Access to Credit and Industrial Emissions*

A. Patrick Behrer[1] Piyush Gandhi[2] Teevrat Garg[3]

[1]The World Bank

[2]University of California, Santa Cruz

[3]University of California, San Diego

January 16, 2026

Abstract

A common theme in the economics of growth and development has been to identify and resolve credit constraints. Does resolving these constraints amerliorate or exacerbate other market failures? This paper examines how improved financial inclusion affects firm-level emissions by exploiting quasi-experimental variation from India's 2005 policy that incentivized banks to expand branch networks in underbanked districts. Using a difference-in-differences design comparing otherwise similar districts around eligibility thresholds, we estimate the causal impact of expanded credit access on firms' environmental performance. The policy led to substantial reductions in industrial $PM_{2.5}$ and CO_2 emissions, with no comparable changes in sectors unlikely to be affected by the reform. Evidence suggests that these gains were mediated by improvements in firm productivity and greater investment in pollution-control equipment, with effects concentrated in districts with a larger share of small firms. These findings highlight that financial inclusion policies can offer substantial co-benefits for environmental sustainability, complementing traditional environmental regulations.

Keywords: Air pollution, emissions, firms, credit access
JEL Codes:

*The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the World Bank and its affiliated organizations, or those of the Executive Directors of the World Bank or the governments they represent.

1 Introduction

Low- and middle-income countries are often characterized by credit constraints (Islam and Rodriguez Meza, 2023; Jack et al., 2016; Banerjee and Duflo, 2014) and disproportionately high levels of environmental pollution (Cui et al., 2023; Landrigan and Fuller, 2012; Suk et al., 2016), reflecting structural challenges in both financial and environmental systems. There exists substantial evidence of misallocation across firms and credit constraints in LMICs —the majority of which focuses on the consequences of misallocation for economic growth, firm performance, and labor markets (Campello and Larrain, 2016; Bai et al., 2018; Aghion et al., 2010; Alfaro and Chari, 2014; Ji et al., 2023).

In this paper, we examine the often overlooked environmental costs of misallocation and how better credit access improves firm sustainability. Andersen (2016) and Andersen (2017) provide theoretical frameworks that describe the impact of increasing access to credit on firm emissions. These frameworks demonstrate that the impact of credit access on firm emissions occurs via several channels that can have opposite signed effects. In particular, easing credit constraints can lead to higher overall pollution if firms increase their output or new firms enter the market. Conversely, it can reduce pollution if firms invest in more efficient production processes, adopt cleaner technologies to lower energy costs or if increased competition enables more productive, less polluting firms to displace lower-productivity, higher-emission producers. As a result, the net effect of expanding credit access on firm emissions is an empirical question.

We exploit quasi-experimental variation generated by the Reserve Bank of India's 2005 policy incentivizing banks to expand branch networks in underbanked districts. The policy's cut-off rule for classifying districts as underbanked (treated) or not (control) enables a difference-in-differences design to identify the causal impact of branch expansion on firms' environmental outcomes. The results show that the policy significantly lowered industrial $PM_{2.5}$ and CO_2 emissions. Evidence points to productivity gains and increased spending on pollution-control equipment as likely channels. The reductions are larger in districts

with a higher concentration of small firms, consistent with these firms having been more credit-constrained prior to the reform.

Our study relates to various strands of literature. First, it studies the impact of credit expansion on firms' environmental performance. Andersen (2016) uses cross-country variation in the timing of credit reforms to analyze the effect of increasing access to credit on firm emissions. Andersen (2017) uses cross-sectional variation in firm credit scores (as a proxy for credit access) to examine the same question. In both cases the results indicate that increasing access to credit leads to reductions in firm emissions and improves air quality. Exploiting cross-county liquidity shocks in the US, Levine et al. (2018) also find that expanding credit access reduce corporate pollution. However, access to credit might allow firms to expand production and as Bombardini and Li (2020) find the increase in output may outweigh any decline in pollution generated by producing a marginal unit more efficiently.

Our paper contributes to this literature by studying the impact of a bank branch expansion policy in a developing country context. Focusing primarily on wealthy countries may miss a key channel through which improved credit markets in LMICs could reduce pollution, addressing misallocation across firms. This is particularly important since emissions intensities tend to be higher in LMICs, external health costs are substantially larger, and it is well-known that there is substantial misallocation across firms in LMICs. For example, Hsieh and Klenow (2014) show that older firms are more plentiful and less efficient in India when compared to those in the United States. Furthermore, Indian firms appear to be substantially credit constrained (Banerjee and Duflo, 2014) and those constraints may be one cause of this misallocation. This hypothesis suggests that a consequence of the preservation of small, low-productivity firms is higher levels of pollution (Cui et al., 2016; Forslid and Okubo, 2011).

Second, this paper relates to the literature about the underlying margins of adjustment which govern reduction in air pollution. Andersen (2016) argues that facilitating access to credit may allow firms to upgrade their production technology, which typically has the

ancillary benefit of reducing per unit emissions, and shifting the composition of firms in ways that reduce economy-wide emissions. Our paper contributes evidence of another potential mechanism: that easing credit access allows firms to invest in abatement technology they had previously avoided, resulting in lower emissions.[1]

Third, our study responds to calls for greater research on how public policy can encourage firms to pursue cleaner production (Jayachandran, 2021). By providing evidence that financial inclusion policies can yield co-benefits for environmental sustainability, we highlight both additional benefits of credit expansion policies as well as "no-regret" policies for reducing industrial pollution.

Finally, this paper also contributes to a growing literature on the analysis of the 2005 Reserve Bank of India bank branch expansion policy. Young (2017) finds that bank branch expansion led to an increase in agriculture and manufacturing output in treated districts. Cramer (2021) studies this policy in the context of healthcare services and finds that branch expansion in underbanked districts led to better health outcomes for residents. Ritadhi et al. find evidence that increased physical proximity of lenders to credit constrained firms increases their capital spending. By examining the policy's impact on firms' environmental outcomes, we extend this body of work to a new and previously unexplored dimension.

2 Policy Background

India has pursued multiple strategies to promote financial inclusion, including product innovations like no-frills accounts, technological solutions such as mobile banking, and efforts to expand banking infrastructure in under-served regions. In this paper, we examine one such policy introduced by the Reserve Bank of India (RBI) in 2005, which provided incentives for banks to open branches in underbanked districts. Since the RBI regulates the approval of

[1]There are a variety of reasons why firms might choose to invest in abatement technology after a credit expansion even if there is no regulatory change accompanying the credit expansion. For example, a credit constrained firm may judge that it is worth the risk of failure to comply with existing regulations in order to deploy limited capital on productive assets. Provided access to credit the same firm may judge that the risk of regulatory penalties now out-weight the (lower) cost of accessing credit.

new bank branches in India, the policy stipulated that banks' ability to secure licenses for preferred locations was conditional on their commitment to expand in underbanked districts. RBI defined the underbanked districts as the ones which had a population to bank branch ratio higher than the national average. This policy rule generates the quasi-experimental variation that forms the basis of our identification strategy, as discussed in Section 4. Moreover, the classification of underbanked districts relied on population data from the 2001 Census, limiting the scope for strategic manipulation of the ratios used in the designation. The majority of Indian districts that did not contain a major urban center were considered underbanked for the purposes of this policy.

3 Data

We use bank branch data from RBI and district wise population from 2001 Census to reconstruct the district wise population to bank branch ratios as outlined by Cramer (2021). We use the ratio to define "underbanked" and "banked" districts for the purpose of our analysis. Within these groups we then define treatment and control based on whether districts were within a certain threshold of the cut-off. Districts above the threshold ("banked") and within the cut-off were categorized as untreated districts and those below the threshold ("underbanked") and within the cut-off were considered treated. The use of the bandwidth defined by the cut-off values ensures that we are comparing districts that are otherwise similar in the treated and control groups and not, for example, banked districts in the NCR to very rural underbanked districts.[2]

To evaluate the impact of bank branch expansion on environmental performance of firms, we use $PM_{2.5}$ and CO_2 emissions data from EDGAR database from 1996 to 2018.[3] The EDGAR data is a multi-purpose, independent global database of anthropogenic emissions

[2]This approach is akin to the growing use of difference-in-discontinuity approaches. We essentially limit our analysis to the set of districts that would be included in the bandwidth of an RD design. We show that our results are robust to a variety of bandwidth choices.

[3]Data available here: https://edgar.jrc.ec.europa.eu/emissions$_d ata_a nd_m aps$

of greenhouse gases and air pollutants. It provides consistent, IPCC-compliant estimates based on satellite observation and standard emissions factors. The database reports annual estimates of pollutant emissions, including $PM_{2.5}$ and CO_2, disaggregated by economic sector and mapped to a uniform global grid at a $0.01^{o} \times 0.01^{o}$ spatial resolution (approximately 1 $\times$ 1 km at the equator). We collect data on emissions sources by economic sector, such as industrial emissions. For robustness checks, we also use emissions data from sectors which are arguably unaffected by the policy change such as the energy sector and agriculture. Furthermore, we also use monthly $PM_{2.5}$ concentration data from Van Donkelaar et al. (2019) as a robustness check for our results. The van Donkelaar data differs from the EDGAR database as it provides data on $PM_{2.5}$ concentrations (a stock) and not emissions (a flow).

To examine the mechanisms driving the changes in firm level emissions, we use data from Annual Survey of Industries (ASI) from 2000-2010. ASI covers all registered factories and contains data on firm investment, input, output, employment and working capital. Lastly, we also use data from three rounds of Economic Census conducted in 1998, 2005 and 2013. It allows us to test heterogeneity of the effects based on firm size.

4 Empirical approach

The implementation of the policy using a cut-off clearly classifies districts as treated (underbanked) and untreated (banked). This allows us to employ the following difference-in-differences estimation:

$$Y_{it} = \delta_0 + \delta_1 \cdot Treatment_i + \delta_2 \cdot Post_t + \beta \cdot (Treatment_i \times Post_t) + \alpha_i + \gamma_t + \lambda_s \cdot t + \varepsilon_{it}$$

where Y_{it} is average total emissions for district i in year t. $Treatment_i$ is the indicator for whether district i is in the treated group. $Post_t$ represents the indicator for the post-treatment period. and $Treatment_i \times Post_t$ is the interaction term capturing the difference-

in-differences effect. α_i and γ_t are district and year fixed effects, respectively. $\lambda_s \cdot t$ is the $State \times Year$ trend and lastly ε_{it} is the error term. We cluster the standard errors at the district level, the level at which the policy was implemented. Our estimates are based on the approach described in Callaway and Sant'Anna (2021) and implemented using their Stata package `csdid`.

The identification assumption for the causal interpretation of β is that the outcomes across treated and untreated districts would have followed parallel trends in the absence of credit expansion policy. We complement our DiD approach with an event study design, which also enables us to test whether treated and control districts exhibited similar trends prior to the policy:

$$Y_{it} = \delta_0 + \delta_1 \cdot Treatment_i + \delta_2 \cdot Post_t + \sum_{k=-10}^{13} \beta_k \cdot (Treatment_i \times Post_t) + \alpha_i + \gamma_t + \lambda_s \cdot t + \varepsilon_{it}$$

If the outcome trends followed a similar trajectory during pre-period, we would not reject the null hypothesis of zero effects for all the pre-period year coefficients. In our main specification, we restrict the analysis to districts with a population to bank branch ratio within +/-2,500 of the national cut-off. We show that the results hold qualitatively using alternate bandwidths.

5 Results

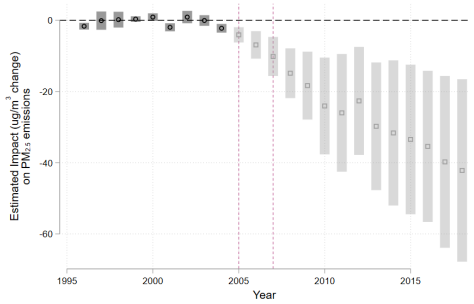
5.1 $PM_{2.5}$ results

We find that after the implementation of the credit expansion, emission rates from industry in the treated districts decreased substantially compared to those in the non-treated units in the immediate vicinity of the treatment threshold. This is true whether we measure emissions based on the EDGAR industry classification (manufacturing of metals, electronics,

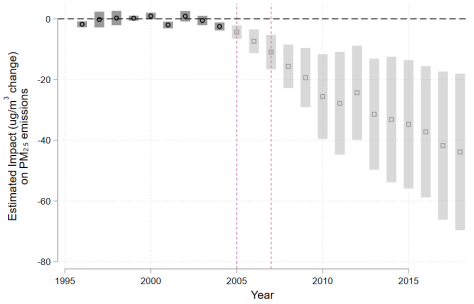
machinery, etc) or a broader classification of industrial activity which includes food, paper, chemicals, etc. (Figure 1).

In contrast, we see no meaningful change in emissions from energy combustion (Figure 1). This is consistent with energy generation primarily occurring at large, potentially state-adjacent firms whose access to credit is plausibly unaffected by the reform. There is a slight increase in emissions from energy production (non-significant) in later years. This is consistent with evidence from ASI data that firms install pollution control technology, so reduce their emissions, but increase production after the implementation of the policy.

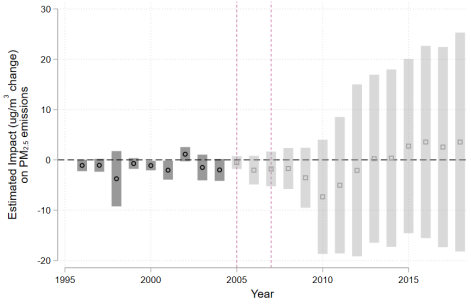
FIGURE 1: Impact of credit expansion on $PM_{2.5}$ emissions



(A) Manufacturing Industrial $PM_{2.5}$ Emissions



(B) Combined Industrial $PM_{2.5}$ Emissions



(C) Energy Sector $PM_{2.5}$ Emissions

NOTES: We plot the change in emissions in the post period for emissions that EDGAR classifies as "industrial" (Panel A), a broader definition of manufacturing emissions based on the classification in the ASI (Panel B), and energy generation (Panel C) in the treated districts relative to the non-treated units where the change is measured relative to all the pre-periods. Our estimates are based on the approach described in Callaway and Sant'Anna (2021) and implemented using their STATA command `csdid`. Treatment is defined as being in a district that was above the threshold being considered "underbanked" by the RBI. We only include districts that were within a bandwidth of 2,500 around the population per bank branch threshold. Our definition of "industrial" and "energy" is based on the categorization in the raw EDGAR data. We generate overall manufacturing emissions measures by combining emissions rates for all sources articulated in the EDGAR data. Full details of the regressions are provided in Table 1.

Table 1 presents the coefficients for the difference-in-differences specification for $PM_{2.5}$ emissions. Column 1 shows the effect of the RBI policy on EDGAR defined industrial $PM_{2.5}$ emissions (manufacturing of metals, electronics, machinery, etc.), column 2 shows the effect on a broader definition of industrial sector (including food, paper, chemicals, etc.) and column 3 shows the effect on energy sector emissions as defined by the EDGAR dataset. We find that the policy caused a reduction in annual industrial $PM_{2.5}$ emissions by 24 kilo-tonnes (kt) in treated districts as compared to the control districts. The effects remain similar if we use a broader definition of industrial emissions. However, the annual $PM_{2.5}$ emissions from energy sector reduced by only 0.79 kt (statistically insignificant). As stated earlier, this aligns with the fact that energy generation is concentrated among large, often state-affiliated firms, whose access to credit is likely unaffected by the reform.

TABLE 1: Impact of credit expansion on industrial & energy $PM_{2.5}$ emissions

	Industrial emissions	Combined emissions	Energy emissions
Post-RBI policy	-24.23	-25.54	-0.79
	(7.31)	(7.43)	(5.79)
95% CI	[-38.56,-9.90]	[-40.11,-10.97]	[-12.14,10.55]
N	3960	3960	3960
Unique Districts	233	233	233
Fixed Effects:			
District	Yes	Yes	Yes
Year	Yes	Yes	Yes
State × Year Trend	Yes	Yes	Yes

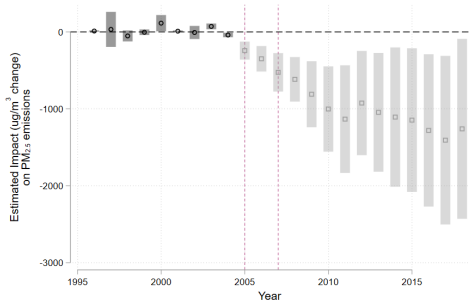
NOTES: Our outcome is the average total quantity of emissions in the district. The source of emissions varies across the columns. Coefficients indicate the change in emissions after the implementation of the credit reform. Errors are clustered at the district level. Our estimates are based on the approach described in Callaway and Sant'Anna (2021) and implemented using their Stata package `csdid`.

5.2 CO_2 results

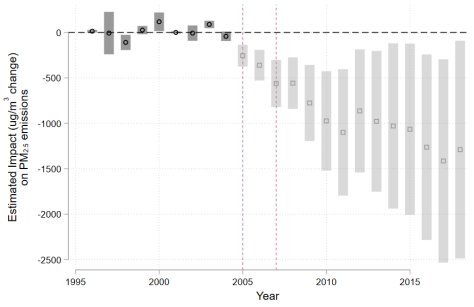
In this section, we examine the impact of credit expansion on global pollutants, focusing specifically on CO_2 emissions. We find that after the implementation of the credit expansion,

emission rates from industry in the treated districts decreased substantially compared to those in the non-treated units in the immediate vicinity of the treatment threshold. This is true whether we measure emissions based on the EDGAR industry classification or a broader classification of industrial activity (Figure 1). Similar to the results for $PM_{2.5}$ we see no meaningful change in emissions from energy combustion.

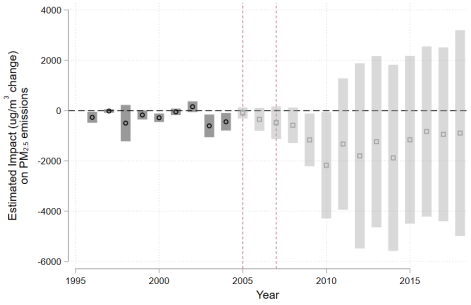
FIGURE 2: Impact of credit expansion on CO_2 emissions



(A) Manufacturing Industrial CO_2 Emissions



(B) Combined Industrial CO_2 Emissions



(C) Energy Sector CO_2 Emissions

NOTES:We plot the change in emissions in the post period emissions that EDGAR classifies as "industrial" (Panel A), a broader definition of manufacturing emissions based on the classification in the ASI (Panel B), and energy generation (Panel C) in the treated districts relative to the non-treated units where the change is measured relative to all the pre-periods. Our estimates are based on the approach described in Callaway and Sant'Anna (2021) and implemented using their STATA command `csdid`. Treatment is defined as being in a district that was above the threshold being considered "underbanked" by the RBI. We only include districts that were within a bandwidth of 2,500 around the population per bank branch threshold. Our definition of "industrial" and "energy" is based on the categorization in the raw EDGAR data. We generate overall manufacturing emissions measures by combining emissions rates for all sources articulated in the EDGAR data. Full details of the regressions are provided in Table 2.

Table 2 presents the coefficients for the impact of credit expansion on CO_2 emissions. Column 1 shows that the policy reduced annual CO_2 emissions in treated districts by over 900 tons as compared to the non-treated districts. The results are similar for a broader definition of industrial activity (column 2). While the coefficient for energy sector is negative,

the confidence intervals are too wide to rule out a null effect.

TABLE 2: Impact of credit expansion on industrial & energy CO_2 emissions, all treated units

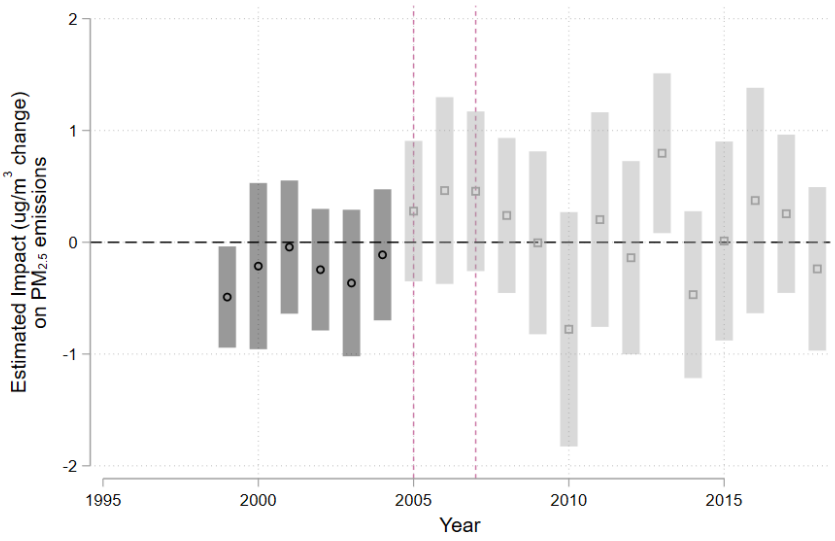
	Industrial emissions	Combined emissions	Energy emissions
Post-RBI policy	-917.77	-891.43	-1066.11
	(324.13)	(326.67)	(1113.98)
95% CI	[-1553.04,-282.49]	[-1531.68,-251.17]	[-3249.47,1117.26]
N	3984	3984	3984
Unique Districts	234	234	234
Fixed Effects:			
District	Yes	Yes	Yes
Year	Yes	Yes	Yes
State × Year Trend	Yes	Yes	Yes

NOTES: Our outcome is the average total quantity of emissions in the district. The source of emissions varies across the columns. Coefficients indicate the change in emissions after the implementation of the credit reform. Errors are clustered at the district level. Our estimates are based on the approach described in Callaway and Sant'Anna (2021) and implemented using their Stata package `csdid`.

5.3 Impacts on $PM_{2.5}$ concentrations

We do not find that the policy had a meaningful impact on $PM_{2.5}$ concentrations as measured by the Van Donkelaar research group (Van Donkelaar et al., 2019) (Figure 3). This is not surprising. We document effects on emissions of one industry in some districts. PM concentrations are determined by total emissions —and we show, for example that energy emissions in treated districts remain broadly unchanged —as well as transport of particulates from non-treated districts (and non-treated countries). The transport of particulates from other locations is a significant source of the total PM burden in South Asia, with some estimates suggesting at least 35% of particulate concentrations in India are attributable to sources outside of India (Mani et al., 2022).

FIGURE 3: Effects on $PM_{2.5}$ concentrations



NOTES: We plot the effect from our primary specification replacing EDGAR emissions as the outcome with $PM_{2.5}$ as measured by the Van Donkelaar research group (Van Donkelaar et al., 2019). We aggregate monthly averages from Van Donkelaar group to annual district averages. The Van Donkelaar data is only available beginning in 1998.

5.4 Firm size heterogeneity

A reasonable hypothesis about credit access is that larger firms have better access to credit prior to the implementation of the treatment. This could be true for a variety of reasons -

large firms may be more sophisticated, more connected to business or political networks, or more likely to have physical assets that can be pledged as collateral (Campello and Larrain, 2016). Regardless of the specific reason however, if large firms do have better access to credit we would expect to see smaller effects of the credit expansion on their activities. While we cannot observe effects on individual firms, or their size, we can test this indirectly be examining aggregate effects across districts with more or fewer of each type of firm. To do so we rely on counts of firms in various size bins from the 2005 Economic Census.

We find that the largest effects are in districts that are in the top quartile of the distribution of small firms (< 20 employees)(Table 3). Effects in these districts are 19 units (56%) larger than those in districts in the top quartile of the distribution of very large firms (>101 employees). Effects are also smaller by 5.5 units (25%) in those districts in the bottom three quartiles of the small firm distribution than those in the bottom three quartiles of the very large firms distribution. Coefficients in both the cases that indicate more large firms – i.e. districts in the top quartile based on the number of very large firms and those in the bottom three quartiles of the count of small firms – are not statistically different from zero.

Comparing our point estimates in places with many very large firms and those with many small firms directly we cannot reject that they are the same, as a consequence of large standard errors. However, our point estimates are larger and more precisely estimated in places with more small firms. That suggests the expansion of credit may have had more of an impact on small firms than very large, consistent with the hypothesis that credit constraints are more binding for small firms.

5.5 Potential Mechanisms

One possible mechanism by which improved access to credit may lead to reduce emissions is through increasing firm productivity. Previous research has highlighted an inverse relationship between firm productivity and emissions intensity (Barrows and Ollivier, 2018; Cui

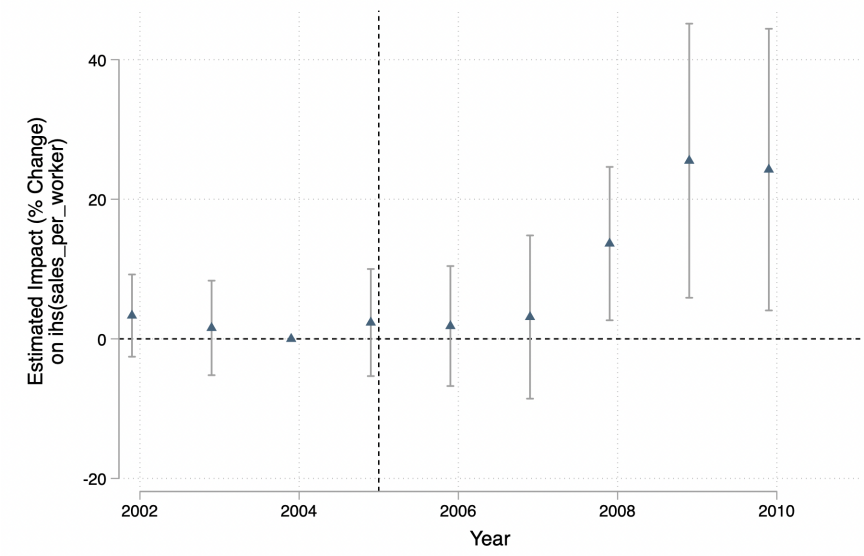
TABLE 3: Impact of credit expansion on industrial $PM_{2.5}$ emissions by firm size

	Very large	Large	Medium	Small
(A) Above 75th percentile				
Post-RBI policy	-34.11	-47.34	-52.39	-53.26
	(19.69)	(19.09)	(28.92)	(19.65)
95% CI	[-72.71,4.49]	[-84.77,-9.92]	[-109.08,4.30]	[-91.77,-14.74]
N	1008	1008	1008	1008
Unique Districts	59	59	59	59
(B) Below 75th percentile				
Post-RBI policy	-21.52	-15.67	-12.03	-15.95
	(9.45)	(7.96)	(8.09)	(10.34)
95% CI	[-40.04,-3.00]	[-31.27,-0.07]	[-27.89,3.83]	[-36.22,4.32]
N	2952	2952	2952	2952
Unique Districts	174	174	174	174
Fixed Effects:				
District	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
State × Year Trend	Yes	Yes	Yes	Yes

NOTES: Our outcome is the average total quantity of emissions in the district. The source of emissions varies across the columns. Coefficients indicate the change in emissions after the implementation of the credit reform. Errors are clustered at the district level. Very large firms are those with more than 101 employees, large have between 50 and 100, medium have between 20 and 50, and small have less than 20. Our estimates are based on the approach described in Callaway and Sant'Anna (2021) and implemented using their Stata package `csdid`.

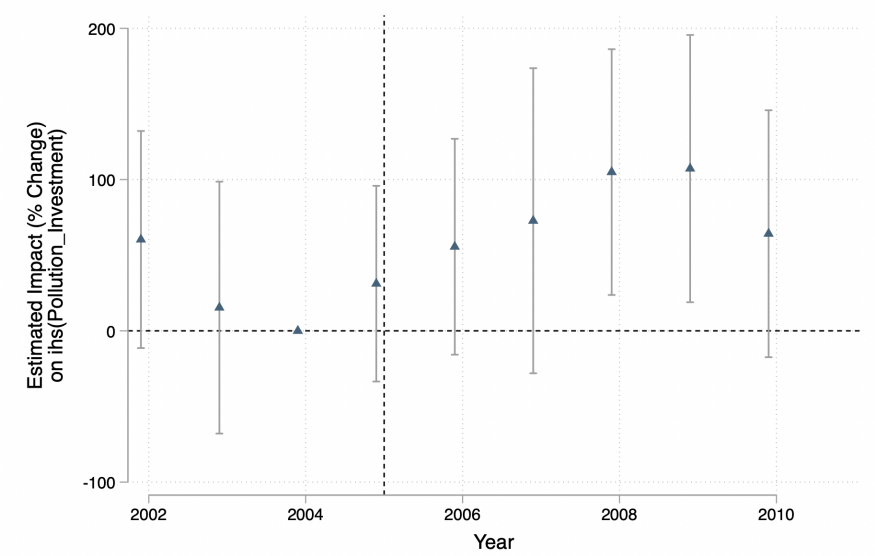
et al., 2016). If credit expansion increases firm productivity, this channel could contribute to lowering overall emissions. One way, among others, for a credit expansion to increase firm productivity is to facilitate investment in more modern production technology. We use ASI data to test this hypothesis using sales per worker as a proxy of firm efficiency. Using an inverse hyperbolic sine transformation, we see suggestive evidence that firms in treated districts become more productive as compared to the firms in untreated districts (Figure 4).

FIGURE 4: IHS of Sales per Worker



Another possible mechanism is that more productive firms make higher abatement investments (Forslid and Okubo, 2011), and as a consequence, have lower emission intensity. We test the plausibility of this mechanism in our context. Using inverse hyperbolic sine transformation of investment in pollution control equipment, reported in the ASI, we find some evidence that firms in treated districts increase the investment in abatement technology post the credit expansion policy (Figure 5). These estimates are noisy but provide suggestive evidence that access to credit allows firms to undertake investments in direct abatement technology that makes production cleaner regardless of other efficiency gains due to any investment in improved production technology.

FIGURE 5: IHS of Pollution Control Equipment



These two potential mechanisms are complementary and may both be at play. Given more access to credit firms may invest in better more modern production technology that improves efficiencies and has the ancillary benefit of reducing per unit pollution. They may also invest in abatement technology either because that technology is coupled with the newer production technology, because they were under-investing in abatement technology relative to their preferred level when credit constrained, or because they wish to expand into markets that have cleaner production requirements (e.g. export markets). Without the ability to see into specific firms we are limited in the extent to which we can differentiate between these possibilities.

5.6 Placebo Tests and Robustness Checks

As a placebo test of the EDGAR emissions data we examine the impact of the credit expansion on $PM_{2.5}$ emissions from refining and transformation (i.e. oil, gas, and chemicals production). Like energy this is an industry dominated by large, capital intensive firms that likely had access to credit prior to the policy and so are plausibly unaffected by the credit expansion. We also study the impacts on agricultural waste burning, an industry potentially

impacted in ambiguous ways.[4] We do this to verify that our effects are not the mechanical consequence of systematic modeling choices made by the EDGAR team. If that were the case we would expect to see similar declines in emissions in these placebo tests as we see in our primary results.[5] We do not observe such a pattern. In both tests the aggregate effect of the policy is no change in emissions in either industry (Table 4).

TABLE 4: Impact of credit expansion on placebo $PM_{2.5}$ emissions

	Refining & Transformation	Agricultural waste burning
Post-RBI policy	-0.22	-0.62
	(0.17)	(0.61)
95% CI	[-0.55,0.10]	[-1.81,0.57]
N	3960	3960
Unique Districts	233	233
Fixed Effects:		
District	Yes	Yes
Year	Yes	Yes
State × Year Trend	Yes	Yes

NOTES: Our outcomes are the average total quantity of emissions from refining and transformation and agricultural waste burning. in the district. Coefficients indicate the change in emissions after the implementation of the credit reform. Errors are clustered at the district level. Our estimates are based on the approach described in Callaway and Sant'Anna (2021) and implemented using their Stata package `csdid`.

We also examine if our results are robust to alternative bandwidths. Tables A1 and A2 present regression results for $PM_{2.5}$ and CO_2 emissions using alternative bandwidths of 2,000 and 3,000 around the cut-off. The results hold qualitatively same when we use alternative cut-offs. Specifically when using the 2,000 bandwidth, the magnitude and confidence intervals remain stable. With the 3,000 bandwidth, the coefficient signs remain unchanged, but

[4]Evidence on the importance of credit constraints in driving crop burning is sparse but Jack et al. (2025) find that farmers report lack of cash on hand as a major factor in their crop residue management strategies. They also find that providing PES payments not to burn with payment occurring up-front leads to larger reductions in burning than PES with payments only upon completion. On the other hand, the current legal regime around crop burning in India provides little reason for farmers to invest in alternatives to burning even if credit was readily accessible and access to credit might facilitate the adoption of mechanized harvesting, which generally leads to more burning.

[5]This approach does not rule out that there are sector specific modeling choices that impact our results, just that there is not a systemic bias downwards in emissions in EDGAR that is driving our results.

wider confidence intervals make them statistically indistinguishable from zero. Figures A1 and A2 present the event study for $PM_{2.5}$ and CO_2 emissions respectively, using alternate bandwidths of 2,000 and 3,000. As seen from the figures, results hold qualitatively for both emissions, though they become slightly imprecise with a bandwidth choice of 3,000.

6 Conclusion

This paper provides new evidence on the environmental consequences of expanding credit access in the context of India's 2005 Reserve Bank of India branch expansion policy. We find that increased credit availability led to substantial reductions in industrial $PM_{2.5}$ and CO_2 emissions. Our analysis suggests that these environmental gains are plausibly mediated by improvements in firm productivity and greater investment in pollution-control technologies. These changes are consistent with the idea that easing credit constraints allows firms to upgrade production processes and reduce emissions intensity. We also document heterogeneity by firm size, with the largest effects observed in districts with a greater share of small firms, consistent with these firms facing tighter pre-reform credit constraints.

From a policy perspective, the results highlight the potential for financial inclusion policies to deliver co-benefits for environmental sustainability in low- and middle-income countries. While the primary motivation for branch expansion was to broaden financial access, the observed environmental improvements suggest that credit market reforms can complement, rather than substitute for, dedicated environmental policy.

References

Aghion, Philippe, George-Marios Angeletos, Abhijit Banerjee, and Kalina Manova (2010), "Volatility and growth: Credit constraints and the composition of investment." *Journal of Monetary Economics*, 57, 246–265.

Alfaro, Laura and Anusha Chari (2014), "Deregulation, misallocation, and size: Evidence from india." *The Journal of Law and Economics*, 57, 897–936.

Andersen, Dana C (2016), "Credit constraints, technology upgrading, and the environment." *Journal of the Association of Environmental and Resource Economists*, 3, 283–319.

Andersen, Dana C (2017), "Do credit constraints favor dirty production? theory and plant-level evidence." *Journal of Environmental Economics and Management*, 84, 189–208.

Bai, John, Daniel Carvalho, and Gordon M Phillips (2018), "The impact of bank credit on labor reallocation and aggregate industry productivity." *The Journal of Finance*, 73, 2787–2836.

Banerjee, Abhijit V and Esther Duflo (2014), "Do firms want to borrow more? testing credit constraints using a directed lending program." *Review of Economic Studies*, 81, 572–607.

Barrows, Geoffrey and Hélène Ollivier (2018), "Cleaner firms or cleaner products? how product mix shapes emission intensity from manufacturing." *Journal of Environmental Economics and Management*, 88, 134–158.

Bombardini, Matilde and Bingjing Li (2020), "Trade, pollution and mortality in china." *Journal of International Economics*, 125, 103321.

Callaway, Brantly and Pedro HC Sant'Anna (2021), "Difference-in-differences with multiple time periods." *Journal of econometrics*, 225, 200–230.

Campello, Murillo and Mauricio Larrain (2016), "Enlarging the contracting space: Collateral menus, access to credit, and economic activity." *The Review of Financial Studies*, 29, 349–383.

Cramer, Kim Fe (2021), "Financial development and health." *Available at SSRN 3917526*.

Cui, Can, Shuping Li, Weichen Zhao, Binyuan Liu, Yuli Shan, and Dabo Guan (2023), "Energy-related co 2 emission accounts and datasets for 40 emerging economies in 2010–2019." *Earth System Science Data*, 15, 1317–1328.

Cui, Jingbo, Harvey Lapan, and GianCarlo Moschini (2016), "Productivity, export, and environmental performance: air pollutants in the united states." *American Journal of Agricultural Economics*, 98, 447–467.

Forslid, Rikard and CEPR Toshihiro Okubo (2011), "Why are firms that export cleaner? international trade, co2 emissions and heterogeneous firms."

Hsieh, Chang-Tai and Peter J Klenow (2014), "The life cycle of plants in india and mexico." *The Quarterly Journal of Economics*, 129, 1035–1084.

Islam, Asif Mohammed and Jorge Luis Rodriguez Meza (2023), "How prevalent are credit-constrained firms in the formal private sector? evidence using global surveys." Technical report, The World Bank.

Jack, B Kelsey, Seema Jayachandran, Namrata Kala, and Rohini Pande (2025), "Money (not) to burn: payments for ecosystem services to reduce crop residue burning." *American Economic Review: Insights*, 7, 39–55.

Jack, William, Michael Kremer, Joost De Laat, and Tavneet Suri (2016), "Borrowing requirements, credit access, and adverse selection: Evidence from kenya." Technical report, National Bureau of Economic Research.

Jayachandran, Seema (2021), "How economic development influences the environment." Working Paper 29191, National Bureau of Economic Research, URL `http://www.nber.org/papers/w29191`.

Ji, Yan, Songyuan Teng, and Robert M Townsend (2023), "Dynamic bank expansion: spatial growth, financial access, and inequality." *Journal of Political Economy*, 131, 2209–2275.

Landrigan, Philip J and RICHARD Fuller (2012), "Environmental pollution: an enormous and invisible burden on health systems in low-and middle-income counties." *World Hosp Health Serv*, 50, 35.

Levine, Ross, Chen Lin, Zigan Wang, and Wensi Xie (2018), "Bank liquidity, credit supply, and the environment." Technical report, National Bureau of Economic Research.

Mani, Muthukumara, Hans Timmer, Martin Raiser, Jostein Nygard, Aiga Stokenberga, Nadia Sharmin, Margaret Triyana, Siddharth Sharma, Martin Heger, Adanna Chukwuma, Michael Norton, Shamemmur Rahman, Isabel Maria Ramos Tellez, Zara Ali, Utkarsh Saxena, and Ahmad Imran Aslam (2022), "Striving for clean air; air pollution and public health in south asia." Technical report.

Ritadhi, SK, Nirupama Kulkarni, and Kanika Mahajan (????), "Banking the underbanked: Capital investment and credit-constrained firms." *Available at SSRN 5242510*.

Suk, William A, Hamid Ahanchian, Kwadwo Ansong Asante, David O Carpenter, Fernando Diaz-Barriga, Eun-Hee Ha, Xia Huo, Malcolm King, Mathuros Ruchirawat, Emerson R da Silva, et al. (2016), "Environmental pollution: an under-recognized threat to children's health, especially in low-and middle-income countries." *Environmental health perspectives*, 124, A41–A45.

Van Donkelaar, Aaron, Randall V Martin, Chi Li, and Richard T Burnett (2019), "Regional estimates of chemical composition of fine particulate matter using a combined geoscience-statistical method with information from satellites, models, and monitors." *Environmental science & technology*, 53, 2595–2611.

Young, Nathaniel (2017), "Banking and growth: Evidence from a regression discontinuity analysis."

For Online Publication
Appendix: Credit Access& Pollution

Appendix 1 Additional Tables

TABLE A1: Impact of credit expansion on industrial & energy $PM_{2.5}$ emissions, robustness to bandwidth

	Industrial emissions	Combined emissions	Energy emissions
(A) Bandwidth of 3000			
Post-RBI policy	-16.42	-17.86	-2.75
	(9.28)	(9.37)	(5.35)
95% CI	[-34.60,1.77]	[-36.22,0.50]	[-13.22,7.73]
N	4680	4680	4680
Unique Districts	275	275	275
(B) Bandwidth of 2000			
Post-RBI policy	-23.72	-24.85	0.63
	(8.59)	(8.78)	(7.79)
95% CI	[-40.57,-6.88]	[-42.06,-7.64]	[-14.63,15.90]
N	3048	3048	3048
Unique Districts	179	179	179
Fixed Effects:			
District	Yes	Yes	Yes
Year	Yes	Yes	Yes
State × Year Trend	Yes	Yes	Yes

NOTES: Our outcome is the average total quantity of emissions in the district. The source of emissions varies across the columns. Coefficients indicate the change in emissions after the implementation of the credit reform. Errors are clustered at the district level. Our estimates are based on the approach described in Callaway and Sant'Anna (2021) and implemented using their Stata package `csdid`.

TABLE A2: Impact of credit expansion on industrial & energy CO_2 emissions, robustness to bandwidth

	Industrial emissions	Combined emissions	Energy emissions
(A) Bandwidth of 3000			
Post-RBI policy	-532.90	-591.77	-1093.04
	(465.95)	(456.62)	(1086.92)
95% CI	[-1446.14,380.34]	[-1486.72,303.18]	[-3223.37,1037.29]
N	4704	4704	4704
Unique Districts	277	277	277
(B) Bandwidth of 2000			
Post-RBI policy	-919.90	-867.41	-881.15
	(367.83)	(372.95)	(1292.77)
95% CI	[-1640.83,-198.97]	[-1598.38,-136.44]	[-3414.92,1652.63]
N	3072	3072	3072
Unique Districts	181	181	181
Fixed Effects:			
District	Yes	Yes	Yes
Year	Yes	Yes	Yes
State × Year Trend	Yes	Yes	Yes

NOTES: Our outcome is the average total quantity of emissions in the district. The source of emissions varies across the columns. Coefficients indicate the change in emissions after the implementation of the credit reform. Errors are clustered at the district level. Our estimates are based on the approach described in Callaway and Sant'Anna (2021) and implemented using their Stata package `csdid`.

TABLE A3: Impact of credit expansion on industrial $PM_{2.5}$ emissions by firm size

	High,Treated	High,Untreated	Low,Treated	Low,Untreated
Post-RBI policy	-24.56	-24.45	-23.89	-26.90
	(7.57)	(13.05)	(7.53)	(7.85)
95% CI	[-39.40,-9.72]	[-50.02,1.12]	[-38.66,-9.13]	[-42.29,-11.51]
N	3960	3960	3960	3960
Unique Districts	233	233	233	233
Fixed Effects:				
District	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
State × Year Trend	Yes	Yes	Yes	Yes

NOTES: Our outcome is the average total quantity of emissions in the district. The source of emissions varies across the columns. Coefficients indicate the change in emissions after the implementation of the credit reform. Errors are clustered at the district level. Very large firms are those with more than 101 employees, large have between 50 and 100, medium have between 20 and 50, and small have less than 20. Our estimates are based on the approach described in Callaway and Sant'Anna (2021) and implemented using their Stata package `csdid`.

TABLE A4: Impact of credit expansion on industrial $PM_{2.5}$ emissions by firm size

	High	Low
Post-RBI policy	-42.09	-54.91
	(22.19)	(11.44)
95% CI	[-85.58,1.40]	[-77.33,-32.49]
N	3960	3960
Unique Districts	233	233
Fixed Effects:		
District	Yes	Yes
Year	Yes	Yes
State × Year Trend	Yes	Yes

NOTES: Our outcome is the average total quantity of emissions in the district. The source of emissions varies across the columns. Coefficients indicate the change in emissions after the implementation of the credit reform. Errors are clustered at the district level. Very large firms are those with more than 101 employees, large have between 50 and 100, medium have between 20 and 50, and small have less than 20. Our estimates are based on the approach described in Callaway and Sant'Anna (2021) and implemented using their Stata package `csdid`.

TABLE A5: Impact of credit expansion on industrial & energy $PM_{2.5}$ emissions [REGHDFE]

	Industrial emissions	Combined emissions	Energy emissions
post=1 × treat=1	-20.047**	-21.378**	-5.907
	(8.583)	(8.747)	(6.399)
N	3,984	3,984	3,984
Fixed Effects:			
District	Yes	Yes	Yes
Year	Yes	Yes	Yes
State × Year Trend	Yes	Yes	Yes

NOTES: Our outcome is the average total quantity of emissions in the district. The source of emissions varies across the columns. Coefficients indicate the change in emissions after the implementation of the credit reform. Errors are clustered at the district level.

TABLE A6: Impact of credit expansion on industrial & energy $PM_{2.5}$ emissions, robustness to bandwidth [REGHDFE]

	Industrial emissions	Combined emissions	Energy emissions
(A) Bandwidth of 3000			
post=1 × treat=1	-14.625	-16.086	-5.528
	(9.832)	(9.970)	(6.136)
N	4,704	4,704	4,704
(B) Bandwidth of 2000			
post=1 × treat=1	-18.039*	-19.323*	-6.159
	(10.447)	(10.736)	(8.310)
N	3,072	3,072	3,072
Fixed Effects:			
District	Yes	Yes	Yes
Year	Yes	Yes	Yes
State × Year Trend	Yes	Yes	Yes

NOTES: Our outcome is the average total quantity of emissions in the district. The source of emissions varies across the columns. Coefficients indicate the change in emissions after the implementation of the credit reform. Errors are clustered at the district level.

TABLE A7: Impact of credit expansion on placebo $PM_{2.5}$ emissions [REGHDFE]

	Refining & Transformation	Agricultural waste burning
post=1 × treat=1	-0.382	-0.115
	(0.451)	(0.704)
N	3,984	3,984
Fixed Effects:		
District	Yes	Yes
Year	Yes	Yes
State × Year Trend	Yes	Yes

NOTES: Our outcomes are the average total quantity of emissions from refining and transformation and agricultural waste burning. in the district. Coefficients indicate the change in emissions after the implementation of the credit reform. Errors are clustered at the district level.

TABLE A8: Impact of credit expansion on industrial $PM_{2.5}$ emissions by firm size [REGHDFE]

	Very large	Large	Medium	Small
(A) Above 75th percentile				
post=1 × treat=1	-5.865	-19.779	-23.417	-35.940*
	(20.680)	(19.246)	(22.472)	(20.588)
N	1,008	1,008	1,008	1,008
(B) Below 75th percentile				
post=1 × treat=1	-21.223**	-23.533**	-18.846**	-15.443
	(9.714)	(9.367)	(8.076)	(9.988)
N	2,952	2,952	2,952	2,952
Fixed Effects:				
District	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
State × Year Trend	Yes	Yes	Yes	Yes

NOTES: Our outcome is the average total quantity of emissions in the district. The source of emissions varies across the columns. Coefficients indicate the change in emissions after the implementation of the credit reform. Errors are clustered at the district level. Very large firms are those with more than 101 employees, large have between 50 and 100, medium have between 20 and 50, and small have less than 20.

TABLE A9: Impact of credit expansion on industrial & energy CO_2 emissions, all treated units [REGHDFE]

	Industrial emissions	Combined emissions	Energy emissions
post=1 × treat=1	-596.822	-599.602	-1889.436
	(372.748)	(378.838)	(1419.769)
N	3,984	3,984	3,984
Fixed Effects:			
District	Yes	Yes	Yes
Year	Yes	Yes	Yes
State × Year Trend	Yes	Yes	Yes

NOTES: Our outcome is the average total quantity of emissions in the district. The source of emissions varies across the columns. Coefficients indicate the change in emissions after the implementation of the credit reform. Errors are clustered at the district level.

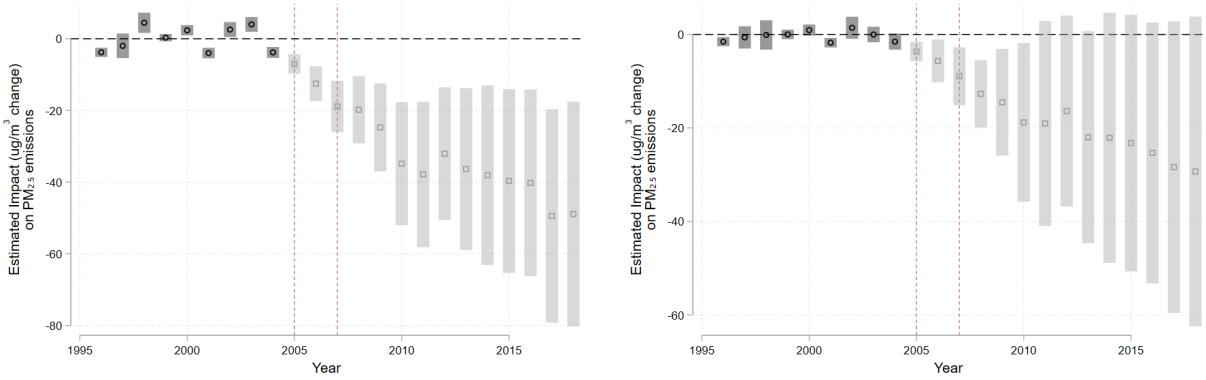
TABLE A10: Impact of credit expansion on industrial & energy CO_2 emissions, robustness to bandwidth [REGHDFE]

	Industrial emissions	Combined emissions	Energy emissions
(A) Bandwidth of 3000			
post=1 × treat=1	-347.912	-420.759	-1508.191
	(474.102)	(492.291)	(1352.752)
N	4,704	4,704	4,704
(B) Bandwidth of 2000			
post=1 × treat=1	-536.806	-480.416	-2065.585
	(444.408)	(454.773)	(1782.338)
N	3,072	3,072	3,072
Fixed Effects:			
District	Yes	Yes	Yes
Year	Yes	Yes	Yes
State × Year Trend	Yes	Yes	Yes

NOTES: Our outcome is the average total quantity of emissions in the district. The source of emissions varies across the columns. Coefficients indicate the change in emissions after the implementation of the credit reform. Errors are clustered at the district level.

Appendix 2 Additional Figures

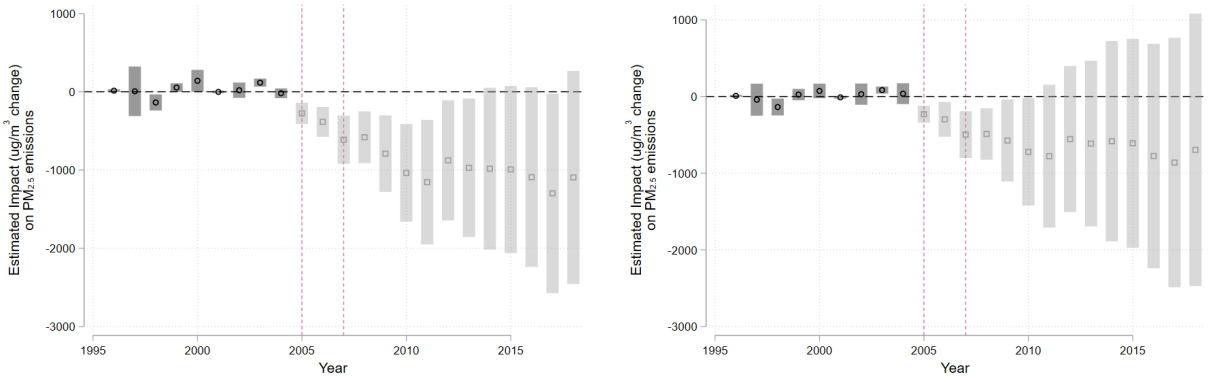
FIGURE A1: Robustness of $PM_{2.5}$ results to alternative bandwidths



(A) Bandwidth of +/- 2000 (B) Bandwidth of +/- 3000

NOTES: We plot the change in emissions in the post period for emissions that EDGAR classifies as "industrial" in the treated districts relative to the non-treated units where the change is measured relative to all the pre-periods. Our estimates are based on the approach described in Callaway and Sant'Anna (2021) and implemented using their STATA command `csdid`. Treatment is defined as being in a district that was above the threshold being considered "underbanked" by the RBI. We only include districts in Panel A that were within a bandwidth of 2,000 around the population per bank branch threshold and in Panel B of 3,000.

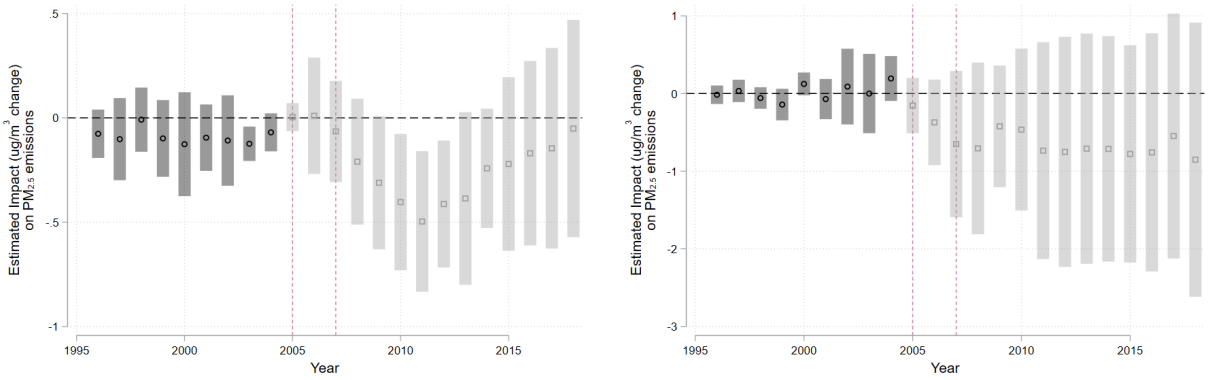
FIGURE A2: Robustness of CO_2 results to alternative bandwidths



(A) Bandwidth of +/- 2000 (B) Bandwidth of +/- 3000

NOTES: We plot the change in emissions in the post period for emissions that EDGAR classifies as "industrial" in the treated districts relative to the non-treated units where the change is measured relative to all the pre-periods. Our estimates are based on the approach described in Callaway and Sant'Anna (2021) and implemented using their STATA command `csdid`. Treatment is defined as being in a district that was above the threshold being considered "underbanked" by the RBI. We only include districts in Panel A that were within a bandwidth of 2,000 around the population per bank branch threshold and in Panel B of 3,000.

FIGURE A3: Placebo impacts of credit expansion on $PM_{2.5}$ emissions



(A) Refining & Transformation (B) Agricultural waste burning

NOTES: As a placebo test we plot the change in emissions in the post period from refining and transformation and agricultural waste burning in the treated districts relative to the non-treated units where the change is measured relative to all the pre-periods. Our estimates are based on the approach described in Callaway and Sant'Anna (2021) and implemented using their STATA command `csdid`. Treatment is defined as being in a district that was above the threshold being considered "underbanked" by the RBI. We only include districts that were within a bandwidth of 2,500 around the population per bank branch threshold. We generate emissions measures by combining emissions rates for the respective sectors as articulated in the EDGAR data. Full details of the regressions are provided in of Table 4.