Credit Information Sharing and Loan Loss Recognition*

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Abstract

Does enhancing banks' information sets and understanding of credit risks improve loan loss recognition? We study this question using a global dataset of staggered initiations and coverage increases of public credit registries (PCRs). Mandated by national regulators, PCRs collect borrower and loan information from lenders and share it with the banks in the financial system. This setting represents a significant improvement in banks' assessment of loss events. We find that PCR initiations and coverage reforms enhance the timeliness of banks' loan loss recognition—the extent to which loan loss provisions capture subsequent nonperforming loans. The effects are greater when PCRs distribute more information and are not driven by changes in borrower quality or supervisory stringency. Overall, these inferences are consistent with improvements in banks' information sets leading to better provisioning decisions.

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INTRODUCTION

Just as the global financial system suffered the crisis of 2007–2009, many observers acknowledged that banks were not timely in recognizing expected defaults prior to the crisis.[1] Timeliness in loan loss provisioning has been a central topic in academic and policy discussions, as early recognition of loan losses through bank accruals could curb procyclicality in bank lending and financial stability (Beatty and Liao 2011; GAO 2013; FASB 2016). But what drives the variation in the timeliness of banks' loan loss recognition (LLR), and how can this practice be improved? Although most arguments and current evidence revolve around banks' incentives and fundamentals, banks' understanding of credit risks also plays a crucial role in their provisioning decisions (Acharya and Ryan 2016).[2] To date, however, there is little empirical evidence on the effect of banks' ability to ascertain credit risks in their LLR practices. Our paper attempts to fill this gap by examining whether and how improvements in information about borrowers' credit profiles influence banks' LLR timeliness.

In the LLR process, banks identify credit risks in their holdings and activities. Provisioning depends on the information banks possess, and managerial assessment is an essential part of this complex process, irrespective of the method employed.[3] Under the incurred loss model, provisions

[1] See, for example, Laux and Leuz (2009, 2010), Vyas (2011), Huizinga and Laeven (2012), Beck, Jakubik, and Piloiu (2013), GAO (2013), Beatty and Liao (2014), and Acharya and Ryan (2016).

[2] These discussions include managers' incentives and discretion (Wahlen 1994), the use of the incurred loss model as opposed to alternative approaches (Laeven and Majnoni 2003; Dugan 2009), and loan composition (Liu and Ryan 2006).

[3] There are two primary frameworks for provisioning: incurred and expected. In most cases, including our sample countries, loan loss provisions are made under the incurred loss framework (US GAAP (FAS 5/FAS 114) and IFRS (IAS 39)). These rules do not permit provisions to include expected credit losses and do limit them to losses that are considered probable (e.g., borrower loss of employment, decrease in collateral values). In contrast, the expected loss model (e.g., IFRS 9) removes this "probable" criterion. This framework, to which banks have been switching, further increases the role of managerial input by requiring that banks use their evaluations and forecasts to estimate future credit losses (Ertan 2019).

should cover losses that are estimated to have been incurred but are not yet charged off (as of the reporting date). Accounting rules require that loan loss estimates incorporate all relevant and observable information, including specific evidence like a borrower's financial health, as well as general changes in economic conditions that could influence default rates. Consistent with the subjectivity and complexity inherent in provisioning, prior academic work explores the effects of, and the cross-sectional variation in, banks' LLR timeliness under the incurred loss framework (e.g., Beatty and Liao 2011; Bushman and Williams 2012).

We expect that an increase in information on loss events and borrower health should help banks evaluate the risks better and improve their loan loss provisions.[4] To test this prediction, we focus on a formal channel of information sharing in a cross-country setting: public credit registries (PCRs). Established and managed by national banking regulators, PCRs are data repositories that contain detailed information about individual and commercial borrowers—especially for small contracts and opaque borrowers (Jappelli and Pagano 1999). PCRs are launched to increase the information available to banks, improve provisioning decisions, and enhance supervision practices (Miller 2003). The main source of this data reservoir is regulated banks, which provide the registry with a variety of inputs ranging from borrower indebtedness and performance to subjective assessments like internal ratings. Credit registries include information on commercial loans as well as individual credit (e.g., mortgages, consumer loans, auto loans, credit cards, overdrafts). In return, credit registries typically share this information with the other banks in the financial

[4] Given that loan loss provisions can be estimated for either an individual loan or a group of homogenous loans, our view is that the information from PCRs can be about either the overall industry/economic trends or an individual borrower. Industry or economic trends can be gleaned from the behavior of different borrower-type pools. Regarding borrower-specific information, suppose a debtor that has borrowed from two banks (Banks A and B) defaults on one of the loans (Bank A's). If Bank A shares this information with Bank B, then Bank B can use this objective evidence to better estimate the loan loss. Information sharing would make Bank B's loan loss provision timelier.

system.[5] In summary, our study exploits regulations that lead to PCR initiations and coverage expansions as a shock to banks' understanding of credit risks. This represents a significant increase in banks' information set that is relevant for provisioning decisions (Acharya and Ryan 2016).

We hand-collect the data on the establishment, coverage, and background of PCRs from the annual reports of national bank supervisors or central banks that are responsible for regulating PCRs in Europe from 2004 to 2015. Out of 23 countries with an established credit registry, 13 experienced a PCR initiation or a change in coverage regulation during our sample period. We focus on the first event for each of these countries and perform a series of bank-level difference-in-differences tests to examine changes in the timeliness of loan loss provisioning—i.e., the extent to which current loan provisions explicitly anticipate the deterioration in loan portfolios. Specifically, we examine the coefficient on next year's change in nonperforming loans (NPLs) in regressions in which the dependent variable is current loan loss provisions (Bushman and Williams 2012). An increase in this coefficient indicates an improvement in timeliness.

PCRs are country-level establishments with mandatory participation; therefore, we can straightforwardly define treatment banks at the country level. We compare these banks' LLR timeliness to that of banks in non-treatment countries but with similar observables over a narrow window of three years before and after the events. We find that mandatory information sharing improves banks' LLR timeliness (by 10 percentage points). That is, information sharing brings forward the recognition of future changes in NPLs. This finding is robust to a variety of alternative specifications, such as using other measures of LLR timeliness, examining narrower event

[5] Despite the similarities in their general framework and objectives, there is vast variation across PCRs. For example, some have a minimum amount threshold for loans they include, whereas others have full coverage of the economy. PCRs also differ from one another in the types of financial institutions they cover and the amount of information they collect and disseminate (see Section 2 for details). We exploit some of this cross-sectional variation in our analyses.

windows, adding controls for banks' portfolio composition and institutional features of the countries, and excluding banks that experienced changes in accounting or regulatory reporting.

Our regulation-based study faces some identification challenges. Although the mandatory nature of PCRs is not susceptible to selection at the bank level, such regulatory decisions are likely affected by country-specific features. In fact, credit reporting surveys suggest that many countries implement PCRs precisely because they aim to enhance banks' information sets. Thus, we do not claim randomness; rather, our goal is to infer whether the treatment in question leads to the desired effect on the treated banks. To explore this issue further and to rule out identification concerns, we perform several additional tests.

First, certain countries may be more likely to initiate PCRs than others, and inherent differences between the treatment and control countries—not the initiation of PCRs—could drive our results. The fact that the pre-PCR trends in timeliness are statistically identical for the treatment and control groups allays this concern. Further, the observable portion and the unobservable but time-invariant portion of such pre-treatment confounding factors are mitigated by a vector of controls, as well as country fixed effects. A second identification concern is that *concurrent* confounds may be responsible for our findings. In particular, PCR initiations could be just one part of broader regulatory initiatives, and this composite structure might be the correct treatment effect. Neither our analyses of the regulations around PCR reforms nor our conversations with practitioners indicate that such confounding factors occur systematically. Empirically, we control for country-level indices that track concurrent changes in regulatory stringency, credit reforms, and local economic trends. Moreover, we account for banks' time-varying financial and regulatory reporting practices.

We argue that three channels could drive the observed treatment effect. PCRs could improve LLR practices by intensifying bank supervision, by systematically affecting borrower pools and borrower default behavior (mitigating adverse selection and moral hazard), or by enhancing banks' understanding of credit risks. While these channels are not mutually exclusive, several cross-sectional tests provide further evidence that banks' learning is a valid mechanism. We find that our inferences hold for countries with increasing as well as decreasing NPLs, which indicates that default risk dynamics alone are unlikely to drive the results. We also note that our empirical approach explicitly controls for bank-level changes in NPLs, which permits us to account for changes in default behavior around the events we study. We also find significant results on subsamples comprising increases and decreases in debt maturity, which suggests that changes in contractual features are not responsible for our inferences.[6] Our findings hold for countries with strong and weak levels of bank supervision. Neither bank supervision nor borrower characteristics can fully explain our findings, which implies that an increase in banks' understanding of risks is playing a role.

We next turn to an alternative global sample of developing countries that initiated PCRs between 1994 and 2008 from the World Bank's credit reporting database to provide additional tests to bolster our inferences. We observe that the results documented in the European setting extend to this alternative setting, providing further external validity for our conclusions. In addition, we exploit a feature of this dataset to provide more direct evidence on the role of banks' learning. In particular, this dataset allows us to examine the impact of the variation in the amount of information PCRs collect and distribute. We find that conditional on the amount of information

[6] Banks' learning from PCRs can be limited in the presence of certain contractual features, such as cross-default provisions, which are typically prevalent in large corporate loans. To speak to this issue, we examine the results among countries with high and low levels of corporate debt. Our conclusions hold in both subsamples.

the PCR collects, the amount it distributes has a significant effect on LLR timeliness. By holding information collection and supervisory effects constant, this test suggests that banks' receipt and use of PCR information drive the treatment effect. Overall, these inferences add credibility to our claims that banks' understanding of credit risks is *a* channel through which information sharing enhances banks' LLR practices.[7]

Our study extends the literature on the determinants of banks' LLR timeliness. Researchers have pointed out the need for a better understanding of loan loss provisioning practices (Beatty and Liao 2014; Bushman 2014), particularly because banks' reporting and disclosure practices have significant implications for the economy (e.g., Beatty and Liao 2011; Granja 2018; Balakrishnan and Ertan 2018, 2019). The literature has thus far focused on managerial incentives and economic factors, such as loan portfolio composition. In this line of work, the implicit assumption is that managers need to be incentivized and monitored to make better decisions (e.g., Altamuro and Beatty 2010; Beatty and Liao 2011; Bushman and Williams 2012, 2015). We contribute to these efforts by documenting that banks' ability to ascertain loan losses can also enhance their LLR practices. Our focus on external information extends the findings of Bhat, Ryan, and Vyas (2018), who report a link between banks' voluntary disclosure of credit risk models and timeliness of loan loss provisions.[8]

Our findings also speak to the work on credit information sharing, which is ever more prevalent given the unprecedented pace and cost-effectiveness of data collection and management.

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[7] Information sharing may also affect provisioning by improving banks' screening. To be sure, this explanation is a variant of the learning channel that we propose. Nevertheless, our findings are not an artifact of more efficient contracts or improved screening to the extent that our research design directly accounts for changes in NPLs.

[8] Acharya and Ryan (2016) note that the disclosure decision could be confounded and that these disclosures serve as noisy proxies of the available information. These critical issues are less of a concern in our setting.

Economists have pointed out the benefits of information sharing among lenders for the availability and quality of credit (e.g., Jappelli and Pagano 1999; Miller 2003). More recently, Sutherland (2018) finds that the establishment of a private credit bureau in the U.S. has reduced relationship-switching costs for borrowers and relationship-specific investments for lenders. Liberti, Sturgess, and Sutherland (2018) examine the staggered voluntary entries of commercial lenders into a U.S. commercial credit bureau and report that lenders leverage their collateral expertise to enter new markets after joining this structure. Unlike this strand of the literature, we explore the effects of information sharing on banks' *accounting* decisions, and we propose banks' ability to better evaluate credit risks as a specific channel.

Finally, the insights we provide also fit squarely within the debate over whether and with whom bank information should be shared. Recent theoretical work questions the conventional wisdom that greater disclosure is desirable and argues that increasing bank disclosures could harm the stability of the financial system (e.g., Goldstein and Sapra 2014; Dang, Gorton, Holmstrom, and Ordonez 2017). One nuance that this line of research has not considered is the sharing of information with a subset of market participants (e.g., peer banks), rather than the public. Our paper provides a bridge between the studies arguing for and against bank transparency.

Several caveats are in order. While our findings indicate the benefits of PCRs, the exchange of information is not without costs. For example, banks may choose to ration credit to riskier borrowers with positive net-present-value projects; therefore, our conclusions do not imply an improvement in social welfare. Further, one would need to incorporate the variation in the existing institutional framework to generalize our results to a broader set of countries—especially for the inferences from the global sample. Other fundamental elements of information regulation, such as enforcement, commitment, and political support, remain critical.

BACKGROUND AND HYPOTHESES

Public Credit Registries: Institutional Overview

The first countries to start public credit registries (PCRs) were in Western Europe—Germany in 1934, followed by France in 1946. Since then, more than 90 countries set up PCRs, and which often facilitate borrower-level information sharing across banks (Djankov, McLeish, and Shleifer 2007).[9] As noted by Jappelli and Pagano (2000), the Committee of Governors of the European Central Bank defines a PCR as "an information system designed to provide commercial banks, central banks, and other supervisory authorities with information about the indebtedness of firms and individuals vis-à-vis the whole banking system." Thus, PCRs are aimed at capturing loans to individuals and businesses alike. The extent of the coverage varies across PCRs and is dictated by a pre-set minimum threshold of loan values for reporting. The information PCRs contain is substantial. Banks and sometimes non-bank lenders are required to report on a regular basis, usually monthly. Typically, information on borrowers is requested regardless of their standing; hence, PCRs contain both positive information (i.e., successful payments) and negative information (i.e., missed payments).

PCRs across the globe share a basic framework in their institutional structure. They are managed and controlled by the national banking regulator, which is often the central bank. Typically, regulated financial institutions are required to participate in PCRs. Noncompliance is sanctioned by supervisory actions, penalty fees, public disclosure of noncompliance, and denial of

[9] Source: World Bank Doing Business (2015). In addition, most countries are considering starting a PCR. For instance, in 2017 India began deliberations about launching one.

PCR data access (Miller 2003). For example, according to Ordinance 22 Article 26 of the Bulgarian National Bank (BNB), the administrator of the Bulgarian PCR,

> *If an institution under Article 4 does not submit the monthly information to the Central Credit Register within the set time limits under Article 10, paragraph 3 or submits information which does not meet the requirements of this Ordinance, the BNB shall discontinue its access to the Central Credit Register for information about credit indebtedness of its customers until provision of the relevant information.*

To ensure data accuracy, the regulator or central bank takes several steps, including data quality checks and on-site examination. Continuing with the example of the Bulgarian registry, BNB Ordinance 22 Article 25 states that

> *The Bulgarian National Bank shall control the compliance with the terms and procedure for providing and using information from the Central Credit. The Bulgarian National Bank may require additional information or any documents related to the control exercised under Article 1, and may also carry out on-site examinations.*

In addition to such verification measures, regulators also enforce compliance with fines or sanctions. For instance, BNB Ordinance 22 Article 27 states: "Where an infringement of this Ordinance by the institutions under Article 4 is identified, the sanctions, fines and supervisory measures provided for in the Law of Credit Institutions shall be imposed." Nevertheless, these actions do not imply full accuracy of all the data fields that banks submit to the PCR (e.g., Giannetti, Liberti, and Sturgess 2017), and inaccuracies would diminish the effect of information sharing.

According to the survey evidence provided by Miller (2003), PCRs are intended to increase the amount of borrower information available to lenders, to improve banks' provisioning decisions, and to enhance supervision by providing additional inputs to the regulator. Some countries were motivated by the idea that financial institutions should know the indebtedness and creditworthiness of existing and potential borrowers in the whole financial system in order to make more informed lending and provisioning decisions. Consistent with this view, World Bank Credit Reporting surveys indicate that almost all banks use the registry data for lending.[10] The supervisory purpose of PCRs relies on the regulator's identification of the main debtors in the financial system, analysis of concentration exposures, and monitoring and enforcement of banks' provisioning policies against problem loans.

Although PCRs share many characteristics, there are also significant differences across jurisdictions. Foremost among these differences are the specifics of the information collected, the coverage of the economy, and the accessibility of the data. All PCRs collect basic data about borrowers, such as their name and contact details and information on outstanding loan amounts, type, and defaults. Some PCRs, however, populate additional fields, such as (commercial) borrowers' financial statements, the value of collateral, and even tax returns or internal bank ratings. For example, the Austrian registry gathers information on interest rates, maturity, and collateral, while in the Czech Republic and Germany, PCR information is limited to loan type and outstanding indebtedness. Credit registries also differ in the minimum loan size above which the information on the loans must be provided to the PCRs. While some PCRs, such as Slovakia's, have a minimum threshold of zero and capture all loans, other PCRs limit their focus to larger loans that pose a major risk to the financial system. For example, the German Credit Registry collects data only on credit exposures of at least one million euros.

[10] Further, bankers indicate in these surveys that credit reporting information is potentially a more important measure of creditworthiness than collateral, the borrower's financial standing, and the borrower's relationship with the bank (see Graph 4 in Miller 2000 and Figure 1.4 in Miller 2003).

Information Sharing and Loan Loss Provisioning: Predictions

Early academic work concentrates on the conditions under which information sharing can mitigate the information asymmetry between borrowers and lenders.[11] PCRs could improve banks' knowledge of the applicant and allow lenders to more efficiently target and price their loans (Padilla and Pagano 1997; Pagano and Jappelli 1993). Registries could also work as a disciplining device by inducing borrowers to avoid strategic default (Padilla and Pagano 1997; Jappelli and Pagano 2002). What is relatively unexplored in this literature is the direct role of PCRs in improving banks' evaluation of credit risks (Kallberg and Udell 2003). More generally, extant research has also linked the existence of a PCR with the absence of significant private credit bureaus, weak creditor protection, and a French civil code legal system. Powell, Mylenko, Miller, and Majnoni (2004) suggest that regardless of the initial motivation, PCRs are often used for a variety of purposes beyond their original objectives. The authors also point out that loan loss provisioning can be improved by information sharing.

In order to appreciate how PCRs and an enhanced information environment could affect LLR practices, it is useful to explore how banks make provisions. At the heart of loan loss provisioning is information about the creditworthiness of a borrower at a given point in time. The objective of the incurred loss model—the predominant framework in our sample—is to set aside a reserve for losses on loans that have likely been incurred as of the balance sheet date but have not yet been charged off. These accounting rules require that loan loss estimates incorporate all

[11] Since the borrower is more informed about its financial position and future profitability than the lender, who has a limited ability to assess the associated credit risk, borrowers with worse prospects (adverse selection: Akerlof 1970; Leland and Pyle 1977) and those that do not intend to honor the contract (moral hazard: Diamond 1984, 1991; Holmstrom and Tirole 1997) dominate the market. Thus, centralized exchanges of credit information among banks and supervisors can mitigate both problems to the extent that such exchanges sidestep costly information production (Hirshleifer 1971).

observable data on losses, such as a specific borrower experiencing financial difficulties and general economic conditions that could change default rates.[12] Put differently, banks recognize loan losses upon discovering a borrower's loss event that indicates possible future defaults. LLR depends on the objective evidence that the bank has about the borrower's health at the time of the balance sheet date, and the identification of loss events is complicated and subjective. For these reasons, it is inherently difficult to recognize existing credit losses earlier in the credit cycle.[13]

Accordingly, a positive shock to the objective evidence banks possess about loss events and borrower creditworthiness—like the information supplied by PCRs—could enhance managers' estimates of loan loss provisions. These inputs could reveal trends about the macroeconomy or pertain to specific borrowers. Systematic trends can be gleaned from the behavior of different borrower pools. Information relating to specific borrowers is often obtained directly from other banks. If an individual/entity that had borrowed from Bank A defaults on its loans, all other banks in the same country gain access to this information. Accordingly, if Bank B had also extended loans to this borrower, even if these loans are performing, Bank B can use this objective evidence to better estimate its loan losses. In this sense, information sharing would make Bank B's loan loss provision timelier. Even though shedding light on the precise nature of learning is beyond the scope of our paper, our conversations with bankers who rely on PCR data suggest that this information is used in a borrower-specific context.

[12] For example, IAS 39 states that "Objective evidence that a financial asset or group of assets is impaired includes observable data that comes to the attention of the holder of the asset about the following loss events: (a) significant financial difficulty of the issuer or obligor; (b) a breach of contract, such as a default or delinquency in interest or principal payments…."

[13] Consistent with this view of the incurred loss model, prior academic work documents significant cross-sectional variation in banks' timeliness of loan loss provisions under incurred loss regimes (e.g., Beatty and Liao 2011; Bushman and Williams 2012; Akins et al. 2017).

PCR initiations/improvements might not enhance loan loss provisioning for several reasons. Despite their potential benefits, compulsory information-sharing mechanisms may be detrimental for various reasons. Foremost among these is the appropriability problem (e.g., Grossman and Stiglitz 1980; Gorton and Winton 2003)—the concern that information sharing may discourage banks from conducting costly information production on their own. Banks may find it cheaper to free-ride on the information collected by others, rather than collecting new data that competitors may then easily exploit. This outcome could reduce the effectiveness of banks' information-gathering activities, which would lead to an overall deterioration of information in the credit markets, followed by hampered lending and provisioning practices.[14]

Another feature that can impede the information-sharing benefits of PCRs is the misreporting of borrower information in PCRs. Giannetti, Liberti, and Sturgess (2017) find that Argentinian banks manipulate the credit ratings of their borrowers before sharing them with competing banks. Although the findings are for only one country and apply to a subjective element in shared credit information (i.e., ratings), it is nevertheless a legitimate concern. Such misreporting can deter banks from relying on PCR data.

Finally, the presence of contractual features such as cross-default clauses in large corporate loans could obviate or reduce the usefulness of information sharing and hence of PCRs. To obtain objective evidence on the role of cross-defaults, we contacted our sample registries regarding the use of cross-default provisions in their jurisdictions. Their responses indicated that cross-default provisions are not commonplace in their respective countries' loan contracts. Nevertheless, to the

[14] Other than the amount of information, the type of information banks rely on also matters. If banks receive and share standardized hard information, they may rely less on soft information, which could have serious adverse consequences.

extent that such cross-defaults rules are present, the usefulness of PCRs is questionable. Given these conflicting views, we state our first prediction in the null form as follows.

Hypothesis 1: PCR initiations and coverage increases do not affect LLR timeliness.

Even though we are interested in the effects of PCRs, the ultimate objective of our study is not to evaluate a policy but rather to understand whether banks' improved understanding of credit risks enhances their provisioning decisions. To this end, we explore the channel between PCR initiations and LLR timeliness by extending our investigation of this relationship with a series of cross-sectional tests. To understand whether changes in the quality of the borrower pool or loan contracts are the only mechanisms at work, we examine whether PCRs' provisioning effects vary across countries with increases and decreases in NPLs and loan maturities. Another channel whereby PCR information can improve timeliness is enhanced bank supervision. Credit registries provide valuable input to supervisors, who use this information to monitor banks. To understand the extent to which bank supervision drives our inferences, we examine how our findings vary with differing levels of bank supervision.

Lastly, we exploit the variation in PCR characteristics to test and strengthen our empirical inferences. We examine the cross-sectional variation in the difference between information collection and information distribution. The information PCRs distribute to banks could increase the latter's ability to evaluate credit risks, while the information PCRs collect but do not distribute should not affect banks' learning. This test is feasible because, in most cases, PCRs distribute to banks a subset of the collected information and because this wedge between data collection and distribution varies across jurisdictions. We should note that while these interesting details add credibility to our inferences, we cannot argue that they are the result of a coin toss; therefore, these variations, too, are susceptible to a certain degree of selection at the country level.

Sample Construction and Descriptive Statistics

We conduct our tests on a global dataset of bank financial characteristics merged with the country-specific details of credit reporting systems. The main sample used in this study focuses on European countries. We determine that 23 out of 51 European countries have an established credit registry.[15] We focus on credit registry events that affected these 23 registries between 2004 and 2015.[16] Most European countries had initiated PCRs in the last century but experienced events that enhanced information sharing post 2004, such as increases in the numbers of borrowers or lenders covered in a registry.

Balakrishnan and Ertan (2020) describe our data collection approach in detail. We obtain the data on credit registry events from two official sources: annual reports of the national central bank/bank supervisor and the website of the credit registry. We search for discussions in these reports about regulations that led to the establishment of a credit registry, a decrease in the minimum loan threshold for reporting, or an increase in the number of lenders reporting. For our sample period, we find that 13 European countries experienced changes to registry operations that are recognized in official reports. In our empirical tests, we focus exclusively on the first event

[15] The countries with a registry are Albania, Armenia, Austria, Azerbaijan, Belarus, Belgium, Bosnia and Herzegovina, Bulgaria, Czech Republic, France, Germany, Ireland, Italy, Latvia, Lithuania, Macedonia, Malta, Portugal, Romania, Slovakia, Slovenia, Spain, and Turkey. Ireland (2017), Malta (2016), and Slovenia (2016) established registries more recently, after the end of our sample period.

[16] Starting from 2004, the World Bank tracks business regulations including credit registry–related events in its Doing Business website and publishes on its website country-level coverage of public credit registries. To utilize this external database to validate our search efforts, we begin our sample period in 2004. Another advantage of our start date is that most supervisory reports become available from the middle 2000s. Our sample ends in 2015 to ensure that we have sufficient observations in the post period to estimate the measures of timeliness of loan loss provisioning.

that occurred in a country within the sample period. Table 1 provides information about the country, timing, and nature of the reforms we study. We also validate the data we collect with regulations cataloged in and registry coverage data reported by the World Bank's Doing Business reports. Table 1 shows that the coverage of the population increases around these events, suggesting that these events result in new information about borrowers to banks.

We obtain the bank-level data from SNL (for events post-2011) and Bankscope (for events pre-2010). We use two databases because the Bankscope data ends in 2014 (and does not allow us to measure the future ΔNPL variable for events that occurred after 2011), and the SNL Financial data for European banks starts from 2009. We limit our attention to "banks" and remove observations with missing regression variables. Country-level macroeconomic characteristics come from the World Bank's Global Financial Development Database. The Appendix shows the definitions of the regression variables, including the data source and field codes, where applicable.

Table 2 Panel A presents the sample statistics. The sample consists of 7,953 bank-years. In this sample, the median (average) *Loan loss provisions* is 0.835 (0.466) percent of total loans, while the mean growth in NPLs is 0.479, 0.613, and 0.743 percent of total loans in years t-1, t, and t+1, respectively. About half of the observations are coded as *Treatment* and *Post*, which suggests a reasonably balanced estimation sample. The median value of *Size* corresponds to a figure of about 680 million dollars (= $e^{20.338}$). The average bank has a capital of almost 10 percent, an ROE of over 6 percent, and a loan growth ratio of over 11 percent. About 62–64 percent of our sample banks report under IFRS and Basel and have a Big Four auditor. Turning to macroeconomic indicators, we observe an average GDP growth of 5.5 percent. The mean GDP per capita is about 45 thousand dollars, and the top five banks constitute 75 percent of a country's total banking assets, on average.

Table 2 Panel B presents a breakdown of our bank-year sample by country. Two patterns emerge. First, the observations that enter the estimation sample follow a distribution that is similar to the distribution of the sample that has nonmissing regression variables. This pattern alleviates the concern that our matching procedure at the bank level yields a nonrepresentative sample. Second, Italy dominates the sample. This observation is not surprising given the dominance of Italy in the original database. However, it also necessitates that we re-examine our tests without Italy, which we do below.

RESEARCH DESIGN AND DATA

Main Model

Our main prediction (*Hypothesis 1*) pertains to the effect of PCR-related information events on banks' LLR practices. We expect banks' loan loss provisioning to improve as a result of a better understanding of borrower creditworthiness. To test our predictions, we estimate the following difference-in-differences model:

$$\begin{aligned}
\textit{Loan Loss Provisions}_{i,t} = \alpha &+ \beta_1 \textit{Post}_{c,t} \times \textit{Treatment}_c \times \Delta \textit{NPL}_{i,t+1} \\
&+ \beta_2 \textit{Post}_{c,t} \times \textit{Treatment}_c \times \Delta \textit{NPL}_{i,t} + \beta_3 \textit{Post}_{c,t} \times \textit{Treatment}_c \times \Delta \textit{NPL}_{i,t-1} \\
&+ \beta_4 \textit{Post}_{c,t} \times \Delta \textit{NPL}_{i,t+1} + \beta_5 \textit{Post}_{c,t} \times \Delta \textit{NPL}_{i,t} + \beta_6 \textit{Post}_{c,t} \times \Delta \textit{NPL}_{i,t-1} \\
&+ \beta_7 \textit{Treatment}_c \times \Delta \textit{NPL}_{it+1} + \beta_8 \textit{Treatment}_c \times \Delta \textit{NPL}_{it} \\
&+ \beta_9 \textit{Treatment}_c \times \Delta \textit{NPL}_{it-1} + \beta_{10} \textit{Post}_{c,t} \times \textit{Treatment}_c + \beta_{11} \textit{Post}_{c,t} \\
&+ \beta_{12} \textit{Treatment}_c + \beta_{13} \Delta \textit{NPL}_{i,t+1} + \beta_{14} \Delta \textit{NPL}_{i,t} + \beta_{15} \Delta \textit{NPL}_{i,t-1} \\
&+ \beta_{16} \textit{Earnings ex. LLP}_{i,t-1} + \boldsymbol{\Theta}\ \boldsymbol{\textit{CONTROLS}} + u_t + w_c + e_{i,t}. \qquad (1)
\end{aligned}$$

In this model, *c* is a country, *i* is a bank, and *t* is a year; each observation is a bank-year. The dependent variable, *Loan Loss Provisions*, is the annual loan loss provisions divided by lagged total loans. (Variable definitions appear in the Appendix.) *Post* and *Treatment* comprise the difference-in-differences model. We estimate this model on a group of banks whose countries established or improved their PCRs (for which the *Treatment* dummy switches on) as well as a control group of banks that are individually matched to treatment banks (for which *Treatment* equals zero). *Post* is also an indicator variable, designed to absorb the confounding effects of overall trends in the dependent variable around the treatment. *Post* equals one for bank-years after the event year. (For control banks, this is the same year as the matched treatment observation.)

The independent variable of interest includes changes in nonperforming loans (*ΔNPL*), as well as *Post* and *Treatment*. Consistent with Bushman and Williams (2012), future, current, and lagged variants of NPL growth variables are included in the model. In the tables, to avoid subscripts, we label $\Delta NPL_{i,t+1}$ as *Future* Δ*NPL*, $\Delta NPL_{i,t}$ as *Current* Δ*NPL*, and $\Delta NPL_{i,t-1}$ as *Lagged* Δ*NPL*. We are interested mainly in the coefficient on the triple differences estimator, β_1. This term captures the association between current loan loss provisions and subsequent changes in NPLs for banks in treatment countries in the post period, relative to a control group of banks over the same period.

Equation (1) is saturated with year fixed effects (u_t) and country fixed effects (w_c). In the presence of time-invariant country fixed effects, *Treatment* is not identified in the estimation models (β_{12} is dropped). The indicator *Post*, however, is identified even when the model includes year fixed effects because the events we study take place in a staggered fashion. *CONTROLS* is a vector that includes bank-level control variables: *Size* (natural logarithm of total USD assets), *Capital* (the ratio of equity to assets), *Profitability* (the return-on-equity ratio), *Loan growth* (annual percentage change in total loans), *Loan intensity* (total loans as a percentage of total assets), *Interest expense* (annual interest costs divided by total liabilities), *Cost-to-income ratio* (operating expenses as a percentage of operating income), and *Earnings ex. LLP* (earnings before loan loss provisions as a fraction of total loans). We also account for additional bank characteristics that could affect loan loss provisioning. We control for the type of financial reporting and regulatory reporting standards the bank uses (*IFRS reporter* and *Basel reporter* dummies). The bank-level definition of these variables allows us to isolate the confounding effect of voluntary as well as mandatory adopters of IFRS and Basel rules. To complement these terms, we also account for *Big Four auditor*, a dummy that switches on if the bank is audited by one of the Big Four audit firms or their local partners/predecessors.

In addition to bank-level controls, in order to account for concurrent macroeconomic conditions, we add to our models *GDP per capita*, *GDP growth*, and *Concentration* (total assets of the five largest banks in the country divided by the total assets of the banking sector). We extend the macroeconomic controls in our robustness checks, in which we control for the extent of commercial loans in the country, as well as the strength of legal rights, private credit bureau coverage, and local economic conditions. (These variables are available for a smaller subset of the sample.)

The link between information sharing and better provisioning decisions is economically coherent. Mandatory exchange of information among *banks* would improve *banks'* understanding and evaluation of loss events, which, in turn, would help *banks* make more informed and timelier provisioning decisions. However, PCR adoptions and changes are not random. PCRs are formed to increase the quality of the information provided to lenders and to improve supervision (Miller

2003). These inputs imply that we analyze the treatment effect on the treated group, rather than a coin toss.

Our identification assumption requires that PCR events be unrelated to the individual banks' provisioning decisions. In the absence of counterfactuals or a randomized experiment, we construct a propensity-score-matched sample of banks that we use as our control group. Specifically, for each bank in a treatment country, we find a matched bank (from a non-treatment country) that is similar to the treatment bank in terms of entity-level observables (i.e., *Size, Profitability*, *Capital*, *Loan intensity*, *Interest expense*, *Cost-to-income ratio*). We ensure that the treatment and control groups are statistically similar at the time of the respective treatment (untabulated). Despite this observation, we realize that a variety of issues might remain. We discuss these concerns below and attempt to address them through several robustness tests.

RESULTS

Main Findings

To examine the effect of changes to the PCRs on banks' loan loss provisioning timeliness, we estimate equation (1). Table 3 presents the relevant results. Our findings show that the coefficient on *Post* × *Treatment* × *Future ΔNPL* is positive and significant. The estimate in column (1) suggests that enhanced information sharing through PCRs is associated with a 10.3 percentage point relative increase in the association between future changes in NPLs and loan loss provisions. The results continue to hold when we include the control variables in column (2). The coefficient estimates on other regressors are typically consistent with those of prior work (e.g., Bushman and

Williams 2012); all variants of NPL growth variables as well as *Earnings ex. LLP* are positively associated with *Loan loss provisions*.

Overall, the findings in Table 3 suggest that credit information sharing enhances LLR timeliness. In an ideal experiment, we would assign information sharing to a randomly selected group of banks to analyze the treatment effect relative to a control group. However, our inferences face several identification challenges, which we address by performing additional tests. We present the results of these analyses in Table 4. First, to verify the validity of a matched sample of banks as a control group and to eliminate concerns about the drivers of regulation, we test the pre-treatment trends in the outcome variable for treatment and control countries. As shown in column (1) of Panel A, the coefficient on *Pre1* × *Treatment* × *Future ΔNPL* is statistically insignificant, suggesting that pre-treatment provisioning behavior is similar for treatment and control countries. According to this assumption, in the absence of the PCR events we explore, the average relationship between *Future NPLs* and *Loan loss provisions* would have continued to be similar across the treatment and control groups.

The second column in Panel A of Table 4 provides further evidence on the timing of treatment. Specifically, we observe a statistically significant coefficient on the triple estimator over a shorter treatment window. This test helps shed light on the timeline over which the treatment effect sets in. Note that the evidence presented in Panel A of Table 4 also alleviates the concern that information sharing could reduce banks' incentives to collect information to the extent that there will be a reduction in the timeliness of LLR. In other words, PCRs could lead banks to free-ride on other banks' information. Over time, such actions would weaken the timeliness of LLR practices. If this were the case, we would observe a reversal in the improved provisioning timeliness in the few years after PCR initiations. Panel A of Table 4 shows that this is not the case.

The positive and significant effects for a relatively long treatment window suggest that PCR reforms have a long-lasting effect (even though we cannot conclude that banks' incentives to collect information remain the same).

We next test the robustness of our findings in alternative subsamples. The treatment and control groups could be inherently different in their loan loss provisioning practices due to omitted characteristics, which could also interfere with the treatment. Panel B of Table 4 contains the results of the tests that get at these issues. The first sensitivity test focuses on a sample of treatment countries and thus limits the main variation to the staggered timing of events. This specification is free from any assumptions about a control sample. As the first column shows, our inferences remain similar.[17] The estimation sample shown in the second column excludes negative provisioning values to ensure that our findings are not an artifact of unusual provisioning behavior. While the tests we present in column (3) are based on a subsample of banks that do *not* change their financial reporting, regulatory reporting, or auditor in the previous year, in the sample shown in column (4), we drop countries that exhibit a negative GDP growth. Finally, in the fifth column, we remove Germany, Azerbaijan, and Italy, which are associated with modest increases in population coverage, as well as their respective control observations. Moreover, as noted above in the discussion of Table 2 Panel B, Italy dominates the sample. Accordingly, excluding Italy serves as a robustness check of the representativeness of the sample. We note that in each of these alternative specifications in Table 4, our conclusions from Table 3 continue to hold.

[17] To ensure consistency with the rest of our analysis, we cluster standard errors by country in column (1) of Table 4, Panel B. However, we recognize the potential concern about the small number of clusters. We estimate the model using country-year clusters, bank clusters, and no clusters (with heteroskedasticity correction only). The t-stat for the coefficient of interest—which is 2.01 in the original model—becomes 1.87, 2.15, and 4.43, respectively.

Even though the tests presented in Panels A and B of Table 4 allay selection concerns, there could be other treatment effects *concurrent* with PCR-related events that could confound our findings. For example, PCR improvements might be just one part of a broader regulatory agenda that aims to enhance creditor rights and maintain macroeconomic stability. If other contemporaneous improvements in the credit landscape (e.g., bankruptcy reforms, the introduction of collateral registries) are the actual treatment effect that frequently coincides with PCR events, our inferences would be erroneous.[18] To tackle these issues and to take into account credit-related changes at the country-year level, we control for indices that trace the strength of creditor rights and coverage of private credit bureaus. Further, we include in the right-hand side country-level stock returns, capital market participation, corporate debt issuance, and the ratio of corporate debt to household debt in order to control for local trends and developments. As Panel C of Table 4 shows, these additional considerations—which are added individually in columns (1) through (6), collectively in column (7), and interactively with the independent variables of interest in columns (8) and (9)—support our conclusions.

Finally, we examine alternative measures of timeliness of provisioning. As noted above, our adoption of the Bushman and Williams (2011) framework offers several advantages. Nonetheless, as a robustness test, we evaluate a different independent variable, *LLR Timeliness*, which we define as the ratio of NPLs to loan loss reserves. This metric is simple and does not necessitate a triple-differences model; however, it could be noisy, especially when *LLR Timeliness* is greater than one. Following prior work, we use two versions of *LLR Timeliness*. *LLR Timeliness (Beatty and Liao 2011)* is the ratio of current loan loss reserves to current NPLs, and *LLR Timeliness (Akins et al.*

[18] To be sure, the staggered adoption of PCRs mitigates these issues, because, in order to invalidate our inferences, confounding factors should systematically correlate with a series of PCR improvements. Further, we account for accounting and regulatory reporting features at the bank-year level.

2017) is the ratio of current loan loss reserves to year-ahead NPLs.[19] The estimation results in Panel D of Table 4 suggest that the difference-in-differences estimator is positive and significant for both variants of *LLR Timeliness*, which supports our prediction that PCR improvements enhance the timeliness of loan loss provisions.

Mechanism Tests: Alternatives to Increases in Banks' Information Set

Our findings thus far suggest that the PCR improvements we study improve the timeliness of banks' loan loss provisions. We contend that three factors could be the underlying mechanism. The first one is learning, which is our assertion. According to this narrative, banks' better understanding of loss events and their possession of enhanced objective evidence make provisions timelier. Another channel through which PCR reforms could improve loan loss provisioning is supervision (the "supervision" hypothesis). This suggests that regulators, with their strengthened access to information, may induce banks to enhance their loan loss provisioning. The third potential mechanism is economic improvements in borrower pools (the "better borrowers" hypothesis). In this view, bank provisions could become timelier as a result of systematically and inherently better borrowers/contracts. This story could manifest in reduced adverse selection (as "bad" borrowers leave the market) and reduced moral hazard (as borrowers behave "better" after the loan initiation).

While these explanations are not mutually exclusive, there are reasons the "supervision" and "better borrowers" arguments may not hold. Most of our events apply to countries with existing registries; therefore, it is unclear why an increase in information (along the intensive margins)

[19] Akins et al. (2017) argue that using subsequent NPLs in the denominator captures the essence of Beatty and Liao (2011) and Bushman and Williams (2012)—greater timeliness in the loan loss accrual process includes the anticipation of changes in NPLs.

would significantly intensify regulatory stringency. (This argument should be much stronger for registry establishments, i.e., along the extensive margin.) Regarding the "better borrowers" argument, it is important to note that our paper does not look at loan issuance or loan defaults. That is, our focus is not on the *amount* of provisioning but on its *timeliness*. While improvements in borrower behavior could reduce NPLs (or provisions and reserves), it is not as clear why it would improve the *relationship* between current loan loss provisions and future NPL changes.

At any rate, to shed light on the underlying mechanism, we conduct a series of cross-sectional tests, the results of which we present in Table 5. We observe that our main findings hold for subsamples that are partitioned based on changes in NPLs and changes in loan maturity (both calculated at the country-year level). If the better-borrowers explanation were the only mechanism, we would find no result for the subsamples with worsening NPLs and loan maturity lengths. However, our findings hold across all subsamples (columns 1–4). To investigate bank supervision, we split our sample on regulatory stringency (the supervisory power variable obtained from Barth et al. 2013). Again, our inferences apply to jurisdictions with low as well as high supervisory power (columns 5 and 6). Collectively, alternatives such as the supervision and credit quality arguments do not fully explain the results, which implies that banks improve their loan loss provisioning decisions by using a richer set of information.

As an additional exercise, we partition the sample on corporate indebtedness to understand whether banks' learning is coming from commercial credit or individual loans. While this issue itself is not a threat to the learning story, it is a useful effort to ascertain the components of banks' learning because the methodology employed for loan-loss provisioning varies across these types of loans. For commercial loans, the information is perhaps used directly at the individual borrower level. In the case of homogenous loans, such as consumer credit, loan loss provisions are typically made for a basket. Accordingly, the information obtained for homogenous loans is likely useful at the group level. More information on defaults, arrears, and other questionable status updates in a basket will trigger an increase in the assessed average (expected) risk for that group of borrowers. We find that our results hold for jurisdictions with high and low levels of corporate debt, although the estimates are economically stronger when corporate indebtedness is high. This observation suggests that both types of credit seem to be relevant for provisioning.

A Global Analysis of PCR Initiations

The preceding analysis provides reassuring evidence that omitted factors, such as local economic shocks, omitted regulations, and changes in borrower pools, do not drive our conclusions. To supplement these inferences and assess the generalizability of our message, we examine the provisioning effects of PCR initiations on a global sample. We obtain information on the establishment date, coverage, and other key features of PCRs from the World Bank's credit reporting database (Bruhn, Farazi, and Kanz 2013).[20] This database is a collation of World Bank surveys of global credit reporting agencies and regulators conducted since the 1990s. We conduct our tests on a global dataset of bank financial characteristics merged with the country-specific details of credit reporting systems. Panel A of Table 6 details the introduction years of the sample PCRs. The survey stops in 2010, and the latest recorded PCR adoption is 2008. The establishment year of the sample treatment countries starts in 1996 due to data unavailability (of international bank financials from Bankscope).

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[20] https://web.archive.org/web/20170909105658/http://econ.worldbank.org/WBSITE/EXTERNAL/EXTDEC/EXTGLOBALFINREPORT/0,,contentMDK:23269620~pagePK:64168182~piPK:64168060~theSitePK:8816097,00.html.

The fact that the global sample consists mostly of developing countries and is smaller than the European sample may raise concerns about the generalizability of the findings from this sample. To provide insights into this issue, we compare the treatment countries in our global sample with our European sample, as well as the U.S., the UK, and Japan (untabulated). On average, the countries in the global sample have weaker creditor rights and low-quality credit information. However, as shown in Panel B of Table 6, bank characteristics are somewhat comparable across the two samples.

We estimate equation (1) on this sample and present the pertinent results in Panel C of Table 6. We find an increase in the loan loss provisioning timeliness of treatment banks, relative to a control group (which we construct based on bank characteristics as in the control observations in the European sample). The coefficients on the triple differences estimator are significantly positive and are greater than those obtained on the European sample (0.284 vs. 0.102). This could be because the initiations of PCRs result in more potent effects than coverage reforms made on pre-existing registries. We also verify that the parallel trends assumption holds for the global sample, and the effects we observe are present for a shorter treatment window (Panel D of Table 6).[21]

The global sample also allows us to shed light on the mechanism. In this setting, we are able to compare the difference between PCRs' information collection and information distribution. This test helps us further examine whether the main treatment effects are driven by information sharing or by other aspects of PCRs, such as changes in borrower characteristics and supervision. Moreover, this is a strong test to rule out the confounding effects of concurrent regulations. To

[21] In untabulated tests, we verify that these global-sample inferences are rosbust to the restrictions that we originally apply to the European sample in Panel B of Table 4. Furthermore, our findings from the global sample hold if we exclude (*i*) the countries whose dates differ in official records and the World Bank data, and (*ii*) the reforms used in the European sample (also untabulated for brevity).

empirically investigate this issue, we use the index values in the World Bank database that measure the extent of information collection and information distribution. These variables are based on 56 yes/no questions related to borrower and loan characteristics, such as demographic attributes, tax records, credit history (e.g., defaults, arrears, bankruptcy, utility payments), loan applications, loan terms (e.g., interest, maturity, collateral), and outstanding loan amount and payments. We then replace *Treatment* with *Info Distribution*, effectively partitioning the treatment space.

The results presented in Table 7 underscore the significant role of information distribution. Specifically, the estimates in column (1) suggest that the greater the information distribution is, the larger the effect of the PCR on LLR timeliness would be. Furthermore, this inference remains unchanged even after we hold the *collection* of information rather constant. As columns (2) and (3) show, information distribution plays an important role in the timeliness of banks' loan loss provisions. Overall, these inferences are consistent with the learning explanation, rather than the supervision story.

CONCLUSION

We use various reforms on public credit registries (PCRs) to examine whether information sharing among banks improves their loan loss recognition. We find evidence that PCR information helps banks better understand credit risks and enhances the timeliness of banks' loan loss recognition practices. The findings we present are relevant to the accounting literature specializing in banking and loan loss provisioning. As Bushman (2016) points out, accounting choices do not occur in a vacuum, and we need a better understanding of what causes banks' provisioning attributes (within and across banks). Our evidence on information sharing is one important input contributing to this endeavor. Our conclusions are consistent with the notion that bank managers' private information creates a wide scope of judgment in accounting choices. Even though we recognize that lenders can use the discretion in loan loss provisioning decisions opportunistically, our evidence suggests that the average lender uses enhanced loan information to make better provisioning decisions. In this respect, our study responds to Beatty and Liao's (2014) call for a better understanding of loan loss provisioning practices.

The PCR setting offers several avenues for future research. We focus on the role of PCRs in loan loss provisioning. However, PCR information is a significant input that banks use extensively for purposes other than provisioning, which also deserves a thorough investigation. For example, information sharing may impact other aspects of the balance sheet, i.e., not only provisioning and assets but also bank capital and liabilities. In particular, improvements in banks' information sets could help them manage their regulatory capital more efficiently, leaving more room for lending to the real economy. Moreover, as prior work has shown, PCRs can affect financial stability, possibly through improved provisioning (Houston, Lin, Lin, and Ma 2010). In particular, researchers argue that credit registries may be used as a regulatory tool to alleviate malfunctioning credit markets (Mian 2012). Future research could shed light on the types of information collected and the characteristics of PCRs that can help improve the functioning of credit markets.

Appendix: Variable Definitions

Variable Name	Definition	Bankscope code	SNL field code
Loan loss provisions	Loan loss provisions divided by year-ago total loans.	data 2095 and 2000	#131958
Δ*NPL*	Yearly change in the ratio of nonperforming loans to total loans (%).	data 18200	#243682
Earnings ex. LLP	Earnings before loan loss provisions divided by lagged loans (%).	data 2105, 2095, and 2000	#132723, #131958, and #131923
Treatment	Indicator equals one if the country of the bank reformed its PCR (see Table 1).	n/a	n/a
Post	Indicator equals one if the year of observation is after the start of *Treatment*.	n/a	n/a
Size	USDmm total assets, in natural logarithm.	data 2025	#132264
Capital	The ratio of equity to assets (%).	data 2055 and 2025	#131939 & 132264
Profitability	Return on equity (%).	data 4025	#132006
Loan growth	Year-over-year growth in loans (%).	data 2000	#131923
Loan intensity	The ratio of loans to assets (%).	data 2000 and 2025	#131923 &132264
Interest expense	Annual interest expense as a fraction of liabilities (%).	data 18045	#243388
Cost-to-income ratio	Operating expense as a percent of operating income (%).	data 4029	#226949
Big Four auditor	Indicator equals one if the bank is audited by a Big Four auditor.	auditor	#243684
IFRS reporter	Indicator equals one if the bank is reporting under IAS or IFRS.	accstand	#132097
Basel reporter	Indicator equals one if the bank is reporting under Basel regulation.	data 30700	#225203
Concentration	Assets of five largest banks as a share of total commercial banking assets (%).	IMF GFD Code: gfddoi06	
GDP per capita	Gross domestic product per capita (constant prices in 2010 USD).	IMF GFD Code: ny_gdp_pcap_kd	
GDP growth	Current prices GDP growth (%).	IMF GFD Code: ny_gdp_mktp_cd	

Notes: Test-specific variables are defined in table captions. Controls are measured with a one-year lag.

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Table 1. Credit Registry Events: Europe

Country	Reform year	Population coverage at year t-1 (%)	Population coverage at year t+1 (%)	Population coverage at year t+3 (%)	Nature of reform
Albania	2008	0.0	9.9	17.0	Registry establishment
Azerbaijan	2005	0.0	1.1	3.1	Registry establishment
Belarus	2008	1.1	23.4	49.5	Registry establishment and significant increase in coverage (non-banks added, threshold reduced)
Belgium	2011	57.2	89	96.4	Significant increase in coverage (non-banks added, threshold reduced)
Bosnia & Herzegovina	2007	0.0	0.0	30.2	Registry establishment
Bulgaria	2011	37.0	56.3	62.9	Significant increase in coverage (non-banks added)
France	2007	12.3	28.3	33.3	Significant increase in coverage (threshold reduced)
Germany	2014	1.3	1.6	1.9	Significant increase in coverage (threshold reduced)
Italy	2009	11.8	16.6	24.1	Significant increase in coverage (threshold reduced)
Latvia	2008	2.6	46.5	59.7	Registry establishment
Lithuania	2012	15.0	28.3	33.9	Significant increase in coverage (non-banks added, threshold reduced)
Macedonia	2008	4.0	28.1	34.3	Significant increase in coverage (threshold reduced) and scope (data on collateral and risk exposures)
Romania	2009	4.5	13.0	14.0	Significant increase in coverage (non-banks added)

Notes:
- The Bosnian registry states that its coverage was non-zero at t+1, inconsistent with the World Bank figures.
- The t-1 data for Germany was missing. The average of t-2 and t values are used instead. Coincidentally, these figures were equal to 1.3 each.
- There was non-zero coverage in Latvia before the registry was established, due to a rudimentary predecessor called the registry of debtors.

Table 2. Sample Statistics

This table presents the summary statistics for the European sample. Each observation is a bank-year. All variables are defined in the Appendix. For indicator variables, degenerate moments are omitted for brevity.

Panel A. Summary statistics

	Mean	stdev	p10	p50	p90	N
Loan loss provisions	0.835	1.431	0.000	0.466	1.927	7,953
Treatment	0.517	.	.	.	.	7,953
Post	0.494	.	.	.	.	7,953
Future ΔNPL	0.743	2.744	-1.240	0.120	3.610	7,953
Current ΔNPL	0.613	2.687	-1.389	0.090	3.300	7,953
Lagged ΔNPL	0.479	2.591	-1.450	0.040	2.970	7,953
Earnings ex. LLP	2.312	3.730	0.329	1.588	4.499	7,953
Size	20.916	2.071	18.767	20.338	23.990	7,953
Capital	9.669	5.828	3.160	8.875	16.314	7,953
Profitability	6.371	8.347	0.750	6.090	14.540	7,953
Loan growth	11.053	21.998	-3.125	7.111	25.581	7,953
Loan intensity	67.931	19.321	40.741	72.157	89.362	7,953
Interest expense	2.194	1.338	0.930	1.800	3.880	7,953
Cost-to-income ratio	65.726	16.941	48.250	64.640	82.520	7,953
Big Four auditor	0.646	.	.	.	.	7,953
IFRS reporter	0.623	.	.	.	.	7,953
Basel reporter	0.644	.	.	.	.	7,953
Concentration	75.450	16.331	54.895	74.237	94.625	7,953
GDP per capita	44,784	21,060	16,748	38,237	75,144	7,953
GDP growth	5.517	10.404	-8.333	7.407	16.667	7,953

Panel B. Number of bank-years throughout the [-3, +3] period where 0 is the year of treatment

Country	In the respective database (see notes), with no data restriction except obs. must be in the [-3, +3] event window	In the respective database, and also conditional on having nonmissing regression variables	In the final estimation sample
Albania	78	30	22
Azerbaijan	124	39	38
Belarus	139	41	35
Belgium	205	26	17
Bosnia H.	160	39	33
Bulgaria	100	35	17
France	1,681	418	400
Germany	1,701	423	416
Italy	4,399	3,053	2,999
Latvia	131	46	46
Lithuania	90	4	4
Macedonia	96	24	21
Romania	176	65	61
Total	9,080	4,243	4,109

Notes:

Data for countries with pre-2010 events is from Bankscope: Albania, Azerbaijan, Belarus, Bosnia and Herzegovina, France, Italy, Latvia, Macedonia, and Romania.

Data for countries with post-2011 events is from SNL Financial: Belgium, Bulgaria, Germany, and Lithuania.

Table 3. Credit Information Sharing and Loan Loss Recognition Timeliness

This table presents the results from the estimation of equation (1). Each observation is a bank-year. *Loan loss provisions* is the ratio of current loan loss provisions to lagged total loans. *Treatment* is an indicator variable that switches on only for banks that belong to countries that introduced a credit registry reform. (See Table 1 for details.) *Post* is an indicator variable that equals one for treatment banks from one year after the credit reform, as well as the matched control observation. *Treatment* is not identified individually in the presence of country fixed effects. *ΔNPL* is the annual change in the ratio of nonperforming loans to total loans. *Future*, *Current*, and *Lagged* define the timing of *ΔNPL* and stand for the next year, current year, and last year, respectively. *Earnings ex. LLP* is income plus loan loss provisions, divided by lagged loans. *Size* is the natural logarithm of the bank's current total assets (USD million). *Capital* is the ratio of equity to assets. *Profitability* is return on equity. *Loan growth* is the annual growth in total loans. *Loan intensity* is the ratio of loans to total assets. *Interest expense* is annual interest expense as a fraction of total loans. *Cost-to-income ratio* is operating expenses divided by operating income. *IFRS reporter*, *Basel reporter*, and *Big Four auditor* are dummy variables indicating reporting under IAS/IFRS, Basel regulation, and having a Big Four auditor, respectively. *Concentration* is the ratio of the assets of the five largest banks as a share of total commercial banking assets. *GDP per capita* is real GDP per capita. *GDP growth* is the annual growth in gross domestic product. The Appendix details the data codes and calculations of regression variables. *T*-statistics (in parentheses) are robust to within-country correlation as well as heteroscedasticity. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
	Loan loss provisions	*Loan loss provisions*
Post × Treatment × Future ΔNPL	0.103**	0.102**
	(2.57)	(2.41)
Post × Future ΔNPL	-0.060*	-0.065*
	(-1.89)	(-1.82)
Treatment × Future ΔNPL	-0.024	-0.025
	(-0.99)	(-1.12)
Post × Treatment × Current ΔNPL	0.067	0.079
	(1.27)	(1.62)
Post × Current ΔNPL	0.026	0.015
	(0.51)	(0.32)
Treatment × Current ΔNPL	-0.093**	-0.093**
	(-2.26)	(-2.42)
Post × Treatment × Lagged ΔNPL	0.009	0.026
	(0.24)	(0.66)
Post × Lagged ΔNPL	0.054*	0.032
	(1.69)	(1.00)
Treatment × Lagged ΔNPL	-0.052**	-0.050**
	(-2.43)	(-2.48)
Post × Treatment	-0.096	-0.125
	(-1.06)	(-1.05)

Post	0.193*	0.218**
	(1.90)	(2.02)
Future ΔNPL	0.037**	0.043**
	(2.07)	(2.16)
Current ΔNPL	0.139***	0.134***
	(3.56)	(3.69)
Lagged ΔNPL	0.082***	0.066***
	(4.20)	(3.31)
Earnings ex. LLP	0.089***	0.109***
	(4.78)	(5.51)
Size		-0.003
		(-0.30)
Capital		0.015
		(1.56)
Profitability		-0.035***
		(-7.45)
Loan growth		0.002
		(1.39)
Loan intensity		0.003
		(1.08)
Interest expense		0.070
		(1.17)
Cost-to-income ratio		-0.002
		(-1.01)
Big Four auditor		0.079*
		(1.71)
IFRS reporter		0.027
		(0.26)
Basel reporter		-0.134
		(-1.28)
Concentration		-0.004
		(-0.87)
GDP per capita		-0.000
		(-0.81)
GDP growth		-0.005
		(-1.05)
Observations	7,953	7,953
Adjusted R-squared	0.380	0.417
Country FE and Year FE	Y	Y

Table 4. Credit Information Sharing and Loan Loss Recognition Timeliness: Robustness

This table explores the robustness of our results. Each observation is a bank-year, and all regression variables are as defined in the Appendix and Table 3. Panel A depicts the results from two alternative specifications. In column (1), a new variable, *Pre1*, switches on for the year of the reform and the year before, providing a test for the pre-treatment parallel trends. In column (2), the length of the treatment window is two years (instead of three years). Panel B presents the results from regressions that rely on alternative subsamples, as indicated in column headings. The sample shown in column (1) is limited to treatment banks; that in column (2) excludes observations with negative *Loan loss provisions*; that in column (3) removes bank-years that moved to IFRS, Basel reporting, or a Big Four auditor in the previous year; that in column (4) drops countries that experience a negative GDP growth; that in column (5) excludes bank-years from Azerbaijan, Germany, Italy, as well as their corresponding matched control observations. In Panel C, additional control variables are added to the main estimation model. *Creditor rights* is the country's Strength of Creditor Rights value, obtained from Doing Business. *Private credit bureau coverage* is the annual percentage change in the country's private credit coverage of the population, obtained from Doing Business. *Capital markets development*, *Stock returns*, and *Commercial loan issuance* come from the World Bank Global Financial Development Database. Respectively, the data codes are GFDD.OM.01 (number of domestically incorporated companies listed on the country's stock exchanges at the end of the year per 1,000,000 people), GFDD.OM.02 (the growth rate of the annual average stock market index), and GFDD.DM.12 (ratio of new syndicated borrowing volume by private entities in non-financial industries to GDP). *Corporate debt to household debt ratio* is from the IMF Global Debt Database. In Panel D, *LLR Timeliness* is the dependent variable. It is calculated as the ratio of current loan loss reserves to nonperforming loans, as indicated in column headings. The coefficient of interest is *Post × Treatment.* All lower order terms and NPLs include the components of the triple differences estimators involving future, current, and lagged changes in NPLs, as well as their interactions with *Post* and *Treatment* indicators. All previous controls include the regressors shown in Table 3. *T*-statistics (in parentheses) are robust to within-country correlation as well as heteroscedasticity. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Timing robustness

	(1)	(2)
	Controlling for pre-treatment trends	Two-year treatment window
	Loan loss provisions	*Loan loss provisions*
Post × Treatment × Future ΔNPL	0.122**	0.116*
	(2.58)	(1.84)
Pre1 × Treatment × Future ΔNPL	0.043	0.048
	(0.88)	(0.97)
Observations	7,953	6,764
Adjusted R-squared	0.419	0.395
All lower order terms and NPLs	Y	Y
All previous controls	Y	Y
Country FE and Year FE	Y	Y

Panel B. Sample robustness

	(1)	(2)	(3)	(4)	(5)
	Excl. controls banks	Excl. negative LLPs	Excl. banks that changed reporting	Excl. countries with negative GDP growth	Excl. Azerbaijan, Germany, Italy, and their control obs.
	Loan loss provisions	*Loan loss provisions*	*Loan loss provisions*	*Loan loss provisions*	*Loan loss provisions*
Post × Treatment × Future ΔNPL	0.063**	0.100**	0.084*	0.135***	0.254**
	(2.01)	(2.25)	(1.85)	(2.78)	(2.28)
Observations	4,109	7,535	7,404	5,798	1,411
Adjusted R-squared	0.437	0.657	0.419	0.396	0.489
Pre-trends included and insignificant	Y	Y	Y	Y	Y
All lower order terms and NPLs	Y	Y	Y	Y	Y
All previous controls	Y	Y	Y	Y	Y
Country FE and Year FE	Y	Y	Y	Y	Y

Panel C. Additional controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Loan loss provisions	*Loan loss provisions*	*Loan loss provisions*	*Loan loss provisions*	*Loan loss provisions*	*Loan loss provisions*	*Loan loss provisions*	*Loan loss provisions*	*Loan loss provisions*
Post × Treatment × Future ΔNPL	0.104**	0.104**	0.111**	0.100**	0.110**	0.086*	0.093**	0.091*	0.096**
	(2.38)	(2.39)	(2.62)	(2.44)	(2.59)	(1.91)	(2.11)	(1.89)	(1.99)
Creditor rights	-0.038						0.030	0.156*	0.030
	(-0.56)						(0.47)	(1.95)	(0.59)
Private credit bureau coverage		-0.000					0.008***	0.005**	0.009***
		(-0.00)					(3.28)	(2.12)	(4.25)
Capital markets development			-0.011***				-0.023***	-0.029***	-0.017**
			(-4.53)				(-3.78)	(-5.10)	(-2.40)
Stocks returns				0.000			0.001	-0.003	0.006**
				(0.08)			(0.32)	(-1.15)	(2.26)
Commercial loan issuance					-0.005		0.003	0.016	0.001
					(-0.69)		(0.35)	(1.18)	(0.07)
Corporate debt to household debt ratio						-0.177	0.014	0.003	0.063
						(-1.20)	(0.07)	(0.02)	(0.32)
Observations	7,808	7,808	7,782	7,720	7,716	7,626	7,317	7,317	7,317
Adjusted R-squared	0.429	0.429	0.417	0.402	0.415	0.399	0.378	0.382	0.395
Controls interacted with *Post*, *Treatment*, and *Post × Treatment*	N	N	N	N	N	N	N	Y	N
Controls interacted with *Future ΔNPL*, *Current ΔNPL*, and *Lagged ΔNPL*	N	N	N	N	N	N	N	N	Y
Pre-trends included and insignificant	Y	Y	Y	Y	Y	Y	Y	Y	Y
All lower order terms and NPLs	Y	Y	Y	Y	Y	Y	Y	Y	Y
All previous controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Country FE and Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Panel D. Measure robustness

	(1)	(2)
	LLR Timeliness (Beatty and Liao 2011)	*LLR Timeliness* (Akins et al. 2017)
Post × Treatment	0.143**	0.198***
	(2.56)	(3.36)
Observations	6,984	7,494
Adjusted R-squared	0.188	0.207
Previous controls	Y	Y
Country FE and Year FE	Y	Y

Table 5. Cross-sectional Tests and the Mechanism

This table presents results from the tests exploring the main findings in alternative subsamples. Each observation is a bank-year, and all regression variables are as defined in the Appendix and Table 3. There are four conditioning variables measured at the country level: Δ *Nonperforming loans* is the change in the ratio of nonperforming loans to total loans (World Bank item GFDD.SI.02), Δ *Loan maturity* is the change in the average maturity of the loans issued in the year (World Bank item GFDD.DM.14), *Supervision strength* is the Barth et al. (2013) index showing the relative strength of the national bank regulator (index Sup Power), and *Corporate debt* is the indebtedness of the private sector relative to GDP (obtained from IMF). All lower order terms and NPLs include the components of the triple differences estimators involving future, current, and lagged changes in NPLs, as well as their interactions with *Post* and *Treatment* indicators. All previous controls include the regressors shown in Table 3. *T*-statistics (in parentheses) are robust to within-country correlation as well as heteroscedasticity. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Conditioning variable: Δ *Nonperforming loans*		Conditioning variable: Δ *Loan maturity*		Conditioning variable: *Supervision strength*		Conditioning variable: *Corporate debt*	
	Decline	Increase	Decline	Increase	Low	High	< GDP	≥ GDP
	Loan loss provisions	*Loan loss provisions*	*Loan loss provisions*	*Loan loss provisions*	*Loan loss provisions*	*Loan loss provisions*	*Loan loss provisions*	*Loan loss provisions*
Post × Treatment × Future ΔNPL	0.109*	0.102**	0.130	0.065**	0.197*	0.103*	0.123**	0.200***
	(1.65)	(2.17)	(1.64)	(2.03)	(1.93)	(1.69)	(1.97)	(3.62)
Observations	3,241	4,489	4,207	3,380	3,622	4,240	5,477	2,149
Adjusted R-squared	0.351	0.442	0.419	0.458	0.298	0.467	0.402	0.420
All lower order terms and NPLs	Y	Y	Y	Y	Y	Y	Y	Y
All previous controls	Y	Y	Y	Y	Y	Y	Y	Y
Country FE and Year FE	Y	Y	Y	Y	Y	Y	Y	Y

Table 6. Credit Information Sharing and Loan Loss Recognition—Results from a Global Sample

This table examines our research question on a global sample, in which each treatment event is a registry establishment. Panel A introduces these events. Panel B presents the sample statistics, Panel C includes the main regression results (similar to Tables 1, 2, and 3 for the European sample), and Panel D contains the results of robustness tests (similar to Panel A of Table 4 for the European sample). In the statistics and estimates shown in Panels B, C, and D, each observation is a bank-year. All variables are defined in the Appendix. *T*-statistics (in parentheses) are robust to within-country correlation as well as heteroscedasticity. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. List of reforms

Country Name	Start Year	Country Name	Start Year
Albania	2008	Latvia	2008
Angola	2002	Lithuania	1996
Azerbaijan	2005	Macedonia	1997
Belarus	2008	Malaysia	2001
Bosnia & Herzegovina	2007	Mauritius	2005
Bulgaria	2000	Nicaragua	2007
China	2005	Nigeria	1998
Costa Rica	1996	Romania	2000
Czech Republic	2002	Slovakia	1997
Ethiopia	2004	Vietnam	1999

Panel B. Descriptive statistics

	Mean	stdev	p10	p50	p90	N
Loan loss provisions	2.074	3.425	0.000	1.007	5.230	1,285
Treatment	0.550	.	.	.	.	1,285
Post	0.581	.	.	.	.	1,285
Future ΔNPL	0.358	6.019	-4.760	-0.140	6.370	1,285
Current ΔNPL	0.327	5.970	-5.000	-0.130	6.110	1,285
Lagged ΔNPL	0.256	5.776	-4.840	-0.130	6.400	1,285
Earnings ex. LLP	6.838	8.811	0.759	4.250	14.697	1,285
Size	21.017	2.223	18.328	20.840	23.856	1,285
Capital	10.896	8.252	3.810	8.862	19.807	1,285
Profitability	12.047	16.885	0.430	12.190	28.630	1,285
Loan growth	22.122	37.276	-12.956	14.955	62.000	1,285
Loan intensity	51.730	19.234	24.433	53.415	75.074	1,285
Interest expense	4.811	3.884	1.130	3.840	9.170	1,285
Cost-to-income ratio	54.562	20.476	31.580	52.020	79.710	1,285
Big Four auditor	0.647	.	.	.	.	1,285
IFRS reporter	0.356	.	.	.	.	1,285
Basel reporter	0.198	.	.	.	.	1,285
Concentration	72.883	18.325	48.410	75.978	96.356	1,285
GDP per capita	13,522	19,105	1,130	6,590	44,394	1,285
GDP growth	9.187	14.867	-7.692	9.722	27.273	1,285

Panel C. Main Regressions

	(1)	(2)
	Loan loss provisions	*Loan loss provisions*
Post × Treatment × Future ΔNPL	0.289***	0.284***
	(2.87)	(2.82)
Post × Future ΔNPL	-0.229**	-0.235**
	(-2.39)	(-2.45)
Treatment × Future ΔNPL	-0.156**	-0.144*
	(-2.09)	(-1.98)
Post × Treatment × Current ΔNPL	-0.012	-0.006
	(-0.12)	(-0.06)
Post × Current ΔNPL	0.042	0.025
	(0.58)	(0.41)
Treatment × Current ΔNPL	0.041	0.035
	(0.60)	(0.51)
Post × Treatment × Lagged ΔNPL	0.047	0.058
	(0.42)	(0.55)
Post × Lagged ΔNPL	-0.105	-0.109
	(-1.09)	(-1.15)
Treatment × Lagged ΔNPL	-0.070	-0.087
	(-0.91)	(-1.23)
Post × Treatment	0.186	0.224
	(0.50)	(0.65)
Post	0.028	0.129
	(0.13)	(0.60)
Future ΔNPL	0.150**	0.143**
	(2.08)	(2.00)
Current ΔNPL	0.070	0.076
	(1.43)	(1.51)
Lagged ΔNPL	0.163**	0.142**
	(2.62)	(2.25)
Earnings ex. LLP	0.097***	0.106***
	(4.47)	(4.66)
Size		-0.073*
		(-1.68)
Capital		-0.026
		(-1.54)
Profitability		-0.031***
		(-4.54)
Loan growth		-0.003
		(-1.06)
Loan intensity		-0.001
		(-0.10)
Interest expense		0.031
		(0.77)
Cost-to-income ratio		-0.010*
		(-1.72)
Big Four auditor		0.013
		(0.07)
IFRS reporter		0.499*
		(1.68)
Basel reporter		0.087
		(0.24)
Concentration		-0.004
		(-0.30)
GDP per capita		0.000
		(1.13)
GDP growth		-0.007
		(-0.59)
Observations	1,285	1,285
Adjusted R-squared	0.513	0.527
Country and Year FE	Y	Y

Panel D. Robustness

	(1)	(2)
	Controlling for pre-treatment trends	Two-year treatment window
	Loan loss provisions	*Loan loss provisions*
Post × Treatment × Future ΔNPL	0.251**	0.242**
	(2.53)	(2.02)
Pre1 × Treatment × Future ΔNPL	-0.054	-0.031
	(-0.48)	(-0.27)
Observations	1,285	1,024
Adjusted R-squared	0.529	0.540
All lower order terms and NPLs	Y	Y
All previous controls	Y	Y
Country FE and Year FE	Y	Y

Table 7. Information Collection and Information Distribution

This table presents results from tests that examine the mediating role of banks' obtaining of information and their learning. Each observation is a bank-year, and all variables are as defined in the Appendix. In this analysis, we replace *Treatment* with *Info distribution*, which is a continuous variable between zero and one, based on the amount of information distributed by the registry to banks. (This variable equals zero for all control banks.) In column (1), we use the main sample. In order to remove confounding effects of registries' collection and distribution of information, the samples in columns (2) and (3) are partitions of the main sample based on the value of an information collection index. Both information collection and information distribution are variables obtained from the World Bank GFDR data (the item codes are *cr_infoc_i* and *cr_infod_i*). These index variables are computed based on whether or not the registry collects/distributes information on 56 different metrics. All lower order terms and NPLs include the components of the triple differences estimators involving future, current, and lagged changes in NPLs, as well as their interactions with *Post* and *Treatment* indicators. All previous controls include the regressors shown in Table 3 and Panel C of Table 6. *T*-statistics (in parentheses) are robust to within-country correlation as well as heteroscedasticity. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)
	Full sample	High-info-collection subsample	Low-info-collection subsample
	Loan loss provisions	*Loan loss provisions*	*Loan loss provisions*
Post × Info distribution × Future ΔNPL	0.071***	0.029*	0.129**
	(2.71)	(1.72)	(2.37)
Observations	1,285	777	394
Adjusted R-squared	0.527	0.555	0.535
All lower order terms and NPLs	Y	Y	Y
All previous controls	Y	Y	Y
Country FE and Year FE	Y	Y	Y