Lending Quality and Contracts Enforcement Reforms[1]

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Abstract

We investigate the causal relationship between the efficiency of country's judicial system and the quality of bank lending, using the contracts enforcement reforms implemented in four European countries as a quasi-natural experiment. We find that strengthening contracts enforcement determines large, significant, and persistent reductions in banks' non-performing-loans (NPLs). Our results have important policy implications: they point at judicial efficiency as a critical determinant of the stability of the banking sector and its resilience to adverse shocks such as the recent Covid-19 pandemic.

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JEL classification: G21, G28

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The problem of Non-Performing Loans (NPLs) is not solving itself - and it has not yet been resolved. While it is true that the amount of NPLs has fallen significantly - by almost 50% since 2014 - the stock of NPLs is still very high. It is also very old. Many of the NPLs that we see on banks' balance sheets have been there for years. […] We have to get a handle on this problem. We have to solve the issue of NPLs while the economy is still resilient. If banks have to sail into the next storm with too many NPLs on their balance sheets, they will be less able to weather it and come out safely on the other side.

[Andrea Enria, 14 June 2019][2]

1. Introduction

One of the most important legacies of the Global Financial Crisis (GFC) of 2008 and the subsequent Euro Sovereign Crises of 2011-2012 has been the sharp and widespread increase in the stock of non-performing loans (NPLs) held by European banks.[3] In turn, such an increase in NPLs worsened the downturn. Moreover, it slowed down the recovery in several countries (ESRB, 2017). NPLs, in fact, represent a burden for both the borrower and the lender. On the one hand, the former cannot use trapped valuable collateral for different purposes and its credit history worsens, negatively impacting her future financing needs (see, for instance, Bernanke, Gertler, and Gilchrist, 1999). Conversely, the latter sees its profitability and liquidity reduced by NPLs (Casu, Girardone and Molyneux, 2015). However, while small amounts of NPLs do not represent much of a concern, especially in the case of profitable banks with sufficiently large loan portfolios, large

[2] Speech by Andrea Enria, Chair of the Supervisory Board of the ECB, at the Conference "EDIS, NPLs, Sovereign Debt and Safe Assets" organized by the Institute for Law and Finance, Frankfurt, 14 June 2019.

[3] By contrast, before the start of the GFC, non-performing loans were relatively low and stable across most EU countries.

amounts of NPLs can undermine banks' capital adequacy, constrain their lending activity[4], and even lead to default.[5] At an aggregate level, this may impair the transmission of monetary policy and undermine the financial stability of the overall banking system, with severe consequences for economic development (Bernanke and Gertler, 1989, 1995; Barseghyan, 2010; Tarazi, Soedarmono, Agusman, Monroe and Gasbarro, 2018; Iannotta, Pennacchi, and Santos, 2019).

Despite in recent years banks and regulators have made a significant effort to reduce the stock of NPLs held by European banks, it is essential to understand how it is possible to reduce NPLs without imposing stricter rules and capital requirements on the banking system which may further depress the economy. Economic theory and policy papers (North, 1990; Bianco, Jappelli, and Pagano, 2005; Jassaud and Kang, 2015; ECB, 2017a, b; Baudino and Yun, 2017; Avramidis, Mylonopoulos, and Pennacchi, 2021) suggest that the efficiency of judicial system plays a vital role in determining how NPLs are handled.[6] Judicial systems that become more efficient imply an improvement in settlement of the contract enforcement and ensure a better framework for collateral

[4] Losses stemming from NPLs also lead to banks capital reduction, and, as highlighted by Peek and Rosengren (1997), Gambacorta and Mistrulli (2004), Watanabe (2007), Berrospide and Edge (2010), Michelangeli and Sette (2016), and Gambacorta and Shin (2018), capital-restricted banks are even more reluctant to grant new loans.

[5] Banks' portfolio quality also influences risk-taking behavior. While prudential banks are typically more cautious when facing increasing levels of NPLs, banks characterized by large amounts of NPLs are more likely to assume higher risks in the future (Bowman and Malmendier, 2015; Eisdorfer 2008; Koudstaal and Wijnbergen, 2012).

[6] In his speech at the European Parliament's Economic and Monetary Affairs Committee on 19 June 2017, the Chair of the Supervisory Board of the ECB, Danièle Nouy, has further reiterated this view "*I would also like to stress that addressing NPLs requires determined action from all stakeholders, not only supervisors. In addition to our work, legal and institutional measures are required, notably in the areas of insolvency and judicial processes*".

repossession.[7] In turn, this increases NPLs' recovery rates, reduces the time of their disposal and enhances the development of a secondary market for bad loans, thus reducing the amount of existing NPLs held by banks.[8] Similarly, more efficient judicial systems also reduce the borrowers' incentives to strategic default – i.e., defaults that occur not because the borrower is unable to repay the obligations (for example, due to bad luck) but because, although potentially solvent, he is unwilling to do so – by increasing the perceived costs of the presumed sanctions. As a result, also the flow of new NPLs is expected to decline.

In this paper we investigate the relationship between judicial efficiency and lending quality. Specifically, we exploit four contracts enforcement reforms implemented in as many European countries (that is Austria in 2007, Belgium in 2007, Norway in 2008 and Sweden in 2008) to investigate how the improvements in contracts enforcement affect the stock of NPLs held by European banks. The main measures introduced by these reforms involve the introduction of tighter deadlines in court procedures, the use of modern information and communication technologies, faster foreclosure processes and a facilitated access to courts. Furthermore, since each reform has been undertaken by the corresponding country independently and only affected domestic banks, these legal changes represent the ideal setting to implement a difference-in-differences (DID) methodology that compares banks operating in the countries affected by the reforms with comparable banks in European countries not affected by the reforms, before and after the passage of the laws.

[7] The majority of bank lending is linked to some type of collateral, such as real estate, whereas only a small part consists of non-collateralised or unsecured loans.

[8] In Europe, low transparency surrounding bad loans, differences in national legislation and inadequate write-downs have strongly contributed to the underdevelopment of a secondary market of NPLs.

Using a rich dataset including more than 400 banks from 11 European countries over the period 2004 – 2011, our differences-in-differences (DID) estimates show that an improvement in the country's judicial system's efficiency determines a large, significant and persistent reduction of NPLs' stocks, which does not vanish even three years after the implementation of the reforms. Quantitatively, the estimates from our preferred specification - in which we control for banks-and-year fixed effects, bank-level, country-level and linear trends variables - show that the reforms have lowered the incidence of NPLs over total loans from a minimum of 1.69 percentage points in the case of Norway to a maximum of 3.44 percentage points in the case of Sweden, relative to countries that have not implemented such reforms.

Our results are not driven by neither pre-existing differences between banks in countries affected by the reforms and banks in unaffected countries - including different trends in NPLs – nor other reforms affecting the countries' business activity, and are robust to an alternative definition of the control group, alternative estimation time-window, and the exclusion of systemically important financial institutions (SIFIs) – i.e., the type of banks featuring the largest share of international lending.

Our paper is related to several strands of literature. The first strand investigates the determinants of NPLs, focusing in particular on country- and bank-characteristics (see, among others, Berger and DeYoung, 1997; Salas and Saurina, 2002; Podpiera and Weill, 2008; Louizis, Vouldis and Metaxas, 2012; Klein, 2013; Berger, Molyneux, and Wilson, 2014; Cetorelli, 2104; Ghosh, 2015; Zhang, Cai, Dickinson, and Kutan, 2016). Regarding macroeconomic factors, this literature shows that NPLs are negatively influenced by economic growth and positively related to lending rates, unemployment and inflation. By contrast, among bank characteristics, NPLs appear

positively related to cost inefficiency and negatively related to equity levels, bank size, and diversification.

Several papers have investigated the impact of NPLs or loan-loss reserves on bank lending policy and lending rates. Albertazzi, Nobili and Signoretti (2016) find that NPLs imply a higher markup on lending rates. Using Spanish credit register data, Jiménez, Ongena, Peydró and Saurina (2012) try to identify the balance sheet channel of monetary policy. Using loan-level data from the Italian credit register, Burlon, Fantino, Nobili and Sene (2016) investigate the role of bad loans and capital for credit rationing. Their results highlight that, controlling for Tier 1 capital, the higher the share of NPLs, the higher the loan margin and the lower the supply of credit.

Another strand of literature has studied the relationship between creditor rights and bank lending. Several studies, in fact, have found that creditor rights' improvements are positively related to the size of credit markets in several countries (La Porta, Lopez-de-Silanes, Shleifer, and Vishny 1998; Levine, 1998, 1999; Djankov, McLiesh, and Shleifer, 2007). Haselmann, Pistor, and Vig (2010) report that the strengthening of individual creditors' claims outside bankruptcy has increased bank lending activities in transition countries, whereas, using the 2002 Indian bankruptcy reform, Vig (2013) highlights that creditor rights' improvements reduced secured credit.

Cerqueiro, Ongena and Roszbach (2016) investigate the role of collateral in debt contracts, credit availability, and bank monitoring. More precisely, by exploiting a legal change that occurred in Sweden on January 1, 2004, as a quasi-natural experimental setting (that reduced the value of all floating liens) and using a rich proprietary data set from a large bank, the authors are able to address the usual econometric identification challenges. They find that, after the change in the law, the bank: *i)* reduces the assessed value of the outstanding collateral; *ii)* following the loss in

collateral value, increases the interest rate on outstanding treated loans by 20 basis points and reduces its internal credit limit to affected borrowers; and *iii)* reduces the intensity of its monitoring on borrowers and collateral.

Cerqueiro, Ongena and Roszbach (2020), instead, investigate how an exogenous transfer of priority rights from banks to other creditors, due to a legal reform occurred in Sweden in 2004, affects corporate financing, investment, as well as firm performance and growth. The authors find that, after the passage of the law, firms reduce their collateral to debt ratio by 5 percent on average and hold less debt and with shorter maturity. Moreover, they also document that, while the legal reform has a significant impact on firms' investment, it has no effect on production technology, since capital, labor, and sales all drop roughly at the same rate.

Finally, our paper is also related to the literature that investigates the consequences of legal systems' efficiency on different economic outcomes, such as employment levels (Pezone, 2020), firm size (Giacomelli and Menon, 2016; Laeven and Woodruff, 2007), firm defaults (Schiantarelli, Stacchini and Strahan, 2020), and financing and asset maturity (Gopalan, Mukherjee, and Singh, 2016). In this regard, Jappelli, Pagano and Bianco (2005) show that improvements in judicial efficiency reduce credit constraints and increase lending activity. However, none of these papers has focused its attention on the causal relationship between judicial efficiency and the lending quality.

The remainder of the paper is organized as follows. Section 2 provides more information about the institutional settings of the enforcing contracts reforms. Section 3 describes our identification strategy and presents the data and the variables employed in our analysis. Section 4 shows the empirical methodology whereas Sections 5 and 6 illustrate our baseline results and several robustness checks. Finally, Section 7 concludes and summarizes the implications of our

findings.

2. Institutional Background and Main Hypothesis

2.1. Judicial Efficiency and Non-Performing Loans

Economic theory suggests that the efficiency of judicial systems, by affecting the lender's ability to recover the value of its credit and the borrower's willingness to repay his obligations, plays a crucial role on both the stock of loans already deteriorated – that is the NPLs accumulated in the banks' balance-sheets - and the flow of good loans that may deteriorate in the future - that is the new NPLs. From a bank's perspective, in fact, the former is strongly influenced by the settlement of the contract enforcement and the collateral repossession framework (The Council of the EU, 2017). In this regard, long (and risky) credit recovery procedures negatively affect recovery rates, reduce the market value of bad loans, and constrain the time of their disposal, thus contributing to the systematic persistence of NPLs in the banks' balance sheets. At the same time, bad loans do not always arise because the borrower is unable to repay her obligations, for example due to bad luck or poor monitoring of the project. They often arise because the borrower, although potentially solvent, is unwilling to repay. Such strategic default may occur when the gains from defaulting are higher than the perceived costs of the presumed sanctions (Bianco, Jappelli, and Pagano, 2005). As highlighted by Schiantarelli, Stacchini and Strahan (2020), lengthy proceedings can increase this moral hazard behavior, thus incentivizing strategic default and, hence, increasing the flows of new NPLs. Overall, an inefficient foreclosure and restructuring framework is less able to manage the flows of new NPLs and the underlying inertia will have a more lasting impact on their accumulation (European Systemic Risk Board, 2017).

While the economic mechanisms highlighted above are well known from a theoretical point of view, the empirical evidence regarding the relationship between judicial systems' efficiency and lending quality is still rather scarce. In this paper, we aim at filling this gap by investigating the causal link between improvements of contracts' enforcements and the level of NPLs in Europe. To this purpose, we formulate the following research hypothesis:

Hypothesis: Banks in countries that implement contract enforcement reforms should exhibit lower NPLs (i.e., greater reduction or smaller increase) compared to banks in countries that instead do not implement such reforms.

In what follows we first provide more information about the contract enforcement reforms analyzed in this study and then highlight the identification strategy employed to test the main hypothesis.

2.2. Contract Enforcement Reforms

Our empirical analysis exploits the implementation of four similar contract-enforcement reforms that occurred in Europe: Austria in 2007, Belgium in 2007, Norway in 2008, and Sweden in 2008. This section provides detailed information on the institutional settings involving such legal changes.[9]

Each year, the Doing Business group at the World Bank evaluates whether various countries have adopted a series of good practices that promote the quality and the efficiency of the court system by measuring the quality of judicial processes and the time and cost of resolving a commercial dispute through a local first-instance court. More importantly, to be classified as

[9] The material reported in this section hinges on the information provided by Doing Business Team at the World Bank about these contracts enforcement reforms.

reforms, such changes in laws and regulations must produce a significant impact on the enforcement of contracts (Doing Business Reforms Database).[10]

In 2007 Austria strengthened the enforcement of contracts by making electronic filing mandatory in civil matters and thereby increasing the efficiency of proceedings. This reform took place with regulation 482/2006, in effect since 1 July 2007, and implied that all filings in civil litigation and enforcement proceedings between lawyers and courts must use an electronic data channel operated by the Ministry of Justice.[11] The law also established that the system used electronic signatures of attorneys; procedural orders were served electronically, or printed and posted automatically overnight, which slightly reduces the time of file handling in court; judgments were delivered by e-mail, which has replaced the formerly formal notification process (Doing Business Reforms Database).[12]

This type of reform typically streamlines and accelerates the process of commencing a lawsuit. At the same time, electronic records tend to be more convenient and reliable: reducing in-person interactions with court officers minimizes the chances for corruption and results in faster trials, better access to courts and more reliable service of process. These features also produce

[10] Specifically, changes in laws and regulations that have any direct impact on the country's score on the quality of judicial processes index are classified as reforms. By contrast, legislative changes on the time and cost to resolve a dispute are classified as reforms only when their impact on the score of the enforcing contracts indicators is sizeable enough (Doing Business Reforms Database).

[11] Section 11 of *Elektronischer Rechtsverkehr* (ERV) - i.e., Law on Electronic Legal Proceedings – and section 89c subs. of *Gerichtsorganisationsgesetz* (GOG) – i.e., Court Organization Act.

[12] This reform follows earlier introduction of e-filing for public registries (land, companies) and with the enforcement agency. Similarly, in court proceedings, uncontested small claim requests (summary judgment) could already be filed and processed electronically before.

significant savings in the costs of enforcing a contract — court users save on reproduction costs and courthouse visits, whereas courts save on storage, archiving and court officers' costs (Doing Business Reforms Database).

In 2007 Belgium improved the process of enforcing contracts by establishing a mandatory procedural calendar that included binding time limits to submit written pleadings and tighter deadlines for the delivery of expert opinions. Act of 26 April 2007 on combating judicial delays (entered into force 1 September 2007), in fact, established that the agenda must be fixed by the parties or, if they fail to agree, by judges. If judges failed to render a judgment within a month after hearing a case, they were subject to disciplinary sanctions. Moreover, Act 15 May 2007 (in force on 1 September 2007) amended certain provisions of the Judicial Code concerning examination by court-appointed expert. This Act introduced tighter deadlines imposed by the court whereas the court determined the advance for expert fees to be paid into an escrow account held by the court. In this way, the court controlled the payment of the expert's fees, a measure likely to reduce the time-period for experts' missions (Doing Business Reforms Database).

Norway strengthened the process of contract enforcement by launching new civil procedural rules on 1 January 2008. These reforms aimed at ensuring a more efficient civil justice that provides all the parties with a rapid, correct and cheap resolution of disputes, especially without full litigation, and facilitated access to courts. In particular, the reforms encouraged the use of modern information and communication technologies whereas the introduction and monitoring of tighter deadlines in court procedures have allowed to expedite proceedings and saved time and costs. According to the law, judges must set time limits whereby all decisions have to be taken within six months. Parties must agree on a trial schedule in pre-trial conferences, and a computer system tracks all deadlines, requiring judges to justify all postponement. At the same

time, judges are encouraged to take on the role of mediator in court sittings or judicial mediator (Doing Business Reforms Database).

In Sweden, the "More modern court proceedings" reforms (entered into force on 1 November 2008) enhanced contracts enforcement through major changes in the procedural rules governing legal proceedings. These reforms, in fact, (a) required parties to move for leave to appeal judgments of the District Court in relation to commercial matters; (b) reinforced the role of the judge in active case management by requiring them to set time-tables identifying the issues, the order in which they shall be dealt with and include time limits; (c) made rules of evidence more flexible so that oral testimony is, under certain circumstances, not required. In those cases, sworn depositions will suffice. At the same time, statutory fees in relation to the enforcement of judgments were reviewed. More generally, they also promoted an extensive use of modern information technology, including video recordings and electronic case flow systems, thus producing significant benefits in terms of time and costs (Doing Business Reforms Database).

Table 1 summarizes the institutional framework involving the contracts enforcement reforms just described.

3. Identification strategy, data and variables

3.1. Identification Strategy

The implementation of the contract enforcement reforms highlighted in the previous section represents an ideal quasi-natural experiment to assess the impact of an improvement of judicial systems on banks' NPLs. Specifically, we rely on a difference-in-differences (DID) approach and compare the NPLs growth between banks whose headquarter is located in the four countries affected by the reforms (the treatment group) and banks whose headquarter is located in European

countries that have never implemented contracts enforcement reforms from 2007 to 2016 (the control group). Countries that belong to our control group are then: Denmark, Finland, France, Germany, Malta, Netherland and Slovenia. To ensure a cleaner DID analysis, we select the control countries as those with no contracts enforcement reforms until 2016 - that is, several years after the end of the treatment period – because the implementation of a law typically requires some time. In this way we rule out the possibility of any potential confounding factors due to the "settlement-time" of the law – i.e., the lag between the time in which a reform is started being prepared and the time in which it actually enters into force.

Regarding the treatment period, in the baseline analysis, we exploit the period spanning from three years before each country's reform (hereafter the pre-event period) to three years after the reform (hereafter the post-event period). Specifically, in the case of Austria and Belgium, the reforms occurred in 2007, hence the pre-event period is represented by the years 2004-2006 whereas the post-event period covers the years 2008-2010. By contrast, in the case of Norway and Sweden, the reforms occurred in 2008, the pre-event period covers the years 2005-2007 whereas the post-event period is 2009-2011. Two reasons mainly determine the choice of these time-windows. First, judicial system reforms may need some years to produce tangible effects due to legislative and administrative times; similarly, banks may need time to conform to the new rules. Second, we prefer to concentrate on such relatively short time-windows to conduct a cleaner DID test and avoid the potential confounding events that instead may occur in a longer period of analysis.

Furthermore, our DID identification enables us to face possible confounding events that may have occurred at the same time of contracts enforcement reforms, as the Basel II Accord (originally published in 2004 and became effective in Europe from 2007), the 2007-2008 Global

Financial Crisis, and the EU stress tests started in 2009.[13] Our DID model, in fact, estimates the Average Treatment Effect (ATE) i.e., the difference in mean outcomes (in our setting, the change in NPLs) between banks in the treatment groups relative to those in the control group in the aftermath of a shock affecting only banks in the first group. In our setting, contracts enforcement reforms certainly affect only banks in the country implementing the reform, and not those in other countries. Conversely, the regulatory and supervisory events mentioned above have impacted on all European banks and, thus, on banks in both the treatment and control groups: therefore, these events do not bias our ATE estimations.

3.2. Data

Our study draws data from three sources. We collect data about the contract enforcement reforms (i.e., the type, the features, and the timing of the legal changes) from the *Doing Business* database, maintained by the World Bank.[14] This database reports the most important changes in regulation in several areas of business activity for 190 economies and contains quantitative indicators on business regulations that can be compared over time and across countries.

The second source of data is *Refinitiv*/*Thomson Reuters Datastream* database, from which we collect country-specific data (namely, data on GDP growth and the bank lending rate to the

[13] The banking stress test was initially proposed in 2009 by the Committee of European Banking Supervisors (CEBS), which later became the European Banking Authority (EBA). There was some criticism about such first stress test, so in 2010 a new EU-wide stress test (based on different rules) was launched, involving 91 banks from 27 member countries. Another stress test exercise, again with different rules, was conducted by the EBA in 2011 (still in the sample period). Not all banks were stress tested.

[14] https://www.doingbusiness.org/

private sector). Finally, to better understand the behavior of banks' bad loans in our sample countries, we construct a comprehensive dataset containing detailed information on these banks' accounting variables from 2004 throughout 2011. Our balance sheet data are obtained from *Bankscope* database, and for each bank we use consolidated statements if reported by *Bankscope*, otherwise we use unconsolidated statements.

Overall, our sample contains 440 banks (of which 203 belong to the treatment group) and 1,847 bank-year observations.

3.3. Variables

Table II provides a description of the variables employed in our empirical analysis (Table II-A) and presents summary statistics based on the entire sample period (Table II-B). All variables are winsorized at the 1% level in both tails to mitigate the problems arising from outliers.

Table II-B shows significant variation in all the important variables. In particular, our dependent variable is *NPLs ratio* and it is measured as total impaired loans divided by total gross loans to customers (i.e., both to firms and households).[15] [16] The numerator of this ratio represents the impaired loans included in gross loans to customers. The denominator, instead, includes mortgage loans, other retail loans, corporate and commercial loans, additional loans and reserves

[15] During our sample period (i.e., 2004-2011), banks' balance sheets rarely reported detailed information on the flow of new NPLs since this type information was not always compulsory, and thus most banks preferred to omit it. Obviously, this limits the possibility of conducting an extensive cross-country analysis on the flow of new NPLs.

[16] Unfortunately, *Bankscope* does not always provide detailed information of NPLs by category (e.g., mortgage, business and consumer loans)

for impaired loans.[17] In line with our main hypothesis, when considering the entire sample period, *NPLs ratio* is on average higher (and more volatile) for the control banks (5.52%) than for the treated banks (2.42%).[18] Regarding the other variables, instead, Table II-B highlights that the average value of the bank lending rate to the private sector (*Lending rate*) over the entire sample period is quite similar for treated and control countries. The average ratio of profits to total assets (*ROA*) of all banks is 0.7%, with a standard deviation of 0.5%, whereas the annual growth rate of loans to customer (*Loan growth*) is on average 0.3% with a standard deviation of 3.33%. On average, the total assets of the control banks (approximately €80.87 billion) are larger than those of the treated banks (approximately €36.46 billion). Finally, the average value of the ratio of total regulatory capital to risk-weighted assets (*Capital ratio*) of all banks is 8.36%, with a standard deviation of 3.23%.

More importantly, when considering only the pre-event period, *NPLs ratio* is similar between treated and control banks. Specifically, since our analysis features four contract enforcement reforms (hence four treated countries) which occur in different years, we compare the pre-event average of *NPLs ratio* in each treated country versus the corresponding pre-event average of the control group. Despite the expected differences in both the country-specific and the financial statement variables between treated and control banks, Table III shows that there are no significant differences in the levels of *NPLs ratio* prior to the judicial reforms: the differences in the average ratios of the two groups are indistinguishable from zero in the pre-event periods (the *t*-statistics ranges from -1.03 of Belgium-2007 to -1.53 of Sweden-2008).

[17] Since all other items are net figures, reserves for impaired loans are included in the denominator.

[18] As a comparison, the mean levels of *NPLs ratio* in our sample are highly consistent with the corresponding data published in the European Banking Authority report on NPLs in Europe (2016).

4. Empirical Strategy

The causal effects of contracts enforcement reforms on the stock of European banks' NPLs are estimated by the following DID model:

$$Y_{ijt} = \alpha_{ij} + \alpha_t + \beta(Post_{it} \times Treated_i) + \delta(Trend_t \times Treated_i) + \gamma X_{i,t-1} + \theta Z_{ij,t-1} + \varepsilon_{ijt}. \quad [1]$$

where Y_{ijt} denotes the value of *NPLs ratio* for bank *j* in country *i* in year *t*, $Post_{it}$ is a treatment period dummy variable taking the value of *1* in the three years after the reform of country *i* enters into force – i.e., the post-event period. $Treated_i$ is a dummy equal to *1* for all banks located in the treated countries and 0 for all banks located in the control countries. We include a full set of bank-fixed effects (α_{ij}) to control for time-invariant unobservable bank characteristics and year-fixed effects (α_t) to control for aggregate time-varying shocks. To control for potentially different linear trends between treated and control banks, we also include an interaction term obtained by multiplying a time trend variable ($Trend_t$) by the dummy $Treated_i$. We cluster standard errors at country-level, thus allowing for correlation of the error term within countries over time (Bertrand, Duflo and Mullainathan, 2004).

The variable of main interest is the interaction term ($Post_{it} \times Treated_i$) which takes the value of *1* only when both the variables $Post_{it}$ and $Treated_i$ are equal to *1*, otherwise such term amounts to zero: we expect a negative value for this coefficient, indicating that an improvement in the enforcement of contracts should lead to a reduction in NPLs. By contrast, less efficient judicial systems, due for example to lengthy processes, delay the debtor's payment in case of default, thus raising moral hazard behaviors and, hence, the flow of new NPLs. At the same time, legal uncertainties and a lengthy foreclosure process worsen the collateral repossession framework and

imply that the disposal of accumulated NPLs takes a longer time, thus contributing to the build-up of NPLs.

In Equation [1], it is important to control for country (*X*) and bank (*Z*) specific variables (lagged by one year) to account for observable time-varying heterogeneity in country and banks characteristics on the quality of bank lending. Regarding country-level controls, and in line with the literature on NPLs determinants, we proxy economic growth using the annual growth rate of real gross domestic product (*GDP growth*) and private sector's cost of credit using the bank lending rate to the private sector (*Lending rate*). According to Salas and Saurina (2002), in fact, when economic growth slows down or becomes negative, companies and households reduce their cash flows; in turn, this decreases their ability to repay bank loans, and increases the incidence of non-performing-loans. On the other hand, a rise in lending rates increases the real value of borrowers' debt and makes debt servicing more expensive. In turn, this increases loan defaults and hence NPLs. Bank-level controls include measures of profitability, credit growth, regulatory capital, and size. Specifically, bank profitability is proxied by the ratio of pre-tax profits to total assets (*ROA*). As highly profitable banks have fewer incentives to engage in high-risk activities, they should exhibit lower levels of NPLs (Makri, Tsagkanos and Bellas, 2014). Our measure of credit growth is the annual growth rate of loans to the customer (*Loan growth*). Several papers have documented that rapid credit extension is one of the most important causes of bad loans (see, among others, Clair, 1992, and Kwan and Eisenbeis, 1997). Banks interested in market share growth, in fact, are more likely to reduce their credit standards, which in turn increase the likelihood to be penalized by adverse selection (Shaffer, 1998). For these reasons, faster loan growth can lead to higher loan losses (Keeton, 1999; Foos, Norden and Weber, 2010). We account for bank net worth by the ratio of total regulatory capital to risk-weighted assets (*Capital ratio*),

and for bank size using the logarithm of bank total assets (*TA*). The "moral hazard" hypothesis suggests that banks with relatively low capital have stronger (moral-hazard) incentives to increase the riskiness of their loan portfolios, which in turn leads to higher non-performing loans (Berger and DeYoung, 1997). Similarly, according to the "too big to fail" hypothesis, large banks may take advantage of their market power position and engage more in risk-taking (Louzis et al, 2012; Cai, Dickinson, and Kutan, 2016). By contrast, another strand of literature suggests a negative relationship between bank size and the level of NPLs. Large banks, in fact, are better able to i) reduce the level of troubled loans because of more diversification opportunities (Salas and Saurina, 2002; Rajan and Dhal, 2003), and ii) evaluate the quality of loans because of richer resources (Hu, Li and Chiu, 2004).

5. Results

Table IV reports the results of the DID regressions. We first estimate Equation [1] considering the four reforms jointly – that is when the treatment group is constituted by the all banks located in Austria, Belgium, Norway and Sweden (Table IV-A). In this case, the estimate of the interaction term (*Post* × *Treated*) provides us with the ATE, that is the average pre-post reforms difference in the *NPLs ratio* of countries that have implemented contracts enforcement reforms, relative to the average pre-post reforms difference of countries that have not implemented such reforms. Specifically, we present estimates from three specifications which differ in the type of controls used to estimate Equation [1]. The first specification only controls for bank and year fixed effects (Column 1). The second specification, instead, controls also for potentially different linear trends between treated and control banks (Column 2). Finally, the third specification includes all the control variables mentioned above, that is banks and year fixed effects, bank-level, country-level

and linear trends variables (Column 3). Table IV-A shows that the *Post* × *Treated* interaction term is negative, large and statistically significant across all three specifications, indicating that the asset quality of affected banks benefited from judicial efficiency reforms, thus confirming our hypothesis that countries that implemented contracts enforcement reforms experienced a significant decrease in their level of NPLs relative to countries that have not implemented such reforms.

Moreover, to better assess the impact of each country's reform on its banks' NPLs, we also estimate Equation [1] separately for each of the reforms in our sample (Table IV-B): in this case, each estimation contains just one treated country at a time, whereas the control group remains the same in all estimations. More importantly, our results highlight that the evidence that more efficient judicial systems contribute to the reduction of NPLs holds not just in aggregate (i.e., when we consider all the reforms jointly), but also for each reform considered separately. Table IV-B, in fact, reports that the ATE estimate of each reform is once again negative, large and statistically significant across all three specifications. To better understand the economic impact of our estimates, the third specification shows for example that an improvement of contracts enforcement reduces NPLs from a minimum of 1.69 percentage points in the case of Norway to a maximum of 3.44 percentage points in the case of Sweden. Moreover, Table V shows that these results are due to the reduction of the total impaired loans (i.e., the numerator of *NPLs ratio*) after the legal change.

The validity of the difference-in-difference methodology hinges on a crucial assumption: the trends in the outcome variable of interest – i.e., *NPLs ratio* - for both treated and control banks must exhibit a similar pattern ("parallel") prior to the implementation of the contracts enforcement reforms. Therefore, it is very important to check that the aforementioned trends are indeed parallel in our setting. We do so in several ways. First, in Table III we already showed that, despite the

expected differences in the financial-statement characteristics of treated and control banks, there are no significant differences in their average levels of NPLs ratios prior to the judicial reforms. Second, in Figure 1 we report the time-series dynamics of the average *NPLs ratio*: their behavior supports the common trend assumption. Third, we also provide estimates of dynamics DID regressions (separately for each reform) in which the dummy variable *Treated* is interacted with individual year dummies to capture the time-varying effects of the contracts enforcement reforms on NPLs before and after the passage of the law. The results reported in Table VI further strengthen the validity of our identification strategy. For each treated country we find that the interactions between the variable *Treated* and the year dummies *before* the passage of the contracts enforcement reforms are not significant, thus confirming that there is a parallel trend in the NPLs ratios of treated and control banks. By contrast, the two trends diverge in the post-event periods as result of the reforms. In this case, the interactions between the variable *Treated* and the year dummies *after* the passage of the reforms are significant and large, highlighting also a strong persistence of the effects of judicial efficiency improvements in all three years after the reforms.

6. Robustness

Like other quasi-natural experimental methods, difference-in-difference test strategies naturally call for several robustness checks that help verify the model's assumptions, thus ensuring internal validity. The first robustness tests we conduct control for differences across countries related to several features of the business activity – which proxy for other reforms affecting the countries' business activity - and for different estimation periods. Moreover, in the Internet Appendix, we also: *i)* address the concerns arising from the use of an unbalanced sample; *ii)* improve the similarities between treated and control banks by providing an alternative definition

of the control countries, which gives rise to event-specific control groups; and *iii)* control for a possible mismatch between the country where the banks headquarter is located and the country where the lending activity occurs by excluding from our sample systemically important financial institutions (SIFIs).

6.1 Controlling for business activity indicators

In this section, we re-estimate Equation [1] also controlling for differences across countries related to several features of the business activity. Specifically, we exploit the indicators provided by the World Bank *Doing Business* database measuring the countries' ease of: *i)* starting a business, *ii)* getting credit, *iii)* paying taxes and *iv)* resolving insolvency. Each indicator is measured on a scale from 0 to 100, where 0 represents the lowest and 100 represents the best performance, and captures the gap of each economy from the best regulatory performance observed in each area of business activity across all economies. Table VII highlights that our findings still hold: the ATE estimates are negative and statistically significant.

6.2 Alternative estimation periods

Our identification strategy exploits an estimation period from three years prior to each country's reform to three years after. Thus, in order to investigate whether this feature of the analysis drives our results, we re-estimate our econometric specifications using alternative estimation periods (Table VIII). The results in these regressions essentially confirm our main findings and conclusions.

7. Conclusions

In the last few years, several policymakers have called for effective measures to reduce the stock of NPLs held by European banks (Nouy, 2017; Enria, 2019; Dombrovskis, 2020). In fact, despite European NPLs have dropped to €580 billion at the end of 2019, after the peak of €1.0 trillion reached at the end of 2016, supervisors believe their level is still too high to be considered comfortable, especially in the case a new economic-financial storm should outbreak. Unfortunately, such a storm has arrived: the Covid-19 pandemic has hit the economy so hard that, in 2020, every EU country is in crisis. In turn, the pandemic will most likely produce long-lasting effects on borrowers' credit standing, new NPLs and, more generally, the stability of the overall banking system. More importantly, as financial crises and recessions are cyclical (Reinhart and Rogoff, 2009), the problem of high NPLs is destined to occur frequently, explaining why a set of effective measures that can attenuate their adverse effects on financial stability and the real economy is urged. Given this background, it is essential to understand how it is possible to reduce NPLs without imposing stricter rules and capital requirements on the banking system.

In this paper, we show that the efficiency of judicial system plays an important role in determining how NPLs are handled. Specifically, using the contracts enforcement reforms implemented in four European countries as a quasi-natural experimental setting, our difference-in-differences estimates show that more efficient judicial systems lead to a significant, large and persistent reduction of NPLs' stocks, which does not vanish even three years after the implementation of the reforms. Moreover, when controlling for banks and year fixed effects, bank-level, country-level and linear trends variables, NPLs' decline ranges from a minimum of 1.69 percentage points in the case of Norway to a maximum of 3.44 percentage points in the case of

Sweden, relative to countries that have never implemented contracts enforcement reforms. Our findings are robust to a variety of robustness checks and extensions.

The economic forces leading to the decline in the NPLs held by European banks of treated countries hinge on several features of the judicial system. On the one hand, more efficient judicial systems – for example, due to faster foreclosure processes, the introduction of tighter deadlines in court procedures, and the use of modern information and communication technologies - imply an improvement in settlement of the contract enforcement and ensure a better framework for collateral repossession. In turn, this increases NPLs' recovery rates, reduces the time of disposal and enhances their market value, thus reducing the amount of existing NPLs held by banks. On the other hand, more efficient judicial systems also reduce the borrowers' incentives to strategic default – i.e., defaults that occur not because the borrower is unable to repay his obligations (for example, due to bad luck) but because, although potentially solvent, he is unwilling to do so – by increasing the perceived costs of the presumed sanctions. As a result, also the flow of new NPLs is expected to decline.

Our results have important policy implications in favor of the view expressed by banking supervisors: the NPLs issue cannot be solved only by applying stricter rules and supervisory practices, but it also requires more broad reforms involving the legal, institutional and judicial systems of countries. Moreover, our results are significant in this historical moment: Covid-19 represents an unprecedented storm, and an efficient judicial system would be crucial to help banks facing the virus' adverse financial effects.

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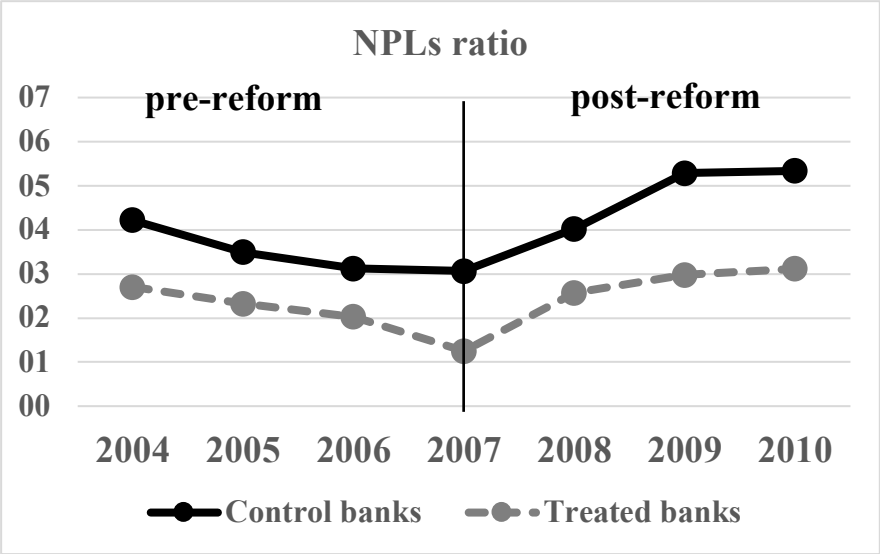
Figure 1: Non-performing loans dynamics for treated and control banks

Figure 1 plots the average values of the total impaired loans as a percentage of total loans shown separately for the countries the implemented contracts enforcement reforms in 2007 (Panel A) and in 2008 (Panel B). In panel A, the treated banks include all banks located in Austria and Belgium. In panel B, the treated banks include all banks located in Norway and Sweden. In both figures, control banks are constituted by all banks located in the European countries that have never implemented enforcing contracts reforms between 2007 and 2016 (i.e., Denmark, Finland, France, Germany, Malta, Netherland, and Slovenia).

Panel A: Average values of *NPLs ratio* for countries that implemented reforms in 2007



Panel B: Average values of *NPLs ratio* for countries that implemented reforms in 2008

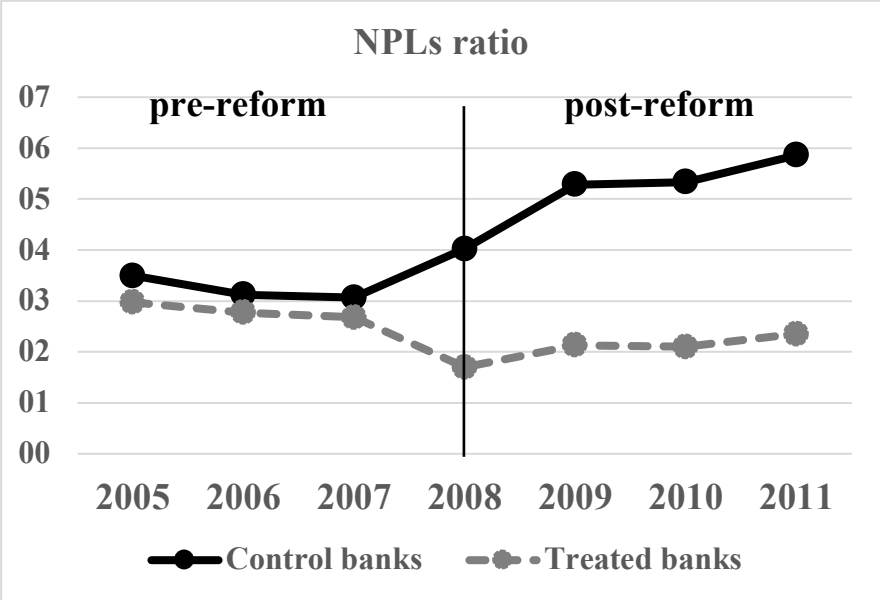


Table I: Contracts enforcement legal changes

Table I provides a summary of the contracts enforcement legal changes analyzed in this paper. Data are from the *Doing Business* database, maintained by the World Bank. The column *Year* refers to the year in which the reform enters in force.

Country	*Year*	*Legal Reform*
Austria	2007	Section 11 of *Elektronischer Rechtsverkehr* (ERV) - i.e., Law on Electronic Legal Proceedings – and section 89c subs. of *Gerichtsorganisationsgesetz* (GOG) – i.e., Court Organization Act.
Belgium	2007	*Act of 26 April 2007* *Act 15 May 2007*
Norway	2008	*Dispute Act* - Reform of Civil Procedure
Sweden	2008	*More modern court proceedings* reform

Table II: Variables definitions and summary statistics

This table consists of two panels. In Panel A we provide a description of all the variables used in the empirical analysis. Panel B reports the summary statistics of the variables over the entire sample period. For each variable, we compute the sample mean, the standard deviation (reported in parentheses), and the 5% and 95% percentiles of the distribution (reported in square brackets). Treated banks refer to all banks located in the four countries that implemented the contracts enforcement reforms (i.e., Austria-2007, Belgium-2007, Norway-2008, and Sweden-2008). Control banks refer to all banks located in the European countries that have never implemented enforcing contracts reforms between 2007 and 2016 (i.e., Denmark, Finland, France, Germany, Malta, Netherland, and Slovenia). All variables are winsorised at 1% and 99% to mitigate the problem of outliers.

Panel A: Variables definitions and sources

Variable	Source	Definition
Dependent variable		
NPLs ratio	Bankscope	Non-performing loans / Total loans
Country-specific controls		
GDP growth	Refinitiv/Datastream	Annual growth rate of real gross domestic product
Lending rate	Refinitiv/Datastream	Bank lending rate to the private sector
Bank-specific controls		
ROA	Bankscope	Profit before tax / Total assets
Loan growth	Bankscope	Annual growth rate of loans to customer
Capital ratio	Bankscope	Total regulatory capital / Risk weighted assets
Total Asset	Bankscope	Total assets

Panel B: Summary Statistics over the entire sample period

Variable	All banks (Obs.=3898)	Treated banks (Obs.=1523)	Control banks (Obs.=2375)
NPLs ratio (%)	4.31 (6.68) [0.26 ; 12.8]	2.42 (3.61) [0.17 ; 7.01]	5.52 (7.83) [0.33 ; 15.99]
GDP growth (%)	1.17 (2.24) [-2.87 ; 3.94]	1.35 (2.42) [-3.77 ; 4.70]	1.05 (2.10) [-2.87 ; 3.72]
Lending rate (%)	4.57 (0.83) [3.51 ; 6.17]	4.79 (0.95) [3.59 ; 6.99]	4.44 (0.71) [3.51 ; 5.50]
Capital ratio (%)	8.36 (3.23) [4.08 ; 13.91]	9.04 (3.17) [4.14 ; 13.91]	7.93 (3.20) [4.08 ; 13.91]
ROA (%)	0.70 (0.50) [-0.08 ; 1.58]	0.80 (0.45) [-0.02 ; 1.36]	0.64 (0.52) [-0.20 ; 1.72]
Loan growth (%)	0.30 (3.33) [-5.10 ; 5.31]	-0.36 (3.35) [-5.77 ; 5.31]	0.72 (3.25) [-4.73 ; 5.31]
Total Asset (bn €)	63.51 (233.89) [0.12 ; 366.14]	36.46 (122.81) [0.09 ; 276.39]	80.87 (281.7) [0.15 ; 523.54]

Table III – Summary statistics over the pre-event period

Table III reports the mean and the standard deviation (reported in parentheses) of our variables over the pre-event periods. In panel A the summary statistics are computed over the pre-event period corresponding to the 2007 reforms (i.e., the years 2004-2006). In panel B the summary statistics are computed over the pre-event period corresponding to the 2008 reforms (i.e., the years 2005-2007). Control banks include all banks located in the European countries that have never implemented enforcing contracts reforms between 2007 and 2016 (i.e., Denmark, Finland, France, Germany, Malta, Netherland, and Slovenia). ***, **, and * implies significance at the 99% level, 95% level, and 90% level, respectively. All variables are winsorised at 1% and 99% to mitigate the problem of outliers.

Panel A: Summary statistics over the pre-event years 2004-2006

	Austrian banks	Belgian banks	Control banks	Difference Austrian vs control banks	Difference Belgian vs control banks
NPLs ratio	2.28	2.38	3.49	-1.21	-1.11
	(0.36)	(0.88)	(2.85)	(1.17)	(1.08)
GDP growth	2.81	2.71	2.64	0.17	0.07
	(0.54)	(0.66)	(1.02)	(0.42)	(0.39)
Lending rate	4.52	4.77	4.87	-0.35***	-0.10
	(0.13)	(0.20)	(0.24)	(0.10)	(0.09)
ROA	1.05	0.61	1.02	0.03	-0.41*
	(0.84)	(0.08)	(0.56)	(0.24)	(0.21)
Capital ratio	9.18	4.07	7.14	2.05	-3.07**
	(3.73)	(0.02)	(3.26)	(1.36)	(1.24)
Loan growth	0.78	0.97	1.13	-0.36	-0.16
	(4.22)	(3.17)	(3.11)	(1.31)	(1.20)
Total Asset	15.25	19.99	15.54	-0.29	4.45***
	(0.58)	(1.22)	(2.12)	(0.87)	(0.81)

Panel B: Summary statistics over the pre-event years 2005-2007

	Norwegian banks	Swedish banks	Control banks	Difference Norwegian vs control banks	Difference Swedish vs control banks
NPLs ratio	2.82	2.72	3.15	-0.32	-0.43
	(1.41)	(1.25)	(2.87)	(0.26)	(0.28)
GDP growth	2.78	3.59	2.75	0.03	0.84***
	(0.26)	(0.71)	(1.21)	(0.11)	(0.12)
Lending rate	4.94	5.83	5.00	-0.06	0.83***
	(0.79)	(0.34)	(0.35)	(0.06)	(.04)
ROA	0.89	0.96	0.94	-0.05	0.02
	(0.33)	(0.52)	(0.55)	(0.05)	(0.06)
Capital ratio	9.36	7.96	6.75	2.61***	1.21***
	(2.38)	(3.51)	(2.87)	(0.29)	(0.34)
Loan growth	-0.14	-0.83	1.19	-1.33***	-2.02***
	(2.52)	(3.19)	(3.02)	(0.31)	(0.34)
Total Asset	13.32	12.80	16.13	-2.81***	-3.32***
	(1.46)	(1.94)	(2.24)	(0.21)	(0.24)

TABLE IV – The impact of contracts enforcement reforms on *NPLs ratio*

Table IV reports the difference-in-differences results from estimating Equation [1] when the four reforms are considered jointly (Panel A) and separately for each reform (Panel B). In Panel A the treatment group includes all banks located in Austria, Belgium, Norway and Sweden. In Panel B the treatment group includes all banks of just one treated country at a time. In both tables, the control group includes all banks located in the European countries that have never implemented enforcing contracts reforms between 2007 and 2016 (i.e., Denmark, Finland, France, Germany, Malta, Netherland, and Slovenia). The dependent variable is *NPLs ratio*. *Post* is a dummy equal to 0 in the pre-event period. *Treated* is a dummy equal to 1 if the bank belongs to the treatment group. *Trend* denotes a time trend variable. The pre-event period spans the years 2004-2006 in the case of Austria and Belgium and the years 2005-2007 in the case of Norway and Sweden. In each table, column (1) controls only for bank and year fixed effects; column (2) also controls for potentially different linear trends between treated and control banks; column (3) includes all control variables (i.e., banks and year fixed effects, bank controls, country controls, and linear trends variables). Country and bank controls are described in Table II and are lagged by one year with respect to the dependent variable. Robust standard errors are clustered at the country level and are reported in parentheses. ***, **, and * implies significance at the 99% level, 95% level, and 90% level, respectively.

Panel A: DID results when the four reforms are considered jointly

	(1)	(2)	(3)
Post × Treated	-2.67***	-2.77***	-2.62***
	(0.41)	(0.52)	(0.42)
Bank fixed effects	yes	yes	yes
Year fixed effects	yes	yes	yes
Trend × Treated		yes	yes
Country controls			yes
Bank controls			yes
Number of banks	440	440	440
Observations	1907	1907	1907
Adj. R2	0.85	0.85	0.85

Panel B: DID results separately for each reform

	Austria-2007			Belgium-2007		
	(1)	(2)	(3)	(1)	(2)	(3)
Post × Treated	-2.39***	-3.15***	-3.26***	-1.06***	-2.01***	-2.37***
	(0.30)	(0.13)	(0.36)	(0.29)	(0.25)	(0.14)
Bank fixed effects	yes	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes	yes
Trend × Treated		yes	yes		yes	yes
Country controls			yes			yes
Bank controls			yes			yes
Number of banks	212	212	212	213	213	213
Observations	731	731	731	742	742	742
Adj. R2	0.82	0.82	0.83	0.82	0.82	0.83

	Norway-2008			Sweden-2008		
	(1)	(2)	(3)	(1)	(2)	(3)
Post × Treated	-2.79***	-2.36***	-1.69**	-2.69***	-3.28***	-3.44***
	(0.46)	(0.52)	(0.62)	(0.45)	(0.54)	(0.76)
Bank fixed effects	yes	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes	yes
Trend × Treated		yes	yes		yes	yes
Country controls			yes			yes
Bank controls			yes			yes
Number of banks	344	344	344	308	308	308
Observations	1316	1316	1316	1202	1202	1202
Adj. R2	0.87	0.87	0.87	0.85	0.85	0.86

TABLE V – The impact of contracts enforcement reforms on *impaired loans*

Table V reports the difference-in-differences results from estimating Equation [1] separately for each reform when the dependent variable is represented by the total impaired loans. The treatment group includes all banks of just one treated country at a time whereas the control group includes all banks located in the European countries that have never implemented enforcing contracts reforms between 2007 and 2016 (i.e., Denmark, Finland, France, Germany, Malta, Netherland, and Slovenia). The dependent variable is represented by the total impaired loans. *Post* is a dummy equal to 0 in the pre-event period. *Treated* is a dummy equal to 1 if the bank belongs to the treatment group. *Trend* denotes a time trend variable. The pre-event period spans the years 2004-2006 in the case of Austria and Belgium and the years 2005-2007 in the case of Norway and Sweden. For each reform we control for banks and year fixed effects, bank controls, country controls, and linear trends variables. Country and bank controls are described in Table II and are lagged by one year with respect to the dependent variable. Robust standard errors are clustered at the country level and are reported in parentheses. ***, **, and * implies significance at the 99% level, 95% level, and 90% level, respectively.

	Austria-2007	**Belgium-2007**	**Norway-2008**	**Sweden-2008**
Post × Treated	-1.41**	-2.89***	-1.13*	-0.98**
	(0.43)	(0.24)	(0.52)	(0.37)
Bank fixed effects	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes
Trend × Treated	yes	yes	yes	yes
Country controls	yes	yes	yes	yes
Bank controls	yes	yes	yes	yes
Number of banks	212	213	344	308
Observations	731	742	1316	1202
Adj. R2	0.87	0.86	0.89	0.89

TABLE VI: DID results with leads and lags

Table VI reports the difference-in-differences results from estimating Equation [1] with leads and lags of the treatment separately for each reform. The treatment group includes all banks of just one treated country at a time whereas the control group includes all banks located in the European countries that have never implemented enforcing contracts reforms between 2007 and 2016 (i.e., Denmark, Finland, France, Germany, Malta, Netherland, and Slovenia). The dependent variable is *NPLs ratio*. *Treated* is a dummy equal to 1 if the bank belongs to the treatment group. *Trend* denotes a time trend variable. *Y2005* is a dummy equal to 1 for year 2005, and zero otherwise. Other year dummies are defined analogously. The pre-event period spans the years 2004-2006 in the case of Austria and Belgium and the years 2005-2007 in the case of Norway and Sweden. For each reform we control for banks and year fixed effects, bank controls, country controls, and linear trends variables. Country and bank controls are described in Table II and are lagged by one year with respect to the dependent variable. Robust standard errors are clustered at the country level and are reported in parentheses. ***, **, and * implies significance at the 99% level, 95% level, and 90% level, respectively

	Austria-2007	**Belgium-2007**	**Norway-2008**	**Sweden-2008**
Y2005 × Treated	0.19	0.28		
	(0.28)	(0.23)		
Y2006 × Treated	0.22	0.42	-0.21	-0.53
	(0.31)	(0.24)	(0.19)	(0.51)
Y2007 × Treated			-0.44	-1.04
			(0.33)	(0.67)
Y2008 × Treated	-1.89***	-0.99**		
	(0.18)	(0.35)		
Y2009 × Treated	-2.47***	-1.29**	-3.30***	-3.77***
	(0.32)	(0.54)	(0.32)	(0.73)
Y2010 × Treated	-1.55***	-0.95*	-2.49***	-3.30***
	(0.22)	(0.43)	(0.33)	(0.47)
Y2011 × Treated			-3.35***	-2.63***
			(0.27)	(0.49)
Bank fixed effects	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes
Trend × Treated	yes	yes	yes	yes
Country controls	yes	yes	yes	yes
Bank controls	yes	yes	yes	yes
Number of banks	212	213	344	308
Observations	731	742	1316	1202
Adj. R2	0.82	0.83	0.87	0.86

TABLE VII – DID results when controlling also for business activity indicators

Table VII reports the difference-in-differences results from estimating Equation [1] with the inclusion of several business activity control variables, separately for each reform. The treatment group includes all banks of just one treated country at a time whereas the control group includes all banks located in the European countries that have never implemented enforcing contracts reforms between 2007 and 2016 (i.e., Denmark, Finland, France, Germany, Malta, Netherland, and Slovenia). The dependent variable is represented by *NPLs ratio*. *Post* is a dummy equal to 0 in the pre-event period. *Treated* is a dummy equal to 1 if the bank belongs to the treatment group. *Trend* denotes a time trend variable. The variables *Starting business score, Getting credit score, Paying taxes score* and *Resolving insolvency score* are provided by the World Bank *Doing Business* database and measure the countries' ease of: *i)* starting a business, *ii)* getting credit, *iii)* paying taxed and *iv)* resolving insolvency, respectively. The pre-event period spans the years 2004-2006 in the case of Austria and Belgium and the years 2005-2007 in the case of Norway and Sweden. For each reform we also control for banks and year fixed effects, bank controls, country controls, and linear trends variables. Country and bank controls are described in Table II and are lagged by one year with respect to the dependent variable. Robust standard errors are clustered at the country level and are reported in parentheses. ***, **, and * implies significance at the 99% level, 95% level, and 90% level, respectively.

	Austria-2007	**Belgium-2007**	**Norway-2008**	**Sweden-2008**
Post × Treated	-2.39***	-2.85***	-1.83*	-5.33***
	(0.42)	(0.30)	(0.89)	(1.07)
Starting business score	0.03	0.03	-0.01	0.11
	(0.05)	(0.06)	(0.06)	(0.14)
Getting credit score	-0.03	-0.03	-0.06	0.00
	(0.05)	(0.05)	(0.04)	(0.04)
Paying taxes score	-0.09	-0.09	-0.07	-0.04
	(0.05)	(0.05)	(0.05)	(0.06)
Resolving insolvency score	0.10**	0.07*	0.06	0.13
	(0.04)	(0.04)	(0.06)	(0.08)
Bank fixed effects	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes
Trend × Treated	yes	yes	yes	yes
Country controls	yes	yes	yes	yes
Bank controls	yes	yes	yes	yes
Number of banks	199	201	335	299
Observations	618	630	1226	1102
Adj. R2	0.84	0.84	0.87	0.86

TABLE VIII – DID results from using alternative estimation periods

Table XIII reports the difference-in-differences results from estimating Equation [1] in the case of alternative estimation periods, separately for each reform. The treatment group includes all banks of just one treated country at a time, whereas the control group includes all banks located in the European countries that have never implemented enforcing contracts reforms between 2007 and 2016 (i.e., Denmark, Finland, France, Germany, Malta, Netherland, and Slovenia). The dependent variable is *NPLs ratio*. *Post* is a dummy equal to 0 in the pre-event period. *Treated* is a dummy equal to 1 if the bank belongs to the treatment group. *Trend* denotes a time trend variable. For each reform, we consider the following estimation periods: from 1 year prior to the reform to 1 year after (first column), from 2 years prior to the reform to 2 years after (second column), and, for comparison, from 3 years prior to the reform to 3 years after (third column). For each reform, we control for banks and year fixed effects, bank controls, country controls, and linear trends variables. Country and bank controls are described in Table II and are lagged by one year with respect to the dependent variable. Robust standard errors are clustered at the country level and are reported in parentheses. ***, **, and * implies significance at the 99% level, 95% level, and 90% level, respectively.

Panel A: DID results using alternative estimation periods (t = 2007)

	Austria-2007			**Belgium-2007**		
Estimation period	(t-1, t+1)	(t-2, t+2)	(t-3, t+3)	(t-1, t+1)	(t-2, t+2)	(t-3, t+3)
Post × Treated	-3.45***	-2.57**	-3.26***	-1.21***	-1.11***	-2.37***
	(0.53)	(1.02)	(0.36)	(0.19)	(0.30)	(0.14)
Bank fixed effects	yes	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes	yes
Trend × Treated	no	yes	yes	no	yes	yes
Country controls	yes	yes	yes	yes	yes	yes
Bank controls	yes	yes	yes	yes	yes	yes
Number of banks	76	177	212	78	182	213
Observations	152	468	731	156	481	742
Adj. R2	0.82	0.77	0.83	0.82	0.77	0.83

Panel B: DID results using alternative estimation periods (t = 2008)

	Norway-2008			**Sweden-2008**		
Estimation period	(t-1, t+1)	(t-2, t+2)	(t-3, t+3)	(t-1, t+1)	(t-2, t+2)	(t-3, t+3)
Post × Treated	-3.67**	-3.27**	-1.69**	-4.86***	-3.20***	-3.44***
	(1.08)	(0.96)	(0.62)	(0.95)	(0.82)	(0.76)
Bank fixed effects	yes	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes	yes
Trend × Treated	no	yes	yes	no	yes	yes
Country controls	yes	yes	yes	yes	yes	yes
Bank controls	yes	yes	yes	yes	yes	yes
Number of banks	200	295	344	176	272	308
Observations	400	882	1316	352	807	1202
Adj. R2	0.71	0.85	0.87	0.67	0.87	0.86

Internet Appendix

Additional Robustness Tests

In this Internet Appendix we perform additional robustness tests that help verify the model's assumptions, thus ensuring internal validity. More precisely, we first address the concerns arising from the use of an unbalanced sample. Next, to improve the similarities between treated and control banks we also provide an alternative definition of the control countries which gives raise to event-specific control groups. Additionally, we control for possible mismatch between the country where the bank's headquarter is located and the country where the lending activity occurs by excluding from our sample systemically important financial institutions (SIFIs). Finally, we investigate the role played by bank health on the impact of the legal reforms on the NPLs.

A.1 Balanced Panel

Since during our sample period some banks cease to exist due to bankruptcy or takeovers whereas others enter the banking market, the sample of banks used in our analysis is clearly unbalanced. Thus, in order to investigate whether our results are driven by this feature of the database, we re-estimate our econometric specifications using only those banks whose data are available for the whole sample period. Table A1 shows that our findings still hold when the sample is balanced: the estimates of *Post* × *Treated* are negative, statistically significant and persistent both when the four reforms are analyzed jointly and when they are investigated separately.

A.2 Alternative definition of control countries

In the baseline regressions reported in Table IV, we select the control countries as those with no contracts enforcement reforms until 2016 - that is several years after the end of the treatment

period. The main advantage of this definition is that it rules out the possibility of potential confounding factors due to the settlement-time of the reforms (i.e., the lag between the time in which a reform is started being prepared and the time in which it actually enters into force). On the other side, however, such conservative approach also implies that the control group is the same for each treated country. Therefore, to improve the matching between the financial characteristics of the two groups of banks, in this section we provide an alternative definition of the control countries based on reform-specific control groups. More precisely, the control group used to investigate the impact of a given reform includes all countries that have never implemented contracts enforcement reforms in the time-window starting from three years prior to that reform until three years after the reform. For example, according to the new definition, the control group corresponding to the reform occurred in Austria-2007 is given by all European countries that have never implemented reforms between 2004 and 2010. The results reported in Table A2 are similar to those shown in the baseline results (Table IV), both in terms of economic magnitude and statistical significance.

A.3 Excluding SIFIs

To further strengthen the validity of our results, we also control for the potential mismatch between the geographical area in which a law is effective and the area where the bank lending activity takes place. Domestic banks, in fact, typically lend more to domestic borrowers, but a certain fraction of lending, especially for large banks, may occur across borders, that is at international level. To better understand the consequences of this point, consider the following example. A German bank lends to a firm located in another country, say Austria, through its local branches. In this case, since its headquarter is located in Germany, the bank would be considered as belonging to the

control group, even though the lending quality of its local branches will benefit from the passage of the reform occurred in Austria. In other words, this implies that our treatment group may underestimate the effects of the contract enforcement reforms. In this section, we address this issue by excluding from our sample systemically important financial institutions (SIFIs) since they are the type of banks that can have the largest share of international lending. More precisely, we exclude seven SIFI[19] and then re-estimate equation [1]. The results reported in Table A3 highlight that the economic magnitude and the statistical significance of the ATE estimates are similar to those shown in the baseline results.

A.4 Controlling for bank health

As additional robustness test we investigate the role played by bank health on the impact of the legal reforms on the NPLs. In line with the banking literature, we employ a definition of bank health based on the capital ratio. To this purpose, we sort all banks in our sample according to their capital ratio (measured as the ratio of regulatory capital over risk-weighted assets) and create two new variables: i) *High Cap.* which is a dummy variable equal to 1 if the bank belongs to the top 25th percentile of the capital ratio distribution, which proxies good-type banks; and ii) *Low Cap.* which is a dummy variable equal to 1 if the bank belongs to the bottom 25th percentile of the capital ratio distribution, which proxies weak-type banks.

In the next step, we interact such variables with the main variable of our analysis, that is *Post* × *Treated*, to better understand the role that bank health plays on the impact of the legal reforms on the NPLs. Table A4 reports the corresponding results:

We notice that the coefficient of the variable *Post* × *Treated* is still negative, large and statistically

[19] More precisely, one from Belgium, three from France, two from Germany, one from Netherlands.

significant across all three specifications, indicating that the asset quality of affected banks benefited from judicial efficiency reforms. Interestingly, the coefficient of the interaction *Post* × *Treated* × *Low Cap.*, instead, is positive and statistically significant across all three specifications, highlighting that weak banks exhibit a lower reduction on NPLs thank good banks, in line with the findings of Schiantarelli, Stracchini and Strahan (2020). For example, column 3 shows that the overall reduction of the NPLs ratio is 2.06 percentage points (i.e., -2.69 + 0.63) in the case of weak banks versus 2.69 percentage points in the case of good banks.

Table A1 – DID results from using a balanced sample of banks

Table A1 reports the difference-in-differences results from estimating Equation [1] using a balanced sample of banks when the four reforms are considered jointly (Panel A) and separately for each reform (Panel B). Specifically, we exclude banks that do not report continuously financial statement information during the estimation period. In Panel A the treatment group includes all banks located in Austria, Belgium, Norway and Sweden. In Panel B the treatment group includes all banks of just one treated country at a time. In both tables, the control group includes all banks located in the European countries that have never implemented enforcing contracts reforms between 2007 and 2016 (i.e., Denmark, Finland, France, Germany, Malta, Netherland, and Slovenia). The dependent variable is *NPLs ratio*. *Post* is a dummy equal to 0 in the pre-event period. *Treated* is a dummy equal to 1 if the bank belongs to the treatment group. *Trend* denotes a time trend variable. The pre-event period spans the years 2004-2006 in the case of Austria and Belgium and the years 2005-2007 in the case of Norway and Sweden. In each table, column (1) controls only for bank and year fixed effects; column (2) also controls for potentially different linear trends between treated and control banks; column (3) includes all control variables (i.e., banks and year fixed effects, bank controls, country controls, and linear trends variables). Country and bank controls are described in Table II and are lagged by one year with respect to the dependent variable. Robust standard errors are clustered at the country level and are reported in parentheses. ***, **, and * implies significance at the 99% level, 95% level, and 90% level, respectively.

Panel A: DID results using a balanced sample - the four reforms are considered jointly

	(1)	(2)	(3)
Post × Treated	-2.15***	-2.81***	-3.02***
	(0.19)	(0.79)	(0.61)
Bank fixed effects	yes	yes	yes
Year fixed effects	yes	yes	yes
Trend × Treated		yes	yes
Country controls			yes
Bank controls			yes
Number of banks	180	180	180
Observations	895	895	895
Adj. R2	0.86	0.86	0.86

Panel B: DID results using a balanced sample - separately for each reform

	Austria-2007			Belgium-2007		
	(1)	(2)	(3)	(1)	(2)	(3)
Post × Treated	-2.17***	-3.51***	-4.45***	-0.72***	-2.01***	-1.76**
	(0.09)	(0.10)	(0.52)	(0.09)	(0.07)	(0.52)
Bank fixed effects	yes	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes	yes
Treated × Trend		yes	yes		yes	yes
Country controls			yes			yes
Bank controls			yes			yes
Number of banks	97	97	97	97	97	97
Observations	359	359	359	362	362	362
Adj. R2	0.85	0.85	0.85	0.85	0.85	0.85

	Sweden-2008			Norway-2008		
	(1)	(2)	(3)	(1)	(2)	(3)
Post × Treated	-2.48***	-3.51***	-4.64***	-2.38***	-1.84**	-1.62*
	(0.34)	(0.74)	(0.90)	(0.34)	(0.73)	(0.70)
Bank fixed effects	yes	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes	yes
Trend × Treated		yes	yes		yes	yes
Country controls			yes			yes
Bank controls			yes			yes
Number of banks	187	187	187	168	168	168
Observations	756	756	756	688	688	688
Adj. R2	0.86	0.86	0.87	0.88	0.88	0.89

Table A2 – DID results from using reform-specific control groups

Table A2 reports the difference-in-differences results from estimating Equation [1] using country-specific control groups for each reform. The treatment group includes all banks of just one treated country at a time, whereas the control group corresponding to a given reform includes all banks located in the European countries that have never implemented contracts enforcement reforms starting from 3 years before that reform until 3 years after the reform. The dependent variable is *NPLs ratio*. *Post* is a dummy equal to 0 in the pre-event period. *Treated* is a dummy equal to 1 if the bank belongs to the treatment group. *Trend* denotes a time trend variable. The pre-event period spans the years 2004-2006 in the case of Austria and Belgium and the years 2005-2007 in the case of Norway and Sweden. For each reform, column (1) controls only for bank and year fixed effects; column (2) also controls for potentially different linear trends between treated and control banks; column (3) includes all control variables (i.e., banks and year fixed effects, bank controls, country controls, and linear trends variables). Country and bank controls are described in Table II and are lagged by one year with respect to the dependent variable. Robust standard errors are clustered at the country level and are reported in parentheses. ***, **, and * implies significance at the 99% level, 95% level, and 90% level, respectively.

	Austria-2007			**Belgium-2007**		
	(1)	(2)	(3)	(1)	(2)	(3)
Post × Treated	-3.31***	-2.84***	-2.99***	-2.02**	-1.45**	-2.26***
	(0.69)	(0.74)	(0.42)	(0.69)	(0.67)	(0.59)
Bank fixed effects	yes	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes	yes
Trend × Treated		yes	yes		yes	yes
Country controls			yes			yes
Banks controls			yes			yes
Number of banks	542	542	542	543	543	543
Observations	1738	1738	1738	1749	1749	1749
Adj. R2	0.78	0.78	0.79	0.78	0.78	0.79

	Norway-2008			**Sweden-2008**		
	(1)	(2)	(3)	(1)	(2)	(3)
Post × Treated	-3.62***	-2.52***	-3.87**	-3.55***	-3.46***	-2.77**
	(0.83)	(0.77)	(1.23)	(0.84)	(0.80)	(0.97)
Bank fixed effects	yes	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes	yes
Trend × Treated		yes	yes		yes	yes
Country controls			yes			yes
Bank controls			yes			yes
Number of banks	407	407	407	371	371	371
Observations	1543	1543	1543	1429	1429	1429
Adj. R2	0.81	0.81	0.83	0.81	0.81	0.83

TABLE A3 – Excluding systematically important financial institutions (SIFIs)

Table A3 reports the difference-in-differences results from estimating Equation [1] when systematically important financial institutions (SIFIs) are not included in the sample of banks, both in the case the four reforms are considered jointly (Panel A) and in the case the impact of each reform is analysed separately (Panel B). In Panel A the treatment group includes all banks located in Austria, Belgium, Norway and Sweden. In Panel B the treatment group includes all banks of just one treated country at a time. In both tables, the control group includes all banks located in the European countries that have never implemented enforcing contracts reforms between 2007 and 2016 (i.e., Denmark, Finland, France, Germany, Malta, Netherland, and Slovenia). The dependent variable is *NPLs ratio*. *Post* is a dummy equal to 0 in the pre-event period. *Treated* is a dummy equal to 1 if the bank belongs to the treatment group. *Trend* denotes a time trend variable. The pre-event period spans the years 2004-2006 in the case of Austria and Belgium and the years 2005-2007 in the case of Norway and Sweden. In each table, column (1) controls only for bank and year fixed effects; column (2) also controls for potentially different linear trends between treated and control banks; column (3) includes all control variables (i.e., banks and year fixed effects, bank controls, country controls, and linear trends variables). Country and bank controls are described in Table II and are lagged by one year with respect to the dependent variable. Robust standard errors are clustered at the country level and are reported in parentheses. ***, **, and * implies significance at the 99% level, 95% level, and 90% level, respectively.

Panel A: DID results when excluding SIFIs - the four reforms are considered jointly

	All Reforms		
	(1)	(2)	(3)
Post × Treated	-2.66***	-2.79***	-2.61***
	(0.43)	(0.54)	(0.46)
Bank fixed effects	yes	yes	yes
Year fixed effects	yes	yes	yes
Trend × Treated		yes	yes
Country controls			yes
Bank controls			yes
Number of banks	434	434	434
Observations	1872	1872	1872
Adj. R2	0.85	0.85	0.85

Panel B: DID results when excluding SIFIs - separately for each reform

	Austria-2007			Belgium-2007		
	(1)	(2)	(3)	(1)	(2)	(3)
Post × Treated	-2.37***	-3.20***	-3.25***	-0.56*	-1.68***	-2.27***
	(0.30)	(0.14)	(0.36)	(0.28)	(0.28)	(0.22)
Banks fixed effects	yes	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes	yes
Trend × Treated		yes	yes		yes	yes
Country controls			yes			yes
Banks controls			yes			yes
Number of banks	207	207	207	207	207	207
Observations	712	712	712	717	717	717
Adj. R2	0.82	0.82	0.83	0.82	0.82	0.83

	Norway-2008			Sweden-2008		
	(1)	(2)	(3)	(1)	(2)	(3)
Post × Treated	-2.77***	-2.36***	-1.65**	-2.68***	-3.28***	-3.50***
	(0.47)	(0.54)	(0.63)	(0.46)	(0.56)	(0.81)
Bank-fixed effects	yes	yes	yes	yes	yes	yes
Year-fixed effects	yes	yes	yes	yes	yes	yes
Trend × Treated		yes	yes		yes	yes
Country controls			yes			yes
Banks controls			yes			yes
Number of banks	339	339	339	303	303	303
Observations	1293	1293	1293	1179	1179	1179
Adj. R2	0.86	0.86	0.87	0.85	0.85	0.86

TABLE A4 – Controlling for bank health

Table A4 reports the difference-in-differences results from estimating Equation [1] when controlling for bank health. The treatment group includes all banks located in Austria, Belgium, Norway and Sweden whereas the control group includes all banks located in the European countries that have never implemented enforcing contracts reforms between 2007 and 2016 (i.e., Denmark, Finland, France, Germany, Malta, Netherland, and Slovenia). The dependent variable is *NPLs ratio*. *Post* is a dummy equal to 0 in the pre-event period. *Treated* is a dummy equal to 1 if the bank belongs to the treatment group. *Trend* denotes a time trend variable. *High Cap.* is a dummy variable equal to 1 if the bank belongs to the top 25th percentile of the capital ratio distribution (measured as the ratio of regulatory capital over risk-weighted assets), which proxies good-type banks. *Low Cap.* is a dummy variable equal to 1 if the bank belongs to the bottom 25th percentile of the capital ratio distribution (measured as the ratio of regulatory capital over risk-weighted assets), which proxies weak-type banks. The pre-event period spans the years 2004-2006 in the case of Austria and Belgium and the years 2005-2007 in the case of Norway and Sweden. Column (1) controls only for bank and year fixed effects; column (2) also controls for potentially different linear trends between treated and control banks; column (3) includes all control variables (i.e., banks and year fixed effects, bank controls, country controls, and linear trends variables). Country and bank controls are described in Table II and are lagged by one year with respect to the dependent variable. Robust standard errors are clustered at the country level and are reported in parentheses. ***, **, and * implies significance at the 99% level, 95% level, and 90% level, respectively.

DID results when controlling for bank health

	All Reforms		
	(1)	(2)	(3)
Post × Treated	-2.72***	-2.85***	-2.69***
	(0.41)	(0.53)	(0.41)
Post × Treated × High Cap.	-0.00	-0.00	-0.12
	(0.09)	(0.09)	(0.13)
Post × Treated × Low Cap.	0.48***	0.49***	0.63***
	(0.14)	(0.13)	(0.08)
Bank fixed effects	yes	yes	yes
Year fixed effects	yes	yes	yes
Trend × Treated		yes	yes
Country controls			yes
Bank controls			yes
Number of banks	440	440	440
Observations	1907	1907	1907
Adj. R2	0.85	0.85	0.85