Implications of the Spanish Bankruptcy Reform on Corporate Cost of Debt

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Abstract

This paper examines the effect of the Royal Decree Law 3/2009 of 27 March 2009 on the cost of debt for Spanish corporations. Using a difference-in-differences framework with a control group composed of the most credit-worthy Spanish firms, we find that, following the reform, the cost of debt decreases more for the less credit-worthy Spanish firms than for the most credit-worthy ones. However, when a sample of Portuguese firms is instead used as control group, the reform appears to increase the cost of debt for Spanish firms. We conclude that the results obtained are insufficient to evaluate the effect of the reform with certainty. In particular, potential effects of the reform are hard to isolate from other simultaneous developments taking place during the financial crisis.

Keywords: Spain, Bankruptcy, Law, Cost of Debt.

Acknowledgements

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1 INTRODUCTION

The aim of this paper is to study the effects of the recent Spanish bankruptcy reform, the Royal Decree Law 3/2009 dated 27 March 2009, on Spanish firms' cost of debt. It is not required that corporate funding costs are explicitly addressed by the lawmaker as one of the aims of the reform in order for this topic to serve interest. On the contrary, the impact on the corporate climate can still be far-reaching if such reform significantly contributes in reducing firms' cost of debt. In the present times of European fiscal austerity, it is particularly interesting to look for ways to stimulate the economy that have little or no effect on the government budget. Properly conducted, legal reforms have the potential to achieve this objective, and in light of this, the following question is posed: *Does there exist a causal effect of the Royal Decree Law 3/2009 on the cost of debt for Spanish firms?*

The paper is organised as follows. Sections 1.1 and 1.2 provide an overview of previous research and recent proceedings in Spanish bankruptcy law. Following the hypothesis formulated in Section 1.3, the empirical framework is exhibited in Section 2. We discuss the selection of treatment and control groups in the difference-in-differences framework in Section 2.1, and in Section 2.2 we introduce the various model specifications that have been used. Section 2 concludes with a presentation of the utilised dataset. Results are examined in Section 3, and corresponding tables and figures can be found in Appendix A. Finally, the paper is concluded in Section 4 with a discussion of the obtained results.

1.1 PREVIOUS RESEARCH

A large subset of the existing literature on corporate bankruptcy studies the direct costs associated with such proceedings, i.e. all expenses that are necessary in order to carry out reorganisation and liquidation processes. Other contributions investigate the indirect costs of bankruptcy, i.e. those generated by the corporate response of a legal procedure (for a deeper survey of the differences between direct and indirect costs of bankruptcy we refer to Senbet and Seward (1995) [14]).

When analysing the indirect costs of bankruptcy several articles adapt a cross-country perspective, for example Qian and Strahan (2007) [10] and Bae and Goyal (2009) [4]. Using a cross-country panel of large firms, Qian and Strahan (2007) [10] shows that stronger credit protection implies more concentrated ownership among loans, longer maturities and lower interest rates. Bae and Goyal (2009) [4] exploits the differences in legal protection of creditors across 48 countries to affirm that banks respond adversely to poor debt enforcement by reducing loan amounts, shortening loan maturities and increasing interest rate spreads on loans.

A second approach in the analysis of indirect costs of bankruptcy is to employ a within-

country perspective, as in Berkowitz and White (2004) [6] and Benmelech and Bergman (2011) [5]. Berkowitz and White (2004) [6] investigates the effect of personal bankruptcy laws on small firms' access to credit and finds that the supply of credit falls when unincorporated firms are located in states with higher homestead exemptions. Benmelech and Bergman (2011) [5] proposes that previous bankruptcies within an industry contribute to increasing the cost of external debt finance industry-wide.

For the purpose of this paper, the articles of greatest interest are those that discuss the effect of bankruptcy reforms on the cost of funding. Scott and Smith (1986) [13] shows that the additional monitoring and foreclosure costs imposed by the 1978 U.S. corporate bankruptcy law reform has raised the cost of funding. Moreover, Araújo et al. (2011) [3] finds that the 2005 Brazilian bankruptcy reform resulted in a significant increase in the total amount of debt and a reduction in the cost of debt, by employing a difference-in-differences setup where other South American countries constitute the control group. Finally, Rodano et al. (2011) [11] exploits the staggered nature of the 2005-2006 Italian bankruptcy law reform to determine the separate effects of both reorganisation and liquidation procedures in bankruptcy on firms' cost of loan financing. In addition, von Lilienfeld-Toal et al. (2012) [15] assumes inelastic supply of credit and investigates the general equilibrium effect that arises following improvement in borrowers' credit-worthiness. In short, such improvement among borrowers causes an outward shift in the aggregate demand function for credit, giving rise to a partial equilibrium. However, since the supply of credit is inelastic not all of the added demand can be priced at the current interest rate. This gives rise to a general equilibrium with higher interest rates that has a negative effect on the amount of credit outstanding. Simply put, the partial equilibrium increases credit access whereas the general equilibrium causes borrowers' credit access to decrease.

The study of the Spanish bankruptcy reform conducted in this paper draws on concepts and ideas elaborated upon in Araújo et al. (2011) [3] and Rodano et al. (2011) [11]. In particular, the triple differences model employed in the former, as well as the method for constructing a suitable control group employed in the latter, are helpful tools that facilitate our study.

1.2 The Spanish Bankruptcy Reform

Corporate bankruptcy proceedings in Spain are primarily governed by the Spanish Insolvency Law 22/2003. Dated 9 July 2003, this relatively recently passed law aimed to modernise Spanish insolvency legislation by making it "less archaic and dispersed and by assuring equality in the treatment of creditors" [9].

As promising the new legislation might have seemed at the time of its passing, a steep

rise in the number of Spanish bankruptcies amid the financial crisis came to highlight certain inefficiencies in the judicial proceedings. Legislators were led to conclude that additional measures needed to be taken to further improve the competitiveness of the Spanish economy. Some of the issues encountered during the crisis were subsequently addressed in the Royal Decree Law 3/2009 of 27 March 2009, which with regard to bankruptcy proceedings made several important changes and amendments to the Spanish Insolvency Law.

The key objectives of the reform were to ease the refinancing procedure of companies in financial distress, to make the insolvency process quicker and less costly, to extend the time period during which agreements between creditors and debtors can be negotiated in order to avoid initiation of full-scale insolvency proceedings, and to, under certain conditions, improve the judicial position of employees at companies involved in insolvency proceedings [8]. For the purpose of this paper, the former two objectives of the Royal Decree Law are particularly interesting, as they can be argued to affect the cost of debt faced by Spanish corporations.

The refinancing of companies is facilitated by allowing for exceptions to be made from Article 71 of the Spanish Insolvency Law, which allows for creditors to cancel harmful actions undertaken by the debtor during two years prior to the declaration of the insolvency proceedings. The possibility of making exceptions from Article 71 makes refinancing a more viable alternative for companies in distress, since real guarantees made in the process are less likely to be canceled by creditors. However, for an exception to be made, creditors whose credits amount to no less than 60% of the total debt due need to agree with the refinancing plan [8].

Perhaps the most important part of the Spanish bankruptcy reform is the attempt to expedite the insolvency process, thereby increasing creditors' recovery rates by reducing costs associated with the insolvency process and increasing the present value of the settlements due. This is achieved by allowing for more proceedings to be conducted in an abbreviated fashion as defined in the Spanish Insolvency Law. As a result of the reform, the feasible set of companies was increased to include all companies whose estimated debt prior to the proceedings did not exceed €10 million, as opposed to €1 million before the reform [8].

1.3   HYPOTHESIS

In light of the changes to bankruptcy legislation inherent in the Royal Decree Law 3/2009, it is plausible to believe that a reduction in corporate cost of debt should be observable following the reform. As described in Section 1.2, the main points of the reform with regards to bankruptcy are to facilitate the refinancing of corporations and to expedite

the insolvency process. The impact of the former on corporate cost of debt is somewhat ambiguous, as refinancing of firms is eased by giving creditors the option (but not the obligation) to renounce certain legal rights. Conditional on bankruptcy, there is no dispute that such a renouncement makes the creditors worse off with regards to recovery rate. However, since the renouncement requires that a majority of the creditors accept the terms of the agreement, it can be assumed that it will not take place unless it makes the creditors better off. I.e. the creditors will only renounce their rights if they believe their claims are more likely to be repaid due to increased longevity of the borrower.

With regards to corporate cost of debt, the implication of the latter point of the reform, to expedite the insolvency process, is more straightforward. Drawing on the discussion in Section 1.2, corporate cost of debt should be expected to decrease as a result of higher recovery rates anticipated by creditors, in turn caused by quicker and less costly court proceedings. With this in mind, the hypothesis of the paper is formulated as follows.

The Spanish bankruptcy reform should serve to lower corporate cost of debt. Following the passing of the Royal Decree Law 3/2009 in March 2009, we believe that a decrease in Spanish firms' cost of debt can be observed, and furthermore that the decrease is largest in magnitude for the least credit-worthy Spanish firms.

2 EMPIRICAL FRAMEWORK

In this section we present the empirical framework used to capture the effect of the Spanish bankruptcy reform on the cost of debt. We employ a variety of difference-in-differences (DID) setups, suitably designed to address the issue at hand. At the center stands the question of how to divide the data into treatment and control groups. To add breadth to the study, we solve this task using within-country as well as cross-country approaches.

Throughout the study we rely on accounting data organised in panels, be it to compute dependent variables such as the cost of debt, or to control for firm specific factors affecting the former. We employ ordinary least squares (OLS) regression models, with and without fixed effects (FE), where we allow for firm and industry fixed effects, respectively. In addition, we also use a modified approach of the DID-setup where we allow for different industry-wide linear trends across time (referred to as the triple differences model).

2.1 SELECTION OF TREATMENT AND CONTROL GROUPS

When using a DID-setup (for a survey of DID-estimation the reader is referred to Wooldridge (2009) [16]) to evaluate the effect of some policy or event on a particular group of objects or individuals (treatment group), one needs a control group which is assumed to be unaffected by the same policy or event. Hence, to correctly assess the effect of the Spanish bankruptcy reform, the choice of treatment and control groups is of critical importance. We use two different approaches when determining control and treatment groups: a within-country approach and a cross-country approach.

The within-country approach implies that both the treatment and the control group consist solely of Spanish companies. This is advantageous in the sense that it mitigates the risk of nation-specific macro shocks (or other nation specific developments) adversely affecting the DID-estimator. However, it also requires an assumption being made, namely that some subset of the Spanish companies is unaffected by the reform and can serve as control group. Inspired by Rodano et al. (2011) [11], we assume that the most credit-worthy Spanish firms are unaffected by the reform. The rationale behind this assumption is that since the safest firms very rarely fail on their payments, improvements of the bankruptcy proceedings from a creditor's perspective are unlikely to affect the cost of debt for these firms.

There is, however, a potential problem with the within-country approach that deserves recognition at this stage. The reform whose effects we are investigating took place in March 2009, in the midst of the financial crisis. Even if it is correct to assume that credit-worthy Spanish companies were unaffected by the reform, there is a possibility that other developments took place during the turmoil which affected the cost of debt

of credit-worthy and non-credit-worthy firms in different ways. Using the within-country approach, such effects cannot be separated from the effect of the reform, which poses a problem. Despite this problem, we choose to proceed with the within-country approach. It can be argued that financial turmoil should disfavour non-credit-worthy firms more than credit-worthy ones with respect to cost of debt, as has been the case in the sovereign bond market. If, despite this, a decrease in the difference in cost of debt between non-safe and safe firms is observed in the midst of the crisis, it is plausible that institutional development is the driving factor behind the decline.

To determine which firms belong to the control group, we compute a statistic similar to the Altman score for private firms, proposed in Altman (1968, 2000) [1; 2], and select the firms above the 75th percentile as control group. The Altman score, Z, is widely regarded as a good estimation of the credit-worthiness of firms, and is computed as

$$Z = 0.717 \cdot X_1 + 0.847 \cdot X_2 + 3.107 \cdot X_3 + 0.420 \cdot X_4 + 0.998 \cdot X_5,$$

where X_1 to X_5 equal the ratios of working capital to total assets, retained earnings to total assets, EBIT to total assets, book value of equity to total liabilities, and sales to total assets, respectively. Data on retained earnings was not available for the vast majority of firms in our sample, whereby X_2 has been omitted from the above formula. For the purpose of selecting the safest firms to be included in the control group, this slight adjustment to the Altman score should not pose any problems.

In the cross-country approach, Spanish firms naturally compose the treatment group, and Portuguese firms are chosen as control group. The rationale behind choosing Portugal as control group is that Portuguese firms were unaffected by the Spanish reform, and that the close cultural and geographical proximity of the countries can be assumed to generate a somewhat similar corporate environment. While possible time-invariant differences between treatment and control groups are accounted for in a DID-setting, there is, however, a risk that country-specific developments other than the reform under study adversely affect the accuracy of the DID-estimate.

2.2 Model Specifications

Let r_{it} denote the cost of debt for firm i during year t, defined as the ratio of interest paid during year t to total long-term debt at the beginning of year t. Furthermore, let $\mathbf{X}_t \in \mathbb{R}^5$ denote a vector of time dummy variables for the years 2006 to 2010, and let $Treatment_i$ be a dummy variable equal to one if firm i belongs to the treatment group and zero otherwise. We let $Post_t$ be another time dummy, equal to one if t is greater than or equal to 2009, i.e. if the reform has taken place, and equal to zero otherwise.

$Revenues_{it}$ and $Assets_{it}$ are the revenues and assets of firm i in thousands of dollars, generated during year t and held at the end of year t, respectively. Let $\log(Revenues_{it})$ and $\log(Assets_{it})$ denote the natural logarithms of the respective variables. $Leverage_{it}$ is the leverage of firm i at the end of year t, defined as the ratio of long-term debt to total assets. These three controls are included as they are considered likely to have influence on a firm's cost of debt. For example, firms with low leverage should have access to cheaper funding than highly levered counterparts, everything else equal. A naive OLS regression model is set up as follows:

$$r_{it} = \beta_0 + \beta_1 \cdot Treatment_i + \beta_2 \cdot Treatment_i \cdot Post_t + \beta_3 \cdot \log(Revenues_{it})$$
$$+ \beta_4 \cdot \log(Assets_{it}) + \beta_5 \cdot Leverage_{it} + \mathbf{\Gamma}^{\mathrm{T}}\mathbf{X}_t + \epsilon_{it},$$

where ϵ_{it} is the error term.

In addition to this OLS model, we model the effect of the reform on firms' cost of debt in a fixed effects framework. This requires making a few assumptions, of which some are discussed in the following. Consider a general fixed effects model

$$y_{it} = \beta_1 x_{it1} + \cdots + \beta_k x_{itk} + a_i + u_{it}, \ t \in \{1, \ldots, T\},$$

where a_i is the unobserved time-fixed effect for panel i, and assume that the model is valid. For the estimators $\hat{\beta}_1, \ldots, \hat{\beta}_k$ to be unbiased, it is required that sampling from the cross section is random, that no explanatory variable is constant over time for all i nor is a linear combination of other explanatory variables, and finally that for each t

$$E\left(u_{it} \mid \mathbf{X_i}, a_i\right) = 0,$$

where $\mathbf{X_i}$ comprises the explanatory variables $x_{it1}, \ldots, x_{itk}$ in all time periods $t = 1, \ldots, T$. (Wooldridge (2009) [16])

For the objective of this paper, we model the effect of the reform on firms' cost of debt under the respective assumptions of industry and firm fixed effects. Let j denote the industry of firm i, where each firm's industry is determined by the four digit NACE code. In the case of industry fixed effects, let r_{ijt} be the cost of debt for firm i in industry j during year t. The model specification becomes

$$r_{ijt} = \beta_0 + \beta_1 \cdot Treatment_i + \beta_2 \cdot Treatment_i \cdot Post_t + \beta_3 \cdot \log(Revenues_{it})$$
$$+ \beta_4 \cdot \log(Assets_{it}) + \beta_5 \cdot Leverage_{it} + \mathbf{\Gamma}^{\mathrm{T}}\mathbf{X}_t + a_j + \epsilon_{it},$$

where a_j is the industry fixed effects term.

In the case of firm fixed effects, the specification is

$$r_{it} = \beta_0 + \beta_1 \cdot Treatment_i \cdot Post_t + \beta_2 \cdot \log(Revenues_{it}) + \beta_3 \cdot \log(Assets_{it})$$
$$+ \beta_4 \cdot Leverage_{it} + \mathbf{\Gamma}^{\mathrm{T}}\mathbf{X}_t + b_i + \epsilon_{it},$$

where b_i is the firm fixed effects term.[1] Moreover, $Treatment_i$ is superfluous in this specification and is thus omitted.

The question now arises whether the respective models are valid with respect to the fixed effects assumptions explained above. In particular, to obtain unbiased estimates, it needs to hold for each t that the expected value of the error term ϵ_{it} is zero conditional on the explanatory variables and the respective fixed effects terms. This is clearly not something that can be taken for granted; the model may be misspecified and variables that truly affect the cost of debt may have been omitted. One such variable is e.g. the duration of the companies' respective debt portfolios. Loans with longer maturity are associated with higher risk for the creditors, which serves to increase interest rates and hence the cost of debt faced by the borrowers. Since no information on duration is available in the dataset used for this paper, there is a risk of omitted variable bias adversely affecting the precision in the DID-estimates ($\hat{\beta}_2$ and $\hat{\beta}_1$ in the respective cases of industry and firm fixed effects).

Despite the fact that omitting a variable can bias the estimates of all coefficients in the model, including the DID-estimate, the fact that an event study is conducted is a source of some comfort. The model is not to be used for predicting future costs of debt. Instead we are only interested in the effect of the reform, and it has to be considered as very unlikely that an omitted variable would cause bias in the DID-estimate to the degree that a potential conclusion no longer holds, i.e. that the sign of the DID-estimate is reversed.

The DID-assumption that the same macro trend prevails across both the treatment and the control groups could be unrealistic when considering how these are formed. While it is rather obvious that Spain and Portugal need not be subject to the same trend, the same argument holds when considering safe versus less safe firms in the within-country approach. Nevertheless, it should not come as a surprise that a firm's credit-worthiness is determinant in how it is affected by macroeconomic conditions. In light of this, we adapt a model where we allow for different linear trends between industries, as described in [7].

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[1]For the different fixed effects specifications, standard errors are clustered on industry and firm level, respectively.

2 EMPIRICAL FRAMEWORK

More specifically, the model specification becomes

$$r_{ijt} = \beta_0 + \beta_1 \cdot Treatment_i + \beta_2 \cdot Treatment_i \cdot Post_t + \beta_3 \cdot \log(Revenues_{it})$$
$$+ \beta_4 \cdot \log(Assets_{it}) + \beta_5 \cdot Leverage_{it} + \mathbf{\Gamma}^{\mathrm{T}}\mathbf{X}_t + a_j + b_j t + \epsilon_{it},$$

where we have introduced an industry specific linear trend term b_j. This model allows for a good deal of heterogeneity, as firms of different industries are allowed to exhibit different trends. It would however be preferable to include a model where firm specific linear trends are allowed, something that was unattainable due to lack of computer power. When implementing this model, we create industry dummies that are interacted with the time variable. A standard industry fixed effects regression is then run where we control for the time-industry interacted variables.

2.3 DATA RETRIEVAL AND DESCRIPTIVE STATISTICS

We collect yearly accounting data from Orbis between 2004 and 2010 on the 14000 largest companies (sorted by assets) in Spain and Portugal, respectively. Observations from financial institutions, e.g. banks and insurance companies, are disregarded as such institutions might be net lenders, for example. For each remaining firm and year, an Altman score is generated where possible. The Altman scores are then averaged over time, and the mean Altman score for each firm is used in the following. Firms with Altman scores below -1000 and above 1000 are dropped as they are considered outliers and are caused, proposedly, by data flaws. The remaining distribution of Spanish Altman scores in the range $[-100, 100]$ is pictured in Figure A.3.

The dependent variable and control variables are generated as described in Section 2.2. Observations with costs of debt below 0 and above 0.15 are dropped. The reason for dropping negative costs of debt is obvious, and the upper limit of 0.15 is imposed as higher actual values are regarded unlikely. One explanation for their appearance in the dataset is that the interest paid during a year in some cases differs significantly from the actual interest expense during that year. The reason for not imposing an even lower upper limit on the cost of debt is that there might exist interest-bearing debt not being of long-term character, which contributes in decreasing the actual cost. In this sense it would be preferable to use total debt rather than long-term debt as the denominator, however such data was not possible to retrieve. Observations with negative revenues and/or assets are also dropped, as such values should be impossible to obtain by definition of the respective variables. Finally, observations from 2004 are dropped as they only are used when generating variables to be included in the model specifications.

Tables A.1 and A.2 present summary statistics for the variables *Cost of Debt*,

Leverage, *Revenues* and *Assets*, for both the treatment and the control groups. While Table A.1 includes statistics for the within-country approach, Table A.2 presents data for the cross-country approach with Portugal as the control group. For example, it is shown in Table A.1 that the less credit-worthy firms indeed have (on average) higher costs of debt, whereas Portuguese firms (quite surprisingly) seem to have lower costs of debt than their Spanish counterparts.

Figure A.1 displays the average cost of debt over time for firms in the control group and in the treatment group for the within-country approach, as well as the difference in average cost of debt. The figure shows that the difference in cost of debt between the two groups indeed decreases after the reform. This result coincides with our hypothesis. However, in Figure A.2 the corresponding graph for the cross-country approach is pictured. Here one sees that the difference in cost of debt actually increases after the reform, which in hindsight is a result that disagrees with our hypothesis.

3   RESULTS

In this section the results of the empirical investigation are presented. The four models developed in Section 2.2 are implemented and the variables used are computed as described in Section 2.3. We find that the results vary depending on the manner in which treatment and control groups are constructed. The section concludes by comparing the obtained results.

3.1   THE WITHIN-COUNTRY APPROACH

This approach relies on the separation of Spanish firms into two groups, one of which is assumed to be unaffected by the bankruptcy reform. As described in Section 2.1, each firm is assigned an Altman score, where the control group comprises all firms with scores above the 75th percentile. Regressions are run in accordance with the models presented in Section 2.2, and the results are presented in Table A.3. Columns 1 through 3 report results from the naive OLS specification and the industry and firm fixed effects specifications, respectively. In column 4 results from the model specification that allows for different linear trends between industries are displayed. The coefficients of most interest are obviously those on $Treatment$ and, even more so, on $Treatment \cdot Post$. With a positive, significant, coefficient on $Treatment$ through all four model specifications, it is clear that firms with lower Altman scores on average face higher costs of debt. With everything else equal, this difference equals approximately one percentage point. However, with a negative and significant coefficient on $Treatment \cdot Post$ in all four columns, the results tell us that Spanish firms indeed seem to face lower costs of debt following the reform, at least in comparison with those firms assumed unaffected by the event. This results agrees with what one might expect after studying Figure A.1. It should be emphasised that the most reliable results perhaps are those presented in columns 3 and 4 (due to their more rigorous specifications), however all columns exhibit relatively small quantitative differences between each other.

The same experiment was conducted with the minor difference that firms with Altman scores between the 25th and 75th percentile were excluded. The outcomes from these regressions are shown in Table A.4, and it is clear that the results exhibit no qualitative differences compared to those in Table A.3. However, the negative sign on $Treatment \cdot Post$ is greater in magnitude in this experiment, which tells us that the least credit-worthy firms seem to be those that the bankruptcy reform had greatest effect on. While this is by no means surprising, one peculiar fact is how the coefficient on $Treatment$ in fact has smaller magnitude compared to the results in Table A.3. This means that the least credit-worthy firms had lower costs of debt before the reform compared to the more

credit-worthy firms being in the 25th to 75th percentiles sorted by Altman score.

While we do not make any causal statements on the interpretation of the control variables, some words should be said on their impact and possible interpretation. As the coefficient on *Leverage* is nowhere significant, essentially nothing can be said about its impact. This is rather surprising as one might suspect that leverage has determinant impact on the magnitude of a firm's cost of debt. Also, the positive sign on $\log(Revenues)$ differs from the results of Santos (2010) [12] and Rodano et al. (2011) [11]. Similarly, the predominantly negative signs[2] on $\log(Assets)$ suggest that the cost of debt decreases when assets increase, a result which conflicts with that obtained by Rodano (2011) [11]. It should however be noted that these control variables might be correlated with *Treatment* through the formula that determines a firm's Altman score, which would make the coefficients on these variables difficult to interpret. In addition, if it were possible to also account for the maturity of debt, perhaps our results would change in this respect as there is likely a correlation between the duration of a firm's debt portfolio and its other characteristics.

To summarise, these results coincide with the hypothesis of Section 1.3, as we indeed see a reduction in the cost of debt for Spanish firms following the bankruptcy reform of 2009. One must however pay regard to the argument presented in Section 2.1, which if true could cause violation of the assumption that credit-worthy firms were unaffected by the reform. In light of this, we carry out an experiment with Portuguese firms as the control group, results from which are presented in what follows.

3.2   THE CROSS-COUNTRY APPROACH

Contrary to the results obtained by employing a within-country approach, our hypothesis cannot be verified when using Portugal as control group. Instead the Spanish bankruptcy reform appears to have increased the cost of debt faced by Spanish firms compared to the sample of Portuguese counterparts. The results are significant at the one percent level regardless of model specification, i.e. even when a triple-differencing methodology is employed to account for potential differences in industry-wide linear trends. Although somewhat disappointing with regards to our hypothesis, the regression results do not come as a complete surprise in light of Figure A.2, which depicts that an increase in the difference in average cost of debt faced by Spanish firms over Portuguese coincides with the time of the reform.

The four regression models in detail described in Section 2.2 are implemented in the same way as in the within-country approach, except for the obvious difference in the def-

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[2]Except when running the firm fixed effects regressions.

inition of the dummy variable *Treatment*. The results obtained from the different model specifications are presented in Table A.5, and are discussed in the following. But before moving on to analysing the regression output, a closer look at Figure A.2 is warranted. Contrary to what might be expected given the prevailing yields in the sovereign bond market, Portuguese firms face a lower average cost of debt than Spanish firms from 2007 and onwards. As depicted in Table A.2 the mean of cost of debt during the time period under study is 6.43% and 6.84% for Portuguese and Spanish firms, respectively.

As in the within-country approach, the coefficient of interest is that on $Treatment \cdot Post$, the DID-estimate. In all cases the obtained coefficient is positive and significant. It is largest in magnitude when the model is specified with firm fixed effects, which is furthermore the best model with regards to explanatory power as measured by R^2. For the purpose of hypothesis verification this is particularly bad news; as the models become more sophisticated the results deteriorate. The coefficients for the DID-estimate range from 0.00385 in the naive OLS case to 0.00560 in the case of firm fixed effects, and based on these results the effect of the Spanish bankruptcy reform can be interpreted as to increase the average cost of debt for Spanish firms somewhere in the range 0.4 to 0.6 percentage points. On the topic of model specification, it should furthermore be noted that including linear trends on industry level has negligible effect on the results. Upon comparing columns (2) and (4) of Table A.5 it can be seen that the results are nearly identical, i.e. linear time trends, if present, are very small in magnitude and lack economic importance.

Drawing on the concepts developed in von Lilienfeld-Toal et al. (2012) [15], the adverse results encountered in this section may be explained by a general equilibrium effect following the reform. Under the assumption that the supply of credit is not perfectly elastic, it is plausible that there exists an offsetting effect of the reform with regards to firms' cost of debt. Suppose that the reform in fact increases the credit-worthiness of Spanish firms, and as a result of this the demand for credit is shifted outwards. Unless the supply of credit is perfectly elastic, the higher demand will lead to increased interest rates. In light of this, a possible explanation of the observed increase in Spanish firms' cost of debt is that the hypothesised effect of the reform is crowded out by inelasticity in the supply of credit.

The regression coefficients for the control variables $\log(Revenues)$ and $\log(Assets)$ are qualitatively the same as when using the within-country approach. I.e. we again arrive at results different from those of Santos (2010) [12] and Rodano et al. (2011) [11]. Furthermore, in the cross-country approach, the regression coefficients on *Leverage* are significant for all model specifications except when using firm fixed effects. Contrary to what one might expect, they are negative. However, as mentioned earlier, too much

emphasis is not put on the control variables.

3.3   Summary of Results

The results presented in this section are ambiguous. The effect of the Spanish bankruptcy reform on firms' cost of debt varies depending on the DID-setting in which the regressions are performed, i.e. on the choice of respective control and treatment groups. The within-country approach generates results that indicate a decrease in the cost of debt for less credit-worthy firms following the reform, when compared to firms with supposedly smaller probability of default. The magnitude of the decrease is nowhere smaller than 0.55 percentage points and can hence be considered economically significant. Contrary to our hypothesis, these results are non-withstanding when the cross-country approach is employed. When Portugal is used as the control group, the results obtained indicate that the cost of debt for Spanish firms increases more than that of Portuguese firms following the reform. The increase ranges from 0.39 to 0.56 percentage points depending on how the regression is specified, so the economic significance cannot be disputed. The contradictory nature of the results is further elaborated on in Section 4.

4   DISCUSSION

As explained in the previous section the results of the respective within- and cross-country approaches do not conform; the qualitative interpretation of the effect of the Spanish bankruptcy reform differs depending on the choice of control group. It is by no means an easy task to decide which DID-setting is most appropriate, the experimental design is in no case flawless. With regards to capturing a supposed effect of the reform on firms' cost of debt, each choice of control group has both pros and cons, and we conclude that the hypothesis of Section 1.3 cannot be verified based solely on the results presented in this paper.

Inherent in the choice of the most credit-worthy Spanish firms as control group for the less credit-worthy ones, as is the case in the within-country approach, is the assumption that the former are unaffected by the reform. As argued in Section 2.1, the rationale for this assumption is that since safe firms rarely fail on their payments, an increase in the recovery rate of creditors (achieved by expediting the bankruptcy process) should have little or no effect on the cost of debt for these firms. However, as economically compelling as this line of thinking appears, one cannot be sure that the effect of the reform is isolated in such a DID-setting. The within-country approach is perhaps warranted in a stable macroeconomic environment, but the Spanish reform took place in March 2009, in the midst of the financial crisis. The great deal of market turmoil at the time of the reform makes it plausible that other factors than the reform itself might have affected the cost of debt of firms in the treatment group in a different way than firms belonging to the control group. If such is the case, the effect these non-homogeneous (with respect to credit-worthiness) developments cannot be separated from the effect of the reform.

Similar questions can be raised regarding the suitability of Portugal as control group for Spain. As argued in Section 2.1, the two countries have quite a lot of similarities, which is rationale for choosing Portuguese firms as control group in the cross-country approach. However, the dynamics of the financial crisis may have been such that the effect on corporate cost of debt was different in Spain than in Portugal. Portugal was by no means unaffected by the crisis, which poses no problem as long as the effects on firms were similar to those in Spain. Since there is no guarantee that this is the case, we are again faced by circumstances which make it hard to isolate the effect of the reform from other, simultaneous, developments. To lessen the risk of Portugal-specific developments adversely affecting the results of the study, it would perhaps be a good idea to include firms from other countries, e.g. France, in the control group. By composing the control group of firms from different nations, country-specific shocks are mitigated (if present). However, the underlying problem remains.

The implication of the discussion in the previous paragraphs is, among other things,

that it becomes difficult to draw any satisfactory conclusions. On top of the arguments presented there, the dataset at hand could also be scrutinised in more depth. The use of a proxy for a firm's cost of debt as defined by the interest paid over long-term debt is by no means optimal. Firstly, accounting data as provided by annual reports does not necessarily have to reflect reality in an acceptable way. Secondly, we would have preferred if data on total debt, rather than on long-term debt, was available. The lack of data also made it impossible to control for other key variables, such as the maturity of a firm's debt. A suggested approach is therefore to conduct the same experiment with the difference that the interest rate on newly issued corporate loans is used instead of a proxy for a firm's cost of debt. Such procedure would mitigate the possible damage caused by a not fully reliable dataset.

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A   FIGURES AND TABLES

Figure A.1: Cost of Debt (1)
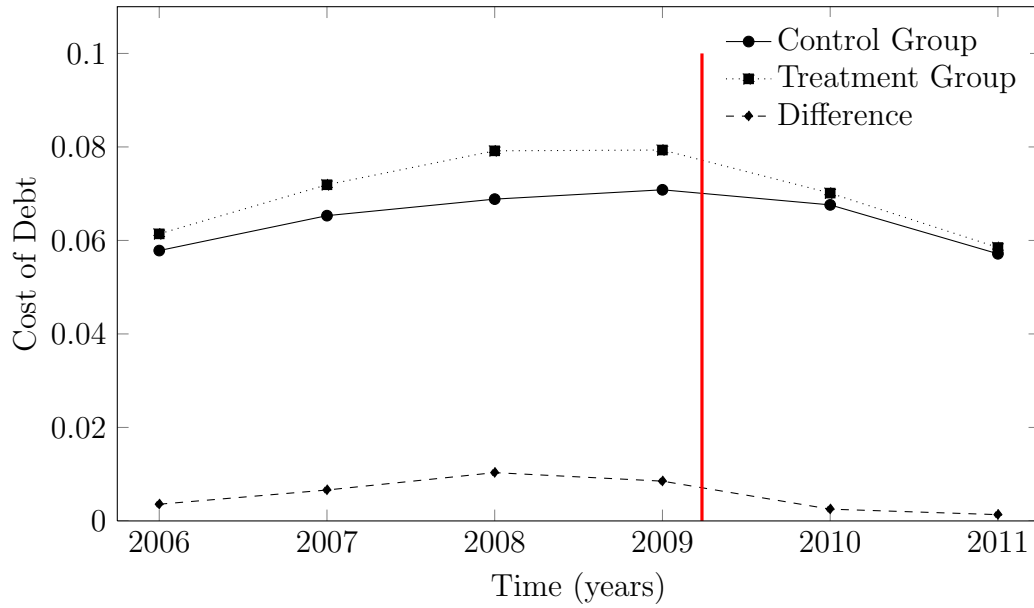
The figure pictures average costs of debt over time, for Spanish firms with Altman scores below (Treatment Group) and above (Control Group) the 75th percentile, respectively. The difference between them is also shown. The vertical line illustrates the time of the reform.

Figure A.2: Cost of Debt (2)
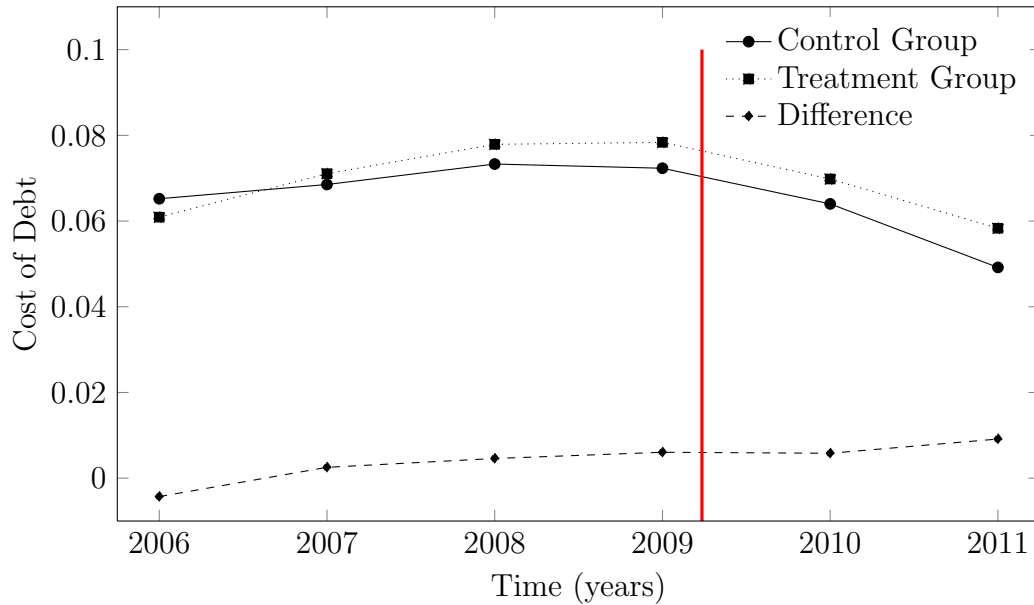
The figure pictures average costs of debt over time, for Spanish (Treatment Group) and Portuguese (Control Group) firms, respectively. The difference between them is also shown. The vertical line illustrates the time of the reform.

Figure A.3: Distribution of Altman Scores for Spain
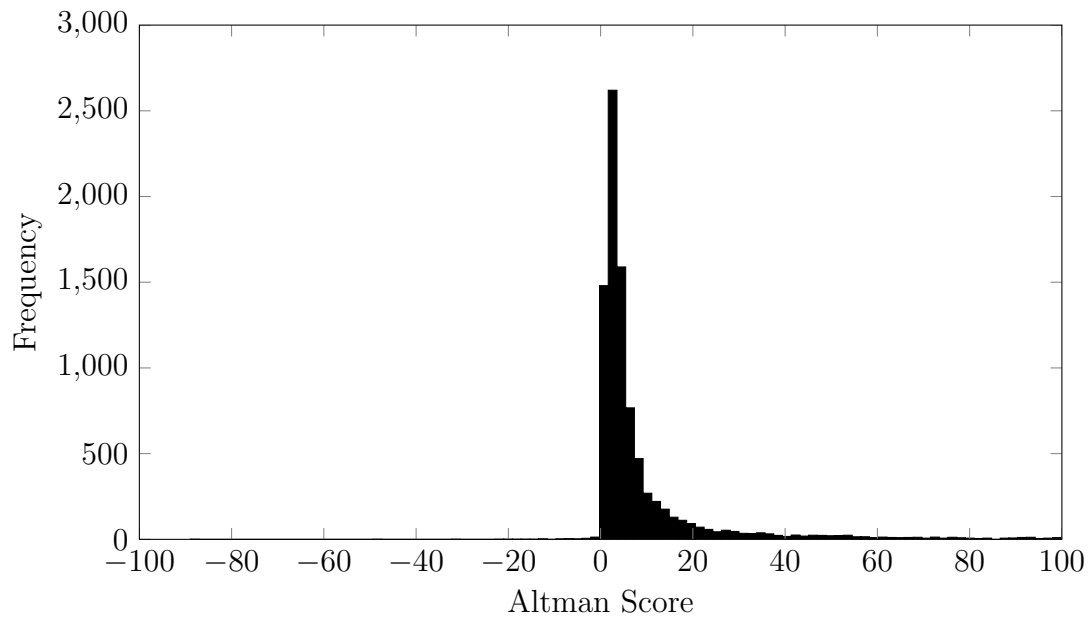
The figure pictures the distribution of Altman scores for Spanish firms. Values above 1000 and below -1000 are dropped and remaining values in the intervals $[-1000, -100]$ and $[100, 1000]$ are for visual purposes not shown here.

Table A.1: Summary Statistics (1)

Variable	Mean	25th Percentile	Median	75th Percentile	Standard Deviation	Min	Max	N
Control Group								
Cost of Debt	0.0638	0.0325	0.0602	0.0930	0.0404	0	0.150	4262
Leverage	0.0658	0.00387	0.0170	0.0645	0.128	0	2.42	4182
Revenues	114000	14300	23200	47800	695000	0	14300000	4262
Assets	119000	9210	18700	46700	697000	45	15400000	4262
Treatment Group								
Cost of Debt	0.0691	0.0419	0.0645	0.0944	0.0363	0	0.150	29739
Leverage	0.259	0.0934	0.193	0.345	1.04	0	172	29271
Revenues	165000	14600	24400	52200	1780000	0	89000000	29739
Assets	286000	12000	26500	76400	3230000	4	173000000	29739

The table reports summary statistics for the variables *Cost of Debt*, *Leverage*, *Revenues* and *Assets*. The latter two variables are reported in thousands of dollars. The results for the control and the treatment groups are reported in separate segments, where the control group consists of those Spanish firms with an Altman score above the 75th percentile and the treatment group consists of the remaining Spanish firms. The presented data is that obtained after removing observations as described in Section 2.3.

Table A.2: Summary Statistics (2)

							Control Group		
Variable	Mean	25th Percentile	Median	75th Percentile	Standard Deviation	Min	Max	N	
Cost of Debt	0.0643	0.0353	0.0603	0.0912	0.0382	0	0.150	31148	
Leverage	0.559	0.143	0.297	0.500	23.5	0	3990	31145	
Revenues	30000	1670	4180	9690	403000	0	21100000	31148	
Assets	59200	4660	7550	16100	783000	0	58000000	31148	
					Treatment Group				
Cost of Debt	0.0684	0.0408	0.0640	0.0943	0.0368	0	0.150	34001	
Leverage	0.235	0.0674	0.167	0.321	0.974	0	172	33453	
Revenues	159000	14600	24200	51800	1690000	0	89000000	34001	
Assets	265000	11500	25300	71900	3030000	4	173000000	34001	

The table reports summary statistics for the variables *Cost of Debt, Leverage, Revenues* and *Assets*. The latter two variables are reported in thousands of dollars. The results for the control and the treatment groups, which consist of Portuguese and Spanish firms respectively, are reported in separate segments. The presented data is that obtained after removing observations as described in Section 2.3.

Table A.3: Impact of Reform on Firms' Cost of Debt (1)

Dependent Variable: *Interest Paid / Long Term Debt*

	(1)	(2)	(3)	(4)
Treatment	0.00972***	0.00927***		0.00916***
	(0.00111)	(0.00130)		(0.00131)
Treatment · Post	-0.00604***	-0.00566***	-0.00592***	-0.00557***
	(0.00139)	(0.00144)	(0.00162)	(0.00147)
2006	0.0104***	0.0109***	0.0142***	0.00519***
	(0.000536)	(0.000493)	(0.000497)	(0.000552)
2007	0.0181***	0.0188***	0.0236***	0.00696***
	(0.000629)	(0.000723)	(0.000649)	(0.000715)
2008	0.0175***	0.0177***	0.0214***	-0.000112
	(0.000624)	(0.00658)	(0.000659)	(0.000724)
2009	0.0147***	0.0139***	0.0149***	-0.00983***
	(0.00139)	(0.00145)	(0.00165)	(0.00139)
2010	0.00251	0.00145	-0.000787	-0.0282***
	(0.00138)	(0.00139)	(0.00165)	(0.00132)
$\log(Revenues)$	0.00592***	0.00471***	0.00295***	0.00468***
	(0.000337)	(0.000966)	(0.000474)	(0.000980)
Leverage	-0.00156	-0.00123	0.000105	-0.00121
	(0.00130)	(0.000976)	(0.000350)	(0.000956)
$\log(Assets)$	-0.00566***	-0.00385***	0.00368***	-0.00387***
	(0.000289)	(0.000927)	(0.000896)	(0.000940)
Firm FE	No	No	Yes	No
Industry FE	No	Yes	No	Yes
N	33419	33419	33419	33419
R^2	0.077	0.071	0.190	0.088

The table reports OLS estimation of the impact of the Spanish bankruptcy reform on firms' cost of debt. Column 1 reports results from the naive OLS specification, whereas columns 2 and 3 include industry and firm fixed effects, respectively. Column 4 reports results from a specification where industry-wide linear trends are allowed. *Treatment* is a binary variable equal to 1 if the firm has an Altman score below the 75th percentile and 0 otherwise. *Post* is a binary variable that equals one in case the reform has taken place, i.e. when $t \geq 2009$. Reported are also time dummies *2006-2010*, as well as several control variables. The constant is included, but not reported. Standard errors are clustered on firm level in columns (1) and (3), and on industry level in columns (2) and (4). ***, **, * denote significance at the 1, 5 and 10 percent levels, respectively.

Table A.4: Impact of Reform on Firms' Cost of Debt (2)

Dependent Variable: *Interest Paid/Long Term Debt*

	(1)	(2)	(3)	(4)
Treatment	0.00912***	0.00873***		0.00873***
	(0.00125)	(0.00185)		(0.00190)
Treatment · Post	-0.00893***	-0.00786***	-0.00665***	-0.00786***
	(0.00145)	(0.00153)	(0.00166)	(0.00166)
2006	0.0113***	0.0119***	0.0143***	0.00650***
	(0.000720)	(0.000664)	(0.000665)	(0.000723)
2007	0.0191***	0.0199***	0.0239***	0.00889***
	(0.000830)	(0.000941)	(0.000837)	(0.000862)
2008	0.0180***	0.0184***	0.0211***	0.00177
	(0.000824)	(0.000848)	(0.000853)	(0.000904)
2009	0.0156***	0.0145***	0.0148***	-0.00762***
	(0.00143)	(0.00148)	(0.00171)	(0.00157)
2010	0.00257	0.00137	-0.000177	-0.0262***
	(0.00143)	(0.00147)	(0.00171)	(0.00164)
$\log(Revenues)$	0.00624***	0.00464***	0.00187***	0.00461***
	(0.000406)	(0.00120)	(0.000499)	(0.00123)
Leverage	-0.00108	-0.000808	0.000207	-0.000779
	(0.000884)	(0.000663)	(0.000233)	(0.000646)
$\log(Assets)$	-0.00561***	-0.00354**	0.00271*	-0.00356*
	(0.000387)	(0.00119)	(0.00111)	(0.00122)
Firm FE	No	No	Yes	No
Industry FE	No	Yes	No	Yes
N	17089	17089	17089	17089
R^2	0.092	0.088	0.195	0.116

The table reports OLS estimation of the impact of the Spanish bankruptcy reform on firms' cost of debt. Column 1 reports results from the naive OLS specification, whereas columns 2 and 3 include industry and firm fixed effects, respectively. Column 4 reports results from a specification where industry-wide linear trends are allowed. *Treatment* is a binary variable equal to 1 if the firm has an Altman score below the 25th percentile and equal to 0 if the score is above the 75th percentile (the remaining observations are dropped). *Post* is a binary variable that equals one in case the reform has taken place, i.e. when $t \geq 2009$. Reported are also time dummies *2006-2010*, as well as several control variables. The constant is included, but not reported. Standard errors are clustered on firm level in columns (1) and (3), and on industry level in columns (2) and (4). ***, **, * denote significance at the 1, 5 and 10 percent levels, respectively.

Table A.5: Impact of Reform on Firms' Cost of Debt (3)

Dependent Variable: *Interest Paid /Long Term Debt*

	(1)	(2)	(3)	(4)
Treatment	-0.00715***	-0.00633***		-0.00630***
	(0.000580)	(0.00121)		(0.00116)
Treatment · Post	0.00385***	0.00418***	0.00559***	0.00413***
	(0.000565)	(0.00108)	(0.000549)	(0.000988)
2006	0.00919***	0.00989***	0.0152***	0.00984***
	(0.000424)	(0.000451)	(0.000380)	(0.000573)
2007	0.0151***	0.0159***	0.0232***	0.0153***
	(0.000479)	(0.000606)	(0.000467)	(0.000864)
2008	0.0145***	0.0148***	0.0205***	0.0137***
	(0.000473)	(0.000527)	(0.000475)	(0.000865)
2009	0.00472***	0.00428***	0.00539***	0.00280***
	(0.000540)	(0.000869)	(0.000568)	(0.000654)
2010	-0.00928***	-0.0101***	-0.0129***	-0.0119***
	(0.000543)	(0.00114)	(0.000573)	(0.000575)
log(*Revenues*)	0.00907***	0.00683***	0.00244***	0.00688***
	(0.000185)	(0.000769)	(0.000271)	(0.000803)
Leverage	-0.0000300*	-0.0000277**	-0.00000435	-0.0000301**
	(0.0000119)	(0.00000921)	(0.00000419)	(0.00000910)
log(*Assets*)	-0.00723***	-0.00454***	0.00289***	-0.00458***
	(0.000196)	(0.000655)	(0.000583)	(0.000676)
Firm FE	No	No	Yes	No
Industry FE	No	Yes	No	Yes
N	64546	64546	64546	64546
R^2	0.128	0.093	0.227	0.104

The table reports OLS estimation of the impact of the Spanish bankruptcy reform on firms' cost of debt. Column 1 reports results from the naive OLS specification, whereas columns 2 and 3 include industry and firm fixed effects, respectively. Column 4 reports results from a specification where industry-wide linear trends are allowed. *Treatment* is a binary variable equal to 1 if the firm is Spanish and equal to 0 if the firm is Portuguese. *Post* is a binary variable that equals one in case the reform has taken place, i.e. when $t \geq 2009$. Reported are also time dummies *2006-2010*, as well as several control variables. The constant is included, but not reported. Standard errors are clustered on firm level in columns (1) and (3), and on industry level in columns (2) and (4). ***, **, * denote significance at the 1, 5 and 10 percent levels, respectively.