

# Judicial Efficiency and Banks Credit Risk Exposure

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# Judicial Efficiency and Banks Credit Risk Exposure

## Abstract

We exploit the outset of a regulation seeking to improve judicial efficiency through the rearrangement of courts' geography in Italy to provide causal evidence on the relationship between judiciary structural reforms and banks financial stability. To this end, we apply a difference-in-differences approach on a dataset on annual proceedings handled by each court over the period 2010-2017, complemented by banks balance sheet information. Our findings yield a negative effect of the reform on both judicial efficiency and Non-Performing Loans ratio. Furthermore, we identify heterogeneous effects based on the existing capacity of the courts to dispose of pending proceedings and geographical location. Digging deeper into this mechanism, we set up a causal mediation analysis to prove that the judicial system affects banks credit risk exposure both indirectly (through judicial efficiency) and directly, thereby influencing borrowers who react to the perceived enforcement.

JEL-Codes: D040, G210, P430.

Keywords: judicial efficiency, non-performing loans, justice reform, difference-in-differences, mediation analysis.

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# 1 Introduction

Non-Performing Loans (NPLs hereafter) are banks' loans that are defaulted or are close to default. Since the 2008 financial crisis, their evolution has gained more and more importance both in the public and academic debate for the negative influence they exert on systemic stability.

On the debtors' side, NPLs work as a negative signal towards possible new creditors, and might therefore hamper access to external finance and future investment opportunities. On the banking side, banks have to go through court to recover the loss.<sup>1</sup> In the meantime, they have to meet the costs of retaining NPLs in terms of capital requirements that eventually prevent - or limit - their ability to create new finance.<sup>2</sup> Moreover, banks with high ratios of NPLs may suffer from the negative market sentiment which affects their ability to gather interbank liquidity or to access the capital markets in general, thereby weakening their balance sheets by making them less resilient to external shocks (Balgova et al., 2018; Bonaccorsi di Patti and Sette, 2012).

The systemic stability of financial systems is threatened by the accumulation and stagnation of NPLs and hence the reduction of Non-Performing loans has become a priority in the political agenda of the European Union, especially for what concerns macro-prudential policies (European Systemic Risk Board, 2019). In this framework, Italy has been under special surveillance, given the high rate of NPLs damaging Italian banks' balance sheets.<sup>3</sup>

A variety of initiatives has been put in place to tackle the NPLs problem, but there remain several structural impediments, mainly related to the effectiveness of the judiciary to enforce creditors' rights.<sup>4</sup> As institutional economics has extensively demonstrated (Acemoglu et al., 2005; Chemin, 2020; North, 1990) the judiciary is one of the main driver of development, given its ability to foster contract enforcement, preserve property rights, enhance growth and hence limit NPLs' accumulation prospects. More specifically, the efficiency of the judicial and legal system stands among the most important determinants of the dynamics of NPLs (Authority, 2016; Carpinelli et al., 2016; Jappelli et al., 2005; Schiantarelli et al., 2016). A lengthy judicial system may impede contract enforcement and collateral repossession, determining the persistence of NPL stocks in banks' balance sheets. The inefficiency of the judicial system can also

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<sup>1</sup>NPL cases are part of the judicial proceedings on foreclosures and bankruptcy. In particular, for what the latter are concerned, creditors rights (including those attributed to banks) can be claimed once the court has declared the debtor defaulted and established the order of creditors' compensation through the collateral. Therefore, the time elapsed for a bank to collect its credit depends on the duration of these proceedings.

<sup>2</sup>The only alternative to the court consists of securitizing NPLs, even though being granted the right of claiming the credit is very often the best solution. Indeed, even in the case a bank decides to securitize the NPLs, the selling price will be influenced by the probability the purchasing party has to recover the NPL, and therefore it will be indirectly impacted by the relative efficiency of the judicial system. Moreover, following the novelty introduced by Basel III, recent European regulations (REGULATION (EU) No 575/2013; DIRECTIVE 2013/36/EU) oblige banks to back up NPLs with well-specified capital requirements, that eventually hamper their ability to offer finance both to firms and to households thus affecting firms' investment and aggregate growth opportunities.

<sup>3</sup>Even if figures have been steadily decreasing since the burst of the crisis, in the second quarter of 2018, the total NPLs ratio in Italy was 10% well above the ratio in France (2.9%), Germany (1.7%) and Spain (4.1%), but still lower than the one in Portugal (11.7%) or in Greece (44.9%)(European Systemic Risk Board, 2019).

<sup>4</sup>The European Commission presented an Action Plan on which the Council agreed in July 2017; in particular, in March 2018 the Commission proposed a full package of legislative measures to tackle the Non-Performing loans problem to the Parliament and the Council which is now in the pipeline for approval.

have an active role in increasing NPLs levels. Indeed, by hampering contract enforcement opportunities, an inefficient judicial system may incentivize moral hazard in the form of borrowers' strategic defaults. If debtors are aware of the problems their creditors could have in enforcing collateral, they could decide not to repay their loans in a timely manner because they know that the negative consequences of this behaviour would be difficult to materialize (or would materialize further in the future). Therefore, the duration of the foreclosure is associated with a systematic increase of credit risk, encouraging borrowers to intentionally default.

All these reasons have called for several interventions on the part of the European Union that has asked for structural reforms able to tackle the enforcement of creditors rights' problem. In this light, judicial reforms are expected to ease the recovery of collateral offering the possibility for banks to improve their balance sheet and enhance their financial stability, both by favouring the collection of charge-offs loans and discouraging borrowers' moral hazard.

As a matter of fact, a few years after the financial crisis, in 2012, the Italian government implemented an important reform of the judicial system (Judicial System Reform, JSR henceforth) aimed at fostering courts' efficiency and decreasing the average procedures' length. The intervention consisted of merging some courts and abolishing all the local branches (*sezioni distaccate*). As a result, the number of courts was severely reduced from 165 to 140, with 23 courts ending up including one or two courts.

The aim of this paper is to assess whether the JSR has affected NPLs and to explore the mechanism behind these dynamics. Indeed, even though it was not designed to face the banking system's problems, the reform could have had strong consequences on its financial stability, directly tackling the probability of loans' recovery.

The decision about the courts' merger was centrally adopted by the government and was supported neither by a preliminary analysis on the existing level of efficiency of the hosting courts nor by a feasibility study on the potential impact of the merger. Therefore, the reform introduced an exogenous variation in the functioning of those courts interested in the merger, whilst other courts were not affected. This scenario allows us to set a quasi-experimental framework to pursue a threefold objective: (i) disentangle the direct effect of the reform on courts' efficiency, (ii) investigate whether it has affected the stock of NPLs hold by those banks located in the competence area of the treated courts, (iii) assess if the effect is fully mediated by judicial efficiency.

Our results point to a general negative effect of the JSR both on courts' efficiency and on banks' NPLs ratio. The effect is heterogeneous with respect to pre-reform levels of courts' efficiency: in particular, the most efficient pre-reform courts seem to be those most negatively affected. On the banks' side, those operating in the catchment area of the treated courts that were already less efficient have been negatively affected more. Furthermore, the effect on judicial efficiency is comparable in magnitude across Italian Nuts 1, whereas a more marked differential increase in NPLs ratio is observed for the Centre and the Southern part of the country. Exploiting these differential dynamics, we explore the mechanisms underpinning the impact of the reform on NPLs. To this end, we rely on mediation analysis and show that indeed judicially efficiency

only partially mediated the effect of the JSR on NPLs and that other factors might have been at work. In particular, we offer evidence supporting the claim that the reform prompted borrowers' strategic defaults, thereby increasing NPLs.

Our paper builds on the literature addressing the impact of judicial efficiency on firms' access to credit going deeper into the analysis of the channels whereby variations in judicial efficiency propagate into the credit market. Indeed, several papers have examined how the legal environment, and, in particular, creditors rights enforcement, may influence firms' access to credit (Bae and Goyal, 2009; Fabbri, 2010; Haselmann and Wachtel, 2010; Jappelli et al., 2005; Moro et al., 2018) or firms' outcomes in general (Giacomelli and Menon, 2013; Laeven and Woodruff, 2007; Ponticelli and Alencar, 2016). Yet, they fell short in accounting for the role played by NPLs, which is considered a crucial determinant through which this relationship could materialize. In a recent contribution, D'Apice et al. (2020) have stressed the importance of judicial efficiency in tackling NPLs accumulation, albeit focusing on "contract enforcement" reforms which occurred in Austria, Belgium, Norway and Sweden between 2007 and 2008. Their findings confirm the positive effect that a more efficient judicial system has on NPLs stock, leveraging on both faster recovery rates and deterrence of borrowers' opportunistic behaviour. However, endogeneity issues still prevent researchers from digging further into the interaction between judicial efficiency and NPLs (Aboal et al., 2014). In this respect, our analysis offers a novel empirical contribution to the literature, exploiting the enforcement of the 2012 Italian JSR as a quasi-natural experiment that enables us to get rid of endogeneity and come up with causal results.

A paper that is very close to ours in offering causal estimates is Chemin (2009), which evaluates the effect of the Pakistani 2002 judicial reform on entrepreneurship. The author implements a two-stage analysis, looking first of all at the impact of a training to judges on judicial efficiency and then at the impact on entrepreneurship. Following Chemin (2009), we assess the impact of the reform on both judicial efficiency and NPLs ratio on two independent levels. We then go further into the analysis and we look into the mechanism by which the JSR exerts its influence on banks credit risk in a causal mediation setting (Imai et al., 2010b; Imai and Yamamoto, 2013; Pieters, 2017; Robins and Greenland, 1992). Besides, we assess how these effects heterogeneously vary across several dimensions and we then seek to disentangle the effect on NPLs mediated by judicial efficiency from other potential mechanisms. Our findings confirm the significant role played by the judiciary system in facing NPLs accumulation and stagnation and offer interesting insights on the existence of concurrent determinants. The remainder of the paper is organized as follows. Section 2 describes the institutional background and judicial efficiency measure. Section 3 briefly presents the dataset, whereas Section 4 discusses the empirical strategy. Results are shown in Section 5 and the robustness checks and heterogeneity are given in Section 6 and Section 7, respectively. Section 8 explores the mechanisms in a causal mediation framework. Section 9 concludes.

## 2 Institutional background

### 2.1 The territorial organization of the Italian judiciary system

The Italian judiciary system is structured in *corti d'appello* or *distretti giudiziari* - hereafter district - that coordinate and exert influence on the judicial activity of several *circondari di tribunale ordinario* - hereafter court - where a public prosecutor's office operates.<sup>5</sup> Before the reform, the activity of each court was supported by local branches located in the court's reference area. As for the territorial organization, each regional county seat has its own district and each province has at least one court.<sup>6</sup>

### 2.2 The 2012 judicial reform

The discussion about the Italian judicial reform started in 2011. The first goal of the intervention was the improvement of the efficiency of Italian courts: as the former Minister of Justice declared "The number of proceedings managed by very small courts is well under the efficiency threshold, and this, among other things, prevent them from specialize and become more efficient". Moreover, the vast majority of the local branches was established in an emergency situation, and they were not suppressed once the urgent situation was solved. Therefore, most of them had no reason to be still active.

The law proposed the suppression of 45 courts and their merge with bigger courts, and the closure of all the local branches. Not only this would have generated an increase in efficiency through courts' specialization, but also a considerable amount of public money saving by preventing judicial buildings' maintenance expenses.

The Government decided to use few incontrovertible criteria to choose which courts should be interested by the reform: population of the area referring to the court; number of incoming proceedings; number of employees of the court and the workload. All the courts laying below the average of these parameters should have been closed. On top of these factors, the infrastructure endowment, the rate of organized crime and the minimum dimension of the served area (corresponding to 200.000 inhabitants) were also taken into account.<sup>7</sup> Eventually, the reform determined the full closure of all the 220 local branches and 26 courts out of 165. Those chosen as hosting courts absorbed all the magistrates and the administrative personnel of the

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<sup>5</sup>Nowadays there are 26 districts - that were not interested in the revision of the judicial geography - and 140 courts (139 left by the JSR and a newly established one-Napoli Nord).

<sup>6</sup>Some regions count more than one district, like for example Lombardy, which has the district of Milan and the district of Brescia, or Sicily with districts in Caltanissetta, Catania, Messina and Palermo.

<sup>7</sup>More specifically, averages were computed over the existing courts located in a provincial seat to identify the thresholds for each criteria. The average population, number of incoming, number of magistrates and workload were estimated equal to 363.769 inhabitants, 18.094 proceedings, 28 magistrates, 638.4 workload respectively. 45 courts complied with the requirement of having actual figures below *all* the thresholds and were thus considered eligible. The need to maintain at least 3 of the actual courts for each district let 8 courts to be excluded *a priori* from the intervention. 6 courts more were exempted from the reforms because the policymaker did not want to penalize areas with high intensity of criminal activity by abolishing the reference court. Finally, for 5 courts, all located in Abruzzo region, the outset of the JSR was postponed to 2022 due to an emergency situation in the aftermath of the earthquake.

suppressed courts without any job loss.<sup>8</sup> For the sake of our analysis, it is important to observe that the level of efficiency of the court at the time of the reform was not considered among the criteria to inform the decision, suggesting that the reform was possibly exogenous to courts' efficiency.

The law was formally implemented in 2012, and even if some months were granted to courts to adopt the changes, most of the units interested by the reform started the restructuring process immediately so that by 2013 the new judiciary geography was fully in place.

In September 2013, the Ministry of Justice established a working group aimed at monitoring the implementation of the reform identifying all the circumstances which may have hampered a positive deployment of the intervention.<sup>9</sup>

The committee analysed the reports produced by absorbing courts, and interviewed several of their Presidents. Many delegates from the absorbing courts declared that thanks to the incoming magistrates they have been able to operate field specialization in managing the cases and therefore they have succeeded in increasing courts efficiency. Nonetheless, many others reported several problems that could have prevented the reform from being effective. Among these, a prominent role was played by infrastructure endowment and the lack of administrative personnel. As for the infrastructures, a rather common problem was the lack in absorbing courts of adequate physical space to host the employees of the suppressed one as well as its judicial catalogues.

Most importantly, what was missing were courtrooms where to celebrate the hearings. As an example, the President of the court of Vicenza, that absorbed the court of Bassano del Grappa, lamented that because of the lack of spaces many first hearings were postponed by a year, whilst bankruptcy and foreclosures activity was completely blocked.

Although quite severe, problems related to infrastructure were signalled by a handful of courts. A more widespread difficulty mentioned by the vast majority of interviewed courts regarded the shortage of administrative employees, which were considered insufficient to carry out the activity of the merged unit. Indeed, this is a long-lasting problem affecting the Italian judicial system: starting from the 90s, the number of administrative headcount within judiciary courts has continuously shrunk going from 52.530 units in 1993 to 36.194 units in 2014. In addition to this chronic situation, the reform gave the possibility to abolished courts' employees to ask for a relocation in a court different from the absorbing one. Consequently, in some cases, the total number of employees of the absorbing court was lower than the sum of the two workforces, whilst the number of proceedings to be managed has consistently increased. Interestingly, the issue was critically mentioned also by those courts that reported a successful transition into the new system.

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<sup>8</sup>The magistrates and the administrative personnel of the suppressed courts had either the possibility of joining the absorbing court maintaining their job description, or they could have asked for relocation in another court.

<sup>9</sup>According to the law implementing the reform, in case particular negative issues were raised through the monitoring, the Government would have had the possibility to introduce corrective measures.



### 2.3 How to measure judicial efficiency

Courts' efficiency is assessed resorting to the index of duration developed by the Italian National Bureau of Statistics which is based, in turn, on pending and closed proceedings and measures efficiency as the number of days necessary to close a proceeding (Efficiency Index–EI):

$$EfficiencyIndex_{it} = \frac{Pending_{it-1} + Pending_{it}}{Incoming_{it} + Closed_{it}} * 365 \quad (1)$$

where  $Pending_{it-1}$  represents the number of hanging proceedings at the beginning of period  $t$ , that is, the number at the end of the previous period;  $Pending_{it}$  is the number of unsolved cases at the end of period  $t$ ;  $Incoming_{it}$  is the amount of new incoming proceedings in period  $t$  and  $Closed_{it}$  is the volume of resolved procedures in period  $t$ . The higher the index is, the lower the level of efficiency. For the sake of our analysis, we focus only on proceedings dealing with arrangement with creditors, bankruptcy and foreclosures that are the field where banks' credit rights could be framed.

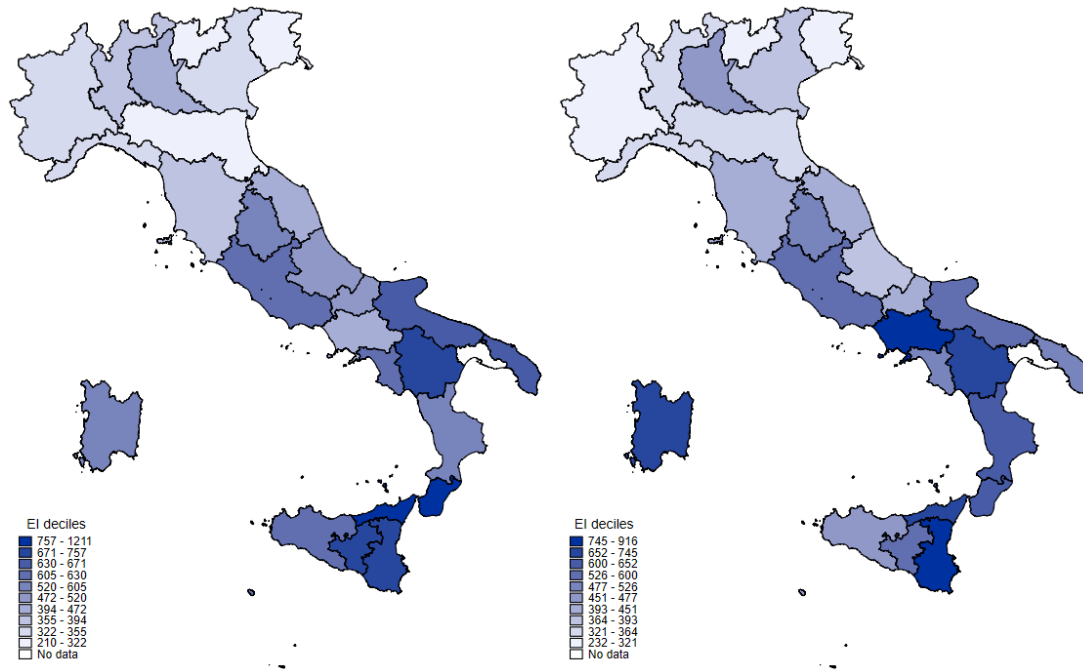
Table A.1 provides some figures on the average efficiency and the size of the whole sample of Italian courts in the period before the reform (2011); it also presents the descriptive statistics disentangled by the three groups of courts targeted by the reform: unaffected, absorbing and dropped ones. In particular, the table suggests that, on average, dropped courts were not necessarily those characterised by longer proceedings as they exhibit an average duration of about 432 days, which is lower than the absorbing courts (505 days) and the full-sample average (476 days). As far as the size in terms of incoming proceedings, we observe that dropped courts are more likely to be smaller (1143 new proceedings on average) than the absorbing (4303 proceedings) and not affected ones (5607 proceedings).

Figure 1 maps the geographical distribution of the average Efficiency Index across Italian judicial districts before (Panel a) and after (Panel b) the implementation of the JSR. Dark-shadowed areas stand for higher values of the EI, hence signalling longer justice duration. The maps highlight the existence of marked differences in the duration of judicial proceedings between Italian geographical areas, with a better performance of the North compared to the South. The figure provides also a suggestive visual inspection of the before-after comparison in the EI, indicating a generalised deterioration of courts' performance with only a few exceptions. To dig deeper into the impact of the JSR on justice functioning, accounting for observed and unobserved factors which might intervene, we propose a more accurate and nuanced analysis in the following sections.

To provide some hints on how NPLs ratio has evolved over time either across geographical areas or judicial efficiency, Figure 2 plots the distribution over the analysed period of NPLs ratio by Italian Nuts 1 (Panel a) and by quartiles of Efficiency Index (Panel b). The vertical whiskers connect the upper and the lower adjacent values; whilst the thick black line, in each box, is the median. In Panel a, the vertical grey boxes are drawn by year and geographical areas, and we observe an increasing trend in NPLs ratio in all the areas up until 2015, followed by a tiny decrease in the last two years. Interestingly, NPLs ratio had comparable values across

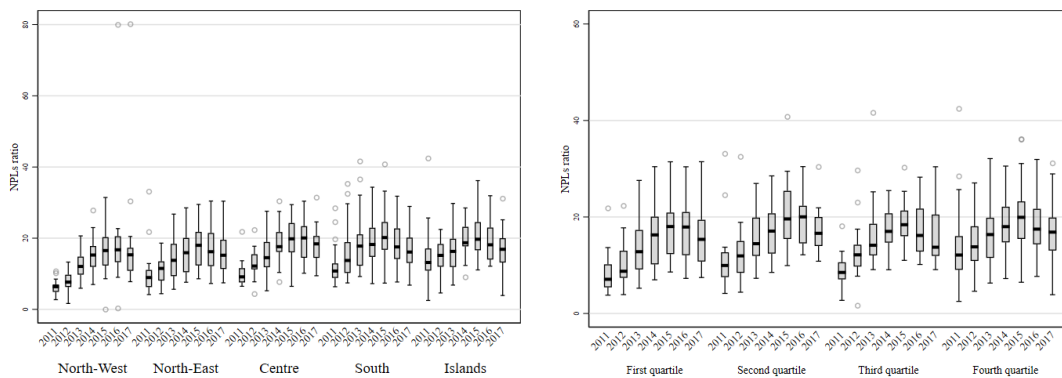
Italian Nuts 1 regions, albeit less dispersion and slightly lower levels are found for the North-West. As far as the efficiency is concerned, it comes out that no marked differences emerge when considering the four groups. Yet, we observe a trend that mimics that of the previous graph with a slight decrease after 2015.

Figure 1: Efficiency Index map: pre and post JSR implementation



Note: Panel a (left): Before JSR. Panel b (right): After JSR. The maps plot the deciles of the EI distribution. Dark-shadowed areas stand for higher values of the Efficiency Index.

Figure 2: NPLs distribution by area and efficiency quartiles



Note: Panel a (left): NPLs ratio by Italian Nuts 1. Panel b (right): NPLs ratio by quartiles of Efficiency Index. Vertical whiskers connect the upper and the lower adjacent values; whilst the thick black line, in each box, is the median.

### 3 Data

To define the Efficiency Index we rely on information retrieved from the Bureau of Judicial Statistics (hereafter DG stat) of the Ministry of Justice. The DG stat database provides yearly information at the court level on incoming, closed and pending proceedings starting from 2008. The information is first grouped into civil justice and criminal branches. Under the civil justice framework, data on proceedings are distinguished by field of judiciary interventions, namely, ordinary jurisdiction, labour, arrangements with creditors, foreclosures and bankruptcy.<sup>10</sup>

The information on the stock of NPLs was gathered from Bankscope, a Bureau Van Dijk database, which contains balance sheet information for about 30.000 public and private banks worldwide. For Italy, the coverage of the dataset is good and we have been able to gather information for a panel of almost 730 banks over the period 2011-2017.<sup>11</sup>

We finally linked the two datasets using the municipality zip code as a key variable. The resulting database counts 722 unique identifiers. This information is then collapsed at the court level to harmonise data over the two steps of the analysis, ending up with an unbalanced panel of 803 observations.

### 4 Empirical strategy

Our analysis seeks to estimate, in the first instance, the causal impact of the JSR on courts' Efficiency Index and, in a second step, to measure the causal impact of the reform on the NPLs ratio. In both cases, the identification strategy is based on a difference-in-differences model, in which we compare the difference in the outcome - either the Efficiency Index or the NPLs ratio - of treated and control courts, before and after the outset of the reform.

However, our policy set up does not lend itself to easy identification of the treated and the control units. Indeed, abolished courts, albeit being directly targeted by the reform, could not serve as treated units because they are observed in the period before the reform, but disappear from the registries after 2013. It is not possible either to simply define the absorbing courts as the treated ones, because the reform changed them in a structural way, making them two different entities before and after the intervention.

We overcome this issue taking inspiration from an empirical approach widely adopted in contributions studying municipalities' mergers (Blesse and Baskaran, 2016; Blom-Hansen et al., 2016; Reingewertz, 2012). To define our treated units in the pre-reform period, we take the absorbing courts, and we artificially build their respective pre-intervention unit by summing-up each of them with the respective absorbed court. The resulting will be our treated units, which

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<sup>10</sup>It has to be noted that upon the judicial reform, DG stat changed the way in which it had been classifying information until 2013. In particular, the "arrangement with creditors" field was reclassified under the bankruptcy field. We, therefore, harmonized previous data by adding the "arrangements with creditors" proceedings to the bankruptcy ones.

<sup>11</sup>The goodness of Bankscope coverage can be evaluated in terms of the ratio total assets in Bankscope over the aggregate size of the Monetary and Financial Institutions - provided by the European Central Bank. The coverage of the database is considered good when the ratio is close to one (Duprey and Lé, 2016).

correspond to the sum of the absorbing and the absorbed courts in the pre-implementation period, and to the absorbing court in the period after the implementation. In other words, the treatment is defined as having included an abolished court, and what we are seeking to test is whether this merger has affected the receiving courts' efficiency and, in turn, improved the possibility for the banks referring to that court to enforce their rights as creditors.

For what courts' efficiency is concerned, the impact of JSR is measured by estimating the following equation using OLS:

$$\ln(EI_{itd}) = \alpha_3(T_i \times Post_t) + f_i + f_t + f_d + u_{itd} \quad (2)$$

$\ln(EI_{itd})$  is the efficiency indicator (in log) for court  $i$  in the judicial district  $d$  in year  $t$ ,  $T_i$  is a binary variable equal to 1 for the courts that had to include another court;  $Post_t$  is a binary variable that is equal to 1 after the implementation of the JSR.  $f_i$  and  $f_d$  are courts and district fixed effects, accounting for those time-invariant unobserved factors which are specific to each court or judicial district (such as structural endowment and facilities), respectively.  $f_t$  are year fixed-effects and allow capturing any time-variant unobserved heterogeneity common to all courts, such as macroeconomic shocks and political cycles.  $u_{itd}$  is a stochastic error term. Finally, estimates are obtained using the court-specific number of judicial officers in the pre-JSR period to weigh observations. This allows to take into account the courts' pre-reform capacity of coping with the flow of proceedings as an element that could possibly impact on courts' efficiency. Standard errors are clustered at the court level.

The second step of the analysis is meant to verify whether the reform not only impacted on courts' efficiency but if it also spread out its effect to banks, influencing their ability to recover credits after firms' default. This allows quantifying the causal impact the JSR had on NPLs ratio. The baseline equation becomes:

$$NPL_{itd} = \beta_3(T_i \times Post_t) + f_i + f_t + f_d + u_{itd} \quad (3)$$

where  $NPL_{itd}$  is the average NPLs ratio (i.e. the amount of NPLs retained by the  $b$ -th bank on the total gross loans granted by the same bank in period  $t$ ) for banks operating in the catchment area of court  $i$  in district  $d$ . The baseline model includes the same battery of fixed effects described above ( $f_i$ ,  $f_d$  and  $f_t$ ).<sup>12</sup>

To account for the differences in NPLs that might originate from a different size of the economic activities, we weigh observations according to the number of businesses operating in the catchment area in the pre-intervention period, i.e. 2008. In other words, a large NPLs ratio in a very active economic area should be interpreted differently from a similar NPLs ratio in a poor or depressed area. Furthermore, we control for the number of banks operating in the competence area of the court to avoid any bias due to a different size of the banking sector in

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<sup>12</sup>The terms  $(\alpha_1 T_i + \alpha_2 Post_t)$ , in Eq. 2, and  $(\beta_1 T_i + \beta_2 Post_t)$ , in Eq. 3, are omitted, as they are collinear with  $f_i$  and  $f_t$ , respectively.

a certain territory.

Parameters  $\alpha_3$  in Eq. 2 and  $\beta_3$  in Eq. 3 offer a first insight on the identification of the causal impact of the reform on either judicial efficiency or NPLs ratio.

Besides, we refine our estimates by augmenting the baseline models with district linear trends ( $f_d \times t$ ) that account for those aspects managed by the judicial district which change smoothly over time. Finally, to take into account factors common to each region in a given year, such as regional economic fluctuations and regional time-specific shocks, we include also region-by-year fixed effects ( $f_{rt}$ ). The validity of our results is then proved in Section 6.

## 5 Main results

Table 1 presents the baseline results on the impact of JSR on courts' efficiency resulting from the empirical specification described in Eq. 2. In particular, the effect of interest is captured by  $\alpha_3$  as it quantifies the percentage change in the Efficiency Index of treated units with respect to control ones, brought about by the implementation of the JSR. Since the Efficiency Index informs about the number of days needed to close a proceeding, a positive coefficient signals a decrease in efficiency.

Table 1: Impact of Judicial Reform on the Efficiency index. DiD baseline estimates

|                     | (1)               | (2)               | (3)               |
|---------------------|-------------------|-------------------|-------------------|
|                     | ln(EI)            | ln(EI)            | ln(EI)            |
| $T_i \times Post_t$ | 0.089*<br>(0.049) | 0.077*<br>(0.042) | 0.103*<br>(0.056) |
| Observations        | 1,057             | 1,057             | 1,049             |
| $R^2$               | 0.79597           | 0.83179           | 0.84300           |
| District lin trend  | N                 | Y                 | Y                 |
| Region-year FE      | N                 | N                 | Y                 |

Note: Robust standard errors clustered at the court level in parentheses. Across columns, all the models are implemented controlling for year, court and district fixed effects. Observations are weighted according to the number of magistrates working within the court.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Estimates in Column 1 are retrieved implementing the baseline DiD, while in Column 2 we introduce district-specific linear trends in order to control for specific time-variant heterogeneity at the judicial district level. Indeed, territorial courts are managed by regional judicial districts that set up the organizational strategy and the availability of funds for the different courts. No information is available - at the territorial level - on this strategy or on the specific amount of funds assigned to each court. Nonetheless, this could be an important aspect influencing courts' efficiency, hence we further refine our main specification controlling for judicial district-specific time trends, assuming they vary smoothly over time. Finally, Column 3 augments the previous specification with region-year FE that helps us accounting also for regional specific time shocks

- regional elections as well as macroeconomic shocks.

The three models return estimates that are almost constant in magnitude, positive and statistically significant suggesting a negative impact of the JSR on treated courts' efficiency. In particular, focusing on our preferred specification in Column 3, the Efficiency Index increased by almost 11%<sup>13</sup>, meaning that, in the aftermath of the reform, treated courts took approximately 38 days more to close a proceeding with respect to untreated ones.

So far, results from Table 1 suggest that not only did not the reform succeed in increasing courts' efficiency, but apparently it worsened the situation.

The failure in accomplishing the stated goal could have had a negative influence on NPLs. Indeed, as we mentioned in Section 1, a bad performing judicial system might hamper the capability of banks to enforce their rights as creditors, yielding to an increase in NPLs ratio. To empirically assess whether this has been the case for the Italian reform, we run the model in Eq. 3 and present results in Table 2.

Table 2: Impact of Judicial Reform on NPLs. DiD baseline estimates

|                     | (1)              | (2)               | (3)                |
|---------------------|------------------|-------------------|--------------------|
|                     | NPLs ratio       | NPLs ratio        | NPLs ratio         |
| $T_i \times Post_t$ | 2.151<br>(1.369) | 2.275*<br>(1.300) | 3.709**<br>(1.424) |
| Observations        | 808              | 808               | 794                |
| $R^2$               | 0.79393          | 0.80065           | 0.82815            |
| District lin trend  | N                | Y                 | Y                  |
| Region-year FE      | N                | N                 | Y                  |

Note: Robust standard errors clustered at the court level in parentheses. Across columns, all the models are implemented controlling for year, court and district fixed effects. Observations are weighted according to the number of businesses operating in the catchment area in 2008.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Here, the DiD coefficient -  $\beta_3$  - informs about how many percentage points the average NPLs ratio within the catchment area of treated courts has varied with respect to that referring to untreated courts.<sup>14</sup>

Across the three columns of Table 2, we use the same specifications as in Table 1 except for controlling here for the number of banks located in the area.<sup>15</sup> All the specifications return a positive coefficient, albeit more precisely estimated only when accounting for district trends and regional-by-year confounders. According to the coefficient of interest, banks located in the catchment area of treated courts experienced a rise in the NPLs ratio equal to almost 3.7 percentage points, with respect to banks in non-treated areas.

<sup>13</sup>The effect magnitude of a one-unit change in the outcome in a log-linear model is calculated as  $(e^{\alpha_3} - 1)$ .

<sup>14</sup>Table 2 reports the WLS estimates over an unbalanced panel. Findings hold also when the model is estimated over the balanced sample. Results are here omitted and available upon request.

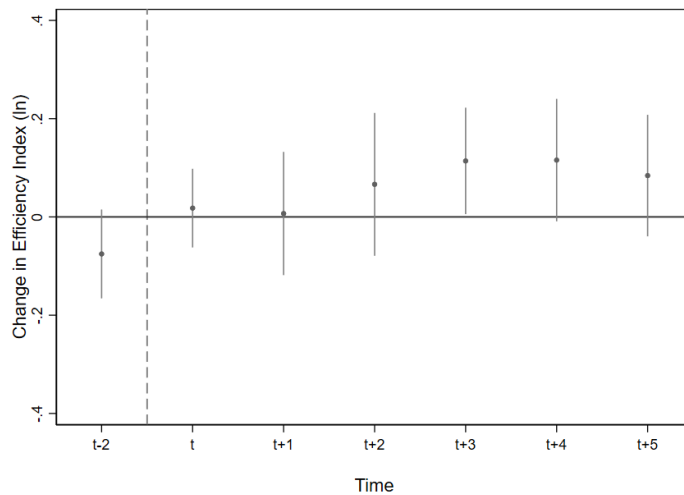
<sup>15</sup>Indeed, a high number of banks could be associated with a higher probability of reporting NPLs.

## 6 Robustness

The DiD estimates presented in the previous section are informative about the causal impact of the reform on the Efficiency Index and on NPLs only if we can prove that the outcome before the intervention followed similar paths across the treated and control groups (*Parallel Trend assumption*). We test the validity of this assumption by implementing an event study *a la'* Autor (Autor, 2003). In particular, we slightly modify Eq. 2 introducing leads and lags terms which identify the years before and after the implementation of the reform, respectively. Regressions are then run using the year before the reform as the baseline.

We graphically present the results of the test in Figure 3.<sup>16</sup> Data at hand allows considering two years before the implementation and five years after (until 2017). Setting the year before the outset of JSR ( $t-1 = 2011$ ) as reference year, we are able to test the absence of any anticipation effect considering only one lead ( $t-2$ ).

Figure 3: Testing the Parallel Trend assumption. Event study on the Efficiency Index



Note: The vertical dashed line identifies the reference year (2011). Black dots are the points estimates of the effect of the JSR in each year; the vertical lines represent the respective 90% confidence intervals. Regressions include fixed effects and controls as from Table 1, col.3.

A statistically significant coefficient associated with the lead term might point out that anticipatory effects were already at work and hence that the outcome for the two groups was already on divergent paths before the enforcement of the intervention. Quite reassuringly, the coefficient associated with the  $t-2$  term is not statistically significant and therefore we are prone to accept the validity of our identifying assumption. Other than testing the validity of the Parallel Trend assumption, the event study analysis allows investigating the dynamics of the policy effects over time. The Figure suggests that the JSR has not had a significant detrimental effect in the aftermath of its approval. We observe instead that its impact has taken some time to materialise, as the coefficients become positive two years after the implementation, even

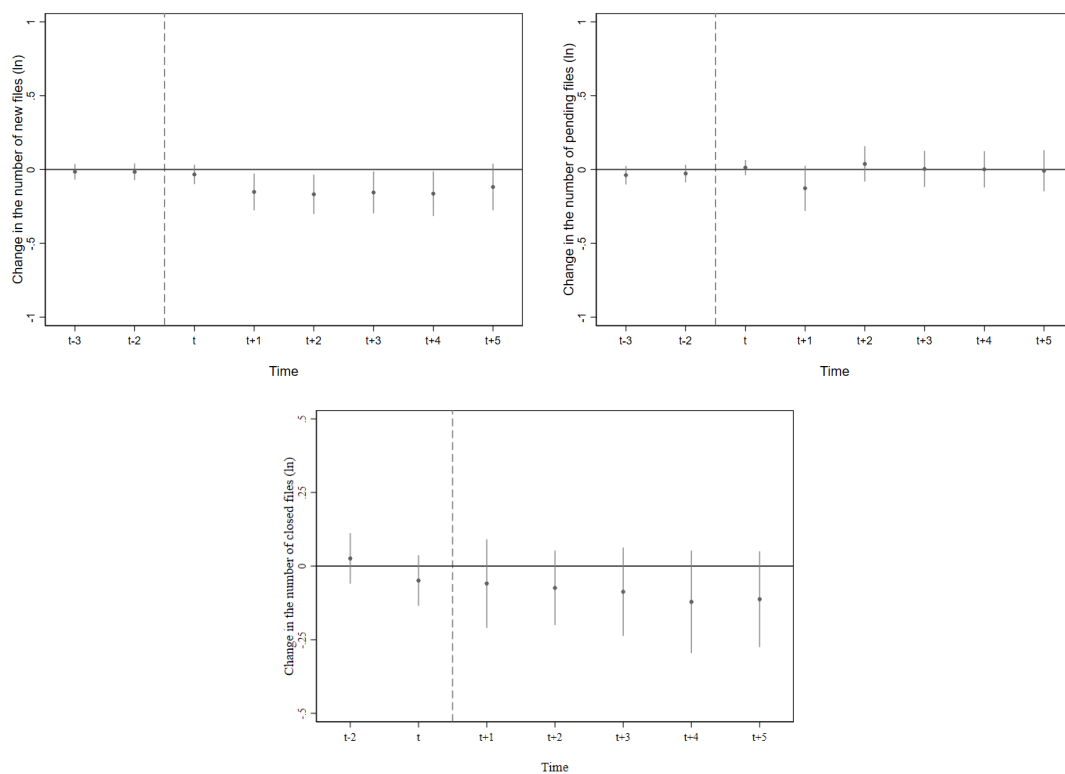
<sup>16</sup>Results are also reported in Table A.2 in the Appendix.

though they are significant at the conventional level only three and four years after. This is in line with the idea of a “bottleneck” effect that might block up courts’ activities, increasing the average time needed to close files.

Controlling for a single lead term, however, might not fully prove the absence of divergent trends before the implementation. Therefore to further substantiate this hypothesis, we turn to analyze the single components of the Efficiency Index, namely the series of the Incoming, Closed and Pending proceedings. For these series, we have one year more available and hence we can test the assumption on a slightly longer time period.

Figure 4 depicts the results obtained by the implementation of the same event study described above, but with an additional lead term ( $t-3$ ).<sup>17</sup>

Figure 4: Testing the Parallel Trend assumption. Event study on the Incoming, Closed and Pending files



Note: The vertical dashed line identifies the reference year (2011). Black dots are the points estimates of the effect of the JSR in each year; the vertical lines represent the respective 90% confidence intervals. Regressions include fixed effects and controls as from Table 1, col.3.

Again, the analysis lends support to the existence of common trends since the two lead terms are not statistically different from zero in each of the three series. By looking at the dynamics after the JSR, it comes out that the number of new incoming proceedings has slightly decreased, the number of pending files remained virtually the same and the number of closed files has also declined even though it is not statistically significant. These findings suggest that the reform

<sup>17</sup>The event study on efficiency components is also shown in Table A.2.

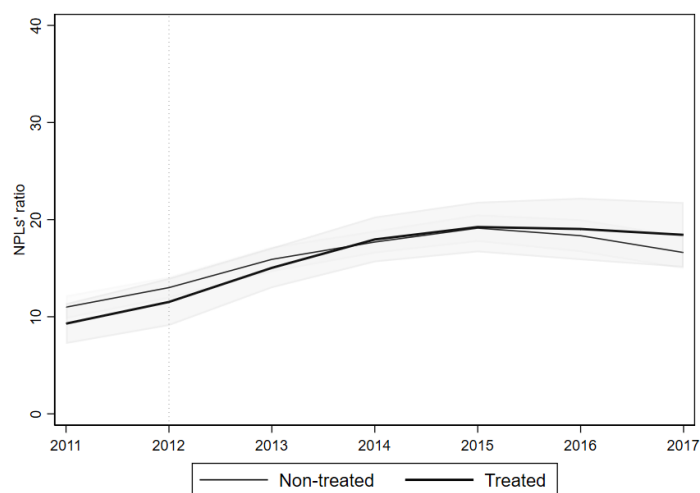


has not brought about an increase in the workload of treated courts, but it has worsened their capacity to efficiently process incoming cases.

To complement the event study analysis described above, Figure A.1 and A.2 plot the trend for the Efficiency Index and its components, respectively, over the period 2010–2017 (starting in 2011 for the Efficiency Index). For the EI we observe that the two lines are not statistically different in the pre-reform period, as the confidence bands overlap. Interestingly, two years after the JSR the line of the treated courts overtakes the untreated, ending up with an EI on average higher. The two lines were parallel in the pre-reform period also for the incoming, pending and closed proceedings.

As far as the NPL outcome is concerned, the event study *à la Autor* is not applicable. Bankscope data is available for Italy from 2011 and this impedes to apply this test to validate the *Parallel Trend* assumption as we cannot rely on a sufficient number of pre-implementation periods. Notwithstanding this, Figure 5 offers suggestive evidence for the absence of significant differences before 2012, through a simple graphical representation of the NPLs trends for the treated and the control groups.

Figure 5: Testing the Parallel Trend assumption. Trends in NPLs series



Note: Raw averages by year. The thick solid lines represent the average values for the treated courts; the thin dashed lines refer to the control courts. The gray areas represent the respective 95% confidence intervals.

Besides assessing the validity of the identification assumption, we also successfully test our results against the hypothesis that estimates could be triggered by some courts rather than others, and to this end we run our preferred specification excluding courts one-by-one (Figure A.3) for both Eq. 2 and Eq. 3. Both figures show that results are not sensitive to the exclusion of a single court.

## 7 Heterogeneity

We have shown before that the JSR had a negative impact on courts' efficiency and on banks' Non-Performing loans. Moreover, our analysis demonstrated that courts treated by the reform experienced a deterioration in the capacity to timely dispose of their workload. This is true especially for what concerns incoming proceedings and exerts a detrimental effect also on the credit risk of banks operating in their catchment area. Here, we zoom on the analysis of how the effect differentiates with respect to some features of the courts' activity and their location. Indeed, it is worthwhile investigating whether the impact of the reform has been heterogeneously different according to the pre-existing level of efficiency of a court and to its operational capacity to finalize proceedings. To this end, we consider the Replacement Index or Rotation Index (RI) as a proxy of the existing level of efficiency of a court.

The RI is given by the ratio between pending proceedings in the period before ( $t-1$ ) and pending proceedings at time  $t$ . The index has the advantage of an easy interpretation, as an RI higher than 1 means that the court is progressively disposing of pending proceedings, therefore it is efficient; conversely, an RI lower than 1 is signalling that the court is not able to dispose of pending cases, hence it is not efficient.

In order to account for this court's heterogeneous level of "disposal capacity of pending proceedings" we interact our treatment variable with the RI in year 2011.

In this setting, Eq. 2 becomes:

$$\ln(EI_{itd}) = \alpha_3(T_i \times Post_t) + \alpha_4(Post_t \times RI_i) + \alpha_5(T_i \times Post_t \times RI_i) + f_i + f_t + f_d + u_{itd} \quad (4)$$

where the term  $(T_i \times Post_t)$  is the differential effect in EI of those courts that were treated by the reform with respect to the untreated, and  $(T_i \times Post_t \times RI_i)$  is the heterogeneous effect that varies according to the existing level of RI. For the analysis on NPLs ratio, the interactions with  $RI_i$  (namely,  $Post_t \times RI_i$  and  $T_i \times Post_t \times RI_i$ ) are instead added to Eq. 3.<sup>18</sup>

In this framework, the effect is given by the first derivative of Eq. 4 with respect to the treatment  $(T_i \times Post_t)$ , that is, the following linear combination:  $\lambda = \alpha_3 + \alpha_5 \times RI$ .  $\lambda$  assumes different values for different levels of the rotation index.

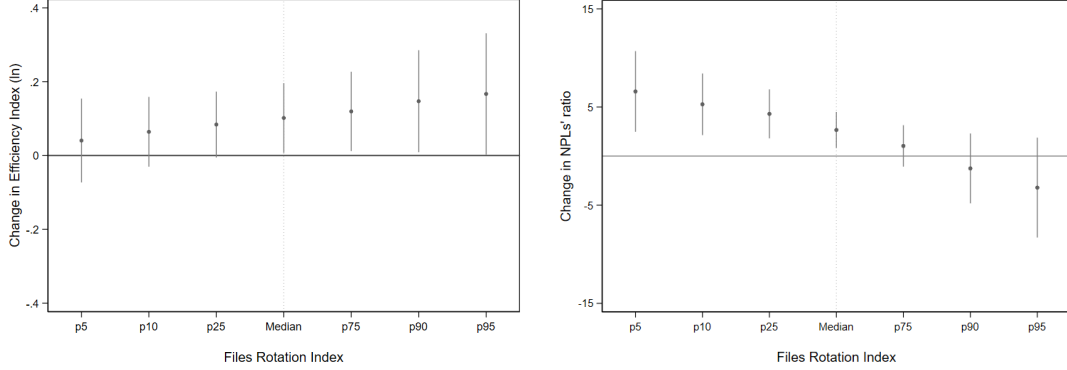
To ease the interpretation of the results, Figure 6 plots the point estimates for  $\lambda$  and the respective confidence intervals for several values of the rotation index, identified by the percentiles of its distribution. The vertical dashed line represents the median value, which in both cases is equal to 1. Hence, for values lower than the median we are considering those courts that were less able to dispose of pending cases, whilst for values higher than the median we are looking at courts that were more efficient. Results on the EI (Panel a) show that the JSR has negatively affected more those courts that were previously more efficient. Conversely, we got the opposite result when we consider NPLs ratio (Panel b). Indeed, the reform had a positive differential effect (i.e increase in NPLs ratio) for those banks operating in the catchment area of courts that were already performing badly. Surprisingly, we identify a reduction in NPLs ratio for those

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<sup>18</sup>Collinear terms  $T_i$ ,  $Post_t$ ,  $RI_i$  and  $(T_i \times RI_i)$  are here not reported.

banks operating in the competence area of courts with a higher RI.

Figure 6: Impact of the JSR on the Efficiency Index and NPLs'ratio. Heterogeneity with respect to pre-reform Rotation Index



Note: The horizontal axis reports the percentiles of RI in 2011. The vertical dashed line identifies the median value. Black dots are the points estimates for the respective level of RI; the vertical lines represent the 90% confidence intervals. Regressions include fixed effects and controls as from Table 1, col.3.

Given the well-known regional gaps that characterise Italy on the social and economic profile, another aspect that might play a non-negligible role in determining a heterogeneous effect is the location of the courts.

To dig deeper into this aspect, we assess the differential impact of the JSR in relation to the Nuts 1 region where the court is located.<sup>19</sup> Those areas are characterised by a historical economic gap, with the Southern regions and the Islands (the so-called *Mezzogiorno*) lagging behind in virtually all the development dimensions. To implement this analysis, we augment our models (Eq. 2 and 3) with the treatment interacted with a categorical variable identifying the Nuts 1:  $(T_i \times Post_t \times Nuts1_i)$ .

The treatment effect is again given by the linear combination of the coefficient associated to treatment with the one of this triple interaction. The effect varies for different categories of the variable  $Nuts1_i$  and is plotted in Figure 7 for both EI (Panel a) and NPLs (Panel b).

When considering the Efficiency Index, we observe no differential impact related to the geographical location of the courts. In the case of NPLs, findings are instead more nuanced. The effect is not statistically different from zero for the North-West of the country, whilst it is always positive and significant for the other areas. In particular, we notice that for the South and Islands the change in NPLs ratio is about 5 percentage points.

These findings provide interesting insights into the existence of a factor that might affect NPLs through JSR, in addition to judicial efficiency. Indeed, if only judicial efficiency was the channel for the reform to affect NPLs, we would have observed a similar pattern in the two figures. Conversely, what we observe is that regional characteristics do not matter in shaping the JSR impact on EI while they do have a significant role in what NPLs are concerned.

<sup>19</sup>Italy has five Nuts 1 regions: North-West, North-East, Centre, South and Islands.

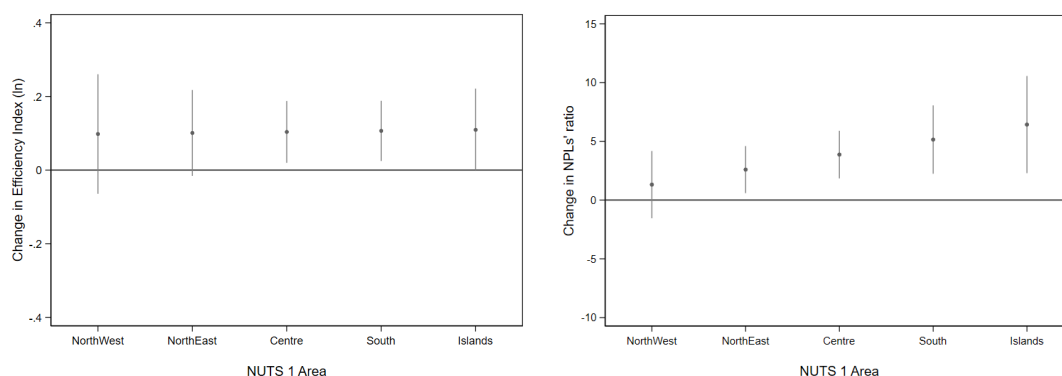
In line with the existing literature, this regional determinant could be related to borrowers who might gerrymandering behave to avoid loans repayment. How this could relate to the geographical differential effect can be explained considering the widely documented Italian regional disparities, with several studies proving that Mezzogiorno regions are characterised by a lower level of social capital and lower control of corruption (Nifo and Vecchione, 2014), hence they might be more exposed to unfair practices. In this context, the perceived effectiveness of JSR could have ignited loans repayment misbehaviour.

## 8 Exploring mechanisms

The heterogeneity analysis has offered shrewd intuitions in exploring the channels that might generate a differential effect exerted by the JSR. In particular, the analysis has shed some lights on the existence of concurrent factors, beside courts' efficiency, through which the judiciary system affects the ability of banks to recover NPLs.

As previously mentioned, a prominent role could have been played by behavioural factors, specifically by the perceived effectiveness of the reform. Was the intervention perceived ineffective in tackling the enforcement of creditors' rights - due to its inability to reduce trials' length - some firms could strategically default on their debts, covered up by the proceeding lengthiness. As a matter of fact, the JSR generated a heated debate both among practitioners and the public. In particular, not only the National Magistrate Association and many local lawyers association (called *local fora*) questioned the effectiveness of the reform in tackling the problem of the lengthiness of Italian trials, but they also issued concerns about its possible short-run negative effects on the ability of the system to cope with the everyday workload. Overall, the message conveyed to the public was controversial, and it is reasonable to conjecture that some firms could have formed biased expectations on the efficiency of the judicial system, enhancing

Figure 7: Impact of the JSR on the Efficiency Index and NPLs'ratio. Heterogeneity by Italian Nuts 1



Note: The horizontal axis reports the Italian Nuts 1 area. Black dots are the points estimates for each region; the vertical lines represent the 90% confidence intervals. Regressions include fixed effects and controls as from Table 1, col.3.

their willingness to default. Eventually, we would have observed a rise in the NPLs ratio which is not totally imputable to a worse performing judiciary system but that could derive from firms' moral hazard driven by the JSR perceived ineffectiveness.

Overall, we can identify two main mechanisms at work here that concur to determine the NPLs dynamics that we observe. On one hand, the impact of the JSR mediated by judicial efficiency; on the other, firms' opportunistic behaviour could have also exerted some influence.

The first channel can be empirically tested in a mediation analysis framework. In a nutshell, mediation analysis aims at identifying "intermediate variables (or mediators) that lie in the causal pathway between the treatment and the outcome" (Imai et al., 2010a). The following system of equations is estimated:

$$M_i = \alpha_1 + \beta_1 T_i + \theta_1 X_i \quad (5)$$

$$Y_i = \alpha_2 + \gamma M_i + \beta_2 T_i + \theta_2 X_i \quad (6)$$

where  $M_i$  is the mediator,  $T_i$  the treatment variable and  $Y_i$  the outcome. Equations 5 and 6 are respectively labelled as mediator and outcome equations.

In particular, there is *full mediation* when a statistically significant  $\gamma$  is combined with a non-significant  $\beta_2$  in the outcome equation (Eq. 6). In other words, the effect of the treatment on the outcome is fully mediated by  $M_i$ . Conversely, in case  $\gamma$  is not statistically significant and  $\beta_2$  is different from zero, the treatment is not mediated at all by  $M_i$ . Moreover, in some instances, there might be *partial mediation*, when the mediator is only partially responsible for the effect of the treatment on the outcome. In these cases, both coefficients are statistically significant. In our setting, the mediator would be the Efficiency Index as a proxy for judicial efficiency, while the outcome would be the NPLs ratio.

Given the two-stage nature of our empirical approach, adapting it to the mediation analysis framework is rather straightforward with Eq. 2 corresponding to the mediator equation, whereas Eq. 3 can be easily transformed into the outcome equation including the (log) Efficiency Index as a control variable:

$$NPL_{itd} = \beta_3(T_i \times Post_t) + \beta_4 \ln(EI_{itd}) + f_i + f_t + f_d + u_{itd} \quad (7)$$

Estimating Eq.7 using OLS and adopting the same set of additional identifying assumptions as in Eq. 3 we get the results presented in Table 3.

It is first of all evident that the magnitude of the DiD coefficient ( $\beta_3$ ) is lower with respect to Table 2, suggesting that the EI contributes to explain the effect of the reform on NPLs.

Most importantly, in the second and third column both  $\beta_3$  and  $\beta_4$  are statistically significant, offering evidence for a partial mediation effect of judicial efficiency on the outcome. To quantify this effect, we resort to the product of coefficients method (MacKinnon et al., 2002) according to which the estimated mediation effect is measured as  $\alpha_3 \times \beta_4$ , where  $\alpha_3$  is the DiD coefficient from Eq. 2. Therefore, we can claim that the effect of the JSR on the NPLs has been partially mediated by judicial efficiency for a total of 0.34 percentage points (out of an overall effect of

Table 3: Mediation analysis results

|                     | (1)                 | (2)                | (3)                |
|---------------------|---------------------|--------------------|--------------------|
|                     | NPLs ratio          | NPLs ratio         | NPLs ratio         |
| $T_i \times Post_t$ | 1.825<br>(1.224)    | 1.959*<br>(1.164)  | 3.277**<br>(1.316) |
| $\ln(EI_{itd})$     | 3.181***<br>(1.164) | 3.036**<br>(1.172) | 2.877*<br>(1.513)  |
| Observations        | 803                 | 803                | 789                |
| $R^2$               | 0.80009             | 0.80562            | 0.83273            |
| Controls            | Y                   | Y                  | Y                  |
| District lin trend  | N                   | Y                  | Y                  |
| Region-year FE      | N                   | N                  | Y                  |

Note: Robust standard errors clustered at the court level in parentheses. Across columns, all the models are implemented controlling for year, court and district fixed effects. Observations are weighted according to the number of businesses operating in the catchment area in 2008.  
\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

3.3 pp).<sup>20</sup>

The evidence offered so far suggests that indeed judicial efficiency has not been the main channel through which the judicial reform has spread out its effects on the banking system. Would it then be reasonable to assign a significant mediation role to firms' opportunistic behaviour? Even though the lack of data on firms' moral hazard prevents us from formally testing this hypothesis, two additional empirical exercises can help to reinforce our conjectures. First of all, in Figure 8 we implement an event study to analyze the evolution of the impact of the reform on the NPLs ratio estimating the following equation:

$$NPL_{itd} = \theta_\tau \sum_{\tau=1}^5 (T_i \times Post_{t+\tau}) + \psi_2 \ln(EI_{itd}) + f_i + f_t + f_d + u_{itd} \quad (8)$$

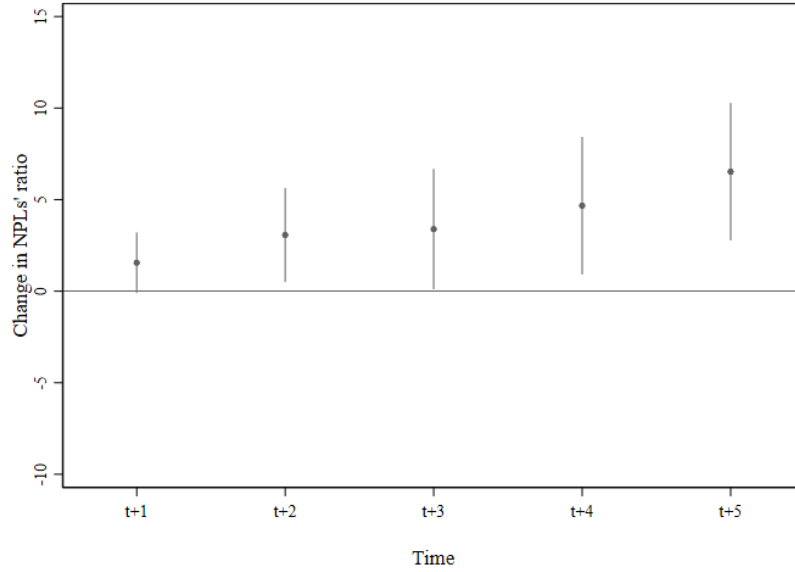
where  $\tau=1..5$ , i.e. we are focusing only on the post-implementation period (2013–2017) and therefore the  $\theta$  coefficients here inform about the effect of the reform in each year after its implementation, taking 2012 as reference.

As expected, the impact of the reform on NPLs follows completely different dynamics with respect to the one displayed when considering the EI as the outcome variable (Figure 3).<sup>21</sup> Therefore, Figure 8 suggests that in the aftermath of the reform NPLs ratio has constantly increased in treated courts with respect to controls. The comparison between the two dynamics advocates for the existence of a concurrent factor that mediates the JSR effect on NPLs ratio and it is also coherent with the hypothesis of a strategic adaptation of firms' attitude to the observed effectiveness of the reform, owing to the fact that the set of fixed effects we use in the

<sup>20</sup>Calculations are based on the results from Column 3 in Table 3.

<sup>21</sup>For the sake of simplicity we take Figure 3 as a reference even though the event study implemented to test the Parallel Trend assumption is slightly different respect to the one adopted in Eq. 8. Nonetheless, we also estimate Eq. 8 using the EI as outcome variable and results - available upon request - mirror those in Figure 3.

Figure 8: Event study: Impact of the JSR on NPLs'ratio



Note: Black dots are the points estimates of the effect of the JSR in each year; the year of implementation (2012) is the reference term; the vertical lines represent the respective 95% confidence intervals. Regressions include fixed effects and controls as from Table 2, col.3.

regressions enables us to control for other possible macroeconomic confounding factors. Along these lines, it is worth underlining that the effect is more marked four and five years after the outset of the policy, denoting the existence of a learning mechanism, which is comparable with an adjustment of borrowers' expectations on the effects of the reform.

Second, to further corroborate our thesis, we exploit the information about the nature of the bank reporting NPLs. In the previous analysis, we have considered all the banks operating in the catchment area of each court, without discriminating on their organisational structure. However, it is worth testing whether the effect of the reform is heterogeneous when comparing commercial and cooperative banks. Indeed, it is well known that cooperative and commercial banks implement different business models, with cooperative banks being more focused on establishing bank-firm relationships based on reciprocity and trust (Berger et al., 2005; Berger and Udell, 2002). They usually have a local scope and this let them exerting tighter monitoring on firms' behaviour. Moreover, the local dimension *per se* helps to avoid opportunistic conduct: in case of misbehaviour, the threat of disclosure to the local community and the subsequent negative reputation effect has a strong detrimental influence. Overall, we could expect that a more widespread presence of cooperative banks should mitigate the negative direct impact of JSR on NPLs ratio, as they might discourage firms to intentionally default.

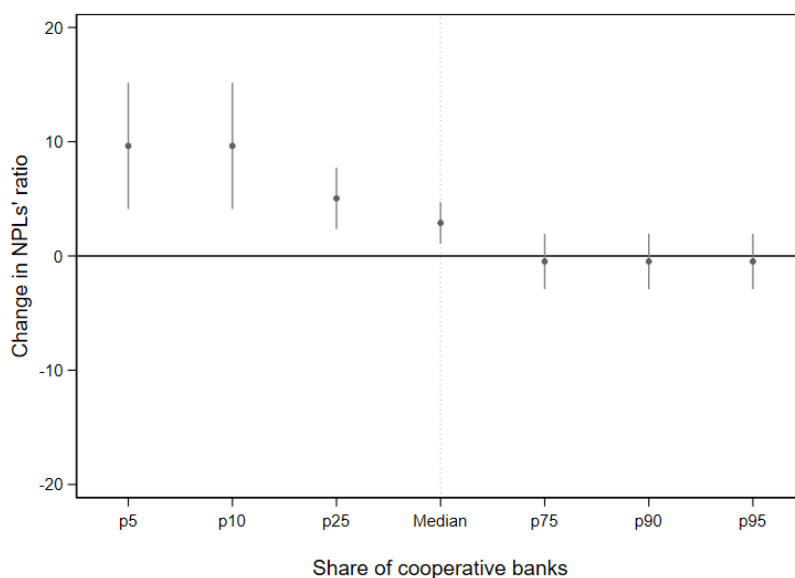
To verify this hypothesis, we augment Eq. 7 with the interaction  $(T_i \times Post_t \times ShCoopBanks_{it})$ , where the term  $ShCoopBanks_{it}$  is the share of cooperative banks over the total number of banks operating in the catchment area of court  $i$  in year  $t$ .<sup>22</sup>

<sup>22</sup>We include also the interaction  $(Post_t \times ShCoopBanks_{it})$  that is here omitted. Moreover, results hold also

As before, the differential effect based on the share of cooperative banks ( $\lambda$ ) is given by the first derivative of the regression equation, with respect to the treatment variable, for different levels of the share. Results are plotted in Figure 9, where the horizontal axis reports the percentiles of  $ShCoopBanks_{it}$  distribution. Interestingly, we observe that the effect on NPLs decreases in magnitude with a higher share of cooperative banks and estimates are not statistically different from zero for those courts where cooperative banks represent more than 50% of the total banks. In other words, in those areas where the presence of cooperative banks is higher (above the median), the reform had an almost null direct effect (i.e it is fully mediated by judicial efficiency).

This result is in line with the assumption that a determinant of the average direct effect of the JSR on NPLs might be represented by borrowers' likelihood to default. Indeed, given the cooperative nature of the financial relationship, this is less likely to happen for cooperative banks.

Figure 9: Impact of the JSR on NPLs' ratio. Heterogeneity with respect to the share of cooperative banks



Note: The horizontal axis reports the percentiles of the share of cooperative banks over the total number of banks. The vertical dashed line identifies the median value. Black dots are the points estimates; the vertical lines represent the 90% confidence intervals. Regressions include fixed effects and controls as from Table 3, col.3.

All in all, this section has investigated some of the potential mechanisms directing the effect of the judicial system reform on banks' NPLs ratio. The analysis has undoubtedly confirmed that the effect passes through the efficiency of the judiciary system. Nonetheless, results have revealed the existence of other concurrent determinants, consistent with the influence of a loyalty component in the bank–borrower relationship.

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if we fix the variable in 2011.



## 9 Conclusions

The analysis on the impact of the judicial system on finance has a long-lasting tradition in economics. The topic has regained attention in the aftermath of the 2008 economic crisis given the potential influence that the judiciary can have in the management of NPLs by banks, and eventually in handling systemic stability.

In this paper, we exploit the outset of a regulation seeking to improve judicial efficiency through the rearrangement of courts' geography in Italy, to provide causal evidence of the relationship between judiciary structural reforms and banks' financial stability. By constructing a dataset on annual proceedings handled by each court over the period 2010–2017, we are able to define an index that measures judicial efficiency in terms of proceedings' duration. We then complement this data with balance sheet information of banks operating in the catchment area of the courts. Our findings yield a negative effect of the reform on both judicial efficiency and NPLs ratio retained by banks located in the competence area of those courts targeted by the reform. The detrimental effect on efficiency depends on the existing capacity of the courts to dispose of pending proceedings with a higher efficiency loss for those that were previously more efficient and shows non-significant geographical differences. Banks' NPLs ratio instead increased only where the disposal capacity of courts was already compromised and in the less developed area of the country.

Taken together these results point to the existence of other concurrent factors, beside courts' efficiency, through which the judiciary system reform affects the ability of banks to recover NPLs. In line with the background literature, a prominent role might be played by borrowers' moral hazard. To dig deeper into this mechanism, we set a causal mediation regression. Although being able to identify the effect mediated by judicial efficiency, we can only provide empirical tests to support our conjectures on the component based on the debtors' behaviour. Thus, we outline the dynamics of the effect in an event-study type of analysis showing that our findings are consistent with a strategic adaptation of firms' attitude to the observed effectiveness of the reform. Furthermore, we point out that the effect is lower in areas characterised by a pronounced presence of cooperative banks, as such operating in a more local dimension that should discourage opportunistic conducts.

More broadly our analysis proves that the role played by the judicial system in shaping banks' credit risk exposure is relevant yet not trivial. Results indicate that judicial efficiency is not the unique channel whereby a structural reform of the judiciary might affect credit relationships and, in turn, banks' financial stability. Also, the reform could encompass other factors related to agents behaviour that might have non-negligible effects on loans stability.

This is particularly relevant in the current economic and financial scenario, worldwide shaped by the Covid-19 outbreak. The European Union is approving one of the most comprehensive package of structural reforms (the Next Generation EU plan), striving to boost economic and financial development. Indeed, the judiciary is at the core of the reforming agenda in many Member States, as judicial efficiency is considered breakthrough in a resilient economy. In light of the above, it would advisable to envisage measures able to account also for potential

concurrent factors that might counteract the main effect.

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## 10 Appendix

Table A.1: Descriptive statistics on judicial efficiency by courts' type

|                          | N   | mean  | sd    | min   | max    |
|--------------------------|-----|-------|-------|-------|--------|
| <b>Whole sample</b>      |     |       |       |       |        |
| Efficiency Index         | 163 | 476.0 | 179.7 | 101.3 | 1,020  |
| Nr of incoming           | 163 | 4,711 | 8,422 | 203   | 81,974 |
| Nr of closed             | 163 | 4,371 | 7,762 | 209   | 71,527 |
| Nr of pending            | 163 | 5,881 | 8,563 | 245   | 69,931 |
| <b>Unaffected courts</b> |     |       |       |       |        |
| Efficiency Index         | 114 | 480.3 | 175.2 | 101.3 | 951.7  |
| Nr of incoming           | 114 | 5,607 | 9,608 | 416   | 81,974 |
| Nr of closed             | 114 | 5,270 | 8,848 | 345   | 71,527 |
| Nr of pending            | 114 | 7,105 | 9,702 | 467   | 69,931 |
| <b>Absorbing courts</b>  |     |       |       |       |        |
| Efficiency Index         | 23  | 504.8 | 219.2 | 201.0 | 1,020  |
| Nr of incoming           | 23  | 4,303 | 5,325 | 761   | 25,953 |
| Nr of closed             | 23  | 3,708 | 4,827 | 525   | 23,906 |
| Nr of pending            | 23  | 4,938 | 4,746 | 759   | 21,701 |
| <b>Dropped courts</b>    |     |       |       |       |        |
| Efficiency Index         | 26  | 431.7 | 159.2 | 184.5 | 765.6  |
| Nr of incoming           | 26  | 1,143 | 620.8 | 203   | 2,466  |
| Nr of closed             | 26  | 1,020 | 526.3 | 209   | 2,493  |
| Nr of pending            | 26  | 1,346 | 891.0 | 245   | 3,830  |

Note: the table reports descriptive statistics on judicial indicators at the beginning of the sample period (2011).

Table A.2: Testing the Common Trend Assumption: Event Study

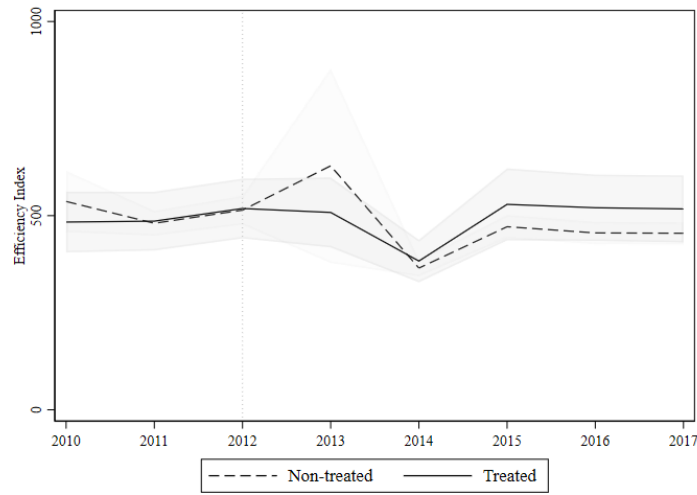
| Dep var: (ln) of        | (1)<br>EI         | (2)<br>Incoming     | (3)<br>Pending    | (4)<br>Closed     |
|-------------------------|-------------------|---------------------|-------------------|-------------------|
| $T_i \times Post_{t-3}$ |                   | -0.015<br>(0.032)   | -0.038<br>(0.038) | 0.049<br>(0.058)  |
| $T_i \times Post_{t-2}$ | -0.075<br>(0.055) | -0.016<br>(0.034)   | -0.027<br>(0.036) | 0.050<br>(0.064)  |
| $T_i \times Post_{t0}$  |                   | -0.033<br>(0.040)   | 0.013<br>(0.031)  | -0.025<br>(0.053) |
| $T_i \times Post_{t+1}$ | 0.007<br>(0.076)  | -0.152**<br>(0.075) | -0.127<br>(0.093) | -0.035<br>(0.092) |
| $T_i \times Post_{t+2}$ | 0.066<br>(0.088)  | -0.168**<br>(0.080) | 0.038<br>(0.072)  | -0.050<br>(0.075) |
| $T_i \times Post_{t+3}$ | 0.114*<br>(0.065) | -0.156*<br>(0.086)  | 0.004<br>(0.074)  | -0.063<br>(0.086) |
| $T_i \times Post_{t+4}$ | 0.116<br>(0.075)  | -0.164*<br>(0.091)  | 0.001<br>(0.074)  | -0.098<br>(0.101) |
| $T_i \times Post_{t+5}$ | 0.084<br>(0.075)  | -0.118<br>(0.095)   | -0.008<br>(0.084) | -0.089<br>(0.092) |
| Observations            | 1,049             | 1,181               | 1,181             | 1,181             |
| $R^2$                   | 0.84431           | 0.93972             | 0.94931           | 0.94116           |
| District lin trend      | Y                 | Y                   | Y                 | Y                 |
| Region-year FE          | Y                 | Y                   | Y                 | Y                 |

Note: Robust standard errors clustered at the court level in parentheses. DiD estimates of the effect of the JSR on the Efficiency Index and its components (log).  $T_i$  is a dummy variable identifying the treated courts.  $Post_{t-3}$ ,  $Post_{t-2}$  are dummy variables equal to 1, respectively, three and two years before the implementation of the JSR and zero otherwise;  $Post_{t0}$  is a dummy variable that is equal to 1 the year of implementation of the JSR;  $Post_{t+1}$ ,  $Post_{t+2}$ ,  $Post_{t+3}$ ,  $Post_{t+4}$  and  $Post_{t+5}$  are dummy variables equal to 1, respectively, 1, 2, 3, 4 or 5 years after implementation of the JSR and zero otherwise.

All the models are implemented controlling for year, court and district fixed effects. Observations are weighted according to the number of magistrates working within the court.

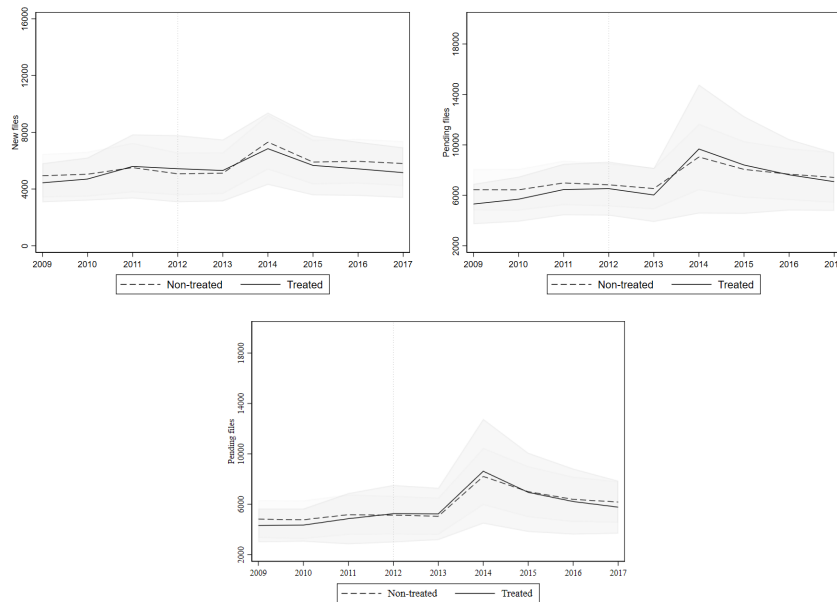
\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Figure A.1: Trends in the Efficiency Index. Treated and control units.



Note: Raw averages by year. The thick solid lines represent the average values for the treated courts; the thin dashed lines refer to the control courts. The gray areas represent the respective 95% confidence intervals.

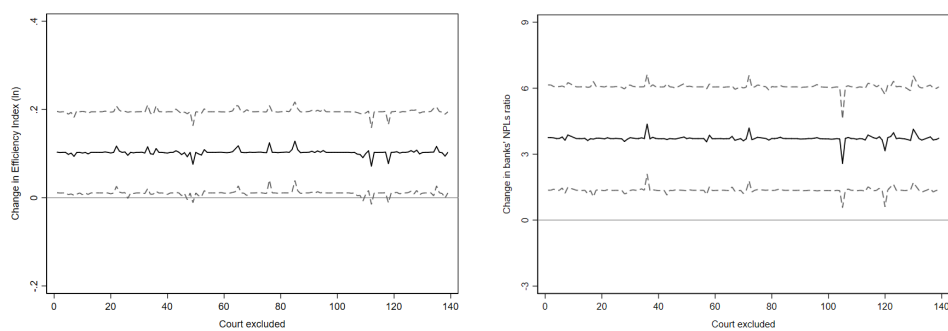
Figure A.2: Trends in the Incoming and Pending proceedings series. Treated and control units.



Note: Raw averages by year. The thick solid lines represent the average values for the treated courts; the thin dashed lines refer to the control courts. The gray areas represent the respective 95% confidence intervals.



Figure A.3: Impact of the JSR on the Efficiency Index and NPLs. Robustness excluding courts one by one



Note: The court excluded is reported on the horizontal axis. The black line connects the respective point estimates as from col.3 in Tables 1 and 2. Dashed lines represent 90% confidence intervals.