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Judicial Efficiency and Lending Quality

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Judicial Efficiency and Lending Quality

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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 enforcing contracts reforms that have been implemented in four European countries as a quasi-natural experiment. We find that improvements of enforcing contracts determine large, significant, and persistent reductions of banks' non-performing-loans (NPLs). These findings are robust to several difference-in-difference tests and reverse causality concerns. Our results have important policy implications especially at the light of the recent Covid-19 pandemic since they may help the banking system mitigate the virus' negative financial effects.

JEL Classification: G21, G28.

Keywords: Judicial Systems, Non-Performing Loans, Banking Stability.

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The problem of 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][1]

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 of the stock of non-performing loans (NPLs) in Europe.[2] In turn, NPLs increase worsened the downturn and slowed down the recovery. NPLs, in fact, represent a burden for both the borrower and the lender. On the one hand, the debtor cannot use trapped valuable collateral for different purposes and its credit history gets worse, negatively impacting her future financing needs (see, for instance, Bernanke et al., 1999). On the other side, banks' liquidity and profitability decrease when hit by non-performing loans. However, while small amounts of NPLs do not represent much of a concern, especially in the case of profitable banks, since the bank can continue its lending activity, large amounts of NPLs can determine significant banks problems with their capital adequacy, constrain their lending activity, and even lead to default. [3] [4]At an aggregate level, this may impair the transmission of monetary policy and undermine the financial stability of the overall banking system, with serious consequences for economic development (Bernanke and Gertler, 1989, 1995; Barseghyan, 2010; Tarazi et al., 2018).

As mentioned by Andrea Enria in his 2019's speech: "*the problem of NPLs is not solving itself – and it has not yet been resolved*". In fact, despite European NPLs have dropped to €580 billions at

[1] 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.
[2] By contrast, before the start of the GFC, non-performing loans were relatively low and stable across most EU countries.
[3] 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.
[4] 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).

the end of 2019, after the peak of €1.0 trillion reached at the end of 2016[5], 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, at the end of 2020, almost every country is in recession or depression. The stock markets' crashes occurred after the significant rise in the number of Covid-19 cases outside mainland China from late February through March[6] anticipated the unusually high and rapid increase in unemployment in many countries and the collapse of entire sectors, including tourism, travelling, hospitality and energy. In turn, to counterbalance the effects of the pandemic, several countries announced the implementation of stimulus programs based on direct spending, the deferral of certain payments, loan guarantees, and asset repurchases. Nevertheless, it is reasonable to expect that Covid-19 will produce long lasting effects on borrowers' credit standing, new NPLs and, more generally, on the stability of the overall banking system.

Therefore, it is very important to understand how it is possible to reduce NPLs without imposing further stricter rules and capital requirements to the banking system. Economic theory and policy papers (North, 1990; Bianco, Jappelli, and Pagano, 2005; Jassaud and Kang, 2015; ECB, 2017a, b; Baudino and Yun, 2017) suggest that the efficiency of judicial system plays an important role in determining how NPLs are handled. Recently, Nouy (2017) 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*".[7] More efficient judicial systems, in fact, imply an improvement in the settlement of the contract enforcement and ensure a better framework for collateral repossession.[8] In turn, this increases NPLs' recovery rates, reduces the time of their disposal

[5] European Systemic Risk Board (2017).
[6] From February 20th until mid-March, the cumulative decline of several US and European stock market indices was ranging from -30% to -40%.
[7] Speech by Danièle Nouy, Chair of the Supervisory Board of the ECB at the European Parliament's Economic and Monetary Affairs Committee, Brussel, 19 June 2017.
[8] 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.

and enhances the development of a secondary market for bad loans, thus contributing to their reduction.[9] Similarly, more efficient judicial systems also reduce the borrowers' incentives to strategic default – i.e., when the default occurs 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 repay – by increasing the perceived costs of the presumed sanctions. As a result, the flow of new NPLs is expected to decline.

In this paper we investigate the relationship between judicial efficiency and lending quality. More precisely, using the enforcing contracts reforms that have been implemented in four European countries (that is Austria in 2007, Belgium in 2007, Norway in 2008 and Sweden in 2008) as a quasi-natural experimental setting, we investigate how the improvements of enforcing contracts affect the behavior of European banks' NPLs. The main measures introduced by these legal changes 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. Since each reform has been undertaken by the corresponding country independently and only affected domestic banks – i.e., no other countries' banks - these legal changes represent the ideal setting for a difference-in-difference (DID) methodology that compares a treated group of banks with a control group, before and after the passage of the law. Therefore, they can be considered as an exogenous shock for all banks in the country. The control group, instead, is constituted by all European banks belonging to countries that have never implemented enforcing contracts reforms in our sample period.

Using a rich dataset including more than 400 banks from 11 European countries over the period 2004 – 2011, our differences-in-differences (DID) test strategy provides causal evidence that an improvement in the country judicial system's efficiency determines a large, significant and persistent reduction of NPLs' stocks, even three years after the implementation of the reforms.

[9] 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.

More precisely, when taking into account several control variables, including banks-and-year fixed effects, bank-level, country-level and linear trends variables, we find that these legal changes lower 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, compared to countries that have not implemented such reforms.

A set of robustness checks highlights that such effect only occurs after the passage of the law, providing further support to the parallel trends assumption, and is robust to an alternative definition of the control group and to the exclusion of systemically important financial institutions (SIFIs) – i.e., the type of banks featuring the largest share of international lending. Similarly, our analysis confirms that these reforms were not induced by a high level of NPLs in the country, thus excluding the reverse causality.

Our paper is related to several strands of literature. A 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; Klein, 2013; Louizis, Vouldis and Metaxas, 2012; Ghosh, 2015; Zhang et al. 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 investigated the consequences of legal systems' efficiency on different economic outcomes, such as employment levels (Pezone, 2017), 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). Finally, our paper is also related to the literature studying the relationship between creditor rights and bank lending. Several studies, in fact, have found that creditor rights' improvements are positively related to size of credit markets in several countries (La Porta, Lopez-de-Silanes, Shleifer, and Vishny 1998; Levine, 1998, 1999; Djankov, McLiesh, and Shleifer, 2007). In particular, Jappelli, Pagano and Bianco (2005) show that improvements in judicial efficiency reduce credit constraints and increase lending activity. 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. 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 and describes our identification strategy. Section 3 presents the data and the variables employed in our analysis. Section 4 shows the empirical methodology and the results, whereas Section 5 illustrates several robustness checks. Finally, Section 6 concludes and summarizes the implications of our findings.

2. Institutional Background and Main Hypothesis

A. Judicial Efficiency and NPLs

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 NPLs accumulated in the banks' balance-sheets and the flows of new non-performing loans. From a bank's perspective, in fact, the persistence in the existing level of

NPLs 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 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 systemic build-up of NPLs. 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 stock (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: Countries that implement contract enforcement reforms should exhibit lower levels of NPLs compared to countries that instead do not implement such reforms.

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

B. Enforcing Contracts Reforms

The events we investigate are represented by the enforcing contracts reforms that occurred in four European countries: Austria in 2007, Belgium in 2007, Norway in 2008, and Sweden in 2008. In this section, we provide detailed information on the institutional settings involving such legal changes.[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 implies 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] In addition, the system uses electronic signatures of attorneys; procedural orders are served electronically, or printed and posted automatically overnight, which slightly reduces the time of file handling in court; judgments are 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 produce also 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 includes binding time limits to submit written pleadings and tighter deadlines

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

for the delivery of expert opinions. Act of 26 April 2007 on combating judicial delays (entered into force 1 September 2007), in fact, establishes that the agenda is fixed by the parties or, if they fail to agree, by judges. If judges fail to render a judgment within a month after hearing a case, they are 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 determines the advance for expert fees to be paid into an escrow account held by the court. In this way, the court controls 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 enforcing contracts 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 a rapid, correct and cheap resolution of disputes, especially without full litigation, and a facilitated access to courts. In particular, the use of modern information and communication technologies is encouraged whereas the introduction and monitoring of tighter deadlines in court procedures allows to expedite on proceedings and save on time and costs. Judges, in fact, must set time limits whereby all decisions have to be taken within 6 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 as mediator in court sittings or as 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) require parties to move for leave to appeal judgments of the District Court in relation to commercial matters; (b) reinforce 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) make 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.

C. Identification Strategy.

The quasi-natural experimental setting considered in this paper is particularly suited for a research design based on the difference-in-difference (DID) methodology. This methodology attempts to find causal claims by comparing the effect of an event (in this case, the enforcing contracts reforms) on groups that are affected (henceforth, the treated) with those that are unaffected (henceforth, untreated or control).[13] Specifically, if we want to determine the impact of these reforms on banks' NPLs, we would need to determine the stock of treated-banks' NPLs before and after the reforms, and compute its difference. However, such difference does not necessarily capture the effect of the passage of the law on bad loans since other factors, both observable and unobservable, might have changed in the meanwhile. Therefore, we need an appropriate control group to account for these common economic shocks. By comparing the variation of NPLs before and after the event of the treated group with the corresponding variation of the control group, the DID strategy allows to eliminate the bias stemming from the changes due to other factors.

In our setting, we consider four different events constituted by the enforcing contracts reforms occurred in Austria in 2007, Belgium in 2007, Norway in 2008, and Sweden in 2008. We define these countries as treated countries and all banks whose headquarter is located in these countries as treated banks. Then, for each event (and hence for each treated country), we define the three years prior to

[13] Difference-in-difference models have been widely employed in the banking literature. See, among others, Haselmann, Pistor, and Vig (2010), Beck, Levine, and Levkov (2010), Vig (2013), Cerqueiro, Ongena, Roszbach (2016), and Fiordelisi, Ricci, and Stentella Lopes (2017), Molyneux, Reghezza, and Xie (2019).

that country's reform as the pre-event period, and the three years after the reform as the post-event period. For example, in the case of Austria, the event occurs 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 (say) Sweden, the event occurs in 2008, the pre-event period covers the years 2005-2007 whereas the post-event period is 2009-2011. The choice of these time-windows is mainly determined by two reasons. First, reforms of the judicial system 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 analysis.

Regarding the choice of the appropriate control group, we adopt a conservative approach and define as untreated countries all European countries that have never implemented enforcing contracts reforms from 2007 to 2016. Countries that belong to our control group are then: Denmark, Finland, France, Germany, Malta, Netherland and Slovenia. Therefore, banks whose headquarters are located in these countries are defined as untreated banks and belong to the control group. The implementation of a law, in fact, requires some time. Therefore, the choice that untreated countries must not have implemented enforcing contracts reforms until 2016 (i.e., at least five years after the end of the post-event period) ensures a cleaner DID test since it rules 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. Moreover, in this way, the control group remains the same for each event analysed in our research.

In the Robustness section, however, we also consider an alternative definition of the untreated countries giving raise to event-specific control groups. Namely, for each event under investigation, we define a country as untreated if it has never implemented enforcing contracts reforms in the time-window starting from three years before the event to four years after the even. This definition implies that the control group associated to (say) the event Austria-2007 may be different from the control group associated to (say) the event Norway-2008, since the time-windows of the two events are different.

To conclude this section, it is important to highlight that endogeneity issues due to reverse causality should be less of a concern in our setting. First, we investigate bank-level effects whereas the enforcing contracts reforms only occur at the country level. In this regard, an individual bank does not have the option to choose the preferred judicial system. Second, the level of European banks' NPLs remains quite stable in the period 2004-2008, that is also when the events we consider occur. Thus, taking also into account that enforcing contracts reforms may require several years of "preparation" before they actually enter into force, it is implausible that the bad loans of a given bank might affect the passage of judicial reforms. Nevertheless, in the Robustness section we investigate whether these reforms have been triggered by the levels of country's NPLs. In line with the motivations explained above, our results confirm that reverse causality does not represent an issue.

3. Data and Variables

A. Data

Our study draws data from three sources. We collect data about the enforcing contracts 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. 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.

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

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 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, in line with our estimation strategy. 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.

B. 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 (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 for impaired loans.[17] In our sample, *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 control variables, the average value of the bank lending rate to the private sector (*Lending rate*) is quite similar for treated and untreated

[15] During our event windows (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, Orbis Bank Focus does not always provide detailed information of NPLs by category (e.g., mortgage, business and consumer loans)
[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).

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 euros) are larger than those of the treated banks (approximately 36.46 billion euros). 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%.

Since our analysis features four different reforms of enforcing contracts which occur in different years (hence four different treated countries), we also compare the pre-event average of *NPLs ratio* of 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 untreated 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).

4. Empirical Analysis and Results

To evaluate the causal effects of enforcing contracts reforms on the stock of non-performing loans in Europe, we estimate the following panel difference-in-difference specification separately for each of the events in our sample:[19]

$$\mathrm{Y}_{ijt} = \alpha_{ij} + \alpha_t + \beta(Post_{it} \times Treated_i) + \delta(Trend_{\mathrm{t}} \times Treated_i) + \gamma X_{j,t-1} + \theta \mathrm{Z}_{ij,t-1} + \varepsilon_{ijt}. \qquad [1]$$

In our setting, Y_{ijt} denotes the value of *NPLs ratio* for bank *i* in country *j* in year *t* and the term ε_{ijt} represents the error term. The dummy variable $Post_t$ is a reform indicator variable that takes the value

[19] This means that each estimation contains just one treated country at a time, whereas the control group remains the same in all estimations.

of *1* in the three years after the reform enters into force – i.e., the post-event period. $Treated_i$ is a dummy that equals *1* if the bank belongs to the treated country (i.e., if the bank's headquarted is located in the country *i* implementing the enforcing contracts reforms) and *0* if instead it belongs to the control group. Thus, the interaction term $Post_t \times Treated_i$ is equal to *1* only when both the variables $Post_t$ and $Treated_i$ are equal to *1*, otherwise such term amounts to *0*. Our specification includes 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. Moreover, we cluster standard errors at country-level, thus allowing for correlation of the error term within countries over time (Bertrand, Duflo and Mullainathan, 2004).

To control for potentially different linear trends between treated and untreated banks, our model also includes an interaction term obtained by multiplying a time trend variable ($Trend_t$) by the dummy $Treated_i$. As highlighted by Cerqueiro, Ongena and Roszbach (2020), however, the drawback of this specification is that it often underestimates the impact of the judicial reforms, since the linear trends variables may absorb part of the effect of interest.

In Equation [1], $X_{j,t-1}$ and $Z_{ij,t-1}$ denote vectors of country- and bank-specific control variables lagged by one year, respectively, 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 $Z_{ij,t-1}$ 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 et al., 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). Bank 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).

In our model, the difference-in-difference effect is captured by β. More precisely, this coefficient measures the average pre-post event difference in the *NPLs ratio* of countries that have implemented contracts enforcement reforms, relative to the pre-post event difference of countries that have not implemented such reforms. As explained in the previous sections, we expect a negative value for this coefficient, indicating that an improvement in the enforcement of contracts should lead to a decline 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 imply that the disposal of accumulated NPLs takes a longer time, thus contributing to the build-up of NPLs.

Table IV reports the results of the DID regressions. For each treated country, 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 untreated 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). For each event, we find 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 enforcing contracts reforms experienced a significant decrease in their level of NPLs relative to countries that did not implement such reforms. To better understand the economic impact of our estimates, the third specification highlights for example that such reduction 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.

The validity of the difference-in-difference methodology hinges on a crucial assumption: the trends in the outcome variable of interest (that is *NPLs ratio*) for both treated and control banks must exhibit a similar pattern ("parallel") prior to the implementation of the enforcing contracts 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 untreated 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 in which the dummy variable *Treated* is interacted with individual year dummies to capture the time-varying effects of the enforcing contracts reforms on NPLs before and after the passage of the law.

The results reported in Table V further strengthen the validity of our identification strategy. For each treated country, in fact, we find that the interactions between the variable *Treated* and the year dummies *before* the passage of the enforcing contracts reforms are not significant, thus confirming that there is a parallel trend in the NPLs ratios of treated and untreated banks. By contrast, the two trends diverge in the post-event periods as result of the enforcing contracts reforms. The interactions between the variable *Treated* and the year dummies *after* the passage of the reforms, in fact, are significant and large, highlighting also a strong persistence of the effects of judicial efficiency improvements in all three years after the reforms are implemented. Therefore, our DID is valid.

5. 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 test we conduct addresses the concerns arising from the use of an unbalanced sample. Second, to improve the similarities between treated and untreated banks we provide an alternative definition of the untreated countries which gives raise to event-specific control groups. Third, we provide estimates of a linear-probability model that helps mitigating potential endogeneity concerns related to reverse causality. Finally, 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).

5.1 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 VI shows that our findings still hold when the sample is balanced: the estimates of *Post* × *Treated* are negative, statistically significant and persistent across all three specifications.

5.2 Alternative definition of untreated countries

In the baseline regressions reported in Table IV, we define a country as untreated if it has never implemented enforcing contract reforms from 2007 until 2016. 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 event, thus raising possible concerns that the untreated banks might not be a proper counterfactual. 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 untreated countries based on event-specific control groups. More precisely, for each event, we define a country as untreated if it has never implemented enforcing contracts reforms in the time-window starting from three years prior to the event to four years after the event. For example, according to the new definition, the control group of the event Austria-2007 is constituted by all European countries that have never implemented reforms between 2004 and 2011. After the event, we require that the country has never implemented enforcing contracts reforms in the next four years – and not just in the next three as in the post-event period - because laws typically require some years of "settlement-time" before entering into force. The results reported in Table VII are similar to those shown in the baseline results (Table IV), both in terms of economic magnitude and statistical significance.

5.3 Exogeneity

As explained in the previous sections, endogeneity issues related to reverse causality are less of a concern in our setting for several reasons: *i)* we investigate bank-level effects whereas the enforcing contracts reforms only occur at the country level, *ii)* the 'settlement' of enforcing contracts reforms may require several years before they actually enter into force, and *iii)* the levels of European banks' NPLs are quite stable up to 2008. To further convince the readers about the internal validity of our DID methodology, we also estimate, for each treated country, a linear-probability model in which the dependent variable is a dummy equal to 1 for the treated banks in the year of the reform and 0 otherwise - i.e., for the treated banks in the pre-event and post-event years. The independent variables, instead, are constituted by the variable *NPLs ratio* lagged one year and all possible control variables employed in Equation [1]. Table VIII shows that the level of NPLs in the year before the reform is not a statistically significant predictor of the enforcing contracts reforms in any of the countries analyzed (i.e., Austria-2007, Belgium-2007, Norway-2008, and Sweden-2008). These results suggest that reverse causality does not represent an issue.

5.4 Excluding SIFIs

As explained above, the fact that judicial efficiency reforms occur at the country level whereas NPLs are measured at the bank level certainly helps mitigating reverse causality concerns. On the other side, however, it may also give raise to 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 reforms occurred in Austria. In other words, this implies that our treatment group may underestimate the effects of enforcing contract reforms. To strengthen the validity of our results, 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[20] and then re-estimate equation (1). For all treated countries except Belgium, the results reported in Table IX highlight that the economic magnitude and the statistical significance of the estimates are similar to those shown in the baseline results. In the case of Belgium, instead, the coefficient (and the significance) slightly decrease when the model is estimated including only bank and year fixed effects (Column 1).

6. Conclusions

In the last few years, several policymakers have called for effective measures to reduce the stock of European NPLs (Nouy, 2017; Enria, 2019; Dombrovskis, 2020). In fact, despite European NPLs have dropped to €580 billions 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, at the end of 2020, every EU country is in recession. In turn, the pandemic will most likely produce long lasting effects on borrowers' credit standing, new NPLs and, more generally, on 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 negative effects on financial stability and the real economy are urged. Given this background, it is very important to understand how it is possible to reduce NPLs without imposing further stricter rules and capital requirements to the banking system.

In this paper we show that the efficiency of judicial system plays an important role in determining how NPLs are handled. Using the enforcing contracts reforms implemented in four European countries as a quasi-natural experimental setting, our difference-in-differences test strategy

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

provides causal evidence that more efficient judicial systems lead to a significant, large and persistent reduction of NPLs' stocks, 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, compared to countries that have never implemented enforcing contracts reforms. Our findings are robust to a variety of robustness checks and extensions, and there is no evidence that reverse causality drives the results.

The economic forces leading to the decline in the stock of NPLs hinge on several features of the judicial system. On the one hand, in fact, 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 the 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 contributing to their reduction. On the other hand, more efficient judicial systems also reduce the borrowers' incentives to strategic default – i.e., when the default occurs 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 repay – by increasing the perceived costs of the presumed sanctions. As a result, 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. Our results are particularly important in this historical moment: Covid-19 represents an unprecedent storm and an efficient judicial system would be crucial to help banks facing the virus' negative financial effects.

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

Figure 1 plots the average values of non-performing loans (as a percentage of total loans) shown separately for the treated and the untreated banks in the pre-event years.

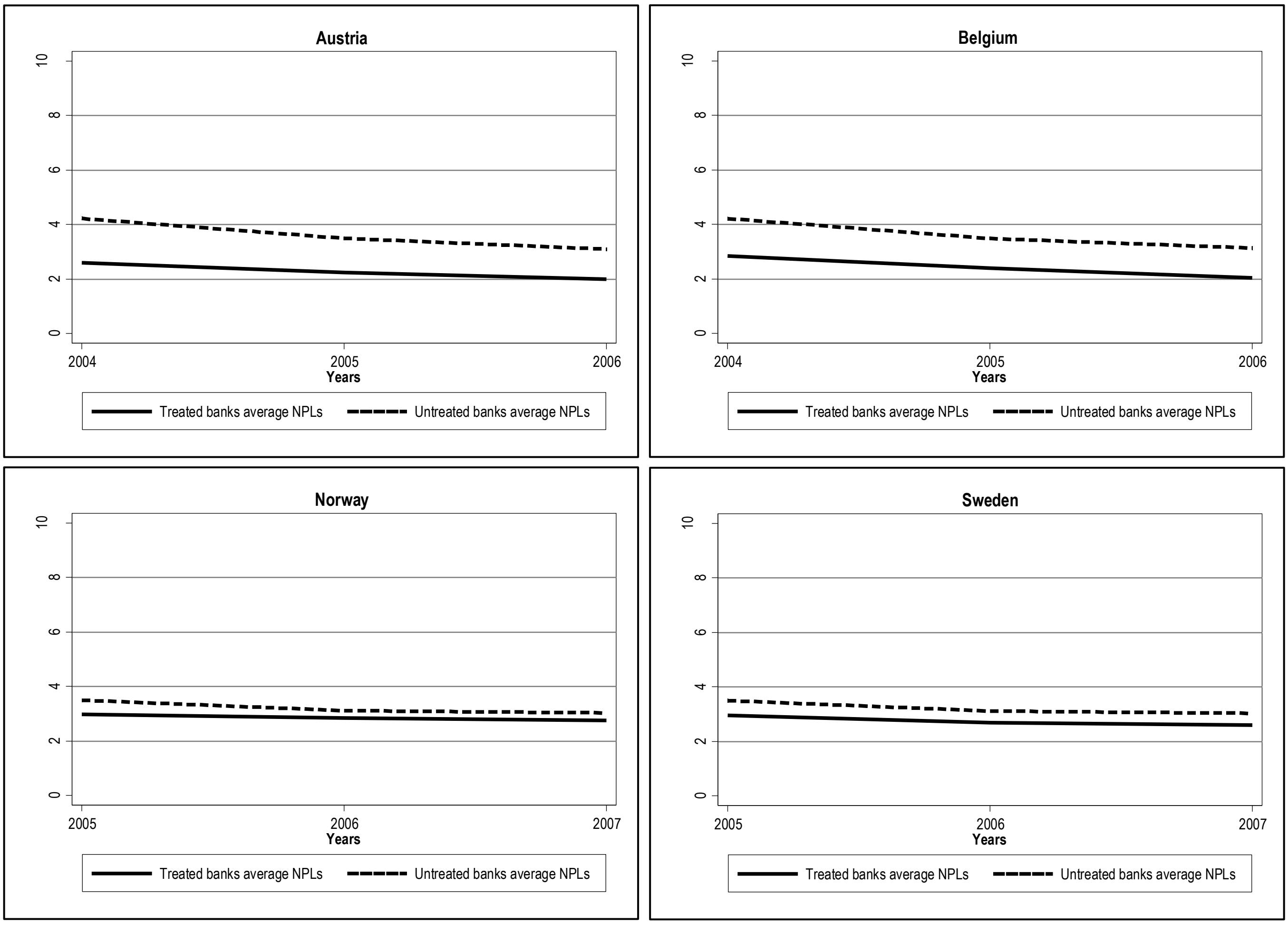

Table I: Enforcing contracts legal changes

Table I provides a summary of the enforcing contracts 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 during our 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 include all banks located in the four countries that implemented the enforcing contracts reform (i.e., Austria-2007, Belgium-2007, Norway-2008, and Sweden-2008). Untreated 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). 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

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

Table III – Pre-event summary statistics

Table III reports the mean and the standard deviation (reported in parentheses) of our variables in the pre-event period. 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. Untreated 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.

Austria

	Treated banks	Untreated banks	Difference
NPLs ratio	2.28	3.49	-1.21
	(0.36)	(2.85)	(1.17)
GDP growth	2.81	2.64	0.17
	(0.54)	(1.02)	(0.42)
Lending rate	4.52	4.87	-0.35***
	(0.13)	(0.24)	(0.10)
ROA	1.05	1.02	0.03
	(0.84)	(0.56)	(0.24)
Capital ratio	9.18	7.14	2.05
	(3.73)	(3.26)	(1.36)
Loan growth	0.78	1.13	-0.36
	(4.22)	(3.11)	(1.31)
Total Asset	15.25	15.54	-0.29
	(0.58)	(2.12)	(0.87)

Belgium

	Treated banks	Untreated banks	Difference
NPLs ratio	2.38	3.49	-1.11
	(0.88)	(2.85)	(1.08)
GDP growth	2.71	2.64	0.07
	(0.66)	(1.02)	(0.39)
Lending rate	4.77	4.87	-0.10
	(0.20)	(0.24)	(0.09)
ROA	0.61	1.02	-0.41*
	(0.08)	(0.56)	(0.21)
Capital ratio	4.07	7.14	-3.07**
	(0.02)	(3.26)	(1.24)
Loan growth	0.97	1.13	-0.16
	(3.17)	(3.11)	(1.20)
Total Asset	19.99	15.54	4.45***
	(1.22)	(2.12)	(0.81)

Norway

	Treated banks	Untreated banks	Difference
NPLs ratio	2.82	3.15	-0.32
	(1.41)	(2.87)	(0.26)
GDP growth	2.78	2.75	0.03
	(0.26)	(1.21)	(0.11)
Lending rate	4.94	5.00	-0.06
	(0.79)	(0.35)	(0.06)
ROA	0.89	0.94	-0.05
	(0.33)	(0.55)	(0.05)
Capital ratio	9.36	6.75	2.61***
	(2.38)	(2.87)	(0.29)
Loan growth	-0.14	1.19	-1.33***
	(2.52)	(3.02)	(0.31)
Total Asset	13.32	16.13	-2.81***
	(1.46)	(2.24)	(0.21)

Sweden

	Treated banks	Untreated banks	Difference
NPLs ratio	2.72	3.15	-0.43
	(1.25)	(2.87)	(0.28)
GDP growth	3.59	2.75	0.84***
	(0.71)	(1.21)	(0.12)
Lending rate	5.83	5.00	0.83***
	(0.34)	(0.35)	(.04)
ROA	0.96	0.94	0.02
	(0.52)	(0.55)	(0.06)
Capital ratio	7.96	6.75	1.21***
	(3.51)	(2.87)	(0.34)
Loan growth	-0.83	1.19	-2.02***
	(3.19)	(3.02)	(0.34)
Total Asset	12.80	16.13	-3.32***
	(1.94)	(2.24)	(0.24)

TABLE IV – The impact of enforcing contract reforms on *NPL ratio*

Table IV reports the difference-in-difference results from estimating Equation (1) separately for each event. The dependent variable is *NPL 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 treated country. *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. Untreated 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). For each treated country, column (1) controls only for bank and year fixed effects; column (2) also controls for potentially different linear trends between treated and untreated 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	-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: DID results with leads and lags

Table V reports the difference-in-difference results from estimating Equation (1) with leads and lags of the treatment separately for each event. The dependent variable is *NPL ratio*. *Treated* is a dummy equal to 1 if the bank belongs to the treated country. *Trend* denotes a time trend variable. *Y2005* is a dummy equal to 1 for year 2005, and zero for 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. Untreated 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). For each treated country, we include 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**	**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 VI – DID results from using a balanced sample of banks

Table VI reports the difference-in-difference results from estimating Equation (1) using a balanced sample of banks for each event. More precisely, for each event, we exclude banks that do not report continuously financial statement information during the estimation period. The dependent variable is *NPL 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 treated country. *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. Untreated 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). For each treated country, column (1) controls only for bank and year fixed effects; column (2) also controls for potentially different linear trends between treated and untreated 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	-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 VII – DID results from using event-specific control groups

Table VII reports the difference-in-difference results from estimating Equation (1) using country-specific control groups for each event. The dependent variable is *NPL 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 treated country. *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 event, the untreated banks include all banks located in the European countries that have never implemented enforcing contracts reforms starting from 3 years before the event to 4 years after the event. For each treated country, column (1) controls only for bank and year fixed effects; column (2) also controls for potentially different linear trends between treated and untreated 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***	-2.86***	-2.92***	-2.04**	-1.49**	-2.31***
	(0.70)	(0.76)	(0.42)	(0.70)	(0.68)	(0.61)
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	-3.41***	-2.99***	-3.04**	-3.31***	-3.94***	-3.51***
	(0.74)	(0.76)	(1.06)	(0.73)	(0.80)	(0.34)
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 VIII – Investigating reverse causality

Table VIII reports the results from estimating a linear probability model for each treated country, in which the dependent variable is a dummy equal to 1 for the treated banks in the year of the reform and 0 otherwise - i.e., in the case of treated banks in the pre-event and post-event years. The independent variables, instead, are constituted by the variable *NPLs ratio* lagged one year and all possible control variables (i.e., banks and year fixed effects, bank controls, country controls, and linear trends variables). 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. 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**
NPLs ratio	-0.0004	0.0011	0.0057	-0.0012
	(0.0008)	(0.0010)	(0.0039)	(0.0015)
Bank fixed effects	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes
Country controls	yes	yes	yes	yes
Bank controls	yes	yes	yes	yes
Observations	93	62	700	438
R2	.95	.99	.84	.90

TABLE IX – Excluding systematically important financial institutions (SIFIs)

Table IX reports the DID results from estimating Equation (1) when systematically important financial institutions (SIFIs) are not included in the sample of treated and untreated banks. The dependent variable is *NPL 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 treated country. *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. Untreated 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). For each treated country, column (1) controls only for bank and year fixed effects; column (2) also controls for potentially different linear trends between treated and untreated 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	-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