Does Creditor Protection Matter for Corporate Borrowing? Evidence from the European Insolvency Regulation*

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Abstract

The 2002 European Insolvency Regulation (EIR) unexpectedly reassigned the insolvency jurisdiction of fully-owned foreign subsidiaries in the European Union, exposing some but not all firms in the same environment to creditor rights changes. Using triple-difference designs, we provide causal evidence that shifts toward stronger (weaker) creditor rights raise (lower) debt-taking. Most prominent creditor-right dimensions matter but often asymmetrically. The subsidiaries change their debt levels to adjust their financing mixes but not real policies, and the debt changes also arise within industry-location-time triplets, supporting a supply-side effect. The U.K. subsample reveals that the changes come from bank (not group or leasing) debt and are conditioned by bank traits, supporting a bank-induced effect. Distress risk and enforcement do not strongly condition our results, casting doubt on other studies exploiting legal reforms. Finally, our evidence does not arise through cultural, religious, or political factors.

KEYWORDS: Creditor rights, corporate borrowing, causal inference, EU regulations.
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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] Policymakers have long treated the strength of creditor protection as a key lever for the efficient functioning of credit markets, and the recent wave of insolvency law reforms suggest that this lever remains an active area of legislative attention.[2] Yet, while many reforms rest on the presumption that better-protected creditors lend more and on better terms, whether and how creditor rights affect corporate debt remain widely-debated questions in the law and finance literature.[3] Cross-country studies often do not 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)). Conversely, intra-country studies exploiting plausibly exogenous shocks to specific creditor rights coming from legal reforms provide only partial and often conflicting evidence (e.g., Rodano et al. (2016); Closset and Urban (2019); Schoenherr and Starmans (2022); Closset et al. (2023); Ghitti et al. (2025)).

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 some but not all firms from one creditor rights regime to another ("treated firms") whilst keeping all other economic and institutional features constant and then comparing their debt-taking decisions with those of the non-reallocated firms ("control firms").

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; Wood

[1] Source: https://www.enterprisesurveys.org/en/data/exploretopics/finance?subGroup=-1subtopic=38.

[2] For instance, the recently adopted EU Directive 2026/799 strengthens creditors' formal role in reorganization proceedings through EU-wide creditors' committees and harmonizes, among others, the avoidance actions used by creditors to "claw back" some of the value of fraudulent transactions that took place shortly before insolvency.

[3] The 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 with better terms (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 and dampening credit demand (e.g., Acharya and Subramanian (2009) and Vig (2013)).

(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, shortly after the EIR became effective, European courts ruled that the COMI should instead be located in the country where the firm's head-office functions are carried out (Wessels (2006)).[4]

This interpretation had important implications for multinational firms. Since 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. Thus, these subsidiaries became subject to the insolvency jurisdiction of courts operating under a possibly different creditor rights regime.

We exploit the 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 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 triple-difference design (DIDID) first compares the debt-taking behavior of foreign subsidiaries with that of domestic subsidiaries before and after the EIR became effective. 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 explore the effects from potential cross-country variations in creditor rights regimes while holding constant the economic environment in which our sample firms operate. We next compare foreign subsidiaries with parents in countries with stronger or weaker creditor rights regimes relative to the host country to foreign subsidiaries whose parent and host countries have similar creditor rights regimes before and after the EIR. This second contrast allows us to isolate the effects from changes in creditor protection from possibly other effects due to being reassigned to a foreign insolvency jurisdiction (*creditor-rights-unrelated effect*).

Studying the most comprehensive sample of signatory countries to the EIR ("EU-12"sample), we show that subsidiaries whose insolvency jurisdiction shifts toward stronger creditor rights regimes raise their debt-taking by 1.9 percent points (t-statistic: 3.11) from before to after the EIR. Conversely, those whose jurisdiction shifts toward weaker regimes lower theirs by 2.9 percent points (t-statistic: –3.27). Critically, the two estimates (which are net of the creditor-rights-unrelated effect) are not only statistically but also economically important, representing 12% and 18% of pre-EIR average leverage, respectively. Using alternative samples, we further reveal that our results are stronger for those samples with more extreme creditor rights variations (*dosage effect*). Interestingly, the creditor-rights-unrelated effect is statistically and economically weaker than the creditor-rights-related effect, with it representing a debt decrease of about 8% relative to pre-EIR leverage. Overall, our initial evidence supports the supply-side view of creditor rights over the demand-side view.

[4]The 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 IA.1 provides additional examples.

We further explore the effect of the reform on debt terms, namely, maturity and cost of debt. We find that our baseline results are mainly driven by short-term debt. Relying on an accounting-based proxy for the cost of debt, we further find that, consistent with the supply-side view of creditor rights, the borrowing costs of subsidiaries whose insolvency jurisdiction shifts to a weaker regime rise, while those of the other shifted (i.e., foreign) subsidiaries do not materially change.

We finally study which and how well-known dimensions of creditor protection affect corporate debt-taking. While most existing studies concentrate on reforms affecting only a single or, at most, a few dimensions, our setting crucially enables us to evaluate the impact of *all* those making up the CR index.[5] Our evidence suggests that all CR-index dimensions matter for corporate borrowing, with the priority of secured creditors playing the most important role. We also document substantial asymmetries in these effects: Granting certain creditor rights leads to sizable increases in debt-taking, whereas removing those same rights generates much weaker decreases, and vice versa. For instance, moving secured creditors to the top of the bankruptcy ladder of priorities raises total debt by 3.6 percentage points (t-statistic: 4.78), whereas moving them down from the top rung reduces that debt only insignificantly. We also construct a pseudo creditor-rights index maximizing our benchmark regression's ability to explain the observed debt changes in our sample data. Strikingly, the pseudo index has an about 74% correlation with the CR index, refuting concerns that the CR index is overly simplistic (e.g., Albertus et al. (2026) and Gurrea-Martínez (2026)).

In the second part of our empirical analysis, we shed more light on the mechanism behind our main results. While it seems unlikely that those results are driven by demand rather than supply-side effects (e.g., no existing theory predicts that stronger creditor rights raise firms' appetite to borrow), we first offer evidence substantiating that claim. More specifically, we document that the EIR induces foreign subsidiaries whose insolvency jurisdiction shifts to a stronger (weaker) creditor rights regime to skew their financing mixes away from (toward) non-debt instruments but not to change their real (e.g., investment) policies. We further reveal that our main results survive the inclusion of granular industry-location-time fixed effects shown to absorb demand effects (Degryse et al. (2019)). Noteworthily, they also survive the inclusion of parent-time fixed effects, suggesting that the EIR-induced debt effects observed by us also arise within multinational groups.

We next confirm that the debt changes found in our baseline estimations are caused by external creditors' actions rather than (e.g.) potential dynamics of groups' internal capital markets. Using the subsample of U.K. subsidiaries for which we can obtain more granular information on the sources of debt, we show that the debt changes are exclusively driven by bank debt, and not group or leasing debt.[6] In particular, we report that foreign U.K. subsidiaries shifted to weaker creditor protection

[5]The single CR-index dimensions are: (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.

[6]Our sample subsidiaries never hold director debt, the final debt subcategory in the FAME database.

regimes lower their bank debt by about two percentage points (t-statistic: –2.15) relative to domestic subsidiaries from pre to post-EIR, representing 23% of average pre-EIR U.K. bank debt. In contrast, those same subsidiaries do not change their group or leasing debt over that same period. Linking U.K. subsidiaries to their main banks, we also examine the role of banks in mediating the reform's debt-taking effects. In tests entirely novel to the literature, we reveal that only banks with either low equity-to-assets ratios or low ROAs (*weak banks*) and those whose business model relies less on commercial lending cut their lending in response to weaker creditor protection, also alleviating concerns that our main evidence is driven by demand rather than supply-side effects.

We also condition our main results on subsidiaries' distress risk, measured using either the original or a re-estimated Altman (1983) z'-score, and host countries' enforcement, measured using popular indexes including the Worldbank enforcement score. Doing so is interesting since, in the absence of a strictly-non-treated group, other studies exploiting within-country legal reforms to identify the causal effects of creditor rights changes use these variables to sort their sample firms into treatment and control groups. Their argument is that distress and enforcement are channels through which creditor rights changes affect firms. At odds with that argument, we document that neither variable strongly positively conditions the EIR-induced debt-taking effects. For example, our evidence suggests that the debt of moderately but not highly distressed subsidiaries rises the most in response to strengthening creditor rights, in line with the intuition that some firms' ability to repay debt is so severely impaired that not even stronger creditor rights are able to induce creditors to lend to them.

Our final tests corroborate that our main evidence does not arise endogenously through religious, cultural, or political factors. While it seems unlikely that it does since we keep these factors constant for all actors except the court officials, we nonetheless let the debt-taking effects spurred by creditor rights changes compete with those spurred by changes in popular proxies for these factors. Overall, we find little to no evidence that changes in those factors confound our main findings.

As standard in our literature, our remaining tests confirm the robustness of our baseline results to: (i) the parallel trends assumption; (ii) placebo tests; and (iii) propensity-score matching.

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 study those rights' 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 these studies 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 prominent legal scholars.[7]

[7] Djankov 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.

More recent studies aim to address these concerns by exploiting within-country legal reforms affecting single creditor rights. The evidence from these studies, however, is mixed, with some finding no relation between creditor rights and corporate borrowing (e.g., Haselmann et al. (2010), who study reforms in Eastern European countries, and Closset et al. (2023), who study several European reforms together); 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, and Agarwal and Singhvi (2023), who study an Indian 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) or relationship lending (Ghitti et al. (2025), who study the above reforms in Italy). A possible explanation is that such empirical settings face important limitations. First, most reforms affect only one or a few different creditor protection dimensions, 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 their sample firms are all subject to the same insolvency laws.

Our paper informs this literature along several critical dimensions. First, we exploit a regulatory change providing a cleaner empirical setting. To wit, the EIR unexpectedly reassigned the insolvency jurisdiction of some foreign subsidiaries across creditor rights regimes without affecting the broader economic and institutional environment. This setting allows us to compare firms which become subject to different creditor rights regimes whilst continuing to operate within the same (host country) environment, enabling us to overcome many limitations of previous studies. It further allows us to examine changes across entire creditor rights regimes rather than isolated legal provisions, avoids confounding simultaneous reforms, and provides a natural control group of otherwise comparable subsidiaries operating under the same host country conditions. Second, we provide novel evidence on which dimensions of creditor rights matter the most for firms' financing decisions. Third, we are, to our best knowledge, the first to investigate the role banks play in mediating the effects of creditor rights changes. Finally, we provide entirely new evidence on how the effects of creditor protection interact with distress risk, enforcement, and other insolvency law institutional features.

Our findings further add 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 that stronger host country creditor rights raise subsidiaries' external debt-taking (e.g., Desai et al. (2004); Lehmann et al. (2004); and Akbel and Schnitzer (2011)). Crucially, these studies rely on standard 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. Also, we exploit granular balance sheet information to examine the role of internal capital markets after a change in the creditor rights regime.

Finally, our results speak directly to the policy debate related to the insolvency and restructuring reforms recently enacted in many countries (e.g., Gurrea-Martínez (2022); van Zwieten (2023)). These reforms are typically based on the presumption that stronger creditor protection improves firms' access to credit. Offering causal evidence that creditor rights changes affect firms' borrowing decisions and identifying which dimensions matter the most, our results provide an empirical basis for evaluating this presumption and informing the design of ongoing and future reforms.

We proceed as follow. Section 2 describes the EIR's institutional setting. Section 3 outlines data sources, sample construction, and methodology. Section 4 presents our baseline results on the effects of the EIR on debt-taking and terms. Section 5 assesses the mechanisms behind our main results. Section 6 discusses standard validation and robustness tests. Section 7 concludes.

2 The 2002 European Insolvency Regulation

In this section, we provide an overview of the 2002 European Insolvency Regulation (EIR). We first outline its history and then detail its implications. We next review several important court rulings on the regulation since these are critical for the setup of our identification strategy. See Chapter 30 in Wood (2007) for more details about the history and repercussions of the EIR.

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, that draft was ultimately rejected by national parliaments due to their 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 bankruptcy law conflicts 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).[8] 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). This recital offered ample room for interpretation. 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 logded and treated, sets stays (or: freezes) on assets as well as priorities, and governs the effects on contracts and creditor actions.

Despite its broad scope, there are a few exceptions to the EIR's objective to universally apply the bankruptcy code of the country in which main 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, rights-in-rem-based security interests.[9] The major implication is that the bankruptcy administrator can neither stop the enforcement of such 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 date on which the EIR became effective, 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 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).[10] 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

[8] To be clear, the EIR mandates that EU courts would also have jurisdiction over companies registered in (say) Japan, Singapore, and the U.S. if their COMI were within one of the EU member states other than Denmark. Unfortunately, we cannot study the effects on those companies since our data identify only EU subsidiaries with EU parents.

[9] We stress that this exception does not matter much in our setting: Out of our twelve EU sample countries, only Sweden and the U.K. allowed for security interests in the form of rights-in-rem over our sample period.

[10] Based 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.

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 IA.1 in the Internet Appendix gives details of noteworthy EU court rulings on the COMIs of multinational groups over the 2002-2006 period (Wood, 2007, pp. 904-909). Strikingly, the table shows that, *in each single case*, EU courts ruled that the COMIs of the involved (fully-owned) subsidiaries are not in their registration countries but in their parents', with the earliest decision handed down in July 2002 — merely two months after the EIR.[11] The last column highlights that the reasons for those rulings are always centered around where the subsidiaries' head-office functions (its selling, recruitment, IT, controlling, etc. functions) are conducted. While we ultimately do not know why judges decided to deviate from the legal consensus, it has been speculated that they did so due to the belief that it is easier to reorganize a multinational group under one administrator operating under one code in one country — rather than under multiple administrators operating under multiple codes in multiple countries (see, e.g., the legal commentary on the 2006 Eurotunnel ruling).

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, we 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 judiciare") 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 analyzed in prior studies exploiting intra-country reforms.

In our empirical work, we exploit the wrinkles of the EIR by investigating the debt-taking decisions of fully-owned foreign subsidiaries which, due to the EIR, become exposed to either stronger or weaker creditor rights regimes than that of their host country depending on their parent's location. We use two control groups. The first is fully-owned domestic subsidiaries which remain exposed to the same creditor rights regime. The second is foreign subsidiaries which become exposed to a creditor

[11] We note that EU judges do not always decide to shift the COMIs of EU companies. For example, they refused to do so in cases in which a stand-alone company with all its operations in one EU country argued that its COMI should be in another to file under a more company-friendly bankruptcy regime (see Wood (2007)).

rights regime offering them a similar degree of protection as that of their host country. The following section gives more details on how we empirically implement our identification strategy.

3 Data and Methodology

3.1 Data Sources

We retrieve data on subsidiaries registered in all 15 EU member states at the start of our sample period and fully-owned by parents in those states 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 in the financial and utilities sectors; (b) observations associated with non-twelve-month fiscal years; and (c) subsidiaries hosted in or owned by firms in Denmark, Greece, and Luxembourg.[12] Overall, our sample features 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 within Europe).

We complement our sample AMADEUS data with granular information on the sources of debt from FAME, Bureau van Dyck's product for U.K. firms, again retrieving data from all April releases between 1999 and 2010 and deleting duplicates. We also use the April 2002 FAME release to retrieve data on the names of U.K. subsidiary's main banks in the pre-reform year (2001). We manually merge the bank names with 1997-2002 data from BANKSCOPE. We use the pre-reform period data from BANKSCOPE to construct proxies for a bank's strength and business model (i.e., its equity-to-assets ratio, ROA, and share of interest from commercial lending activity).

Next, we use data from Andrei Shleifer's website to proxy for the strength of creditor rights.[13] 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. Conversely, we borrow La Porta et al.'s (1998) subindexes measured around 1994-1995 when analyzing the effects of each single CR index component.[14] As Spain and Italy reformed their bankruptcy codes in 2003 and 2005/2006, respectively, their index values change (Spain)

[12] Denmark 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.

[13] The URL of Andrei Shleifer's website is: https://shleifer.scholars.harvard.edu/.

[14] We 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

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, implying a constant index value within country over our sample years. The index ranges from zero to four, with zero (four) indicating the weakest (strongest) protection. The index displays great variations across our EU-12 countries: It is zero for France; one for Finland, Ireland, and Portugal; two for Belgium, Italy, and Spain; three for Austria, Germany, and Netherlands; and four for the U.K.

We also collect data from Andrei Shleifer's website on enforcement variables, including the number of days required by a country's legal system to resolve a business dispute and the formalism of its legal system, along with several religious and political variables. We also study the Worldbank's enforcement score. We finally use the cultural variables of Hofstede (2001).

3.2 Variable Definitions

Our main outcome variable is *TotalDebt*, computed as the sum of short (AMADEUS mnemonic: loan) and long-term (ltdb) debt scaled by assets (at). We further look into *Short-(Long-)termDebt*, defined as short-(long-)term debt scaled by assets, as well as *InterestToDebt*, defined as the annual interest expense (inte) scaled by one-year-lagged debt. In our financing mix analysis, we rely on *TradeCredit*, *OtherCurrentLiabilities*, *OtherNonCurrentLiabilities*, and *ShareholderFunds*, defined as accounts payable (cred), other current liabilities (ocli), other non-current liabilities (oncl), and shareholder funds (shfd) all scaled by assets, respectively. In our real policy tests, we define *TangibleAssetGrowth* as the annual change in tangible assets (tfas) plus depreciaton (depr) scaled by one-year-lagged tangible assets; *WorkingCapitalGrowth* as the net annual percent change in working capital, where working capital is inventory (stok) plus accounts receivables (debt) minus accounts payable (cred); *WorkerGrowth* as the net annual percent change in the number of employees (empl); and *CashGrowth* as the net annual percent change in cash (cash). Using the FAME U.K. data, we define *BankDebt* as the sum of bank overdrafts, other short-term debt, and other long-term debt; *GroupDebt* as the sum of short and long-term group loans; *NetGroupDebt* as the difference between the sum of short and long-term group debt and group-loan assets; and *LeasingDebt* as the sum of short and long-term hire-purchase leases, all scaled by assets.

Our first treatment variable (*Treat*) is a dummy variable equal to one for subsidiaries which are consistently fully-owned by a parent registered in a country other than the subsidiary's registration country over the pre-EIR period ("foreign subsidiaries") and to zero for those consistently fully-owned by a parent registered in the same country ("domestic subsidiaries").[15] Conversely, our second treat-

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.

[15]To be conservative, we exclude fully-owned subsidiaries with parents in different foreign countries over the pre-EIR period. As there are close to no such subsidiaries, this choice does not materially affect our empirical results.

ment variables indicate the direction of treatment (i.e., whether the insolvency jurisdiction of a foreign subsidiary shifts to a more, similarly, or less creditor-friendly regime). In detail, Up^{CRI} ($Down^{CRI}$) is a dummy variable equal to one if the pre-EIR CR index value of the country of a subsidiary's parent is at least two units higher (lower) than that of its host country and else zero. The shock variable (*Shock*) is a dummy variable equal to one over the post-EIR (2002-2006) period and else zero.

In further analysis, we also use dummy variables based on the single CR index components. In particular, On^x (Off^x) is 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), else zero. In the specifications augmented by additional country-specific variables (see below), we define Up^k ($Down^k$) to be 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) k-value than its pre-EIR registration country, else zero.

We use the average Altman (1983) z'-score for private firms over the pre-EIR period to determine a subsidiary's distress risk. In line with Altman (1983), we calculate the original score as:

$$\begin{aligned} z'\text{-}score &= 0.717 \times WorkingCapital + 0.847 \times RetainedEarnings + 3.107 \times EBIT \\ &\quad + 0.998 \times Sales + 0.420 \times BookEquity, \end{aligned} \tag{1}$$

where *WorkingCapital* is working capital (wkca), *RetainedEarnings* is other shareholder funds (osfd), *EBIT* is operating profits (oppl), and *Sales* is sales revenue, all scaled by assets. Finally, *BookEquity* is shareholder funds scaled by total liabilities (ncli+culi). A possible concern with our implementation of the original score is that we approximate retained earnings with other shareholder funds, which may include other items. A more general concern is that the original score is estimated from U.S. public manufacturing firms, possibly implying that it does not capture the distress risk of private EU subsidiaries well. Given that, we also re-estimate the slopes of the z'-score with our pre-EIR data and form an updated z'-score. Note that both scores decrease with distress risk.[16]

Our controls are *Size*, *FirmAge*, *Employees*, *Profitability*, *FixedAssets*, and *CashHoldings*. *Size* is the natural log of assets. *FirmAge* 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), *FixedAssets* tangible fixed assets (tfas), and *CashHoldings* the cash reserve (cash), all scaled by assets. To avoid "bad-control problems" (see, e.g., Angrist and Pischke (2009)), we calculate the firm-specific average of each control taken over the pre-EIR period and then

[16]We use a subsidiary's historical legal status from AMADEUS to identify its status, classifying those as bankrupt whose legal status is "active (default of payments)," "active (insolvency proceedings)," "active (receivership)," "bankruptcy," "dissolved (bankruptcy)," "dissolved (liquidation)," "in bankruptcy," or "in liquidation." Recognizing that the slopes from an OLS regression are proportional to those from multiple discriminant analysis when there are only two status categories, we next regress a dummy variable equal to one for non-bankrupt subsidiaries and else zero on the one-year-lagged predictor variables in Eq. (1) over our pre-EIR subsidiary data. Strikingly, the original and re-estimated z'-scores share a pooled-sample correlation of about 77%, validating the robustness of the original score.

interact that with country and year dummies in our main tests (see next subsection). We report more details on all variables' definitions and their sources in Table A.1 in Appendix A.

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

3.3 Methodology

We use triple (DIDID) difference estimators to identify the causal effects of creditor rights, other subsidiary and country-specific variables, and (some of) their interactions on debt financing. We first pool the subsidiaries from various sets of EU-12 countries and estimate the model:

$$\begin{aligned} Debt_{i,s,t} &= \beta_1(Treat_{i,s} \times Shock_t \times Up^{CRI}_{i,s}) + \beta_2(Treat_{i,s} \times Shock_t \times Down^{CRI}_{i,s}) + \beta_3(Treat_{i,s} \times Shock_t) \\ &+ \sum_{s\in S}\sum_{y\in Y}\sum_{c\in C}\gamma_{s,y,c}(DState^s_i \times DYear^y_t \times MCon^c_i) + \alpha_i + \alpha_t + \epsilon_{i,s,t}, \end{aligned} \tag{2}$$

where *Debt* is either the *TotalDebt*, *ShortermDebt*, *LongtermDebt*, or *InterestToDebt* of subsidiary i in member state s at the end of fiscal year t. *Treat* and *Shock* are defined in the prior subsection, while S, Y, and C are the sets of EU member states, years, and control variables in our specifications, respectively. $DState^s$ is a dummy equal to one if a subsidiary is registered in state s and else zero, $DYear^y$ 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. We estimate regression (2) on (i) the EU-12 countries; (ii) France and the U.K.; (iii) France, Italy, Germany, Spain, and the U.K. (the five largest EU countries); and (iv) the EU-12 countries excluding France and the U.K.

β_1 (β_2) captures the *incremental* effect from being shifted to a foreign regime with stronger (weaker) creditor rights than the host country. Conversely, β_3 captures the effect from being shifted to a similarly-friendly foreign regime (*creditor-rights-unrelated effect*), possibly stemming from unfamiliarity with the foreign country's norms, culture, and/or legal system. Alternatively, the unrelated effect may also arise through creditors being uncertain about whether EU courts will continue to use the heads-office function interpretation of the COMI in their future rulings (see Sussman (2008)).

We sometimes run regression (2) on subsamples of our data formed from a subsidiary or country-specific variable, such as a subsidiary's distress risk or the enforcement level in its host country. To test for statistical differences in the DID and DIDID effects (i.e., in β_1 to β_3) across the subsamples,

we estimate the following regressions using only data from the extreme subsamples:

$$\begin{aligned} Debt_{i,s,t} &= \boldsymbol{\beta}_1'(Treat_{i,s} \times Shock_t \times Up_{i,s}^{CRI} \times \mathbf{Top}) + \boldsymbol{\beta}_2'(Treat_{i,s} \times Shock_t \times Down_{i,s}^{CRI} \times \mathbf{Top}) \\ &+ \boldsymbol{\beta}_3'(Treat_{i,s} \times Shock_t \times \mathbf{Top}) + \sum_{s\in S}\sum_{y\in Y}\sum_{g\in G}\sum_{c\in C}\gamma_{s,y,g,c}(DState_i^s \times DYear_t^y \times DG^g \times MCon_i^c) \\ &+ \alpha_i + \alpha_t^g + \epsilon_{i,s,t}, \end{aligned} \tag{3}$$

where $\boldsymbol{\beta}_1$, $\boldsymbol{\beta}_2$, and $\boldsymbol{\beta}_3$ are $[2\times 1]$ vectors of parameters, **Top** is a $[2\times 1]$ vector containing one and a dummy variable equal to one if the variable from which the subsamples are formed is in the top subsample and else zero, G is the set of both subsamples, and DG^g is a dummy variable equal to one if the current observation belongs to subsample g and else zero. The second entry in the $\boldsymbol{\beta}$ vectors gives the differences in the DID and DIDID effects across the exreme subsamples.

In further tests, we delve into the CR index subcomponents to study how each of them influences corporate borrowing and to find out whether they produce 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 slightly modify regression (2) as follows:

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

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) on the EU-12 sample since we require sufficient variations across countries to accurately estimate parameters, especially since the rights are highly correlated.

Finally, we run horse races to control for other factors potentially explaining our evidence, such as EIR-induced changes in the religious, cultural, and politics beliefs of court officials. To implement that analysis, we estimate the following augmented version of equation (2):

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

where k is one of the competing variables. In regression (5), we interpret β_1 (β_2) as the effect from being shifted to a country with stronger (weaker) creditor rights *controlling for changes in variable*

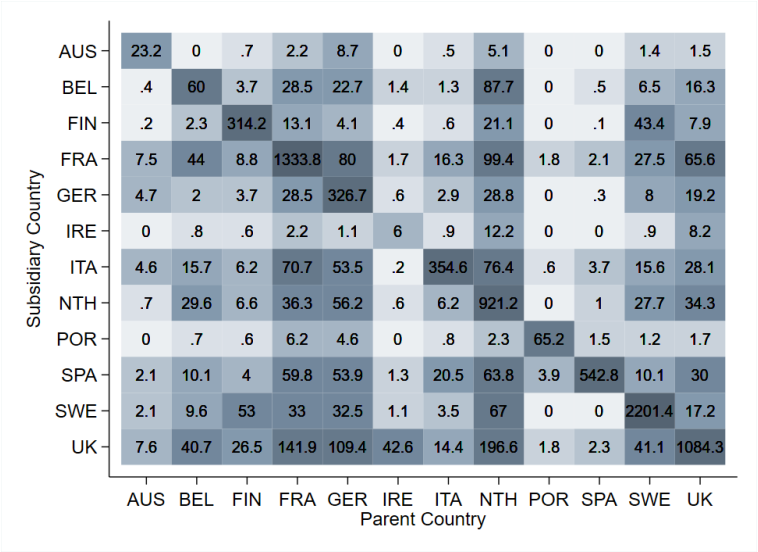


Figure 1. The figure offers a heatplot of the annual number of subsidiaries hosted in one of the EU-12 sample countries (rows) and fully owned by a parent in the same or another such country (columns). The darker cells in the heatmap indicate a greater number of observations.

k. 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.

4 The Debt-Financing Effects of the EIR

In this section, we study the causal effects of creditors rights on corporate borrowing. We first discuss descriptive statistics. We next examine the overall debt-taking effects of the EIR in pooled-EU-country samples, alongside an analysis of debt terms. We then decompose the CR index into its underlying rights, to analyze the separate effects from awarding or withdrawing each. We finally evaluate whether the CR index is a holistic measure of all insolvency law aspects relevant to real-world creditors.

4.1 Descriptive Statistics

Table IA.2 in the Internet Appendix presents 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. Focusing on the average annual numbers, Figure 1 offers a heatmap representation of those table entries. Both the table and figure show that the majority of countries feature more than 100 subsidiaries per year on average, with France, the Netherlands, Sweden, and the U.K. having the largest numbers. For most countries, the vast majority of these subsidiaries are

domestic (i.e., have a parent registered in their host country) — see the diagonal of Figure 1. Exceptions are 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 141 foreign subsidiaries in Belgium (corresponding to 83% of all foreign subsidiaries in the country) have their parents located either in its neighboring countries or those sharing similar CR index values (i.e., in France, Germany, Italy, Netherlands, and Spain). Notwithstanding, especially the larger countries feature subsidiaries with parents from the vast majority of other EU-12 countries. This observation is of critical importance for us since the statistical power of our DIDID identification strategy relies crucially on the number of host-vs.-parent country pairs with dissimilar creditor rights.

Table IA.3 in the Internet Appendix reports country-specific as well as overall descriptive statistics including average (Mean), median (Med.), and standard deviation (St.D.) for our main outcome and control variables, where the controls are subsidiary-specific pre-EIR averages repeated for each fiscal year of a subsidiary. The table points to 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, average total debt has a highly positive correlation of 61% with the CR index (not shown in table), in line with 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 the U.K., suggesting that debt-taking is highly positively skewed and thus unevenly distributed. Finally, we also see 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 pattern arises in Germany, Finland, and Sweden.

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). As such, all 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 6).

4.2 The Debt-Taking Effects of the Reform

Table 1 presents the results from estimating regression (2) with *TotalDebt* as dependent variable on different country samples. At the bottom of the table, we also report the total number of observations ("Observations"), the number of those associated with foreign subsidiaries (*Treat* = 1; "Treated Obs."), and the number of those associated with foreign subsidiaries with parents in countries with an at least two points higher (lower) CR-index value than their host countries ("Treated Obs (SR)" ("Treated Obs

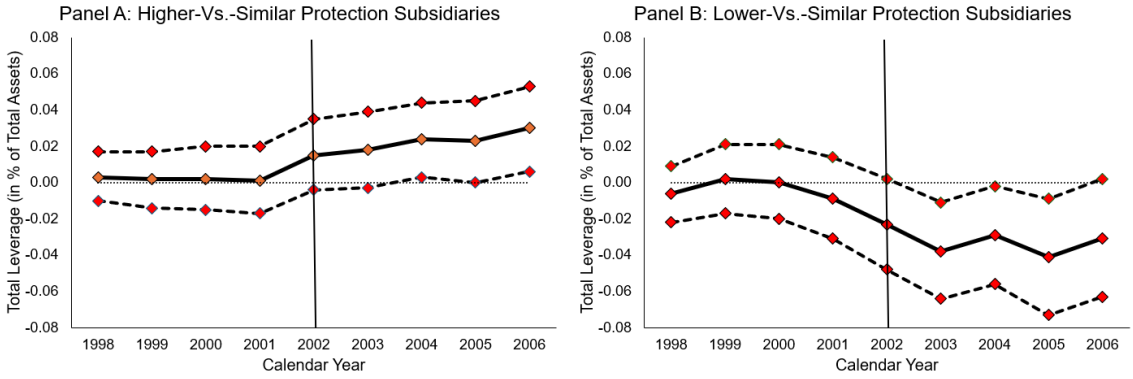


Figure 2. The figure plots the annual triple-difference (DIDID) coefficients from a regression equivalent to that in column (1) of Table 1 except that we replace the double interaction with an interaction between *Treat* and a vector of dummy variables, each equal to one for one year between 1998 and 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.

(WR)")). We also report the adjusted R-squared ("Ad. R^2"). Our evidence suggests a positive causal effect of creditor rights on debt-taking. In the EU-12 sample (column (1)), subsidiaries with parents in countries with stronger creditor right regimes than those of their 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 involving Up^{CRI}). In contrast, subsidiaries with parents in countries with weaker creditor right regimes than those of their host countries *lower* their total debt by 2.9 percent points (t-statistic: –3.27) relative to the same control subsidiaries over the same period (see the coefficient on the triple interaction involving $Down^{CRI}$). Noteworthily, these changes are not only statistically but also economically meaningful, representing 12% and 18% of average pre-EIR *TotalDebt*, respectively.

TABLE 1 ABOUT HERE.

Figure 2 gives the point estimates of the triple-interaction-term sets of a dynamic model in which we replace the *Shock* dummy in regression (2) with a vector of year dummy variables (excluding the 1997 one). Panel A (B) vividly shows that foreign subsidiaries with parents in countries with stronger (weaker) creditor protection do not change their debt relative to those with parents in countries with a similar protection pre-EIR but markedly raise (lower) it when the EIR becomes effective.

Table 1 yields two further insights. First, it points to so-called *dosage effects*: The creditor-rights-related impact, as reflected by the absolute coefficients on the triple interactions, is stronger in samples featuring greater creditor rights variations (see columns (2) and (3)). Conversely, that same impact is attenuated in the sample featuring smaller variations, with the Up^{CRI} triple-interaction coefficient even being insignificant (column (4)). Second, the table suggests a significant creditor-rights-unrelated

debt-financing effect. Although statistically and economically weaker than the creditor-rights-related effect, foreign subsidiaries with parents in countries with similar creditor rights as their host countries lower their debt by 1.3 percent points (t-statistic: –2.67) relative to domestic subsidiaries from pre to post-EIR (see the coefficient on the double interaction in column (1)). In economic magnitudes, that decline represents about 8% of average pre-EIR *TotalDebt*. This creditor-right-unrelated effect may capture a degree of unfamiliarity with a foreign country's norms, culture, and/or legal system as well as legal uncertainty about the future insolvency jurisdiction of a firm (Sussman (2008)).

4.3 The Debt-Contracting-Term Effects of the Reform

We next explore the causal effects of the EIR on debt-contracting terms, specifically, debt maturity and borrowing costs. We split total debt into short and long-term debt to study maturity effects. Next, we use the ratio of the annual interest expense to one-year-lagged debt (i.e., *InterestToDebt*) to investigate borrowing cost effects (as, e.g., Pittman and Fortin (2004) and Francis et al. (2005)). While our cost proxy does not isolate the interest expense of newly originated debt and may partly reflect differences in debt composition and maturity structure, it is a standard accounting-based proxy in private-firm settings in which actual loan-level borrowing rates are hardly ever available.

Table 2 shows that the EIR's total debt-taking effects found in Table 1 are almost exclusively driven by short-term debt (columns (2) and (3)). Several reasons may explain this result. First, short-term lending is far more popular among our sample firms than long-term lending (recall the descriptive statistics in Table IA.3 in the Internet Appendix). Second, it is easier to change short-term debt in response to a shock, such as the EIR. Third, the EIR does not affect *rights-in-rem*. Security interests in the form of rights-in-rem are typically attached to durable, long-lived assets. Theory postulates that such assets are better suited to support longer-term borrowing since matching debt maturity to an asset's lifetime reduces refinancing and liquidation risk (Hart and Moore, 1994). As such, the rights-in-rem exclusion may have left long-term borrowing largely insulated from the reform, at least in those countries allowing for such rights in secured lending: Sweden and the U.K.

TABLE 2 ABOUT HERE.

The table further suggests that the EIR increases the borrowing costs of foreign subsidiaries with parents in countries with weaker creditor rights than their host countries, in line with the supply-side view of creditor rights (La Porta et al. (1997) and Ghitti et al. (2025)). To be more specific, the interest-to-debt ratio of those subsidiaries rises by 3.3 percent points (t-statistic: 2.15) relative to that of foreign subsidiaries with parents in countries with more similar creditor rights from before to after the EIR (column (4)). The drop is economically meaningful, representing 13% of average pre-EIR

borrowing costs. In contrast, the borrowing costs of foreign subsidiaries with parents in countries with stronger creditor rights relative to their host countries do not materially change.

4.4 The Effects of Granting or Withdrawing Single Rights

Prior intra-country studies look into only one single or a few well-known dimensions of creditor protection (e.g., Rodano et al. (2016), Closset and Urban (2019), Schoenherr and Starmans (2022), De Vito and Jacob (2023)). In contrast, our setting allows us to estimate and compare the effects of a much larger set of dimensions (or rights). More importantly, it uniquely enables us to contrast the effect of granting one single right (On^x) with that of withdrawing that same right (Off^x). To facilitate our analysis of the effects of single rights, we split the CR index into its subindexes. The subindexes indicate whether: secured creditors are paid first (SPF); creditors can block reorganizations (CC); managers do not stay in place (MR); and there is no automatic stay on assets (NOS).

Table 3 presents the results from estimating regression (4) with *TotalDebt* as dependent variable on the EU-12 sample. The first four columns report the effects of each single right in isolation, while the other two jointly consider the SPF, CC, and NOS rights and all rights, respectively.[17] 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). Columns (1) to (4) show that each single right produces the anticipated effects: Upon the grant (withdrawal) of the right, debt rises (drops). Notwithstanding, both the statistical and economic significance of the effects vary across rights as well as whether a right is granted or withdrawn. For example, while shifting secured creditors to the top rung of the bankruptcy ladder of priorities (column (1)) and the lifting of automatic stays (column (4)) raise debt, 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. Moreover, while granting creditors the right to block reorganizations (column (2)) raises debt by a mere 1.2 percent points (t-statistic: 1.95), withdrawing that same right lowers it by a much more pronounced 2.7 percent points (t-statistic: –3.31). All these differences become sharper when we jointly consider multiple rights in the same specification (columns (5) and (6)). For instance, column (6) shows that only granting the SPF and MR rights significantly raises debt, 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 outcome.

TABLE 3 ABOUT HERE.

[17] Since the CC and MR rights share a cross-country correlation of about 80%, there is the justified concern that standard errors may severely blow up if we include both variables in the same specification.

LLSV-DMS Creditor Rights Index:	Pseudo Creditor Rights Index:		
	0 (Low)	1	2 (High)
0 (Low)	France		
1	Ireland Portugal	Finland Sweden	
2		Belgium Italy	Spain
3		Germany Netherlands	Austria
4 (High)			UK

Figure 3. The figure plots the three values of a pdeudo creditor rights index (in the columns) against the five values of the La Porta et al. (1998)-Djankov et al. (2007) CR index (in the rows). The pseudo index is that permutation of a three-value index maximizing the difference between the Up^{CRI} triple-interaction and the $Down^{CRI}$ triple-interaction t-statistics in regression (2) run on the EU-12 sample.

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 yield highly asymmetric effects. One possible reason for the stronger effect observed upon withdrawing a right (see the CC and NOS rights) could be that its unexpected disappearance cannot be promptly and easily replaced by alternative solutions, for example, private contracts (e.g., Franks et al. (2021)). Yet, the same reason can hardly explain why, in other instances, granting a right generates a stronger effect (see the SPF right).

4.5 The Validity of the CR Index

All our tests hinge on the ability of the CR index to accurately measure creditor rights. However, recent studies claim that the index is overly simplistic, omitting many critical legal provisions and ignoring that many countries have multiple procedures (e.g., Deakin et al. (2017), Albertus et al. (2026), and Gurrea-Martínez (2026)). To assess the validity of the CR index, we next create a pseudo index. Using the EU-12 sample, we assign each country each one of three values (0, 1, or 2) to indicate either low, moderate, or high creditor rights, respectively. Doing so results in $3^{12} = 531,441$ possible ways to assign these values, with each one representing a "candidate pseudo index."[18] For each candidate index, we then redefine Up^{CRI} ($Down^{CRI}$) to be one if the parent country's assigned value is at least one point higher (lower) than the host country's and else zero. Using our new definitions for Up^{CRI} and $Down^{CRI}$, we next re-estimate regression (2) for each candidate index. We finally identify the candidate index maximizing the difference between the t-statistics on the Up^{CRI} and $Down^{CRI}$ triple interaction terms as the one best capturing the debt changes in our sample data.

[18]While we initially aimed to create a pseudo index with five values (like the CR index), the problem is that such an index would have had $5^{12} = 244,140,625$ permutations, making the estimation infeasible. For comparison, the estimation of the three-value index took us about three weeks on five high-performance cluster nodes.

Figure 3 plots the identified index's values (in the columns) against the real CR index's values (in the rows). The figure reveals that the two indexes are remarkably close, agreeing on which countries award creditors a low, moderate, and high degree of legal protections.[19] In accordance, the correlation between the two indexes is 74%. Overall, our evidence suggests that the CR index comprehensively captures most aspects of insolvency law which real-world creditors care about.

5 The Mechanisms Behind the Debt-Taking Effects

We now study the mechanisms driving our debt-taking evidence in Section 4. We first show that the debt changes induced by the EIR are triggered by supply rather than demand-side effects. We next report that they come through bank lending rather than internal capital markets or leasing. After that, we reveal that the changes are not strongly positively conditioned by a subsidiary's distress risk or the strength of enforcement in its host country. Finally, we demonstrate that they do not arise endogenously through creditor rights capturing cultural, religious, or political factors.

5.1 Demand-Versus-Supply-Side Effects

We first explore whether the debt changes spurred by the EIR are driven by supply (i.e., a rise in creditors' willingness to lend) or demand (i.e., a rise in firms' appetite to borrow) side effects. While it is unlikely that stronger creditor rights induce firms to ask for more credit, and vice versa, we nonetheless follow two strategies to shed more light on that issue. First, we examine how the foreign subsidiaries shifted to creditor rights regimes more (less) friendly than those of their host countries adjust their real and non-debt financial policies in response to their greater (reduced) debt-taking. If we find that these subsidiaries mostly re-optimize their financing mixes by skewing them away from (toward) non-debt instruments but do not change their real investments, then this evidence would point to a supply-side effect underlying the debt-taking effects. Second, we rerun our regression (2) using more granular dynamic fixed effects shown in the literature to absorb demand (and other) effects.

5.1.1 The Effects of the EIR on Real and Non-Debt Financing Policies

Starting with the financial and real policies analysis, Table 4 offers the results from running regression (2) using *TradeCredit*, *OtherCurrentLiabilities*, *OtherNonCurrentLiabilities*, and *ShareholderFunds* as dependent variables in Panel A and *TangibleAssetGrowth*, *WorkingCpitalGrowth*, *Work-*

[19] While Spain seems a minor outlier, we note that it upgraded its creditor protection in 2003 (at which point we exclude it from our sample). If creditors already reacted to the likely anticipated increase in creditor rights in 2002, then this possibility may explain why the pseudo index is slightly more optimistic about Spain than the CR index.

erGrowth, and *CashGrowth* in Panel B on the EU-12 sample. The table suggests that the foreign subsidiaries shifted to more or less creditor-friendly regimes re-optimize their capital structure but do not adjust their real policies. Panel A, for example, shows that the subsidiaries shifted to friendlier regimes and taking out more debt reduce their trade credit by 0.009 (t-statistic: –2.12) and their other non-current liabilities by 0.011 (t-statistic: –3.01) relative to those shifted to similarly friendly regimes (columns (1) and (3)). Conversely, the subsidiaries shifted to less friendly regimes and taking out less debt raise their other current liabilities by 0.009 (t-statistic: 1.85) and their other non-current liabilities by 0.006 (t-statistic: 2.03; columns (2) and (3)). Notably, column (4) shows that neither type of subsidiary adjusts their equity financing, perhaps because our sample subsidiaries are all fully owned by their parents, so that changes in their equity largely reflect internal capital allocation decisions rather than responses to external financing conditions. More importantly, Panel B reports that neither adjusts their investments into physical capital, working capital, labor, or cash.

TABLE 4 ABOUT HERE.

5.1.2 Alternative Fixed Effects Specifications

We next turn to re-estimating regression (2) with more granular fixed effects. Using static firm and year fixed effects, our baseline specification absorbs firm-invariant characteristics and common shocks. While this specification — combined with our DIDID strategy and the pre-reform control interactions — already accounts for a wide range of confounding factors, a natural concern is that our estimates could reflect time-varying shocks correlated with treatment status but unrelated to the EIR, as, for example, a differential credit demand between foreign subsidiaries with parents in countries with more, similarly, or less creditor friendly regimes. To mitigate such concerns, we use four increasingly demanding fixed-effect sets in our estimations: (i) static firm and dynamic industry-time; (ii) static firm and dynamic industry-location-time, (iii) static firm and dynamic parent-time; and (iv) static firm plus dynamic industry-year and dynamic parent-year fixed effects.

Let us be explicit about what the four alternative specifications achieve. Specification (ii) absorbs industry-specific shocks, including industry-level credit cycles, sector-specific demand shocks, and industry-wide responses to macroeconomic developments contemporaneous with the EIR. Using it, we address concerns that our treated (i.e., foreign-owned) subsidiaries may be disproportionally concentrated in sectors experiencing differential conditions unrelated to the EIR-induced creditor rights changes. To further control for credit demand, specification (ii) follows recent banking studies (see, e.g., Degryse et al. (2019) and Pisicoli et al. (2026)) by allowing industry-specific trends to vary

flexibly across sub-national regions.[20] This specification accounts for time-varying local demand for credit: Subsidiaries within the same industry but located in different regions may face different credit conditions shaping their demand for external capital, such as regional economic activity, growth opportunities, and/or the presence of a local banking sector. This specification isolates variation in debt financing occuring *within* the same industry-region-year combination, ensuring that our estimates do not capture variations in the local demand for credit possibly correlated with treatment status. Specification (iii) addresses a different source of potential confounding: time-varying heterogeneity at the level of the parent company. One possible concern is that our results could be affected by parent-specific financial policies (or shocks), such as liquidity management decisions at the group level or shocks occurring to the parent firm unrelated to the EIR. To address this concern, we augment the baseline specification with dynamic parent-year fixed effects. This specification captures any differential effect of the EIR across subsidiaries of the same parent differently exposed to the EIR-induced creditor rights changes. Finally, the most saturated specification, namely, (iv), includes both dynamic industry-year and dynamic parent-year fixed effects.

Table 5 presents the results from estimations using our baseline specification (for comparison) and fixed effect specifications (i) to (iv) in columns (1) to (5), respectively. The table shows that our main conclusions are robust to the alternative fixed effect sets, offering strong evidence that our results are not driven by industry and/or parent-level confounding dynamics.

TABLE 5 ABOUT HERE.

Taken together, the evidence in this section supports the notion that the EIR-induced debt changes found in Section 4 are triggered by supply rather than demand-side effects.

5.2 Bank-Versus-Group/Lease Financing Effects

We next examine whether the debt changes induced by the EIR arise through bank or non-bank debt changes. Desai et al. (2004) and Akbel and Schnitzer (2011) show that subsidiaries rely heavily on debt financing from their parent and other group members, opening up the possibility that internal capital market dynamics play a role in our setup. While it is conceivable that internal creditors, who are legally identical to external creditors, benefit from some rights underlying the CR index (as, e.g., the right to be paid first), they are likely to suffer from others (as, e.g, the replacement of the management team). Given that, our evidence in Table 3 that awarding (withdrawing) the right to replace managers raises (lowers) debt-taking casts some doubt on whether internal capital markets greatly influence

[20] We classify each subsidiary's region using its postcode or, if missing, its locality matched to the corresponding Nomenclature of Territorial Units for Statistics (NUTS) classification at the 2-digit level. That granular level captures areas such as the *regions* in France and Italy, group of *sub-regions* in the U.K., or *bundesland/regierungsbezirk* in Germany.

our main debt-taking results. Conversely, another source of non-bank debt is leasing (e.g., Ang and Peterson (1984) and Eisfeldt and Rampini (2009)). While theoretically relevant, leasing was far less common in Europe during the early 2000s than in other markets, as North-America.[21]

Notwithstanding these arguments, we use U.K. subsidiary data to offer more direct evidence that the EIR's debt-taking effects come from bank, and not group and/or leasing, debt. Our U.K. data allow us to do so since they include more granular information on subsidiaries' sources of debt as well as the identities of their main banks. We first estimate a simplified version of regression (2) involving only the double but not the triple interaction terms on the subcomponents of total debt: bank, group, and leasing debt. We next condition the effects from that regression on bank characteristics, such as their financial strength (i.e., their equity ratio and ROA) and lending experience. Before doing so, we, however, verify that our main debt-taking results also hold for the U.K. subsample.

In Section IA3 in the Internet Appendix, we explain how we modify our empirical design to run DID and DIDID estimations on the U.K.-only subsample. Based on the widely-held presumption that the U.K. grants the strongest legal protection to creditors in the EU, the creditors of all U.K. foreign subsidiaries — independent of their parent country — should be adversely affected by the EIR. As such, we expect the debt-financing of U.K. subsidiaries to drop relative to domestic subsidiaries after the EIR. Table IA.4 in the Internet Appendix confirms that hypothesis. For instance, foreign U.K. subsidiaries lowered their total debt by 3.7 percent point (t-statistic: 3.65) relative to domestic ones from pre to post-EIR, representing 10% of average pre-EIR total debt in the U.K.

5.2.1 The Effects of the EIR on Debt Subcategories

Table 6 reports the results from estimating the simplified regression using *BankDebt*, *GroupDebt*, *NetGroupDebt*, and *LeasingDebt* as dependent variable on the U.K. subsample. The table suggests that the debt adjustments of foreign U.K. subsidiaries found in Table IA.4 are almost exclusively driven by bank debt. Specifically, those subsidiaries lower their bank debt by about two percent points (t-statistic: –2.15) in comparison to domestic ones from pre to post-EIR, representing 23% of average pre-EIR bank debt in the U.K (column (1)). In contrast, the other columns reveal that the corresponding changes in their group leverage, net group leverage (i.e., received minus awarded group loans), and leasing are a highly insignificant –0.1, –0.4, and 0.2 points, respectively. As Table IA.4 reports that the U.K. subsample produces both a creditor-rights-related and unrelated debt effect, it immediately follows that both these effects must come through bank debt adjustments.

TABLE 6 ABOUT HERE.

[21] On average, only 13% of average total gross fixed investment in our sample countries was funded through leasing over our sample period. This figure is small in comparison to 20% for Australia, 22% for Canada, and 30% for the U.S. (see Leaseurope (2006) and White-Clark (2013)). A notable exception, though, was the U.K., with an average of 20%.

5.2.2 The Role of Banks on the EIR's Effects

We next dig deeper into the roles played by the main banks of our sample subsidiaries. Theory suggests that stronger banks may be less affected by 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, possibly leading them to moderate the positive causal effect of creditor protection on debt-taking in our main tests. To test this hypothesis, we re-estimate the simplified regression on the subsample of U.K. subsidiaries financed by stronger or weaker banks as well as on banks with more or less lending expertise, respectively. In addition, we estimate a fully-interacted version of that regression on the overall U.K. subsidiary subsample. We use banks' 2001 equity-to-assets and mean pre-reform ROA to measure banks' strength and the mean pre-reform share of interest obtained from commercial lending to measure their lending expertise.

Table 7 presents the results from estimating the regressions conditioning on main banks' strength or lending expertise. The table suggests that only weak banks and those with little lending expertise reduce their corporate lending in response to the EIR-induced weakening of creditor rights. Using the equity-to-assets ratio to measure bank strength, we, for example, find that while the debt-financing of foreign U.K. subsidiarities 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 those funded by strong banks is an insignificant 2.1 points (contrast columns (1) and (2)). Columns (3) reports that the difference between the two estimates, 4.5 percent points, is significant at the 10% level. Critically, we note that the debt change is never significant in the weak and inexperienced bank subsamples. Again, the upshot is that the change must be driven almost exclusively by banks since non-bank-induced changes would still show up in the weak and inexperienced bank subsamples. In the same vein, the insignificant debt changes in those subsamples also cement our claim that the EIR-induced debt-taking effects come, at least predominately, from the supply side.

TABLE 7 ABOUT HERE.

Overall, the results in this section strongly point to bank (and not group or leasing) debt changes as the main driver of the EIR-induced changes in total debt-taking in Section 4.

5.3 Channel-Versus-No Channel: Distress Risk and Enforcement

Many studies argue that a high corporate distress risk and a strong local enforcement regime act as channels through which creditor rights changes affect firms. This argument is of critical importance

in the recent empirical law and finance literature since studies exploiting intra-country legal reforms to identify the causal effects of creditor rights use it to sort their sample firms into treated and control firms. They do so because, strictly speaking, all their sample firms are treated due to them being subject to the same laws. Yet, whether distress risk and enforcement positively condition the effects of creditor rights changes is an open question. Consider, for example, the possibility that highly distressed firms are so impaired in their ability to repay debt that not even strengthening creditor rights can induce creditors to lend to them. In this case, the highly distressed firms are less treated than their healthier counterparts, invalidating the distress-based identification strategies.

We first study the modulating role of a subsidiary's distress risk on our benchmark debt-taking evidence. We next look into the corresponding role of the strength of enforcement.

5.3.1 The Modulating Role of Corporate Distress Risk

Rodano et al. (2016), Schoenherr and Starmans (2022), Ghitti et al. (2025), and many others rely on the argument that more distressed firms are more exposed to creditor rights changes resulting from legal reforms to sort their sample firms into treated and control firms. We evaluate the validity of this argument by rerunning regression (2) on subsamples of subsidiaries formed based on their distress risk and regression (3) on the pooled top-and-bottom subsamples. We use both the original Altman (1983) z-score and a version of it re-estimated on our pre-EIR subsidiary data to measure distress risk.[22] Keeping in mind that the z-score is negatively related to distress risk, we then sort our sample firms into tercile subsamples based on their pre-EIR average z-score.

Table 8 gives the results from the distress subsample regressions. While columns (1) to (4) use the original z-score to measure distress risk, columns (5) to (8) rely on its re-estimated version. Conversely, while columns (1), (2), and (3) (alternatively: (5), (6), and (7)) study highly, moderately, and weakly distressed subsidiaries, respectively, column (4) ((8)) shows the differences in the DID and DIDID effects across the high-versus-low distress risk subsamples. Independent of the distress measure, the table suggests that the debt of moderately distressed subsidiaries rises more strongly in response to improving creditor rights than that of weakly and highly distressed subsidiaries, in disagreement with the argument that more distressed firms are more affected by insolvency law reforms. Looking into the original z-score, columns (1) to (3), for example, show that the debt of moderately distressed firms rises by 2.1 percent points (t-statistic: 2.13), relative to 1.7 points (t-statistic: 1.71) for weakly and 1.4 points (t-statistic: 1.11) for highly distressed subsidiaries. In line with its small magnitude, column (4) demonstrates that the difference in the debt rises across the highly and weakly distressed subsidiaries is a statistically insignificant –0.3 percent points (t-statistic: –0.19).

[22] Using the Zmijewski (1984) rather than the Altman (1983) z-score or a version of it re-estimated on our pre-EIR subsidiary data does not change our conclusions. Conversely, we are unable to use a subsidiary's credit rating as measure of its distress risk since only about 14% of our subsidiaries are rated before the EIR.

TABLE 8 ABOUT HERE.

The table paints a more nuanced picture of the modulating role of a subsidiary's distress risk on its debt changes in response to deteriorating creditor rights. More specifically, while the original z-score confirms the argument that more distressed firms are more strongly affected (see columns (1) to (3)), its re-estimated version supports the view that weakly, moderately, and highly distressed firms are all similarly impacted (columns (5) to (7)). Notwithstanding, columns (4) and (8) show that the differences in the debt decreases induced through weaker creditor rights are statistically insignificant across the highly and weakly distressed subsidiaries independent of the distress proxy.

5.3.2 The Modulating Role of Country-Level Enforcement

Safavian and Sharma (2007) offer evidence that stronger enforcement amplifies the effects of within-country creditor right improvements on bank financing in 27 European countries, suggesting that enforcement is a channel through which creditor rights work.[23] Based on that evidence, Ponticelli and Alencar (2016), Rodano et al. (2016), De Vito and Jacob (2023), and many others use enforcement proxies to sort their sample firms into treated and control firms to determine the causal effects of reform-induced credit rights changes. Relying on the unique twist that the EIR can shift a subsidiary's insolvency jurisdiction but not the enforcement procedures to which it is subject (i.e., it is still local enforcement authorities who execute measures against firms), we also test the validity of this strategy by rerunning regression (2) on subsamples of subsidiaries formed from the strength of their host countries' enforcement regimes and regression (3) on the full sample. We use the following variables to proxy for enforcement: (i) the number of days it takes the courts in a country to settle a business dispute case (*CourtTime*; Djankov et al. (2007)); (ii) the formality of its legal system (*LegalFormalism*; Djankov et al. (2003));[24] and (iii) its Worldbank enforcement score (*EnforcementScore*).[25]

Table 9 offers the enforcement subsample regression results. Columns (1) to (3) rely on *CourtTime*, (4) to (6) *LegalFormalism*, and (7) to (9) the *EnforcementScore* as enforcement proxy. Conversely, while columns (1) and (2) ((4) and (5)) [(7) and (8)] study above and below median enforcement subsamples, respectively, column (3) ((6)) [(9)] looks into the differences in the DID and DIDID effects between

[23] Other studies show that enforcement has separate debt-taking effects over and above creditor rights. The theoretical work of Jappelli et al. (2005), for example, suggests that stronger enforcement broadens debt access at each creditor rights level. Supporting their theory, they empirically document that bank lending is lower in Italian regions with greater backlogs of pending trials (see also Cristini et al. (2001) and Pinheiro and Cabral (2001)).

[24] Djankov et al.'s (2003) evidence suggests that more formal legal systems require longer to settle cases.

[25] We select the number of days to settle a business dispute rather those to settle an eviction or bounced check since it is more applicable in our setting. Notwithstanding, since the three duration variables are highly correlated across the EU-12 countries, the other two produce similar conclusions. Conversely, while we use the overall Worldbank enforcement score in our main tests, we also looked into its three underlying subscores, reflecting the time to resolve a commercial dispute, the cost of such a dispute, and the quality of the judicial process.

them. The table offers some weak evidence that strengthening creditor rights produce greater debt increases in stronger enforcement regimes. In particular, columns (1) and (2) ((7) and (8)) suggest that the debt of subsidiaries in low court time (high enforcement score) regimes rises by 0.4 (0.8) percent point more in response to improving creditor rights than that of subsidiaries in the opposite regimes. Digging deeper into the *EnforcementScore*, its entire positive effect comes from the subscore capturing the time to resolve a dispute and not those capturing the cost of a dispute and judicial quality (untabulated). In contrast, columns (4) and (5) reveal that the debt increase is 0.7 percent point lower in less formal regimes. Finally, columns (3), (6), and (9) suggest that the differences across the corresponding subsamples are always too small to be statistically significant.

TABLE 9 ABOUT HERE.

In comparison, the table offers no evidence suggesting that enforcement acts as a channel through which creditor rights deteriorations affect firms. To be more specific, the debt decreases induced through weakening creditor rights are marginally larger in the high *CourtTime* (columns (1) and (2)), high *LegalFormalism* ((5) and (6)), and low *EnforcementScore* ((7) and (8)) than in the corresponding opposite subsamples. Yet, as before, the differences across the corresponding subsamples are always too small to attract statistical significance (see columns (3), (6), and (9)).

An obvious concern about our enforcement subsample tests is that the strength of enforcement may not vary much within the EU, inducing the tests to have low statistical power. Yet, to our own surprise, this concern is largely unfounded. As can easily be verified, the variation of each of our enforcement proxies over the EU-12 countries is close to its global variation.

All in all, this section casts doubt on the idea that corporate distress risk and enforcement act as channels through which creditor rights affect firms. Although we find some weak albeit insignificant evidence that a high distress risk (strong enforcement) produces more negative (positive) debt effects in response to weakening (strengthening) creditor rights, we find no evidence that it produces more positive (negative) effects in response to strengthening (weakening) rights. The striking implication is that empirical law and finance studies exploiting intra-country legal reforms and using these two variables as identification contrast may yield significantly biased conclusions.

5.4 Creditor Rights-Versus-Confounding Factor Effects

A concern with our identification strategy is that our baseline results may be endogenously driven by confounding factors also varying with the EIR. We address this concern with several "horse-race specifications" (recall regression (5)). If our baseline results were indeed driven by these factors, we would expect their inclusion to absorb the effects of the CR index. We study two broad groups of

institutional confounding factors shaping a country's legal environment: its *culture* and *political institutions* (La Porta et al. (2008)). Culture includes the dominant religion and Hofstede's (2001) five cultural dimensions. The political institutions include the type of a country's electoral system, the degree of influence its left-wing political parties exert, and the degree of power its workers wilt (see the Internet Appendix for more details about the corresponding proxy variables).[26] A key advantage of our identification strategy is that it keeps constant the culture, religion, and political beliefs of all actors — except those of the courts. Thus, if these beliefs affect our conclusions, their influence must seep in through the court officials dealing with the insolvencies of our sample subsidiaries.

In Tables IA.5 and IA.6 in the Internet Appendix, we give the results from estimating regression (5) contrasting the effects of creditor rights changes with those of changes in each of the culture and political institution proxies (i.e., the k-variable) on the EU-12 country sample. The tables show that the debt-taking effects of creditor rights remain strong and robust across all specifications, while those of the other institutional characteristics are either insignificant or counterintuitive.

Overall, the evidence in this section mitigates concerns that the firm debt-taking effects spurred by the EIR-induced changes in creditor right in our main analysis could be endogenously driven by other formal institutions, such as the enforcement of the law, or informal institutions, such as culture and politics, a well-known critique of law and finance studies.

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

In this section, we run several standard robustness tests to corroborate the validity of our identification strategy. We first show that the parallel trends assumption holds in our setting. We next offer the results from placebo tests. We finally propensity-score (PS) match domestic and foreign subsidiaries, confirming that our main and PS-matched samples produce identical conclusions. Section IA5 in the Internet Appendix provides more details about the empirical tests in this section.

A condition for DIDID tests to yield valid inferences is that the treated and controls display similar trends in outcome variables before the shock ("parallel-trends condition"). We offer two pieces of evidence related to this requirement. Figure 2 shows 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 also offer more formal evidence in Table IA.7 in the Internet Appendix. While Panel A contrasts the change in total debt and the (non-averaged) controls over each pre-EIR year across domestic and foreign subsidiaries in

[26] Although Botero et al. (2004) and Pagano and Volpin (2005) differ in their account of the underlying causal mechanism, both document a systematic empirical association between a country's electoral system, the political power of the left, and the extent of labor market regulations. We rely on the associations reported by them to group the three variables together, without taking a stance on its origin. Doing so goes beyond the scope our paper.

the EU-12 country sample, Panel B (C) runs the analogous comparison across foreign subsidiaries with parents in countries with stronger (weaker) creditor protection than their host countries and those with parents in countries with similar rights. Panel A suggests only small and non-systematic differences in the changes in the variables across domestic and foreign subsidiaries. More critically for our purposes, Panels B and C point to even smaller differences in the changes across foreign subsidiaries with parents in countries with different levels of creditor protection.

Table IA.8 presents the results from estimating placebo versions of regression (2) using total debt as dependent variable on all EU-12 subsidiaries. For convenience, column (1) reports the same results as in Table 1. We first consider as treated only those foreign subsidiaries with parents in Denmark or Norway, that is, European countries not affected by the EIR. Second, we create a sample with only non-fully-owned subsidiaries, where, to be conservative, we define those as subsidiaries in which the parent's stake is less than 80%. Third, we randomly reshuffle the parent countries of the foreign (but not domestic) subsidiaries in our sample. Across all different placebo samples, Table IA.8 shows insignificant effects and in some cases the wrong signs (e.g., columns (2) and (3)).

In Table IA.9 in the Internet Appendix, we finally re-estimate our regression in Table 1 on a PS-matched sample of domestic and foreign subsidiaries, to further rule out that the pre-EIR differences in Table IA.7 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. The table shows that estimates on the PS-matched samples are consistent with those of our main specifications, despite the significantly smaller sample size.

Taken together, the evidence in this section mitigates concerns that our main debt-taking conclusions may reflect some pre-existing and/or alternative effect.

7 Conclusion

Motivated by the limitations of existing studies, we exploit 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 also observe that these changes are mainly driven by short-term debt, and that borrowing costs increase upon a shift toward weaker creditor rights. Interestingly, further tests reveal that all single rights making up the CR index have the predicted debt-taking effects, although

the effects from granting and withdrawing the same right can be highly asymmetric. Differing from recent claims in the literature, we finally demonstrate that the CR index seems to be a close to holistic measure of all insolvency law aspects which are of relevance to real world creditors.

We also offer evidence that the EIR-induced debt effects arise through a bank-based supply-side mechanism. Supporting a supply-side mechanism, we show that our sample subsidiaries use the greater (reduced) debt-taking to re-optimize their financing mixes but not to adjust their real (e.g., investment) policies. Moreover, our debt-taking evidence survives granular industry-location-time fixed effects used in the literature to absorb demand effects. Supporting a bank-based mechanism, we use the subsample of U.K. subsidiaries to (i) show that only bank (but not group and leasing) debt reacts to the reform; and (ii) demonstrate that bank characteristics modulate our results. Strikingly, we also reveal that corporate distress risk and country-level enforcement do not strongly condition the debt effects induced through the EIR-driven creditor rights variations, casting doubt on the identification strategies used in other empirical law and finance studies exploiting intra-country reforms. We finally mitigate concerns that creditor rights may endogenously reflect other confounding factors by show that controlling for these does not greatly alter our main conclusions.

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Table 1. The Debt-Financing Effects of the EIR: Debt-Taking

This table reports the results from estimating the regression:

$$Debt_{i,s,t} = \beta_1(Treat_{i,s} \times Shock_t \times Up^{CRI}_{i,s}) + \beta_2(Treat_{i,s} \times Shock_t \times Down^{CRI}_{i,s}) + \beta_3(Treat_{i,s} \times Shock_t) + \sum_{s\in S}\sum_{y\in Y}\sum_{c\in C}\gamma_{s,y,c}(DState^s_i \times DYear^y_t \times MCon^c_i) + \alpha_i + \alpha_t + \epsilon_{i,s,t},$$

where *Debt* is the total debt of subsidiary i in member state s at the end of fiscal year t. *Treat* is a dummy equal to one for foreign subsidiaries and else zero. We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. *Shock* is a dummy equal to one starting from 2002 to 2006 and zero between 1997 and 2001. Up^{CRI} (Down^{CRI}) is a dummy 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 (lower) than its host country (see Table IA.2 in the Internet Appendix for the index values). S, Y, and C are the different sets of EU member states, all years, and all our control variables in our specification, respectively. $DState^s$ is a dummy equal to one if a subsidiary is registered in state s and else zero, $DYear^y$ 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. 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.). α_i and α_t are static subsidiary (Firm FE) and year (Time FE) fixed effects, respectively. We cluster standard errors by subsidiary (Cluster Firm). Plain numbers are estimates, whereas t-statistics are in square brackets. 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. R^2). We offer more details on how we construct the analysis variables in Table A.1 in the appendix of this paper.

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

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

Table 2. The Debt-Financing Effects of the EIR: Debt-Contracting Terms

This table reports the results from estimating the regression:

$$Debt_{i,s,t} = \beta_1(Treat_{i,s} \times Shock_t \times Up^{CRI}_{i,s}) + \beta_2(Treat_{i,s} \times Shock_t \times Down^{CRI}_{i,s}) + \beta_3(Treat_{i,s} \times Shock_t) + \sum_{s\in S}\sum_{y\in Y}\sum_{c\in C}\gamma_{s,y,c}(DState^s_i \times DYear^y_t \times MCon^c_i) + \alpha_i + \alpha_t + \epsilon_{i,s,t},$$

where *Debt* is the total (for comparison; column (1)), short-term ((2)), and long-term ((3)) debt and the interest-to-debt ratio ((4)) of subsidiary i in member state s at the end of fiscal year t. *Treat* is a dummy equal to one for foreign subsidiaries and else zero. We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. *Shock* is a dummy equal to one starting from 2002 to 2006 and zero between 1997 and 2001. Up^{CRI} ($Down^{CRI}$) is a dummy 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 (lower) than its host country (see Table IA.2 in the Internet Appendix for the index values). The triple sum is constructed analogously to that used in Table 1 (Control Int.). α_i and α_t are static subsidiary (Firm FE) and year (Time FE) fixed effects, respectively. We cluster standard errors by subsidiary (Cluster Firm). Plain numbers are estimates, whereas t-statistics are in square brackets. 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. R^2). We offer more details on how we construct the analysis variables in Table A.1 in the appendix of this paper.

	All Countries			
	Total Debt (Repeated)	Short-Term Debt	Long-Term Debt	Interest-to-Debt Ratio
	(1)	(2)	(3)	(4)
$Treat \times Shock \times Up^{CRI}$	0.019***	0.016***	0.001	0.003
	(3.112)	(3.240)	(0.307)	(0.077)
$Treat \times Shock \times Down^{CRI}$	–0.029***	–0.018**	–0.009	0.033**
	(–3.266)	(–2.410)	(–1.548)	(2.153)
$Treat \times Shock$	–0.013***	–0.013***	0.002	0.002
	(–2.674)	(–3.214)	(0.585)	(0.163)
Control Int.	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Cluster Firm	✓	✓	✓	✓
Observations	95,935	95,935	95,935	54,855
Treated Obs.	23,717	23,717	23,717	15,082
Treated Obs. (SR)	5,882	5,882	5,882	3,251
Treated Obs. (WR)	5,880	5,880	5,880	3,959
Adj. R^2	73.9%	72.9%	63.8%	45.8%

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

Table 3. The Debt-Financing Effects of the EIR: Single Creditor-Rights Dimensions

This table reports the results from estimating the regression:

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

where *Debt* is the total debt of subsidiary i in member state s at the end of fiscal year t. *Treat* is a dummy equal to one for foreign subsidiaries and else zero. We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. *Shock* is a dummy equal to one starting from 2002 to 2006 and zero between 1997 and 2001. On^x (Off^x) is a dummy variable equal to one if a subsidiary's host country does not (does) grant right x but its parent's host country does (does not). R is a set including the rights underlying the CR index, namely: (i) secured creditors are paid first (SPF); (ii) creditors need to give their consent to reorganize (CC); (iii) managers do not stay in place (MR); and (iv) there is no automatic stay on assets (NOS). The triple sum is constructed analogously to that used in Table 1 (Control Int.). α_i and α_t are static subsidiary (Firm FE) and year (Time FE) fixed effects, respectively. We cluster standard errors by subsidiary (Cluster Firm). Plain numbers are estimates, whereas t-statistics are in square brackets. The bottom rows report the number of observations (Observations), the number of treated (Treated Obs.), granting-rights-country treated (Treated Obs. (ON)), and withdrawing-rights-country treated (Treated Obs. (OFF)) observations, and the adjusted R-squared (Adj. R^2). We offer more details on how we construct the analysis variables in Table A.1 in the appendix of this paper.

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

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Table 3. The Debt-Financing Effects of the EIR: Single Creditor-Rights Dimensions (cont.)

	Creditor Rights Index Subcomponent					
	Secured Creditors Paid First (SPF)	Creditor Consent to Reorganize (CC)	Managers Do Not Stay in Place (MR)	No Auto. Stay (NOS)	SPF, CC, and NOS	All Together
	(1)	(2)	(3)	(4)	(5)	(6)
Control Int.	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
Cluster Firm	✓	✓	✓	✓	✓	✓
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. 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 4. The Effects of the EIR: Real and Non-Debt Financing Policies

This table reports the results from estimating the regression:

$$Y_{i,s,t} = \beta_1(Treat_{i,s} \times Shock_t \times Up^{CRI}_{i,s}) + \beta_2(Treat_{i,s} \times Shock_t \times Down^{CRI}_{i,s}) + \beta_3(Treat_{i,s} \times Shock_t) + \sum_{s\in S}\sum_{y\in Y}\sum_{c\in C}\gamma_{s,y,c}(DState^s_i \times DYear^y_t \times MCon^c_i) + \alpha_i + \alpha_t + \epsilon_{i,s,t},$$

where Y includes the trade credit, other current liabilities, other non-current liabilities, and shareholder funds (Panel A) as well as the tangible asset growth, working capital growth, worker growth, and cash growth (Panel B) of subsidiary i in member state s at the end of fiscal year t. *Treat* is a dummy equal to one for foreign subsidiaries and else zero. We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. *Shock* is a dummy equal to one starting from 2002 to 2006 and zero between 1997 and 2001. Up^{CRI} ($Down^{CRI}$) is a dummy 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 (lower) than its host country (see Table IA.2 in the Internet Appendix for the index values). The triple sum is constructed analogously to that used in Table 1 (Control Int.). α_i and α_t are static subsidiary (Firm FE) and year (Time FE) fixed effects, respectively. We cluster standard errors by subsidiary (Cluster Firm). Plain numbers are estimates, whereas t-statistics are in square brackets. 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. R^2). We offer more details on how we construct the analysis variables in Table A.1 in the appendix of this paper.

Panel A: Non-Debt Financing Variables

	Trade Credit	Other Current Liabilities	Other Non-Current Liabilities	Shareholder Funds
	(1)	(2)	(3)	(4)
Treat × *Shock* × Up^{CRI}	–0.009**	–0.005	–0.011***	0.011
	(–2.124)	(–0.880)	(–3.015)	(1.473)
Treat × *Shock* × $Down^{CRI}$	–0.002	0.009*	0.006**	0.008
	(–0.504)	(1.853)	(2.030)	(0.992)
Treat × *Shock*	0.002	0.005	0.003	0.006
	(0.838)	(1.445)	(1.489)	(1.351)
Control Int.	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Cluster Firm	✓	✓	✓	✓
Observations	105,757	105,923	93,907	107,666
Treated Obs.	27,375	27,442	24,184	27,816
Treated Obs. (SR)	6,396	6,410	5,854	6,431
Treated Obs. (WR)	6,642	6,663	5,738	6,842
Adj. R^2	77.7%	68.3%	71.0%	70.9%

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Table 4. The Effects of the EIR: Real and Non-Debt Financing Policies (cont.)

Panel B: Investment Variables				
	Tangible Asset Growth	Working Capital Growth	Worker Growth	Cash Growth
	(1)	(2)	(3)	(4)
$Treat \times Shock \times Up^{CRI}$	0.030	–0.008	–0.131	–0.008
	(1.505)	(–1.476)	(–1.609)	(–0.516)
$Treat \times Shock \times Down^{CRI}$	–0.041	–0.001	0.093	–0.031
	(–1.247)	(–0.154)	(0.669)	(–1.098)
$Treat \times Shock$	–0.021	–0.003	–0.061	0.021
	(–0.636)	(–0.394)	(–0.457)	(0.795)
Control Int.	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Cluster Firm	✓	✓	✓	✓
Observations	83,166	80,915	82,431	85,819
Treated Obs.	21,818	20,702	21,709	22,066
Treated Obs. (SR)	5,110	4,522	5,208	5,283
Treated Obs. (WR)	5,227	5,109	5,130	5,214
Adj. R^2	36.3%	15.5%	1.8%	3.1%

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

Table 5. The Debt-Financing Effects of the EIR: Alternative Fixed Effects

This table reports the results from estimating the regression:

$$Debt_{i,s,t} = \beta_1(Treat_{i,s} \times Shock_t \times Up^{CRI}_{i,s}) + \beta_2(Treat_{i,s} \times Shock_t \times Down^{CRI}_{i,s}) + \beta_3(Treat_{i,s} \times Shock_t) + \sum_{s\in S}\sum_{y\in Y}\sum_{c\in C}\gamma_{s,y,c}(DState^s_i \times DYear^y_t \times MCon^c_i) + \mathbf{Z} + \epsilon_{i,s,t},$$

where *Debt* is the total debt of subsidiary i in member state s at the end of fiscal year t. *Treat* is a dummy equal to one for foreign subsidiaries and else zero. We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. *Shock* is a dummy equal to one starting from 2002 to 2006 and zero between 1997 and 2001. Up^{CRI} ($Down^{CRI}$) is a dummy 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 (lower) than its host country (see Table IA.2 in the Internet Appendix for the index values). The triple sum is constructed analogously to that used in Table 1 (Control Int.). Z is a matrix of different fixed effects sets depending on the specification: Static firm (i) and year (t); static firm (i) and dynamic industry-year (jt); static firm (i) and dynamic industry-region-year (jgt); static firm (i) and dynamic parent-year (pt); and static firm (i), dynamic industry-year (jt), and dynamic parent-year (pt) fixed effects in columns (1) to (5), respectively. We use the two-digit US SIC code to define industries and the two digit NUTS code to define regions. We cluster standard errors by subsidiary (Cluster Firm). Plain numbers are estimates, whereas t-statistics are in square brackets. 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. R^2). We offer more details on how we construct the analysis variables in Table A.1 in the appendix of this paper.

	Fixed Effects Specification				
	Static Firm-Time (Repeated)	Static Firm + Dynamic Ind.-Time	Static Firm + Dynamic Ind.-Region-Time	Static Firm + Dynamic Parent-Time	Static Firm + Dynamic Ind.-Time + Dynamic Parent-Time
	(1)	(2)	(3)	(4)	(5)
Treat × *Shock* × Up^{CRI}	0.019***	0.020***	0.023***	0.040*	0.047**
	(3.112)	(3.136)	(3.299)	(1.847)	(2.080)
Treat × *Shock* × $Down^{CRI}$	–0.029***	–0.030***	–0.036***	–0.060***	–0.060***
	(–3.266)	(–3.318)	(–3.102)	(–2.678)	(–2.607)
Treat × *Shock*	–0.013***	–0.012**	–0.015***	–0.001	–0.003
	(–2.674)	(–2.358)	(–2.747)	(–0.075)	(–0.271)
Control Int.	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓
Time FE	✓				
Industry-Time FE		✓			✓
Industry-Region-Time FE			✓		
Parent-Time FE				✓	✓
Cluster Firm	✓	✓	✓	✓	✓
Observations	95,935	92,210	79,707	49,706	47,339
Treated Obs.	23,717	22,931	19,633	15,618	14,997
Treated Obs. (SR)	5,882	5,702	5,093	3,651	3,527
Treated Obs. (WR)	5,880	5,706	4,582	4,250	4,094
Adj. R^2	0.739	0.741	0.771	0.768	0.768

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

Table 6. The Debt-Financing Effects of the EIR: Sources of Debt

This table reports the results from estimating the following regression on only U.K. subsidiaries:

$$Debt_{i,s,t} = \beta_1(Treat_{i,s} \times Shock_t) \quad + \sum_{y \in Y} \sum_{c \in C} \gamma_{y,c}(DYear_t^y \times MCon_i^c) + \alpha_i + \alpha_t + \epsilon_{i,t},$$

where *Debt* is either the bank, group, net group, and leasing debt of U.K. subsidiary i at the end of fiscal year t. *Treat* is a dummy equal to one for foreign subsidiaries and else zero. We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. *Shock* is a dummy equal to one starting from 2002 to 2006 and zero between 1997 and 2001. The double sum is constructed analogously to that used in Table 1 (Control Int.). α_i and α_t are static subsidiary (Firm FE) and year (Time FE) fixed effects, respectively. We cluster standard errors by subsidiary (Cluster Firm). Plain numbers are estimates, whereas t-statistics are in square brackets. The bottom rows report the number of observations (Observations), the number of treated (Treated Obs.), and the adjusted R-squared (Adj. R^2). We offer more details on how we construct the analysis variables in Table A.1 in the appendix of this paper.

	Debt Component Variable			
	Bank Debt	Group Debt	Net Group Debt	Leasing Debt
	(1)	(2)	(3)	(4)
Treat × *Shock*	–0.020**	–0.001	–0.004	0.002
	(–2.153)	(–0.044)	(–0.251)	(0.784)
Control Int.	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Cluster Firm	✓	✓	✓	✓
Observations	5,505	5,505	5,492	5,505
Treated Obs.	1,801	1,801	1,800	1,801
Adj. R^2	62.6%	68.9%	67.8%	78.4%

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

Table 7. The Debt-Financing Effects of the EIR: The Role of Banks

This table reports the results from estimating the following regression on the subsample of U.K. subsidiaries whose main bank's financial strength or lending experience is either above or below the median:

$$Debt_{i,s,t} = \beta_1(Treat_{i,s} \times Shock_t) \quad + \sum_{y \in Y} \sum_{c \in C} \gamma_{y,c}(DYear_t^y \times MCon_i^c) + \alpha_i + \alpha_t + \epsilon_{i,t},$$

and the following regression on the full sample of U.K. subsidiaries:

$$Debt_{i,s,t} = \boldsymbol{\beta_1'}(Treat_{i,s} \times Shock_t \times \mathbf{HBC}_i) \quad + \sum_{y \in Y} \sum_{b \in B} \sum_{c \in C} \gamma_{y,b,c}(DYear_t^y \times DBC_t^b \times MCon_i^c) + \alpha_i + \alpha_t + \epsilon_{i,t},$$

where *Debt* is the total debt of U.K. subsidiary i at the end of fiscal year t. *Treat* is a dummy equal to one for foreign subsidiaries and else zero. We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. *Shock* is a dummy equal to one starting from 2002 to 2006 and zero between 1997 and 2001. **HBC** is a vector containing one and a dummy variable equal to one if a subsidiary's main-bank characteristic is above the median and else zero. The double/triple sum is constructed analogously to that used in Table 1 (Control Int.). α_i and α_t are static subsidiary (Firm FE) and year (Time FE) fixed effects, respectively. The bank characteristics 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 (Lending Activity). We always cluster standard errors by subsidiary (Cluster Firm). Plain numbers are estimates, whereas t-statistics are in square brackets. The bottom rows report the number of observations (Observations), the number of treated observations (Treated Obs.), and the adjusted R-squared (Adj. R^2). We offer more details on how we construct the analysis variables in Table A.1 in the appendix of this paper.

	Bank Characteristic								
	Equity-to-Assets			ROA			Lending Activity		
	High	Low	Both	High	Low	Both	High	Low	Both
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treat × *Shock*	–0.021	–0.066***	–0.066***	–0.012	–0.062***	–0.062***	–0.017	–0.064***	–0.064***
	(–0.973)	(–4.142)	(–4.137)	(–0.707)	(–3.959)	(–3.957)	(–0.997)	(–3.987)	(–3.986)
Treat × *Shock* × *HBC*			0.045*			0.050**			0.046**
			(1.718)			(2.109)			(1.968)
Control Int.	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cluster Firm	✓	✓	✓	✓	✓	✓	✓	✓	✓
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 8. The Debt-Taking Effects of the EIR: The Role of Distress Risk

This table reports the results from estimating the following regression on distress-risk-tercile subsamples:

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

and the following regression on the pooled top-and-bottom distress-risk-tercile subsamples:

$$Debt_{i,s,t} = \boldsymbol{\beta_1'}(Treat_{i,s} \times Shock_t \times Up_{i,s}^{CRI} \times \mathbf{HD}_{i,s}) + \boldsymbol{\beta_2'}(Treat_{i,s} \times Shock_t \times Down_{i,s}^{CRI} \times \mathbf{HD}_{i,s}) + \boldsymbol{\beta_3'}(Treat_{i,s} \times Shock_t \times \mathbf{HD}_{i,s}) + \sum_{s\in S}\sum_{y\in Y}\sum_{d\in D}\sum_{c\in C}\gamma_{s,y,d,c}(DState_i^s \times DYear_t^y \times DDT_i^d \times MCon_i^c) + \alpha_i + \alpha_t + \epsilon_{i,s,t},$$

where *Debt* is the total debt of subsidiary i in member state s at the end of fiscal year t. *Treat* is a dummy equal to one for foreign subsidiaries and else zero. We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. *Shock* is a dummy equal to one starting from 2002 to 2006 and zero between 1997 and 2001. Up^{CRI} ($Down^{CRI}$) is a dummy 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 (lower) than its host country (see Table IA.2 in the Internet Appendix for the index values). **HD** is a vector containing one and a dummy variable equal to one if a subsidiary's distress risk is in the top tercile and else zero. We measure distress risk using either the original Altman (1983) z'-score (columns (1) to (4)) or a version of it re-estimated on our data ((5) to (8)). The triple/quadruple sum is constructed analogously to that used in Table 1 (Control Int.). α_i and α_t are static subsidiary (Firm FE) and year (Time FE) fixed effects, respectively. We cluster standard errors by subsidiary (Cluster Firm). Plain numbers are estimates, whereas *t*-statistics are in square brackets. 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. R^2). We offer more details on how we construct the analysis variables in Table A.1 in the appendix of this paper.

	(–1) × Original Z'-Score				(–1) × Re-Estimated Z'-Score			
	Top Tercile	Middle Tercile	Bottom Tercile	Top & Bottom	Top Tercile	Middle Tercile	Bottom Tercile	Top & Bottom
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × *Shock* × Up^{CRI}	0.014	0.021**	0.017*	0.017*	0.012	0.029***	0.006	0.006
	(1.112)	(2.136)	(1.713)	(1.713)	(0.996)	(2.824)	(0.628)	(0.628)
Treat × *Shock* × $Down^{CRI}$	–0.044***	–0.030**	–0.011	–0.011	–0.030*	–0.032**	–0.026*	–0.026*
	(–2.738)	(–2.231)	(–0.699)	(–0.699)	(–1.946)	(–2.166)	(–1.767)	(–1.767)
Treat × *Shock*	–0.016*	–0.009	–0.011	–0.011	–0.010	–0.014*	–0.005	–0.005
	(–1.712)	(–1.207)	(–1.367)	(–1.367)	(–1.021)	(–1.914)	(–0.572)	(–0.572)
Treat × *Shock* × Up^{CRI} × *HD*				–0.003				0.007
				(–0.190)				(0.446)
Treat × *Shock* × $Down^{CRI}$ × *HD*				–0.032				–0.004
				(–1.417)				(–0.201)
Treat × *Shock* × *HD*				–0.005				–0.005
				(–0.406)				(–0.421)
Control Int.	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Cluster Firm	✓	✓	✓	✓	✓	✓	✓	✓
Observations	31,311	30,684	30,379	61,690	31,743	30,379	30,384	62,127
Treated Obs.	1,925	2,128	1,774	3,699	2,337	1,870	1,620	3,957
Treated Obs. (SR)	4,081	4,201	3,399	7,480	4,804	3,683	3,213	8,017
Treated Obs. (WR)	2,333	1,956	1,403	3,736	2,578	1,745	1,374	3,952
Adj. R2	75.1%	3.7%	65.6%	75.1%	4.1%	72.1%	62.9%	4.0%

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

Table 9. The Debt-Taking Effects of the EIR: The Role of Enforcement

This table reports the results from estimating the following regression on subsidiaries registered in host countries with an either above or below median enforcement proxy variable value:

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

and the following regression on the full sample of subsidiaries:

$$Debt_{i,s,t} = \boldsymbol{\beta_1'}(Treat_{i,s} \times Shock_t \times Up_{i,s}^{CRI} \times \mathbf{HE}_{i,s}) + \boldsymbol{\beta_2'}(Treat_{i,s} \times Shock_t \times Down_{i,s}^{CRI} \times \mathbf{HE}_{i,s}) + \boldsymbol{\beta_3'}(Treat_{i,s} \times Shock_t \times \mathbf{HE}_{i,s}) + \sum_{s\in S}\sum_{y\in Y}\sum_{e\in E}\sum_{c\in C}\gamma_{s,y,e,c}(DState_i^s \times DYear_t^y \times DE_i^e MCon_i^c) + \alpha_i + \alpha_t + \epsilon_{i,s,t},$$

where *Debt* is the total debt of subsidiary i in member state s at the end of fiscal year t. *Treat* is a dummy equal to one for foreign subsidiaries and else zero. We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. *Shock* is a dummy equal to one starting from 2002 to 2006 and zero between 1997 and 2001. Up^{CRI} (Down^{CRI}) is a dummy 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 (lower) than its host country (see Table IA.2 in the Internet Appendix for the index values). **HE** is a vector containing one and a dummy variable equal to one if a subsidiary's host-country enforcement proxy variable value is above the median and else zero. The enforcement variables are the total duration for enforcing a business dispute in a country ("Court Time"), a measure of the country's legal formalism ("Legal Formalism"), and its Worldbank strength-of-enforcement index ("WB Enforcement Index"). The triple/quadruple sum is constructed analogously to that used in Table 1 (Control Int.). α_i and α_t are static subsidiary (Firm FE) and year (Time FE) fixed effects, respectively. We cluster standard errors by subsidiary (Cluster Firm). Plain numbers are estimates, whereas *t*-statistics are in square brackets. 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. R^2). We offer more details on how we construct the analysis variables in Table A.1 in the appendix of this paper.

	Enforcement Variable								
	Court Time			Legal Formalism			WB Enforcement Score		
	Low	High	Both	Low	High	Both	High	Low	Both
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treat×*Shock*×Up^{CRI}	0.017	0.013*	0.013*	0.008	0.015*	0.015*	0.018**	0.010	0.010
	(1.642)	(1.780)	(1.780)	(0.904)	(1.777)	(1.777)	(2.104)	(1.254)	(1.254)
Treat×*Shock*×$Down^{CRI}$	–0.024*	–0.029***	–0.029***	–0.025**	–0.031**	–0.031**	–0.020*	–0.030***	–0.030***
	(–1.923)	(–2.678)	(–2.678)	(–2.314)	(–2.281)	(–2.281)	(–1.690)	(–2.698)	(–2.698)
Treat×*Shock*	–0.005	–0.016***	–0.016***	–0.020***	–0.001	–0.001	–0.005	–0.017***	–0.017***
	(–0.622)	(–2.696)	(–2.696)	(–3.020)	(–0.089)	(–0.089)	(–0.783)	(–2.647)	(–2.647)
Treat×*Shock*×Up^{CRI}×*HE*			0.004			–0.007			0.008
			(0.286)			(–0.572)			(0.654)
Treat×*Shock*×$Down^{CRI}$×*HE*			0.005			0.006			0.010
			(0.274)			(0.342)			(0.598)
Treat×*Shock*×*HE*			0.011			–0.019**			0.012
			(1.040)			(–2.039)			(1.235)
Control Int.	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cluster Firm	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	42,656	53,279	95,935	55,119	40,816	95,935	39,469	56,466	95,935
Treated Obs	3,729	2,153	5,882	1,631	4,251	5,882	3,966	1,916	5,882
Treated Obs. (SR)	5,190	6,765	11,955	6,695	5,260	11,955	4,250	7,705	11,955
Treated Obs (WR)	1,979	3,901	5,880	4,167	1,713	5,880	1,449	4,431	5,880
Adj. R2	61.9%	77.2%	73.9%	77.8%	60.2%	73.9%	58.5%	76.8%	73.9%

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

Appendix A Variable Definitions

Table A.1. Variable Definitions

This table reports the names and definitions of our analysis variables. Panel A focuses on the AMADEUS (Panel A.1) and FAME (Panel A.2) subsidiary-level variables as well as the BANKSCOPE (Panel A.3) bank-level variables. Conversely, Panel B concentrates on the country-level Andrei Shleifer creditor protection and enforcement variables (Panel B.1), Gert Hofstede's cultural dimensions (Panel B.2), and Andrei Shleifer political institution variables (Panel B.3).

Variable	Definition
Panel A: AMADEUS, FAME, and BANKSCOPE Variables	
Panel A.1: AMADEUS Subsidiary Variables:	
TotalDebt	Sum of short (loan) and long (ltdb) term debt scaled by assets (at).
ShortTermDebt	Short-term debt (loan) scaled by assets (at).
LongTermDebt	Long-term debt (ltdb) scaled by assets (at).
InterestToDebt	Interest expense (inte) scaled by one-year-lagged total debt (loan+ltdb).
TradeCredit	Accounts payable (cred) scaled by assets (at).
OtherCurrentLiabilities	Other current liabilities (oncl) scaled by assets (at).
OtherNonCurrentLiabilities	Other non-current liabilities (ocli) scaled by assets (at).
ShareholderFunds	Shareholder funds (shfd) scaled by assets (at).
TangibleAssetGrowth	End-minus-start fiscal year tangible fixed assets (tfas) plus depreciation (depr) scaled by start-year tangible fixed assets (tfas).
WorkingCapitalGrowth	Annual percent change in working capital, where working capital is inventories (stok) plus accounts receivable (debt) minus accounts payable (cred).
WorkerGrowth	Annual percent change in the number of workers (empl).
CashGrowth	Annual percent change in cash reserves (cash).
Size	Natural log of assets (at).
FirmAge	Difference between account date and date of incorporation, in years.
Employees	Natural log of the number of employees (empl).
Profitability	Earnings before interest and taxes (ebta) scaled by assets (at).
FixedAssets	Tangible fixed assets (tfas) scaled by assets (at).
CashHoldings	Cash (cash) scaled by assets (at).
Panel A.2: FAME Subsidiary Variables:	
BankDebt	Sum of bank overdrafts and short and long-term other debt scaled by assets.
GroupDebt	Sum of short and long-term group debt scaled by assets.
NetGroupDebt	Short plus long-term group debt minus group debt assets scaled by assets.
LeasingDebt	Sum of short and long-term leasing debt scaled by assets.
Panel A.3: BANKSCOPE Bank Variables:	
EquityToAssets	Equity scaled by assets
ReturnonAssets (*ROA*)	Sum of pre-tax income and loan loss provisions scaled by assets.
LendingActivity	Interest income scaled by operating income.

(continued on next page)

Table A.1. Variable Definitions (cont.)

Variable	Definition
Panel B: Andrei Shleifer and Gert Hofstede Country Variables	
Panel B.1: Andrei Shleifer Credit Rights and Enforcement Variables:	
CRIndex	Creditor rights index of La Porta et al. (1998) and Djankov et al. (2007), constructed as the sum of four subindexes based on whether: (i) secured creditors are paid first (SPF); (ii) creditors need to give their consent to reorganize (CC); (iii) managers do not stay in place (MR); and (iv) there is no automatic stay on assets (NOS). The index ranges from 0 to 4.
CourtTime	The number of days to settle a business dispute from Djankov et al. (2003).
LegalFormalism	Formalism index of Djankov et al. (2003) measuring substantive and procedural statutory intervention in judicial cases at lower-level civil trial courts. The index ranges from 0 to 7, with 7 indicating a higher level of control or intervention in the judicial process.
EnforcementScore	The Worldbank's 2003 overall enforcement score for a country, comprised of the subscores for its time to resolve a commercial dispute, the costs of resolving such a dispute, and the overall quality of judicial processes.
Catholic	Dummy variable equal to one if the majority of a country's population practices the catholic religion and else zero.
Panel B.2: Gert Hofstede Culture Variables:	
PowerDistance	The extent to which the less powerful members of organizations and institutions (like the family) accept and expect that power is distributed unequally.
Individualism	The extent to which people feel independent, as opposed to being interdependent as members of larger wholes.
Masculinity	The extent to which the use of force is endorsed socially.
UncertaintyAvoidance	A society's tolerance for uncertainty and ambiguity.
LongTermOrientation	The extent to which a society focuses more on the long than short-term future.
Panel B.3: Andrei Shleifer Political Institutions Variables:	
ProportionalElectionSystem	A dummy variable equal to one if legislators were elected based on the percentage of votes received by their party, else zero. This variables is measured as the average from 1975 through 1995 as provided by Beck et al. (2001).
LeftPower	The share of years over which the chief executive and largest party in power have left or center political orientations.
UnionDensity	The percentage of the total workforce affiliated to labor unions in 1997 as provided by Botero et al. (2004).
StrengthLaborLaws	The employment laws index of Botero et al. (2004) measuring the protection of labor and employment laws as the average of subindexes based on: (1) alternative employment contracts; (2) cost of increasing hours worked; (3) cost of firing workers; and (4) dismissal procedures.

Internet Appendix

IA1 Landmark Rulings on the COMIs of Multinationals

In Table IA.1, we offer details on noteworthy rulings of EU courts on the COMIs of multinational business group members. We extract those rulings from Chapter 30 of Wood (2007). The table shows the case names, judgment dates, the registration countries of the parents and their subsidiaries, the court's COMI decision, and whether the COMI was shifted from subsidiaries' registration countries to those of their parents. In the final column, we summarize the courts' reasoning for their judgments. We discuss those rulings in greater detail in Section 2.2 of our main paper.

TABLE IA.1 ABOUT HERE.

IA2 Sample Composition and Descriptive Statistics

In Tables IA.2 and IA.3, we show the host-parent country composition of our EU12 subsidiary sample and offer descriptive statistics on our main analysis variables, respectively. Table 1 in our main paper gives a graphical representation of the content of Table IA.2. We discuss both our sample composition and descriptive statistics in greater detail in Section 4.1 of our main paper.

TABLE IA.2 ABOUT HERE.

TABLE IA.3 ABOUT HERE.

IA3 The Debt-Taking Effects of the EIR in the U.K.

In Section 5 in our main paper, we exploit the U.K subsidiaries subsample for which we can retrieve more granular information on the sources of debt and the identity of housebanks. Before performing those tests, we however confirm that our main debt-taking results also hold for that subsample. As such, we modify our benchmark regression (2) in our main paper using a combination of DID and DIDID estimators, so that we can apply it to the U.K. We start with the following DID regression:

$$Debt_{i,t} = \beta(Treat_i \times Shock_t) + \sum_{y \in Y} \sum_{c \in C} \gamma_{y,c}(DYear_t^y \times MCon_i^c) + \alpha_i + \alpha_t + \epsilon_{i,t}, \tag{IA3.1}$$

where *Debt* is total debt of U.K. subsidiary i at the end of fiscal year t, *Treat* and *Shock* are defined in the main text, 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 as in the main specification. The main advantage of running regression (IA3.1) on U.K. subsidiaries is that under the assumption that the U.K. grants the strongest legal protection to creditors in the EU (see, e.g., La Porta et al. (1998)), the creditors of all foreign subsidiaries — independent of their parent country — should be adversely affected by the EIR. As such, we expect β, our parameter of interest, to be negative.

Even if the above assumption is correct, foreign U.K. subsidiaries with parents in countries with much weaker creditor rights than the U.K. should be more negatively affected than those with parents in countries with more similar rights. To test this hypothesis, we estimate:

$$\begin{aligned} Debt_{i,t} &= \beta_1(Treat_i \times Shock_t \times Similar_i^{CRI}) + \beta_2(Treat_i \times Shock_t \times Down_i^{CRI}) \\ &+ \sum_{y \in Y} \sum_{c \in C} \gamma_{y,c}(DYear_t^y \times MCon_i^c) + \alpha_i + \alpha_t + \epsilon_{i,t}, \end{aligned} \tag{IA3.2}$$

where we expect β_1 to be smaller in magnitude than β_2. In essence, equation (IA3.3) decomposes the DID effect from equation (IA3.1) into its "subeffects" originating from regimes similar and those different from the U.K. (the effects on $Similar^{CRI}$ and $Different^{CRI}$, respectively).

To see whether the debt-taking effects across the two types of creditor protection regimes are statistically different from one another, we also run the DIDID regression:

$$\begin{aligned} Debt_{i,t} &= \beta_1(Treat_i \times Shock_t) + \beta_2(Treat_i \times Shock_t \times Down_i^{CRI}) \\ &+ \sum_{y \in Y} \sum_{c \in C} \gamma_{y,c}(DYear_t^y \times MCon_i^c) + \alpha_i + \alpha_t + \epsilon_{i,t}, \end{aligned} \tag{IA3.3}$$

where β_1 now captures the *creditor-rights-unrelated* effect, as in our benchmark regression.

Table IA.4 offers the results from estimating the three type of regressions. Across all specifications, foreign U.K. subsidiaries significantly lowered their debt in response to the EIR. For instance, those subsidiaries lowered their total debt by 3.7 percent point (t-statistic: 3.65) relative to domestic ones from pre to post-EIR (column (1)), representing 10% of average U.K. pre-EIR debt. Controlling for the creditor-rights-unrelated effects, the creditor-rights-related effect reduces to 2.5 percent point but remains significant at the 10% level (t-statistic: –1.70; see column (3)).

TABLE IA.4 ABOUT HERE.

IA4 The Confounding Effects of Culture and Politics

In this section, we offer more details about our analysis of culture and political institutions as potential confounding effect of creditor rights on debt financing in Section 5.4 of our main paper. That analysis addresses a common critique of the law and finance literature, namely, that a country's religious, cultural, and political beliefs must shape its legal environment and may thus endogenously drive the relation between creditor rights and financial and real outcomes (La Porta et al. (2008)).

To facilitate our analysis, we estimate regression (5) in our main paper comparing creditor rights changes with changes in religious, cultural, and political institutions (i.e., x-variable) in the EU-12 sample. We use the fraction of a country's catholic population as proxy for religious beliefs; Hofstede's (2001) five culture indexes (i.e. 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 political institutions.

Table IA.5 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 strong and robust (i.e., the estimates on both Up^{CRI} and $Down^{CRI}$ mirror those in our main tests). In contrast, the effects of the religion and culture proxies (the estimates on Up^{CulVar} and $Down^{CulVar}$) are either insignificant or counterintuitive.

TABLE IA.5 ABOUT HERE.

Table IA.6 reports results for the political institutional 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.[27] As for the political proxies, most of their effects are either insignificant or counter-intuitive, like the results on union density (column (3)).

TABLE IA.6 ABOUT HERE.

Overall, our results speak to a common critique of the law and finance literature — that estimated effects of creditor rights on corporate financing may instead reflect the cultural and political institutions correlated with a country's legal environment, rather than creditor rights themselves. Notably, our findings suggest that the debt-financing effects of strengthening or weakening creditor rights are not driven by these alternative institutional and cultural dimensions.

IA5 Validation and Robustness Tests

This section offers more details on the validation and robustness tests in Section 6 of the main paper.

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

IA5.1 Parallel-Trend Tests

A condition for DIDID tests to yield valid inferences is that the treated and controls display similar trends in outcome variables before the shock ("parallel-trends condition"). In Table IA.7, we report changes in total debt and the control variables across different sub-samples. In particular, Panel A contrasts the change in total debt and the (non-averaged) controls over each pre-EIR year across domestic and foreign subsidiaries for the EU-12 countries; 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 IA.7 ABOUT HERE.

IA5.2 Placebo Tests

Table IA.8 presents the results from estimating placebo versions of regression (2) in our main paper using total debt as dependent variable on all EU-12 subsidiaries. For convenience, column (1) reports the same results as in Table 1. 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. Further, the sample statistics at the bottom of the table shows that there are only few foreign subsidiaries with parents in Denmark and Norway, possibly implying that these tests lack statistical power. 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 IA.8 shows insignificant effects and in some cases the wrong signs (e.g., columns (2) and (3)).

TABLE IA.8 ABOUT HERE.

IA5.3 PS-Matching Tests

We finally repeat our main regressions in Table 1 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 IA.7 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 IA.9 shows the results from the regressions run on the PS-matched samples. Although the PS-matched samples are significantly smaller than the original ones, 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 (1), 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 IA.9 ABOUT HERE.

Table IA.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 accouting 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	Subsidiaires 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, fin-nancing; 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 IA.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 ony 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).

		Parent Country												
Subsidiary 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	Total
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Austria	MeanObs.	23		1	2	9		1	5			1	2	43
	%RowSum	53.6%		1.6%	5.1%	20.1%		1.2%	11.8%			3.2%	3.5%	
Belgium	MeanObs.	0	60	4	29	23	1	1	88		1	7	16	229
	%RowSum	0.2%	26.2%	1.6%	12.4%	9.9%	0.6%	0.6%	38.3%		0.2%	2.8%	7.1%	
Finland	MeanObs.	0	2	314	13	4	0	1	21		0	43	8	407
	%RowSum	0.0%	0.6%	77.1%	3.2%	1.0%	0.1%	0.1%	5.2%		0.0%	10.7%	1.9%	
France	MeanObs.	8	44	9	1,334	80	2	16	99	2	2	28	66	1,689
	%RowSum	0.4%	2.6%	0.5%	79.0%	4.7%	0.1%	1.0%	5.9%	0.1%	0.1%	1.6%	3.9%	
Germany	MeanObs.	5	2	4	29	327	1	3	29		0	8	19	425
	%RowSum	1.1%	0.5%	0.9%	6.7%	76.8%	0.1%	0.7%	6.8%		0.1%	1.9%	4.5%	
Ireland	MeanObs.		1	1	2	1	6	1	12			1	8	33
	%RowSum		2.4%	1.8%	6.7%	3.3%	18.2%	2.7%	37.1%			2.7%	24.9%	
Italy	MeanObs.	5	16	6	71	54	0	355	76	1	4	16	28	630
	%RowSum	0.7%	2.5%	1.0%	11.2%	8.5%	0.0%	56.3%	12.1%	0.1%	0.6%	2.5%	4.5%	
Netherlands	MeanObs.	1	30	7	36	56	1	6	921		1	28	34	1,120
	%RowSum	0.1%	2.6%	0.6%	3.2%	5.0%	0.1%	0.6%	82.2%		0.1%	2.5%	3.1%	
Portugal	MeanObs.		1	1	6	5		1	2	65	2	1	2	85
	%RowSum		0.8%	0.7%	7.3%	5.4%		0.9%	2.7%	76.9%	1.8%	1.4%	2.0%	
Spain	MeanObs.	2	10	4	60	54	1	21	64	4	543	10	30	802
	%RowSum	0.3%	1.3%	0.5%	7.5%	6.7%	0.2%	2.6%	8.0%	0.5%	67.7%	1.3%	3.7%	
Sweden	MeanObs.	2	10	53	33	33	1	4	67			2,201	17	2,420
	%RowSum	0.1%	0.4%	2.2%	1.4%	1.3%	0.0%	0.1%	2.8%			91.0%	0.7%	
UK	MeanObs.	8	41	27	142	109	43	14	197	2	2	41	1,084	1,709
	%RowSum	0.4%	2.4%	1.6%	8.3%	6.4%	2.5%	0.8%	11.5%	0.1%	0.1%	2.4%	63.4%	
Total		53	216	429	1,756	753	56	423	1,582	73	554	2,385	1,314	9,594

Table IA.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.). TotalDebt is the sum of short (Amadeus mnemonic: loan) and long-term (ltdb) debt scaled by assets (toas). In accordance, ShorttermDebt and LongTermDebt are short and long-term debt both scaled by assets, respectively. Size is the natural log of assets, FirmAge the difference between the current date and the date of incorporation (in years), Employees the natural log of the number of employees, Profitability is EBITDA scaled by assets, FixedAssets is tangible fixed scaled by assets, and CashHoldings 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 Debt	Long-term Debt	Short-term Debt	Size (PRA)	Firm Age (PRA)	Employees (PRA)	Profitability (PRA)	Fixed Assets (PRA)	Cash Holdings (PRA)
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Austria	Mean	0.154	0.065	0.022	10.707	13.300	5.380	0.106	0.262	0.063
N=433	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	Mean	0.203	0.103	0.087	9.859	25.213	4.373	0.109	0.175	0.090
N=2,290	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	Mean	0.080	0.012	0.061	9.557	18.955	4.745	0.164	0.224	0.077
N=4,074	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	Mean	0.085	0.064	0.013	9.736	21.103	4.760	0.087	0.135	0.077
N=16,885	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	Mean	0.170	0.012	0.154	10.759	20.585	5.579	0.152	0.241	0.056
N=4,254	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	Mean	0.174	0.067	0.097	10.025	19.741	4.452	0.067	0.221	0.112
N=329	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	Mean	0.133	0.104	0.025	10.411	20.702	4.774	0.099	0.164	0.045
N=6,299	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 IA.3. Descriptive Statistics (cont.)

		Total Debt	Long-term Debt	Short-term Debt	Size (PRA)	Firm Age (PRA)	Employees (PRA)	Profitability (PRA)	Fixed Assets (PRA)	Cash Holdings (PRA)
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Netherlands	Mean	0.082	0.049	0.025	10.138	33.622	5.068	0.141	0.240	0.090
N=11,204	Med.	0.000	0.000	0.000	9.937	26.059	5.026	0.128	0.184	0.051
	St.D.	0.137	0.088	0.060	1.264	25.872	1.249	0.094	0.198	0.103
Portugal	Mean	0.174	0.078	0.093	10.275	19.733	5.390	0.110	0.256	0.038
N=848	Med.	0.113	0.021	0.017	10.205	11.075	5.244	0.089	0.233	0.020
	St.D.	0.184	0.107	0.143	1.341	18.607	1.195	0.087	0.194	0.046
Spain	Mean	0.163	0.070	0.088	9.584	17.251	4.441	0.105	0.210	0.096
N=8,023	Med.	0.074	0.007	0.005	9.547	12.962	4.512	0.095	0.151	0.053
	St.D.	0.195	0.109	0.154	1.263	13.289	1.317	0.088	0.196	0.106
Sweden	Mean	0.085	0.011	0.069	9.146	29.410	4.260	0.127	0.200	0.071
N=24,204	Med.	0.000	0.000	0.000	8.993	24.069	4.248	0.117	0.137	0.030
	St.D.	0.142	0.031	0.121	1.242	22.227	1.238	0.110	0.183	0.092
UK	Mean	0.397	0.294	0.095	10.601	27.325	5.434	0.111	0.260	0.091
N=17,092	Med.	0.352	0.217	0.000	10.423	18.538	5.371	0.107	0.185	0.049
	St.D.	0.299	0.270	0.183	1.461	23.312	1.499	0.120	0.236	0.105
All	Mean	0.158	0.089	0.061	9.871	25.353	4.798	0.117	0.205	0.078
N=95,935	Med.	0.039	0.003	0.000	9.739	18.009	4.773	0.106	0.141	0.039
	St.D.	0.223	0.170	0.134	1.429	21.292	1.392	0.110	0.195	0.095

Table IA.4. The Debt-Financing Effects of the EIR: The U.K. Sample

This table reports the results from estimating the regressions:

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

$$Debt_{i,t} = \beta_1(Treat_i \times Shock_t \times Similar_i^{CRI}) + \beta_2(Treat_i \times Shock_t \times Down_i^{CRI}) + \sum_{y \in Y} \sum_{c \in C} \gamma_{y,c}(DYear_t^y \times MCon_i^c) + \alpha_i + \alpha_t + \epsilon_{i,t},$$

and

$$Debt_{i,t} = \beta_1(Treat_i \times Shock_t) + \beta_2(Treat_i \times Shock_t \times Down_i^{CRI}) + \sum_{y \in Y} \sum_{c \in C} \gamma_{y,c}(DYear_t^y \times MCon_i^c) + \alpha_i + \alpha_t + \epsilon_{i,t},$$

in columns (1), (2), and (3), respectively. *Debt* is the total debt of U.K. subsidiary i at the end of fiscal year t, *Treat* and *Shock* are defined in the main text. Similar^{CRI} (Down^{CRI}) is a dummy variable equal to one if a foreign subsidiary's parent country has a La Porta et al.(1998)-Djankov et al. (2007) creditor rights index value only one unit lower (at least two units lower) than the U.K. The double sum is constructed analogously to that used in Table 1 in the main paper (Control Int.). α_i and α_t are static subsidiary (Firm FE) and year (Time FE) fixed effects, respectively. We cluster standard errors by subsidiary (Cluster Firm). Plain numbers are estimates, whereas t-statistics are in square bracket. The bottom rows report the number of observations (Observations), the number of treated (Treated Obs.), and weak-rights-country treated (Treated Obs. (WR)) observations, and the adjusted R-squared (Adj. R^2). We offer more details on how we construct the analysis variables in Table A.1 in the appendix of the main paper.

	United Kingdom		
	DID Model	Split DID Model	DIDID Model
	(1)	(2)	(3)
Treat × *Shock*	–0.037***		–0.025**
	(–3.648)		(–2.067)
Treat × *Shock* × *Similar*^{CRI}		–0.025**	
		(–2.067)	
Treat × *Shock* × *Down*^{CRI}		–0.050***	–0.025*
		(–3.795)	(–1.697)
Control Int.	✓	✓	✓
Firm FE	✓	✓	✓
Time FE	✓	✓	✓
Cluster Firm	✓	✓	✓
Observations	17,092	17,092	17,092
Treated Obs.	6,185	6,185	6,185
Treated Obs. (WR)	3,154	3,154	3,154
Adj. R^2	1.2%	1.2%	1.2%

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

Table IA.5. Creditor Rights-Versus-Culture

This table reports the results from estimating the following regression:

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

where *Debt* is the total debt of subsidiary i in member state s at the end of fiscal year t. *Treat* is a dummy equal to one for foreign subsidiaries and else zero. We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. *Shock* is a dummy equal to one starting from 2002 to 2006 and zero between 1997 and 2001. Up^{CRI} ($Down^{CRI}$) is a dummy 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 (lower) than its host country (see Table IA.2 in the Internet Appendix for the index values). Up^{CulVar} ($Down^{CulVar}$) is 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 (lower) than the host country and else zero. The culture indexes are a catholic dummy equal to one if the majority religion in a country is Catholicism and else zero, and the five Hofstede cultural indexes (power distance, individualism, masculinity, uncertainty avoidance, and long-term orientation). The triple sum is constructed analogously to that used in Table 1 in the main paper (Control Int.). α_i and α_t are static subsidiary (Firm FE) and year (Time FE) fixed effects, respectively. We cluster standard errors by subsidiary (Cluster Firm). Plain numbers are estimates, whereas t-statistics are in square brackets. The bottom rows report the number of observations (Observations), the number of treated (Treated Obs.), strong-rights-country treated (Treated Obs. (SR)), weak-rights-country treated (Treated Obs. (WR)) observations, high-culture-value treated (Treated Obs. (HCV)), low-culture-value treated (Treated Obs. (LCV)) observations, and the adjusted R-squared (Adj. R^2). We offer more details on how we construct the analysis variables in Table A.1 in the appendix of the main paper.

	Culture Variable					
	Catholic	Power Distance	Individualism	Masculinity	Uncertainty Avoidance	Long-term Orientation
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat \times Shock \times Up^{CRI}$	0.017***	0.015**	0.019***	0.020***	0.018***	0.021***
	(2.784)	(2.316)	(2.714)	(3.310)	(2.836)	(3.224)
$Treat \times Shock \times Down^{CRI}$	–0.025***	–0.034***	–0.024***	–0.029***	–0.024***	–0.028***
	(–2.881)	(–3.730)	(–2.589)	(–3.154)	(–2.649)	(–2.843)
$Treat \times Shock \times Up^{CulVar}$	–0.013	0.012	–0.025**	–0.002	0.010	0.003
	(–1.639)	(1.494)	(–2.233)	(–0.214)	(0.791)	(0.368)
$Treat \times Shock \times Down^{CulVar}$	0.009	0.013*	–0.035***	–0.001	0.021*	0.010
	(1.370)	(1.656)	(–3.203)	(–0.059)	(1.685)	(1.086)
$Treat \times Shock$	–0.011**	–0.018***	0.015	–0.012	–0.027**	–0.018**
	(–1.961)	(–2.911)	(1.609)	(–1.273)	(–2.052)	(–1.980)
Control Int.	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
Cluster Firm	✓	✓	✓	✓	✓	✓
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 IA.6. Creditor Rights-Versus-Political Institutions

This table reports the results from estimating the regression:

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

where *Debt* is the total debt of subsidiary i in member state s at the end of fiscal year t. *Treat* is a dummy equal to one for foreign subsidiaries and else zero. We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. *Shock* is a dummy equal to one starting from 2002 to 2006 and zero between 1997 and 2001. Up^{CRI} (Down^{CRI}) is a dummy 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 (lower) than its host country (see Table IA.2 in the Internet Appendix for the index values). Up^{PolIns} (Down^{PolIns}) is a dummy variable equal to one if a foreign subsidiary's parent is located in a country either with a politics/labor protection value at least half-a-standard-deviation higher (lower) than the host country and else zero. The proxies for political institutions are: a dummy variable equal to one if a country has a proportional election system and else zero ("ProportionalElectionSystem"); the fraction of years which the country was governed by a left-wing party ("LeftPower"); the fraction of the unionized work population in the country ("UnionDensity"); and an index capturing the strength of the country's labor laws ("StrengthLaborLaws"). The triple sum is constructed analogously to that used in Table 1 in the main paper (Control Int.). α_i and α_t are static subsidiary (Firm FE) and year (Time FE) fixed effects, respectively. We cluster standard errors by subsidiary (Cluster Firm). Plain numbers are estimates, whereas t-statistics are in square brackets. The bottom rows report the number of observations (Observations), the number of treated (Treated Obs.), strong-rights-country treated (Treated Obs. (SR)), weak-rights-country treated (Treated Obs. (WR)) observations, high-political-value treated (Treated Obs. (HPV)), low-political-value treated (Treated Obs. (LPV)) observations, and the adjusted R-squared (Adj. R^2). We offer more details on how we construct the analysis variables in Table A.1 in the appendix of the main paper.

	Politics		Labor Protection	
	Proportional Election System	Left Power	Union Density	Strength Labor Laws
	(1)	(2)	(3)	(4)
$Treat \times Shock \times Up^{CRI}$	0.019***	0.022***	0.012*	0.013**
	(3.109)	(3.358)	(1.822)	(2.126)
$Treat \times Shock \times Down^{CRI}$	–0.028***	–0.037***	–0.036***	–0.025***
	(–3.251)	(–3.836)	(–4.019)	(–2.738)
$Treat \times Shock \times Up^{PolIns}$	–0.003	0.017*	0.017*	–0.014
	(–0.491)	(1.781)	(1.879)	(–1.632)
$Treat \times Shock \times Down^{PolIns}$	0.000	–0.001	0.018**	0.001
	(0.025)	(0.104)	(2.034)	(0.153)
$Treat \times Shock$	–0.011**	–0.015**	–0.023***	–0.007
	(–2.127)	(–2.379)	(–3.097)	(–1.436)
Control Int.	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Cluster Firm	✓	✓	✓	✓
Observations	95,935	95,935	95,935	95,935
Treated Obs	23,717	23,717	23,717	23,717
Treated Obs. (HPV)	11,946	6,362	10,490	9,858
Treated Obs (LPV)	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 IA.7. Parallel Trend Tests

In this table, we report the results from parallel trend tests for the EU-12 country sample. 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 debt, 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 round brackets are their (robust) *t*-statistics. We run the tests using only consistently fully owned subsidiaries before the reform. We offer more details on how we construct the analysis variables in Table A.1 in the appendix of the main paper.

	Difference in Annual Change Between Treated and Controls			
	1998	1999	2000	2001
	(1)	(2)	(3)	(4)
Panel A: Domestic-Versus-Foreign Subsidiaries				
TotalDebt	–0.002	–0.021	–0.002	–0.084
	(–0.085)	(–0.880)	(–0.087)	(–3.592)
Size	–0.016	–0.015	–0.047	–0.014
	(–0.714)	(–0.691)	(–2.118)	(–0.707)
Employees	0.013	–0.024	0.031	0.059
	(0.556)	(–1.139)	(1.228)	(2.370)
Profitability	0.024	–0.049	–0.031	0.021
	(0.965)	(–2.242)	(–1.425)	(1.017)
FxedAssets	–0.028	–0.014	0.006	0.015
	(–1.082)	(–0.691)	(0.267)	(0.747)
CashHoldings	0.004	0.004	–0.014	0.094
	(0.130)	(0.157)	(–0.641)	(4.155)
Panel B: Stronger-Versus-Similar Creditor-Rights Foreign Subsidiaries				
TotalDebt	–0.100	–0.037	–0.014	–0.031
	(–1.997)	(–0.899)	(–0.349)	(–0.723)
Size	–0.002	–0.086	0.151	0.066
	(–0.034)	(–1.812)	(2.912)	(1.395)
Employees	–0.077	–0.026	0.017	–0.039
	(–1.381)	(–0.591)	(0.343)	(–0.768)
Profitability	–0.007	0.120	0.018	–0.049
	(–0.116)	(2.425)	(0.365)	(–1.092)
FxedAssets	0.044	0.050	0.022	0.010
	(0.681)	(1.100)	(0.466)	(0.223)
CashHoldings	–0.036	0.078	–0.064	–0.007
	(–0.648)	(1.629)	(–1.335)	(–0.135)

(continued on next page)

Table IA.7. Parallel Trend Test Results (cont.)

	Difference in Annual Change Between Treated and Controls			
	1998	1999	2000	2001
	(1)	(2)	(3)	(4)
Panel C: Weaker-Versus-Similar Creditor-Rights Foreign Subsidiaries				
TotalDebt	–0.028	0.082	–0.001	–0.017
	(–0.508)	(1.562)	(–0.016)	(–0.334)
Size	–0.065	0.046	0.052	0.022
	(–1.392)	(1.004)	(1.099)	(0.491)
Employees	–0.070	–0.014	0.005	0.018
	(–1.418)	(–0.323)	(0.088)	(0.323)
Profitability	–0.001	0.066	–0.002	0.040
	(–0.022)	(1.340)	(–0.038)	(0.843)
FxedAssets	–0.110	0.068	0.001	0.022
	(–1.988)	(1.479)	(0.028)	(0.460)
CashHoldings	0.048	0.000	0.039	0.055
	(0.762)	(0.006)	(0.862)	(1.056)

Table IA.8. The Debt-Financing Effects of the EIR: Placebo Tests

This table reports the results from estimating the regression:

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

where *Debt* is the total debt of subsidiary i in member state s at the end of fiscal year t. *Treat* is a dummy equal to one for foreign subsidiaries and else zero. We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. *Shock* is a dummy equal to one starting from 2002 to 2006 and zero between 1997 and 2001. Up^{CRI} ($Down^{CRI}$) is a dummy 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 (lower) than its host country (see Table IA.2 in the Internet Appendix for the index values). The triple sum is constructed analogously to that used in Table 1 in the main paper (Control Int.). α_i and α_t are static subsidiary (Firm FE) and year (Time FE) fixed effects, respectively. Column (1) reports the results from column (1) of Table 1 in the main paper for the reader's convenience. Column (2) reports results with only foreign subsidiaries with parents in Denmark and Norway. Column (3) reports results with non-fully owned subsidiaries, where the non-fully owned subsidiaries are only up to 80% owned by their parents. Column (4) reports results from randomly reshuffling of the foreign subsidiaries' parent countries. We cluster standard errors by subsidiary (Cluster Firm). Plain numbers are estimates, whereas t-statistics are in square bracket. 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. R^2). We offer more details on how we construct the analysis variables in Table A.1 in the appendix of the main paper.

	All Countries			
	Main Sample (Repeated)	Shifted to Non-Treated Countries (DNK/NOR)	Partially Owned Subsidiaries	Random Reshuffling Treated Firms
	(1)	(2)	(3)	(4)
Treat × *Shock* × Up^{CRI}	0.019***	0.029	0.002	–0.003
	(3.112)	(1.467)	(0.161)	(–0.371)
Treat × *Shock* × $Down^{CRI}$	–0.029***	0.016	–0.008	–0.006
	(–3.266)	(0.312)	(–0.551)	(–0.750)
Treat × *Shock*	–0.013***	–0.028	–0.003	–0.012***
	(–2.674)	(–1.563)	(–0.368)	(–2.585)
Control Int.	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Cluster Firm	✓	✓	✓	✓
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. R^2	73.9%	36.1%	70.2%	73.9%

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

Table IA.9. The Debt-Financing Effects of the EIR: PS-Matching

This table reports the results from estimating the regression:

$$Debt_{i,s,t} = \beta_1(Treat_{i,s} \times Shock_t \times Up^{CRI}_{i,s}) + \beta_2(Treat_{i,s} \times Shock_t \times Down^{CRI}_{i,s}) + \beta_3(Treat_{i,s} \times Shock_t) + \sum_{s \in S}\sum_{y \in Y}\sum_{c \in C} \gamma_{s,y,c}(DState^s_i \times DYear^y_t \times MCon^c_i) + \alpha_i + \alpha_t + \epsilon_{i,s,t},$$

where *Debt* is the total debt of subsidiary i in member state s at the end of fiscal year t. *Treat* is a dummy equal to one for foreign subsidiaries and else zero. We identify as foreign subsidiaries those consistently owned by a foreign parent before the reform. *Shock* is a dummy equal to one starting from 2002 to 2006 and zero between 1997 and 2001. Up^{CRI} ($Down^{CRI}$) is a dummy 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 (lower) than its host country (see Table IA.2 in the Internet Appendix for the index values). The triple sum is constructed analogously to that used in Table 1 in the main paper (Control Int.). α_i and α_t are static subsidiary (Firm FE) and year (Time FE) fixed effects, respectively. 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. We cluster standard errors by firm (Cluster Firm). Plain numbers are estimates, whereas t-statistics are in square brackets. 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. R^2). We offer more details on how we construct the analysis variables in Table A.1 in the appendix of the main paper.

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

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