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# Are Crop Residue Burning Bans Effective? Evidence from India

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**Abstract**

Crop residue burning (CRB) is a leading cause of high air pollution in developing countries. We examine the effectiveness of India's largest ban on CRB using a difference-in-differences strategy that exploits its implementation in select states. We find that there was a reduction in fire counts by 30% of the pre-ban mean albeit waning to near-zero two-three years after the ban. Using state-level data on fines, we show that burning initially reduced in areas where the ban was relatively better enforced, generating uncertainty for farmers. However, low levels of overall enforcement led to a return to the old status-quo.

**JEL Codes**: O13, Q52, Q58

**Keywords**: Crop residue burning, bans, fires

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# 1 Introduction

In recent years crop residue burning (CRB) has drawn a large amount of public attention due to its harmful effects on environmental and human health. Farmers across the world burn copious amounts of the agricultural residue they generate—around 50% in the developing world—leading to 3.5% of all greenhouse gas emissions (Ritchie, 2020). The practice also destroys a considerable amount of the organic matter in soil, potentially leading to crop productivity-related losses (Bhuvaneshwari et al., 2019). In India, CRB is infamous for the haze it creates across the northern states of Delhi, Punjab, Haryana and Uttar Pradesh (Lan et al., 2022). In fact, it is found to be the leading cause of air pollution in North India and consequently, premature mortality in the country—accounting for approximately 17.8% of all deaths as of 2019 (Pandey et al., 2021). As a result, it is of considerable importance to policymakers and governments to better understand CRB and ways to disincentivize it. One commonly utilized instrument for addressing CRB is the imposition of bans.

In this paper, we study the efficacy of India's largest and most important ban on agricultural residue burning. The ban was imposed by the National Green Tribunal, a specialized judicial body under the Ministry of Environment, Forest and Climate Change, on 10th December 2015 (Ministry of Agriculture & Farmers Welfare, 2019; National Green Tribunal, 2015) and was applied in five states – Punjab, Haryana, Uttar Pradesh, Delhi and Rajasthan. To measure the incidence of agricultural fires, we utilize data from NASA MODIS Active Fires product which provides the location and date of every granular 1-km fire pixel the satellite detected from 2011 to 2020 (Giglio et al., 2021). We group these fires into $10\text{km}^2$ grids on every date in our data to arrive at a granular measure of fires. Using a difference-in-differences design, we find that the years following the ban saw a reduction in fires amounting to approximately 30% of the pre-ban period mean. Event-study evidence indicates that much of this reduction seems to have come from the second year following the ban. Immediately afterward, the effect of the ban declines to near-zero, where it remains until the end of our data. Testing for mechanisms through which this reduction could have taken place, we find that how much farmers reduce the incidence of crop residue or stubble burning in each state in aggregate terms is in line with how much each state fines per (estimated) landholding burnt—which is at most Rs. 37.91 (USD 0.5), across states and time. We also note that, wherever imposed, these fines do not increase much with time. Consequently, we propose one plausible explanation that explains these trends: farmers reduce burning in response to uncertainty in the implementation of the ban. However, after two periods when they learn that the implementation of the ban will not be strictly followed and the fines will remain small, they resume their pre-ban crop residue burning behavior.

Overall, our results are both novel and surprising. Across states, the popular perception of the ban—or most bans for that matter—is that they are largely ineffective (Jack et al., 2022; Lan et al., 2022). Other solutions such as crop diversification and the utilization of agricultural residue-related biomass in thermal power generation have thus come to occupy mainstream discussions on the subject (Shyamsundar et al., 2019). Our results, however, suggest that farmers did in fact respond cautiously to this ban. It rather seems to be the case that without effective implementation and adequate fines, the small likelihood of being fined proves to be a less compelling reason to stop burning in the long run.

Our paper contributes to the growing literature on crop residue burning. The existing literature in this area studies the environmental, health and economic impacts of pollution arising from burning crop residue. Analyzing a representative panel of cities in China, Guo (2021) finds that straw burning worsened air quality for up to 8 days, with the first day seeing a 9.4% increase in the particulate matter. Studying pollution from straw burning in China, He et al. (2020) finds that just a $10\mu g/m^3$ increase in $PM_{2.5}$ in the air from CRB increased monthly mortality by 3.5%. Similarly, using remote sensed and survey data from China, Lai et al. (2022) finds that burning driven air pollution reduced short-term cognition among older individuals. Zivin et al. (2020) finds that test-takers of China's national college entrance exam are negatively affected due to pollution from agriculture fires in the vicinity. In the context of India, Singh and Dey (2021) finds that extremely high biomass burning in surrounding areas at birth reduced the height of children by about 1%. Garg et al. (2024) finds a significant increase in infant mortality due to farm fires. In Brazil, another country where farm fires are a widespread practice, Rangel and Vogl (2019) finds that late-pregnancy exposure to smoke from agricultural fires reduced the birthweight and in utero survival rates of newborns. Finally, using crime data in Pakistan, Ayesh (2023) reports a significant increase in crime annually from pollution resulting from biomass burning in the country—costing up to 900 million USD. Additionally, a related set of papers study other forms of biomass burning such as forest fires and wild fires and find negative impacts on a variety of (similar) outcomes like citizen health and incidence of disease (Sheldon and Sankaran, 2017; Moeltner et al., 2013), persistence in the impact on health outcome decades later (Rosales-Rueda and Triyana, 2019), labor supply (Kim et al., 2017), early life mortality (Jayachandran, 2009) and economic growth (Meier et al., 2023).

However, few studies analyze policies that can help mitigate CRB. In the Indian context, Jack et al. (2022) finds that offering conditional cash transfers help reduce the residue burning significantly. The study closest in spirit to ours is Sekhri et al. (2023), which examines the same ban we consider in this paper. The study finds little to no change in satellite-detected burned area on account of the ban. However, imputing burned area involves utilizing other remote-sensed parameters which could potentially generate measurement error (Giglio et al., 2022). Our choice of the outcome variable (active fires product) allows us to detect fires more precisely – thereby complementing their work. In another study, Cao and Ma (2023) examines the efficacy of two different policies instituted to reduce stubble burning in China: first, the introduction of biomass power plants (BPPs) around the country, and second, bans on crop residue burning. The study shows that the introduction of a BPP reduced burning in nearby areas by 14%, with farmers closer to the plants showing a larger reduction. The bans, in comparison, enforced supposedly through fines ranging between 70-300 USD, per incident are reported to be considerably less effective. they seem to help reduce burning only during the night – when monitoring was easier. Our paper situates itself in this strand of literature and reports findings in the Indian context. It is important to note two crucial differences between the ban in China and the NGT ban in our context. First, the bans they study were not considered the primary way to combat straw burning by the Chinese government – as indicated by its choice to focus on providing farmers with alternative ways to utilize leftover straw. Second, the enforcement of the bans in China is not well known. The degree to which farmers were to be regulated and fined was highly decentralized, making it different from our context where directives to regulate crop residue burning were uniform across states, though implementation varied.

The remainder of the paper is organized as follows. Section 2 provides background on the practice of CRB and the history of bans against it in India with a description of our data in Section 3. Sections 4 and 5 detail our empirical strategy and results respectively. Section 6 concludes.

# 2 Background

## 2.1 Crop Burning in India

Indian farmers burn about 100 million metric tons of surplus crop residue every year, about a fifth of the residue generated from crop production (Lan et al., 2022). Paddy or rice (particularly hybrid rice) is the crop whose residue is burnt the most. The Indian Agricultural Research Institute (IARI) reports that greater than 60% of all rice stubble generated in India is set ablaze – with Punjab and Haryana being the key contributors (Abdurrahman et al., 2020). Around 80 per cent of all crop residue burning is concentrated in the months of October-December i.e., after the Kharif (post-monsoon) harvest (Down to Earth).

The northern states of India have a long history of crop residue burning, however, this practice has now extended to other states over the last decade, particularly in well irrigated areas, where cropping patterns are intense and farming has become more mechanised. Estimations using satellite data show a 60% increase in the number of agricultural fires between

2002 and 2016 (Lan et al., 2022), with a further 15% increase from 2016 to 2021 (Xu and You, 2023). The reasons for the practice vary across states. Farmers in Punjab and Haryana burn the residue due to a short period between harvesting of rice and planting of wheat, necessitated by the 2009 Groundwater Act (Jain, 2023) coupled with the scarcity of labor and limited farm mechanisation. In other states, the explanations vary from clearing of the residue being expensive to the poor acceptability of paddy straw as fodder to the burning process helping to eliminate pests.

## 2.2 Bans on Crop Burning

Crop residue burning has been illegal in India for over four decades as per The Air Pollution Control Act of 1981. Nonetheless, the practice continued, leading to large bouts of haze in Delhi and the National Capital Region, which brought the gravity of the problem into public consciousness in the early and mid-2010s (Raman and Mukerjee, 2019). These conditions led to the most prominent ban on crop residue burning in North India – the one imposed on 10th December 2015 by the National Green Tribunal (NGT), a statutory body that deals with environmental disputes (National Green Tribunal, 2015).[1] In particular, the NGT banned all forms of agricultural residue burning in five states: Delhi, Punjab, Haryana, Uttar Pradesh and Rajasthan.

Governments – led primarily by the Central government – have also tried to implement other policies to curb the practice. The notable ones include the Agricultural Mechanization for In-Situ Management of Crop Residue Scheme (2018) (Press Information Bureau, Government of India, 2021a) which subsidizes the use of agricultural machinery assisting in crop residue management, the Sub-mission On Agriculture Mechanization (2014) (Press Information Bureau, Government of India, 2021b) which subsidizes agricultural machinery purchases (including machines like happy seeders which help in better crop residue management), mandating biomass/biomass pellet co-firing for power generation within thermal

[1]Henceforth, we refer to this policy as 'the NGT ban' or simply, 'the ban'.

plants (2023) (Ministry of Power, 2023) and the National Clean Air Program (2019) (Ministry of Environment, Forest and Climate Change, 2023). However, unlike the 2015 ban, none of them tackle the problem directly.

# 3 Data

In this section, we describe the datasets we use in our analyses – satellite data on crop fires and administrative data on the penalties levied by states for CRB under the ban.

## 3.1 Fires

We use data on crop fires from 12 states between 2011 and 2020, five of which implemented the ban on crop fires in 2015 (treated states) and seven additional states that did not impose bans but that share a border with the states where bans were imposed. These control states include Uttarakhand, Himachal, Madhya Pradesh, Gujarat, Bihar, Jharkhand and Chhattisgarh.

We use data on fires (or fire events) from NASA's Moderate Resolution Imaging Spectroradiometer (MODIS) Active Fires Product (MODIS C6/C6.1). The product is published as a part of NASA's Fire Information for Resource Management System (FIRMS). The data provide the location of the centroid of every 500 $m^2$ pixel where a fire has been detected along with its date of detection.[2] The data also provides a 'confidence' value for each fire – loosely interpreted as the probability of that pixel *actually* being a fire. We group these fires into grids of 10 x 10 km. Grids on state borders are cut to be contained within the relevant state. Drawing from previous papers studying crop fires in India using this data (Singh and Dey, 2021), we utilize the confidence-weighted sum of fires in every grid on a given date as our main outcome. Table 1 provides a summary of our main outcome variable across states for the period preceding the ban. Appendix Figure A.1 plots the district-level mean of the

[2]Note that by fire, we also mean *fire pixel.*

confidence-weighted fires per grid from 1st January 2011 to 10th December 2015. Punjab and Haryana record the largest number of daily fires per grid as expected. However, we note a significant degree of fires in our control states as well – particularly in Madhya Pradesh and Chhattisgarh. The southern states also show a large number of fires, but only in a few pockets.

Satellite data are more accurate at identifying fires, especially agricultural residue fires. It also avoids problems related to self-reporting biases that arise when trying to identify them through surveys. It is simply never in the interest of any farmer to self-report CRB, especially given that fines or other punitive action could potentially be imposed. Moreover, satellite data provide granular and high frequency information which is useful for tracking changes over time. One concern is that an identified fire may not necessarily be an agricultural residue fire. However, despite this caveat, the utilization of satellite data stands to be the best choice for our analysis and continues to be used as a proxy for CRB in the literature (Walker et al., 2022; Walker, 2024).

Notably, we measure fires using the active fires product. Another alternative is to use the burned area product. We choose the former for two reasons. First, NASA recommends that active fire pixels not be used to estimate burned area due to sizeable "spatial and temporal sampling issues" (Giglio et al., 2021). The Burned Area Product does precisely this, although marginally improves the analysis by utilizing surface reflectance parameters, i.e. measures of the quantum of light the Earth's surface in a given area reflects.Second, it is widely acknowledged that the Burned Area Product's calculations must be considered low confidence due to the complexities in accurately measuring agricultural burning (Hall et al., 2016; Giglio et al., 2022).[3]

[3]Note that this critique is not as relevant for identification of fires because the complexity in moving from identification of fires to area for agricultural fires arises from the other parameters the burned area product uses.

## 3.2 Fines

Following the ban, the NGT also announced that farmers who burnt stubble would be fined between Rs. 2500 to Rs. 15,000 (USD 36 - USD 215) based on the size of the landholding burnt (National Green Tribunal, 2015). Our data on these agricultural fines is derived from two sources. The first is a press release by the Government of India regarding the fines collected under the ban (Ministry of Agriculture & Farmers Welfare, 2019). Second, we filed requests under the Right to Information (RTI) Act in early 2024 requesting state level data on environmental compensation collected from non-compliant farmers. However, these data must be interpreted with caution as they are reported by state governments and we cannot verify them using independent sources. Our analyses only compare states with different levels of stringency in the enforcement of fines, and so the exact values of the fines do not matter.

Table 2, Panel A, shows the amount of fines annually collected by each state after the ban. To gauge whether the ban had a sufficient deterrence effect, Panel B shows the estimate for the amount fined per burnt landholding. To calculate this estimate, we first obtain the proportion of farming households which burn crop residue across states from existing studies (Liu et al., 2020; Lopes et al., 2020; Kemanth et al., 2024; Kaushal and Prashar, 2021; Kumar et al., 2015; Jack et al., 2022). The average rates across states noted by these studies vary – Punjab (50-90%), Haryana (10-50%), Uttar Pradesh ($\geq$ 10%). We combine the smallest estimated figures for the percentage of farmers who burn stubble across these studies (Liu et al., 2020) with the total number of agricultural landholdings in each state from the Agricultural Census of 2015, and find the average size of the fine collected to be low. The highest average fine per 'burnt' landholding between 2015-19 was Rs. 37 in Haryana during 2018-19. Additionally, if we assume that every burnt farm landholding was fined the lowest amount possible (Rs. 2,500), the maximum proportion of landholdings that could have been fined on average in a given year is approximately 4.2% in Haryana, 0.42% in Punjab and 0.05% in Uttar Pradesh. These low shares indicate weak enforcement of the ban, which was supposed to levy a fine on non-compliant farmers. Nonetheless, a key takeaway is that Haryana levied the most fines per burnt landholding followed by Punjab. There was almost no collection of fines in Delhi and Rajasthan.

# 4 Empirical Strategy

We use a difference-in-differences (DiD) strategy to estimate the effect of the ban. The five states where the ban was imposed form the treated states, while the seven states that border them constitute the control group. We estimate the following model specification:

$$Fires_{ist} = \beta Ban_s \times Post_t + \mu_i + \gamma_t + \epsilon_{ist} \tag{1}$$

$Fires_{isty}$ is the confidence-weighted count of fires in grid $i$ in state $s$ on date $t$. $Ban_s$ is a binary indicator that takes a value of one for grids located in the treated states and zero otherwise. $Post_t$ is a binary indicator that takes on a value of one starting 2016 (month following the ban) and zero for dates before it. $\mu_i$ and $\gamma_t$ represent grid and date fixed effects respectively. To keep consistency with the level of our observations and considering the granularity of our data, we cluster the standard errors at the grid level. However, we also present specifications with state clusters given that the state is the level of treatment. Here, $\beta$ shows the effect of the ban on the fire counts.

We also aggregate the grid-date level data to the district-month-year level, which allows us to construct bootstrap (wild) p-values that are more appropriate for a small number of clusters.[4] We include district and month-year fixed effects and cluster our standard errors at the district and state level. There may be a concern that the selection of states for the imposition of the ban is not exogenous, and that our treatment and control states have different trends in the outcome variable. To check for differential pre-trends we supplement

[4] We are unable to provide bootstrap p-values for the grid level specifications due to the large number of fixed effects which make the process extremely memory-inefficient.

the DiD estimates with event study plots that allow us to examine pre-trends in outcomes directly and evaluate whether the assumption of parallel trends in the absence of the ban is likely to hold. We estimate the following event study specification:

$$Fires_{ist} = \sum\nolimits_{y \in \mathbb{S}} \beta_y Ban_s \times \mathbb{1}[Year = y] + \mu_i + \gamma_t + \epsilon_{ist} \tag{2}$$

where $y$ refers to the year corresponding to the date $t$. We use year 2015 (year before the ban) as the base year relative to which all estimates are calculated. Set $\mathbb{S}$ thus includes every year from 2011 to 2020 except 2015. Here, $\beta_y$ show the year-wise effects on the fire counts, with 2015 as the base year. If there are no differences in pre-trends across treated and control states, then we expect $\beta_{2011}$-$\beta_{2014}$ to be insignificant and close to zero.

# 5 Results

Table 3 reports the results from our main specification (1). Columns (1) and (3) include month-year fixed effects, whereas (2) and (4) use more stringent date fixed effects. Columns (1)-(2) cluster the standard errors at the grid level while (3)-(4) cluster them at the state level. Across these specifications, we find that the ban reduces the number of fires per grid by 0.0013, approximately 30% of the pre-period mean number of fires in our data.[5] The difference-in-differences coefficient is significant at all conventional levels of significance when clustering the standard errors at the grid level and at the 90% level when clustering them at the state level.

Panel (a), Figure 1 shows our main event study plot. The estimates show that the ban reduces fire counts in 2017 and 2018, even though the ban was introduced in 2015. This result is not surprising given that fines were not levied from April 2015-March 2016 since the ban was only introduced in December 2015 (Table 2). Given their past experience with

[5]We use 'number of fires' interchangeably with the 'confidence weighted number of fires' because in no instance do we use the absolute number of fires in our data for our analyses.

bans, the farmers may not have anticipated implementation of fines and continued to burn in 2016. Fines were indeed levied from April 2016-March 2017 even though the proportion of offenders caught were very small. This may have led farmers to be cautious in the 2017 cropping cycle. Even though the fines collected were small, there was uncertainty that the implementation might improve over time. While the implementation improved in 2017 and 2018, the improvement was small and perhaps resulted in farmers realizing that the government would not implement the fines effectively. The negative effect, thus, lasted only for two years and thereafter we observe no differential change in fire counts in treated relative to control states. It seems that the impact of the ban is near-linearly decreasing with time i.e., after the second year of the ban, the burning seems to gradually return as we see a similar change across treated and control states. The plot also shows that the effect of the ban is significant only for one year—the second year after its implementation. Importantly, we do not find any differential trends in fires across treated and control states before the ban was introduced, which allays any concern that the observed fall in fires in 2017-18 was due to an underlying decreasing trend in fires across them.

To summarize, the event study estimates shed light on the possible mechanisms at play even though the actual imposition of fines was low. One possible explanation is that farmers, initially, did not expect any fines to be implemented but were taken aback when some farmers were penalized in 2016. Thus, due to the uncertainty involved in the magnitude of punitive action from the state in response to burning, they burnt less residue in the next cropping cycle. However, over a period of time, they realized that implementation is unlikely to improve much and reverted to crop residue burning resulting in no difference across treated and control states in the long run.

## 5.1 Heterogeneity

If the above mechanism of uncertainty holds, then we should find that either the states which collected more fines in 2016-17 or states where the probability of being caught was higher (like Rajasthan and Delhi where CRB only occurs in certain limited areas) see a larger reduction in crop burning as there is likely to be more ambiguity for farmers residing in these states. Table 4 reports the heterogeneous estimates across the treated states. Columns (1) and (3) include month-year fixed effects, whereas (2) and (4) control for date fixed effects. Again, columns (1)-(2) cluster the standard errors at the grid level while (3)-(4) cluster them at the state level. We find that relative to control states, there is a reduction in fire counts in Punjab by 6% of the state's pre-period grid mean. Haryana, on the other hand witnesses a 22% fall in the fire counts over the state's pre-period mean. The only state for which there seems to be no significant effect arising from the ban is Uttar Pradesh when we cluster the standard errors at the state level (columns 3-4).

These results are consistent with how much each state collected in fines per estimated burning landholder in 2016-17 (Table 2, Panel B). We also find that there is a large reduction in Delhi and Rajasthan, even though no fines were collected in these states in 2016. Delhi was in the middle of a political storm due to deteriorating air quality in 2016 in the capital city. Several newspapers reported the 'The Great Smog of Delhi' in 2016 (TOI). This created a frenzy among the general public leading to an uproar against the practice of crop residue burning in the state. Given the large fines levied in the neighbouring state of Haryana, farmers in Delhi may have anticipated implementation of penalties in Delhi in the next cropping cycle. Since agricultural activity occurs only in small pockets of Delhi, catching the erring farmers would not be difficult for the authorities. Again, Rajasthan only has a few regions where rice is cultivated. Therefore, the probability of being caught is higher for farmers in that state. Additionally, in Appendix Table A.1 we also specifically test whether farmers in the top two fine-levying states, Punjab and Haryana, responded differently to the ban relative to the other states. Overall, we find that these two states witness a significant reduction in fire counts of $\sim$10% relative to the pre-ban fire counts in these states. The reduction for the other states is only a third in magnitude and in fact becomes insignificant when standard errors are clustered at the state level.

### 5.2 Robustness

One concern that often accompanies the use of satellite data is that it may have higher measurement error at a granular level. To ameliorate this concern, we present our main results aggregated at the district-month-year level as opposed to grid-date level. Our outcome is now defined as the probability-weighted sum of fires per square kilometre in a district, month and year. Appendix Table A.2 reports the results for this aggregation, with district fixed effects. Columns (1)-(3) include year fixed effects while columns (2)-(4) include month-year fixed effects as controls for time. Further, columns (1)-(2) report estimates clustered at district level and columns (3)-(4) at state level. The wild clustered p-values for the estimates are reported in the last row. We find that our results using grid-level data continue to hold in the district-level aggregated specifications as well. We find a 28% (=0.0006/0.00216) reduction in fires over the pre-period district level mean of fire counts after the ban was implemented. While the state level clustered standard errors are larger, the state level wild clustered p-values continue to show a significant effect of the ban. We also estimate the event study coefficients using the data at the district level and plot the estimates in Panel B of Figure 1. Again, we find that the pattern observed using the grid level data continues to hold at the district level. There is a fall in the fire counts in the treated states vs. control states, which occurs 2-3 years after the ban, but the effect dissipates thereafter.

Additionally, we also consider whether our results remain robust to including only the neighboring districts across the treated and control states. This specification can help gauge whether differential implementation of the ban across the state borders drives our results. Appendix Table A.3 shows a 16% reduction in fire counts in the neighboring treated districts relative to control districts over the pre-period district level mean of fire counts after the ban. While the estimates are imprecise and only significant at 17% level, the magnitude is large. Notably, this specification reduces our sample size considerably resulting in larger standard errors.

Finally, we also show the robustness of our results to an extended time period. In

Appendix Table A.4, we present our main results using data from 2002 to 2021 (all the years for which satellite data is available). Columns (1) and (3) include month-year fixed effects, whereas (2) and (4) control for date fixed effects. Again, columns (1)-(2) cluster the standard errors at the grid level while (3)-(4) cluster them at the state level. Broadly, our findings remain the same – almost a 30% reduction in fire counts. The effect of the ban continues to be significant and similar in magnitude to our main analyses utilizing grid clusters—although only significant at the 15% level when using state clusters.

# 6 Conclusion

Overall, our results show that however poorly perceived, India's largest ban on crop residue burning seems to have significantly reduced the practice across the states in which it was relatively better implemented. However, the negative effect lasted for only two-three years after the ban. Our results in conjunction with the data on fines suggest that bans when accompanied by imposition of penalties as a mechanism for enforcement can be effective. In fact, the effect in early years seems to arise from uncertainty in the extent of ban enforcement since the state governments penalized at least some offenders. Over time the farmers are likely to have learnt that the implementation was weak and unlikely to improve. Thereafter, when the uncertainty in how the government would implement the ban diminished, the residue burning returned to its pre-ban levels. This result is consistent with experimental evidence from the tax compliance literature which shows that uncertainty in enforcement increases compliance when individuals perceive that they do not receive a public good in exchange for their taxes (Alm et al., 1992; Beck and Jung, 1989). Our findings show that bans are not ineffective as long as there is state commitment to implement them with adequate monetary fines. In the absence of state commitment, other policies that directly reduce the cost of proper disposal of residue are likely to be more effective and feasible.

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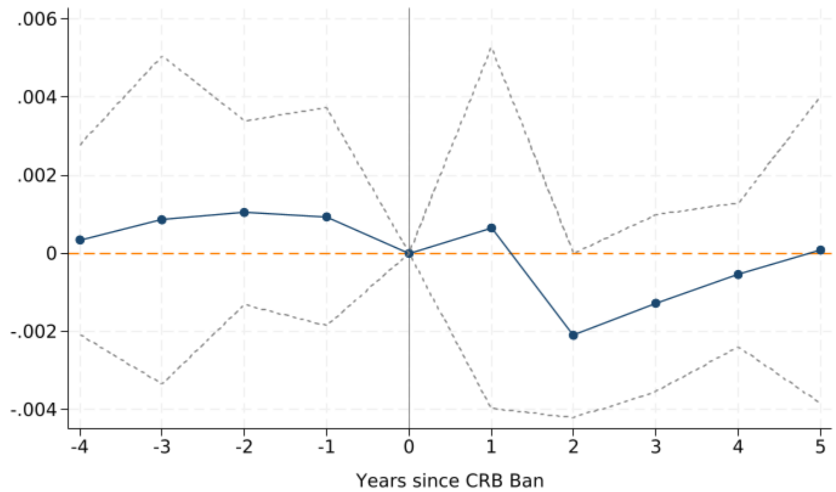
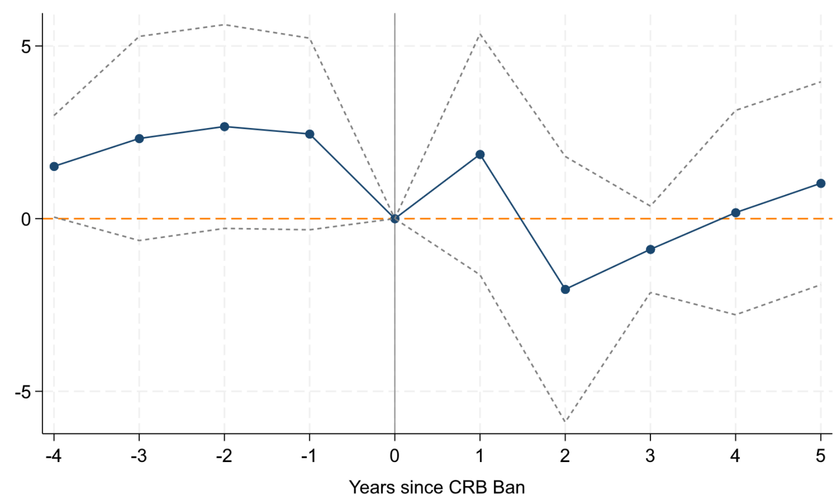
Figure 1: Impact of Ban on Fires: Event Study Estimates

(a) Grid-level




(b) District-level




*Notes:* The plots show the $\beta_y$ estimates from equation 2 with grid level data in Panel (a) and district level data in Panel (b). The dependent variable is the confidence-weighted sum of fires in a given grid—observed daily between 1st January 2011 and 31st December 2020 in Panel (a). This specification includes grid and date fixed effects and errors clustered by state in which the observed grid is placed in. The dependent variable is the confidence-weighted sum of fires in a given district-month-year in Panel (b). This specification includes district and month-year fixed effects and errors clustered by state in which the observed district is placed in. The dotted line represents 2015, i.e. year zero for the implementation for the ban and 95% confidence intervals are plotted.

Table 1: Daily Means of Confidence Fires per Grid by State: Pre-Ban

| State Name | Mean | Std. Dev. |
|---|---|---|
| Bihar | 0.0014 | 0.0452 |
| Chhattisgarh | 0.0040 | 0.7894 |
| Delhi | 0.0008 | 0.0309 |
| Gujarat | 0.0011 | 0.0337 |
| Haryana | 0.0109 | 1.3684 |
| Himachal Pradesh | 0.0011 | 0.0395 |
| Jharkhand | 0.0032 | 0.0674 |
| Madhya Pradesh | 0.0035 | 0.0813 |
| Punjab | 0.0557 | 0.3822 |
| Rajasthan | 0.0005 | 0.0272 |
| Uttar Pradesh | 0.0023 | 0.0547 |
| Uttarakhand | 0.0034 | 0.0713 |
| Total | 0.0043 | 0.0940 |

*Notes:* The summarized variable is the confidence-weighted sum (count) of fires in a grid. The sample considered includes grids in states in which the ban was implemented and our control states for the period between 1st January 2011 and 31st December 2015 (observed daily).
*Source:* Giglio et al. (2021) (MODIS Collection 6 Active Fire Product User's Guide Revision C).

Table 2: Environmental Compensation Collected by Treated States

| State | 2015-16 | 2016-17 | 2017-18 | 2018-19 |
|---|---|---|---|---|
| Panel A: Total Value (INR lakhs) | | | | |
| Punjab | 0 | 73.22 | 133.94 | 167.58 |
| Haryana | 0 | 19.38 | 52.78 | 61.72 |
| Uttar Pradesh | 0 | 0 | 0 | 28.60 |
| Delhi | 0 | 0 | 0 | 0 |
| Rajasthan | 0 | 0 | 0 | 0 |
| Panel B: Fine per burnt landholding (INR) | | | | |
| Punjab | 0 | 12.64 | 23.12 | 28.93 |
| Haryana | 0 | 11.90 | 32.41 | 37.91 |
| Uttar Pradesh | 0 | 0 | 0 | 0.12 |
| Delhi | 0 | 0 | 0 | 0 |
| Rajasthan | 0 | 0 | 0 | 0 |

*Notes:* Panel A shows the total value of fines levied, in INR lakhs. Panel B shows the estimates for fine (in INR) per burnt landholding, obtained by dividing the total value of fines by estimated count of landholdings that engage in CRB—calculated based on figures from the Agricultural Census of 2015-16 and an estimate of percentage landholdings burnt using studies in the literature.

*Source:* Ministry of Agriculture & Farmers Welfare (2019), RTIs we filed Giglio et al. (2021), Liu et al. (2020) and the Agricultural Census of 2015-16.

Table 3: Impact of Ban on Fires

| | Probability Weighted Fire Count | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Ban*Post | -0.0013*** | -0.0013*** | -0.0013* | -0.0013* |
| | (0.0001) | (0.0001) | (0.0006) | (0.0006) |
| Observations | 58805994 | 58805994 | 58805994 | 58805994 |
| Adjusted $R^2$ | 0.030 | 0.035 | 0.030 | 0.035 |
| Outcome Mean | 0.0047 | 0.0047 | 0.0047 | 0.0047 |
| Time FE | Month-Year | Date | Month-Year | Date |
| Grid FE | Yes | Yes | Yes | Yes |
| Clusters | Grid | Grid | State | State |

*Notes:* The dependent variable is the confidence-weighted sum (count) of fires in a grid. The sample includes grids in states in which the ban was implemented and our control states for a 10 year period starting from 1st January 2011 (observed every day). Ban equals 1 only for the states in which the ban was implemented and post equals 1 starting 2016. Each column reports the effective number of observations after incorporating the included fixed effects. Robust standard errors clustered at the level mentioned in the row named 'Clusters' in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
*Source:* Giglio et al. (2021) (MODIS Collection 6 Active Fire Product User's Guide Revision C).

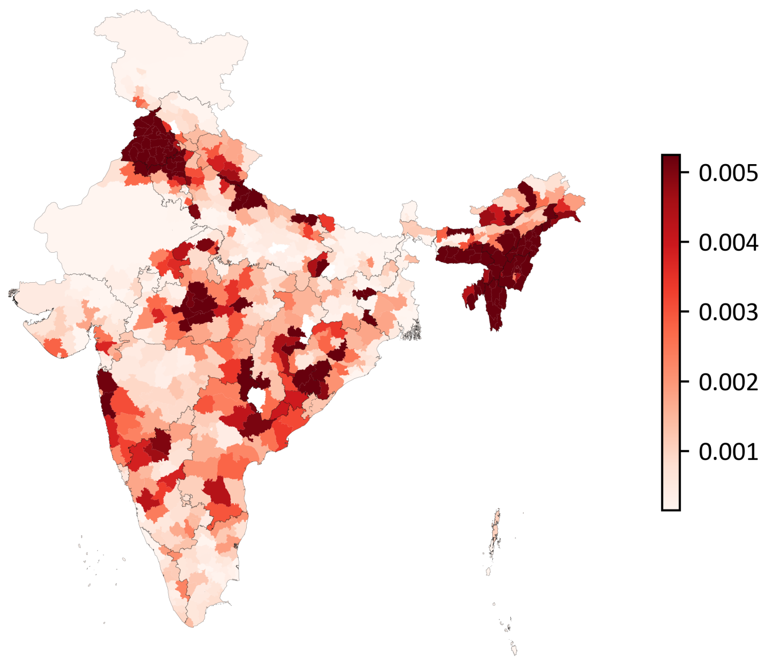
Table 4: Impact of Ban on Fires: Heterogeneity by State

| | Probability Weighted Fire Count | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Post*Punjab | -0.0035*** | -0.0035*** | -0.0035*** | -0.0035*** |
| | (0.0007) | (0.0007) | (0.0005) | (0.0005) |
| Post*Haryana | -0.0024*** | -0.0024*** | -0.0024*** | -0.0024*** |
| | (0.0003) | (0.0003) | (0.0005) | (0.0005) |
| Post*UP | -0.0006*** | -0.0006*** | -0.0006 | -0.0006 |
| | (0.0001) | (0.0001) | (0.0005) | (0.0005) |
| Post*Delhi | -0.0011*** | -0.0011*** | -0.0011** | -0.0011** |
| | (0.0002) | (0.0002) | (0.0005) | (0.0005) |
| Post*Rajasthan | -0.0012*** | -0.0012*** | -0.0012** | -0.0012** |
| | (0.0001) | (0.0001) | (0.0005) | (0.0005) |
| Observations | 58805994 | 58805994 | 58805994 | 58805994 |
| Adjusted $R^2$ | 0.030 | 0.035 | 0.030 | 0.035 |
| Mean Y | 0.0047 | 0.0047 | 0.0047 | 0.0047 |
| Time FE | Month-Year | Date | Month-Year | Date |
| Grid FE | Yes | Yes | Yes | Yes |
| Clusters | Grid | Grid | State | State |
| Trends | None | None | None | None |

*Notes:* The dependent variable is the confidence-weighted sum (count) of fires in a grid. The sample includes grids in states in which the ban was implemented and our control states for a 10 year period starting from 1st January 2011 (observed every day). Post equals 1 starting 2016 and Punjab, Haryana, UP, Delhi and Rajasthan represent binaries for if a grid in the data belongs to one of those states. Each column reports the effective number of observations after incorporating the included fixed effects. Robust standard errors clustered at the level mentioned in the row named 'Clusters' in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.
*Source:* Giglio et al. (2021) (MODIS Collection 6 Active Fire Product User's Guide Revision C).

# A Appendix: Figures and Tables

Figure A.1: Average Fires per day (2011-2015)




*Notes:* The plot shows the district-level mean of the confidence-weighted fires per grid over the period between 1st January 2011 to 31st December 2015—after winsorization of the top and bottom 15% of observations to present the true extent of variation. While these are vegetation related fires, hilly regions could also witness wild forest fires.

*Source:* Giglio et al. (2021) (MODIS Collection 6 Active Fire Product User's Guide Revision C).

Table A.1: Impact of Ban on Fires: Heterogeneity by Top Fine Levying States

| | Probability Weighted Fire Count | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Ban*Post | -0.0010*** | -0.0010*** | -0.0010 | -0.0010 |
| | (0.0001) | (0.0001) | (0.0005) | (0.0005) |
| Post*Punjab/Haryana | -0.0021*** | -0.0021*** | -0.0021*** | -0.0021*** |
| | (0.0004) | (0.0004) | (0.0005) | (0.0005) |
| Observations | 58805994 | 58805994 | 58805994 | 58805994 |
| Adjusted $R^2$ | 0.030 | 0.035 | 0.030 | 0.035 |
| Mean Y | 0.0047 | 0.0047 | 0.0047 | 0.0047 |
| Time FE | Month-Year | Date | Month-Year | Date |
| Grid FE | Yes | Yes | Yes | Yes |
| Clusters | Grid | Grid | State | State |

*Notes:* The dependent variable is the confidence-weighted sum (count) of fires in a grid. The sample includes grids in states in which the ban was implemented and our control states for a 10 year period starting from 1st January 2011 (observed every day). Post equals 1 starting 2016 and Punjab and Haryana represent binaries for if a grid in the data belongs to one of those states. Thus, Punjab/Haryana represents a binary for if a grid belongs to either one. Each column reports the effective number of observations after incorporating the included fixed effects. Robust standard errors clustered at the level mentioned in the row named 'Clusters' in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
*Source:* Giglio et al. (2021) (MODIS Collection 6 Active Fire Product User's Guide Revision C).

Table A.2: Impact of Ban on Fires: Robustness to Level of Aggregation (District)

| | Probability Weighted Fire Count Per Sqkm (District) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Ban*Post | -0.0005*** | -0.0005*** | -0.0005* | -0.0005* |
| | (0.0001) | (0.0001) | (0.0003) | (0.0003) |
| Observations | 39000 | 39000 | 39000 | 39000 |
| Adjusted $R^2$ | 0.156 | 0.198 | 0.156 | 0.198 |
| Outcome Mean | 0.0022 | 0.0022 | 0.0022 | 0.0022 |
| Time FE | Year | Month-Year | Year | Month-Year |
| District F.E | Yes | Yes | Yes | Yes |
| Clusters | District | District | State | State |
| WCB DiD p-value | 0.0005 | 0.0003 | 0.0762 | 0.0762 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

*Notes:* The dependent variable is the district-month level sum of the confidence-weighted sum (count) of fires in a grid divided by the area of the district (in $km^2$). The sample includes districts in states in which the ban was implemented and our control states for a 10 year period starting from January 2011 (observed every month). Ban equals 1 only for the states in which the ban was implemented and post equals 1 starting 2016. Each column reports the effective number of observations after incorporating the included fixed effects. Robust standard errors clustered at the level mentioned in the row named 'Clusters' in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
*Source:* Giglio et al. (2021) (MODIS Collection 6 Active Fire Product User's Guide Revision C).

Table A.3: Impact of Ban on Fires (Bordering Districts, District-level)

| | Probability Weighted Fire Count Per Sqkm (District) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Ban*Post | -0.0002 | -0.0002 | -0.0002 | -0.0002 |
| | (0.0001) | (0.0001) | (0.0001) | (0.0001) |
| Observations | 10080 | 10080 | 10080 | 10080 |
| Adjusted $R^2$ | 0.113 | 0.229 | 0.113 | 0.229 |
| Outcome Mean | 0.0012 | 0.0012 | 0.0012 | 0.0012 |
| Time FE | Year | Month-Year | Year | Month-Year |
| District FE | Yes | Yes | Yes | Yes |
| Clusters | State | State | District | District |
| WCB DiD p-value | 0.1729 | 0.1729 | 0.1663 | 0.1650 |

Standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Notes:* The dependent variable is the district-month level sum of the confidence-weighted sum (count) of fires in a grid divided by the area of the district (in $km^2$). The sample includes bordering districts between our control and treated states in which the ban was implemented for a 10 year period starting from January 2011. Ban equals 1 only for the states in which the ban was implemented and post equals 1 starting 2016. Each column reports the effective number of observations after incorporating the included fixed effects. Robust standard errors clustered at the level indicated in the row named 'Clusters' are included in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
*Source:* Giglio et al. (2021) (MODIS Collection 6 Active Fire Product User's Guide Revision C).

Table A.4: Impact of Ban on Fires: Robustness to Longer Time Period (2002-21)

| | Probability Weighted Fire Count | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Ban*Post | -0.0014$^{***}$ | -0.0014$^{***}$ | -0.0014 | -0.0014 |
| | (0.0001) | (0.0001) | (0.0008) | (0.0008) |
| Observations | 117595890 | 117595890 | 117595890 | 117595890 |
| Adjusted $R^2$ | 0.027 | 0.032 | 0.027 | 0.032 |
| Outcome Mean | 0.0043 | 0.0043 | 0.0043 | 0.0043 |
| Time FE | Month-Year | Date | Month-Year | Date |
| Grid FE | Yes | Yes | Yes | Yes |
| Clusters | Grid | Grid | State | State |

*Notes:* The dependent variable is the confidence-weighted sum (count) of fires in a grid. The sample includes grids in states in which the ban was implemented and our control states for a 20 year period starting from 1st January 2002 (observed every day). Ban equals 1 only for the states in which the ban was implemented and post equals 1 starting 11th December 2015. Each column reports the effective number of observations after incorporating the included fixed effects. Robust standard errors clustered at the level mentioned in the row named 'Clusters' in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
*Source:* Giglio et al. (2021) (MODIS Collection 6 Active Fire Product User's Guide Revision C).