# Contagious Effects of a Political Intervention in Debt Contracts: Evidence Using Loan-Level Data

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Using an unexpected government regulation that restricted the ability of microfinance institutions to recover loans in one Indian state, I examine whether this intervention affected bank loan performance. The bank loan delinquency rate significantly increased as a result. In response, the ex post bank credit supply declined by more than half. For identification, I compare loans from branches located in regions subject to this intervention with loans from nearby branches of the same bank located in regions not subject to the intervention. I conclude that political interventions in credit markets could have significant spillover effects. (*JEL* D14, G18, G21)

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A large body of literature has examined the consequences of political intervention in private debt contracts (Agarwal et al. 2016; Bolton and Rosenthal 2002; Kroszner 2003; Melzer 2011; Mian and Sufi 2014) on ex post outcomes within the segment of the credit market subject to the intervention. However, the possibility of such an intervention having a contagious effect on other segments of the credit market that are not subject to the intervention has not received much attention. Such an analysis is important to understand the full costs and benefits of political interventions in debt contracts.[1]

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[1] Furthermore, most of the extant evidence on the implications of political interventions comes from studies that use either the Great Depression (Bolton and Rosenthal 2002; Rucker and Alston 1987) or the recent financial crisis (Mayer et al. 2014) as the economic setting. It is difficult to study spillover effects using such settings, as even parts of the credit market not subject to interventions were in turmoil in these settings, and many interventions took place simultaneously.


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I use a large shock delivered by an unexpected government regulation to microfinance institutions' ability to recover loans in one Indian state as an economic setting and examine the plausible spillover impact of this intervention on bank loans in the geographic region subject to the intervention. Specifically, I ask the following two questions. First, did the performance of bank loans, defined in terms of the default rate, deteriorate in regions subject to the microfinance intervention? Second, anticipating deterioration in loan performance, did banks curtail credit in the post-intervention period?

The economic setting for this study is provided by an ordinance issued by the state government of Andhra Pradesh[2] on October 15, 2010. Breza and Kinnan (2016) study the impact of the above event on the local economy. They document significant negative effects on consumption, employment, and entrepreneurship. Breza (2012) uses an earlier incident of sudden increase in microfinance defaults in one district of Andhra Pradesh to study the impact of a borrower's loan repayment decision on peers. I focus on bank loans. The government imposed severe restrictions on the loan recovery practices followed by microfinance lenders in the state. That the state government issued an ordinance[3] and did not wait for the legislation to pass shows the urgency and speed with which the government reacted.

The reason for this quick reaction was an allegation leveled by the opposition parties and media that the harsh recovery practices adopted by microfinance institutions led 57 individuals to commit suicide in Andhra Pradesh (Kar 2013). As I will discuss in Section 2.1, the numbers compiled by the national crimes database do not show an abnormal increase in suicides related to high indebtedness in the state of Andhra Pradesh when compared with neighboring states. Therefore, it is reasonable to categorize the state government's reaction as an exercise to limit political damage caused by the opposition's propaganda and not as a reasonable reaction to the sudden implosion of an economic crisis. Careful analysis of news around the time of the event shows that no other significant economic event affecting only the state of Andhra Pradesh took place close to the notification date. The impact of the regulation on microfinance lending in Andhra Pradesh was swift and significant; the microfinance delinquency ratio increased from less than 1% to nearly 90% (Kar 2013). The lone listed microfinance stock, SKS microfinance, fell by 6.5% on October 15, 2010, by 26% within one month and by 48% within three months.[4]

2 Andhra Pradesh was one of the largest states of India. It is now divided into two states, Andhra Pradesh and Telangana. I consider the undivided Andhra Pradesh state in this study.

3 An ordinance is issued by an executive to promulgate a law when the legislature is in recess. The ordinance becomes a law as soon it is signed by the governor of the state and notified in the state gazette. However, the ordinance needs to be approved by the state legislature within six months. Otherwise, it lapses and, hence, ceases to be a law.

4 The stock eventually lost nearly 90% of its original market value. The benchmark market index lost only about 7% during this period.

I collect proprietary transaction-level data relating to crop loans from a large government-owned bank in India. I have time series information about all transactions executed by these borrowers. The data set includes loans made by bank branches located in Andhra Pradesh and nearby branches located in the border districts of two other neighboring states, Maharashtra and Karnataka. All bank branches from which I obtained data are located in border districts of the above three states.

A crucial aspect of my analysis is that all the loans I examine in this study are bullet loans with tenure of one year. In other words, repayment of the loans, along with interest, is expected in one installment. No intermediate payments are specified. A loan is considered to be in default if the total outstanding amount including interest is not repaid on or before the due date. A loan lent on, say, January 1 of a year, will be considered in default if it is not fully repaid on or before December 31 of the same year.

I first identify loans made before October 15, 2010 (event date, henceforth), whose due dates were immediately after the event date. I call such loans "affected loans." For example, take the case of a loan made on November 15, 2009. The due date for this loan was November 15, 2010, one month after the event date. Similarly, I identify loans whose due dates were just prior to the event date. I call such loans "unaffected loans." For example, consider a loan made on September 15, 2009. The due date for this loan was September 15, 2010, one month before the event date. This way of identifying affected and unaffected loans has two distinct advantages. First, the decision to provide a loan is made long before the event. Hence, the loans are not affected by loan officer selection after the event. Second, given the bullet loan structure, the decision of whether to default on affected (unaffected) loans has to be made after (before) the event date. This requirement significantly strengthens identification. Using the above facts, I design difference-in-differences (DID, henceforth) and regression discontinuity (RD, henceforth) tests to assess the impact of the event on bank loan performance.

For DID tests, the first difference is provided by the difference in default rates between affected and unaffected loans made by branches located in Andhra Pradesh. Using the fact that the law change applied only to Andhra Pradesh, I design a counterfactual difference: the difference in default rates between affected and unaffected loans made by branches located in the other two states. The difference between the above two differences is the DID measure. I examine and find the existence of a close to parallel trend between the treatment and control regions during the pre-event period. All the branches I study are located in the border districts of their respective states and, hence, are in close geographical proximity. As expected, I do not find significant differences between the treatment and control regions in terms of pre-treatment observable characteristics, such as loan performance, loan size, rainfall, crop area and yield, and agricultural credit.

I find that the default rate of treatment loans is about 18.6 (19) percentage points higher when compared with the default rate of control loans based on DID estimates when I limit the sample to loans having due dates within three (six) months on either side of the event date. Here, I compare loans with due dates between October 15, 2010, and January 15, 2011 (April 15, 2011), with loans with due dates between July 15, 2010 (April 15, 2010), and October 14, 2010. The above differences are both statistically and economically significant.

I use the classification of a loan as a nonperforming asset (NPA) as the second loan performance measure. A crop loan that remains in default for at least two crop seasons is considered an NPA. A crop season is defined as six months for rice, the underlying crop for loans under consideration. Therefore, a loan will be categorized as an NPA if it remains in default for at least 365 days. A loan borrowed on, say, January 1, 2010, will be classified as an NPA if it is not fully repaid on or before December 31, 2011. The loan would be classified as being in default on December 31, 2010. The provisioning requirement goes up significantly if a loan is recognized as an NPA. A loan being classified as an NPA impacts the loan officer's appraisal scores negatively (Bhowal, Subramanian, and Tantri 2014). If one bank loan of a borrower is categorized as an NPA, all other loans of that borrower with the same bank are also classified as NPAs. This impacts the borrower's credit score significantly. For tests that use NPA status as the dependent variable, I consider loans that were in default but had not reached the due date for recognition as an NPA as of October 15, 2010, as "affected loans," and loans in default whose NPA due dates were just before October 15, 2010, as "unaffected loans." I find that the NPA rate of "affected loans" is 30.2 percentage points higher than that of "unaffected loans" based on DID estimates when I limit the sample to defaulted loans with NPA due dates within three months on either side of the event date. The results remain qualitatively similar when I extend the window to six months.

To buttress the findings further, I use a robust RD design (Calonico, Cattaneo, and Titiunik 2014). Here, I use the notification date, October 15, 2010, as the cutoff and the distance between the due date of a loan and the cutoff as the running variable. For example, for a loan with the due date of October 12 (18), the running variable takes the value of $-3$ (3). As before, loan performance, denoted by whether or not a loan defaults (or turns into an NPA), is the dependent variable. The due date, respectively, refers to the due date for default or for becoming an NPA when I use default or NPA as the dependent variable. The RD test detects a 12.4 percentage point jump in the default rate and a 24.5 percentage point jump in the NPA rate at the cutoff for loans made by Andhra Pradesh branches. No such significant discontinuity is detected in control regions.

I perform several robustness tests to rule out other explanations. First, I perform placebo tests and find that the results are robust. Second, I limit the treatment and control samples to regions that are contiguous and, hence, are located very close to each other. The main results are replicated even in this subsample. Third, I perform two external validation tests. For the first external validation test, I collect data pertaining to the delinquency rates of all listed banks in India, including private banks, and I compare the delinquency rates of banks with high exposure to Andhra Pradesh with those of other banks in a DID setting. The level of exposure and the period (pre- or post-intervention) provide the two differences. I find that banks with high exposure to Andhra Pradesh experience higher delinquency rates in the post-notification period based on DID estimates. For the second external validation test, I use the Indian farm debt waiver of 2008 as the economic setting. Gine and Kanz (2017) find that the waiver program led to deterioration in the loan performance of the beneficiaries. Given the findings described so far, I expect the impact of the waiver to spill over to the nonbeneficiaries as well. Using the RD strategy described above, I find that the waiver program led to worsening of the loan performance of the nonbeneficiaries as well. This supports the thesis that political intervention could have a contagious impact. Here, the contagion spreads within the segment.

In the second part of the study, I look at the intervention's impact on subsequent functioning of the credit markets. I employ the robust RD design with a similar setup as described before. Here, I organize the data at a branch-day level. I calculate the daily loan amount at the branch level, which becomes the dependent variable. I find that the daily loan amount lent by Andhra Pradesh branches experiences a sharp discontinuity at the cutoff. The daily loan amount at the branch level falls by 54.8%. However, I do not detect any significant discontinuity in the loan amount lent by branches located outside Andhra Pradesh. Furthermore, the decline in loans seems to be driven by the intensive margin, as I do not detect any discontinuity in the number of loans granted per day.

Although I show that a government intervention in one segment of the credit market has adverse spillover impacts on loan performance and loan supply in other segments, due to data limitations, I cannot precisely pin down the exact mechanism at work. First, it is possible that the curtailment of microfinance credit adversely affected Andhra Pradesh's agricultural economy (Breza and Kinnan 2016; Sane and Thomas 2016) and that this impact led to increased bank loan defaults. The results show a sharp jump in defaults immediately after the event, implying that at least a part of the result is not caused by the above mechanism. The real effects of a loan supply shock are likely to take some time to manifest and spread through economic linkages.[5] However, it is not possible to fully rule out the above explanation.

A second possibility is that bank borrowers defaulted anticipating a future government intervention in bank loans. In this case, the reaction is likely to be quick. Therefore, a decline in loan performance immediately after the event

[5] I thank an anonymous referee for pointing this out.

is consistent with the moral hazard view. This discussion applies to the flow of bank credit in the post-event period as well. It is possible that loan officers tightened their lending anticipating deterioration in loan performance. It is also possible that adverse economic effects of the intervention led to reduced loan demand. Admittedly, I do not have data pertaining to loan applications, and, hence, disentangling the impact of demand from supply is a challenge. However, as stated above, the manifestation of demand effect is likely to take time, and, hence, an immediate decline in credit flow is likely to be the result of the tightening of supply. However, the existence of demand effects cannot be completely ruled out.

I contribute to two distinct strands of literature. First, this study is related to a large body of empirical literature that examines the consequences of political interventions in private loan contracts (Agarwal et al. 2016; Bolton and Rosenthal 2002: De and Tantri 2014; Gine and Kanz 2017; Mayer et al. 2014; Mukherjee, Subramanian, and Tantri 2014; Rucker and Alston 1987). I contribute to this literature by showing that a government intervention in one segment of the credit market could cause ripple effects on other segments. Second, I also contribute to the literature that highlights the possibility of a financial contagion (Acemoglu et al. 2015; Allen and Gale 2000; Bae, Karolyi, and Stulz 2003; Bekaert et al. 2014; Billio et al. 2012). I argue that a political intervention in one segment of the credit market could be the source of a contagion.

## 1. Institutional Background

### 1.1 Indian banking

The Indian banking landscape, even to this day, is dominated by public sector banks. Public sector banks account for more than 70% of the bank credit in India.[6] Banks and micro-finance institutions are linked directly, as bank loans are one of the most important sources of funding for micro-finance institutions (ET Bureau 2015). Banks in India are tightly regulated by the Reserve Bank of India.

### 1.2 Agricultural lending in India

As described earlier, I use crop loan data. The majority of the Indian population, even to this day, is dependent on agriculture. Agriculture is considered as a "priority sector," and, hence, all banks are required to direct at least 18% of their credit to agriculture (Cole 2009b). Although banking is a federal subject, local politicians often intervene in the recovery process with an eye on borrowers' votes (Mukherjee, Subramanian, and Tantri 2014).[7] Local politicians are

6 https://rbi.org.in/scripts/BS_PressReleaseDisplay.aspx?prid=38738.

7 Moreover, agricultural loans are excluded from the purview of many recent laws that have significantly enhanced creditor rights (Bhue, Prabhala, and Tantri 2015).

members of state-level bankers' committees that set broad guidelines regarding agricultural lending in the state.[8]

**1.2.1 Loan features.** Having briefly discussed the broad institutional framework applicable to agricultural lending in India, I proceed to describe the relevant loan features. As noted earlier, all the loans I consider are crop loans with tenure of 12 months. I verify this fact using a sample loan contract obtained from the bank. The underlying crop is rice. Moreover, the loans I study are bullet loans to be repaid in a single installment. In other words, there are no intermediate payments to be made.[9] This type of loan structure suits farmers who are likely to earn most of their revenue within a short interval, that is, immediately following the harvest season.

**1.2.2 Measures of loan performance.** Broadly, a crop loan that is not fully repaid is classified as either current, in default, or as an NPA. Every loan is classified as *current* until the due date. A loan that is *not fully* repaid as on the due date is considered to be *in default*. Figure 8, presented in the Online Appendix, depicts the above classifications. I provide an example to clarify the above classification further. Suppose, three farmers, A, B, and C, borrow INR[10] 10,000 each on January 1, 2010. The due date for all three loans will be December 31, 2010. All three loans are classified as current until the end of the day on December 31, 2010. Assume that A repays her/his loan on December 31, 2010, just before the end of the working hours of the bank. A's loan account gets closed. Also assume that B and C do not fully repay their loans by the end of the day on December 31, 2010. Crop loans lent to B and C will be classified as loans in default starting from January 1, 2011. It is crucial to note that even a delay of one day beyond the due date in loan repayment leads to classification of a loan as being in default. Classification of a borrower's loan as a loan in default negatively impacts the borrower's credit score, possibly restricting future access to credit. The default status also triggers recovery procedures, starting from constant reminders to issue of legal notice.

The next stage of loan classification after default is the NPA stage. A loan that remains in default for at least two crop seasons is classified as an NPA.[11] In case of rice, the underlying crop in the sample, a crop season is defined as six months. Therefore, effectively, a loan that remains in default for 365 days or more is classified as an NPA. Going back to the earlier example, the deadline

[8] The state-level bankers' committee determines the definition of the season for each crop, reviews the flow of agricultural credit, and makes nonbinding recommendations to banks regarding the allocation of credit to agriculture within the state, and constantly monitors the flow of bank credit to agriculture within the state.

[9] However, I observe that some borrowers do make partial payments.

[10] INR stands for Indian Rupee.

[11] Source: https://rbi.org.in/Scripts/BS_ViewMasCirculardetails.aspx?id=9009.

for NPA classification for loans borrowed on January 1, 2010, is December 31, 2011, that is, one year from the due date of December 31, 2010, and—given the 365-day loan tenure—two years from the date of loan issuance. Suppose B repays the loan in full along with interest and penalty before December 31, 2010, but C fails to do so. C's loan will be classified as an NPA starting from January 1, 2011.

The classification of a loan as an NPA has significant consequences for the bank, the loan officer, and the borrower. From the bank's point of view, the provisioning requirement is significantly higher for NPAs when compared with default.[12] NPA management gets a weightage of 10% in a loan officer's annual appraisal. I present a pro forma appraisal form in the Online Appendix. Classification of a loan as an NPA allows a bank to initiate action under The Securitisation and Reconstruction of Financial Assets and Enforcement of Security Interest Act 2002 (SARFAESI Act). The SARFAESI Act allows the lender to seize collateral without obtaining prior permission from a court of law. Prior to SARFAESI, a lender needed explicit approval from a court to seize collateral. A lender can proceed under SARFAESI only after classifying a loan as an NPA and providing a 60-day notice to the borrower (Alok, Chaurey, and Nukala 2016; Vig 2013). Mere default does not trigger SARFAESI action. Finally, if one loan borrowed by a borrower from a bank turns into an NPA, then all other loans borrowed by the same borrower from the same bank are considered as NPAs. Therefore, the impact of NPA classification on a borrower's credit score is much higher than the impact of default.

### 1.3 Microfinance

Microfinance is a form of financial service for the poor and small enterprises (Kannan, and Panneerselvam 2013). It is based on the idea of providing financial services to the poor through market-based structures. Muhammad Yunus, who started the Grameen Bank in Bangladesh, pioneered the recent round of the microfinance revolution in emerging and under-developed economies. The key feature of microfinance lending is that the focus is on lending to close-knit groups and not to individuals (Banerjee et al. 2015; Breza 2012). Such groups work as informal enforcement mechanisms. As per World Bank's estimates, microfinance now serves 16 million people all over the world (Longhurst 2013).

Microfinance in India grew at an annualized rate of 83% between 2003 and 2010 (M CRILL 2012). The delinquency rate of microfinance loans made during that period was less than 1% (Kaur and Dey 2013). Although industry insiders attribute this performance to better process, there have been allegations that microfinance lenders adopted coercive recovery practices (Kar 2013; Shylendra 2006). Andhra Pradesh, the state under consideration in this study, was at the forefront of microfinance growth in India. Lending to close-knit

[12] Source: https://rbi.org.in/Scripts/BS_ViewMasCirculardetails.aspx?id=9009.

self-help groups was the delivery model adopted by microfinance institutions in Andhra Pradesh.

## 2. The Event

### 2.1 Background

Microfinance was not tightly regulated in India in the late 1990s and early 2000s (Singh 2003). In 2006, 10 people who also happened to be microfinance borrowers committed suicide in the Krishna district of Andhra Pradesh. Local media and politicians blamed the suicides on "coercive" recovery practices adopted by microfinance institutions. This led to the forced shutdown of two branches of two large microfinance lenders (Ghate 2008). The government of Andhra Pradesh refrained from intervening in the microfinance sector until the year 2010. In 2010, newspapers reported that 57 microfinance borrowers committed suicide due to the "coercive recovery practices" of microfinance institutions and "excessive" borrowing. These reports created a huge furor, with the opposition parties blaming the government for being a mute spectator (Bandyopadhyay 2014).

However, a cursory look at the national crimes database shows that, in total, 15,901 individuals committed suicide in Andhra Pradesh in the year 2010, and this number was not significantly different from those of the neighboring states of Maharashtra, Karnataka, and Tamil Nadu. More importantly, a careful look at the suicide notes reveals that bankruptcy or a sudden change in economic status were not the major reasons for the suicides (IANS 2015). Given the above details, it appears that the government's decision to intervene was a knee-jerk reaction to limit political damage.

### 2.2 Ordinance

As a response, the Andhra Pradesh government issued an ordinance titled "The Andhra Pradesh Micro Finance Institutions (Regulation of Money Lending) Rules 2010" on October 15, 2010. This ordinance imposed severe restrictions on the recovery practices of microfinance institutions. Some of the important restrictions included the following:

- Compulsory registration of all microfinance institutions with the district administration
- Ban on the door-to-door collection of loans
- Public disclosure of interest rates
- Heavy punishment, including imprisonment

The ordinance was ratified by the state assembly on December 15, 2010.[13] The direct impact of the notification was stark. Loan performance deteriorated

[13] The ordinance was challenged in the High Court and later in the Supreme Court and was upheld by both.

**Table 1**
**Sample construction**

| Variable | |
|---|---|
| Total number of bank branches | 13 |
| Number of branches in Andhra Pradesh | 8 |
| Number of branches in Karnataka | 2 |
| Number of branches in Maharashtra | 3 |
| Average distance between branches within Andhra Pradesh (in miles) | 150 |
| Average distance between Andhra Pradesh and Maharashtra branches | 222 |
| Average distance between Andhra Pradesh and Karnataka branches | 277 |
| Total number of loans | 37,556 |
| Number of loans from Andhra Pradesh branches | 27,963 |
| Number of loans from Karnataka branches | 4,100 |
| Number of loans from Maharashtra branches | 5,493 |
| Number of loans outstanding but not in default as of Oct. 15, 2010 | 3,994 |
| Total number of loan officers | 44 |

and loan uptake decreased in the microfinance sector. The loan repayment rate fell drastically from 99% to 10%. Microfinance loan growth fell from 95% to 17% per annum (Kar 2013). The enormity of the regulation is evidenced by the steep fall in the stock price of the only listed microfinance stock, SKS microfinance. INR 100 invested in SKS just before the announcement of the regulation was worth only INR 11 in a year's time, whereas the benchmark index lost only about 7% during this period.

## 3. Data, Variable Definitions, and Summary Statistics

In this section, I describe the data and provide basic summary statistics.

### 3.1 Data

I obtain transaction-level data from a large government-owned bank in India. The bank operates more than 1,000 branches and has more than 70 years of operating history. Bhowal, Subramanian, and Tantri (2014) use the same data to study the costs of job rotation. Details about the data are presented in Table 1. I obtain data from 14 branches of the bank located in three large states of India, namely, Andhra Pradesh, Karnataka, and Maharashtra.[14] The data collection exercise was not undertaken with the view to examine the microfinance event. Its purpose was to study the impact of various institutional changes on farm loans. Of the 14 branches, one branch located in Andhra Pradesh did not have any agricultural loans outstanding during the event period. Therefore, I use loan data drawn from 13 branches of the bank. The data set spans the period between October 2005 and May 2012. The bank also provided us information about whether or not a borrower is a beneficiary of the national debt waiver awarded in the year 2008.

[14] The data were collected from the branches, and, hence, the sample size is limited to 14 branches. The border districts were chosen first. The bank branches within these districts were selected through a lottery.

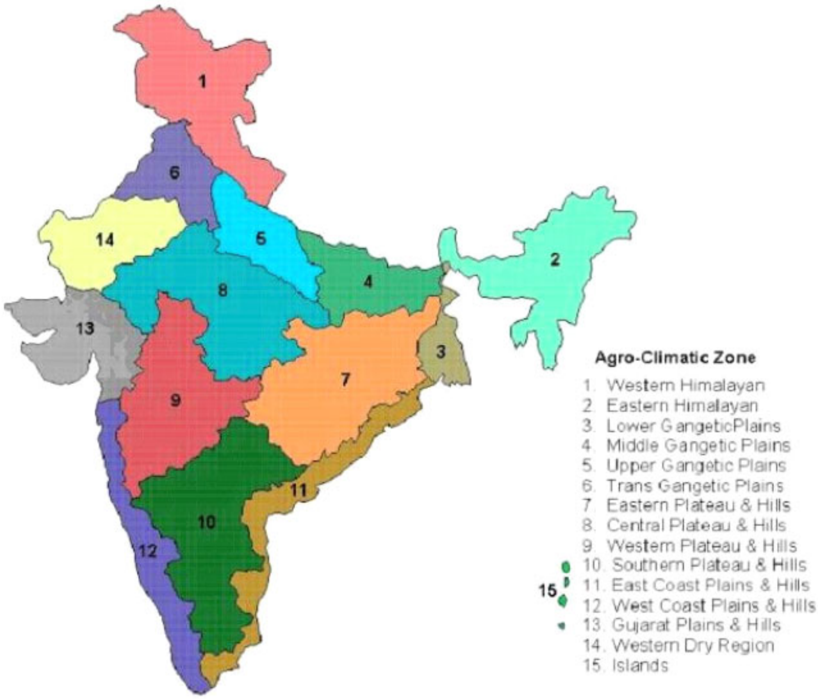


**Figure 1**
**Agro-climatic regions of India**
This figure shows the different agro-climatic regions of India. The bank branches from which I obtain data are all located in zone number 10. Source: Planning Commission of India.

As described earlier, I follow Ponticelli and Alencar (2016) and Banerjee and Iyer (2005) in designing the identification strategy. I compare the outcomes at bank branches located in areas that received the treatment with outcomes at bank branches located in neighboring areas that did not receive the treatment. Therefore, the distance between the treated and control branches becomes a critical factor for identification. As shown in Figure 1, all 14 bank branches are part of a single agro-climatic region and, hence, are subject to similar weather shocks.

I calculate the pairwise distance from each branch located in Andhra Pradesh to the branches located in the other two states. I report the average for these pairwise distances in Table 1. The average distance, so calculated, between the Andhra Pradesh and Maharashtra branches is 222 miles, and the average distance between the Andhra Pradesh and Karnataka branches is 277 miles. This result compares well with the average distance of 150 miles between branches located within Andhra Pradesh. Furthermore, the three states under consideration are large states of India, with a combined area of 299,126 square

**Table 2**
**Comparison between Andhra Pradesh districts and other districts in terms of key observable characteristics during the pre-intervention period**

| | Andhra Pradesh | Other states |
|---|---|---|
| Crop yield (kg per hectare) | 1,848 | 1,822 |
| Crop area (000 hectares) | 3,932 | 3,980 |
| Agricultural credit (million rupees) | 10,870 | 11,580 |
| Inflation (value of GDP deflator) | 169 | 175 |
| Rainfall (in cm) | 930 | 893 |

In this table, I compare Andhra Pradesh districts and the other two states' districts in terms of variables ranging from crop yield to rainfall. I consider only those districts where the bank branches from which I source data are located. I calculate the within-state average for the years 2009 and 2010. I obtain data related to crop yields and crop area from the federal agricultural ministry, rainfall from the meteorological department, credit from the central bank, and inflation from the national statistics office.

miles.[15] Not surprisingly, the treatment and control regions share similar observable characteristics. I provide these details in Table 2. I compare the regions on several observable characteristics, such as rainfall, crop yield, area under cultivation, and agricultural credit, before the intervention.

In total, I have data relating to 37,556 loans lent before the event date by 44 loan officers. The data set has information about a total of 43,767 loans. Here, I report the summary numbers by restricting the sample to loans lent before the microfinance intervention. Out of these, 27,963 loans were lent by bank branches located in the state of Andhra Pradesh, 4,100 by those in Karnataka, and 5,493 by those in Maharashtra. These totals correspond to 3,107 loans per branch for Andhra Pradesh branches, 2,050 for Karnataka branches, and 1,831 for Maharashtra branches. Andhra Pradesh's average is significantly affected by the Karimnagar branch, which alone accounts for 12,148 loans. If I exclude this branch, the number of loans per branch for Andhra Pradesh falls to 1,977, which is comparable with those of the other two states. As noted earlier, the focus here is on outstanding loans that were lent before October 15, 2010, but were not in default as of October 15, 2010. There are 3,994 such loans. The above details are reported in Table 1.

## 4. Empirical Strategy and Results

I use the fact that the notification under study was issued by the state government of Andhra Pradesh and, hence, was applicable only within the borders of Andhra Pradesh after a particular date. Several influential studies (Banerjee and Iyer 2005; Butler and Cornaggia 2011; Ponticelli and Alencar 2016) use regions that border the treatment regions in order to estimate the counterfactual. The crucial identification assumption is that regions located close to each other are likely to share similar observable and unobservable characteristics, and, hence,

[15] Undivided Andhra Pradesh has an area of 106,195 square miles; Karnataka has an area of 74,122 miles; and Maharashtra has an area of 118,809 square miles.

any difference in outcomes that manifests after a treatment can reasonably be attributed to the treatment. In the same spirit, I consider branches located in Andhra Pradesh as treatment branches and branches located in regions very close to the Andhra Pradesh branches but situated in the states of Karnataka and Maharashtra as control branches.

Both the treatment and control branches are located within a single agro-climatic zone in India. I illustrate this fact in Figure 1. The Planning Commission of India[16] divided the country into 15 zones based on agro-climatic conditions. Agro-climatic conditions mainly refer to soil types, rainfall, temperature, and water availability, which influence the type of vegetation. All the branches are located in the southern plateau and hills zone. This zone is denoted by number 10 in Figure 1. All these branches are clustered in the northwest corner of zone 10. It is well known that weather and other natural factors play a key role in Indian agriculture (Burgess et al. 2011; Rosenzweig and Wolpin 1993). Moreover, as described in Section 3, all branches of the other states are located in close proximity to the Andhra Pradesh branches, and, hence, borrowers from across the border are likely to be similar and subject to similar economic shocks. In Table 2, I show that the treatment and control regions compare well in terms of several observable characteristics, such as rainfall, crop yield, area cultivated, flow of agricultural credit, and literacy in the pre-event period.

The second part of the identification strategy involves identifying the affected loans. A significant identification challenge faced by studies that compare the outcomes of loans made after a natural experiment with those of loans made before the natural experiment is that post-period loans could be influenced by selection by the loan officer. For example, if loan officers expect borrower behavior to deteriorate after a debt waiver, then they may tighten credit. Credit may be liberalized if loan officers expect the borrowers to do better in the post-waiver period. In this situation, a study that finds the worsening of (improvement in) borrower behavior after a waiver by analyzing post-waiver loans may not be able to capture the impact of selection by loan officers. This selection may render pre- and post-waiver loans incomparable.

The natural experiment I study provides us with an opportunity to overcome the above limitation. The microfinance regulation only made loan recovery difficult in the microfinance segment. Prima facie, it had no impact on existing bank loans. I use this aspect of the natural experiment to identify a group of affected loans. I first identify loans made by Andhra Pradesh-based bank branches that were outstanding but not overdue as of October 15, 2010, and whose due dates were immediately after October 15, 2010. I call these loans "affected loans."Given that the loans are all bullet loans with a single stipulated payment at the end of the tenure and that their tenure is one year, these loans

16 The Planning Commission was abolished in 2014. Currently, the National Institution for Transforming India (NITI) Aayog performs most of the functions of the erstwhile Planning Commission.

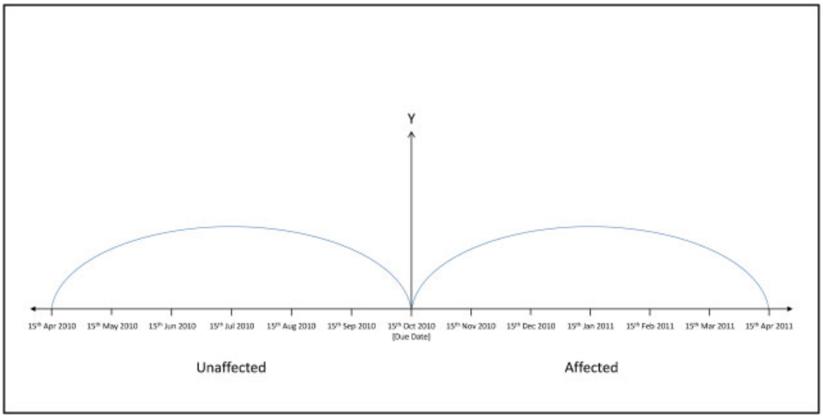


**Figure 2**
**Unaffected and affected loans: Identification**
This figure depicts the treatment and control groups. Due dates of loans are represented in the horizontal axis with October 15, 2010, at the center. Loans with due dates before October 15, 2010, form the control group, and loans with due dates after October 15, 2010, form the treatment group.

must have been made before October 15, 2010. Since all of these loans were provided before the passage of the ordinance, there is little chance of a loan officer factoring in plausible borrower reaction to the event when lending these loans. Therefore, the question of disentangling the borrower reaction and loan officer reaction is less important because the loan officer would have made her/his decision by the time the notification was issued. Thus, any outcome that arises can reasonably be attributed to the borrower.

Next, I identify loans whose due dates were just before October 15, 2010. I call these loans "unaffected"loans. These loans were made in advance of the event date, and also the decision to repay or default was made before the event date. In the case of affected loans, default could have happened only after the event date. I depict the classification of loans into affected and unaffected groups in Figure 2.

The above classification of branches into treatment and control branches and loans into affected and unaffected loans creates a good set-up for employing the DID methodology. The difference in default rates between affected and unaffected loans in the treatment region (Andhra Pradesh) provides the first difference. The counterfactual is the difference between affected and unaffected loans in the control (Karnataka and Maharashtra) region. The difference between the two differences is the DID measure. The event also lends itself nicely to an RD design. In an RD framework, October 15, normalized to zero, could be used as the cutoff, and the distance from the cutoff, in terms of days, could be used as a running variable. By design, affected loans fall to the right of the cutoff, and unaffected loans fall to the left. The RD can be estimated separately for control and treatment regions.

**Table 3**
**Univariate comparison**

| Variable | Group | Difference (%) | $t$-stat |
|---|---|---|---|
| Default | Treatment | 13.27*** | 6.29 |
| Default | Control | 2.37 | 1.32 |
| NPA | Treatment | 7.98*** | 4.31 |
| NPA | Control | 1.92 | 0.95 |

In this table, I present the results of univariate tests that compare the treatment and control groups in terms of default and NPA rate. Column 1 presents the outcome variable of interest. Column 2 indicates the sample used. Column 3 presents the difference in outcomes between branches located in Andhra Pradesh and the other two states. Column 4 presents the $t$-statistics for the difference. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

## 4.1 DID test

The preceding discussion on empirical strategy sets the stage for tests designed to establish causality. The results of the univariate tests presented in Table 3 show that the default rate increased significantly in treatment regions in the post-intervention period. No significant increase is detected in control regions.

I estimate the following DID regression equation.

$$\begin{aligned} Y_{\text{itj}} = {} & \alpha_{itj} + \beta_1 \times \text{Andhra}_{ij} \times \text{Affected Loans}_{it} \\ & + \beta_2 \times \text{Affected Loans}_{it} + \beta_3 \times Month_{it} + \beta_4 \times Branch_{ij} \\ & + \beta_5 \times X_{itj} + \beta_6 \times D_{tj} + \epsilon_{itj}. \end{aligned} \tag{1}$$

Each observation represents a loan $i$ with due date within a month $t$ and made by a branch $j$. The dependent variable, $Y_{itj}$, is a dummy variable that takes the value of one if loan $i$ with due date within month $t$ and provided by branch $j$ defaults and zero otherwise. The explanatory variable, $Andhra_{ij}$, is a dummy variable that takes the value of one for loans made by branches located in Andhra Pradesh and zero otherwise. The explanatory variable, *Affected loans*$_{it}$, is a dummy variable that takes the value of one for loans with a due date after October 15, 2010, and zero otherwise. The variables $Month_{it}$ and $Branch_{ij}$ represent month fixed effects and branch fixed effects, respectively. $X_{itj}$ represents loan-level controls, such as loan size and the tenure of the loan officer when the loan was issued. $D_{tj}$ represents time-varying district-level controls, such as level of crop yield, total agricultural credit, and inflation.

The explanatory variable of interest is the interaction between $Andhra_{ij}$ and *Affected loans*$_{it}$. The results are reported in Table 4. In Columns 1 and 2 (3 and 4), I limit the sample to loans with a due date within three (six) months before and after October 15, 2010. I include branch and month fixed effects in all specifications. District and loan-level control variables are included in Columns 2 and 4. Standard errors are clustered at the branch X month level and adjusted for heteroscedasticity.

As shown in the table, the interaction term is consistently positive and statistically significant at conventional levels. The increase in the default rate ranges from 17.9 to 22.6 percentage points based on DID estimates. The default

**Table 4**
**Impact on default rate: Difference-in-differences test**

| | Default | | | |
|---|---|---|---|---|
| Dependent variable | (1) | (2) | (3) | (4) |
| Andhra X Affected loans | 0.186** | 0.179** | 0.190** | 0.226* |
| | [2.102] | [2.052] | [2.124] | [1.928] |
| Affected loans | 0.125 | 0.071 | 0.063 | 0.105 |
| | [1.158] | [0.607] | [0.537] | [0.808] |
| Loan-level factors | No | Yes | No | Yes |
| District-level factors | No | Yes | No | Yes |
| Branch fixed effects | Yes | Yes | Yes | Yes |
| Month fixed effects | Yes | Yes | Yes | Yes |
| Observations | 2,167 | 2,167 | 3,959 | 3,959 |
| Adjusted R-squared | 0.234 | 0.238 | 0.206 | 0.224 |

In this table, I present the results of tests that examine the impact of the microfinance regulation on bank loan defaults. The data are organized at the loan level. In Columns 1 and 2, the data are limited to loans whose due dates are within the range of three months before and three months after October 15, 2010, that is, July 15, 2010, to January 15, 2011. In Columns 3 and 4, the data are limited to loans whose due dates are within the range of six months before and six months after October 15, 2010, that is, April 15, 2010, to April 15, 2011. The dependent variable, default, is a dummy variable that takes the value of one if the loan under consideration defaults and zero otherwise. The explanatory variable, Andhra, is a dummy variable that takes the value of one for loans made by branches located in Andhra Pradesh and zero otherwise. Affected loans is also a dummy variable that takes the value of one for loans having due dates immediately after the event date and zero otherwise. The main explanatory variable is the interaction between the above two variables. I use branch and month fixed effects in all columns, and I use loan-level and district-level economic variables as controls in Columns 2 and 4. Standard errors are clustered at the branch X month level and adjusted for heteroscedasticity. $t$-statistics are presented inside parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

rate during six months immediately before the event in Andhra Pradesh is close to 48%. An increase in default rate by 22.6 percentage points implies an increase by 47.08% when compared with the pre-intervention level. Therefore, the increase is economically significant. Furthermore, the results continue to hold even if I drop the largest branch, Karimnagar.[17]

From the abovementioned results, it is reasonable to infer that government intervention in the microfinance segment did have a contagious impact on bank loans.

## 4.2 Impact on nonperforming assets

Given the institutional details presented in Section 1, it is not surprising that the default rate of agricultural loans is extremely high at 62% in the overall sample and 48% during six months immediately before the event. This number seems abnormally high. However, none of the institutional features relevant for banking changed during the sample period. Furthermore, I look at changes based on DID estimates rather than changes in levels. Nonetheless, I reexamine the results using a second measure of loan performance. As per the Reserve Bank of India norms, a crop loan is considered as an NPA only if it remains in default for at least two crop seasons. The loans for which I have data are all crop loans, and a crop season is defined as six months for rice, the underlying crop

[17] The result excluding Karimnagar is presented in Table 14 in the Online Appendix.

for the sample of loans used in this study. The bank is required to significantly increase provisioning once an account is classified as an NPA, and, hence, the NPA number is relevant from the point of view of bank profitability. The average NPA rate is nearly 27%, which is significantly lower than the default rate. This result shows that a large number of defaulters eventually repay. I estimate the DID regression equation using loan status in terms of NPA as the dependent variable.

Given the importance of NPAs described in Section 1.2.1, I proceed to test if the microfinance regulation affected NPAs. For the purpose of this test, I define affected loans as loans in default but not categorized as NPAs as of October 15, 2010. In line with the spirit of the earlier DID test, I consider loans whose NPA due dates were immediately after (before) October 15, 2010, as affected (unaffected) loans.

Univariate results presented in Table 3 show an increase in NPA rate in the treatment regions in the post-intervention period when compared with the pre-intervention period. No such increase is found in control regions. Formally, I estimate regression Equation (1). Here, the dependent variable, NPA, is a dummy variable that takes the value of one if the loan under consideration becomes an NPA and zero otherwise. All other terms have the same meanings as assigned in Equation (1).

The sample used here is not the same as that used in tests whose results are reported in Table 4. Here, loans lent immediately after October 15, 2008, form the treatment group, and loans lent immediately before that date form the control group. These are loans lent close to two years before the event date. Loans studied in Table 4 are lent close to one year before the event date.

The results are reported in Table 5. The arrangement of rows and columns exactly mimics the arrangement in Table 4. The main explanatory variable that I focus on, the interaction between the treatment dummy and affected loan dummy, is positive in all columns and is statistically significant. The NPA rate seems to be higher based on the DID estimates by 18.7 to 30.2 percentage points. Given that the average NPA rate in the sample is 27%, the increase shown here is economically significant.

Note also that the increase in NPAs is significantly higher than the increase in the default rate. As I noted in Section 1.2.1, NPA recognition leads to significant negative consequences for banks, bankers, and borrowers. Therefore, in the normal course, almost half the defaulters repay before their loans are categorized as NPAs.[18] If the microfinance intervention led to an economic slowdown or an increase in strategic default in anticipation of future interventions, then it is reasonable to expect that a large portion of defaulters

[18] In Figure 7 presented in the Online Appendix, I plot the proportion of defaulted loans that are recovered before being classified as NPAs against the time in default in the pre-event period. I find that the proportion of loans recovered increases as the due date for NPA classification nears. However, given the data limitations, it is not possible to disentangle the impact of loan officers' efforts from borrowers' repayment on their own to avoid the NPA tag.

**Table 5**
**Impact on NPA rate: Difference-in-differences test**

| | NPA | | | |
|---|---|---|---|---|
| Dependent variable | (1) | (2) | (3) | (4) |
| Andhra X Affected loans | 0.302*** | 0.303*** | 0.241*** | 0.187*** |
| | [5.503] | [5.454] | [4.735] | [2.888] |
| Affected loans | 0.037 | 0.036 | 0.075 | 0.131** |
| | [0.834] | [0.786] | [1.406] | [2.231] |
| Loan-level factors | No | Yes | No | Yes |
| District-level factors | No | Yes | No | Yes |
| Branch fixed effects | Yes | Yes | Yes | Yes |
| Month fixed effects | Yes | Yes | Yes | Yes |
| Observations | 4,604 | 4,604 | 5,354 | 5,353 |
| Adjusted R-squared | 0.228 | 0.229 | 0.208 | 0.227 |

In this table, I present the results of tests that examine the impact of the microfinance regulation on bank loan NPAs. The data are organized at the loan level. In Columns 1 and 2, the data are limited to loans whose NPA due dates are within the range of three months before and three months after October 15, 2010, that is, July 15, 2010, to January 15, 2011. In Columns 3 and 4, the data are limited to loans whose NPA due dates are within the range of six months before and six months after October 15, 2010, that is, April 15, 2010, to April 15, 2011. The dependent variable, NPA, is a dummy variable that takes the value of one if the loan under consideration turns into an NPA and zero otherwise. The explanatory variable, Andhra, is a dummy variable that takes the value of one for loans made by branches located in Andhra Pradesh and zero otherwise. Affected Loans is also a dummy variable that takes the value of one for loans whose NPA due dates are immediately after October 15, 2010. The main explanatory variable is the interaction between the above two variables. I use branch and month fixed effects in all columns, and I use loan-level and district-level economic variables as controls in Columns 2 and 4. Standard errors are clustered at the branch X month level and adjusted for heteroscedasticity. $t$-statistics are presented inside parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

who would have repaid otherwise do not do so in the post-event period. It is also reasonable to assume that both economic slowdown and spillover of moral hazard are likely to impact past defaulters more than good borrowers.

### 4.3 Regression discontinuity test for loan performance

As noted earlier, the event nicely lends itself to an RD test as well. I perform an RD test to examine the impact of the event on loan performance. I use October 15, 2010, as the cutoff date and the distance of a loan's due date from October 15, 2010, as the running variable. I use a measure of loan performance as the dependent variable. For tests that use NPA as the dependent variable, the running variable is defined as the distance of a loan's due date for recognition as an NPA from October 15, 2010. Essentially, like in Sections 4.1 and 4.2, affected loans fall to the right of the cutoff, and unaffected loans fall to the left.

I use the method designed by Calonico, Cattaneo, and Titiunik (2014). This method recognizes that routinely employed polynomial estimators are extremely sensitive to specific bandwidths employed. Calonico, Cattaneo, and Titiunik (2014) show that both conventional RD tests as well as recently developed nonparametric local polynomial estimators make bandwidth choices that lead to "bias in the distributional approximation of the estimator." They first bias-correct the RD estimator by recentering the $t$-statistics. This step leads to their bias-corrected estimators. They also recognize that this approximation may lead to a "low-quality distributional approximation." They devise a novel method to calculate standard errors that account for the additional variability introduced by the previous step. They call these standard errors robust standard errors. I estimate both bias-corrected and robust RD estimators. I also report conventional RD coefficients.

All three estimates are from local linear regressions. The bias-corrected and robust coefficients differ from the conventional coefficients due to the application of bias correction procedure suggested in Calonico, Cattaneo, and Titiunik (2014).[19] The robust and bias-corrected coefficients have the same values, but their respective standard errors differ. Bandwidth selection is based on Imbens and Kalyanaraman (2011). I use the results based on robust standard errors for inference.

To absorb the impact of other related factors, I first calculate the residuals by employing branch and month fixed effects. I exclude the main running variable, distance, from the above regression. The dependent variable in this first-stage regression is a dummy variable that represents loan performance. The residuals calculated in this way represent, as closely as possible, the impact of the running variable on the dependent variable. I use these residuals as the dependent variable in robust RD tests.

I report the results in panel A of Table 6. In Columns 1 and 2, I use the sample of loans made by branches located in Andhra Pradesh. A dummy variable representing default (NPA) is the dependent variable in Column 1 (2). Here, I expect to see a sharp deterioration in loan performance immediately after the cutoff. The results show an increase of 12.4 percentage points in the rate of default and an increase of 24.5 percentage points in the NPA rate at the cutoff.

In Figures 5 and 6, I present the results of an RD regression wherein I use only one stage and do not employ any control variable. In Figures 2 and 3 presented in the Online Appendix, I depict the results of RD regressions wherein I adopt a two-stage procedure by first obtaining residuals. The regressions used are local linear, and the bandwidth used is six months before and after the event in terms of the due date. Regardless of the method used, I detect a pattern of sharp deterioration in loan performance at the cutoff followed by a steady increase in the default and NPA rates.

In Columns 3 and 4 of panel A of Table 6, I estimate the RD test using the sample of loans from regions not affected by the event. As expected, I do not find any significant increase in either the default rate (in Column 3) or the NPA rate (in Column 4). From the above results, it is reasonable to conclude that only the affected loans in treatment regions experience a sharp deterioration in performance. The RD results corroborate the earlier results obtained through DID tests.

As noted before, bandwidth selection is based on Imbens and Kalyanaraman (2011). In the spirit of RD, and as a further robustness check, I limit the sample

[19] A detailed description of the procedure is given in Section 2.

**Table 6**
**Loan performance: Regression discontinuity**

*A*

| | Treatment region | | Control region | |
|---|---|---|---|---|
| Dependent variable | Default | NPA | Default | NPA |
| Robust | 0.124** | 0.245*** | 0.086 | −0.058 |
| | [2.536] | [6.282] | [0.801] | [−1.334] |
| Bias-corrected | 0.124*** | 0.245*** | 0.086 | −0.058 |
| | [3.300] | [8.157] | [0.960] | [−1.473] |
| Conventional | 0.193*** | 0.062** | 0.025 | −0.057 |
| | [5.119] | [2.057] | [0.282] | [−1.457] |
| Observations | 4,444 | 7,219 | 640 | 800 |

*B*

| Dependent variable | Default | NPA | Default | NPA |
|---|---|---|---|---|
| Robust | 0.349* | 0.425*** | 0.287 | 0.048 |
| | [1.743] | [7.067] | [1.090] | [0.898] |
| Conventional | 0.311** | 0.411*** | 0.317 | 0.052 |
| | [2.123] | [8.238] | [1.398] | [1.136] |
| Bias-corrected | 0.349** | 0.425*** | 0.287 | 0.048 |
| | [2.382] | [8.516] | [1.266] | [1.051] |
| Observations | 1801 | 3,950 | 366 | 654 |

*C*

| Dependent variable | Default | NPA | Default | NPA |
|---|---|---|---|---|
| Robust | 0.375* | 0.322*** | 0.434 | 0.221 |
| | [1.902] | [3.525] | [1.101] | [1.478] |
| Conventional | 0.275* | 0.308*** | 0.342 | 0.179 |
| | [1.782] | [3.882] | [1.004] | [1.405] |
| Bias-corrected | 0.375** | 0.322*** | 0.434 | 0.221* |
| | [2.430] | [4.057] | [1.273] | [1.736] |
| Observations | 287 | 981 | 66 | 277 |

This table reports the RD results for the impact of the microfinance regulation on loan performance. The data are organized at the loan level. In Columns 1 and 2, I consider only Andhra Pradesh branches, and in Columns 3 and 4, I consider branches located in the other two states. The RD specification estimates the significance of $E[Y_i(1)-Y_i(0)|X_i=\bar{x}]$. I use the procedure developed by Calonico, Cattaneo, and Titiunik (2014) to estimate robust and bias-corrected standard errors. The distance of a loan's due date for default (due date for NPA) from October 15, 2010, is the running variable in Columns 1 and 3 (2 and 4), and October 15, 2010, normalized to zero, serves as the cutoff. Measures of loan performance as defined in Tables 4 and 5 serve as dependent variables in different specifications. I first obtain the residuals of the regression of the dependent variable on branch and month fixed effects. I then use the residuals as the dependent variable in the RD test. In panel A, bandwidth selection is based on the method devised by Imbens and Lemieux (2008). In panel B (C) I use the bandwidth of three months (one month) on either side of the cutoff. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

to loans having due dates within three months on either side of the cutoff date. I report the results in panel B of Table 6. For an additional robustness check, I limit the bandwidth to one month. Results with the one-month bandwidth are reported in panel C.

In Columns 1 and 2 of both panels B and C, I restrict the sample to loans lent by branches located in Andhra Pradesh. In line with the earlier results, I find a sharp discontinuity in loan performance immediately after the cutoff. In non-Andhra Pradesh states, as shown in Columns 3 and 4, I do not observe any significant discontinuity. The number of observations sharply falls to 66

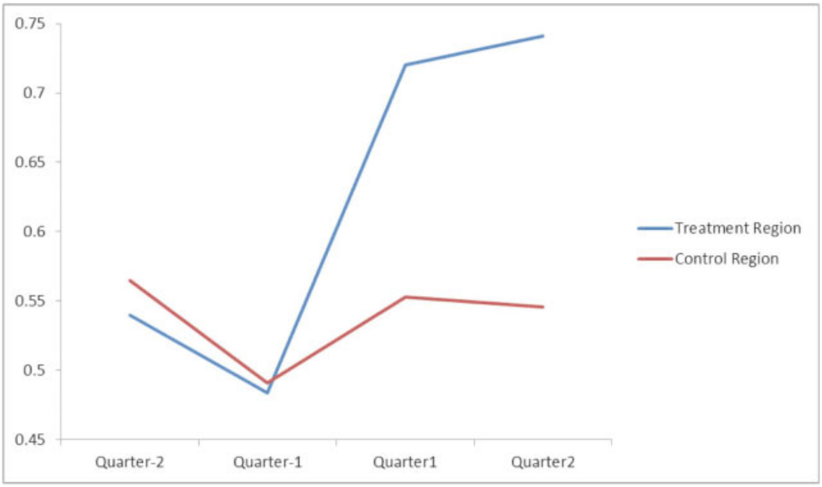


**Figure 3**
**Pre- and post-trends**
This figure compares the treatment and control regions based on pre- and post-trends. The proportion of loans in default in a quarter is the variable of interest plotted in the vertical axis. Quarter −1 (−2) refers to a period between 1 and 90 (90 and 180) days before the event date. Quarter 1 (2) refers to a period between 1 and 90 (90 and 180) days after the event date.

in Column 3 of panel C. Therefore, I do not use bandwidths closer than one month.

### 4.4 Appropriateness of the methodology

**4.4.1 Parallel trend.** A crucial prerequisite for the validity of the DID method is the existence of a parallel trend in the pre-event period with respect to the outcome of interest (Bertrand, Duflo, and Mullainathan 2004). I check for the existence of a parallel trend in two ways.

**4.4.2 Parallel trend using raw default data.** First, in Figure 3, I plot the default rates over time. I expect to see a parallel trend in default rates between Andhra Pradesh-based branches and branches located in other states for loans having due dates just before the event date.

These are unconditional numbers derived using raw data. The blue line represents loans lent by Andhra Pradesh branches, and the red line represents loans lent by branches located in the other two states. Each point in the horizontal axis represents a quarter. Quarter 1 (2) refers to the period between 0 and 90 (90 and 180) days after the event date. Similarly, quarter -1 (-2) refers to the period between 0 and 90 (90 and 180) days before the event date. A loan that has a due date in a quarter is associated with that quarter. Default rate is the ratio between the number of loans in default and the total number of loans due in a quarter. As can be seen in the figure, the default rates in the two quarters before the event move almost in parallel. The line representing the treatment region breaks out from quarter 1 and continues its upward movement in quarter 2 as well. On the other hand, the line representing the control regions does not show any significant directional movement in the post-event period. Moreover, notice that the difference between the default rates of Andhra Pradesh (treatment region) and the other two states (control regions) in the pre-event period ranges from -2% to -3%, and the same during the post-event period ranges from 15% to 18%. These numbers are broadly in line with the DID coefficient values presented in Table 4. In Figure 1 presented in the Online Appendix, I plot the default rates at monthly frequency by limiting the sample to three months before and after the event. The figure shows a parallel trend between the treatment and control regions during the pre-event period.

**4.4.3 Parallel trend using a simple regression framework.** Following, Following, Barrot (2016), I break the pre-period into subperiods. I then interact each subperiod with the treatment dummy.

I estimate the following regression equation:

$$\begin{aligned} Y_{\mathrm{itj}} = \alpha_{itj} + \beta_1 \times \mathrm{Andhra}_{ij} \times \mathrm{Pre\text{-}Period} = -3 \\ + \beta_2 \times \mathrm{Andhra}_{ij} \times \mathrm{Pre\text{-}Period} = -2 + \beta_3 \times \mathrm{Andhra}_{ij} \times \mathrm{Pre\text{-}Period} = -1 \\ + \beta_4 \times \mathrm{Andhra}_{ij} \times \mathrm{Post\text{-}Period}_{it} + \beta_5 \times \mathit{Month}_{it} + \beta_6 \times \mathit{Branch}_{ij} \\ + \beta_6 \times X_{itj} + \beta_7 * D_{tj} + \epsilon_{ijt}. \end{aligned} \tag{2}$$

The data are organized at the level of a loan $i$. The dependent variable is a dummy variable that takes the value of one if loan $i$ with a due date in a quarter $t$ and made by a branch $j$ defaults and zero otherwise. I report the results in Table 7. Each observation is a loan. The sample period spans three (six) months before and after the event date in Columns 1 and 2 (3 and 4). The *Post-period* is a dummy variable that takes the value of one for loans having due dates after the event date. In Columns 1 and 2, *Pre-period 3* is a dummy variable that takes the value of one for loans having due dates between 61 and 90 days before the event date. Similarly, *Pre-period=2 (Pre-period=1)* is a dummy variable that takes the value of one for loans having due dates between 31 and 60 (1 and 30) days before the event date. In Columns 3 and 4, the pre-period is counted in quarters. *Pre-period=2 (Pre-period=1)* is a dummy variable that takes the value of one for loans having due dates between 1 and 90 (91 and 180) days before the event date. *Andhra* is a dummy variable that takes the value of one for loans lent by Andhra Pradesh-based branches and zero otherwise. The main explanatory variables of interest are the interaction terms between the pre-periods and the Andhra dummy.

If a parallel trend exists between the treatment and control regions, I expect the interaction between the pre-event period dummies and treatment region to be statistically indistinguishable from zero. As expected, all the pre-period

**Table 7**
**Default rate: Pre- and post-trends**

| | Default | | | |
|---|---|---|---|---|
| Dependent variable | (1) | (2) | (3) | (4) |
| Andhra X Pre-period=-3 | 0.043 | 0.045 | | |
| | [0.327] | [0.342] | | |
| Andhra X Pre-period=-2 | 0.070 | 0.073 | 0.030 | −0.147 |
| | [0.898] | [0.935] | [0.551] | [−0.568] |
| Andhra X Pre-period=-3 | 0.001 | −0.000 | 0.162 | 0.156 |
| | [0.009] | [−0.001] | [1.156] | [1.106] |
| Andhra X Post-period | 0.227* | 0.231* | 0.370** | 0.402** |
| | [1.967] | [1.992] | [2.292] | [2.452] |
| Loan-level factors | No | Yes | No | Yes |
| District-level factors | No | Yes | No | Yes |
| Branch fixed effects | Yes | Yes | Yes | Yes |
| Month fixed effects | Yes | Yes | Yes | Yes |
| Observations | 2,167 | 2,167 | 3,959 | 3,959 |
| Adjusted R-squared | 0.234 | 0.235 | 0.206 | 0.209 |

In this table, I present the results of tests that examine the impact of the microfinance regulation on bank loan defaults. Specifically, I test if any pre-trend exists. Each observation is a loan. The sample period spans three (six months) before and after the event date in Columns 1 and 2 (3 and 4). The Post-period is a dummy variable that takes the value of one for loans having due dates after the event date. In Columns 1 and 2, Pre-period 3 is a dummy variable that takes the value of one for loans having due dates between 61 and 90 days before the event date and zero otherwise. Similarly, Pre-period=2 (Pre-period=1) is a dummy variable that takes the value of one for loans having due dates between 31 and 60 (1 and 30) days before the event date and zero otherwise. In Columns 3 and 4, the pre-period is counted in quarters. Pre-period=2 (Pre-period=1) is a dummy variable that takes the value of one for loans having due dates between 1 and 90 (91 and 180) days before the event date and zero otherwise. The dependent variable, default, is a dummy variable that takes the value of one if the loan under consideration defaults and zero otherwise. The explanatory variable, Andhra, is a dummy variable that takes the value of one for loans made by branches located in Andhra Pradesh and zero otherwise. Standard errors are clustered at the branch X month level and adjusted for heteroscedasticity. *t*-statistics are presented inside parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

interaction terms are statistically indistinguishable from zero. Expectedly, interaction between the Post-Period dummy and the Andhra dummy indicates an increase in the default rate in the post-event period in a DID sense.

The results from comparing the default rate using raw data and from estimating the ordinary least squares (OLS) regression equation in line with Barrot (2016) help us address the concern regarding pre-trends influencing the results.

**4.4.4 Validity of the regression discontinuity: McCrary test.** For the RD design to be valid, it is important to rule out the possibility of bunching around the cutoff, plausibly in anticipation of the event (Imbens and Lemieux 2008). I perform the test prescribed by McCrary (2008) to see whether bunching around the cutoff exists. I depict the results in Figure 4. There is no evidence of any significant bunching around the cutoff.

As noted in Section 4.3, there are differences between the samples used in RD tests that use NPA classification as the dependent variable and those that use default as the dependent variable. Therefore, I perform the McCrary (2008) test using the sample used for the NPA test. I report the results in Figure 6

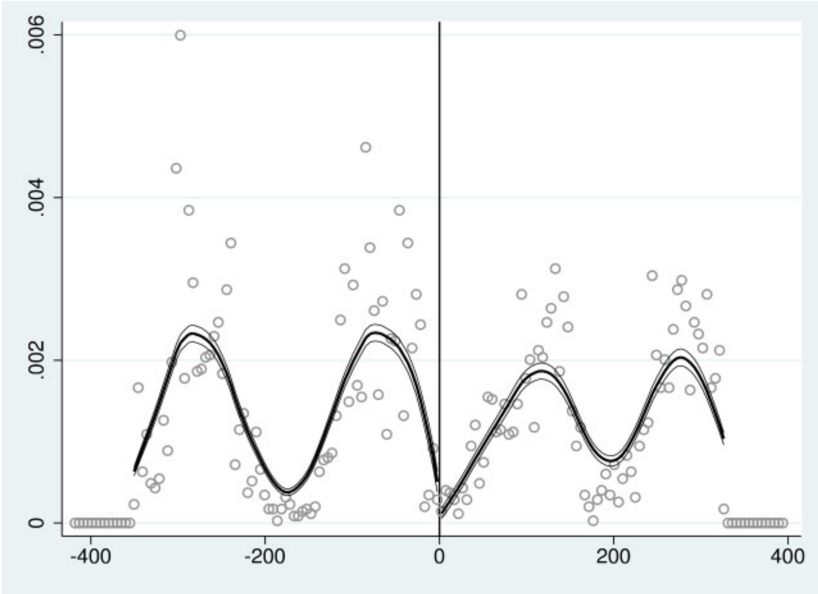


**Figure 4**
**Results of the McCrary test**
The discontinuity estimate has a coefficient of −0.28 and *t*-statistics of −0.71. The discontinuity is statistically indistinguishable from zero.

presented in the Online Appendix. I do not find any significant bunching close to the cutoff.[20]

## 4.5 Placebo tests

To address any residual concerns regarding the existence of pre-trends, I perform several placebo tests. I first estimate the DID regression Equation (1) using placebo event dates. I report the results in Table 7 presented in the Online Appendix. I consider October 15, 2009 (October 15, 2008), as the placebo event date in Columns 1 to 4 (Columns 5 to 8). All variables have the same meanings like in Equation (1). As before, I focus on the interaction between the treatment region dummy and the affected loan dummy. As shown in the table, the interaction term is statistically insignificant in all eight columns. I perform another set of placebo tests using the RD design and find similar results. Here, I use 12 placebo event dates. I use the 15th of every month from September 2010 to October 2009 as placebo event dates. I report these results in Table 8 presented in the Online Appendix. I do not find a significant jump at the cutoff after any placebo event date. Therefore, it is reasonable to conclude that the

[20] A large-scale national-level debt waiver was awarded in February 2008 and executed by the end of June 2008. I restrict the sample to loans lent after the waiver in both the above tests. This explains the difference in bandwidths in Figure 4 of the main draft and Figure 6 of the Online Appendix.

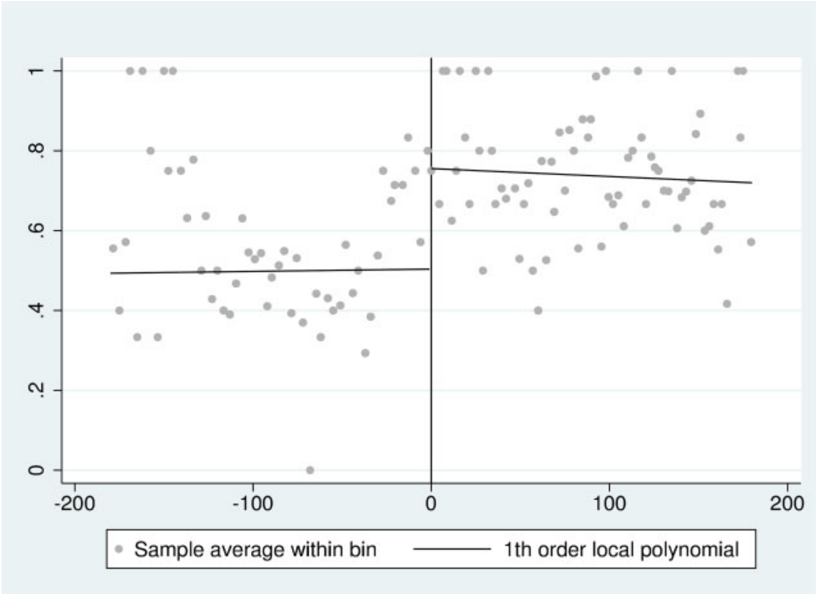


**Figure 5**
**RD plot: Impact on default**
This figure depicts the results of the RD test that examines the impact of the event on loan default rates in the treatment region. I use the method designed by Calonico, Cattaneo, and Titiunik (2014). I use October 15, 2010, as the cutoff date and the distance of a loan's due date for default from October 15, 2010, as the running variable. The RD test uses local linear regression. The economic magnitude of the discontinuity is 25.2 percentage points.

phenomenon I detect in this study is not an event that occurs every year after October 15 or every month after the 15th day.

### 4.6 Addressing identification concerns and external validity

I perform a series of robustness tests to ameliorate the concern that the results are driven by some other correlated shock that affected only Andhra Pradesh. A careful reading of local newspapers and official gazettes of the three state governments under consideration does not reveal the existence of any such shock. Nonetheless, to rule out any residual concerns, I perform additional tests.

**4.6.1 Close branch test.** All bank branches I study are located close to the borders of the respective states. I further tighten identification by limiting the sample to branches that are closest to each other on either side of the border. I include branches located in the Mahbubnagar and Medak districts of Andhra Pradesh, the Raichur district of Karnataka, and the Nanded district of Maharashtra. Mahbubnagar district shares its borders with Raichur district of Karnataka, and Medak district shares its border with Nanded district of Maharashtra.

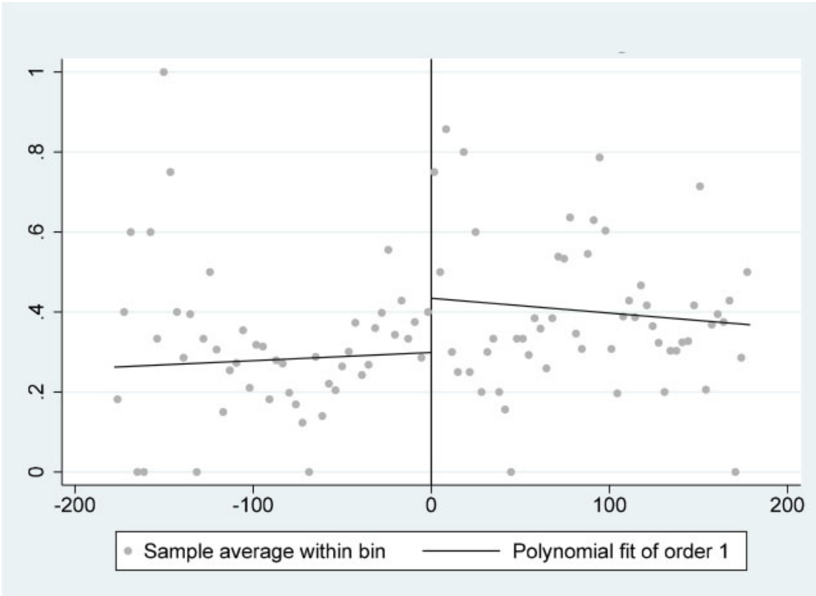


**Figure 6**
**RD plot: Impact on NPA**
This figure depicts the results of the RD test that examines the impact of the event on loan NPA rates in the treatment region. I use the method designed by Calonico, Cattaneo, and Titiunik (2014). I use October 15, 2010, as the cutoff date and the distance of a loan's due date for NPA from October 15, 2010, as the running variable. The RD test uses local linear regression. The economic magnitude of the discontinuity is 13.7 percentage points.

I estimate the DID regression Equation (1) using the above sample of nearby branches and report the results in Columns 1 to 4 of Table 8. The arrangement of rows and columns exactly mimics that of Table 4. In line with the earlier results that use the entire sample, I find that the loan default rate increases by between 22.4 to 33.6 percentage points. The economic magnitude of the coefficients of interest is slightly higher than what I find using the entire sample. However, in one of the four specifications, the coefficient of interest narrowly misses the conventional significance level of 10%. I repeat the tests using the entire sample, like in Table 4, and control for the distance from each branch to the Andhra Pradesh border. The results remain materially unchanged. I report these results in Columns 3 and 4 of Table 14 presented in the Online Appendix.

**4.6.2 External validity: Impact on banks highly exposed to Andhra Pradesh.** Admittedly, all the sample loans are made by a single government-owned bank. This fact raises a concern that the results could be driven by features peculiar to government-owned banks. However, I exploit the differences between branches of the same government-owned bank located in areas that received the treatment and branches located in other areas. Therefore, it is unlikely that the results are caused by peculiar government

**Table 8**
**Impact on default rate: Difference-in-differences test using nearby branches**

| | Default | | | |
|---|---|---|---|---|
| Dependent variable | (1) | (2) | (3) | (4) |
| Andhra X Affected loans | 0.252** | 0.225 | 0.224* | 0.336** |
| | [2.157] | [1.511] | [1.797] | [2.331] |
| Affected loans | 0.078 | 0.045 | −0.006 | 0.047 |
| | [0.528] | [0.280] | [−0.031] | [0.270] |
| Loan-level factors | No | Yes | No | Yes |
| District-level factors | No | Yes | No | Yes |
| Branch fixed effects | Yes | Yes | Yes | Yes |
| Month fixed effects | Yes | Yes | Yes | Yes |
| Observations | 942 | 942 | 2,097 | 2,097 |
| Adjusted R-squared | 0.317 | 0.321 | 0.225 | 0.253 |

In this table, I present the results of tests that examine the impact of the microfinance regulation on bank loan defaults. I restrict the sample to branches that are close to each other on either side of the border. The data are organized at the loan level. In Columns 1 and 2, the data are limited to loans whose due dates are within the range of three months before and three months after October 15, 2010, that is, July 15, 2010, to January 15, 2011. In Columns 3 and 4, the data are limited to loans whose due dates are within the range of six months before and six months after October 15, 2010, that is, April 15, 2010, to April 15, 2011. The dependent variable, default, is a dummy variable that takes the value of one if the loan under consideration defaults and zero otherwise. The explanatory variable, Andhra loans, is a dummy variable that takes the value of one for loans made by branches located in Andhra Pradesh and zero otherwise. Affected loans is also a dummy variable that takes the value of one for loans having due dates immediately after the event date and zero otherwise. The main explanatory variable is the interaction between the above two variables. I use branch and month fixed effects in all columns, and I use loan-level and district-level economic variables as controls in Columns 2 and 4. Standard errors are clustered at the branch X month level and adjusted for heteroscedasticity. $t$-statistics are presented inside parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

bank characteristics. Nonetheless, to address any residual concerns, I collect data relating to the delinquency rates of all listed banks in India for the period between 2008 and 2011. I obtain these data from the website of the Reserve Bank of India, the Indian central bank. The choice of sample period is dictated by the availability of the data. The data set includes 39 banks in total, of which, 23 are government owned and 16 are privately owned.

I estimate the following regression equation.

$$\begin{aligned} Y_{\mathrm{itj}} = \alpha_{itj} + \beta_1 \times \text{Exposure to Andhra}_i \times \text{Post}_{it} \\ + \beta_2 * \text{Exposure to Andhra}_i + \beta_3 * \text{Post}_{it} \\ + \beta_4 * Public_i + \beta_5 * X_{itj} + \epsilon_{ij}. \end{aligned} \tag{3}$$

Here, the data are organized at the bank-year level. The dependent variable is the ratio between the value of delinquent loans and that of total loans at the bank-year level. *Exposure to Andhra* is the ratio between the number of branches of a bank located in Andhra Pradesh and total number of branches at the beginning of the financial year 2010. *Post* is a dummy variable that takes the value of one for loans made in the year 2011 and zero otherwise. *Public* is a dummy variable that takes the value of one for government-owned banks and zero otherwise. Standard errors are clustered at the bank level and adjusted for heteroscedasticity. I focus on the interaction between the *Exposure To Andhra* and the *Post* variables.

**Table 9**
**External validity test using bank exposure**

| | NPA Ratio | | | |
|---|---|---|---|---|
| Dependent variable | (1) | (2) | (3) | (4) |
| Exposure X Post | 0.004* | 0.004* | 0.005** | 0.005** |
| | [1.700] | [1.694] | [2.485] | [2.162] |
| Public X Post | | | 0.486** | 0.486* |
| | | | [2.178] | [1.895] |
| Exposure | −0.011*** | −0.012*** | −0.012*** | |
| | [−2.886] | [−2.840] | [−2.850] | |
| Public | | −0.424 | −0.546 | |
| | | [−0.919] | [−1.185] | |
| Bank fixed effects | No | No | No | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes |
| Observations | 152 | 152 | 152 | 152 |
| Adjusted R-squared | 0.036 | 0.062 | 0.068 | 0.738 |

In this table, I present the results of external validity tests. The data are organized at the bank-year level. I collect information about the loan delinquency rates of all listed banks in India for the period between 2008 and 2011. The dependent variable is the ratio between NPAs and the total advances at the bank-year level. I calculate the level of exposure of a bank to Andhra Pradesh state by using the ratio of the number of bank branches located in Andhra Pradesh and total number of branches as of the beginning of the year 2010. This ratio is the independent variable, exposure. Post is a dummy variable that takes the value of one for years after the intervention and zero otherwise. Public is a dummy variable that takes the value of one for government-owned banks and zero otherwise. The main independent variable is the interaction between post and exposure. I include year fixed effects in all columns. In Column 4, I include bank fixed effects. Standard errors are clustered at the bank level and adjusted for heteroscedasticity. *t*-statistics are presented inside parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

The results are reported in Table 9. In Column 1, I include only year fixed effects and find that banks with high exposure to Andhra Pradesh experience higher delinquency in the post-intervention period. In Column 2, I include the $Public$ dummy, which absorbs time-invariant factors common to public sector banks. In Column 3, along with the $Public$ dummy, I also include the $PublicXPost$ variable, which absorbs any shock to public sector banks that is correlated with the microfinance intervention. Finally, in Column 4, I include bank and year fixed effects. Regardless of the specification used, I find that banks with high exposure to Andhra Pradesh experience a higher default rate in the post-micro-finance-intervention period. The variables $Public$ and $PublicXPost$ ensure that the phenomenon detected is not driven by factors peculiar to government-owned banks.

As a further robustness test, I check whether the impact is any different between government-owned and private banks by including a triple interaction between *Exposure To Andhra*, $Post$, and $Public$. I report the results in Table 12 presented in the Online Appendix. As shown in the table, the triple interaction is statistically insignificant. There does not seem to be a significant difference between government-owned and private banks in terms of borrower response.

**4.6.3 External validity: Using the debt waiver program of 2008.** I perform a second external validation test using the national debt waiver program for past

**Table 10**
**External validity: Using the debt waiver program of 2008**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dependent variable | Default_dummy | NPA_dummy | Default_dummy | NPA_dummy |
| Robust | 0.342*** | 0.241*** | 0.083 | −0.390 |
| | [3.898] | [3.530] | [0.271] | [−1.113] |
| Bias-corrected | 0.342*** | 0.241*** | 0.083 | −0.390 |
| | [4.656] | [4.037] | [0.336] | [−1.408] |
| Conventional | 0.333*** | 0.217*** | 0.088 | −0.384 |
| | [4.533] | [3.649] | [0.356] | [−1.388] |
| Observations | 3,956 | 3,956 | 2,719 | 2,719 |

This table reports the RD results regarding the impact of the debt waiver program of 2008 on loan performance of nonbeneficiaries. The data are organized at the loan level. The sample is drawn from all 14 branches. The RD specification estimates the significance of $E[Y_i(1)-Y_i(0)|X_i=\bar{x}]$. I use the procedure developed by Calonico, Cattaneo, and Titiunik (2014) to estimate robust and bias-corrected standard errors. In Columns 1 and 2, the distance of a loan's due date for default from February 29, 2008, is the running variable, and February 29, 2008, normalized to zero, serves as the cutoff. I use the placebo cutoff date of February 28, 2007, in Columns 3 and 4. Measures of loan performance as defined in Tables 4 and 5 serve as dependent variables in different specifications. I first obtain the residuals of the regression of the dependent variable on branch and month fixed effects. I then use these residuals as the dependent variable in the RD test. $t$-statistics are presented inside parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

defaulters announced in the year 2008.[21] Crucially, to obtain waiver benefits, it was stipulated that a borrower should have defaulted on or before December 31, 2007, a full two months before the waiver. Several studies (Kanz 2015, De and Tantri 2014) that examine the program find that the program led to significant moral hazard in loan repayment among the beneficiaries. Given the findings using the microfinance event, I hypothesize that even the nonbeneficiaries are likely to default more.

I test the above hypothesis using the regression discontinuity framework described in Section 4.3 on a sample of nonbeneficiaries. The waiver was announced on February 29, 2008. In the RD tests, I compare nonbeneficiary borrowers having due dates immediately prior to February 29, 2008 ("affected borrowers") with those having due dates immediately after February 29, 2008 ("notaffected borrowers"). As before, both categories of loans were lent before the announcement of the waiver. The event date, February 29, 2008, is the cutoff, and the distance of a loan's due date from the cutoff is the running variable.

As a prerequisite to the RD test, I perform the test recommended by McCrary (2008) and report the results in Figure 5 presented in the Online Appendix. I do not detect any significant bunching at the cutoff. I then perform the RD test and report the results in Table 10. The data are organized at the loan level. The data belong to all 14 branches located in the states of Andhra Pradesh, Karnataka, and Maharashtra.

[21] The waiver was awarded to past defaulters. Those with a landholding of less than 2 hectares received full waiver. Defaulters with a landholding of more than 2 hectares received only a partial waiver, which was limited to 25% of the total debt owed.

In Column 1, the dependent variable is the default dummy. As reported in the Table, I find that the default rate of waiver nonbeneficiaries having due dates after the waiver is 34.2 percentage points higher than that of comparable nonbeneficiaries having due dates just before the waiver. In Column 2, I use NPA status as the dependent variable and find similar results. Here, the economic magnitude of the discontinuity is 24.1 percentage points.[22] Finally, I perform placebo tests by using February 28, 2007, as the placebo event date and report the results in Columns 3 and 4 of Table 10. I do not detect any significant jump at the cutoff in these tests. These results support the thesis that the impact of the waiver spills over to nonbeneficiaries.

I present the above findings as part of the robustness exercise and not as the main results. This is because the waiver of 2008 was a nationwide program, and, hence, unlike in the case of the microfinance event, wherein I use the border regions of non-Andhra Pradesh states to calculate the counterfactual difference, it is very difficult to obtain the counterfactual difference.

**4.6.4 Other robustness tests.** I perform several other robustness tests to address any residual concerns. First, there could be a concern that the results are driven by an unobserved economic shock that is not related to the microfinance event but moved in tandem with it. The close branch test, whose results are reported in Section 4.6.1, helps in addressing this concern substantially. Nonetheless, as a further robustness check, using a separate within-Andhra Pradesh loan-level dataset, I find that the deterioration in loan performance is limited to types of loans that are known to be subject to political intervention (Cole 2009b). A general economic shock is likely to impact all sectors and not just sectors where the chances of political intervention are high. I describe these results in Section 1 of the Online Appendix and report the same in Table 1 presented in the Online Appendix. Second, I test and find that the deterioration in loan performance is higher in regions having high microfinance coverage before the event. I describe these results in Section 1.3 of the Online Appendix and report the same in Table 3 presented in the Online Appendix.

## 5. Flow of Bank Credit After the Event

### 5.1 Impact on Andhra Pradesh branches

In the second part of the study, I focus on loan officers' (banks') responses to deterioration in loan performance. I examine the impact of the intervention on the flow of credit. An important caveat that needs to be mentioned at this stage

[22] I cannot perform a test using the NPA due date as the cut-off as in Section 4.3. In other words, I cannot use the sample of borrowers whose due dates for recognition of NPA fall on either side of the cutoff date of February 29, 2008. This is because defaulting borrowers are eligible for the waiver regardless of whether or not their loans were classified as NPAs as on February 29, 2008. Therefore, I do not have a category of defaulting nonbeneficiaries whose loans are close to be classified as NPAs. Thus, I test for the long-term impact using the due date for default as the cutoff and NPA status as the dependent variable.

**Table 11**
**Loan officer response**

| | Loan amount | | | |
|---|---|---|---|---|
| | Andhra | | Non-Andhra | |
| Dependent variable | (1) | (2) | (3) | (4) |
| Robust | −0.548** | −0.479** | 0.165 | 0.132 |
| | [−2.489] | [−2.395] | [0.298] | [0.331] |
| Bias-corrected | −0.548*** | −0.479*** | 0.165 | 0.132 |
| | [−4.569] | [−3.643] | [0.643] | [0.464] |
| Conventional | −0.071 | 0.016 | −0.044 | 0.104 |
| | [−0.589] | [0.125] | [−0.170] | [0.367] |
| Observations | 774 | 635 | 549 | 376 |

This table reports the RD results regarding the impact of the microfinance regulation on the daily loan amount disbursed by the bank. The RD specification estimates the significance of $E[Y_i(1)-Y_i(0)|X_i=\bar{x}]$. I use the procedure developed by Calonico, Cattaneo, and Titiunik (2014) to estimate robust and bias-corrected standard errors. The distance from October 15, 2010, is the running variable, and October 15, 2010, normalized to zero, serves as the cutoff. The dependent variable, Loan Amount, is the natural logarithm of the daily loan amount. I first obtain the residuals of the regression of the dependent variable on branch and month fixed effects. I then use these residuals as the dependent variable in the RD test. Loans made by the Andhra Pradesh branches are covered in Columns 1 and 2. Loans made by branches located in the other two states are covered in Columns 3 and 4. $t$-statistics are presented inside parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

is that I observe only equilibrium outcomes in the post-intervention period and, hence, are unable to fully disentangle demand and supply effects. A second crucial difference between this part of the study and the previous part is that, in the previous part, the treatment loans had already been provided when the regulation was issued. In this part, I look at new loans issued after the announcement of the intervention.

The RD setup is similar to the one used in Section 4.3. The dependent variable is the natural logarithm of the daily value of loans made at the branch level. An observation represents a branch-day.

As before, I use the robust RD framework developed by Calonico, Cattaneo, and Titiunik (2014). To absorb the impact of other time-invariant and time-varying factors, I first calculate the residuals from a regression of the daily loan amount on branch and month fixed effects. I exclude the main running variable, distance, from the above regression. The residuals calculated in this way represent, as closely as possible, the impact of the running variable on the dependent variable. I use these residuals as the dependent variable in robust RD tests. I estimate the RD tests separately for the treatment and control regions.

The results are reported in Table 11. In Columns 1 and 2, I perform RD tests using loans made by the Andhra Pradesh branches. In Column 1, I include branch and month fixed effects in the first stage, whereas in Column 2, I include other loan-level and district-level covariates. The daily loan amount is lower by 47.9% to 54.8% as one crosses the discontinuity from left to right. In other words, the daily loan amount falls by nearly half immediately after the issue of the notification. This result is depicted in Figure 7. Notice a sharp fall around the cutoff. From the figure, it appears that the supply of credit did not recover to previous levels even six months after the event date.

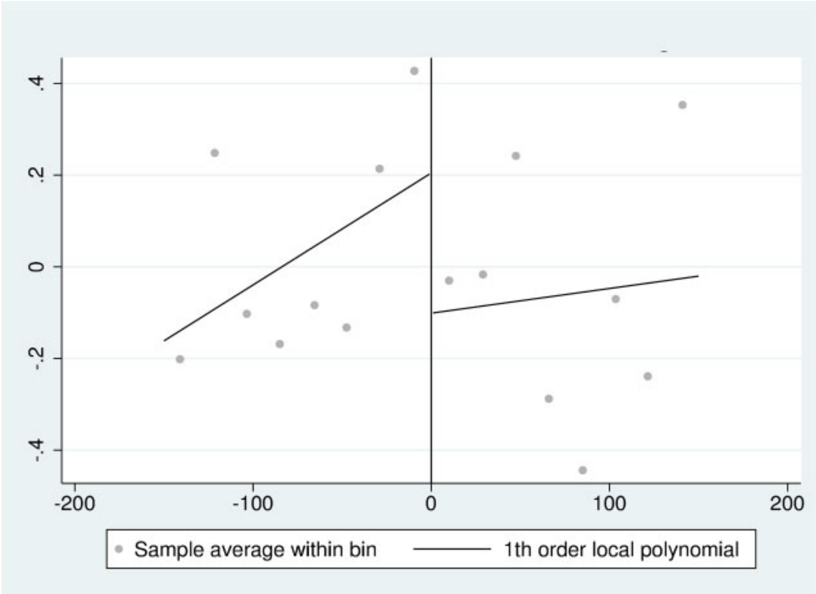


**Figure 7**
**RD plot: Impact on lending-intensive margin**
This figure depicts the results of the RD test that examines the impact of the event on the quantity of loans made in the treatment region. I use the method designed by Calonico, Cattaneo, and Titiunik (2014). I use October 15, 2010, as the cutoff date and the distance of the day under consideration from October 15, 2010, as the running variable. To absorb the impact of other related factors, I first calculate the residuals by employing branch and month fixed effects. I exclude the main running variable, distance, from the above regression. The dependent variable in this first-stage regression is the natural logarithm of the value of loans made at a branch-day level. I use the residuals as the dependent variable in robust RD tests.

A sudden and discontinuous decrease in bank credit immediately after the event indicates that the results could be driven more by supply than by demand. As discussed before, the demand side effects, if any, are likely to take time to manifest. However, it is not possible to fully rule out demand-based explanations.

## 5.2 Impact on branches located in neighboring states

To rule out the possibility that the results presented in Section 5.1 are the result of a common economic shock other than the microfinance intervention, I conduct RD tests on the sample of loans from branches that are located outside Andhra Pradesh but close to the Andhra Pradesh border. All other details about the specifications used remain the same as before. The results are reported in Columns 3 and 4 of Table 11. As reported in the table, I do not observe any significant discontinuity at the cutoff. Thus, there is unlikely to be any other shock working in tandem with the microfinance intervention.

## 6. Discussion

The results presented so far indicate that deterioration in loan performance and reduction in the flow of credit are a result of the microfinance intervention. The chances of these results being caused by some other factor are remote. In one of the robustness tests reported in Section 4.6.1, I compare branches located in districts that are very close to each other and find that the results continue to hold. It is unlikely that an economic shock from a source other than the microfinance intervention discussed in this paper would hit one side of the border and not the other side. Second, a careful newspaper search around the event did not find reports of any other economic shock hitting only Andhra Pradesh during October 2010.

I now discuss the plausible mechanisms through which the spillover effects could have spread. One possibility is that the curtailment of microfinance loans created economic distress in Andhra Pradesh (Breza and Kinnan 2016), and this distress in turn led to increased bank loan defaults. Suppose a farmer borrows from both a bank and a microfinance institution; if microfinance loans are suddenly cut off, the farmer may experience distress. Farmers may not have sufficient funds to invest in critical inputs, and, hence, their production may suffer. This may lead to a situation wherein the farmer is forced to default on bank loans as well. However, it is likely that the impact of credit constraints will take some time before the real outcomes manifest. However, I see an immediate increase in the default rate after October 15, 2010. Therefore, the real impact of the curtailment of microfinance credit is unlikely to explain the results of this study fully.

Another plausible channel is a change in bank borrowers' behavior in anticipation of government intervention in bank loans as well. Political intervention in bank loan contracts is quite common in emerging economies (Cole 2009b; Gine and Kanz 2017; Khwaja and Mian 2005; Mukherjee, Subramanian, and Tantri 2014). It is possible that borrowers considered the microfinance intervention as a signal regarding the government type and, hence, expected further intervention in bank loans. In addition, the effects of this channel are likely to manifest quickly. Borrowers who are on the verge of repaying their loans could stop repayment as soon as they learn about the intervention and understand its implications. Although this explanation certainly seems plausible, I are unable to test it given data limitations. It is difficult to detect strategic default in the absence of a survey. Even with a survey, there is no guarantee that participants will reveal their true intentions.

The moral hazard is likely to manifest quickly, while the indirect impact of economic distress caused by the virtual shutdown of microfinance is likely to manifest with a lag. Therefore, it is reasonable to infer that the results are more in line with the moral hazard argument than with the demand-side economic distress argument. At least a part of the result is caused by increased expectation of future political intervention in bank loans and change in borrower behavior in anticipation. However, it is difficult to disentangle the impact of increased expectations of the government intervention and change in borrower behavior. Loan performance is likely to deteriorate if both occur simultaneously.

This discussion also applies to the second part of the study, wherein I attempt to identify bank response in terms of subsequent credit. From the results presented in Section 5 and timing of the response, it appears reasonable to infer that the decline in credit is a supply response in anticipation of bad loan performance. It is also possible that loan officers resort to ever-greening of loans in order to limit the damage to their appraisal scores. Tantri (2017) shows that ever-greening, as identified by their measure, increased in Andhra Pradesh after the microfinance intervention. The purpose of ever-greening is to show lower defaults, and hence a discontinuous increase in ever-greening could understate the actual deterioration in loan performance and actual reduction in the flow of credit. Ever-greening also involves granting of fictitious loans to show that the previous loans have been repaid and new loans have been issued. Therefore, an increase in ever-greening also implies an overstatement of actual new credit lent. This suggests that, in reality, the flow of credit could have declined more than what I show in this study.

## 7. Conclusion

In this study, I ask if a political intervention in debt contracts in one segment of the credit market becomes contagious, thereby negatively affecting other segments of the credit market as well. The empirical setting for this study is provided by a sudden order issued by one Indian state government on October 15, 2010. The order imposed severe restrictions on the loan recovery processes to be followed by microfinance institutions. This sudden action was driven by media reports of suicides committed by microfinance borrowers, allegedly due to harsh recovery practices followed by the lenders. Using proprietary loan-level data provided to us by a bank, I test the implications of this intervention on the repayment of bank loans. I use a sample of loans made by bank branches situated outside Andhra Pradesh but close to its border to assess the counterfactual.

I find that bank loans made by Andhra Pradesh branches that were outstanding but not overdue as on October 15, 2010, defaulted significantly more when compared with similar loans made by other comparable branches of the same bank located outside Andhra Pradesh. I establish the results using the DID and RD methods. In the second part of the study, I look at the impact on new loans made after the intervention. I detect a sharp decline of about 54% in the average daily value of loans lent by Andhra Pradesh-based branches. I attribute this to the rational anticipation of adverse loan performance by loan officers. It appears that political intervention in the microfinance segment of the credit market adversely affected bank loans because of spillover effects.

This study points out an important cost of political intervention in debt contracts. By costs, I mean the increased default rate on bank loans and reduction in the flow of credit. Policymakers are likely to weigh in costs and benefits of an intervention such as the one examined in this study. I point out that it is not sufficient to only factor in the impact on borrowers who deal with the segment of the credit market that is likely to be subject to an intervention. As I show in this study, borrowers who deal with other segments of the credit market may also be affected by the intervention. Depending on the strength of the intervention and linkages, the spillover impact may vary. Therefore, it is advisable that policymakers consider the contagious effect pointed out in this study before designing any such intervention.

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