

Does Creditor Protection Matter for Corporate Borrowing?

Evidence from the European Insolvency Regulation*

KEVIN ARETZ[†]

University of Manchester

MARIA-TERESA MARCHICA[‡]

University of Manchester

May 2026

Abstract

We exploit a natural experiment created by the 2002 European Insolvency Regulation (EIR) to identify the causal effect of creditor rights on corporate borrowing. The EIR unexpectedly reassigned the insolvency jurisdiction of fully-owned foreign subsidiaries within the European Union from their host country to the country in which their parents are headquartered, exposing otherwise similar firms operating in the same environment to different creditor rights regimes. Using double and triple-difference designs, we show that shifts toward stronger (weaker) creditor-rights regimes raise (lower) debt financing. Also, several dimensions of creditor protection matter for borrowing but often asymmetrically, and enforcement conditions the reform effects. Linking UK subsidiaries to their house-banks, we reveal that especially weaker banks cut their lending in response to weaker creditor rights. We find little evidence that broader cultural or political factors explain our results.

KEYWORDS: Creditor rights, corporate borrowing, causal inference, EU regulations.

JEL CLASSIFICATION: G32, G33, D22

*We are grateful to seminar participants at Deakin University, Neoma Business School, University of Durham, University of Exeter, University of Manchester, University of Melbourne, University of New South Wales (UNSW), University of Padua, University of Sydney, and Queen's University Belfast. All errors remain our own.

[†]Corresponding author, Alliance Manchester Business School, The University of Manchester, Booth Street West, Manchester M15 6PB, UK. kevin.aretz@manchester.ac.uk.

[‡]Alliance Manchester Business School, The University of Manchester, Booth Street West, Manchester M15 6PB, UK. maria.marchica@manchester.ac.uk.

1 Introduction

Corporate debt is a central source of external financing for firms, with, even today, a large share of corporate investment still being financed through debt, particularly, bank lending.¹ The law and finance literature argues that the legal protection granted to creditors plausibly influences the availability of credit and firms' willingness to borrow.² Yet, whether and how creditor rights affect corporate debt-taking decisions in the real-world remains an open question. Cross-country studies typically fail to find a robust relation between creditor rights and corporate borrowing (see, e.g., La Porta et al. (1997) and Djankov et al. (2007)) or show no systematic real or financial improvements following the strengthening of creditor rights (Oto-Peralías and Romero-Ávila (2017)). Intra-country studies that exploit plausibly exogenous shocks to specific creditor rights coming from legal reforms provide only partial and often conflicting evidence (e.g., Scott and Smith (1986); Rodano et al. (2016); Closset and Urban (2019); Schoenherr and Starmans (2022); and many others).

Legal scholars have raised serious concerns about the empirical strategies used to identify the effects of creditor rights in law and finance studies, possibly explaining the weak and inconsistent evidence. Their central critique is that identifying causal effects in this literature is inherently difficult because creditor rights are deeply intertwined with legal, political, and cultural institutions, making it challenging to disentangle the independent effect of creditor protection on corporate outcomes (see, e.g., Klick (2013)). As they stress, identifying the causal effect of creditor rights would ideally require randomly reallocating a subset of firms from one creditor rights regime to another ("treated firms") and comparing their changes in debt-taking with those of other firms which have remained in the same regime ("control firms") — while keeping all other institutional features constant.

In this paper, we exploit a regulatory change closely approximating this ideal experiment, the 2002 European Insolvency Regulation (EIR). The main purpose of the EIR was to harmonize jurisdictional rules governing insolvency proceedings across European Union (EU) member states. In particular, the regulation established that the courts with jurisdiction to open a firm's main insolvency proceedings are those located in the country where the firm has its "centre of main interests" (COMI; Wessels (2006) and Wood (2007)). Traditionally, legal scholars interpreted the COMI as coinciding with the country where a firm is legally registered and would normally file for insolvency. However,

¹Source: <https://www.enterprisesurveys.org/en/data/exploretopics/finance?subGroup=-1&subtopic=38>.

²The supply-side view argues that stronger creditor protection increases lenders' ability to recover funds in distress and thus encourages the extension of credit, boosting credit supply (e.g., Townsend (1979); Aghion and Bolton (1992); Hart and Moore (1994, 1998); and Levine (1997)). In contrast, the demand-side view argues that stronger creditor protection in default may increase the risk of inefficient liquidation, potentially discouraging managers from relying on debt financing. The upshot is that credit demand dampens (e.g., Acharya and Subramanian (2009) and Vig (2013)).

shortly after the introduction of the EIR, European courts clarified that the COMI should instead be located in the country where the firm's head-office functions are carried out (Wessels (2006)).³

This interpretation had important implications for multinational firms. Because the head-office functions of fully-owned foreign subsidiaries are typically performed by their parent companies, the EIR effectively shifted the COMI of many subsidiaries from their host countries to the countries in which their parents are headquartered. As a result, these subsidiaries became subject to the insolvency jurisdiction of courts operating under a (likely) different creditor-rights regime.

We exploit the above wrinkles of the EIR to identify the causal effect of creditor rights on corporate debt-taking. Our empirical strategy focuses on fully-owned foreign subsidiaries operating within the EU. We measure the strength of creditor protection using the creditor rights (CR) index of La Porta et al. (1998) and Djankov et al. (2007). Our baseline design compares the debt-taking behavior of foreign subsidiaries with that of otherwise similar domestic subsidiaries before and after the introduction of the EIR. Intuitively, while domestic subsidiaries remain subject to the creditor rights regime of their host country, foreign subsidiaries experience a shift toward the regime of their parent country. This setting allows us to exploit cross-country variation in creditor-rights regimes while holding constant the economic environment in which firms operate.

We begin by examining fully-owned subsidiaries operating in either France or the U.K., the countries with the weakest and strongest creditor-rights regimes in our sample, respectively (La Porta et al. (1998)). Using a double-difference design comparing foreign with domestic subsidiaries, we find that the reassignment of insolvency jurisdiction induced by the EIR significantly affected corporate borrowing. In particular, foreign subsidiaries in France increased their total debt relative to domestic subsidiaries by 1.6 percent points (t -statistic: 2.58) after the reform. Conversely, foreign subsidiaries in the U.K. exhibit the opposite pattern, with their debt decreasing by 3.7 percent points (t -statistic: -3.65). These effects are economically meaningful, representing 18% and 10% of pre-EIR average total debt, respectively. Delving into the French (U.K.) effects, results also provide initial evidence that shifts toward stronger creditor rights regimes increase firms' reliance on debt financing, whereas shifts toward weaker regimes reduce that same financing.⁴

We next extend the analysis to broader samples of European subsidiaries. Using a triple-difference design (DIDID), we compare (i) foreign subsidiaries with parents in countries with stronger or weaker creditor-rights regimes relative to the host country to (ii) foreign subsidiaries whose parent and host countries have similar creditor-rights regimes before and after the EIR. This approach allows us to isolate the effect of changes in creditor protection from other consequences of being reassigned to a

³The first ruling along these lines was handed down as early as July 2002, in an English administration order for Enron Director SA, a Spanish subsidiary of a U.K. parent. Table 1 provides additional examples.

⁴We also find that our results come mainly from short-term debt, with only minor effects on long-term debt in France but not the U.K. The reason is likely that the EIR explicitly excludes *rights-in-rem* typically used to secure long-term lending in the U.K., such as, for example, floating charges.

foreign insolvency jurisdiction (*creditor-rights-unrelated effect*). Results based on the comprehensive EU-12 sample show that subsidiaries whose insolvency jurisdiction shifts toward stronger creditor-rights regimes increase their debt-taking by 1.9 percent points (t -statistic: 3.11) after the EIR, while those shifting toward weaker regimes reduce theirs by 2.9 percent points (t -statistic: -3.27). The estimates net of the creditor-rights-unrelated effect are also economically important, representing 12% and 18% of pre-EIR average total debt, respectively. Overall, our initial evidence strongly supports the supply-side view of creditor rights over the demand-side view.

One advantage of our setting is that it allows us to examine how specific dimensions of creditor protection affect firms' debt-taking decisions. While most existing studies concentrate on reforms affecting only one or a few creditor rights, our setting enables us to evaluate the impact of the individual components of the CR index.⁵ Our DIDID results indicate that several dimensions of creditor protection influence corporate borrowing, with the priority of secured creditors playing a particularly important role. We also document substantial asymmetries in these effects: Granting certain creditor rights leads to sizable increases in debt-taking, whereas removing the same rights generates much weaker responses, and vice versa. For example, granting secured creditors the right to be paid first raises total debt by 3.6 percentage points (t -statistic: 4.78), whereas withdrawing that right leads to only a small and statistically insignificant decline in debt.

We further assess additional dimensions of insolvency law not explicitly captured by the CR index. Our results indicate that several institutional features of insolvency procedures do matter for corporate borrowing decisions beyond the components of the index. This evidence highlights the broader role of insolvency-law design in shaping firms' financing choices.

We also examine how the effects of creditor rights (often dubbed the "letter of the law") compare with the role of legal enforcement (the "execution of the law"). Our evidence demonstrates that changes in creditor protection significantly affect firms' borrowing decisions across a wide range of enforcement regimes. By contrast, enforcement alone plays an only limited role in explaining debt-taking behavior. However, when we consider the interaction between creditor rights and enforcement, the effects of stronger creditor protection become more pronounced in countries with more efficient enforcement systems. For instance, we find that the effect of stronger creditor rights is 3.8 percentage points (t -statistics: 2.85) in systems in which courts take less time to settle a business dispute, while the corresponding effect is 2.8 percentage points (t -statistics: 2.42) in systems in which courts take more time. This finding highlights the importance of both the design of insolvency laws and their effective implementation in practice.

As an innovative addition to the literature, we also link the UK subsidiaries to their house-banks, allowing us to evaluate how bank characteristics condition the reform's debt-taking effects. Strikingly,

⁵The subindexes capture whether: (i) secured creditors are paid first; (ii) there are creditor restrictions to reorganize; (iii) managers do not stay in place; and (iv) there is no automatic stay on assets.

our evidence suggests that only banks with low equity-to-assets ratios or ROAs (i.e., weak banks) and those whose business models depend less on commercial credit cut their lending in response to the decline in creditor rights in the UK. These results further corroborate the notion that our main findings are driven by supply (and not demand) side adjustments triggered by the EIR.

Finally, we examine whether broader religious, cultural, or political factors often reasoned to be related to legal institutions explain our findings. Some critiques of the law and finance literature argue that the apparent effects of creditor protection reflect deeper cultural or political characteristics rather than the legal rules themselves (e.g., La Porta et al. (2008)). Exploiting our empirical setting, which holds constant the broader institutional environment in which firms operate — except for that of the courts, we find little evidence that such factors drive firms' debt-taking decisions across a wide range of proxies for religion, culture, and politics. Instead, the results consistently point to the dominating role of creditor protection in shaping corporate borrowing.

Related Literature. Our paper contributes to the law and finance literature. Early contributions in this literature rely primarily on cross-country variations in creditor rights indexes to investigate their effects on firm behavior and real outcomes (e.g., La Porta et al. (1997); Giannetti (2003); Djankov et al. (2007); Öztekin (2015)). A key challenge for this approach is that differences in legal rules are often intertwined with other institutional, political, and cultural factors, making it difficult to isolate the causal impact of creditor protection, as pointed out by several prominent legal scholars.⁶

More recent studies attempt to address these concerns by exploiting within-country legal reforms affecting specific creditor rights. The evidence from these studies, however, is mixed, with some not finding a relation between creditor rights and corporate borrowing (e.g., Haselmann et al. (2010) who study reforms in Eastern European countries); others showing either a positive (e.g., Rodano et al. (2016) and De Vito and Jacob (2023) who study reforms in Italy) or negative (e.g., Closset and Urban (2019) who study a German reform) relation, and yet again others finding an ambiguous relation depending on managerial bankruptcy costs (e.g., Schoenherr and Starmans (2022) who study a reform in South Korea). One possible explanation is that such empirical settings face several important limitations. First, most reforms affect only one or a few different dimensions of creditor protection, making it difficult to capture the overall effect of creditor rights regimes and compare across studies. Second, legal reforms are often bundled with other institutional changes, complicating the identification of the role played by creditor protection. Third, these studies do not use a clean control group since all firms within one country are subject to the same insolvency framework.

⁶Djankov et al.'s (2007) findings of a causal positive link between creditor rights and corporate borrowing are mainly driven by creditor rights shocks in ex-communist countries that experienced multiple simultaneous reforms in their legal and institutional frameworks. The other reforms make it difficult to disentangle the effects of creditor rights from other changes. Moreover, when clustering standard errors at the country level, their evidence disappears.

Our paper contributes to this literature by exploiting a regulatory change providing a cleaner empirical setting. The EIR unexpectedly reassigned the insolvency jurisdiction of many foreign subsidiaries across creditor rights regimes while leaving the broader economic and institutional environment unchanged. This setting allows us to compare firms that become subject to different creditor rights regimes while continuing to operate within the same (host country) environment. As a result, our empirical design overcomes many limitations of previous studies. It also allows us to examine changes across entire creditor rights regimes (e.g., switching from the French to the U.K. regime) rather than isolated legal provisions, avoids confounding reforms that occur simultaneously within a country, and provides a natural control group of otherwise comparable subsidiaries operating under the same host country conditions. Beyond identifying the causal effect of creditor protection on corporate borrowing, our setting also allows us to provide new evidence on which specific dimensions of creditor rights matter most for firms' financing decisions and how the effects of creditor protection interact with legal enforcement and other institutional features of insolvency law.

Our results also speak to the literature on the capital structure determinants of multinational business groups. While some studies focus on the role of national tax rates as a main determinant of foreign subsidiaries' capital structures (e.g., Huizinga et al. (2008) and Faulkender and Smith (2016)), only a few show, both theoretically and empirically, that stronger host country creditor rights increase subsidiaries' external debt-taking (e.g., Desai et al. (2004); Lehmann et al. (2004); and Akbel and Schnitzer (2011)). Crucially, these studies rely on simple cross-country comparisons and assume that foreign subsidiaries are subject to local insolvency laws. We add to this literature by exploiting a regulatory setting relaxing this assumption and allowing us to provide causal evidence on the relation between creditor rights and the debt-taking of foreign subsidiaries in the EU.

Finally, our findings also have implications for ongoing policy debates on the design of insolvency frameworks and creditor protection, as many jurisdictions have recently undertaken reforms of their restructuring and bankruptcy regimes (e.g., Gurrea-Martínez (2022); van Zwieten (2023)). By showing that changes in creditor rights *causally* affect firms' borrowing decisions, our results highlight the role of insolvency law institutions in shaping firms' access to external finance.

We proceed as follow. Section 2 describes the EIR's institutional setting. Section 3 outlines data sources, sample construction, and methodology. Section 4 presents the baseline results of EIR effects on debt-taking. Section 5 contrasts the effects of creditor rights and enforcement. Section 6 assesses how bank characteristics condition the reform's debt-taking effects. Section 7 evaluates popular critiques of the law and finance literature. Section 8 reports standard placebo, parallel-trend, and propensity-score matching tests. Section 9 sums up and concludes.

2 The 2002 European Insolvency Regulation

In this section, we offer an overview of the European Insolvency Regulation (EIR). We first outline the history of the EIR and detail its implications. We next review several important court rulings concerning the regulation as they are crucial to set up our identification strategy. See Chapter 30 in Wood (2007) for more details about the history as well as repercussions of the regulation.

2.1 History and Summary of the Regulation

In 1963, the European Economic Community (EEC), the forerunner of the EU, aimed to develop a harmonized bankruptcy code for its member states, producing a first draft in 1970. Yet, national parliaments ultimately rejected that draft due to widely differing views on insolvency matters. As a result, the EEC developed a second less ambitious draft in 1980 proposing a set of principles to avoid conflicts of bankruptcy laws across member states. After several revisions, that draft was signed into law as Council Regulation No 1346/2000 on 29 May 2000 (the “EIR”) and came into force on 31 May 2002 in all EU member states (except Denmark) without requiring further national legislation. The regulation applies to all reorganization and liquidation procedures (as long as the debtor disinvests some real assets) and debtors except for financial institutions.

The main objective of the EIR is to identify the courts of the EU states which have jurisdiction to open main bankruptcy proceedings over debtors. In essence, the EIR mandates that these are the courts in the country in which the debtor has its “centre of main interests” (COMI).⁷ Although the EIR does not define the term COMI, it states that “in the case of a company or legal person, the place of the registered office shall be presumed to be the centre of its main interests in the absence of proof to the contrary” (art 3(1)), seemingly in line with the pre-reform convention that companies file for bankruptcy in their country of registration. However, one of the recitals providing an interpretative guide of the EIR stresses that: “the COMI should correspond to the place where the debtor conducts *the administration of his interests on a regular basis* and is therefore ascertainable by third parties” (Recital 13). That recital offered ample room for different interpretations. Determining the COMI is crucial for our setting because the bankruptcy code of the country where the main bankruptcy proceeding is opened determines the conditions for filing as well as its effects, conduct, and closure. To be more specific, it identifies the assets of the bankrupt estate, defines the rights of the company, creditors, and the bankruptcy administrator, ascertains how claims are lodged and treated, sets stays on assets as well as priorities, and governs the effects on contracts and creditor actions.

⁷To be clear, the EIR mandates that EU courts would also have jurisdiction over companies registered in (say) Japan, Singapore, and the US if their COMI were within one of the EU member states other than Denmark. Unfortunately, we are unable to test the effects on companies registered in these countries since our data identify only the subsidiaries registered in one of the European countries of parent companies registered in those same countries.

Despite its broad scope, there are several exceptions to the EIR's tenet to universally apply the bankruptcy code of the country in which proceedings are opened. Critically, the opening of procedures does not affect the *rights-in-rem* of creditors with respect to specific or indefinite collections of tangible or intangible, movable or immovable assets located in another EU state, as, for example, security interests held by them. The implication is that the bankruptcy administrator can neither stop the enforcement of security interests nor use, dispose, or substitute the pledged assets. Other important exceptions are immovable property (governed by the state in which the property is located) and employment (governed by the state of the contract) contracts.

2.2 The Interpretation of the COMI: The Head Office Functions View

Shortly after the enactment of the EIR in 2002, EU courts surprised the legal and business communities with landmark rulings on the COMIs of multinational business groups. As the EIR does not contain provisions dealing with such groups, it is necessary to determine the COMI of each group member separately. Both before and after the enactment of the EIR, the legal consensus was that the COMI of group members should be in their country of registration (see, e.g., Wessels (2006, 2009), among others).⁸ However, rather surprisingly, EU courts distanced themselves from the pre-EIR legal consensus. Relying on the novel arguments made in Moss and Smith (2002) that it is more appropriate to consider the country in which a *company's head office functions are carried out* rather than its registration country, they almost always ruled that the COMI of a subsidiary registered in one EU country but fully-controlled by a parent in another is in its parent's country.

Table 1 provides details of “noteworthy” EU court rulings concerning the COMIs of multinational business groups over the 2002-2006 period (Wood, 2007, pp. 904-909). Strikingly, *in each single case*, EU courts ruled that the COMI should be shifted from the subsidiaries' registration countries to that of its parent, with the earliest decision handed down in July 2002 — merely two months after the enactment of the EIR.⁹ As Table 1 shows in the last column, the reasons for such court rulings always relate to the subsidiaries' head-office functions (e.g., its management, selling, recruitment, IT, and controlling functions) being conducted by its parent. While it is ultimately unknown why judges ruled in that way, it has been speculated that they, quite reasonably, deemed it to be more probable to successfully reorganize a multinational group under one (main) administrator operating under one unified bankruptcy code in one country — rather than under multiple administrators operating under different codes in different countries (e.g., Eurotunnel ruling in August 2006).

⁸Based on Recital 13's passage that the COMI must be “ascertainable by third parties,” legal scholars argued that creditors can hardly be expected to investigate the ownership ties between group members.

⁹We note that EU judges do not always decide to shift the COMIs of EU companies. For example, they often do not do so when a (stand-alone) company with all its operations within its registration country tries to fraudulently argue that its COMI is in another country, to file under an insolvency regime more beneficial to it (see Wood (2007)).

TABLE 1 ABOUT HERE.

Let us be explicit about what the EIR and its court interpretation imply for subsidiaries hosted in one EU country but fully controlled by a parent in another. As an example, let us consider the French subsidiary of Adidas, a multinational German sportswear company. While the subsidiary is registered in Strasbourg, its parent is registered in Herzogenaurach in Germany. In case the subsidiary wanted to file for reorganization, it would have had to do so under France’s principle procedure (“redressment judiciaire”) before the EIR but under Germany’s (“Insolvenzordnung”) after it. As a result, the protection awarded to creditors by insolvency law would significantly change from before to after the EIR. First, control rights over the subsidiary in bankruptcy would shift from a court-appointed administrator to creditors under the court’s supervision. Second, secured creditors would move from an intermediate rung to the top of the bankruptcy ladder of priorities. Third, creditors would become able to veto reorganizations. Finally, secured creditors would no longer suffer from an unlimited but only a limited automatic stay on assets. The upshot would be that the La Porta et al. (1998)-Djankov et al. (2007) CR index value would rise from zero to three from pre to post-EIR, a much more pronounced change in creditor protection than those examined in prior studies exploiting intra-country reforms.

In our empirical work, we exploit the above wrinkles of the EIR by investigating the effects on debt-taking decisions of fully-owned foreign subsidiaries that after the EIR become exposed to either stronger or weaker creditor rights regimes than that of their host country, depending on where their parent companies are located. The control group is always fully-owned domestic subsidiaries that remain exposed to the same creditor rights regime before and after EIR. In the following section we give more details on how we empirically implement our identification strategy.

3 Data and Methodology

3.1 Data Sources

We retrieve data on fully-owned subsidiaries registered in all 15 EU member states at the start of our sample period from Bureau van Dyck’s AMADEUS. Since each release of AMADEUS features only the most recent eight years of firm-specific financial data and excludes inactive firms after four years, we pool the data from each April release between 1999 and 2010 and delete duplicates. We exclude the following observations: (a) subsidiaries not owned by firms registered in our sample EU countries and those operating in the financial and utilities sectors; (b) subsidiary-month observations associated with fiscal years whose length differs from twelve months; and (c) subsidiaries

hosted in or owned by firms in Denmark, Greece, and Luxembourg.¹⁰ Overall, our sample features fully-owned subsidiaries from Austria, Belgium, Finland, France, Germany, Ireland, Italy, the Netherlands, Portugal, Spain, Sweden, and the U.K. (“EU-12”) over the 1997-2006 sample period for a total number of 95,935 firm-year observations and 14,436 unique firms. In falsification tests, we also study partially owned subsidiaries as well as subsidiarity registered in Denmark (the EU country which opted out of the EIR) and Norway (a non-EU country located within Europe).

Next, we use data from Andrei Shleifer’s website to proxy for the strength of creditor rights.¹¹ To be specific, we rely on the La Porta et al. (1998)-Djankov et al. (2007) CR index over the 1997-2004 period for most of our analysis, while we borrow La Porta et al.’s (1998) subindexes measured around 1994-1995 when we analyse the effects of each single CR index component.¹² As Italy and Spain reformed their bankruptcy codes in 2005/2006 and 2003, respectively, their index values change (Spain) or would have changed (Italy) in those years. To avoid bias, we thus include data on subsidiaries either registered in those countries or with parents in those countries only until the reform years. As a result, the CR index value is time-invariant within country over our sample years.

Moreover, we collect data on other creditor rights differing across our sample EU member states from Marshall’s (2002) comparative work; on enforcement variables including the efficiency of a country’s judicial system, the number of days required by its legal system to resolve a business dispute, and the formalism of its legal system from Andrei Shleifer’s website; on cultural variables from Hofstede (2001); and religious and political variables from Andrei Shleifer’s website.

Finally, we extract data on the names of UK subsidiary’s house-banks from the April 2002 release of Bureau van Dyck’s FAME. We manually merge the bank names with 1997-2002 data from Bankscope, enabling us to calculate banks’ equity-to-assets ratios, ROAs, and shares of interest obtained from commercial lending activity over the pre-reform period.

3.2 Variable Definitions

Our main outcome variable is *TotalDebt*, computed as the sum of Short (AMADEUS mnemonic: loan) and Long-term (ltdeb) Debt scaled by Assets (at). We also look into *Short-(Long)-term Debt*, defined analogously as short-(long)-term debt scaled by assets. Our first treatment variable (*Treat*) is a dummy variable equal to one for subsidiaries consistently fully-owned by a parent registered

¹⁰Denmark opted out of the EIR in accordance with its rights under the Treaty of Amsterdam. Conversely, Greece and Luxembourg host too few foreign subsidiaries to be meaningfully included in our empirical analysis.

¹¹The URL of Andrei Shleifer’s website is: <https://shleifer.scholars.harvard.edu/>.

¹²We follow the corrections in Djankov et al. (2007) to align the subindexes with the overall index. In particular, we change the value of the management-does-not-stay-in-place subindex to one for the Netherlands and the value of the restrictions-for-going-into-reorganization subindex to zero for Sweden. Since the countries in our sample do not observe changes in their overall creditor rights index values over the 1993-2006 period (with the only exceptions being Italy and Spain), it is straightforward to relate the overall index values to their underlying subindex values.

in a country different from the subsidiary's registration country ("foreign") over the pre-EIR (1997-2001) period and equal to zero for those subsidiaries consistently fully-owned by a parent registered in their same country ("host") over that same period.¹³ The shock variable (*Shock*) is a dummy variable equal to one over the post-EIR (2002-2006) period and else zero.

In our initial tests on French and U.K. subsidiaries, we also condition on the strength of treatment through defining dummy variables capturing how similar (different) the creditor rights regime of the parent country is relative to that of a subsidiary's host country, France or the U.K. Under the assumption that France has the weakest creditor protection in the EU, as reflected by a CR index value of zero, *similar* regimes to it are those with a pre-EIR CR index value of one or two ($Similar^{CRI} = 1$, else zero), while *different* regimes are those with a value of three or four ($Different^{CRI} = 1$, else zero). Conversely, under the assumption that the U.K. has the strongest creditor protection in the EU, as reflected by a CR index value of four, *similar* regimes to it are those with a pre-EIR CR index value of two or three, while *different* regimes are those with a value of zero or one.

In our main analysis, our second treatment variables are Up^{CRI} and $Down^{CRI}$, where Up^{CRI} ($Down^{CRI}$) is a dummy variable equal to one if the pre-EIR CR index of the country of a subsidiary's parent is at least two units higher (lower) than the pre-EIR CR index of the host country and else zero. In further analysis, we also include dummy variables based on the subvalues of the CR index, with On^x (Off^x) equal to one if the pre-EIR country of a subsidiary's parent grants (does not grant) right x to creditors but its pre-EIR host country does not (does), and else zero. In the last part, we also augment our main specification with additional country-specific variables, with Up^x ($Down^x$) equal to one if the pre-EIR country of a subsidiary's parent has an at least one EU-12-country standard deviation higher (lower) x -value than its pre-EIR registration country and else zero.

Our controls are *Size*, *Firm Age*, *Employees*, *Profitability*, *Fixed Assets*, and *Cash Holdings*. *Size* is the natural log of assets. *Firm Age* is the difference between the account date and the date of incorporation, in years. *Employees* is the natural log of the number of employees (empl). *Profitability* is earnings before interest and taxes (ebta), fixed assets tangible fixed assets (tfas), and cash holdings the cash reserve (cash), all scaled by assets. To avoid "bad-control problems" (see, e.g., Angrist and Pischke (2009)), we form the firm-specific average of each control taken over the pre-EIR period and then interact that with country and year dummies (see next subsection).

To mitigate the noise often inherent in small private-firm data, we winsorize our firm level data at the 5th and 95th percentiles by subsidiary registration country.

¹³To be conservative, we exclude fully owned subsidiaries with parents in different foreign countries over the pre-EIR period. Yet, as their number is tiny, this decision does not materially affect our empirical results.

3.3 Methodology

We use both double (DID) and triple (DIDID) difference estimators to find the causal effects of creditor rights on debt financing, other country-specific variables, and (some of) their interactions. We start with the following DID specification for French and U.K. subsidiaries only:

$$Debt_{i,s,t} = \beta(Treat_{i,s} \times Shock_t) + \sum_{y \in Y} \sum_{c \in C} \gamma_{y,c} (DYear_t^y \times MCon_i^c) + \alpha_i + \alpha_t + \epsilon_{i,s,t}, \quad (1)$$

where $Debt$ is the total, short-term, or long-term debt of subsidiary i in member state s (either France or the U.K.) at the end of fiscal year t , $Treat$ and $Shock$ are defined in the prior subsection, Y and C are the sets of all years in our sample period and all our control variables, $DYear^y$ is a dummy variable equal to one if the current year is equal to y and else zero, and $MCon^c$ is the pre-EIR average of control variable c for subsidiary i . α_i and α_t are static subsidiary and year fixed effects, respectively. We cluster standard errors by subsidiary. The main advantage of running regression (1) on either French or U.K. subsidiaries is that under the assumption that France (the U.K.) grants the weakest (strongest) legal protection to creditors in the EU, the creditors of all foreign subsidiaries — independent of their parent country — should benefit (suffer) from the EIR. As such, we expect β , our parameter of interest, to be positive for French and negative for U.K. subsidiaries.

Notwithstanding, even if our assumption above is correct, foreign French (U.K.) subsidiaries with parents in countries with much stronger (weaker) creditor rights than France (the U.K.) should benefit (suffer) more from the EIR than those with parents in countries with more similar rights. To test that hypothesis, we augment the model in equation (1) as follows:

$$\begin{aligned} Debt_{i,s,t} = & \beta_1(Treat_{i,s} \times Shock_t \times Similar_{i,s}^{CRI}) + \beta_2(Treat_{i,s} \times Shock_t \times Different_{i,s}^{CRI}) \\ & + \sum_{y \in Y} \sum_{c \in C} \gamma_{y,c} (DYear_t^y \times MCon_i^c) + \alpha_i + \alpha_t + \epsilon_{i,s,t}, \end{aligned} \quad (2)$$

where we expect β_1 to be smaller in magnitude than β_2 . In essence, equation (2) decomposes the DID effect from equation (1) into its “subeffects” coming from creditor rights regimes similar (different) from the one of the subsidiary host country, $Similar^{CRI}$ ($Different^{CRI}$).

We next pool subsidiaries from various sets of EU-12 countries and estimate the model:

$$\begin{aligned} Debt_{i,s,t} = & \beta_1(Treat_{i,s} \times Shock_t \times Up_{i,s}^{CRI}) + \beta_2(Treat_{i,s} \times Shock_t \times Down_{i,s}^{CRI}) + \beta_3(Treat_{i,s} \times Shock_t) \\ & + \sum_{s \in S} \sum_{y \in Y} \sum_{c \in C} \gamma_{s,y,c} (DState_i^s \times DYear_t^y \times MCon_i^c) + \alpha_i + \alpha_t + \epsilon_{i,s,t}, \end{aligned} \quad (3)$$

where S is the set of EU member states included in the regression, and $DState^s$ is a dummy equal to one if a subsidiary is registered in state s and else zero. We run regression (3) on (i) France and the

U.K.; (ii) France, Italy, Germany, Spain, and the U.K.; (iii) the EU-12 countries excluding France and the U.K.; and (iv) the EU-12 countries. β_1 (β_2) captures the *incremental* effect from being exposed to a foreign regime with stronger (weaker) creditor rights than the host country. Conversely, β_3 captures the effect from a change to a regime similar to the one of the subsidiary's host country. We call this the *creditor-rights-unrelated effect* possibly stemming from unfamiliarity with the foreign country's norms, culture, and/or legal system. Alternatively, a creditor-rights-unrelated effect may also arise through legal uncertainty about whether EU courts will continue to use the heads-office function interpretation of the COMI in their future rulings (see Sussman (2008)).

In further tests, we delve into the various components of the CR index to investigate how each of them influences corporate borrowing and also to find out whether they have symmetric debt-taking effects (i.e., whether the magnitudes of the effects from granting and withdrawing the same right are similar to one another). To achieve that goal, we run regression (3):

$$\begin{aligned} Debt_{i,s,t} = & \sum_{x \in R} \beta_1^x (Treat_{i,s} \times Shock_t \times On_{i,s}^x) + \sum_{x \in R} \beta_2^x (Treat_{i,s} \times Shock_t \times Off_{i,s}^x) + \beta_3 (Treat_{i,s} \times Shock_t) \\ & + \sum_{s \in S} \sum_{y \in Y} \sum_{c \in C} \gamma_{s,y,c} (DState_i^s \times DYear_t^y \times MCon_i^c) + \alpha_i + \alpha_t + \epsilon_{i,s,t}, \end{aligned} \quad (4)$$

where R is a set including the rights underlying the CR index, namely: (i) secured creditors are paid first; (ii) creditors need to give their consent to reorganize; (iii) managers do not stay in place; and (iv) there is no automatic stay on assets. β_1^x (β_2^x) now captures the effect from granting (withdrawing) right x . We run regression (4) only on the EU-12 sample since we require sufficient variations across countries to accurately estimate parameters, especially since the rights are highly correlated.

We finally run horse races to (i) compare the relevance of the CR index relative to creditor rights not included in it and to (ii) control for other factors potentially explaining our evidence, such as enforcement as well as cultural, religious, and politics factors. To facilitate that analysis, we estimate the following augmented version of equation (3):

$$\begin{aligned} Debt_{i,s,t} = & \beta_1 (Treat_{i,s} \times Shock_t \times Up_{i,s}^{CRI}) + \beta_2 (Treat_{i,s} \times Shock_t \times Down_{i,s}^{CRI}) \\ & + \beta_3 (Treat_{i,s} \times Shock_t \times Up_{i,s}^x) + \beta_4 (Treat_{i,s} \times Shock_t \times Down_{i,s}^x) + \beta_5 (Treat_{i,s} \times Shock_t) \\ & + \sum_{s \in S} \sum_{y \in Y} \sum_{c \in C} \gamma_{s,y,c} (DState_i^s \times DYear_t^y \times MCon_i^c) + \alpha_i + \alpha_t + \epsilon_{i,s,t}, \end{aligned} \quad (5)$$

where x is one of the competing variables. In regression (5), we can interpret β_1 (β_2) as the effect from being shifted to a country with stronger (weaker) creditor rights *controlling for changes in variable x* . As before, we run the regression only on the EU-12 sample since we require sufficient variations in both the CR index and the other variable to accurately estimate parameters.

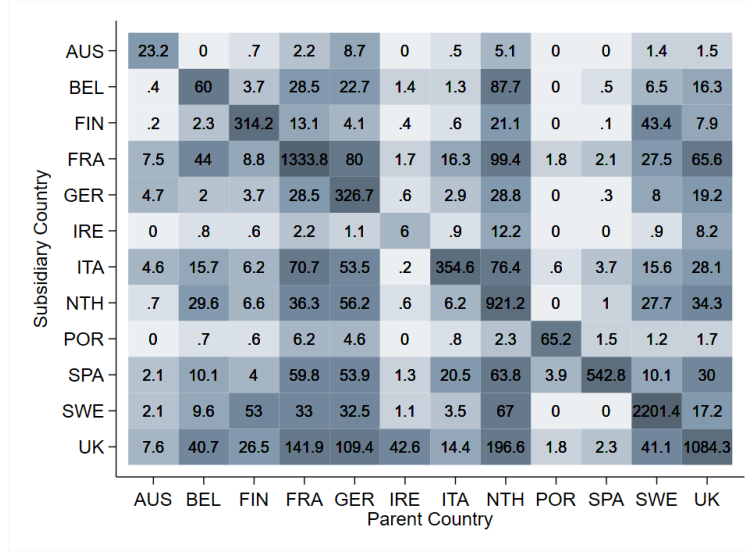


Figure 1. The figure offers a heatmap of the annual number of subsidiaries hosted in one of the EU-12 sample countries (rows) and fully owned by a parent in the same or another such country (columns). The darker cells in the heatmap indicate a greater number of observations.

4 The Debt-Financing Effects of the EIR

In this section, we establish the causal effects of creditors rights on firm's debt-taking. To do so, we first offer descriptive statistics for our subsidiary data. We next examine the overall and the CR-index-conditional debt-financing effects of the EIR first in either France or the U.K. and then in pooled-EU-country samples. We then decompose the CR index into its subindexes and verify the effects of the regulation conditional on changes in those. We finally also compare the CR index with legal creditor protections other than those included in the index.

4.1 Descriptive Statistics

Table 2 shows the average annual number (MeanObs.) and percent (%RowSum) of fully-owned subsidiaries in our EU-12 country sample by both their host (rows) and their parents' (columns) registration countries, where each percent is relative to the total number of subsidiaries in the host country. In the bottom row (final column), we report the total number of subsidiaries for each parent (host) country. In the column headers, we report the CR-index value for each EU-12 country. While all EU-12 countries are developed economies, they still differ significantly in terms of their CR index values. The majority of countries feature more than 100 subsidiaries per year on average, with France, the Netherlands, Sweden, and the U.K. featuring the largest numbers. For most countries, the vast majority of these subsidiaries are domestic (i.e., have a parent registered in their host country) except for Belgium (26.2%) and Ireland (18.2%). In line with expectations, many foreign subsidiaries have

their parents in both geographically and culturally close countries. For instance, almost 83% of foreign subsidiaries in Belgium have their parents located in its neighboring countries and countries sharing similar CR Index values (i.e., France, Germany, Italy, Netherlands, and Spain). Notwithstanding, especially the larger countries feature subsidiaries with parents from the vast majority of other EU-12 countries, as can perhaps more easily be seen from the heatmap of the number of observations per pair offered in Figure 1. This is important since the statistical power of our identification strategy relies crucially on the number of host-parent registration-country pairs in our data.

TABLE 2 ABOUT HERE.

Table 3 reports country-specific and overall descriptive statistics, namely, average (Mean), median (Med.), and standard deviation (St.D.), of the outcome and control variables, where the control variables are subsidiary-specific pre-EIR averages repeated for each fiscal year of a subsidiary. The table suggests that there are strong differences in corporate debt-taking across the EU-12 countries. While the average Finnish subsidiary has a total debt of only 8.0 percent as fraction of its assets, the corresponding figure for the average U.K. subsidiary is a markedly larger 39.7 percent. Interestingly, the overall average total debt has a highly positive correlation of 0.61 with the CR index (not shown in table), in line with the results of previous studies (as, e.g., Giannetti (2003), Djankov et al. (2007), and Öztekin (2015)). Also, median total debt is much smaller than mean total debt in all countries (except perhaps the U.K.), suggesting that debt-taking is highly positively skewed and thus unevenly distributed across firms. Finally, there are also strong differences in debt maturity composition across countries. While both mean and median short-term debt, for example, significantly exceed their long-term debt counterparts in Italy, France, and the U.K., the opposite happens in Germany, Finland, and Sweden.

TABLE 3 ABOUT HERE.

We observe also strong differences in the controls across countries. While the median Austrian subsidiary is, for example, much younger than the median Swedish one (8.3 *vs.* 24.1 years), it is still larger in terms of its assets (€40.9k *vs.* €0.8k) and employees (233.9 *vs.* 69.7). Indeed, our main tests include the control-variable interactions to account for such differences. To further mitigate concern that these differences could influence our empirical conclusions, we also propensity-score (PS) match foreign with domestic subsidiaries along these dimensions (see Section 8).

4.2 The Debt Effects of the EIR in France and the U.K.

Table 4 reports the estimates from equations (1) and (2) (Panels A and B, respectively) on subsidiaries from either France (columns (1) to (3)) or the U.K. ((4) to (6)). While columns (1) and (4) focus on

total debt, columns (2) and (5) ((3) and (6)) consider short-(long)-term debt. At the bottom of the table, we also report the total number of observations (“Observations”), the number of those treated ($Treat = 1$; “Treated Obs.”), the number of those treated whose parent country has a CR-index value different from its host ($Different^{CRI} = 1$; “Treated Obs. (SR)”), and the number of those treated whose parent country has a value similar to its host ($Similar^{CRI} = 1$; “Treated obs. (WR)”). We further report the overall adjusted R-squared for the two regression types (“Ad. R2 (Panel A or B)”).

TABLE 4 ABOUT HERE.

Our evidence supports the supply-side view’s prediction that stronger (weaker) creditor rights boost (dampen) corporate debt-taking. Since France (the U.K.) grants the weakest (strongest) legal protection to creditors, any change in protection favors (hurts) creditors, plausibly making them more (less) willing to lend. Accordingly, the table shows that the EIR raised (lowered) debt-taking in France (the U.K.). For instance, foreign French (U.K.) subsidiaries raised (lowered) their total debt by 1.6 (3.7) percent point (t -statistic: 2.58 (−3.65)) relative to domestic ones from pre to post-EIR (Panel A). These changes are also economically important, representing 12% (10%) of average pre-EIR total debt in France (the U.K.). Interestingly, Panel B suggests that most of the effect is driven by foreign subsidiaries with parents in countries with dissimilar creditor rights relative to the host countries. While foreign France (U.K.) subsidiaries with parents in countries with very different creditor rights regimes raised (lowered) their total debt by 1.7 (4.7) percent point (t -statistic: 2.53 (−3.49)) relative to domestic ones from pre- to post-EIR, the corresponding change for those with parents in countries with similar creditor rights is insignificant (smaller but significant).

Table 4 provides evidence on the maturity of debt, showing that the effects of the EIR on total debt are mostly driven by short-term debt. One possible reason might be that short-term lending is far more popular than long-term lending in France and the U.K. (e.g., recall the descriptive statistics in Table 3). Another reason might be that the EIR excludes *rights-in-rem* often used to secure long-term lending, especially in common law countries.¹⁴ Accordingly, we find that for foreign U.K. subsidiaries with parents in countries with much weaker rights than the U.K., the change in long-term debt is insignificant independently from the creditor rights regime of the parent country. Instead, that change is 0.30 percent point and marginally significant (t -statistic: 1.72) for foreign French subsidiaries when the parents countries have a different (i.e., stronger) creditor rights regime than France.

Finally, statistics at the bottom of the table corroborate that the French and U.K. samples feature a reasonably number of treated observations. While about 21% of the overall number of French observations, 16,885, are treated, the corresponding number for the U.K., 17,092, is about 36%. Of

¹⁴While long-term lending is often secured through floating charges — a specific *right-in-rem* — in the U.K. (see Franks and Sussman (2005)), French laws do not allow for such rights in secured lending (see Aretz et al. (2020)).

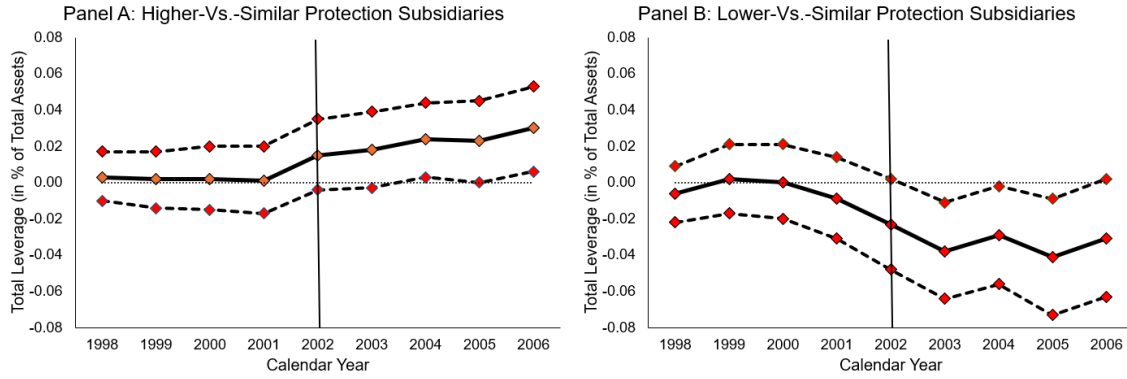


Figure 2. The figure plots the annual triple-difference (DID) coefficients from a regression equivalent to that in column (4) of Table 5 except that we replace the double interaction with an interaction between *Treat* and a vector of dummy variables, each equal to one for one year between 1998 and 2006 and else zero, and the triple interactions with interactions between *Treat*, either Up^{CRI} or $Down^{CRI}$, and that same vector. The black lines are the coefficients, whereas the broken lines are their 95% confidence bands.

the treated French (U.K.) observations, about 69% (40%) come from subsidiaries with parents in countries with strongly different creditor rights relative to their host country.

4.3 The Debt Effects of the EIR in the EU-12 Sample

Table 5 shows the results from estimating regression (3) with total debt as dependent variable on different sets of EU-12 countries. While most of the statistics at the bottom of the table are defined as in Table 4, “Treated Obs (SR)” (“Treated Obs (WR)”) now gives the number of observations coming from foreign subsidiaries with parents in countries with an at least two point higher (lower) CR-index value than their host countries. Across all subsamples, our evidence confirms a positive effect of creditor rights on debt-taking. For instance, in the sample including all countries, subsidiaries with parents in countries with stronger creditor right regimes than the one of the host countries raise their total debt by 1.9 percent points (t -statistic: 3.11) relative to those subsidiaries with parents in countries with more similar rights from pre to post-EIR (see the coefficient on the triple interaction between *Treat*, *Shock*, and Up^{CRI}). In contrast, the total debt of foreign subsidiaries with parents in countries with markedly weaker creditor rights *drops* by 2.9 percent points (t -statistic: -3.27) relative to those subsidiaries with parents in countries with more similar rights over that same period (see the coefficient on the triple interaction with $Down^{CRI}$). As before, these changes are economically meaningful, representing 12% and 18% of average pre-EIR total debt, respectively.

TABLE 5 ABOUT HERE.

We also perform a dynamic analysis by estimating regression (3) with *Shock* replaced by a vector of yearly dummies using 1997 as base year. Figure 2 plots the estimated coefficients of the yearly triple

interactions for foreign subsidiaries with parents in stronger (Panel A) or weaker (Panel B) creditor rights countries. In line with the parallel trends assumption, differences are both economically and statistically small before the EIR. Yet, they markedly increase (decrease) around the EIR year and afterward for the subsidiaries with parents in stronger (weaker) creditor rights countries.

Table 5 yields two additional insights. First, the results point to so-called dosage effects, implying that the creditor-rights-related impact (i.e., the absolute coefficients on the triple interactions) is stronger in samples including countries with more extreme creditor rights (column (1)). Excluding those countries, the reform's impact reduces to the point at which one of the interaction terms, Up^{CRI} , becomes insignificant (column (3)). Second, the table shows that the EIR also generated a creditor-rights-unrelated debt-financing effect. Although statistically and economically weaker than the DIDID terms, the estimate of the double interaction term, $Treat \times Shock$, reveals that, after the EIR, the total debt of foreign subsidiaries with parents in countries with similar creditor rights as their host countries dropped by 1.3 percent points (t -statistic: -2.67) relative to domestic subsidiaries (column (4)). This effect might capture a degree of unfamiliarity with the foreign country's norms, culture, and/or legal system as well as legal uncertainty about about the country in which borrowers would have to file for insolvency in the long-term future (see Sussman (2008)).

4.4 The Effects of Granting or Withdrawing Single Rights

We now decompose the CR index into each of its subindexes, namely, secured creditors are paid first (SPF); creditors can block reorganizations (CC); managers do not stay in place (MR); and no automatic stay on assets (NOS). Previous intra-country studies have analysed one or just a few dimensions of creditors protection (e.g., Rodano et al. (2016), Closset and Urban (2019), Schoenherr and Starman (2022), De Vito and Jacob (2023)). On the contrary, our setting allows us to compare the effects of a much larger set of rights and, more importantly, to contrast the effect of granting one specific right with that of withdrawing that same right. In Table 6, the first four columns report the effects of each single right in isolation, while columns (5) and (6) jointly consider the SPF, CC, and NOS rights and all rights together, respectively.¹⁵ Finally, Treated Obs (ON) ((OFF)) at the bottom of the table indicates the number of observations for which at least one of the rights is granted (withdrawn).

TABLE 6 ABOUT HERE.

Columns (1) to (4) show that each single creditor right produces the anticipated effects: When the right is granted (withdrawn), debt-taking increases (decreases). Notwithstanding, both the statistical and economic significance of the effects vary across single rights and whether a right is granted or

¹⁵Since the CC and MR rights share a cross-country correlation of about 0.80, there is the justified concern that standard errors may severely blow up if we include both variables in the same specification.

withdrawn. For example, while both shifting secured creditors to the top of the bankruptcy ladder of priorities (column (1)) and the lifting of automatic stays on assets (column (4)) raise debt-taking, the increase is 3.6 percent points (t -statistic: 4.78) for the former but only 1.0 points (t -statistic: 1.81) for the latter right. Further, granting creditors the right to block reorganizations (column (2)) raises debt-taking by 1.2 percent points (t -statistic: 1.95), while withdrawing that same right lowers debt-taking by a larger extent, 2.7 percent points (t -statistic: -3.31). Such differences become sharper when rights are considered together in the same specification (columns (5) and (6)). For instance, column (6) shows that only granting the SPF and MR rights significantly raises debt-taking, while only withdrawing the CC and NOS rights lowers it in the most comprehensive model. Surprisingly, granting the CC right lowers debt financing (t -statistic: -1.70) in that same model, although inflated standard errors caused by multicollinearity are likely to be behind that finding.

While it is likely that some legal protections matter more to creditors than others, it is still puzzling that granting and withdrawing the same right yields highly asymmetric effects. One possible reason for the stronger effect observed upon withdrawing a right (e.g., the CC and NOS rights) could be that its unexpected disappearance cannot be promptly and easily replaced by alternative solutions, for example, private contracts (see, e.g., Franks et al. (2021)). Yet, the same reason can hardly explain why, in other instances, granting a right generates a stronger effect (e.g., the SPF right).

4.5 The Effects of Other Creditors Rights

We next examine whether legal creditor protections other than those included in the CR index are important to creditors, in line with Armour et al.'s (2009) arguments. We first analyse whether changes in those other creditor rights have an impact on debt-taking and then compare their effects with those stemming from changes in the CR index, as described in regression (5). To identify such other rights, we rely on Marshall's (2002) comparative analysis of insolvency laws in the EU countries before the EIR. In detail, we consider the following four alternative rights: (a) firms must prove that they are insolvent to start procedures; (b) the debtor-in-possession can terminate existing contracts; (c) a high number of creditors must approve reorganizations; and (d) the debtor is not discharged upon the closure of procedures. While right (a) prevents firms from strategically filing, right (b) gives firms more flexibility to redirect cash toward creditors. Conversely, while right (c) enhances creditors' ability to block reorganizations, right (d) gives them more ways to recoup their loan losses.

TABLE 7 ABOUT HERE.

Table 7 demonstrates that the non-CR-index rights matter to creditors to a certain extent and, ultimately, shape corporate borrowing. Notwithstanding, their effects are highly asymmetrical. While

column (6), for example, shows that imposing a higher creditor-value threshold to approve reorganizations (right (b)) raises debt by 2.2 percent points (t -statistic: 2.60) controlling for CR-index variations, the corresponding number for the imposition of a lower threshold is an insignificant 0.7 percent point (t -statistic: 0.93). Strikingly, columns (1) and (2) reveal that the lifting of the proof of insolvency requirement (right (a)) raises (and not lowers) debt by 3.3 and 2.3 percent points (t -statistics: 4.82 and 3.22), respectively, suggesting that creditors are not too concerned about strategic defaults and rather strongly prefer if firms have the flexibility to file for insolvency relatively early.

Taken together, this evidence further suggests that stronger (weaker) credit rights foster (impede) firm debt-taking, consistent with the supply-side view of creditor protection. Notwithstanding, it also shows that the strength of debt-taking effects varies strongly across different legal rights and depending on whether a right is granted or withdrawn. Finally, the section reveals that rights excluded from the La Porta et al. (1998)-Djankov et al. (2007) CR index also matter for firm debt.

5 Letter-Vs.-Execution of the Law

In this section, we study the role of enforcement. In their theoretical work, Jappelli et al. (2005) show that better enforcement broadens debt access at each creditor rights level. Their empirical work reveals that bank lending is lower in Italian regions with greater backlogs of pending trials over the 1984 to 1995 period (see also Cristini et al. (2001) and Pinheiro and Cabral (2001)). Safavian and Sharma (2007) report that better enforcement amplifies the effects of within-country creditor right improvements on bank financing in 27 European countries, suggesting that enforcement is the channel through which creditor rights work. In agreement with this evidence, Ponticelli and Alencar (2016) shows that a bankruptcy law reform in Brazil aiming at strengthening creditor rights increased secured debt, investment, and output values mainly in those municipalities with less congested courts. Although relevant, the results of these studies could be affected by endogeneity and/or limitations in identifying meaningful exogenous creditor rights variations.

We leverage on our identification setting to first investigate the role of enforcement compared to that of creditor rights in shaping corporate borrowing and, second, to analyze the role of creditor rights conditional on enforcement. To this end, we use the following proxies for enforcement: (i) the number of days it takes courts to settle a business dispute (*Court Time*) as provided by Djankov et al. (2007); (ii) the formality of the legal system (*Legal Formalism*) as provided by Djankov et al. (2003); and (iii) a risk-rating-agency-based assessment of the efficiency of the judicial system with respect to business (*Judicial Efficiency*) as provided by La Porta et al. (1998).¹⁶

¹⁶We select the number of days to settle a business dispute as first proxy for enforcement because this variable relates more to our setting than the number of days it takes to settle either an eviction or bounced check case. Yet, since these variables are highly correlated (0.80) across the EU-12 countries, we reach identical conclusions if we

Table 8 reports the results from regressions investigating the separate and conditional causal effects of enforcement. Running a horse race between creditor rights and enforcement, Panel A shows the estimates from equation (5) using total debt as dependent variable and one of the enforcement proxies as x -variable on the EU-12 country sample. Conversely, Panel B reports estimates from equation (3) using total debt as dependent variable on EU-12 country-pair subsamples for which both a subsidiary's host and parent countries have an above or below EU-12-median value for one of the enforcement variables. Finally, the bottom of the table now also shows Treated Obs. (HE) ((LE)), the number of observations shifted to a stronger (weaker) enforcement regime.

TABLE 8 ABOUT HERE.

Panel A suggest that, at least within the EU, creditors pay more attention to their legal rights than the enforcement of those rights. Across our three enforcement proxies, most of their effects are insignificant except for improvements in the judicial system boosting corporate debt-taking (see column (2)). Importantly, even after controlling for enforcement, the effects of changes in creditor rights remain strongly significant, economically important, and similar to before.

Despite the only weak separate enforcement effects, Panel B show that enforcement can condition the debt financing effects of strengthening or weakening creditor rights. To wit, increases in debt-taking generated by improved creditor rights are stronger in high enforcement systems, in line with Safavian and Sharma (2007). For instance, the court time results indicate that while the debt of foreign subsidiaries with parents in countries with stronger creditor rights increases by 3.8 percent points (t -statistic: 2.85) from pre to post-EIR in the lower number-of-days sample (column (1)), the corresponding increase within the higher number-of-days sample is a smaller 2.8 percent points (t -statistic: 2.42) in column (2). We find similar results for the other enforcement proxies. In contrast and new to the literature, the decline in debt-taking generated by worsened creditor rights, however, tends to be stronger in two out of three low enforcement samples (columns (3) and (5)).

All in all, this section suggests that while, at least within the EU, enforcement has only weak separate debt financing effects controlling for creditor rights, it conditions the effects of both strengthening and weakening creditor rights. Interestingly, however, it does so with different signs, magnifying the positive effects of strengthening rights but cushioning the negative effects of weakening rights. Thus, enforcement is not simply a channel through which creditor rights work.

rely on one of the other variables. The second proxy for enforcement captures the formality dimension of a legal system suggesting that a more formal system requires longer to settle cases. Finally, the third proxy for enforcement is the result of “[...] an assessment of the efficiency and integrity of the legal environment as it affects business [...] produced by the country risk-rating agency International Business Corp.” (La Porta et al. (1998)). Although it captures an important dimension of enforcement, the main limitation of such proxy is that it has been measured twenty years earlier than our sample period, between 1980 to 1983.

6 Bank Strength and the EIR's Debt-Financing Effects

We next condition the debt-financing effects of the EIR in the U.K. on the strength and commercial lending expertise of subsidiaries' home-banks. Stronger banks may depend less on creditor rights since they are better placed to absorb losses and to monitor their borrowers (see Jayaraman and Thakor (2013)). Conversely, banks with more lending expertise may be better able to deal with borrower distress out-of-court (Franks and Sussman (2005)). The upshot is that both bank strength and lending expertise could act as substitutes for creditor rights. To test that hypothesis, we separately estimate regression (1) on U.K. subsidiaries funded by stronger or weaker banks or banks with more or less lending expertise. In addition, we further pool the corresponding subsamples and estimate a fully-interacted version of that same regression. We use banks' 2001 equity-to-assets and mean pre-reform ROA to measure their strength and the mean pre-reform share of interest obtained from commercial lending to measure their lending expertise.

Table 9 presents the results from the regressions conditioning on bank strength and lending expertise. The table suggests that only weak banks and those with little lending expertise reduce their corporate lending in response to the drop in creditor rights induced through the EIR. Using the equity-to-assets ratio to measure bank strength, we, for example, find that while the debt-financing of foreign U.K. subsidiaries funded by weak banks drops by 6.6 percent points (t -statistic: -4.14) relative to that of domestic subsidiaries from pre to post-EIR, the corresponding number for foreign U.K. subsidiaries funded by strong banks is an insignificant 2.1 percent points (t -statistic: -0.97 ; contrast columns (1) and (2)). As a result, column (3) reveals that the difference between the two estimates is a marginally significant 4.5 percent points (t -statistic: 1.72).

TABLE 9 ABOUT HERE.

Overall, our results in this section suggest that creditor rights can act as a backstop for banks with a limited capacity to absorb loan losses or to monitor their borrowers and those with little lending expertise, offering them a safety net in case of borrower distress. They are also important in corroborating that the EIR's debt-financing effects come from the supply — and not demand — side, with important supply-side characteristics conditioning these effects.

7 Religion, Culture, and Politics

One important concern in the law and finance literature is that a country's religious, cultural, and political beliefs must shape its legal environment. As such, these variables might endogenously drive the relation between creditor rights and financial and real outcomes (La Porta et al. (2008)). The

advantage of our identification setting is that we keep constant the cultural, religious, and political beliefs of all actors — except the courts. Put differently, if those beliefs affect our conclusions, then their influence must seep in through the court officials in the insolvency proceedings.

To test this possibility, we estimate regression (5) comparing changes in creditor rights with changes in a set of proxies for religious, cultural, and political beliefs (i.e., x -variable) using the EU-12 country sample. We use the fraction of a country’s catholic population as proxy for religious beliefs; Hofstede’s (2001) five culture indexes (power distance, individualism, masculinity, uncertainty avoidance, and long-term orientation) as culture proxies; and whether a country has a proportional election system, the share of years over which it was governed by a left-wing party or coalition, its share of unionized workers, and the strength of its labor laws as proxies for its left-leaning tendencies.

Table 10 reports results for the religious and cultural proxies. At the bottom of the table, Treated Obs. (HCV) ((LCV)) is the number of observations shifted to a country with a higher (lower) religious or culture proxy variable value. The table shows that after controlling for religion or culture, the effects of creditor rights on debt-financing remain very strong and robust (estimates of both Up^{CRI} and $Down^{CRI}$ mirror those in our main tests). In contrast, the effects of religion and culture proxies (estimates of both Up^{CulVar} and $Down^{CulVar}$) are either insignificant or counterintuitive.

TABLE 10 ABOUT HERE.

Table 11 reports results for the political proxies. A higher value for each of these proxies is often interpreted as indicating a more equal society in which it may be harder for creditors to seize control of defaulting borrowers and recoup their loans. At the bottom of the table, Treated Obs. (HSV) ((LSV)) is the number of observations shifted to a country with a higher (lower) political proxy variable value. The table shows that controlling for the politics variables does not materially alter the robustness of the creditor right effects.¹⁷ As for the political proxies, most of their effects are either insignificant or counter-intuitive such as the results on union density (column (3)).

TABLE 11 ABOUT HERE.

Overall, the evidence in this section mitigates concerns that the firm debt-taking effects spurred by strengthening and weakening creditor right detected in our main analysis could be endogenously driven by religious, cultural, and political beliefs, a common critique of law and finance studies.

¹⁷A minor exception is the rise in debt-taking in response to stronger creditor rights when we control for the share of unionized workers, which becomes significant at the 90% confidence level (t -statistic: 1.82; see column (3)).

8 Placebo, Parallel-Trend, and PS-Matching Tests

In this section, we provide the results from placebo, parallel-trend, and robustness tests corroborating our conclusions. We first show that our main findings do not arise when we assign treatment status to foreign subsidiaries unlikely to be affected by the EIR, either due to their parents' registration countries, their ownership structure, or a random reshuffling of treatment status. We next verify that our empirical setting complies with the parallel-trends assumption. To further alleviate the concern that (observable) differences between treated and control subsidiaries could distort our conclusions, we finally reestimate our main regressions on propensity-score (PS) matched subsidiaries.

8.1 Placebo Tests

Table 12 presents the results from estimating placebo versions of equation (3) using total debt as dependent variable on French and U.K. (Panel A) or all EU-12 (Panel B) subsidiaries. For convenience, column (1) reports the same results as in Table 5. In column (2), we select as treated only those foreign subsidiaries with parents in Denmark or Norway, neither of which is a signatory to the EIR. We acknowledge that this placebo test is imperfect since while recognition of EIR rulings is not automatic in non-signatory states, those states can recognize them voluntarily. In column (3), we exclusively consider non-fully-owned subsidiaries, where, to be conservative, we define those as subsidiaries in which the parent's stake is less than 80%. As clarified by several EU court rulings, the managers of parents with a less than 100% stake cannot exert complete control over their subsidiaries' head office functions, implying that the subsidiaries would continue to file for insolvency in their host (and not parent) countries after the EIR. In column (4), we randomly reshuffle the parent countries of the foreign (but not domestic) subsidiaries in our sample. Across all placebo tests, Table 12 shows insignificant effects and in some cases the wrong signs (e.g., columns (2) and (3)).

TABLE 12 ABOUT HERE.

8.2 Parallel-Trend Tests

A condition for DID and DIDID tests to yield valid inferences is that the treated and controls display similar trends in outcome variables before the shock ("parallel-trends condition"). Although Figure 2 already offers informal evidence that our foreign subsidiaries with parents in countries with stronger or weaker creditor rights than their host countries produce similar pre-EIR debt trends relative to those with parents in countries with similar rights, we offer more formal evidence in Table 13. While Panel A contrasts the change in total debt and our (non-averaged) controls over each pre-EIR

year across domestic and foreign subsidiaries for France and the U.K. (columns (1) to (4)) and the EU-12 countries ((5) to (8)), Panel B (C) runs the analogous comparison across foreign subsidiaries with parents in countries with stronger (weaker) creditor rights than their host countries and those with parents in countries with similar rights. Panel A confirms that there are only small and non-systematic differences in the changes across domestic and foreign subsidiaries. More importantly, Panels B and C corroborate that there are even fewer differences in the changes across foreign subsidiaries with parents in countries with different levels of creditor protection.

TABLE 13 ABOUT HERE.

8.3 PS-Matching Tests

We finally repeat our main regressions in Table 5 on a PS-matched sample of domestic and foreign subsidiaries to further rule out that the pre-EIR differences in those types of subsidiaries in Table 13 can distort our conclusions. To that end, we run country-specific logistic regressions of *Treat* on the pre-EIR averages of total debt and the controls in 2001, matching each foreign subsidiary to a unique domestic subsidiary according to the fitted regression value but excluding pairs for which the distance exceeds 0.01 in the common support of the PS-matching line. While we do not report results, we have verified that the matched domestic and foreign subsidiaries do indeed no longer vary (either economically or statistically) across the matching variables.

Table 14 gives the results from the regressions run on the PS-matched samples. Although the PS-matched samples are significantly smaller than the original ones (see “Observations”), the table suggests that they nonetheless produce results in complete accordance with those obtained from our main specifications. Looking into the EU-12 sample, column (4), for example, reveals that while the total debt of foreign subsidiaries with parents in countries with stronger creditor rights than their host countries rises by 2.4 percent points (*t*-statistic: 3.40) relative to those with parents in countries with similar rights from pre to post-EIR, the corresponding number for those with parents in countries with weaker creditor rights is −2.7 percent points (*t*-statistics: −2.86).

TABLE 14 ABOUT HERE.

Taken together, the evidence in this section mitigates several valid concerns about the results from our main tests, namely, that they reflect some pre-existing and/or alternative effect.

9 Conclusion

Motivated by the limitations of existing studies, we use the EIR to identify the causal effects of creditor rights on corporate borrowing. The EIR shifted the insolvency jurisdiction of fully-owned EU subsidiaries from their host to their parent country, with important repercussions for the identity of the insolvency administrator, the bankruptcy ladder of priorities, creditor powers, and automatic stays, all legal aspects captured by the La Porta et al. (1998)-Djankov et al. (2007) CR index. Supporting the supply-side view of creditor rights, we show that the regulation led foreign subsidiaries with parents in countries with stronger (weaker) CR-index creditor rights than their host countries to take out more (less) debt. We further demonstrate that the single rights making up the CR index have heterogeneous and asymmetric effects and do not capture all angles of insolvency law relevant to creditors. While enforcement has only weak separate effects controlling for creditor rights, it can compound the effects of strengthening or weakening creditor rights. In a unique innovation, we also show that the debt-financing effects of creditor right variations are critically conditioned by the strength of subsidiaries' banks and their commercial lending expertise. We finally refute the popular critique that the links between creditor rights and financial or real variables in prior work are endogenously driven by creditor rights reflecting cultural, religious, and political beliefs.

References

- Acharya, Viral V., and Krishnamurthy V. Subramanian, 2009, Bankruptcy codes and innovation, *Review of Financial Studies* 22, 4949–4988.
- Aghion, Philippe, and Patrick Bolton, 1992, An incomplete contracts approach to financial contracting, *Review of Economic Studies* 59, 473–94.
- Akbel, Basak, and Monika Schnitzer, 2011, Creditor rights and debt allocation within multinationals, *Journal of Banking & Finance* 35, 1367–1379.
- Angrist, Joshua D., and Jörn-Steffen Pischke, 2009, *Mostly Harmless Econometrics: An Empiricist's Companion* (Princeton University Press, Princeton, NJ).
- Aretz, Kevin, Murillo Campello, and Maria Teresa Marchica, 2020, Access to collateral and the democratization of credit: France's reform of the Napoleonic security code, *Journal of Finance* 75, 45–90.
- Armour, John, Simon Deakin, Priya Lele, and Mathias Siems, 2009, How do legal rules evolve? Evidence from a cross-country comparison of shareholder, creditor and worker protection, *International Review of Law and Economics* 29, 282–303.
- Closset, Frédéric, and Daniel Urban, 2019, The balance of power between creditors and the firm: Evidence from German insolvency law, *Journal of Corporate Finance* 58, 454–477.
- Cristini, Marcela, Ramiro Moya, and Andrew Powell, 2001, The importance of an effective legal system for credit markets: The case of Argentina, Working Paper IDB Working Paper No. 142, Inter-American Development Bank, Washington, D.C.
- De Vito, Antonio, and Martin Jacob, 2023, The role of creditor protection in lending and tax avoidance, *Journal of Financial and Quantitative Analysis* 58, 2096–2130.
- Desai, Mihir A., C. Fritz Foley, and James R. Hines Jr., 2004, A multinational perspective on capital structure choice and internal capital markets, *Journal of Finance* 59, 2451–2487.

- Djankov, Simeon, Rafael La Porta, Florencio Lopez-De-Silanes, and Andrei Shleifer, 2003, Courts, *Quarterly Journal of Economics* 118, 453–517.
- Djankov, Simeon, Caralee McLiesh, and Andrei Shleifer, 2007, Private credit in 129 countries, *Journal of Financial Economics* 84, 299–329.
- Faulkender, Michael, and Jason M. Smith, 2016, Taxes and leverage at multinational corporations, *Journal of Financial Economics* 122, 1–20.
- Franks, Julian R., Gunjan Seth, Oren Sussman, and Vikrant Vig, 2021, The limits of Coase: A study of financial distress in the shipping industry, *Working Paper*.
- Franks, Julian R., and Oren Sussman, 2005, Financial distress and bank restructuring of small- to medium-sized UK companies, *Review of Finance* 9, 65–96.
- Giannetti, Mariassunta, 2003, Do better institutions mitigate agency problems? Evidence from corporate finance choices, *Journal of Financial and Quantitative Analysis* 38, 185–212.
- Gurrea-Martínez, Aurelio, 2022, The future of insolvency law in a post-pandemic world, *International Insolvency Review* 31, 385–402.
- Hart, Oliver, and John Moore, 1994, A theory of debt based on the inalienability of human capital, *Quarterly Journal of Economics* 109, 841–879.
- Hart, Oliver, and John Moore, 1998, Default and renegotiation: A dynamic model of debt, *Quarterly Journal of Economics* 113, 1–41.
- Haselmann, Rainer, Katharina Pistor, and Vikrant Vig, 2010, How law affects lending, *Review of Financial Studies* 23, 549–580.
- Hofstede, Geert, 2001, *Culture's Consequences: Comparing Values, Behaviors, Institutions and Organizations Across Nations* (Sage Publications).
- Huizinga, Harry, Luc Laeven, and Gaetan Nicodeme, 2008, Capital structure and international debt shifting, *Journal of Financial Economics* 88, 80–118.
- Jappelli, Tullio, Marco Pagano, and Magda Bianco, 2005, Courts and banks: Effects of judicial enforcement on credit markets, *Journal of Money, Credit and Banking* 37, 223–244.
- Jayaraman, Sudarshan, and Anjan V. Thakor, 2013, The effect of creditor rights on bank monitoring, capital structure and risk-taking, Technical Report Finance Working Paper No. 387/2013, European Corporate Governance Institute (ECGI).
- Klick, Jonathan, 2013, Shleifer's failure, *Texas Law Review* 91:899, 898–909.
- La Porta, Rafael, Florencio Lopez-De-Silanes, and Andrei Shleifer, 2008, The economic consequences of legal origins, *Journal of Economic Literature* 46, 285–332.
- La Porta, Rafael, Florencio Lopez-De-Silanes, Andrei Shleifer, and Robert W. Vishny, 1997, Legal determinants of external finance, *Journal of Finance* 52, 1131–1150.
- La Porta, Rafael, Florencio Lopez-de-Silanes, Andrei Shleifer, and Robert W. Vishny, 1998, Law and finance, *Journal of Political Economy* 106, 1113–1155.
- Lehmann, Alexander, Selin Sayek, and Hyoung Goo Kang, 2004, Multinational affiliates and local financial markets, *Working Paper*.
- Levine, Ross, 1997, Financial development and economic growth: Views and agenda, *Journal of Economic Literature* 35, 688–726.
- Marshall, Jennifer, 2002, Comparing Europe's insolvency rules, *International Financial Law Review* 21, 23–30.
- Moss, Gabriel, and Tom Smith, 2002, Commentary on Council Regulation 1346/2000 on insolvency proceedings, in Gabriel Moss, Ian Fletcher, and Isaacs Stuart, eds., *The EC Regulation on Insolvency Proceedings: A Commentary and Annotated Guide*, 225–352 (Oxford University Press).
- Oto-Peralías, Daniel, and Diego Romero-Ávila, 2017, Legal reforms and economic performance: Revisiting the evidence, *World Development Report*.

- Öztekin, Özde, 2015, Capital structure decisions around the world: Which factors are reliably important?, *Journal of Financial and Quantitative Analysis* 50, 301–323.
- Pinheiro, Armando Castelar, and C. Cabral, 2001, Credit markets in Brazil: The role of the judiciary and other institutions, in Marco Pagano, ed., *Defusing Default: Incentives and Institutions* (Inter-American Development Bank, Washington, D.C.).
- Ponticelli, Jacopo, and Leonardo S. Alencar, 2016, Court enforcement, bank loans, and firm investment: Evidence from a bankruptcy reform in Brazil, *Quarterly Journal of Economics* 131, 1365–1413.
- Rodano, Giacomo, Nicolas Serrano-Velarde, and Emanuele Tarantino, 2016, Bankruptcy law and bank financing, *Journal of Financial Economics* 120, 363–382.
- Safavian, Mehnaz, and Siddharth Sharma, 2007, When do creditor rights work?, *Journal of Comparative Economics* 35, 484–508.
- Schoenherr, David, and Jan Starmans, 2022, When should bankruptcy law be creditor- or debtor-friendly? Theory and evidence, *Journal of Finance* 77, 2669–2717.
- Scott, Jonathan A., and Terence C. Smith, 1986, The effect of the Bankruptcy Reform Act of 1978 on small business loan pricing, *Journal of Financial Economics* 16, 119–140.
- Sussman, Oren, 2008, The economics of the EU's corporate-insolvency law and the quest for harmonisation by market forces, in *European Financial Markets And Institutions*, Xavier Freixas (ed.), Philipp Hartmann (ed.), Colin Mayer (ed.) (Oxford University Press).
- Townsend, Robert M, 1979, Optimal contracts and competitive markets with costly state verification, *Journal of Economic Theory* 21, 265–293.
- van Zwieten, Kristin, 2023, Restructuring law reform in the United Kingdom, *European Business Organization Law Review* 24, 287–315.
- Vig, Vikrant, 2013, Access to collateral and corporate debt structure: Evidence from a natural experiment, *Journal of Finance* 68, 881–928.
- Wessels, Bob, 2006, The place of the registered office of a company: A cornerstone in the application of the EC insolvency regulation, *European Company Law* 3, 183–190.
- Wessels, Bob, 2009, The ongoing struggle of multinational groups of companies under the EC insolvency regulation, *European Company Law* 6, 169–176.
- Wood, Philip R., 2007, *Principles of international insolvency* (Sweet Maxwell Ltd).

Table 1. Noteworthy EU Court Rulings on the COMIs of Multinational Business Groups

In this table, we show noteworthy EU court rulings on the centre of main interests (COMI) of multinational business groups. We give the case name, judgment date, countries of registration of the subsidiaries and the parent, the COMI decision, whether the subsidiary COMIs were shifted from the country of registration to the parent's country, and the court's reason for their decision. The cases are all those involving multinational business groups from Chapter 30 of Wood (2007).

Case	Judge- ment Date	Subsidiary Country of Registration	Parent Country	COMI	COMI Shift?	Reason
Re Enron Director SA	Jul-02	Spain	UK	UK	YES	Head office functions conducted in UK.
Re CrissCross Telecom- munications	May-03	Pan-European	UK	UK	YES	Management, administration, and accounting functions performed in the UK; most contracts in London; bills sent to London address.
Cirrio Del Monte	Aug-03	Netherlands	Italy	Italy	YES	Management and control exercised from Italy; and directors all resided in Italy.
Daisytek	2004	Pan-European	UK	UK	YES	Parent approval of contracts; consultation on both recruitment and IT strategies; CEO spent much of his time managing the subsidiaries.
Hettlage	May-04	Austria	Germany	Germany	YES	Purchasing, selling, and marketing personnel and accounting systems operated from Germany.
Parmalat	Jun-04	Slovakia	Hungary	Hungary	YES	Financial affairs conducted from Hungary; and main decisions also made in Hungary.
Zenith	Jul-04	Austria	Germany	Germany	YES	Business exclusively administered by German parent; management decisions also made there.
Hulka AG	Aug-04	Austria	Germany	Germany	YES	Financial affairs entirely conducted from Germany.
RE AIM Underwriting	2005	Ireland	UK	UK	YES	Financial affairs conducted from the UK; director resided in the UK; parent had full control for the subsidiary's management and administration.
MG Rover	2005	Pan-European	UK	UK	YES	Subsidiaries do not have an autonomous and independent trading existence from parent.
EMTEC	Feb-06	Pan-European	France	France	YES	All subsidiaries managed from France with respect to purchasing, employees, accounting, IT, financing; many board meetings in France.
Eurotunnel	Aug-06	Pan-European	France	France	YES	Strategic and operational management entirely conducted from France; essential parts of the business carried out in France.

Table 2. The Host and Parent Countries of EU-12 Subsidiaries

In this table, we show the average number (MeanObs) and proportion (%RowSum) of subsidiaries in the EU-12 countries by host (rows) and parent (columns) country, where the host country is the country in which the subsidiary is registered and the parent country the one in which its parent is registered. In the bottom row (final column), we also report the total number of subsidiaries for each parent (host) country. We calculate each statistic first by sample year and then average over time. We calculate the proportion as the fraction of subsidiaries with some parent country within a host country. The table only considers subsidiaries fully (i.e., 100%) owned by their parents. CR is the 2001 value of the creditor rights index of La Porta et al. (1998) and Djankov et al. (2007).

Subsidiary Country	Parent Country											
	Austria CR=3	Bel- gium CR=2	Fin- land CR=1	France CR=0	Ger- many CR=3	Ireland CR=1	Italy CR=2	Nether- lands CR=3	Portu- gal CR=1	Spain CR=2	Sweden CR=1	UK CR=4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Austria	MeanObs. %RowSum	23 53.6%	1 1.6%	2 5.1%	9 20.1%	1 1.2%	1 1.2%	5 11.8%			1 3.2%	2 3.5%
Belgium	MeanObs. %RowSum	0 0.2%	4 1.6%	29 12.4%	23 9.9%	1 0.6%	1 0.6%	88 38.3%		1 0.2%	7 2.8%	16 7.1%
Finland	MeanObs. %RowSum	0 0.0%	314 77.1%	13 3.2%	4 1.0%	0 0.1%	1 0.1%	21 5.2%		0 0.0%	43 10.7%	8 1.9%
France	MeanObs. %RowSum	8 0.4%	9 0.5%	1,334 79.0%	80 4.7%	2 0.1%	16 1.0%	99 5.9%	2 0.1%	2 0.1%	28 1.6%	66 3.9%
Germany	MeanObs. %RowSum	5 1.1%	4 0.9%	29 6.7%	327 76.8%	1 0.1%	3 0.7%	29 6.8%		0 0.1%	8 1.9%	19 4.5%
Ireland	MeanObs. %RowSum	1 2.4%	1 1.8%	2 6.7%	1 3.3%	6 18.2%	1 2.7%	12 37.1%			1 2.7%	8 24.9%
Italy	MeanObs. %RowSum	5 0.7%	6 1.0%	71 11.2%	54 8.5%	0 0.0%	355 56.3%	76 12.1%	1 0.1%	4 0.6%	16 2.5%	28 4.5%
Netherlands	MeanObs. %RowSum	1 0.1%	7 0.6%	36 3.2%	56 5.0%	1 0.1%	6 0.6%	921 82.2%		1 0.1%	28 2.5%	34 3.1%
Portugal	MeanObs. %RowSum	1 0.8%	1 0.7%	6 7.3%	5 5.4%		1 0.9%	2 2.7%	65 76.9%	2 1.8%	1 1.4%	2 2.0%
Spain	MeanObs. %RowSum	2 0.3%	4 0.5%	60 7.5%	54 6.7%	1 0.2%	21 2.6%	64 8.0%	4 0.5%	543 67.7%	10 1.3%	30 3.7%
Sweden	MeanObs. %RowSum	2 0.1%	53 2.2%	33 1.4%	33 1.3%	1 0.0%	4 0.1%	67 2.8%			2,201 91.0%	17 0.7%
UK	MeanObs. %RowSum	8 0.4%	27 1.6%	142 8.3%	109 6.4%	43 2.5%	14 0.8%	197 11.5%	2 0.1%	2 0.1%	41 2.4%	1,084 63.4%
Total		53	216	429	1,756	753	423	1,582	73	554	2,385	1,314
												9,594

Table 3. Descriptive Statistics

In this table, we report descriptive statistics for our main analysis variables separately for firms registered in each EU-12 country and for the entire sample. The descriptive statistics include the mean (Mean), median (Med.), and standard deviation (St.D.). TotalLev(verage) is the sum of short (Amadeus mnemonic: loan) and long-term (ltdb) debt scaled by assets (toas). In accordance, ShortTermLev(verage) and LongTermLev(verage) are short and long-term debt both scaled by assets, respectively. LNSize is the natural log of assets, CompanyAge the difference between the current date and the date of incorporation (in years), LNEmployees the natural log of the number of employees, Profitability is EBITDA scaled by assets, FixedAssets is tangible fixed scaled by assets, and cash holdings is the cash reserve (cash) scaled by assets. PRA indicates that we set a variable's value to its pre-reform (i.e., before 2002) average by firm. We winsorize all variables at the 5th and 95th percentiles by subsidiary country. Our sample period is 1997 to 2006.

		Total Lev	Short-term Lev	Long-term Lev	LN Size (PRA)	Company Age (PRA)	LN Employees (PRA)	Profitability (PRA)	Fixed Assets (PRA)	Cash Holdings (PRA)
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Austria N=433	Mean	0.154	0.065	0.022	10.707	13.300	5.380	0.106	0.262	0.063
	Med.	0.032	0.000	0.000	10.619	8.304	5.455	0.090	0.244	0.035
	St.D.	0.207	0.130	0.039	1.108	14.369	1.167	0.113	0.187	0.075
Belgium N=2,290	Mean	0.203	0.103	0.087	9.859	25.213	4.373	0.109	0.175	0.090
	Med.	0.125	0.014	0.000	9.633	21.776	4.411	0.093	0.118	0.060
	St.D.	0.221	0.146	0.149	1.271	17.548	1.207	0.092	0.169	0.087
Finland N=4,074	Mean	0.080	0.012	0.061	9.557	18.955	4.745	0.164	0.224	0.077
	Med.	0.000	0.000	0.000	9.397	11.053	4.686	0.147	0.150	0.040
	St.D.	0.134	0.032	0.109	1.438	18.970	1.324	0.122	0.203	0.095
France N=16,885	Mean	0.085	0.064	0.013	9.736	21.103	4.760	0.087	0.135	0.077
	Med.	0.023	0.014	0.000	9.625	15.077	4.730	0.080	0.088	0.040
	St.D.	0.126	0.099	0.045	1.326	17.548	1.316	0.093	0.136	0.089
Germany N=4,254	Mean	0.170	0.012	0.154	10.759	20.585	5.579	0.152	0.241	0.056
	Med.	0.016	0.000	0.003	10.551	11.807	5.668	0.114	0.179	0.021
	St.D.	0.247	0.041	0.243	1.412	19.545	1.335	0.178	0.221	0.080
Ireland N=329	Mean	0.174	0.067	0.097	10.025	19.741	4.452	0.067	0.221	0.112
	Med.	0.021	0.002	0.000	9.618	12.585	4.315	0.050	0.164	0.074
	St.D.	0.286	0.149	0.226	1.345	19.490	1.251	0.185	0.196	0.101
Italy N=6,299	Mean	0.133	0.104	0.025	10.411	20.702	4.774	0.099	0.164	0.045
	Med.	0.056	0.033	0.000	10.302	16.518	4.776	0.089	0.115	0.025
	St.D.	0.162	0.134	0.060	1.186	14.181	1.265	0.078	0.159	0.050

(continued on next page)

Table 3. Descriptive Statistics (cont.)

	Total Lev	Short-term Lev	Long-term Lev	LN Size (PRA)	Company Age (PRA)	LN Employees (PRA)	Profitability (PRA)	Fixed Assets (PRA)	Cash Holdings (PRA)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Spain N=8,023	Mean	0.163	0.088	9.584	17.251	4.441	0.105	0.210	0.096
	Med.	0.074	0.005	9.547	12.962	4.512	0.095	0.151	0.053
	St.D.	0.195	0.109	1.263	13.289	1.317	0.088	0.196	0.106
Sweden N=24,204	Mean	0.085	0.069	9.146	29.410	4.260	0.127	0.200	0.071
	Med.	0.000	0.000	8.993	24.069	4.248	0.117	0.137	0.030
	St.D.	0.142	0.031	1.242	22.227	1.238	0.110	0.183	0.092
UK N=17,092	Mean	0.397	0.294	10.601	27.325	5.434	0.111	0.260	0.091
	Med.	0.352	0.217	10.423	18.538	5.371	0.107	0.185	0.049
	St.D.	0.299	0.270	1.461	23.312	1.499	0.120	0.236	0.105
All N=95,935	Mean	0.158	0.089	9.871	25.353	4.798	0.117	0.205	0.078
	Med.	0.039	0.003	9.739	18.009	4.773	0.106	0.141	0.039
	St.D.	0.223	0.170	1.429	21.292	1.392	0.110	0.195	0.095

Table 4. The Debt-Financing Effects of the Reform in France and the UK

In this table, we report the results from panel regressions of total, short-term, and long-term debt financing on an interaction between a dummy variable equal to one for foreign subsidiaries and else zero (Treat) and a dummy variable equal to one starting from 2002 and else zero (Shock; Panel A) or the interactions of that product with dummy variables equal to one if a subsidiary's parent country has stronger (Different^{CRI}) or weaker (Similar^{CRI}) creditor rights relative to its host country (Panel B) and else zero, control variables, and fixed effects separately run on French and UK subsidiaries. We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. For France (the UK), the weak-rights countries have a La Porta et al. (1998)-Djankov et al. (2007) creditor rights index value of one or two (zero or one) and the strong-rights countries of three or four (two or three). The control variables are the natural log of size, company age, the natural log of the number of employees, profitability, fixed assets, and cash holdings. For each control, we interact its pre-reform average by firm with dummy variables equal to one for one specific year and else zero (Control Int.). We use both static firm (Firm FE) and year (Time FE) fixed effects. We cluster standard errors by firm (Cluster Firm). *t*-statistics are included within brackets. The bottom rows report the total number of observations (Observations), the number of treated (Treated Obs.), strong-rights-country treated (Treated Obs. (SR)), and weak-rights-country treated (Treated Obs. (WR)) observations, and the adjusted R-squared (Adj. R²). We run the regressions using only consistently fully owned subsidiaries before the reform. See the caption of Table 3 for variable definitions. Our sample period is 1997 to 2006.

	France (Weak Creditor Rights)			United Kingdom (Strong Creditor Rights)		
	Total Leverage	Short-Term Leverage	Long-Term Leverage	Total Leverage	Short-Term Leverage	Long-Term Leverage
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Overall DID Effect						
Treat × Shock	0.016*** (2.583)	0.010** (2.014)	0.002 (1.292)	-0.037*** (-3.648)	-0.036*** (-3.788)	0.003 (0.415)
Panel B: Creditor-Rights-Specific Conditional DID Effect						
Treat × Shock × Different ^{CRI}	0.017** (2.531)	0.010* (1.930)	0.003* (1.722)	-0.047*** (-3.491)	-0.044*** (-3.616)	0.001 (0.091)
Treat × Shock × Similar ^{CRI}	0.014 (1.120)	0.009 (0.941)	0.000 (0.007)	-0.030** (-2.562)	-0.030*** (-2.763)	0.004 (0.527)
Control Int.	X	X	X	X	X	X
Firm FE	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
Cluster Firm	X	X	X	X	X	X
Observations	16,885	16,885	16,885	17,092	17,092	17,092
Treated Obs.	3,653	3,653	3,653	6,185	6,185	6,185
Treated Obs. (SR)	2,550	2,550	2,550	3,696	3,696	3,696
Treated Obs. (WR)	1,103	1,103	1,103	2,489	2,489	2,489
Adj. R2 (Panel A)	48.8%	47.9%	32.3%	65.7%	59.2%	61.2%
Adj. R2 (Panel B)	48.8%	47.9%	32.3%	65.7%	59.2%	61.2%

Statistical significance levels: *** *p*-value<0.01, ** *p*-value<0.05, * *p*-value<0.10.

Table 5. The Debt-Financing Effects in the EU-12: Strong-Vs.-Weak Creditor Rights

In this table, we report the results from panel regressions of total debt financing on an interaction between a dummy variable equal to one for foreign subsidiaries and else zero (Treat) and a dummy variable equal to one starting from 2002 and else zero (Shock), two triple interactions between Treat, Shock, and a dummy variable equal to one if a foreign subsidiary's parent is located in a country with a La Porta et al. (1998)-Djankov et al. (2007) creditor rights index value at least two points higher (Up^{CRI}) or lower ($Down^{CRI}$) than its host country (see Table 2 for the index values), control variables, and fixed effects run on various subsidiary country sets. We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. The control variables are the natural log of size, company age, the natural log of the number of employees, profitability, fixed assets, and cash holdings. For each control, we interact its pre-reform average by firm with dummy variables equal to one for one specific year and else zero and equal to one for one specific country and else zero (Control Int.). We use both static firm (Firm FE) and year (Time FE) fixed effects. We cluster standard errors by firm (Cluster Firm). The standard numbers are parameter estimates, while those in square brackets are t -statistics. The bottom rows report the number of observations (Observations), the number of treated (Treated Obs.), strong-rights-country treated (Treated Obs. (SR)), and weak-rights-country treated (Treated Obs. (WR)) observations, and the adjusted R-squared (Adj. R²). We run the regressions using only consistently fully owned subsidiaries before the reform. See the caption of Table 3 for variable definitions. Our sample period is 1997 to 2006.

	Country Sample			
	France & UK	France, Italy, Germany, Spain & UK	All countries exc. France & UK	All Countries
	(1)	(2)	(3)	(4)
Treat $\times$ Shock $\times$ Up ^{CRI}	0.028** (2.334)	0.027*** (2.910)	0.011 (1.619)	0.019*** (3.112)
Treat $\times$ Shock $\times$ Down ^{CRI}	-0.032** (-2.238)	-0.033*** (-3.003)	-0.021** (-2.330)	-0.029*** (-3.266)
Treat $\times$ Shock	-0.017 (-1.570)	-0.014* (-1.818)	-0.011** (-2.502)	-0.013*** (-2.674)
Control Int.	X	X	X	X
Firm FE	X	X	X	X
Time FE	X	X	X	X
Cluster Firm	X	X	X	X
Observations	33,977	52,553	61,958	95,935
Treated Obs.	9,838	16,118	13,879	23,717
Treated Obs. (SR)	3,248	3,832	2,634	5,882
Treated Obs. (WR)	3,154	4,812	2,726	5,880
Adj. R ²	74.9%	72.5%	67.4%	73.9%

Statistical significance levels: *** p -value<0.01, ** p -value<0.05, * p -value<0.10.

Table 6. The Effects of the Creditor-Rights Subcomponents

In this table, we report the results from panel regressions of total debt financing on an interaction between a dummy variable equal to one for foreign subsidiaries and else zero (Treat) and a dummy variable equal to one starting from 2002 and else zero (Shock), a set of triple interactions between Treat, Shock, and a dummy variable equal to one if a foreign subsidiary's parent country has some legal feature but its host country has not and else zero (On), a set of triple interactions between Treat, Shock, and a dummy variable equal to one if a foreign subsidiary's host country has some legal feature but its parent country has not and else zero (Off), control variables, and fixed effects run on the whole EU sample. The legal features are (i) secured creditors are paid first (SPF); creditor consent is required to file for reorganization (CC); managers do not stay in place (MR); and no automatic stay of assets (NOS). We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. The control variables are the natural log of size, company age, the natural log of the number of employees, profitability, fixed assets, and cash holdings. For each control, we interact its pre-reform average by firm with dummy variables equal to one for one specific year and else zero and equal to one for one specific country and else zero (Control Int.). We use both static firm (Firm FE) and year (Time FE) fixed effects. We cluster standard errors by firm (Cluster Firm). The standard numbers are parameter estimates, while those in square brackets are *t*-statistics. The bottom rows also report the number of observations (Observations), the number of treated (Treated Obs.), switched-on treated (Treated Obs. (On)), and switched-off treated (Treated Obs. (Off)) observations, and the adjusted R-squared (Adj. R²). We run the regressions using only consistently fully owned subsidiaries before the reform. See the caption of Table 3 for variable definitions. Our sample period is 1997 to 2006.

	Creditor Rights Index Subcomponent					
	Secured Creditors Paid First (SPF)	Creditor Consent to Reor- ganize (CC)	Mana- gers Do Not Stay in Place (MR)	No Auto. Stay (NOS)	SPF, CC, and NOS	All Toge- ther
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Shock × On ^{SPF}	0.036*** (4.783)				0.026*** (3.213)	0.027*** (3.254)
Treat × Shock × Off ^{SPF}	-0.006 (-0.710)				0.013 (1.316)	0.014 (1.346)
Treat × Shock × On ^{CC}		0.012* (1.946)			-0.004 (-0.560)	-0.014* (-1.701)
Treat × Shock × Off ^{CC}		-0.027*** (-3.312)			-0.025*** (-2.579)	-0.020* (-1.896)
Treat × Shock × On ^{MR}			0.016*** (2.576)			0.015** (2.012)
Treat × Shock × Off ^{MR}			-0.020** (-2.243)			-0.005 (-0.498)
Treat × Shock × On ^{NOS}				0.010* (1.806)	0.005 (0.900)	0.009 (1.433)
Treat × Shock × Off ^{NOS}				-0.029*** (-2.975)	-0.019* (-1.873)	-0.019* (-1.867)
Treat × Shock	-0.020*** (-4.551)	-0.010** (-2.041)	-0.012** (-2.541)	-0.009** (-2.199)	-0.010* (-1.874)	-0.013** (-2.005)

(continued on next page)

Table 6. The Effects of the Creditor-Rights Subcomponents (cont.)

	Creditor Rights Index Subcomponent					
	Secured Creditors Paid First (SPF)	Creditor Consent to Reor- ganize (CC)	Mana- gers Do Not Stay in Place (MR)	No Auto. Stay (NOS)	SPF, CC, and NOS	All Toge- ther
	(1)	(2)	(3)	(4)	(5)	(6)
Control Int.	X	X	X	X	X	X
Firm FE	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
Cluster Firm	X	X	X	X	X	X
Observations	95,935	95,935	95,935	95,935	95,935	95,935
Treated Obs.	23,717	23,717	23,717	23,717	43,109	47,350
Treated Obs. (ON)	3,653	7,791	6,780	5,594	11,264	12,544
Treated Obs (OFF)	4,357	5,647	6,000	8,396	11,634	13,417
Adj. R2	73.9%	73.9%	73.9%	73.9%	73.9%	73.9%

Statistical significance levels: *** p -value<0.01, ** p -value<0.05, * p -value<0.10.

Table 7. The Effects of Other Creditor Rights Dimensions

In this table, we report the results from panel regressions of total debt financing on an interaction between a dummy variable equal to one for foreign subsidiaries and else zero (Treat) and a dummy variable equal to one starting from 2002 and else zero (Shock), two triple interactions between Treat, Shock, and a dummy variable equal to one if a foreign subsidiary's parent is located in a country with an at least half-a-standard-deviation higher (Up^{OCR}) or lower ($Down^{OCR}$) value for some alternative creditor rights measure, sometimes two triple interactions between Treat, Shock, and a dummy variable equal to one if a foreign subsidiary's parent is located in a country with a La Porta et al. (1998)-Djankov et al. (2007) creditor rights index value at least two points higher (Up^{CRI}) or lower ($Down^{CRI}$) than its host country (see Table 2 for the index values), control variables, and fixed effects run on the whole EU sample. The alternative measures capture whether (i) a firm must offer proof that it is insolvent to file; (ii) the managers or trustees in charge of the insolvent firm can prematurely terminate contracts; (iii) the percentage of creditor value required to approve a reorganization plan; and (iv) managers are not discharged upon closure of the proceedings. The control variables are the natural log of size, company age, the natural log of the number of employees, profitability, fixed assets, and cash holdings. For each control, we interact its pre-reform average by firm with dummy variables equal to one for one specific year and else zero and equal to one for one specific country and else zero (Control Int.). We use both static firm (Firm FE) and year (Time FE) fixed effects. We cluster standard errors by firm (Cluster Firm). The standard numbers are parameter estimates, while those in square brackets are t -statistics. The bottom rows report the total number of observations (Observations), the number of treated (Treated Obs.), strong-other-rights-country treated (Treated Obs. (SOR)), and weak-other-rights-country treated (Treated Obs. (WOR)) observations, and the adjusted R-squared (Adj. R^2). We run the regressions using only consistently fully owned subsidiaries before the reform. See the caption of Table 3 for variable definitions. Our sample period is 1997 to 2006.

	Other Insolvency Law Characteristics							
	Proof of Insolvency Required		Ability to Terminate Contracts		% Creditor Value Req. to Approve Reorganizing		No Discharge Debtor Upon Closure	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat $\times$ Shock $\times$ Up^{OCR}	0.007 (0.935)	0.013 (1.628)	0.039*** (5.815)	0.025*** (3.717)	0.031*** (3.730)	0.022*** (2.602)	-0.001 (-0.119)	-0.002 (-0.214)
Treat $\times$ Shock $\times$ $Down^{OCR}$	0.033*** (4.818)	0.023*** (3.216)	0.002 (0.224)	0.019* (1.905)	-0.004 (-0.523)	0.007 (0.934)	-0.033*** (-3.549)	-0.021** (-2.118)
Treat $\times$ Shock $\times$ Up^{CRI}		0.016** (2.569)		0.012* (1.912)		0.012 (1.629)		0.010 (1.506)
Treat $\times$ Shock $\times$ $Down^{CRI}$		-0.026*** (-2.940)		-0.030*** (-3.190)		-0.028*** (-3.291)		-0.025*** (-2.804)
Treat $\times$ Shock	-0.024*** (-5.052)	-0.022*** (-3.654)	-0.025*** (-5.284)	-0.021*** (-3.697)	-0.021*** (-3.321)	-0.020*** (-2.753)	0.002 (0.215)	-0.000 (-0.045)
Control Int.	X	X	X	X	X	X	X	X
Firm FE	X	X	X	X	X	X	X	X
Time FE	X	X	X	X	X	X	X	X
Cluster Firm	X	X	X	X	X	X	X	X
Observations	95,935	95,935	95,935	95,935	95,935	95,935	95,935	95,935
Treated Obs.	23,717	23,717	23,717	23,717	23,717	23,717	23,717	23,717
Treated Obs. (SOR)	6,870	6,870	5,564	5,564	6,268	6,268	10,779	10,779
Treated Obs. (WOR)	4,869	4,869	4,222	4,222	10,030	10,030	10,103	10,103
Adj. R^2	73.9%	73.9%	73.9%	73.9%	73.9%	73.9%	73.9%	73.9%

Statistical significance levels: *** p -value<0.01, ** p -value<0.05, * p -value<0.10.

Table 8. Creditor Rights-Versus-Enforcement

In this table, we report the results from panel regressions of total debt financing on an interaction between a dummy variable equal to one for foreign subsidiaries and else zero (Treat) and a dummy variable equal to one starting from 2002 and else zero (Shock), two triple interactions between Treat, Shock, and a dummy variable equal to one if a foreign subsidiary's parent is located in a country with a La Porta et al. (1998)-Djankov et al. (2007) creditor rights index value at least two points higher (Up^{CRI}) or lower ($Down^{CRI}$) than its host country (see Table 2 for the index values), control variables, and fixed effects run on the whole EU sample. In Panel A, we add to the explanatory variables two triple interactions between Treat, Shock, and a dummy variable equal to one if a foreign subsidiary's parent is located in a country with an at least half-a-standard-deviation higher ($Up^{Enforce}$) or lower ($Down^{Enforce}$) enforcement variable value than its host, whereas in Panel B we rerun the regressions on the subsample of subsidiaries for which both the host and parent country have an enforcement variable value above or below the median. The enforcement variables are the total duration for enforcing a business dispute in the country ("Court Time"), a measure of the country's legal formalism ("Legal Formalism"), and the efficiency of a country's judicial system ("Judicial Efficiency"). The control variables are the natural log of size, company age, the natural log of the number of employees, profitability, fixed assets, and cash holdings. For each control, we interact its pre-reform average by firm with dummy variables equal to one for one specific year and else zero and equal to one for one specific country and else zero (Control Int.). We use both static firm (Firm FE) and year (Time FE) fixed effects. We cluster standard errors by firm (Cluster Firm). The standard numbers are parameter estimates, while those in square brackets are *t*-statistics. The bottom rows report the total number of observations (Observations), the number of treated (Treated Obs.), strong-rights-country treated (Treated Obs. (SR)), weak-rights-country treated (Treated Obs. (WR)), high-enforcement treated (Treated Obs. (HE)), and low-enforcement treated (Treated Obs. (LE)) observations, and the adjusted R-squared (Adj. R²). We run the regressions using only consistently fully owned subsidiaries before the reform. See the caption of Table 3 for variable definitions. Our sample period is 1997 to 2006.

Panel A: The Separate Effects of Creditor Rights and Enforcement			
	Enforcement Variable		
	Court Time	Legal Formalism	Judicial Efficiency
	(2)	(3)	(1)
Treat × Shock × Up^{CRI}	0.018*** (2.849)	0.018*** (2.939)	0.013** (2.030)
Treat × Shock × $Down^{CRI}$	-0.028*** (-3.239)	-0.028*** (-3.181)	-0.028*** (-3.260)
Treat × Shock × $Up^{Enforce}$	-0.001 (-0.081)	0.005 (0.779)	0.023*** (3.010)
Treat × Shock × $Down^{Enforce}$	0.011 (1.252)	-0.008 (-0.984)	0.006 (0.698)
Treat × Shock	-0.015** (-2.147)	-0.011* (-1.707)	-0.021*** (-3.030)
Control Int.	X	X	X
Firm FE	X	X	X
Time FE	X	X	X
Cluster Firm	X	X	X
Observations	95,935	95,935	95,935
Treated Obs	23,717	23,717	23,717
Treated Obs. (SR)	5,882	5,882	5,882
Treated Obs. (WR)	5,880	5,880	5,880
Treated Obs. (HE)	12,541	9,184	10,969
Treated Obs. (LE)	6,034	10,134	7,297
Adj. R ²	73.9%	73.9%	73.9%

(continued on next page)

Table 8. Creditor Rights-Versus-Enforcement (cont.)

Panel B: The Conditional Effect of Enforcement on Creditor Rights						
	Enforcement Variable					
	Court Time		Legal Formalism		Judicial Efficiency	
	Low	High	Low	High	High	Low
	(3)	(4)	(5)	(6)	(1)	(2)
Treat $\times$ Shock $\times$ Up ^{CRI}	0.038*** (2.852)	0.028** (2.420)	0.023* (1.927)	0.012 (0.770)	0.019** (2.023)	0.013 (0.918)
Treat $\times$ Shock $\times$ Down ^{CRI}	-0.011 (-0.734)	-0.050*** (-2.849)	-0.036** (-2.034)	-0.022 (-1.396)	-0.025 (-1.539)	-0.030* (-1.908)
Treat $\times$ Shock	-0.022** (-2.068)	-0.008 (-1.289)	-0.029*** (-2.694)	-0.005 (-0.477)	-0.024*** (-2.702)	-0.004 (-0.314)
Control Int.	X	X	X	X	X	X
Firm FE	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
Cluster Firm	X	X	X	X	X	X
Observations	39,231	43,807	49,040	34,516	50,520	32,927
Treated Obs	7,473	3,347	6,414	4,924	5,370	5,859
Treated Obs. (SR)	2,366	671	1,242	1,207	1,151	1,624
Treated Obs (WR)	1,520	1,283	1,554	1,597	1,033	1,840
Adj. R2	61.6%	77.9%	77.9%	60.2%	78.1%	61.1%

Statistical significance levels: *** p -value<0.01, ** p -value<0.05, * p -value<0.10.

Table 9. The Conditional Role of Bank Strength for the Debt-Financing Effects of the Reform

In this table, we report the results from panel regressions of total debt financing on combinations of a double interaction between a dummy variable equal to one for foreign subsidiaries and else zero (Treat) and a dummy variable equal to one starting from 2002 and else zero (Shock), a triple interaction between Treat, Shock, and a dummy variable equal to one if the pre-2002 strength of a subsidiary's house bank is above the median and else zero (HBC), control variables, and fixed effects run on UK subsidiaries. We run the regressions separately for the subsample of subsidiaries with an above-median house bank strength (columns (1), (4), and (7)), the subsample of those with a below-median strength ((2), (5), and (8)), and the pooled sample ((3), (6), and (9)). We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. The bank strength variables are the 2002 equity-to-assets ratio, the pre-2002 average return on assets (ROA), and the pre-2002 average share of revenue generated through lending. The control variables are the natural log of size, company age, the natural log of the number of employees, profitability, fixed assets, and cash holdings. For each control, we interact its pre-reform average by firm with dummy variables equal to one for one specific year and else zero (Control Int.). We use both static firm (Firm FE) and year (Time FE) fixed effects. We cluster standard errors by firm (Cluster Firm). The standard numbers are parameter estimates, while those in square brackets are *t*-statistics. The bottom rows report the number of observations (Observations), the number of treated (Treated Obs.), and the adjusted R-squared (Adj. R²). We run the regressions using only consistently fully owned subsidiaries before the reform. See the caption of Table 3 for variable definitions. Our sample period is 1997 to 2006.

	Bank Characteristics (BC):								
	Equity-to-Assets			Return-on-Assets			Lending Activity		
	High	Low	Both	High	Low	Both	High	Low	Both
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treat × Shock	-0.021 (-0.973)	-0.066*** (-4.142)	-0.066*** (-4.137)	-0.012 (-0.707)	-0.062*** (-3.959)	-0.062*** (-3.957)	-0.017 (-0.997)	-0.064*** (-3.987)	-0.064*** (-3.986)
Treat × Shock × HBC			0.045* (1.718)			0.050** (2.109)			0.046** (1.968)
Control Int.	X	X	X	X	X	X	X	X	X
Firm FE	X	X	X	X	X	X	X	X	X
Time FE	X	X	X	X	X	X	X	X	X
Cluster Firm	X	X	X	X	X	X	X	X	X
Observations	3,870	6,541	10,411	4,902	6,914	11,816	5,301	6,524	11,825
Treated Obs.	1,049	2,460	3,509	1,449	2,535	3,984	1,561	2,423	3,984
Adj. R ²	67.5%	61.2%	63.6%	68.6%	61.0%	64.2%	67.8%	61.4%	64.3%

Statistical significance levels: *** *p*-value<0.01, ** *p*-value<0.05, * *p*-value<0.10.

Table 10. Creditor Rights-Versus-Culture

In this table, we report the results from panel regressions of total debt financing on an interaction between a dummy variable equal to one for foreign subsidiaries and else zero (Treat) and a dummy variable equal to one starting from 2002 and else zero (Shock), two triple interactions between Treat, Shock, and a dummy variable equal to one if a foreign subsidiary's parent is located in a country with a La Porta et al. (1998)-Djankov et al. (2007) creditor rights index value at least two points higher (Up^{CRI}) or lower ($Down^{CRI}$) than its host country (see Table 2 for the index values), two triple interactions between Treat, Shock, and a dummy variable equal to one if a foreign subsidiary's parent is located in a country with a culture index value at least half-a-standard-deviation higher (Up^{CulVar}) or lower ($Down^{CulVar}$) than the host country and else zero, control variables, and fixed effects run on the whole EU sample. We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. The culture indexes a catholic dummy equal to one if the majority religion in a country is catholicism and else zero and the first five Hofstede cultural indexes, power distance, individualism, masculinity, uncertainty avoidance, and long-term orientation. The control variables are the natural log of size, company age, the natural log of the number of employees, profitability, fixed assets, and cash holdings. For each control, we interact its pre-reform average by firm with dummy variables equal to one for one specific year and else zero and equal to one for one specific country and else zero (Control Int.). We use both static firm (Firm FE) and year (Time FE) fixed effects. We cluster standard errors by firm (Cluster Firm). The standard numbers are parameter estimates, while those in square brackets are t -statistics. The bottom rows report the number of observations (Observations), the number of treated (Treated Obs.), high-culture-value-treated (Treated Obs. (SR)), and low-culture-value-treated (Treated Obs. (SR)) observations, and the adjusted R-squared (Adj. R²). We run the regressions using only consistently fully owned subsidiaries before the reform. See the caption of Table 3 for variable definitions. Our sample period is 1997 to 2006.

	Culture Variable					
	Catholic	Power Distance	Individualism	Masculinity	Uncertainty Avoidance	Long-term Orientation
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Shock × Up^{CRI}	0.017*** (2.784)	0.015** (2.316)	0.019*** (2.714)	0.020*** (3.310)	0.018*** (2.836)	0.021*** (3.224)
Treat × Shock × $Down^{CRI}$	-0.025*** (-2.881)	-0.034*** (-3.730)	-0.024*** (-2.589)	-0.029*** (-3.154)	-0.024*** (-2.649)	-0.028*** (-2.843)
Treat × Shock × Up^{CulVar}	-0.013 (-1.639)	0.012 (1.494)	-0.025** (-2.233)	-0.002 (-0.214)	0.010 (0.791)	0.003 (0.368)
Treat × Shock × $Down^{CulVar}$	0.009 (1.370)	0.013* (1.656)	-0.035*** (-3.203)	-0.001 (-0.059)	0.021* (1.685)	0.010 (1.086)
Treat × Shock	-0.011** (-1.961)	-0.018*** (-2.911)	0.015 (1.609)	-0.012 (-1.273)	-0.027** (-2.052)	-0.018** (-1.980)
Control Int.	X	X	X	X	X	X
Firm FE	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
Cluster Firm	X	X	X	X	X	X
Observations	95,935	95,935	95,935	95,935	95,935	95,935
Treated Obs	23,717	23,717	23,717	23,717	23,717	23,717
Treated Obs. (HCV)	6,690	6,818	10,597	8,484	10,965	13,940
Treated Obs (LCV)	6,230	9,392	12,062	11,770	10,468	6,243
Adj. R ²	73.9%	73.9%	73.9%	73.9%	73.9%	73.9%

Statistical significance levels: *** p -value<0.01, ** p -value<0.05, * p -value<0.10.

Table 11. Creditor Rights-Versus-Socialistic Tendencies

In this table, we report the results from panel regressions of total debt financing on an interaction between a dummy variable equal to one for foreign subsidiaries and else zero (Treat) and a dummy variable equal to one starting from 2002 and else zero (Shock), two triple interactions between Treat, Shock, and a dummy variable equal to one if a foreign subsidiary's parent is located in a country with a La Porta et al. (1998)-Djankov et al. (2007) creditor rights index value at least two points higher (Up^{CRI}) or lower ($Down^{CRI}$) than its host country (see Table 2 for the index values), two triple interactions between Treat, Shock, and a dummy variable equal to one if a foreign subsidiary's parent is located in a country either with proportional election system or a politics/labor protection value at least half-a-standard-deviation higher ($Up^{SocialVar}$) or lower ($Down^{SocialVar}$) than the host country and else zero, control variables, and fixed effects run on the whole EU sample. We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. The proxies for a more socialistic political system are: a dummy variable equal to one if a country has a proportional election system and else zero ("Proportional Election System"); the fraction of years which the country was governed by a left-wing party ("Left Power"); the fraction of the unionized work population in the country ("Union Density"); and an index capturing the strength of the country's labor laws ("Strength Labor Laws"). The control variables are the natural log of size, company age, the natural log of the number of employees, profitability, fixed assets, and cash holdings. For each control, we interact its pre-reform average by firm with dummy variables equal to one for one specific year and else zero and equal to one for one specific country and else zero (Control Int.). We use both static firm (Firm FE) and year (Time FE) fixed effects. We cluster standard errors by firm (Cluster Firm). *t*-statistics are included in the squared brackets. The bottom rows report the number of observations (Observations), the number of treated (Treated Obs.), high-socialistic-tendency-value-treated (Treated Obs. (SR)), and low-socialistic-tendency-value-treated (Treated Obs. (SR)) observations, and the adjusted R-squared (Adj. R²). We run the regressions using only consistently fully owned subsidiaries before the reform. See the caption of Table 3 for variable definitions. Our sample period is 1997 to 2006.

	Politics		Labor Protection	
	Proportional Election System	Left Power	Union Density	Strength Labor Laws
	(1)	(2)	(3)	(4)
Treat × Shock × Up^{CRI}	0.019*** (3.109)	0.022*** (3.358)	0.012* (1.822)	0.013** (2.126)
Treat × Shock × $Down^{CRI}$	-0.028*** (-3.251)	-0.037*** (-3.836)	-0.036*** (-4.019)	-0.025*** (-2.738)
Treat × Shock × $Up^{SocialVar}$	-0.003 (-0.491)	0.017* (1.781)	0.017* (1.879)	-0.014 (-1.632)
Treat × Shock × $Down^{SocialVar}$	0.000 (0.025)	-0.001 (0.104)	0.018** (2.034)	0.001 (0.153)
Treat × Shock	-0.011** (-2.127)	-0.015** (-2.379)	-0.023*** (-3.097)	-0.007 (-1.436)
Control Int.	X	X	X	X
Firm FE	X	X	X	X
Time FE	X	X	X	X
Cluster Firm	X	X	X	X
Observations	95,935	95,935	95,935	95,935
Treated Obs	23,717	23,717	23,717	23,717
Treated Obs. (HSV)	11,946	6,362	10,490	9,858
Treated Obs (LSV)	7,377	7,449	8,610	4,581
Adj. R ²	73.9%	73.9%	73.9%	73.9%

Statistical significance levels: *** p -value<0.01, ** p -value<0.05, * p -value<0.10.

Table 12. Placebo Tests

In this table, we report the results from placebo panel regressions of total debt financing on an interaction between a dummy variable equal to one for foreign subsidiaries and else zero (Treat) and a dummy variable equal to one starting from 2002 and else zero (Shock), two triple interactions between Treat, Shock, and a dummy variable equal to one if a foreign subsidiary's parent is located in a country with a La Porta et al. (1998)-Djankov et al. (2007) creditor rights index value at least two points higher (Up^{CRI}) or lower (Down^{CRI}) than its host country (see Table 2 for the index values), control variables, and fixed effects run on the set of French and UK (Panel A) and whole-EU (Panel B) subsidiaries. In column (1), we repeat our results from columns (1) and (4) of Table 5 for the reader's convenience. In column (2), we consider only foreign subsidiaries with parents in Denmark and Norway. In column (3), we consider non fully (rather than fully) owned subsidiaries, where the non fully owned subsidiaries are only up to 80% owned by their parents. In column (4), we randomly reshuffle the foreign subsidiaries' parent countries. The control variables are the natural log of size, company age, the natural log of the number of employees, profitability, fixed assets, and cash holdings. For each control, we interact its pre-reform average by firm with dummy variables equal to one for one specific year and else zero and equal to one for one specific country and else zero (Control Int.). We use both static firm (Firm FE) and year (Time FE) fixed effects. We cluster standard errors by firm (Cluster Firm). The standard numbers are parameter estimates, while those in square brackets are *t*-statistics. The bottom rows report the number of observations (Observations), the number of treated (Treated Obs.), strong-rights-country treated (Treated Obs. (SR)), and weak-rights-country treated (Treated Obs. (WR)) observations, and the adjusted R-squared (Adj. R²). See the caption of Table 3 for variable definitions. Our sample period is 1997 to 2006.

	Main Sample (Repeated)	Shifted to Non-Treated Countries (DNK/NOR)	Partially Owned Subsidiaries	Random Reshuffling Treated Firms
	(1)	(2)	(3)	(4)
Panel A: French and UK Subsidiaries				
Treat × Shock × Up ^{CRI}	0.028** (2.334)	0.052 (1.298)	−0.001 (−0.065)	0.002 (0.144)
Treat × Shock × Down ^{CRI}	−0.032** (−2.238)	0.038 (0.618)	−0.015 (−0.549)	−0.009 (−0.706)
Treat × Shock	−0.017 (−1.570)	−0.046 (−1.254)	0.003 (0.142)	−0.012 (−1.238)
Control Int.	X	X	X	X
Firm FE	X	X	X	X
Time FE	X	X	X	X
Cluster Firm	X	X	X	X
Observations	33,977	25,233	40,079	33,977
Treated Obs.	9,838	776	4,916	9,838
Treated Obs. (SR)	3,248	180	3,563	3,155
Treated Obs. (WR)	3,154	192	692	3,486
Adj. R ²	74.9%	41.6%	67.1%	74.8%

(continued on next page)

Table 12. Placebo Tests (cont.)

	Main Sample (Repeated)	Shifted to Non-Treated Countries (DNK/NOR)	Partially Owned Subsidiaries	Random Reshuffling Treated Firms
	(1)	(2)	(3)	(4)
Panel B: All EU Subsidiaries				
Treat $\times$ Shock $\times$ Up ^{CRI}	0.019*** (3.112)	0.029 (1.467)	0.002 (0.161)	−0.003 (−0.371)
Treat $\times$ Shock $\times$ Down ^{CRI}	−0.029*** (−3.266)	0.016 (0.312)	−0.008 (−0.551)	−0.006 (−0.750)
Treat $\times$ Shock	−0.013*** (−2.674)	−0.028 (−1.563)	−0.003 (−0.368)	−0.012*** (−2.585)
Control Int.	X	X	X	X
Firm FE	X	X	X	X
Time FE	X	X	X	X
Cluster Firm	X	X	X	X
Observations	95,935	74,992	72,211	95,935
Treated Obs.	23,717	1,879	9,729	23,717
Treated Obs. (SR)	5,882	611	4,349	5,314
Treated Obs. (WR)	5,880	196	1,781	5,935
Adj. R2	73.9%	36.1%	70.2%	73.9%

Statistical significance levels: *** p -value<0.01, ** p -value<0.05, * p -value<0.10.

Table 13. Parallel Trend Tests

In this table, we report the results from parallel trend tests for the set of French and UK (columns (1) to (4)) and whole-EU ((5) to (9)) subsidiaries. In particular, we show the average difference in the annual change in several analysis variables over each pre-reform year (1998-2001) across domestic subsidiaries and foreign subsidiaries with parents in countries with a La Porta et al. (1998)-Djankov et al. (2007) creditor rights score within one point of those of their host countries (Panel A), foreign subsidiaries with parents in countries with an at least two points higher score and those with parents in countries within one point (Panel B), and foreign subsidiaries with parents in countries with an at least two points lower score and those with parents in countries within one point (Panel C). The analysis variables are total leverage, the natural log of size, the natural log of the number of employees, profitability, fixed assets, and cash holdings. The standard numbers are the averages, while those in square brackets are their (robust) t -statistics. We run the tests using only consistently fully owned subsidiaries before the reform. See the caption of Table 3 for variable definitions.

	Difference in Annual Change Between Treated and Controls							
	France & UK				All Countries			
	1998	1999	2000	2001	1998	1999	2000	2001
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Domestic-Versus-Foreign Subsidiaries								
TotalLeverage	0.051 (1.207)	-0.001 (-0.017)	0.058 (1.446)	-0.086 (-2.261)	-0.002 (-0.085)	-0.021 (-0.880)	-0.002 (-0.087)	-0.084 (-3.592)
LNSize	-0.119 (-3.110)	-0.026 (-0.754)	-0.130 (-3.239)	-0.069 (-2.060)	-0.016 (-0.714)	-0.015 (-0.691)	-0.047 (-2.118)	-0.014 (-0.707)
LNEmployees	0.030 (0.624)	-0.028 (-0.787)	-0.029 (-0.663)	-0.034 (-0.993)	0.013 (0.556)	-0.024 (-1.139)	0.031 (1.228)	0.059 (2.370)
Profitability	-0.111 (-2.509)	-0.053 (-1.333)	-0.020 (-0.474)	-0.016 (-0.414)	0.024 (0.965)	-0.049 (-2.242)	-0.031 (-1.425)	0.021 (1.017)
FxedAssets	-0.016 (-0.358)	0.011 (0.318)	-0.007 (-0.190)	0.011 (0.290)	-0.028 (-1.082)	-0.014 (-0.691)	0.006 (0.267)	0.015 (0.747)
CashHoldings	-0.016 (-0.314)	0.021 (0.526)	-0.042 (-1.067)	0.102 (2.496)	0.004 (0.130)	0.004 (0.157)	-0.014 (-0.641)	0.094 (4.155)
Panel B: Strong-Versus-Similar Creditor-Rights Parent-Country Foreign Subsidiaries								
TotalLeverage	-0.022 (-0.285)	-0.019 (-0.258)	-0.133 (-1.914)	0.046 (0.694)	-0.100 (-1.997)	-0.037 (-0.899)	-0.014 (-0.349)	-0.031 (-0.723)
LNSize	-0.033 (-0.393)	-0.279 (-3.851)	0.402 (4.330)	0.176 (2.329)	-0.002 (-0.034)	-0.086 (-1.812)	0.151 (2.912)	0.066 (1.395)
LNEmployees	0.020 (0.180)	-0.052 (-0.770)	0.200 (2.301)	0.144 (2.141)	-0.077 (-1.381)	-0.026 (-0.591)	0.017 (0.343)	-0.039 (-0.768)
Profitability	0.013 (0.149)	0.222 (3.100)	0.073 (0.975)	-0.098 (-1.382)	-0.007 (-0.116)	0.120 (2.425)	0.018 (0.365)	-0.049 (-1.092)
FxedAssets	-0.070 (-0.825)	0.020 (0.284)	0.064 (0.820)	0.056 (0.761)	0.044 (0.681)	0.050 (1.100)	0.022 (0.466)	0.010 (0.223)
CashHoldings	0.010 (0.109)	0.100 (1.285)	-0.106 (-1.413)	-0.042 (-0.532)	-0.036 (-0.648)	0.078 (1.629)	-0.064 (-1.335)	-0.007 (-0.135)

(continued on next page)

Table 13. Parallel Trend Test Results (cont.)

Difference in Annual Change Between Treated and Controls								
	France & UK				All Countries			
	1998	1999	2000	2001	1998	1999	2000	2001
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel C: Weak-Versus-Similar Creditor-Rights Parent-Country Foreign Subsidiaries								
TotalLeverage	0.040	0.131	−0.045	0.002	−0.028	0.082	−0.001	−0.017
	(0.438)	(1.426)	(−0.509)	(0.018)	(−0.508)	(1.562)	(−0.016)	(−0.334)
LNSize	−0.129	0.032	0.016	0.046	−0.065	0.046	0.052	0.022
	(−1.851)	(0.405)	(0.198)	(0.648)	(−1.392)	(1.004)	(1.099)	(0.491)
LNEmployees	−0.071	−0.039	−0.097	0.052	−0.070	−0.014	0.005	0.018
	(−0.712)	(−0.534)	(−0.914)	(0.785)	(−1.418)	(−0.323)	(0.088)	(0.323)
Profitability	−0.101	0.167	0.113	−0.091	−0.001	0.066	−0.002	0.040
	(−1.063)	(1.960)	(1.202)	(−1.122)	(−0.022)	(1.340)	(−0.038)	(0.843)
FxedAssets	−0.168	0.037	0.004	−0.022	−0.110	0.068	0.001	0.022
	(−1.639)	(0.475)	(0.051)	(−0.251)	(−1.988)	(1.479)	(0.028)	(0.460)
CashHoldings	0.125	−0.052	−0.015	0.028	0.048	0.000	0.039	0.055
	(1.073)	(−0.639)	(−0.179)	(0.316)	(0.762)	(0.006)	(0.862)	(1.056)

Table 14. The Debt-Financing Effects of the Reform: PS-Matching

In this table, we report the results from panel regressions of total debt financing on an interaction between a dummy variable equal to one for foreign subsidiaries and else zero (Treat) and a dummy variable equal to one starting from 2002 and else zero (Shock), two triple interactions between Treat, Shock, and a dummy variable equal to one if a foreign subsidiary's parent is located in a country with a La Porta et al. (1998)-Djankov et al. (2007) creditor rights index value at least two points higher (Up^{CRI}) or lower ($Down^{CRI}$) than its host country (see Table 2 for the index values), control variables, and fixed effects run on various propensity-score (PS) matched subsidiary country sets. We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. To PS-match, we run a country-specific cross-sectional logit regression of Treat on the pre-reform averages of the control variables and then assign to each treated firm that control firm with the closest fitted value within a caliper of 0.005. The control variables are the natural log of size, company age, the natural log of the number of employees, profitability, fixed assets, and cash holdings. For each control, we interact its pre-reform average by firm with dummy variables equal to one for one specific year and else zero and equal to one for one specific country and else zero (Control Int.). We use both static firm (Firm FE) and year (Time FE) fixed effects. We cluster standard errors by firm (Cluster Firm). The standard numbers are parameter estimates, while those in square brackets are t -statistics. The bottom rows report the number of observations (Observations), the number of treated (Treated Obs.), strong-rights-country treated (Treated Obs. (SR)), and weak-rights-country treated (Treated Obs. (WR)) observations, and the adjusted R-squared (Adj. R²). We run the regressions using only consistently fully owned subsidiaries before the reform. See the caption of Table 3 for variable definitions. Our sample period is 1997 to 2006.

	Country Sample			
	France & UK	France, Italy, Germany & UK	All countries exc. France & UK	All Countries
	(1)	(2)	(3)	(4)
Treat $\times$ Shock $\times$ Up ^{CRI}	0.040*** (3.010)	0.034*** (3.306)	0.010 (1.379)	0.024*** (3.409)
Treat $\times$ Shock $\times$ Down ^{CRI}	-0.029* (-1.928)	-0.029** (-2.542)	-0.019* (-1.905)	-0.027*** (-2.864)
Treat $\times$ Shock	-0.030*** (-2.581)	-0.021** (-2.477)	-0.016*** (-2.932)	-0.021*** (-3.637)
Control Int.	X	X	X	X
Firm FE	X	X	X	X
Time FE	X	X	X	X
Cluster Firm	X	X	X	X
Observations	17,042	27,797	20,490	37,532
Treated Obs.	8,729	14,127	10,222	18,951
Treated Obs. (SR)	2,986	3,491	1,947	4,933
Treated Obs. (WR)	2,680	4,033	2,012	4,692
Adj. R ²	72.2%	71.1%	66.0%	73.2%

Statistical significance levels: *** p -value<0.01, ** p -value<0.05, * p -value<0.10.