Table 1—Firm Level: Pollution Monitoring and Enforcement Activities

Outcome	Any Air (1)	Suspension (2)	Upgrading (3)	Fine (4)	Warning (5)
Panel A. Any enforcement action related to air pollution					
$Mon_{<10km} \times Post$	0.0033	0.0014	0.0014	0.0014	−0.000058
	(0.00056)	(0.00045)	(0.00041)	(0.00043)	(0.00016)
Mean outcome	0.0046	0.0024	0.0025	0.0022	0.00070
Observations	1,155,296	1,155,296	1,155,296	1,155,296	1,155,296
Conley SE	[0.00040]	[0.00031]	[0.00031]	[0.00030]	[0.00017]
Outcome	# Air	Low intensity	High intensity	Lenient	Strict
Panel B. Intensity and strictness of enforcement action related to air pollution					
$Mon_{<10km} \times Post$	0.0031	0.0027	0.00018	0.00028	0.00070
	(0.00064)	(0.00047)	(0.00013)	(0.00015)	(0.00034)
$Mon_{<10km} \times Post \times H.\ Polluter$	0.040	0.0017	0.014	−0.0037	0.022
	(0.011)	(0.0068)	(0.0028)	(0.0016)	(0.0058)
Mean outcome	0.0052	0.0042	0.00040	0.00065	0.0016
Observations	1,155,296	1,155,296	1,155,296	1,155,296	1,155,296

Notes: This table reports estimates of the impact of air pollution monitoring on the probability of being subject to different air-pollution-related enforcement actions by the local government. All regressions control for fixed effects specific to firm, industry-by-time, and province-by-time interactions. Robust standard errors clustered on the city in parentheses. In panel A, standard errors based on the spatial HAC technique suggested by Conley (1999) are reported in brackets, using a Bartlett kernel and bandwidth of 100 km. Panel B reports heterogeneity for firms identified as high polluters according to ESR during the pre-period. The outcome "low intensity" ("high intensity") corresponds to a dummy variable indicating that a firm received only one (at least two) enforcement actions in a quarter. The outcome "lenient" is a dummy variable that equals one if only one punishment (among "suspension," "upgrading," and "fine") is issued against a firm in a quarter. In contrast, the dummy variable "strict" is defined as one if all three types of punishments are issued against a firm in a quarter.

We rely on the Environmental Survey and Reporting Database (ESR) to identify these firms. The ESR is put together by the central government and includes firms that are considered to be major polluters (in total responsible for 65 percent of local emissions).[25] In panel B of Table 1, we estimate the differential enforcement response against these firms. The estimates in column 1 suggest that there is a larger increase in the number of enforcement actions against these firms (significant at the 1 percent level). The following four columns report what type of enforcement these firms receive. We start by differentiating between low and high enforcement intensity, where we define "low" as receiving one enforcement action in a quarter and "high" as receiving more than one action. The results in columns 2–3 show that low-intensity enforcement is not significantly different between low- and high-polluting firms, but that all high-intensity enforcement focuses on key polluters in the presence of monitoring. Next, we consider the strictness of enforcement action. To capture this, we construct two additional dummy variables that classify enforcement records as either lenient or strict. Since there is no clear ranking of the three main punishment types discussed above ("suspension," "upgrading," and "fine") and enforcement records often include multiple punishments, we consider the two

[25] The ESR database has been used in several recent paper (see, e.g., He, Wang, and Zhang 2020). We use the ESR firms identified between 2010 and 2014, the period before introducing air quality monitors. In total, this corresponds to 1,445 of the firms in our baseline firm sample.

Table 2—Firm Level: Enforcement Response by Rainfall and Wind Direction

	Any air			Any air	
	(1)	(2)		(3)	(4)
Panel A. Rainfall			*Panel B. Wind Direction*		
$Rain_{>\tilde{x}}$	−0.00027	0.00031	*Upwind*	0.00033	0.000043
	(0.00033)	(0.00027)		(0.00023)	(0.00031)
$Mon_{<10km} \times Post$	0.0043	0.0041	$Mon_{<10km} \times Post$	0.0024	0.0027
	(0.00069)	(0.00074)		(0.00057)	(0.00067)
$Mon_{<10km} \times Rain_{>\tilde{x}}$		0.00014	$Mon_{<10km} \times Upwind$		−0.00035
		(0.00024)			(0.00045)
$Mon_{<10km} \times Post \times Rain_{>\tilde{x}}$	−0.0018	−0.0015	$Mon_{<10km} \times Post \times Upwind$	0.0035	0.0024
	(0.00068)	(0.00076)		(0.00082)	(0.0011)
$Post \times Rain_{>\tilde{x}}$		−0.0018	$Post \times Upwind$		0.0015
		(0.0011)			(0.00075)
Mean outcome	0.0046	0.0046	Mean outcome	0.0046	0.0046
Observations	1,155,296	1,155,296	Observations	1,155,296	1,155,296

Notes: This table reports results from augmented versions of our baseline model (equation (2)). Panel A (columns 1–2) adds interactions with an indicator for rainfall being above the median ($Rain_{>\tilde{x}}$); i.e., the interaction captures the differential effect on enforcement when pollution is relatively low (see online Appendix Table C6). Panel B (columns 3–4) adds interactions with an indicator for a firm being upwind from a monitor (online Appendix Figure D4 illustrates this classification); i.e., the interaction captures the differential effect for firms whose emissions are moved by the wind toward the monitor. All regressions control for fixed effects specific to firm, industry-by-time, and province-by-time interactions. Robust standard errors clustered on the city in parentheses.

monitor. However, as indicated by the triple interaction, the effect is even stronger for firms close to a monitor. These results also provide additional support for the validity of this exercise by showing that there is no differential response to rainfall shocks in the pre-period in areas close to the monitors compared to areas farther away.

We then move on to explore how the direction of winds affects the enforcement response. Emissions from firms that are upwind from a monitor will be moved by the wind toward the monitor, while emissions from all other firms are moved away from the monitor. Since upwind firms arguably have a larger impact on the pollution recorded by the monitor, local government officials have a greater incentive to enforce regulations for these firms. To test this we follow previous work (Freeman et al. 2019) and identify a firm as "upwind" if it is within 45 degrees of the dominant quarterly wind vector that passes through the monitor. Figure D4 in the online Appendix illustrates how we classify upwind firms. Following the same approach as above, we interact whether a firm is upwind in a quarter with our main treatment variables. The results are reported in panel B of Table 2. Column 3 shows that the coefficient for the upwind indicator is small and not significantly different from zero—suggesting that firms do not face differential enforcement by quarterly winds before the introduction of monitors. However, following the introduction of monitors, the enforcement response is stronger against upwind firms (as shown by the coefficient for $Mon_{<10km} \times Post \times Upwind$). Upwind firms face an increase in enforcement when monitored that is more than twice as large $(0.0024 + 0.0035 = 0.0059)$ as that faced by other firms (0.0024). Column 4 further documents that while this step-up in enforcement is particularly pronounced for firms close to the monitor, it is also detectable for firms farther than 10 km from a monitor (as shown by the estimate for $Post \times Upwind$). This suggests that more

Table 3—City Level: Impact of Monitoring

Empirical strategy:	DiD (1)	DiD + IV (2)	RD (3)	Diff-in-disc (4)
Panel A. Aerosol optical depth				
# Monitors	−0.031	−0.046	−0.039	−0.029
	(0.0069)	(0.013)	(0.015)	(0.018)
Observations	16,335	16,335	3,209	8,508
Panel B. log(*# firms receiving any air pollution enforcement*)				
# Monitors	0.15	0.19	0.26	0.28
	(0.046)	(0.098)	(0.10)	(0.16)
Observations	5,664	5,664	1,116	2,976
Panel C. Number of monitors (*first stage*)				
Estimate		0.72	1.28	1.28
		(0.11)	(0.23)	(0.23)
Kernel			Uniform	Uniform
Bandwidth			11.3	11.3

Notes: This table reports the main results for the two main outcomes for each of the four different empirical strategies used in the city-level analysis. Panel A reports results for aerosol optical depth, and panel B for the log of the number of firms receiving any enforcement action related to air pollution. Columns 1 and 2 show the estimates from equation (4), controlling for city fixed effects, time-by-pollution-reduction-target fixed effect, population and the geographical size of the built-up area at baseline interacted with the post variable, and time-varying controls for total precipitation, average temperature, and the age of the mayor. Column 1 exploits variation in the actual number of monitors installed, while column 2 instruments the actual number of monitors with the assigned number. The corresponding first-stage estimate is reported in panel C. Column 3 reports regression discontinuity estimates using local linear regression, a uniform kernel, and the MSE-optimal bandwidth proposed by Calonico, Cattaneo, and Titiunik (2014). We control for cutoff fixed effects and the average of the outcome in 2010–2011, details in online Appendix B.1. The first-stage effect of being assigned to the group above the cutoff on the number of monitors installed is reported in panel C. Column 4 reports estimates from the difference-in-discontinuity approach suggested by Grembi, Nannicini, and Troiano (2016), details in online Appendix B.2. Robust standard errors clustered on the city in parentheses.

Panel A in online Appendix Table C11 reports the results and shows that the overall effects are close to the results for our baseline sample.[35]

Second, to quantify the RD results above, we use the bias-corrected local linear regressions approach suggested by Calonico, Cattaneo, and Titiunik (2014), using a uniform kernel and controlling for cutoff fixed effects and the 2010–2011 baseline value of our outcome variables.[36] We report the formal specification in online Appendix B.1. Column 3 in Table 3 reports our baseline estimate, which uses the optimal bandwidth suggested by the same authors. Results are comparable to our DiD and IV estimates discussed above and suggest a 26 percent increase in enforcement and a 3.9 percent reduction in AOD. We again investigate the robustness of these estimates in the online Appendix. We start with establishing in online Appendix Table C10 that these estimates are also robust to excluding the large provinces Xinjiang and Tibet. Online Appendix Figure D7 shows that estimates are

[35] We are able to include the non-ASIF firms in this analysis since we can match them to cities even if we don't know the exact geographic location within the city.

[36] We explore the implications of these choices in Table C12 in the online Appendix. This shows that the choice of kernel has limited impact on the results, while the inclusion of baseline controls is important for precision but less so for the size of the estimates.

Table 4—Monitor Reassignment, Data Quality, and Policy Impact

Sample:	All	Incentivized		Background			
Outcome:	$\log(PM_{2.5})$					*AOD*	$\log(\#\,firms)$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
AOD	0.30	0.30	0.27	0.39	0.37		
	(0.031)	(0.033)	(0.038)	(0.050)	(0.057)		
AOD × *Reassigned*			0.10		0.050		
			(0.047)		(0.11)		
# Monitors						−0.025	0.12
						(0.0070)	(0.045)
# Monitors × *Reassigned*						−0.014	0.065
						(0.0030)	(0.028)
Mean outcome	3.68	3.71	3.71	3.42	3.42	0.34	0.58
Observations	17,535	15,496	15,496	2,039	2,039	16,322	5,646

Notes: This table reports the AOD elasticity of $PM_{2.5}$. Each column is from a separate regression. Columns 1–5 control for average temperature, rainfall, mayor's age, and fixed effects specific to monitor and time (month by year). Columns 6 and 7 control for city fixed effects, time-by-pollution-reduction-target fixed effect, population and the geographical size of the built-up area at baseline interacted with the post variable, and time-varying controls for total precipitation, average temperature, and the age of the mayor. Robust standard errors clustered on the city in parentheses.

instruments). To make sure that the changes we observe are due to improved monitor data rather than satellite data, we conduct a placebo analysis using the background monitors described in Section IB. The readings from these monitors are not used by the central government to evaluate the performance of the local government. Hence, there are weaker incentives for officials to manipulate this information. Columns 4 and 5 report the results. We notice that the overall elasticity between air pollution measures reported from monitors and satellites is larger for this sample. When looking at the reassignment, we find that the elasticity change is about half in magnitude and not statistically distinguishable from zero. Taken together, this evidence is consistent with less manipulation of the background monitors from the start and no change after the reassignment. This supports our conclusion above that the change in elasticity that we observe for the main sample is driven by changes in the data reported from the monitors. However, we are cautious against drawing too strong conclusions from these patterns since the estimates for the background monitors are imprecisely estimated and not statistically different from those for the main monitors.

The next exercise we carry out is to check whether local governments exert more effort to decrease pollution after monitors have been reassigned, since manipulation is then a less viable option. The results are reported in columns 6 and 7 of Table 4 and show that effects are indeed stronger after monitors have been reassigned. Column 6 shows a 1.4 percentage point greater reduction in pollution and column 7 a 6.5 percentage point larger increase in enforcement per monitor after the retraction. These pieces of evidence are consistent with local governments switching from data manipulation toward exerting more effort to enforce environmental regulations. Again, we emphasize that these results must be interpreted with caution because we are only exploiting temporal variation and thus need to assume that there are