espa˜na Occasional Paper Series #0302.*

[44] Jaggia, Priscilla Butt, and Anjan V. Thakor, 1994, Firm-specific human capital and optimal capital structure, *International Economic Review* 35(2), 283–308.

[45] Jimeno, Juan F., and Luis Toharia, 1994, *Unemployment and labour market flexibility: Spain* (International Labour Office, Geneva).

[46] Hanka, Gordon, 1998, Debt and the terms of employment, *Journal of Financial Economics* 48, 245-282.

[47] Hirsch, Barry T., 1991, Union Coverage and Profitability among U.S. Firms, *Review of Economics and Statistics* 73, 69-77.

[48] Holmlund, Bertil. and Donald Storrie, 2002, Temporary work in turbulent times: The Swedish example, *Economic Journal*, 112: F245–F269.

[49] Kahl, Matthias, Jason Lunn, and Mattias Nilsson, 2012, Operating Leverage and Corporate Financial Policies, *AFA 2012 Chicago Meetings Paper* .

[50] Kugler, Adriana and Giovanni Pica, 2004, Effects of employment protection and product market regulations on the Italian labour market, in (J. Messina, C. Michelacci, J. Turunen, and G. Zoega, eds.), *Labour Market Adjustments in Europe*, 107–137 (Cheltenham: Edward Elgar).

[51] Kugler, Adriana, Juan F. Jimeno, and Virginia Hernanz, 2003, Employment Consequences of Restrictive Permanent Contracts: Evidence from Spanish Labor Market Reforms, *working paper.*

[52] Kuzmina, Olga, and Olga Kuznetsova, 2018, Operational and Financial Hedging: Evidence from Export and Import Behavior, *Journal of Corporate Finance*, 2018, 48 (1): 109–121.

[53] Lee, David S., and Alexandre Mas, 2012, Long-Run Impacts of Unions on Firms: New Evidence from Financial Markets, 1961–1999, *Quarterly Journal of Economics* 127 (1), 333-378.

[54] Lemmon, Michael L., Michael R. Roberts, and Jaime F. Zender, 2008, Back to the beginning: Persistence and the cross-section of corporate capital structure, *Journal of Finance* 63(4), 1575–1608.

[55] Lindbeck, Assar and Dennis J., Snower, 1986, Involuntary Unemployment as an Insider-Outsider Dilemma, in W. Beckerman, ed., Wage Rigidity and Unemploy-ment, London: Duckworth,97-125.

[56] MacKay, Peter, 2003, Real flexibility and financial structure: An empirical analysis, *Review of Financial Studies* 16(4), 1131-1165.

[57] MacKie-Mason, Jeffrey K., 1990, Do taxes affect corporate financing decisions?, *Journal of Finance* 45, 1471–1493.

[58] Mandelker, Gershon N., and S. Ghon Rhee, 1984, The impact of the degrees of operating and financial leverage on systematic risk of common stock, *Journal of Financial and Quantitative Analysis* 19(1), 45–57.

[59] Matsa, David A., 2010, Capital structure as a strategic variable: evidence from collective bargaining, *Journal of Finance* 65(3), 1197–1232.

[60] Matsa, David A., 2018, Capital Structure and the Firm's Workforce, *Annual Review of Financial Economics* 10, 387-412.

[61] Mauer, David C., and Alexander J. Triantis, 1994, Interactions of corporate financing and investment decisions: A dynamic framework, *Journal of Finance* 49(4), 1253-1277.

[62] Modigliani, Franco, and Merton H. Miller, 1958, The cost of capital, corporation finance and the theory of investment, *American Economic Review* 48(3): 261-297.

[63] Myant, Martin, and Laura Brandhuber, 2017, Uses and abuses of the OECD's Employment Protection Legislation index in research and EU policy making in *Myths of employment deregu-*

lation: how it neither creates jobs nor reduces labour market segmentation, edited by Agnieszka Piasna and Martin Myant, ETUI, Brussels.

[64] OECD Employment Outlook 2017.

[65] OECD Fiscal Federalism 2016: Making Decentralisation Work, OECD Publishing, Paris.

[66] Pagano, Marco, and Paolo Volpin, 2008, Labor and finance, *LBS Working Paper.*

[67] Paravisini, Daniel, 2008, Local Bank Financial Constraints and Firm Access to External Finance, *Journal of Finance* 63(5), 2161-2193.

[68] Perotti, Enrico C., and Kathryn E. Spier, 1993, Capital structure as a bargaining tool: the role of leverage in contract renegotiation, *American Economic Review* 83(5), 1131–1141.

[69] Petersen, Mitchell A, 1994, Cash flow variability and firm's pension choice A role for operating leverage, *Journal of Financial Economics* 36, 361-383.

[70] Petersen, Mitchell A., and Raghuram G. Rajan, 1997, Trade credit: Theories and evidence, *Review of Financial Studies* 10, 661–691.

[71] Rajan, Raghuram G., and Luigi Zingales, 1995, What Do We Know about Capital Structure? Some Evidence from International Data, *Journal of Finance* 50(5), 1421-1460.

[72] Reinartz, Sebastian J., and Thomas Schmid, 2016, Production Flexibility, Product Markets, and Capital Structure Decisions, *Review of Financial Studies* 29(6), 1501–1548.

[73] Ruback, Richard, and Martin Zimmerman, 1984, Unionization and Profitability: Evidence from the Capital Market, *Journal of Political Economy* 92, 1134-1157.

[74] Saint-Paul, Gilles, 1996, *Dual Labour Markets, a Macroeconomic Perspective*, Cambridge: MIT Press.

[75] Sapienza, Paola, Luigi Zingales, and Dario Maestripieri, 2009, Gender differences in financial risk aversion and career choices are affected by testosterone, *Proceedings of the National Academy of Sciences* 106 (36) 15268-15273.

[76] Schmalz, Martin C., 2016, Unionization, cash, and leverage, Working paper, University of Michigan.

[77] Serfling, Matthew, 2016, Firing Costs an Capital Structure Decisions, *Journal of Finance* 71(5), 2239-2286.

[78] Simintzi, Elena, Vikrant Vig, and Paolo Volpin, 2010, Labor and capital: is debt a bargaining tool?, WFA 2010 Victoria Meetings Paper.

[79] Simintzi, Elena, Vikrant Vig, and Paolo Volpin, 2015, Labor Protection and Leverage, *Review of Financial Studies* 28(2), 561–591.

[80] Shleifer, Andrei and Robert W. Vishny, 1992, Liquidation Values and Debt Capacity: A Market Equilibrium Approach, *Journal of Finance* 47(4), 1343-1366.

[81] Titman, Sheridan, and Roberto Wessels, 1988, The determinants of capital structure, *Journal of Finance* 43(1), 1-19.

[82] Van Horne, James C., 1977, *Financial Management and Policy* (Prentice Hall, Englewood Cliffs, NJ).

[83] Vig, Vikrant, 2013, Access to Collateral and Corporate Debt Structure: Evidence from a Natural Experiment, *Journal of Finance* 48(3), 881-928.

[84] Wasmer, Etienne, 2006, Interpreting Europe-US Labor Market Differences: the Specificity of Human Capital Investments, *American Economic Review* 96 (3), 811-831

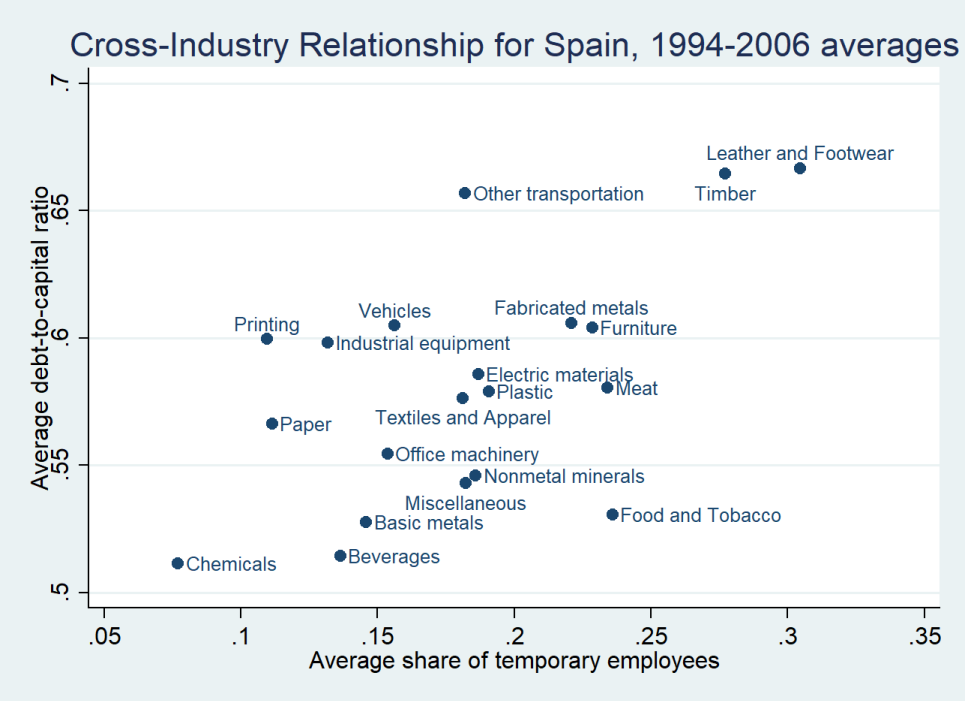
[85] Williamson, Oliver E., 1988, Corporate finance and corporate governance, *Journal of Finance* 43(3), 567-591.

[86] Wilner, B.S., 2000. The Exploitation of Relationships in Financial Distress: The Case of Trade Credit." *Journal of Finance* 55, 153-178.

[87] Zingales, Luigi, 2000, In Search of New Foundations, *Journal of Finance* 55(4), Papers and Proceedings of the Sixtieth Annual Meeting of the American Finance Association, Boston, Massachusetts, 1623-1653.

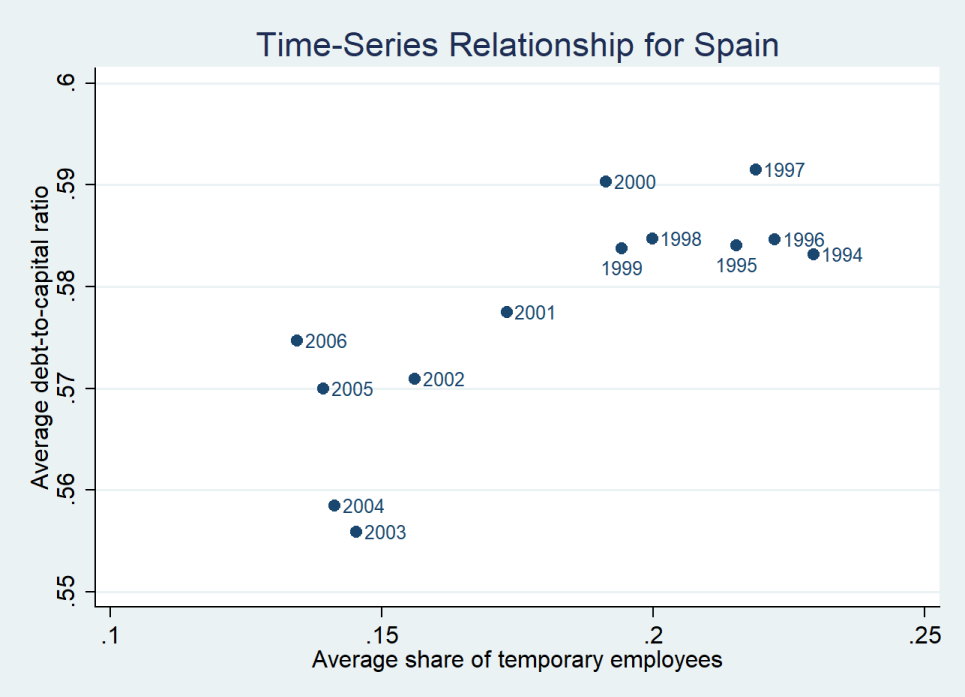
Figures and Tables

Figure 1



Note: This figure plots the relationship between average firm-level leverage (defined as the ratio of total debt to capital) 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 ($Subsidy_{grt}$) 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) with non-missing debt-to-assets ratio. Total Assets is book value of total assets of the firm, in 2006 Euros. Total Debt / Total Capital s is the ratio of total debt (which is the sum of short-term and long-term debts) to total capital (those debts plus book equity). 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. z-score is the modified Altman's z-score (see also the corresponding footnote in the text). Tangibility is the share of gross land and buildings in total assets. Average wage is the total wage bill per employee, in 2006 Euros. Net flow of credit is the ratio of the change in total debt to lagged total capital. Net flow of equity is the ratio of the change in total equity to lagged total capital. Trading on an Exchange is a dummy that equals 1 if the firm's equity trades on an exchange, and 0 otherwise (available every 4 years). Being Part of a Group is a dummy that equals 1 if the firm is part of a group of companies, and 0 otherwise (available every 4 years) All firmlevel control variables are winsorized at 1% tails.

Variable	Mean	Std. deviation	N
Capital Structure:			
Total Assets	64.4mln	288mln	18365
Total Debt / Total Capital (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
Control Variables:			
Size	16.013	2.014	18347
Profitability	0.225	0.134	18346
R&D	0.007	0.017	18246
z-score	2.237	1.326	18132
Tangibility	0.139	0.152	18228
Average Wage	28790	12278	18365
Other Dependent Variables:			
Net Flow of Credit	0.069	0.281	15258
Net Flow of Equity	0.038	0.121	15258
Trading on an Exchange	0.026	0.159	4975
Being Part of a Group	0.357	0.479	4970

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:

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

where D_{it} is the leverage (ratio of total debt to capital) 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, α_{st} are industry-year fixed effects, η_i are firm fixed effects, and X_{it} are firm-level controls (log of sales, operating profit margin, R&D expenses over sales, and modified Altman's z-score; 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	0.0543***	0.0481***		0.140**		0.167***
	(0.0119)	(0.0115)		(0.0607)		(0.0584)
Lagged2 Expected Subsidy			-0.0362***		-0.0358***	
			(0.00574)		(0.00563)	
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
Industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	2241	2219	2226	2226	2203	2203
Observations	17673	17340	16889	16889	16562	16562
Within R^2	0.049	0.140	0.173		0.176	
F-statistic			39.88		40.55	

Table V. Capital Structure and Employment Flexibility: Robustness

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

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

where D_{it} is the leverage (ratio of total debt to capital) 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, α_{st} are industry-year fixed effects, η_i are firm fixed effects, and X_{it} are firm-level controls (log of sales, operating profit margin, R&D expenses over sales, and modified Altman's z-score; 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, 2 and 5,6 additionally include firm-level controls for tangibility and average wage. Specifications 3, 4 and 5,6 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
	Additional controls (tangibility and average wage)		Additional FE		Additional controls and FE		Originally-sampled firms	
	1	2	3	4	5	6	7	8
Lagged Temp		0.164***		0.146**		0.166***		0.230***
		(0.0582)		(0.0655)		(0.0611)		(0.0846)
Lagged2 Expected Subsidy	-0.0359***		-0.0374***		-0.0368***		-0.0379***	
	(0.00561)		(0.00668)		(0.00652)		(0.00586)	
Firm-level controls	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	No	No	No	No	Yes	Yes
Industry-year FE	Yes	Yes	No	No	No	No	Yes	Yes
Region-industry-year FE	No	No	Yes	Yes	Yes	Yes	No	No
Number of firms	2203	2203	2226	2226	2203	2203	1530	1530
Observations	16562	16562	16889	16889	16562	16562	12563	12563
Within R^2	0.178		0.312		0.318		0.180	
F-statistic	40.86		31.33		31.84		41.97	

Table VI. 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 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	-0.0176***	-0.0177***	-0.0210***	-0.0185**
	(0.00317)	(0.00346)	(0.00493)	(0.00877)
Firm FE	Yes	Yes	Yes	Yes
Industry-year FE	Yes	No	Yes	Yes
Region-industry-year FE	No	Yes	No	No
Number of firms	2989	2989	1507	398
Observations	22022	22022	10609	3529
Within R^2	0.118	0.253	0.122	0.157

Table VII. 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:

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

where D_{it} is the leverage (ratio of total debt to capital) 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, α_{st} are industry-year fixed effects, η_i are firm fixed effects, and X_{it} are firm-level controls (log of sales, operating profit margin, R&D expenses over sales, and modified Altman's z-score; included in specifications 2 and 4). $High_{i0}$ 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 (specifications 1 and 2) or as having less buildings and land that the industry median (specifications 3 and 4), 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			
	Second Stage 1	Second Stage 2	Second Stage 3	Second Stage 4
$High_{i0}$ * Lagged Temp	0.261**	0.379***	0.318***	0.400***
	(0.119)	(0.116)	(0.0966)	(0.0976)
Low_{i0} * Lagged Temp	0.0679	0.0651	-0.00863	-0.00544
	(0.0680)	(0.0634)	(0.0758)	(0.0687)
Firm-level controls	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes
Number of firms	2141	2121	2141	2121
Observations	16291	16039	16291	16039
1st stage F-statistic	12.21	11.77	12.50	11.99
Implied estimate of the difference	0.193	0.314**	0.327***	0.405***
standard deviation of the difference	(0.128)	(0.122)	(0.116)	(0.112)

Table VIII. Value Effects of Employment Flexibility

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

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

where Y_{it} is labor productivity (Panel A; log of sales per worker) or profitability (Panel B; operating profit margin) 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, α_{st} are industry-year fixed effects, η_i are firm fixed effects, and X_{it} are firm-level controls (log of sales, R&D expenses over sales; included in all specifications). In both panels specification 1 estimates the above relationship for all firms; specifications 2 (5) - for firms with high (low) bankruptcy costs (defined as having no (positive) buildings and land on its balance sheet); specifications 3 (6) - for firms with cash flow over total assets below (above) the industry top tercile in year t; specifications 4 (7) - for firms with high (low) external finance need (defined as in Demirguc-Kunt and Maximovich, 2002; above (below) the industry bottom tericle 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.

		High value of flexibility			**Low value of flexibility**		
Panel A: Labor productivity	All firms	High Bankruptcy Costs	Low Cash Flow	High External Need	Low Bankruptcy Costs	High Cash Flow	Low External Need
	1	2	3	4	5	6	7
Lagged Temp	0.196	0.454**	0.250**	0.311***	0.0820	0.216	0.0123
	(0.120)	(0.211)	(0.117)	(0.120)	(0.153)	(0.243)	(0.290)
Firm-level controls		Yes	Yes	Yes	Yes	Yes	Yes
Firm FE		Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE		Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE		Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	2259	746	1974	1936	1428	1286	1280
Observations	17297	5353	11087	11180	11340	5057	4973
1st stage F-statistic	39.64	18.48	35.51	41.09	43.42	37.14	14.46

		High value of flexibility			**Low value of flexibility**		
Panel B: Profitability	All firms	High Bankruptcy Costs	Low Cash Flow	High External Need	Low Bankruptcy Costs	High Cash Flow	Low External Need
	1	2	3	4	5	6	7
Lagged Temp	0.0437	0.121*	0.0620	0.108**	-0.00653	-0.0234	0.0988
	(0.0363)	(0.0684)	(0.0431)	(0.0434)	(0.0460)	(0.0578)	(0.0985)
Firm-level controls		Yes	Yes	Yes	Yes	Yes	Yes
Firm FE		Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE		Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE		Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	2260	746	1974	1936	1428	1286	1280
Observations	17303	5353	11087	11180	11337	5057	4973
1st stage F-statistic	39.63	18.48	35.51	41.09	43.40	37.14	14.46

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

This table reports the results of the following specification:

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

where D_{it} is the leverage (ratio of total debt to capital) 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, α_{st} are industry-year fixed effects, η_i are firm fixed effects, and X_{it} are firm-level controls (log of sales, operating profit margin, R&D expenses over sales, and modified Altman's z-score; included in specifications Table IV Col 6 and Table V Col 2, Col 6, and Col 8). Specifications Table V Col 2 and Col 6 additionally include firm-level controls for tangibility and average wage. Specifications Table V Col 4 and Col 6 include region-year-industry fixed effects. Specification Table V 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.

	Table IV		Table V			
	Col 4	Col 6	Col 2	Col 4	Col 6	Col 8
Lagged2 Expected Subsidy	-0.00506**	-0.00599***	-0.00587**	-0.00546*	-0.00609**	-0.00873***
	(0.00241)	(0.00231)	(0.00230)	(0.00292)	(0.00270)	(0.00325)
Firm-level controls	No	Yes	Yes	No	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	No	No	Yes
Industry-year FE	Yes	Yes	Yes	No	No	Yes
Region-year-industry FE	No	No	No	Yes	Yes	No
Number of firms	2226	2203	2203	2226	2203	1530
Observations	16889	16562	16562	16889	16562	12563
Within R^2	0.048	0.140	0.142	0.195	0.280	0.145

Internet Appendix: Not for Publication

Internet Appendix Table 1. Capital Structure and Employment Flexibility: Firms outside of the bargaining process

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

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

where D_{it} is the leverage (ratio of total debt to capital) 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, α_{st} are industry-year fixed effects, η_i are firm fixed effects, and X_{it} are firm-level controls (log of sales, operating profit margin, R&D expenses over sales, and modified Altman's z-score; included in specifications Table IV Col 6 and Table V Col 2 and Col 6). The instrument is Lagged2 Expected Subsidy, defined in Section 2. Specifications Table V Col 2 and Col 6 additionally include firm-level controls for tangibility and average wage. Specifications Table V Col 4 and Col 6 include 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. Panel A estimates the specifications for small firms (those below industry median by sales). Panel B estimates the specifications for firms with low productivity growth (those below industry median by the growth of sales per worker). 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.

Panel A: Small firms	Table IV		Table V			
	Col 4	Col 6	Col 2	Col 4	Col 6	Col 8
Lagged Temp	0.0810	0.139*	0.139*	0.114	0.168**	0.242**
	(0.0809)	(0.0773)	(0.0765)	(0.0887)	(0.0831)	(0.106)
Firm-level controls	No	Yes	Yes	No	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	No	No	Yes
Industry-year FE	Yes	Yes	Yes	No	No	Yes
Region-industry-year FE	No	No	No	Yes	Yes	No
Number of firms	1183	1170	1170	1183	1170	795
Observations	8116	8000	8000	8116	8000	5846
F-statistic	33.84	33.89	34.09	23.84	23.57	22.78

Panel B: Low-productivity-growth firms	Table IV		Table V			
	Col 4	Col 6	Col 2	Col 4	Col 6	Col 8
Lagged Temp	0.186**	0.216**	0.211**	0.165*	0.229**	0.308**
	(0.0834)	(0.0862)	(0.0866)	(0.0965)	(0.102)	(0.131)
Firm-level controls	No	Yes	Yes	No	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	No	No	Yes
Industry-year FE	Yes	Yes	Yes	No	No	Yes
Region-industry-year FE	No	No	No	Yes	Yes	No
Number of firms	1885	1862	1862	1885	1862	1323
Observations	8128	7979	7979	8128	7979	6036
F-statistic	38.32	38.40	39.23	24.85	25.94	29.45

Internet Appendix Table 2. Capital Structure and Employment Flexibility: Robustness to Additional Interactions

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

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

where D_{it} is the leverage (ratio of total debt to capital) 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, α_{st} are industry-year fixed effects, η_i are firm fixed effects, and X_{it} are firm-level controls (log of sales, operating profit margin, R&D expenses over sales, and modified Altman's z-score; included in specifications Table IV Col 6 and Table V Col 2 and Col 6). The instrument is Lagged2 Expected Subsidy, defined in Section 2, times employment-to-assets ratio (1st-year value kept constant) in Panel A, and both this one and Lagged2 Expected Subsidy in Panel B. Specifications Table V Col 2 and Col 6 additionally include firm-level controls for tangibility and average wage. Specifications Table V Col 4 and Col 6 include region-year-industry fixed effects. Specification Table V Col 8 estimates the results using only the firms that are present in the sample in 1994. All specifications additionally inlcude the interactions between $Subsidy_{it-2}$ (the subsidy amount, defined similarly to the one in Section 2, but without the w_{Ti0} component, lagged two years and measured in thousand Euro) with the volatility of firm product demand (measured as the standard deviation of the three-valued indicator for the condition of the main market), competition (share in the main market), human capital specificity (share of personnel training costs in total labor costs), productivity (log sales per worker), size (log sales), profitability (operating profit margin), and average leverage. 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.

	Table IV		Table V			
	Col 4	Col 6	Col 2	Col 4	Col 6	Col 8
Lagged Temp	0.138**	0.153**	0.151**	0.161**	0.170***	0.182**
	(0.0612)	(0.0603)	(0.0600)	(0.0659)	(0.0634)	(0.0869)
Firm-level controls	No	Yes	Yes	No	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	No	No	Yes
Industry-year FE	Yes	Yes	Yes	No	No	Yes
Region-industry-year FE	No	No	No	Yes	Yes	No
Additional interactions with $Subsidy_{it-2}$ as controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	14802	14566	14566	14802	14566	10861
Observations	1670	1667	1667	1670	1667	1090
F-statistic	37.80	38.75	39.22	28.57	29.19	40.80

Internet Appendix Table 3. Capital Structure and Employment Flexibility: Robustness to Alternative Instruments

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

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

where D_{it} is the leverage (ratio of total debt to capital) 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, α_{st} are industry-year fixed effects, η_i are firm fixed effects, and Xit are firm-level controls (log of sales, operating profit margin, R&D expenses over sales, and modified Altman's z-score; included in specifications Table IV Col 6 and Table V Col 2 and Col 6). The instrument is Lagged2 Expected Subsidy, defined in Section 2, times employment-to-assets ratio (1st-year value kept constant) in Panel A, and both this one and Lagged2 Expected Subsidy in Panel B. Specifications Table V Col 2 and Col 6 additionally include firm-level controls for tangibility and average wage. Specifications Table V Col 4 and Col 6 include region-year industry fixed effects. Specification Table V 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.

Panel A	Table IV		Table V			
	Col 4	Col 6	Col 2	Col 4	Col 6	Col 8
Lagged Temp	0.200	0.340***	0.333***	0.176	0.297***	0.480***
	(0.122)	(0.119)	(0.119)	(0.115)	(0.111)	(0.182)
Firm-level controls	No	Yes	Yes	No	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	No	No	Yes
Industry-year FE	Yes	Yes	Yes	No	No	Yes
Region-industry-year FE	No	No	No	Yes	Yes	No
Number of firms	2223	2200	2200	2223	2200	1527
Observations	16848	16521	16521	16848	16521	12522
F-statistic	25.24	25.71	25.63	18.89	19.17	16.08

Panel B	Table IV		Table V			
	Col 4	Col 6	Col 2	Col 4	Col 6	Col 8
Lagged Temp	0.132**	0.147**	0.143**	0.143**	0.153**	0.203**
	(0.0599)	(0.0574)	(0.0572)	(0.0665)	(0.0617)	(0.0844)
Firm-level controls	No	Yes	Yes	No	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	No	No	Yes
Industry-year FE	Yes	Yes	Yes	No	No	Yes
Region-industry-year FE	No	No	No	Yes	Yes	No
Number of firms	2223	2200	2200	2223	2200	1527
Observations	16848	16521	16521	16848	16521	12522
F-statistic	19.07	19.44	19.55	15.12	15.40	20.68

Internet Appendix Table 4. Capital Structure and Employment Flexibility: Cash-Abundant and Short-of-Cash Firms

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

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

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, α_{st} are industry-year fixed effects, η_i are firm fixed effects, and X_{it} are firm-level controls (log of sales, operating profit margin, R&D expenses over sales, and modified Altman's z-score; included in specifications 2, 4, and 6). In Panel A specifications 1 and 2 (3 and 4) estimate the above specification for firms with cash flow over total assets above industry median in year t-2 (t). Specifications 5 and 6 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. In Panel B similar specification are estimated for the complementary subsampe of low cash flow firms. 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					
Panel A: High cash flow firms	Operating Cash Flow / above industry median in year t-2		Operating Cash Flow / above industry median in year t		Accumulated OCF / Assets above industry median in year t	
	1	2	3	4	5	6
Lagged Temp	0.276**	0.277**	0.194**	0.206**	0.213*	0.205
	(0.125)	(0.126)	(0.0888)	(0.0855)	(0.125)	(0.141)
Firm-level controls	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	1546	1532	1720	1699	1441	1425
Observations	7005	6895	8220	8062	6948	6834
1st stage F-statistic	17.10	17.88	29.72	30.81	18.00	18.13

	IV-2SLS					
Panel B: Low cash flow firms	Operating Cash Flow / below industry median in year t-2		Operating Cash Flow / below industry median in year t		Accumulated OCF / Assets below industry median in year t	
Lagged Temp	0.0878	0.0857	0.113*	0.160**	0.192	0.239*
	(0.100)	(0.0911)	(0.0657)	(0.0685)	(0.143)	(0.137)
Firm-level controls	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	1508	1493	1750	1733	1460	1452
Observations	6603	6496	8086	7946	6986	6903
1st stage F-statistic	23.08	23.26	35.18	34.89	14.66	14.96
Implied estimate of the differenc	0.189	0.191	0.0810	0.0461	0.0207	-0.0347
standard deviation of the differe	(0.143)	(0.142)	(0.0958)	(0.0925)	(0.188)	(0.191)

Internet Appendix Table 5. Capital Structure and Employment Flexibility: Other Dependent Variables

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

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

where Y_{it} are different dependent variables 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, α_{st} are industry-year fixed effects, η_i are firm fixed effects, and X_{it} are firm-level controls (log of sales, operating profit margin, R&D expenses over sales, and modified Altman's z-score; included in specifications Table IV Col 6 and Table 5 Col 2, Col 6, and Col 8). Specifications Table V Col 2 and Col 6 additionally include firm-level controls for tangibility and average wage. Specifications Table V Col 4 and Col 6 include region-year-industry fixed effects. Specification Table V 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.

All panels	Table IV		Table V			
	Col 4	Col 6	Col 2	Col 4	Col 6	Col 8
Firm-level controls	No	Yes	Yes	No	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	No	No	Yes
Industry-year FE	Yes	Yes	Yes	No	No	Yes
Region-industry-year FE	No	No	No	Yes	Yes	No
Panel A	Net Flow of Credit					
Lagged Temp	0.229***	0.304***	0.272***	0.224***	0.267***	0.230***
	(0.0833)	(0.0931)	(0.0936)	(0.0792)	(0.0951)	(0.0846)
Number of firms	2223	2181	2181	2223	2181	1512
Observations	16878	16352	16352	16878	16352	12394
Panel B	Net Flow of Equity					
Lagged Temp	0.0506	0.0597	0.0523	0.0427	0.0512	0.115**
	(0.0401)	(0.0430)	(0.0426)	(0.0398)	(0.0437)	(0.0507)
one-sided p-value for H_0: $\beta \leq 0$	0.10	0.08	0.11	0.14	0.12	0.01
Number of firms	2201	2181	2181	2201	2181	1512
Observations	16669	16352	16352	16669	16352	12394
Panel C	Trading on an Exchange (dummy)					
Lagged Temp	-0.0154	-0.0168	-0.0170	-0.0590	-0.0485	-0.0658
	(0.0450)	(0.0441)	(0.0436)	(0.0626)	(0.0583)	(0.0729)
Number of firms	1493	1413	1413	1493	1413	976
Observations	3765	3550	3550	3765	3550	2510
Panel D	Being Part of a Group (dummy)					
Lagged Temp	-0.409**	-0.246	-0.245	-0.399**	-0.235	-0.503**
	(0.181)	(0.165)	(0.167)	(0.197)	(0.190)	(0.246)
Number of firms	1492	1412	1412	1492	1412	975
Observations	3763	3548	3548	3763	3548	2508

Internet Appendix Table 6. Cross-Sectional Heterogeneity in Bankruptcy Costs: First-Stage Estimates

This table reports the results of the first-stage of Table VIII IV-2SLS specification:

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

where D_{it} is the leverage (ratio of total debt to capital) 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, α_{st} are industry-year fixed effects, η_i are firm fixed effects, and X_{it} are firm-level controls (log of sales, operating profit margin, R&D expenses over sales, and modified Altman's z-score; included in specifications 2 and 4). $High_{i0}$ 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 (specifications 1 and 2) or as having less buildings and land that the industry median (specifications 3 and 4), 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		First Stage		First Stage		First Stage	
	1 $High_{i0}$*$Temp_{it}$	1 Low_{i0}*$Temp_{it}$	2 $High_{i0}$*$Temp_{it}$	2 Low_{i0}*$Temp_{it}$	3 $High_{i0}$*$Temp_{it}$	3 Low_{i0}*$Temp_{it}$	4 $High_{i0}$*$Temp_{it}$	4 Low_{i0}*$Temp_{it}$
$High_{i0}$ * Lagged Expected Subsidy	-0.0265***	-0.00402***	-0.0264***	-0.00431***	-0.0280***	-0.00441***	-0.0277***	-0.00462***
	(0.00560)	(0.00137)	(0.00577)	(0.00137)	(0.00574)	(0.00137)	(0.00578)	(0.00136)
Low_{i0} * Lagged Expected Subsidy	0.000672	-0.0393***	0.000566	-0.0395***	-0.000433	-0.0376***	-0.000630	-0.0379***
	(0.000867)	(0.00580)	(0.000907)	(0.00564)	(0.00124)	(0.00545)	(0.00130)	(0.00543)
Firm-level controls	No		Yes		No		Yes	
Firm FE	Yes		Yes		Yes		Yes	
Region-year FE	Yes		Yes		Yes		Yes	
Industry-year FE	Yes		Yes		Yes		Yes	
Number of firms	2141		2121		2141		2121	
Observations	16291		16039		16291		16039	
1st stage F-statistic	12.21		11.77		12.50		11.99	

Internet Appendix Table 7. Cross-Sectional Heterogeneity in Bankruptcy Costs: Robustness to Other Correlates

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

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

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, α_{st} are industry-year fixed effects, η_i are firm fixed effects, and X_{it} are firm-level controls (log of sales, operating profit margin, R&D expenses over sales, and modified Altman's z-score; 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 less buildings and land that the industry median, in the year the firm enters the data, and 0 otherwise. Low_{i0} is equal to 1-$High_{i0}$. $HighVar_{i0}$ is the dummy variable that equals 1 if the firm has sales above the industry median (columns 3 and 4), profitability above the industry median (columns 5 and 6), is in highly-competitive environment (columns 7 and 8), all three together (columns 9 and 10), all in the year the firm enters the data, and 0 otherwise. 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.

	Base		With additional splits							
	Table VII Col 3	Table VII Col 4	High LnSales		High Profitability		High Competition		All three together	
	1	2	3	4	5	6	7	8	9	10
$High_{i0}$ * Lagged Temp	0.318***	0.400***	0.339***	0.428***	0.329***	0.415***	0.340**	0.376**	0.371**	0.423**
	(0.0966)	(0.0976)	(0.0948)	(0.0952)	(0.107)	(0.106)	(0.170)	(0.160)	(0.173)	(0.165)
Low_{i0} * Lagged Temp	-0.00863	-0.00544	0.0601	0.0907	0.00608	0.0154	-0.114	-0.130	-0.0404	-0.0127
	(0.0758)	(0.0687)	(0.102)	(0.0952)	(0.0881)	(0.0817)	(0.145)	(0.138)	(0.209)	(0.207)
Firm-level controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	2141	2121	2140	2120	2139	2119	1341	1326	1339	1324
Observations	16291	16039	16279	16027	16271	16019	11150	10964	11130	10944
Implied estimate of the difference	0.327***	0.405***	0.278**	0.337**	0.322***	0.400***	0.454***	0.505***	0.411*	0.435**
standard deviation of the difference	(0.116)	(0.112)	(0.136)	(0.134)	(0.114)	(0.111)	(0.175)	(0.168)	(0.211)	(0.210)

Internet Appendix Table 8. Average Wages and Employment Flexibility

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

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

where Y_{it} ia average wage bill per employee 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, α_{st} are industry-year fixed effects, η_i are firm fixed effects, and X_{it} are firm-level controls (log of sales, operating profit margin, R&D expenses over sales, and modified Altman's z-score; included in specifications Table IV Col 6 and Table 5 Col 2, Col 6, and Col 8). Specifications Table V Col 2 and Col 6 additionally include firm-level control for tangibility. Specifications Table V Col 4 and Col 6 include region-year-industry fixed effects. Specification Table V 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.

Panel A	Table IV		Table V			
	Col 4	Col 6	Col 2	Col 4	Col 6	Col 8
Lagged Temp	1,183	2,238	2,194	1,508	2,577	1,546
	(1,968)	(2,128)	(2,124)	(2,203)	(2,458)	(2,753)
Firm-level controls	No	Yes	Yes	No	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	No	No	Yes
Industry-year FE	Yes	Yes	Yes	No	No	Yes
Region-industry-year FE	No	No	No	Yes	Yes	No
Number of firms	2264	2203	2203	2264	2203	1530
Observations	17420	16562	16562	17420	16562	12563
F-statistic	38.99	40.55	40.33	30.21	31.28	41.97