Mountains of Evidence:
The Effects of Abnormal Air Pollution on Crime*

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Abstract

We find that air pollution increases crime in a city that ranks in the worst two percentiles worldwide for dirty winter air. Our identification strategy employs distinct geographic features of Almaty, Kazakhstan: cleaner mountain winds and frequent temperature inversions. Using these variables to instrument for PM2.5 air pollution, we estimate a PM2.5 elasticity of the expected crime rate more than four times as large as similar estimates from cleaner cities. Among crime types, we estimate statistically significant effects of air pollution on property crime, and we find no evidence of an effect on violent crime. These results are consistent with theory that air pollution induces higher discounting rather than aggression. We extend this theory and find that whether air pollution has distinct effects on crimes of varying severity depends on whether the population is more heterogenous in the outside option or in the discount factor. Using microdata on crime severity, we find statistically significant increases in both major and minor crime rates from air pollution, and we fail to reject common PM2.5 elasticities of minor and major crime rates. The greater scale of major crimes implies that they contribute more to the total crime rate increase from air pollution.

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1 Introduction

The adverse effects of air pollution on human health are now widely known, but recent findings of the negative short-term effects of air pollution on human behavior, including criminal behavior, are alarming (see, for a recent survey, Aguilar-Gomez et al. 2022). Observed effects of air pollution on crime at even healthy levels of air pollution coming from high-income countries, such as in the United States (U.S.) and the United Kingdom (U.K.), raise the question of how air pollution impacts human behavior in urban areas with abnormally high levels of air pollution.

The main contribution of our study is its finding that air pollution substantially increases criminal activity in a city, Almaty, Kazakhstan, where air pollution regularly exceeds international safety standards. Based on air pollution measures of fine particulate matter of diameter 2.5 microns or less (PM2.5), we estimate an elasticity of the expected crime rate with respect to air pollution equal to 0.39. This elasticity point estimate is more than 4 times as large as point estimates with respect to other air pollutants in Chicago and London. Our preferred estimates instrument for PM2.5 using mountain winds that affect nearby city districts as well as temperature inversions. Our primary empirical strategy is estimation using a linear instrumental variables (IV) approach and we report similar results in our appendix for pseudo-Poisson maximum likelihood (PPML) control function estimation. Reduced-form estimates of the relationship between our instruments and the crime rate further corroborate our main approach. Our finding contrasts with evidence from India (Singh and Visaria 2021) and Mexico City (Zarate-Barrera 2022) where the authors suggest that avoidance behavior at high levels of air pollution leads to negative marginal effects of air pollution on crime rates.

The city of Almaty, the largest in Kazakhstan, has unique features that help us to overcome the challenges and identify the effects of abnormal air pollution. Because Almaty winters still rely heavily on coal heating, the city has ranked among the dirtiest 2% of world cities in air pollution during winter (IQAir 2021), based on the concentration of PM2.5. For winter pollution sensor data that we access between December 2017 and March 2020, the average PM2.5 concentration of 89.7 $\mu g/m^3$ and the average Air Quality Index (AQI) of 162.1 are several times larger than we typically observe in high-income economies.[1] Almaty's mountains are a blessing and a curse for the city's air: winds blowing from the mountains

1. Specifically, the Almaty winter average AQI is more than five times the average for London or Chicago, which are two cities where the pollution-crime relationship has been studied rigorously. The Almaty winter average PM2.5 concentration is 9 times what has been observed in a nationwide U.S. study of pollution and crime. See Appendix Table A1 for precise comparisons between the sample we study and prominent studies of air pollution affecting crime.

are a source of clean air, but mountains also function as a barrier that traps the dirty air in the city. Thus, Almaty is an ideal setting for the now-common identification strategies of using wind direction (e.g. Schlenker and Walker 2016) and temperature inversions (e.g. Arceo et al. 2016) as instrumental variables (IV) that induce variation in air pollution. Our success in combining these approaches could be informative for researchers studying other mountain-adjacent cities. To explore the effects of abnormal air pollution on crime, we assemble district-level data from PM2.5 sensors over three Almaty winters that we match to crime reports, and we exploit the advantages of Almaty's mountains for our identification.

An additional finding is that air pollution affects only property crime and not violent crime in our sample. Which crimes are affected by air pollution matters for understanding how air pollution distorts behavior. Our findings contrast with three U.S. studies (Jones 2022; Herrnstadt et al. 2021; Burkhardt et al. 2019) whose authors find statistically significant effects only on violent crime other than robbery and no effects on property crime. These studies suggest that aggression is the exclusive mechanism for air pollution increasing crime. Our results support the relevance of the alternative mechanism, proposed by Bondy et al. (2020), that air pollution causes higher discounting in intertemporal choices, after they observe that air pollution affects both property crime and violent crime in London.

The final sections of our paper advance theory and empirics on how air pollution impacts crimes of distinct severity. Our theory builds on Becker (1968), in which one commits a crime only if the expected value of crime is greater than an outside option, and Bondy et al. (2020), in assuming that air pollution reduces individual discount factors. Our theoretical analysis concludes that the impact of pollution by severity depends on what type of exogenous population heterogeneity is more relevant in determining crime rates. We find that if the relevant population heterogeneity is in the outside option, then PM2.5 elasticities of crime rates are larger for more severe crimes. But when we consider a case of population heterogeneity in the discount factor itself, we find that the elasticity does not vary with crime severity. For our empirical analysis, we collect data on crime severity as assessed by Almaty police, and we focus on a partition into major and minor crimes.[2] Both our IV and reduced-form estimations find positive and statistically significant effects of air pollution on both minor and major crime rates. We fail to reject the hypothesis of common elasticities by severity, though this result may be in part a consequence of lower precision in our estimate for PM2.5 elasticities of minor crimes. The greater scale of major crimes implies they contribute more to the total crime rate increase from air pollution.

Our paper proceeds as follows. Section 2 provides background for our setting of Almaty, Kazakhstan, a highly-polluted city in winter. Section 3 discusses our data sources for crime,

2. We elaborate on the partition into major and minor crimes to begin Section 5.5

air pollution, and weather. Section 4 details our empirical strategy, which ultimately focuses on using mountain winds and temperature inversions as IV, and presents our results for air pollution's effect on the total crime rate. Section 5 discusses our results for individual crime types and presents our theory and empirics on pollution's effects by crime severity, and Section 6 concludes.

2 Background

The city of Almaty has a combination of useful characteristics for our study: relatively high crime, high average winter air pollution, and large air pollution variation within winter. Almaty is the largest city by population in Kazakhstan with 2 million inhabitants, so it is typically included in global comparisons of cities.

In 2020, the city had the 29th-highest crime index among 374 cities globally, according to Numbeo's crowdsourced data (Numbeo 2020). For perspective, Almaty ranks higher in the 2020 Numbeo crime index than any European city. Among the U.S. cities, Almaty ranks in between St. Louis (5th in the U.S.) and Milwaukee (6th in the U.S.).

Almaty is also among the most polluted cities in the world based on PM2.5. Based on average PM2.5 pollution in 2021, IQAir ranks Almaty 340th among 6475 cities globally (94th percentile). But this statistic reflects the aggregate of Almaty's clean summers and dirty winters. In January of 2021, Almaty's average PM2.5 pollution of $96.4\mu g/m^3$ ranked 77th in the world (98th percentile).

Centralized coal-powered heating in Almaty's winter contributes to abnormal air pollution, though there is substantial variation in pollution due to weather patterns. The city's combined heat and power (CHP) plants, a legacy of the Soviet centralized heating system, still rely heavily on burning coal. There are three CHP plants around the region, and two of them are within or near city borders: the Almaty-2 CHP within Alatau district to the city's northwest, and the Almaty-3 CHP right outside of Turksib district to the city's northeast. CHPs are responsible for 60% of heat production, and 70% of CHP production is coal-fueled, according to the World Bank (Zlatev et al. 2021). Small residential stoves and boilers makes the situation worse. Only one-third of Kazakhstan households have district heating, while two-thirds use smaller stoves and boilers that have low stacks and no filters, so their emissions tend to reach urban centers (Zlatev et al. 2021). Almaty exhibits a strong seasonal pattern in PM2.5 pollution despite having the largest central heating network and the lowest share of coal heating in the country. There are plans to switch Almaty-2 CHP to purely gas by 2025, but the phased switching will not begin until 2023.[3]

Our choice to use PM2.5 to proxy for air pollution is motivated by research confirming that PM2.5 is the most problematic pollutant in Almaty, as well as relevant global evidence of its negative impact. Kerimray et al. (2020) estimate that annual averages of PM2.5 in Almaty over 2013-18 exceeded WHO annual limits by 5.3 times, while the similar figures for PM10 and NO2 were 3.9 and 3.2. International benchmarks for SO2 and CO were less regularly exceeded. The authors further argue that PM2.5, PM10, and SO2 are the pollutants primarily omitted by heating sources that increase in winter. So for the three winters we study, we conclude that PM2.5 data best captures the variation in unhealthy air pollution. PM2.5 air pollution receives ample attention globally given its high potential for harm not just outdoors, but also indoors, no matter the ventilation mode. For instance, Cyrys et al. (2004) find that more than 75 percent of daily indoor variation of PM2.5 is explained by its outdoor variation.

Importantly for our empirical analysis, Almaty exhibits substantial variation in air pollution due to its location on the foothills of the Northern Tian Shan mountain range (also known as Trans-Ili Alatau), closest to the southeast of the city. Mountains just southeast of the city's center serve as both a clean-air source and a dirty-air barrier. Elevation within the city is visible in Figure 1. When wind blows from the direction of the mountains, the air is cleaner in the city districts closer to the mountains. A second large source of variation is temperature inversions, which occur when cool air is trapped at the ground under a layer of warm air. As Zlatev et al. (2021) confirm, temperature inversions are a major contributor to pollution in Almaty, because the mountains create an additional barrier that prevents air circulation. We illustrate one image of an Almaty temperature inversion in Figure A1 of the appendix. We discuss mountain winds and temperature inversions further upon detailing our instrumental variables strategy in Section 4.3.

3 Data

Our study gathers data on air pollution, crime, and weather for Almaty for full months over three winters, defined broadly to be December, January, February, and March starting

3. "Almaty combined heat and power plant-2 to switch to gas by 2025." *QazaqTV.com*, November 2, 2020, https://web.archive.org/web/20220517123921/https://old.qazaqtv.com/en/view/business/page_217948_almaty-combined-heat-and-power-plant2-to-switch-to-gas-by-2025/.

from December 2017 and ending with March 2020.[4] We focus our data collection efforts on winter months due to the abnormal pollution particular to Almaty winters that we detailed in the preceding section. Pollution is not only higher during these months, as we document later in this section, but the potential for exogenous variation due to mountain winds and temperature inversions is higher in winter as well.[5]

The rest of the section proceeds as follows. We first detail our data sources. We next discuss our choices and constraints for data aggregation, and lastly we discuss descriptive summary statistics and figures for our sample. This discussion also compares our pollution sample to related work.

3.1 Data sources

Our data on air pollution comes from AirKaz.org (2017-20), an independent air quality monitoring network which measures PM2.5 across Almaty, and more recently in other cities of the country. As the recent World Bank report of Zlatev et al. (2021) notes, AirKaz, and not the government, was until recently the only source for PM2.5 data in Almaty. Moreover, evidence that governments manipulate pollution sensor data (Zou 2021) suggests that sensor data from an independent monitoring organization is a clear advantage. The AirKaz network reports real-time PM2.5 data on the website using sensors across Almaty.

AirKaz provides a public archive of PM2.5 data containing daily averages for every sensor in Almaty between mid-March 2017 and early September 2020. Based on the overall time range of AirKaz and the rationale presented at the outset of this section to focus on the months December to March, we focus on three winters spanning 12 months: between Dec. 2017 to Mar. 2018, Dec. 2018 to Mar. 2019, and Dec. 2019 to Mar. 2020. Our analysis of pollution data over the 3-winter horizon is constrained to be at the daily level, as only daily averages for sensors are provided.[6] We confirm in the AirKaz data that the months we selected have higher pollution: the PM2.5 average is $89.7\mu g/m^3$ for the 12 months we

4. The selection of months is based on covering both the "meteorological" definition of winter of December, January, and February as well as the "astronomical" definition of winter from roughly December 22 until March 21. See, e.g., "Meteorological versus astronomical seasons." *ncei.noaa.gov*, March 18, 2021, https://web.archive.org/web/20220601012450/https://www.ncei.noaa.gov/news/meteorological-versus-astronomical-seasons

5. We confirm this point about our instruments when conducting placebo tests for effects of summer pollution instrumented by summer instruments on current crime. First-stage F-statistics for the 183-day shifts in Table A9 reflect that our instruments weakly instrument pollution in the summer.

6. An alternative approach to gather higher frequency data is to scrape AirKaz in real-time. We have done so in an earlier working version of this study, using data at an 8-hour frequency, but only for the period December 21, 2019 until March 31, 2020. Our main results of PM2.5 pollution's effect on the total crime rate also hold for this higher-frequency analysis of pollution, though the analysis based on one winter of data is more limited in controlling for seasonality.

study and scrape crime data to match, while the average for the rest of the AirKaz data is $30.2\mu g/m^3$.

Our crime data comes from Qamqor (2017-20), the official governmental source run by the Committee on Legal Statistics and Special Accounts of the General Prosecutor's Office. This source reports the initiation of investigation for each crime on the city map and shows information on crime type (i.e., articles of the criminal code), crime severity, time of day, and location. Students under our supervision scraped the webpage to obtain complete crime data for the 12 winter months for which we have AirKaz pollution data. The degree of individual offenses is assigned by the police departments. The assigned severity of crime varies for offenses within a single crime code, such as robbery. With this data, we can later assess whether the effects of air pollution on crime vary by crime severity.

We follow common practice in constructing crime rates per 100,000 population, so we require population data. We obtain population data by city districts, i.e. rayons, from Kazakhstan Bureau of National Statistics (2017-2020). The population data is annual, valid for the start of each calender year, and we match each cross-section of annual data to each winter.

We use the online weather resource Reliable Prognosis, rp5.kz (Raspisaniye Pogodi 2017-20a) for wind direction, temperatures, and most of our time-varying weather controls. The site is developed and maintained by a company licensed to operate in the field of hydrometeorology since 2004. The site aggregates data from thousands of weather stations worldwide, including stations in Almaty. This source has been previously used in several environmental studies in Kazakhstan (e.g., Kerimray et al. 2020; Assanov et al. 2021). The Almaty airport weather station, near the city center, is our primary source of weather data. The site provides weather observations from the station for every half hour. Table 1 lists all the specific weather variables we consider as controls.

We use a second weather station in Almaty (Raspisaniye Pogodi 2017-20b) to obtain precipitation data and high-elevation temperature data. This station reports weather every 3 hours through most of the sample, though evening data at 6pm and 9pm is missing in winter of 2017-18. To recognize temperature inversions, we compare temperatures from the two weather stations at the two different elevations. We base our temperature inversion measure on observing an inversion for every observation in the day, and precipitation is based on average precipitation over the day expressed at a per 24-hour rate.

We discuss the wind direction and temperature inversion data further when detailing our instrumental variables in section 4.3.

3.2 Data aggregation

We conduct our main analyses with two sample of different time aggregation: (1) an unbalanced panel of daily data of crime rates and pollution for 7 city rayons and weather, covering 2275 rayon-date observations, and (2) a balanced panel of data at 8-hour frequencies of crime rates (and no pollution) for 8 city rayons and city weather, covering 8736 rayon-time observations, where time refers to 0-8h, 8h-16h, or 16h-24h within a date. The first panel, which we refer to as the "daily data", we use to estimate pollution's effect on the crime rate. The second panel, which we refer to as the "8-hour data", we use for reduced-form analysis of our instrumental variables' effect of the crime rate. The second panel can have greater rayon coverage and higher frequency because it is not limited by the pollution sensor data availability. We explain further the choices and constraints in constructing each panel.

Both panels are aggregated within time periods to the 8 rayons of Almaty. The rayon is the natural unit of analysis for the city, and other borders we select would be more arbitrary. The rayon is the only unit for which we have population data to construct crime rates, and our individual crime location data is already officially classified by rayon. For the pollution data, we classify pollution sensors by rayon according to the geographic location and rayon borders. Zhetysu rayon contains no sensors during the 12 months of our sample, so our pollution data covers only 7 of the 8 rayons.

For the reduced-form analysis, we have more flexibility in choosing the level of time aggregation for almost all variables. The aggregation choice involves tradeoffs. A higher level of aggregation reduces the sample size and precision of estimates. It also reduces potential for identification coming from variation within a day. But a finer level of aggregation may have more substantial violations of the stable unit treatment value assumption (SUTVA) that there are no unmodelled spillovers between treatment and unit of analysis, and avoiding major violations of SUTVA is necessary for any causal interpretation of our results (see, e.g., Morgan and Winship 2014). Within finer aggregation, pollution in one time period is more likely to impact crime in a different time period, and crime itself is more likely to occur in one period and be reported in another.[7] Herrnstadt et al. (2021) likewise mention the possibility of such spillovers to justify focusing on a daily sample.

Given the tradeoffs of aggregation, we focus on an 8-hour time unit for our reduced-form analysis, while also being careful to consider daily aggregation for robustness. The three times-of-day roughly correspond to overnight, daytime, and evening during Almaty winters. The additional variation we observe in crime rate and in mountain winds at the

7. The SUTVA argument applied to space also suggests appeal in using the rayon as the unit of analysis rather than a finer level of geographic aggregation. We also check that our main analysis is robust to aggregating northern and southern rayons in column 1 of Table A7.

8-hour aggregation in Table 1, as well as the larger sample size, make the 8-hour time unit appealing. However, the potential for spillovers between time periods and the need for a stable unit of analysis suggest aggregation only marginally finer than the daily aggregation that is common practice in the pollution-crime literature, so we focus on 8 hours rather than any finer aggregation.

3.3 Descriptive statistics and figures

Here we focus our discussion on data for the total crime rate and pollution, and we postpone discussion of other variables until later in the paper. The crime rate summary statistics in Table 1 reports crime rates per 100K at a per 24-hour rate. To understand better drivers of the total crime rate, we note that the 4 most common crime codes reported (for major theft, fraud, robbery, and petty theft) account for 88% of all crimes reported in our sample.[8]

The pollution section of our summary statistics in Table 1 confirm that air pollution is large and variable in our data. Though we mainly use log(PM2.5) in our analysis, we also report PM2.5 in $\mu g/m^3$ and the Air Quality Index (AQI) of the U.S. Environmental Protection Agency (EPA), as AQI is a convenient standard for comparing across studies of different pollution types. Our sample's mean PM2.5 concentration of $89.71\mu g/m^3$ is more than 17 times the annual concentration of 5 recommended by the World Health Organization (WHO). Our sample's average AQI of 162.13 is within the range that the EPA considers very unhealthy for all groups.

Our Appendix Table A1 provides comprehensive comparisons of the range of pollution in our study to other studies of pollution and crime, though we also provide a few key comparisons here. The mean pollution that we observe is much larger than almost all other studies of pollution. Our sample's average PM2.5 level is more than 3 times the average PM10 level studied in Chicago 2001-12 (Herrnstadt et al. 2021). The maximum AQI of 103.6 that Bondy et al. (2020) report based on all measured pollutants studied in London from 2004-05 is the 11th percentile of the AQI distribution for our Almaty data. Comparing our sample to contemporary studies of pollution and crime in emerging markets, we observe lower pollution in Kazakhstan than in India, where Singh and Visaria (2021) conclude that avoidance behavior causes pollution to reduce crime. Our average PM2.5 level is 4 times the average level that Zarate-Barrera (2022) observes in Mexico City. The level of pollution that she identifies as a cutoff when avoidance behavior leads to marginal pollution reducing

8. Like most other crime studies, we are reliant on reporting. One concern is whether crime detection time is lengthy for fraud, the second-most common crime type in our sample. A survey for Kazakhstan by PwC (2016) notes that asset misappropriation is the most common type of fraud and is usually detected promptly.

crime, AQI 120, is the 18th percentile of the AQI distribution of our Almaty. So, winter pollution in Almaty is typically at levels well beyond the levels that have led to pollution reducing crime in Mexico City.

To understand the overall distribution of PM2.5 in our sample, we plot a histogram of both PM2.5 and log(PM2.5) in Figure 2. We can observe that PM2.5 data is substantially right skewed. We calculate that that the sample median PM2.5 pollution is 76.9, well below the mean of 89.7 that we report in Table 1. For log(PM2.5), the distribution is more symmetric: we calculate a median of 4.34 which is almost identical to the mean of 4.33 reported in the table.

To understand common time series variation in pollution and the crime rate, we plot in Figure 3 the daily averages across rayons of the crime rate and log(PM2.5) for our daily data. The crime rate we plot is normalized by dividing through by its sample mean. Understanding the time patterns in the data is important since the limits in variation of instruments across rayons within days will prevent us from simply using date fixed effects to correct for all spurious time relationships. Apparent periodicity in the data by week suggests the need to adjust for day-of-week effects, as is done in, e.g., Bondy et al. (2020). We see seasonal patterns in the data, especially with lower pollution as the winter progress, and this suggests the need to include day-of-year effects. Importantly, there can be common time trends within a year: crime follows more of a downward seasonal trend in 2020 rather than other years, and this suggests a need for our analysis to include week-year fixed effects.

4 Estimation strategy and main results

This section focuses on estimating the effects of PM2.5 air pollution on the total crime rate. Our preferred empirical strategy employs mountain winds and temperature inversions as instrumental variables for the endogenous explanatory variable of log(PM2.5). We primarily rely on IV estimation of linear models, though we also consider a PPML control function approach in our appendix. To best support the exclusion restriction that air pollution is the only channel through which our instrumental variables affect the crime rate, we control for alternative channels by including time-varying weather controls and various fixed effects.

The remainder of this section proceeds as follows: we first discuss methodological choices common across our approaches such as the choice of fixed effects and controls. Largely, we follow related studies in our choice of controls and fixed effects, and we justify any deviations. The first results we present are for a parsimonious but limited fixed effects model that treats pollution as exogenous. Given potential exogeneity violations of air pollution, we next develop our IV strategy. We present results for our linear IV estimation, which instruments for the potentially endogenous pollution variable. We provide further confirmation for these results by estimating the reduced-form effects of the IVs on the total crime rate. Lastly, we summarize various robustness checks that are presented fully in the Appendix.

4.1 General methodological issues

We discuss here methodological choices common to all of our approaches. These include our focus on linear models, our approach to estimating standard errors, our choice of fixed effects, our choice of controls, and our approach to estimating standard errors.

We rely primarily on linear models for our main estimation approach, while we leave discussion of PPML estimation to the appendix. With our daily data, only 5 observations (0.22%) of the sample are zeroes, whereas a larger share of zeroes would more clearly justify a need for PPML estimation. Importantly for our linear models, we can estimate standard errors of Driscoll and Kraay (1998) which allow for panel correlation and also address serial correlation.[9] Driscoll-Kraay standard errors are ideal for addressing both panel and serial correlation in our data, since our panel dimension of 7 or 8 rayons is too small to satisfy the asymptotics necessary for full two-way clustering by rayon and date.[10]

We focus on specifications using log(PM2.5) rather than PM2.5 pollution as the endogenous explanatory variable. One reason for the transformation builds on the analysis of Section 3.3: the distribution of PM2.5 is substantially skewed, while log(PM2.5) substantially reduces the skew. The transformation then reduces the possibility that results could be driven by high-leverage observations in the right tail.[11]

The dependent variable for all models in this section is the total crime rate normalized by dividing through by the sample mean total crime rate of the balanced panel. The coefficient estimate can then be interpreted as the PM2.5 elasticity of the total crime rate, evaluated at the mean crime rate. With the log transformation of PM2.5 as the explanatory variable, no additional normalization of PM2.5 is required for this elasticity interpretation.

Rayon fixed effects allow us to control for rayon-constant unobservables that could otherwise lead to spurious relationships between air pollution and crime. There would be upward bias if lower-income individuals live in high-pollution rayons and commit crimes where they live. There would instead be downward bias if criminals target higher-income areas with less

9. We select the bandwidth recommended by Newey and West (1994), which is the typical default value in computational implementations.

10. The minimum dimension of clusters is what is relevant for asymptotics in two-way clustering, and 8 clusters would be too few for clustering asymptotics (see, e.g., Cameron and Miller 2015).

11. We also estimate results for both linear IV and PPML using PM2.5 as the endogenous explanatory variable (in Appendix Tables A4a and A4b) to confirm that our finding of a pollution-crime relationship does not hinge on this transformation decision.

pollution.

Various time fixed effects allow us to control for unobservables that are constant across specific time categories. Following the discussion of our time-series pollution and crime rate plots in Section 3.3, these time fixed effects are essential to avoid spurious results from seasonal or weekly common time trends. Because the instrumental variables we ultimately consider are largely constant across rayons, we cannot use time fixed effects for each individual time period, but we can control for time effects of higher frequency. Day-of-week effects control for weekly patterns in behavior. Day-of-year effects control for seasonal patterns across the three years in our data. Week-year fixed effects control for trends that may be specific to an individual year. We also consider rayon-day-of-year effects to control for season patterns that may vary across space. And when we estimate using 8-hour frequency rather than daily frequency, we estimate 8-hour-day-of-week and 8-hour-day-of-year effects to control for weekly patterns and seasonal patterns even within a day.

There are potential important causal pathways from weather characteristics to both air pollution and crime (and not vice versa), so we consider several weather variables as controls that are common across our rayons. The list of weather controls includes temperature indicators for bins of size 5-degrees Celsius, relative humidity, wind velocity, total precipitation, and barometric pressure.

In reporting our main estimates, we build up the model by first including only $log(PM2.5)$ as an explanatory variable with essential rayon fixed effects and day-of-week effects. We then gradually add in day-of-year fixed effects, controls, and rayon-day-of-year effects.

We conclude this subsection by summarizing how our methodological choices compare to other prominent air pollution and crime studies referenced earlier. We follow both the Chicago and London study in reporting both linear and PPML results, and there is no consistent standard as to which approach is the main focus. Our choice of controls reflect variables that were common to both the Chicago study and the London study. We follow the London study in using rayon fixed effects and day-of-week fixed effects, and we are somewhat more aggressive in addressing time trends by using week-year fixed effects rather than month-year fixed effects.[12] With three winters of data, we can address well seasonality through day-of-year effects and rayon-day-of-year effects (whereas the London study spanned fewer years), so we do so. And to recap points already discussed at length, we rely on Driscoll-Kraay standard errors to address both panel and serial correlation given our small panel dimension, so we cannot rely on multi-way clustering like in the London study or the

12. Table A6 includes additional robustness checks of specification motivated by the London study. Column 3 of this table reports IV estimates using month-year FE rather than week-year FE. Column 5 of this table reports IV estimates using four additional controls that were in main specifications of the London study (see the table notes for specifics).

Chicago analysis of highways. We focus on log(PM2.5) rather than PM2.5 because of the skewness of PM2.5 data in the presence of abnormal air pollution in our sample.

4.2 Linear fixed effects estimation with exogenous PM2.5

Our first estimates are for a linear fixed effects model that presumes the exogeneity of air pollution, conditional on various fixed effects and controls, and we postpone consideration of air pollution endogeneity to the next subsection. Our model for the effects of air pollution on the total crime rate, aggregated across all crimes for rayon i within a 24-hour period t, takes the form

$$Crimerate_{it} = log(PM2.5)_{it}\beta + X_t\xi^{(0)} + T_t\tau^{(0)} + \alpha_i^{(0)} + \varepsilon_{it}, \tag{1}$$

where X_t denotes the weather and seasonal controls, T_t denotes indicators for the various time fixed effects, and rayon fixed effects are represented by the parameters $\alpha_i^{(0)}$. For parameter vectors related to controls and fixed effects that are common across our estimation equations, we denote the corresponding parameters with the same greek letters but use superscripts, e.g., (0) above, to clarify that these coefficients will vary across equations. In the last specification that we estimate in our usual progression of fixed effects, we include rayon-day-of-year effects, in which case the equation above becomes specified correctly once replacing $\alpha_i^{(0)}$ with $\alpha_{it}^{(0)}$.

We report the linear fixed effects results in Table 2, and we find a positive relationship between $log(PM2.5)$ and the total crime rate under the presumption that $log(PM2.5)$ is exogenous.[13] The null hypothesis of no relationship is rejected at the 1% level for 4 of the 5 specifications and at the 5% level for the first specification without fixed effects and controls.

Note that the estimated coefficient falls from 0.152 to 0.136 with the inclusion of day-of-year fixed effects (moving from Column 2 to Column 3) and from 0.136 to 0.0725 adding week-year fixed effects (moving from Column 3 to Column 4). The results suggest there is important bias from common trends controlled for by the week-year fixed effects, even after adjusting for the seasonal effects.

Even with our richest set of fixed effects and controls, the linear fixed effects approach can still lead to biased estimates. One possible source of bias is unobserved shocks that affect both pollution and crime. Such shocks may not be absorbed by our fixed effects if they are lower frequency than the time fixed effects we include, or if they are idiosyncratic to both time and place. And as Deryugina et al. (2019) note in justifying their IV strategy, the nonrandom assignment of PM2.5 exposure can be problematic if there are heterogeneous

13. The R-squared that we report for all linear fixed effects specifications is the total R-squared.

treatment effects, and IV can remove bias in estimation of average treatment effects. To address problems inherent with the linear fixed effects approach, we pursue an instrumental variables strategy, and we discuss further potential sources of bias after obtaining our IV estimates.

4.3 Instrumental variables strategy

The terrain of Almaty makes the city well-suited for an identification strategy based on the effects of mountain winds and temperature inversions on air pollution. Recalling Figure 1, there is high terrain from the Tien Shan Mountains to the city's southeast corner. Winds from the southeast have the shortest path from the mountain regions to the city. Moreover, the winds from the mountains also protect the city from air pollution emitted by two coal plants, one within the northeasternmost rayon of Alatau and the other just northwest of the northwesternmost rayon of Turksib. Temperature inversions, in which cold air is trapped below a layer of warm air, trap pollution in the city, as seen in Figure A1. As Table 1 shows, our approach to measuring temperature inversions finds them for more than 35% of the observations in either of our samples.

We design our mountain wind instrument to have the strongest impact on air pollution, based on our a priori knowledge of Almaty's geography. We define our instrument to be the [0,1] share of weather observations within each time period for which winds are blowing from the southeast, south-by-southeast, or east-by-southeast for the city's 5 southern rayons (so there is no variation in the instrument across these five rayons). We then remain agnostic about the effects of other wind directions in our main specifications or the effects of wind on the northern rayons. We define the northern rayons to be untreated by the mountain winds, i.e. the instrument is constant at zero, because the northern rayons are more distant from the mountains, and there is no reason to expect that winds from heavy urban activity to the city's southeast would be cleaner than other directions where there is less economic activity. The winds' impact specific to the southern rayons closer to the mountains also provides an important source of cross-sectional identification. Although the recent air pollution literature focuses on models with many instruments for many wind directions across many cities in one country (as in Deryugina et al. 2019) or many regions in one large city (as in Bondy et al. 2020), our mountain wind instrument is appropriate for our context, a medium-sized city with features that allow us to identify a specific source of wind variation on pollution.[14]

14. We also consider the robustness of our results to a flexible IV specification with several wind instruments for different regions, in Column 4 of Table A5. This specification uses as instruments our baseline inversion measure and 8 wind-direction instruments (4 wind direction instruments for northern rayons, 4 wind direction instruments for southern rayons, and no wind is the omitted category).

Almaty is an ideal city for measuring temperature inversions, which trap air pollution throughout the city. Because the inversion affects the whole city, it allows us to assess air pollution treatment of the northern rayons which are too distant from the mountain winds, while also providing an additional source of identification for air pollution's effects on the southern rayons. Because of Almaty's varied elevation, we can identify inversions from on-the-ground weather stations, one located at a low elevation similar to most of the city and one located at the city's high-elevation southeast corner. We can then compare temperatures between the weather stations to assess whether cold air is trapped below the warm air for every time period. We define a dummy which equals 1 if within a day all low-elevation temperature readings are less than high-elevation temperature readings, the later of which are most often available at regular 3-hour periods.[15] From observing 8 measurements of inverted temperatures within the day, we can confirm that cold air is trapped below the warm air. We use this daily inversion measure both for specifications with daily aggregation and the reduced-form specifications with 8-hour time frequency so that we identify suitably strong inversions.[16] Even with this demanding standard for our inversion measure, we still find them for more than 35% of observations.

To assess whether our instruments satisfy the requirement of relevance, we must report a weak instrument test which is valid in the presence of Driscoll-Kraay standard errors. Unless otherwise indicated, we report the F-statistics of Kleibergen and Paap (2006). We discuss further assessment of instrument strength across specifications in the next subsection.

For our instruments to satisfy the necessary exclusion restriction, we must rule out other pathways between our instruments and the crime rate. Common causes of our instruments and the crime rate are the clearest potential violation. Plausible common causes include weather variables, times-of-day unobservables, and seasonal unobservables. The current weather controls and fixed effects previously discussed are then crucial to our identification strategy. Ultimately, our identifying assumption is that after conditioning on all current weather controls and fixed effects, there is no remaining relationship between our instruments and the crime rate, other than through air pollution. After conditioning on controls and fixed effects, we see no obvious pathway between mountains winds or inversions and outcomes of

15. There are missing readings between 6pm and midnight from the high-elevation station in the winter of 2017-18. In this case, we define the inversion based on 5 readings.

16. We also consider alternative instruments for inversions. One possibility is temperature difference rather than an indicator, and we estimate a specification like this in Table A5 column 5. For our reduced-form specifications, we consider a measure of inversions based on maintaining temperature differences for only an 8-hour frequency, which would include weaker inversions.

criminal activity.[17]

4.4 Linear IV results

We follow a standard IV estimation strategy, where we estimate equation 1 while using mountain winds and temperature inversions as excluded IV to instrument for the potential endogenous explanatory variable, log(PM2.5). So when estimating our null hypothesis of interest, no effect of log(PM2.5) on the total crime rate, we consider only the variation in air pollution through our excluded instruments of mountain wind and temperature inversions. We estimate the model using code by Correia (2017) and Baum et al. (2010). The former facilitates absorption of high-dimensional fixed effects, and the latter we use to calculate our IV estimates with Driscoll-Kraay standard errors. In our tables, we follow the same progression of controls and fixed effects as in the earlier linear fixed effects estimation.

Linear IV point estimates are equivalent to estimates from a two-stage least squares (TSLS) approach, and the first-stage equation is useful in evaluating strength of the excluded instruments. We can write the first-stage equation as follows, with Z_{it} used to denote the excluded instruments.

$$log(PM2.5)_{it} = Z_{it}\zeta + X_t\xi^{(1)} + T_t\tau^{(1)} + \alpha_i^{(1)} + u_{it}. \tag{2}$$

Appendix Table A2a reports linear estimates from this first stage.

Our IV results in Table 3 confirm a large, positive air pollution elasticity of the total crime rate that is always statistically different from 0 at the 5% level across all specifications and at the 1% level for the two specifications richest in fixed effects. Diagnostics indicating instrument strength and overidentification restrictions remain consistent with our identification strategy, though the specification with rayon-day-of-week effects requires some additional attention. In the appendix, we include additional estimates from the second stage controls in Table A2b.

Comparing across specifications, we see the results are relatively stable once controls have been included, and there is reason to be skeptical of results without controls. The first stage F-statistic falls between column 1 and 2, because the controls have a close relationship with the instruments. Because the controls also have a relationship with the crime rate, the validity of IV exogeneity assumption requires inclusion of controls to shut down pathways between the instruments and the crime rate.

17. We also follow the advice of a recent survey by Mellon (2021) to consider other possible weather relationships in the literature that could violate exclusion restrictions, but there is no study there that suggests relationships of mountain winds or inversions other than through their impact on air pollution.

Among the four sets of estimates with controls, our preferred specification is column 4 with the rayon fixed effects, day-of-week fixed effects, day-of-year fixed effects, and week-year fixed effects. For this specification, the PM2.5 elasticity of the total crime rate is 0.392 with a standard error of 0.106. The F-statistic of 30.22 suggests instruments are sufficiently strong. The (Hansen's) J-statistic p-value is 0.851 so we cannot reject the validity of the overidentifying restrictions. Additionally for this specification, we conduct a GMM-distance endogeneity test (using methods of Baum et al. (2010)) and we reject the hypothesis of no endogeneity with a p-value 0.0015.

The first stage F-statistics results already suggest that our mountain wind and inversion instruments are sufficiently strong, though we discuss here limited-information maximum likelihood (LIML) results confirming instrument strength. Since LIML is less unbiased, though less efficient than our baseline IV estimation, Angrist and Pischke (2009) suggest comparing LIML estimates to IV estimates to check instrument strength. We report LIML results in Appendix Table A2c. For the first four columns where instruments are strongest and the F-statistic is always well above 20, the LIML estimate deviates at most 0.3% from the IV estimate. For the fifth column, where we include the rayon-day-of-year fixed effects, the F-statistic is lower at 15.36, as some the variation in the wind instrument is seasonal. The LIML estimate of 0.430 is still within 2% of the IV estimate of 0.422, and the results of the fifth column overall confirm that seasonal trends specific to rayons are not driving our results.[18]

The IV estimate for the PM2.5 coefficient in Table 3 is noticeably larger than our estimates from the linear fixed effects model where air pollution is exogenous (Table 2). This variation in estimates begs for an explanation. One possibility, detailed at the end of Section 4.2, is that the linear fixed effects estimates still suffer from unobserved shocks not absorbed by our fixed effects. We speculate that this problem could be especially relevant for high-pollution settings in winter. A shock that leads households to stay at home would, in a middle-income urban center like Almaty, increase demand for coal-fueled central heating or home burning of fuels—major sources of Almaty pollution documented by Zlatev et al. (2021). The shock then would increase air pollution, while staying at home would also reduce social interactions that lead to crime. The consequence of such unobserved shocks would be downward bias in our estimate of air pollution's effect on crime.[19]

18. The robustness of results in reduced-form estimation when adding rayon-day-of-year effects, between column 4 and column 5 in Table A3c and Table 4 also provide also support that seasonality specific to rayons is not driving our results.

19. Our controls do eliminate some bias when we estimate without IV. For example, people will stay home more on an unusually cold day. By including binned temperature among our controls, we eliminate this channel. The increase in coefficients between Column 1 and 2 in Table 2 is consistent with our controls reducing downward bias. But our controls cannot eliminate all such bias, hence the need for IV.

Our preferred estimates from column 4 of Table 3 suggest an air pollution elasticity of crime rate that is larger than other leading pollution-crime estimates. With the IV model for the effect of log(PM2.5) on the crime rate, we can interpret our point estimate $\hat{\gamma} = 0.392$ for the coefficient of log(PM2.5) as the air pollution elasticity of the expected crime rate evaluated at the sample mean crime rate. This is a larger elasticity than in related work. For Chicago, we determine from Herrnstadt et al. (2021) that the average PM10 elasticity of violent crime is 0.06.[20] For London, Bondy et al. (2020) report an AQI elasticity of the total crime rate equal to 0.08 for their preferred IV estimates. Our estimate is precise enough that these point estimates lie well below the 95% confidence interval for our elasticity estimate of [0.183, 0.602].

A widespread approach to assessing overall economic importance is comparing the effect of a 1 standard deviation (s.d.) change in pollution to the standard deviation of the crime variable of interest, and here again, we find relatively large effects for the literature, though we suggest interpreting such results with some caution. In our sample, a 1 standard deviation increase of log(PM2.5) $\sigma = 0.58$ (which roughly corresponds to an 80% increase in PM2.5 air pollution) would then increase the expected crime rate by 22.7% from its mean. Since 1 s.d. in the crime rate is roughly 60% of the mean crime rate, this change corresponds to an increase of 0.37 standard deviations of the crime rate from the sample's mean. For the Chicago-wide IV estimation that Herrnstadt et al. (2021) emphasize for policy analysis, a 1 s.d. increase in PM10 air pollution raises violent crime by 2.9%, which corresponds to 0.09 of a s.d. of daily violent crime for their sample, when evaluated at the sample mean of daily violent crime. Bondy et al. (2020) highlight their estimate that a day of pollution in London with AQI above 35, relative to a baseline day with less than 20 AQI, increases criminal activity by 0.04 s.d. We present such results since they are common in the literature, but we suggest interpreting them with some caution. Considering that our fixed effects and controls explain over 40% of the variation in log(PM2.5) in the first stage, we are then estimating the effects of log(PM2.5) over a smaller variation than the sample's overall log(PM2.5) variation, so the 0.37 s.d. figure we derive depends on extrapolation that may not be immediately obvious.

Our estimates ultimately answer an important question about the marginal effects of air pollution on total crime when air pollution is already at abnormal levels. Ex-ante, we could not know whether all negative effects on human behavior occur at relatively low levels of pollution found in the U.S. or U.K., or if there are larger effects at larger levels of pollution

20. Herrnstadt et al. (2021) emphasize their IV result that a 1 s.d. increase in PM10 air pollution raises total Chicago crime by 2.9%. This estimate implies an average PM10 elasticity of violent crime of roughly 0.06, since the authors estimate log(crime) as a function of standardized PM10, and the sample's mean PM10 is roughly twice the standard deviation.

common in low or middle-income countries. We find even larger PM2.5 elasticities of the crime rate at the levels of winter pollution that we observe in Almaty.

4.5 Reduced-form approach

The last estimation strategy we discuss uses linear fixed effects to assess a reduced-form relationship between our instrumental variables and the crime rate. With the reduced-form estimation, we have a choice of our level of time aggregation, because we have higher-frequency crime data and no longer need to rely on daily pollution data. We focus on estimating reduced-form with an 8-hour frequency, as explained in Section 3.2. We follow the same progression of controls and fixed effects as in prior tables.

$$Crimerate_{it} = Z_{it}\omega + X_t\xi^{(3)} + T_t\tau^{(3)} + \alpha_i^{(3)} + \upsilon_{it}, \tag{3}$$

where Z_{it} includes the IVs of wind direction and temperature inversion. We focus on inference using the null hypothesis that $\omega = 0$, such that there is no reduced-form effect of the instruments on the crime rate. The reduced-form approach captures many possible pathways between our instruments and the crime rate, whereas any IV approach will capture only pathways related through the measured air pollution. Reduced-form estimates could then capture exclusion restriction violations, e.g., if our instruments affect other pollutants that are uncorrelated with log(PM2.5), or if we mismeasure log(PM2.5). Reduced-form estimates of the expected signs (a negative relationship between mountain winds and crime rate, a positive relationship between temperature inversions and crime rate) offer additional evidence that our main approach is not (on net) missing any major pathways between our instruments and the crime rate.

Reduced-form estimates provide further support for our main empirical strategy. Table 4 shows that across specifications, we estimate that mountain wind decreases crime, and inversions increase crime. The estimated coefficients are almost all statistically distinct from 0 at the 1% level (two are at the 5% level), and the coefficient signs are as we would expect from our identifying assumption that the instruments are related to the crime rate only through PM2.5 air pollution. With a sample size of 8,736 with the 8-hour frequency, precision of estimates is improved. As we noted in Table 1, the variation in mountain wind is larger in the 8-hour sample, and we can take advantage of this for identification. The results are consistent with our exclusion restriction, and so our reduced-form results permit greater confidence in the reliability of our IV estimates.

The reduced-form approach we pursue here is by now common practice in the air pollution literature. The survey of Aguilar-Gomez et al. (2022) offers the reduced-form approach as one solution to assessing underidentification when the number of pollutants exceeds the number of the instruments (whereas bias can be introduced by adding other pollutants as controls or separately estimating IV equations for each pollution variable). Within the specific literature measuring effects on crime, our reduced-from approach is similar to Herrnstadt et al. (2021) estimating effects of wind blowing from highways or Jones (2022) estimating effects of dust storms.

4.6 Robustness and placebo specifications

A variety of alternative estimation approaches, model specifications, and placebo tests further confirm our main results. We detail these approaches in Appendix A, and we summarize them here, aside from omitting a few that we have already mentioned. Our checks broadly confirm the robustness of the log(PM2.5) coefficients estimated in our main specification. The rejection of the null hypothesis that the pollution coefficient is as a high as 0.08 (the AQI elasticity estimate from the London study) is also quite robust though results of this test are more sensitive to our specification choices.

PPML estimation is worth considering for robustness, as it offers the advantage of restricting the conditional expected mean to be nonnegative, but it overall yields similar results to our baseline IV approach. There is ultimately no consistent pattern of whether PPML standard errors are larger than IV standard errors. For PPML estimation with endogenous PM2.5, we opt for the control function approach as a textbook method for approaching estimation with an endogenous explanatory variable.[21] A coefficient of interest, the PPML control function estimate for our preferred set of fixed effects, exhibits a standard error 20% higher than the IV standard error using the preferred set of fixed effects. We still reject the hypothesis of no pollution effect at the 5% level and we reject the hypothesis of a pollution effect equal to 0.08 at the 10% level.

To check the robustness of our preferred IV specification, we consider alternative instrumental variable specifications, alternative sets of controls, and coarser forms of aggregation. All specifications yield similar results in terms of estimating an effect of air pollution statistically distinct from zero. We additionally estimate placebo IV specifications and find no statistically significant effects of air pollution from irrelevant past and future periods on the current crime rate, when we consider shifts of 1 week, 1 month, and 6 months.

We also run a number of specifications to check the robustness of our reduced-form estimation. We estimate the reduced-form at the daily level of aggregation rather than 8 hours: this specification yields similar point estimates, though the mountain wind coefficient

21. See Wooldridge (2010) and Cameron and Trivedi (2013). We also rely on a recent book chapter (Lin and Wooldridge 2019) for methodological guidance, and we use software by Correia et al. (2020) for estimation.

is less precisely estimated. We estimate the reduced-form using a definition of inversions defined over 8 hours rather than 1 day: we still estimate a statistically significant relationship between inversions and crime, though the effect of an inversion in increasing crime is smaller since the inversions indicated are weaker on average. We estimate reduced-form results for current and lagged treatments, and these suggest our results are adequate in considering only contemporaneous treatment.

We run our reduced-form estimation using missing pollution in our daily data as a dummy variable, and we also interact the dummy with instruments in our reduced-form estimation. We see no noticeable change in results while we fail reject the joint test that the three dummy coefficients are zero, so this specification suggests that the missing pollution data is random and not affecting our IV estimation.

5 Results for crime types

This section focuses on estimating effects of air pollution for individual crime types and broader crime categories. These estimates offer insight into the mechanisms of how air pollution affects crime. We apply existing theory and also build new theory.

To frame our initial analysis of crime types, we first briefly summarize existing theories on the mechanisms that could lead air pollution to increase crime: heightened aggression that affects violent crime, and higher discounting that could affect property crime or violent crime. Using IV estimation, we find that increases in total crime are driven by increases in property crime and not violent crime, and theft in particular among property crime.

To explore additional disaggregated impacts of air pollution, we build on the theory that air pollution increases discounting and assess whether the consequences could be heterogenous for crimes of distinct severity. In our context, greater severity means crimes with larger punishment and greater probability of enforcement. Our theoretical analysis finds whether there is an impact by severity depends on the source of population heterogeneity that is most relevant in affecting crime rates. We analyze two extreme cases of exogenous population heterogeneity. When the relevant population heterogeneity is in the outside option, the pollution elasticity of the crime rate is larger for crimes of higher severity. When we consider population heterogeneity in the discount factor itself, the pollution elasticity of the crime rate does not vary by crime severity.

Our empirical analysis partitions our data with observed crime severity in Almaty into minor crimes and major crimes. Minor crimes are those officially classified with a code that literally translates as "not large severity", while crimes with codes reflecting more severe crimes are classified as major crimes. Using both an IV approach and a reduced-form approach, we assess how air pollution affects major vs. minor crimes. We find that pollution increases both major and minor crime rates, and we fail to reject the hypothesis that major and minor crimes have equal elasticities.

Throughout this section, we normalize crime rates for all crime types by dividing by the mean of the crime rate for the crime type, unless indicated otherwise. When using these normalized crime rates as dependent variables, we can interpret log(PM2.5) coefficients in IV models as the PM2.5 elasticities of these crime rates, evaluated at the mean crime rate for the crime type.

As for the empirical approach in this section, our IV estimation follows the approach of Section 4.4, and our reduced-form estimation follows the approach of Section 4.5, except we use other crime rates as the dependent variable rather than the total crime rate. We estimate all models with our usual controls and preferred set of fixed effects (rayon, day-of-week, day-of-year, and week-year).

5.1 Mechanisms for air pollution affecting crime

We briefly survey prior evidence for the relevant mechanisms through which air pollution could increase crime. We focus on mechanisms that operate through potential perpetrators' preferences: aggressive behavior and higher discounting.

The mechanism supported by the most evidence to date is that air pollution increases aggression. This mechanism finds support from the U.S. evidence that air pollution increases violent crime and has no effect on property crime. Results from U.S. data—Herrnstadt et al. (2021) using Chicago data, Jones (2022) using U.S. dust storms, and Burkhardt et al. (2019) across U.S. cities—are consistent with this hypothesis. The channel of aggression relates to a broader literature on hotter weather increasing conflict (Burke et al. 2015). A "taste for violence" can be integrated into economic models of criminals' preferences (Baysan et al. 2019), which the authors use to predict drug violence in Mexico, and their approach also helps to predict crime patterns from heat and rainfall in India (Blakeslee et al. 2021).

A distinct mechanism that can increase either violent crime and property crime is air pollution causing higher discounting of the future among criminals. Bondy et al. (2020) propose this mechanism within the classic Becker (1968) model of crime as a rational intertemporal choice, after finding that air pollution increases particular property crimes and violent crimes in London in 2004-05. The authors cite, in support of this mechanism, the experimental evidence of Koppel et al. (2017) on physical stress causing impatience and Riis-Vestergaard et al. (2018) on cortisol altering in time preference in favor of small immediate rewards. The two lab studies are relevant under the presumption that air pollution creates stress and discomfort.

Beyond the evidence that Bondy et al. (2020) cite, the hypothesis of higher discounting aligns well with findings of hyperbolic discounting and heterogeneous time preferences in modern studies from the economics of crime (surveyed in Chalfin and McCrary 2017) and criminology (surveyed in Apel 2022). Experimental evidence suggests that criminals disregard chances of apprehension in emotionally charged situations, and through interviewing criminals we learn that they seek out means for short-term alteration of their preferences (e.g. through substances) to reduce fears of being caught. Temporarily-heightened discounting of future consequences is therefore broadly important in explaining criminal behavior beyond just the increase in crime resulting from air pollution.

5.2 Effect of air pollution on common crime types

To assess heterogeneity of the impact of air pollution on crime, we first consider the four most common crime codes that we observe: major theft, fraud, robbery, and petty theft. Major theft and petty theft fall under distinct crime codes and largely differ by the magnitude of property involved.[22] All these crimes are classified as property crimes by the criminal code of Kazakhstan (Zakon.kz 2014), though robbery also includes elements that could allow it to be classified as violent crime.

We can see from Table 1 the relative importance of these four common crime types. For the daily data, the mean of each crime type divided by the mean total crime rate is as follows: 69.1% for major theft, 13.6% for fraud, 3.0% for robbery and 2.4% for petty theft.

We estimate IV specifications using each crime rate as a dependent variable. Columns 1 to 4 of Table 5 summarize the results. The log(PM2.5) coefficient for theft, the most common crime type, is statistically significant at the 1% level. The coefficient for fraud is also statistically significant at the 10% level. We estimate positive effects for petty theft and robbery but these are not precisely estimated.

In finding evidence of pollution affecting major theft, a property crime, our results run counter to the U.S. evidence, where effects have been found only for violent crimes. These results are then more in line with Bondy et al. (2020), who estimate statistically significant effects for some property crimes.

22. What we refer to as major theft is labelled as just "theft" in the crime code, though we call it major theft to distinguish it from petty theft. Major thefts are never classified as "not large severity" in our data so they also meet the definition of major crime that we apply later.

5.3 Effects of air pollution on property crime and violent crime

We next consider how air pollution affects broad aggregates of property crime and violent crime. Our definition of property crime is taken precisely from the criminal code of Kazakhstan (Zakon.kz 2014), and includes 14 crime codes for which we observe offenses. There is no official categorization of violent crime, so we aggregate every crime code whose description mentions violence, murder, bodily harm, or the threat of these. We do not include crimes of negligence that resulted in death or harm. On this basis, violent crime overlaps slightly with property crime for 3 codes—specifically, those covering robbery, brigandage, and extortion. Our definition of violent crime ultimately includes 17 crime codes for which we observe an offense during our sample period.

We can see from summary statistics in Table 1 that the vast majority of crimes are property crimes. The mean property crime rate in our daily sample is 90% of the total crime rate, whereas the mean violent crime rate is 5% of the total crime rate.

Columns 5 and 6 of Table 5 report our results for property crime and violent crime. We estimate a large coefficient for property crime, and the effect is statistically significant at the 1% level. For violent crime, we have more precisely estimated an impact statistically indistinguishable from zero and even slightly negative.[23]

Our results here contrast with much of the literature to date, which has consistently found an impact on violent crime. And with the exception of Bondy et al. (2020), this literature has found no impact on property crime.

Though we proceed to focus on theory where pollution affects the discount factor, because the Bondy et al. (2020) reading of the evidence suggests the discount factor is the preference parameter that decreases with higher pollution, we acknowledge that pollution could still in theory affect other crime preference parameters to reduce the perceived costs of crime. For example, a theory where pollution decreases a risk aversion parameter in the utility function would yield similar results, because the derivative of the expected utility of a crime with respect to either parameter (risk aversion or the discount factor) takes a similar form. So we add the disclaimer that results aligning with either theory should not be seen as specific evidence in support of the discount rate hypothesis, relative to theories of pollution affecting other utility parameters.

23. This result does not depend on our definition of violent crime. One alternative definition that we consider is violent crime excluding the three codes that overlap with the property crime definition. A second alternative definition is a narrow one including only murder, serious bodily harm, and medium bodily harm. Regardless of the definition, the point estimates for violent crime are slightly negative and statistically indistinct from zero. We do not report these results in tables due to their redundancy with our original definition of violent crime.

5.4 Theory of air pollution's impact on major and minor crimes

Since we have microdata on crime by severity, we build theory to understand how pollution's effects could vary by crime severity. The main conclusion of our theory is that the type of exogenous population heterogeneity matters for whether pollution has a heterogenous impact on crimes of distinct severity.

We consider two extreme cases. The first case we consider involves an exogenous distribution of households whose outside options vary. Outside option heterogeneity is a common starting point for earlier empirical literature on crime determinants: for example, the survey of Freeman (1999) lists legitimate labor market experiences first among incentives relevant for empirical studies of criminal decisions. For the second case, we consider the extreme of no heterogeneity in the outside option but heterogeneity in the discount factor. As noted earlier, studies today focus more on the role of discounting in criminal behavior: per the survey of Chalfin and McCrary (2017), "recent writing has increasingly characterized deterrence as part of a dynamic framework in which offender behavior is sensitive to their time preferences." So we simplify by focusing individually on these two forms of heterogeneity, in the outside option or in the discount factor, given their prominence in different eras of literature. Each model can be interpreted as a scenario when one or the other type of heterogeneity is substantially more important to the population of interest.

Model with outside option heterogeneity: We can summarize the crucial assumptions of the theory that follows as (1) enforcement and punishments are increasing in crime severity, (2) the key exogenous source of population heterogeneity in crime decisions is variation in the outside option of committing crime, and (3) crime opportunities are independent of crime severity.

We restrict the model of Becker (1968) by assuming that individuals have identical valuations of crime conditional on the discount factor $\beta \in (0, 1)$ and the severity of the crime s which determine its conditional payoffs, while there is exogenous population heterogeneity is in the outside option. Utilities of the outside option are normalized to be in the unit interval. We assume that the population distribution is uniform.[24]

Following Becker (1968), an individual commits crime if the expected utiltiy of crime is greater than the individual's outside option. Under our restrictions, an individual with outside option k commits a crime if and only if the following inequailty holds

24. We can extend results to a generic population cumulative distribution function F for the outside option. In this case, the share of population willing to commit a crime $x = F(V)$ rather than simply $x = V$ in the uniform case. We then find $x_\beta = F'V_\beta < 0$ and $x_{\beta s} = F'V_{\beta s} + F''V_\beta V_s$. The results of this section can be extended by adding additional structure such as $F' > 0$, $F'' < 0$, and crime punishments being strong enough, so $V_s < 0$. These restrictions on F are reasonably satisfied by the Pareto distribution, for example.

$$p(s)U(W(s) - \beta S(s)) + (1 - p(s))U(W(s)) \geq k$$

where the value of crime $W(s)$, the chance of punishment $p(s)$, and the cost of punishment $S(s)$ depend only on s, and we assume these are all strictly positive. We assume individuals have weakly concave and increasing utility, i.e., $U'' \leq 0$ and $U' > 0$, so the model can apply to populations that are all risk-neutral or all risk-averse. We henceforth write the left-hand side of the inequality, the expected utility of crime, as $V(s, \beta)$. (To preview results, $V(s, \beta)$ will also equal the share of individuals willing to commit a crime.)

We impose additional restrictions on the functions of s above: (1) $W'(s) > 0$, (2) $p'(s) > 0$, and (3) $W'(s) - \beta S'(s) \leq 0$. The first restriction ensures that the higher-severity crime is more appealing, while the second and third restrictions both reflect that expected punishment is higher for the higher-severity crime. The second restriction reflects a higher chance of being caught for the higher-severity crime. The third implies that marginal punishment for higher-severity crimes is at least proportional to marginal benefits. Notice that the first and third restriction also imply that $S' > 0$, i.e., punishment is higher for the higher severity crime.

We assume that higher pollution causes a lower discount factor. In our setup, lower pollution will uniformly increase a given population's common discount factor.

We define the observed crime rate per person $c(s, \beta)$ at some time to be the product of $x(s, \beta)$ (crimes committed per crime opportunities), enforcement $p(s)$ (crimes observed per crimes committed), and ω (a constant reflecting crime opportunities per person). We normalize units so $\omega = 1$ and do not consider it further.

We define $e(s, \beta)$ to be c_β / c, the discount factor elasticity of the crime rate, and this elasticity will be our main object of empirical interest. Importantly for interpretation, notice that comparative statics implying $e_s < 0$ will imply air pollution elasticities that increase with severity, given that the discount factor is decreasing in air pollution.

We derive comparative statics in s and β for values of these parameters such that the population share willing to commit the crime is in (0,1), i.e. neither 0 nor 1.

Results of model with outside option heterogeneity: We focus on the population share $x(s, \beta)$ willing to commit a crime of stakes s with discount factor β, and proceed to find the sign of the cross derivative $x_{s\beta}$. We first show that the first derivative $x_\beta < 0$, i.e., lower pollution that increases the discount factor will reduce crimes of all severities. We then show that the cross-deriative $x_{s\beta} < 0$, reflecting that this decrease is even larger for the higher-severity crime.

The first step of the derivation is to show $x(s, \beta) = V(s, \beta)$, so we can refer to the derivatives of x and V interchangeably. Let $\bar{k}$ be in the individual indifferent between crime and no crime, so $V(s,\beta) = \bar{k}$. Then individuals with $k < \bar{k}$ will commit crime, and individuals $k > \bar{k}$ will not, so $\bar{k}$ is also the share of individuals willing to commit crime x, and $x(s,\beta) = \bar{k} = V(s,\beta)$.

Next we argue that $V_\beta < 0$, i.e., a higher discount factor lowers crime of any severity. According to the definition of $V(s,\beta)$,

$$V_\beta = -pSU'(W - \beta S) < 0 \tag{4}$$

where the inequality follows because p, S, and $U'(\bullet)$ are all positive. Intuitively, a higher discount factor increases the perceived cost of expected punishment and reduces the number of individuals willing to commit crime.

Next we sign $V_{\beta s}$. Differentiating (4) according to the product rule, we have

$$V_{\beta s} = -p'SU' - pS'U' - pS[(W' - \beta S')U''] < 0,$$

where the inequality follows because all the terms p, p', S, S', and U' are assumed to be positive, while $(W' - \beta S')U''$ is non-negative since each term in the product is assumed non-positive. There is intuition for each of three negated terms in the cross-derivative. Both the first and second terms reflect higher deterrence for high-severity crimes from higher discount factors—specifically, the first term reflects the larger chance of being caught for a high severity crime ($p' > 0$) and the second term reflects the larger punishment for the high severity crime ($S' > 0$).[25] The third term contributes additional deterrence for risk-averse individuals if crime punishment is sufficiently increasing in severity, though our assumptions also permit this third term to be zero (e.g., if individuals are risk-neutral).

Next we sign V_s, which we can write as

$$V_s = W'E(U') - \beta S'U'(W - \beta S) + p'(U(W - \beta S) - U(W))$$

where $E(U')$ is the expected marginal utility. We can interpret the first term as the expected additional benefit from a crime of greater severity, the second term reflects costs of greater punishment, and the third term reflects costs of greater enforcement. The third term is strictly negative since $p' > 0$ and $U(W - \beta S) < U(W)$. As for the first and second terms, we have that $W'E(U') - \beta S'U'(W - \beta S) \leq 0$, because $W' \leq \beta S'$ from our assumptions on punishment strength, and $E(U') \leq U'(W - \beta S)$ because utility is weakly concave. We can

25. Our assumption that both $p' > 0$ and $S' > 0$ is stricter than necessary to obtain this sign for $V_{\beta s}$. As long as one of the two derivatives is sufficiently large, i.e. either enforcement is strong or punishment is strong enough, then the condition can still hold.

then conclude that $V_s < 0$.[26]

Now we turn to comparative statics of the discount factor elasticity of the crime rate $e(s, \beta)$. Notice $e(s, \beta) = x_\beta / x$, since by definition, $e(s, \beta) = c_\beta / c$ and $c = xp\omega$, and $p(s)$ and ω do not depend on β. The sign of e_s, through quotient rule differentiation, then matches the sign of $x_{\beta s} x - x_s x_\beta$. Since $x = V$, we have proven that the three derivatives in the expression are each negative, so $e_s < 0$. We summarize results as follows:

Result 1. *Assume that a population has outside options and preferences following "Model with outside option heterogeneity" above. Then the discount factor elasticity of the crime rate $e(s, \beta)$ is decreasing in severity, i.e., $e_s < 0$. Since PM2.5 decreases the discount factor, the PM2.5 elasticity of the crime rate is increasing in severity.*

Model with discount factor heterogeneity: Our model of discount factor heterogeneity follows the previous model but for two distinctions. First, we consider the extreme of no heterogeneity in the outside option k. Second, we assume the discount factor is the product $B\beta$, where β is common to the population and may fall due to pollution, and B is uniformly distributed over the population. Importantly, the source of heterogeneity B now complements β.[27] Consequently, more patient individuals with higher B experience a larger reduction in the discount factor when there is a common fall in β from pollution.

Results of model with discount factor heterogeneity: We can now write the expected utility of crime as the reduced form $V(s, \beta, B)$. If $\bar{B}$ reflects the individual who is indifferent between a crime and no crime, then the share x committing crime then equals $\bar{B}$, since all individuals with parameter lower than $\bar{B}$ have a lower discount factor of potential punishment, so they would also prefer crime. Then $\bar{B}(s, \beta)$ or equivalently, $x(s, \beta)$ is defined by the implicit function $V(s, \beta, \bar{B}) = k$.

As in the previous derivation, the discount factor elasticity of the crime rate $e(s, \beta) = x_\beta / x$. Here this equals $\bar{B}_\beta / \bar{B}$. From the implicit function theorem, $\bar{B}_\beta = -V_\beta / V_B$, which evaluates to $-\bar{B}/\beta$. The elasticity is then a constant $-1/\beta$, which does not depend on s.

Result 2. *Assume that a population has preferences and discount factor heterogeneity following "Model with discount factor heterogeneity" above. Then the discount factor elasticity of the crime rate $e(s, \beta) = -1/\beta$, and the PM2.5 elasticity of the crime rate does not vary with crime severity.*

26. $V_s < 0$ can still be consistent with observing more major crimes than minor crimes, as in our data, because of low $p(s)$ for the minor crimes.

27. The complementarity between B and β is crucial. Assume instead the discount factor were $B + \beta$. Then $\bar{B}_\beta = -1$, so $e(s, \beta) = -1/\bar{B}$, and $e_s = \bar{B}_s / \bar{B}^2 < 0$, since $\bar{B}_s = -V_B / V_s < 0$. So $e_s < 0$, like the case of outside option heterogeneity.

We then have a contrast between the two models. Whether PM2.5 elasticities are increasing in severity or not hinges on which form of population heterogeneity is more important.

5.5 Estimating air pollution effects on major and minor crimes

Guided by our theory, we construct aggregates of crimes of distinct severity and estimate the impact of pollution on these. We classify crimes whose degree is coded as "not large severity" (translated from Russian), to be minor crimes. Other degree codes reflecting greater severity we classify as major crimes. To illustrate the cutoff between the categories, we discuss maximum arrest terms for the most common major crimes and the most common minor crimes. The two most common minor crimes are petty theft, which is punishable by arrest of up to 20 days for an initial offense or 50 days for a repeat offense, and fraud classified as "not large severity", which is punishable by arrest of up to two years. The two most common major crimes are theft, which is punishable by arrest up to three years, or fraud of greater severity, which is punishable by arrest of up to four years. In Table 1, we see the major crime rate mean is 82% of the total crime rate mean, while the minor crime rate mean is 17% of the total crime rate mean. The two add up to less than 100% as there is a small number of crimes with unspecified severity.

We are additionally interested in comparing the results for major crimes and minor crimes for only property crimes and for only theft and fraud. Looking at major crimes and minor crimes for all crime types, we are more likely to be comparing across types of crimes, e.g., murder vs. theft, rather than different magnitudes of the same kind of crime, e.g., major fraud vs. minor fraud. The theory we have constructed could compare any two crimes, but the one dimension of crime severity we have modelled may fit better when considering distinct severities of crimes of a similar type.

We estimate specifications for both IV and the reduced-form to assess the distinct impacts of pollution on these crime categories. The use of reduced-form analysis to assess impact of pollution on crime types follows Herrnstadt et al. (2021).

Table 6a presents our IV results for the effect of log(PM2.5) on each crime rate. Importantly, we estimate statistically significant and positive elasticities of air pollution for all major crime types and minor crime types. Noticeably, the point estimates for pollution elasticities of minor crime rates are larger then the elasticities for major crime rates, though the minor crime rate elasticities are less precisely estimated. When considering all crimes, the 95% confidence interval for the elasticity of minor crimes is [0.19, 1.18] and for major crimes it is [0.12, 0.54].

We confirm that the differences between the coefficients in the paired major crime and minor crime columns are not statistically significant, based on linear IV estimation using as a dependent variable the difference between the normalized major and minor crime rates (we report results in Appendix Table A10a). Among all crimes, the 95% confidence interval for the difference between major and minor elasticities is [-0.86, 0.14]. For property crimes, the interval for the difference is [-1.00, 0.10], and for major theft and fraud the interval is [-0.84, 0.23].

We use the interval estimators to reflect on our theoretical analysis. The model of discount factor heterogeneity predicts a zero difference in the elasticities of major and minor crime rates, and this prediction of 0 lies within all the estimated intervals. Regarding the model of outside option heterogeneity, our estimates places some bounds on the extent to which the PM2.5 elasticities of crime rates could be increasing in severity.

Table 6b presents our results for reduced-form estimation. Again, we find statistically significant and positive effects of our instruments on all major crime rates and all minor crime rates. And again, the point estimates for coefficients (in absolute terms) are larger for the minor crimes than for the major crimes. We assess whether the differences of coefficients in column pairs are statistically significant, by estimating reduced-form models with the difference in the normalized major and minor crime rates as the dependent variable (we report results in Appendix Table A10b). We find that the difference in the inversion coefficients for major and minor property crimes is marginally statistically significant at the 5% level, though the other coefficient differences are not statistically significant. Largely, the reduced-form results corroborate our IV results.

Lastly, we remark that the contributions to the increase in the total crime rate from air pollution is still much larger for the major crimes, given the greater prevalence of major crimes in our data. We can estimate these contributions using our Table 6a estimates and the delta method. The contribution of major crimes to the total crime rate increase is 0.268 and the contribution of minor crimes is just 0.114, and we can conclude that this difference is statistically significant at the 10% level.[28] These results can be interpreted as arising through the larger scale of major crimes, given that we do not estimate larger elasticities of major crimes.

28. This conclusion is based on estimating an IV regression, which uses as a dependent variable the difference between major and minor crime rates when each is normalized by the total crime rate. We report the results for such a regression in Appendix table A11, and repeat it for property crimes and for major theft and fraud.

6 Conclusion

Our study makes several contributions to understanding the effects of air pollution on crime. By employing instrumental variables available to a mountain-adjacent city, we identify large effects of abnormal air pollution on crime. Through finding the effects of log(PM2.5) air pollution on property crimes and not violent crimes, we find evidence against the possibility that higher aggression is the exclusive channel through which air pollution affects crime. We extend theory of crime to assess whether higher discounting would have larger effects on elasticities on major crimes relative to minor crimes. We find empirical support for pollution increasing both major crime rates and minor crime rates.

One question left open by our research is why we observe pollution increasing crime at abnormal levels of pollution, while other studies of emerging markets thus far (Singh and Visaria 2021; Zarate-Barrera 2022) have found decreases in crime due to avoidance behavior. One possible explanation is that avoidance behavior has been more limited in Almaty than elsewhere. Some circumstantial evidence supports this possibility: the founders of the Anti Smog air pollution mask company in Kazakhstan highlight "lack of awareness regarding the pollution problem as a key challenge in their business,"[29] and Zlatev et al. (2021) confirm that real-time data on PM2.5 air pollution in Almaty was scarce until 2017. A rigorous exploration of why abnormal air pollution causes greater distortions in behavior in specific locations like Almaty remains an important question for future research.

A related question left open is why the literature to date, now including our paper, has found effects on property crime only in London and Almaty, while effects in the United States and Mexico City are concentrated in violent crime. Addressing this question seems important both in terms of understanding the mechanisms of how pollution affects criminal behavior and in distinct causes of criminal behavior across countries and cultures. Relatedly, we still lack direct evidence for the mechanisms that we emphasize as relevant.

Our study takes first steps toward exploring how air pollution's effects may vary by crime severity. Though we did not find strong empirical evidence in distinguishing between our two models concerning whether PM2.5 elasticities are increasing in crime severity, our approaches here seem promising for future work in achieving a finer understanding of air pollution's impact on crime. Additionally, our approach suggests how measurement of pollution's heterogenous effects could offer broader insight into a population's criminal behavior.

We hope that this study can help policymakers to better recognize that air pollution is not only a health problem but also a cause of broader economic harm. As Hanlon (2020)

29. Nazira Kozhanova, "Anti Smog air pollution mask company founders fund air quality research, raise awareness," *The Astana Times*, December 12, 2019, https://astanatimes.com/2019/12/anti-smog-air-pollution-mask-company-founders-fund-air-quality-research-raise-awareness/.

shows for the case of 1851-1911 industrial Britain, air pollution has severe negative long-run consequences for urban employment, while Fu et al. (2021) show for contemporary China that air pollution has a substantial negative short-run impact on manufacturing productivity. We complement such studies by illuminating one specific channel by which air pollution brings immediate harm to Kazakhstan's largest city of Almaty, while suggesting that air pollution more broadly disrupts sound decision-making. Our study adds to the mountain of evidence that air pollution mitigation is not a tradeoff between improving public health and economic growth, but a priority for both short-term and long-term economic prosperity.

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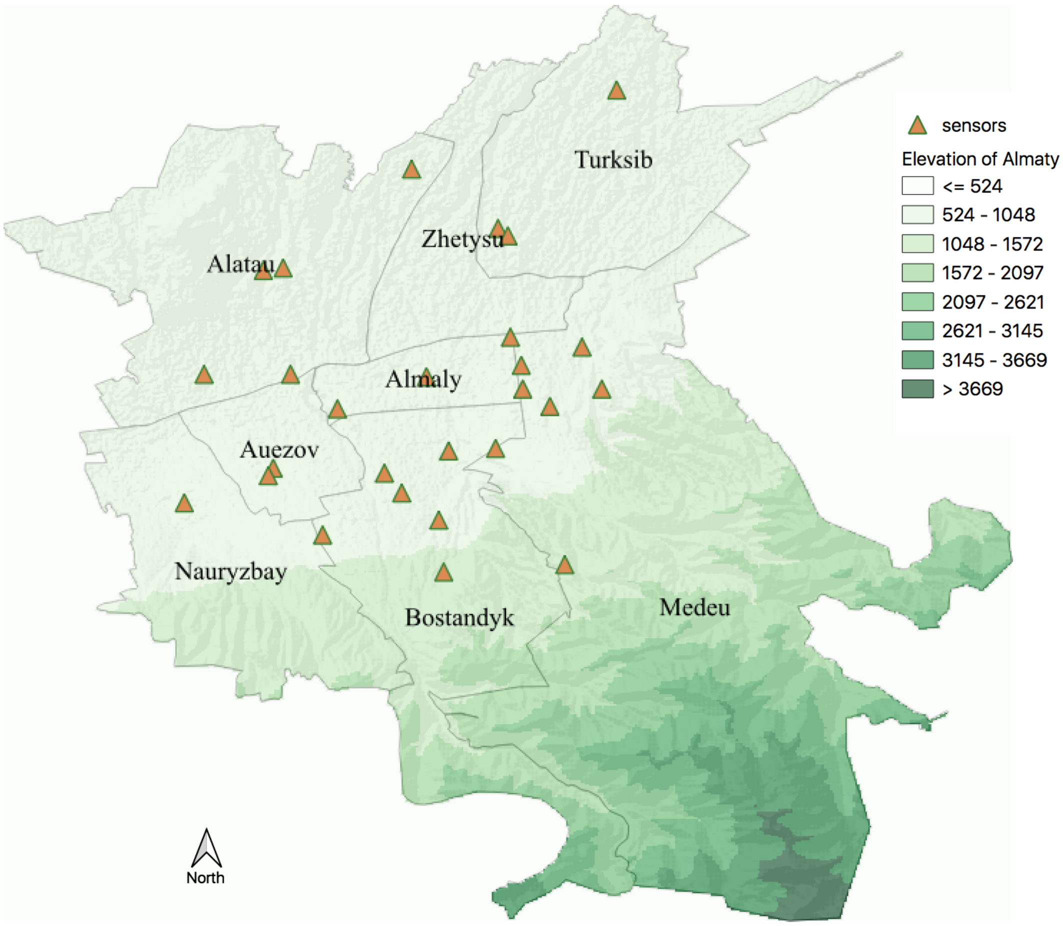

Figure 1: Districts and elevation of Almaty (in meters).

Notes: Produced with QGIS, using map data from Amey et al. (2021) and OCHA ROCCA (2019).

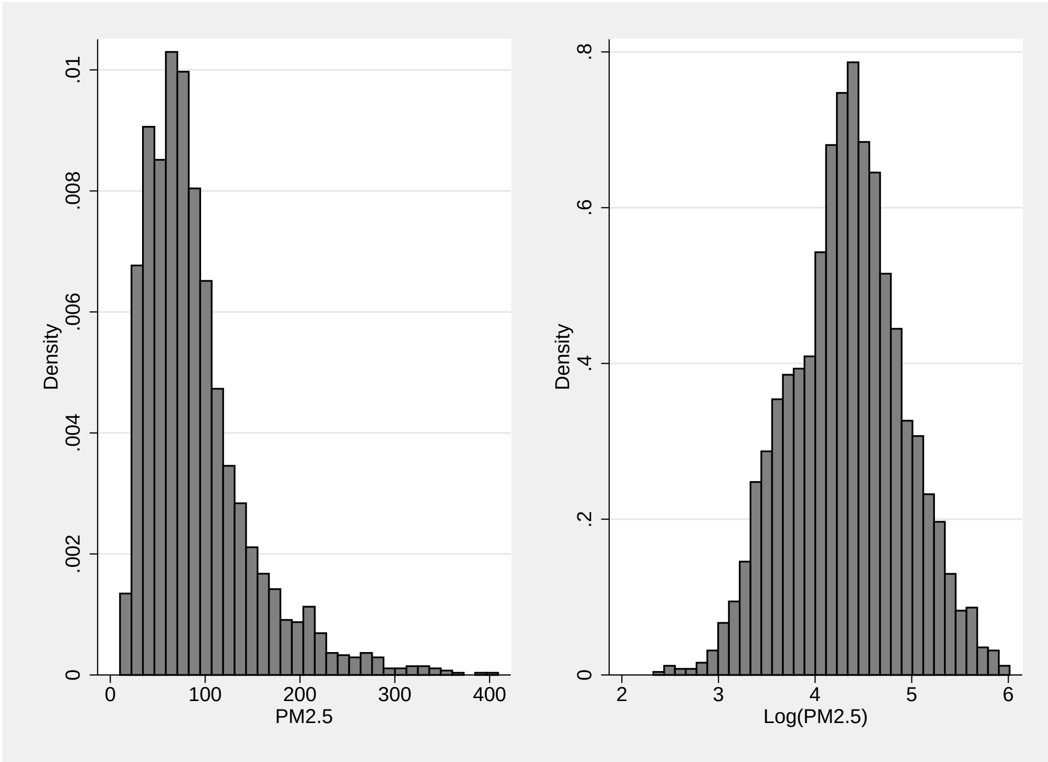

Figure 2: Histograms of daily winter pollution in Almaty's rayons

Notes: Each of the 2275 observations is pollution ($\mu g/m^3$ of PM2.5) averaged for one Almaty rayon over one day during the winters of Dec. 2017 - Mar. 2018, Dec. 2018 - Mar. 2019, and Dec. 2019 - Mar. 2020.

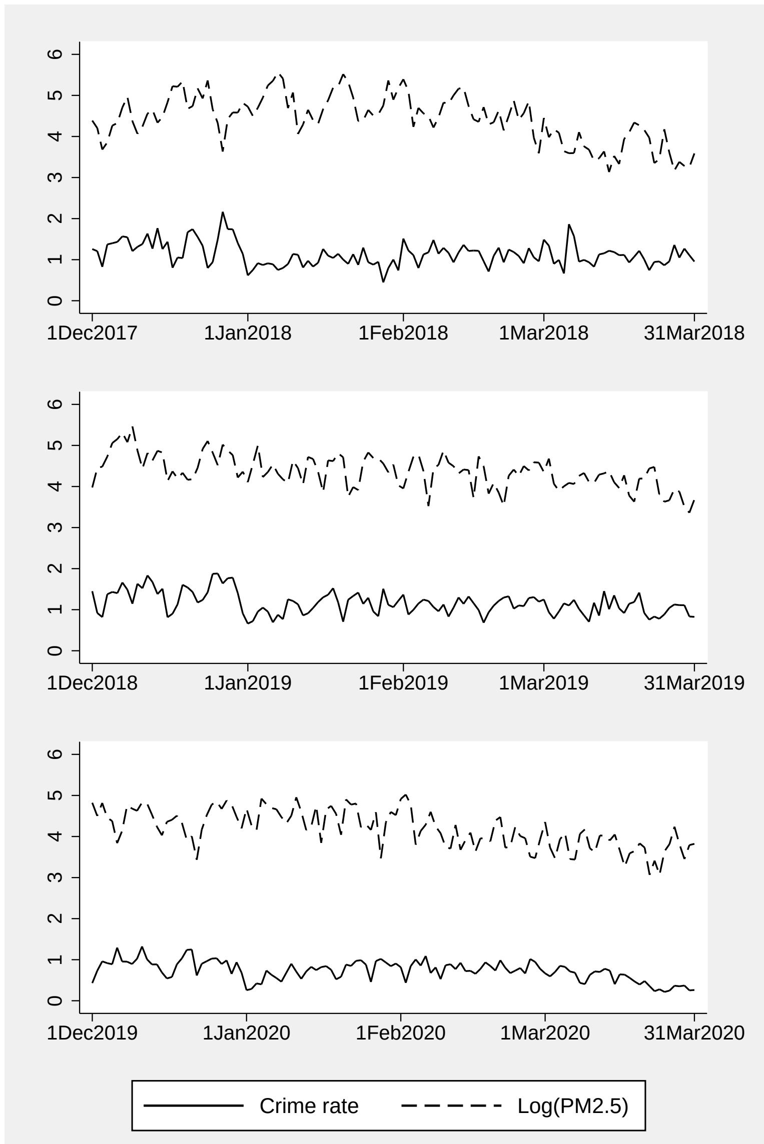

Figure 3: Almaty crime and pollution over three winters

Notes: For each day, we plot the observed crime rate and Log(PM2.5) pollution averaged over Almaty rayons. All crime rates have been normalized by dividing through by the average daily rayon crime rate over the 12 months observed.

Table 1: Summary statistics

	Daily data, N=2275				8-hour data, N=8736			
Variables	Mean	S.D.	Min.	Max.	Mean	S.D.	Min.	Max.
Crime rates								
Total	8.23	5.03	0.00	36.56	8.07	7.19	0.00	55.61
Major theft	5.68	3.73	0.00	27.34	5.55	5.30	0.00	41.71
Petty theft	0.20	0.42	0.00	5.83	0.23	0.76	0.00	12.83
Fraud	1.12	1.12	0.00	15.45	1.09	1.86	0.00	42.13
Robbery	0.25	0.36	0.00	2.78	0.25	0.63	0.00	7.36
Property crime	7.41	4.56	0.00	34.75	7.28	6.62	0.00	52.83
Violent crime	0.44	0.52	0.00	5.55	0.44	0.87	0.00	11.11
Major crime	6.74	4.22	0.00	30.58	6.62	6.04	0.00	45.88
Minor crime	1.41	1.34	0.00	14.55	1.34	2.03	0.00	39.36
Major property crime	6.40	4.03	0.00	29.66	6.29	5.79	0.00	45.88
Minor property crime	0.95	1.01	0.00	14.26	0.91	1.62	0.00	39.36
Major theft or fraud	6.07	3.87	0.00	27.34	5.96	5.61	0.00	44.94
Minor theft or fraud	0.87	0.91	0.00	6.66	0.83	1.50	0.00	14.98
Pollution measures								
PM2.5 ($\mu g/m^3$)	89.71	55.43	10.24	408.90				
Log(PM2.5)	4.33	0.58	2.33	6.01				
Air Quality Index	162.13	49.34	42.50	438.96				
Instrumental variables								
Mountain wind	0.06	0.08	0.00	0.33	0.05	0.11	0.00	0.81
Inversion	0.36	0.48	0.00	1.00	0.37	0.48	0.00	1.00
Control variables								
Temperature (C)	-1.66	7.13	-27.65	16.60	-1.72	7.50	-30.69	20.94
Humidity (%)	81.99	12.92	31.52	99.13	82.04	15.09	22.56	100.00
Wind velocity (m/s)	2.19	0.76	0.50	5.79	2.19	0.98	0.19	8.00
Precipitation (mm)	0.48	1.12	0.00	6.83	0.44	1.02	0.00	6.83
Atm. pressure (mmHg)	704.30	3.81	691.70	716.04	704.31	4.09	691.01	716.92

Notes: Data is collected over the following three winters: Dec. 2017 - Mar. 2018, Dec. 2018 - Mar. 2019, and Dec. 2019 - Mar. 2020. All crime rates are reported per 100K rayon population and per 24 hours. For the 8-hour sample, the crime rates are measured over 8 hours and converted to a per-24-hour rate. For the 8-hour period sample, only the inversions and precipitation are measured at a daily frequency. Weather control variables other than precipitation are averages over the relevant time unit (24 hours or 8 hours) of readings taken every half hour.

Table 2: Log(PM2.5) effect on total crime rate,
Linear fixed effects estimates

	(1)	(2)	(3)	(4)	(5)
Log(PM2.5)	0.123**	0.152***	0.136***	0.0725***	0.0843***
	(0.0478)	(0.0361)	(0.0340)	(0.0237)	(0.0280)
Rayon FE	yes	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes	yes
Controls	no	yes	yes	yes	yes
Day-of-Year FE	no	no	yes	yes	yes
Week-Year FE	no	no	no	yes	yes
Rayon-Day-of-Year FE	no	no	no	no	yes
R-Squared	0.502	0.521	0.603	0.696	0.793
Observations	2275	2275	2275	2275	2249

Notes: Estimation in this table presumes log(PM2.5) is exogenous. We report Driscoll-Kraay standard errors for all coefficients. The dependent variable is the total crime rate normalized by dividing by the mean total crime rate of the sample. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, and barometric pressure. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table 3: Log(PM2.5) effect on total crime rate,
Linear IV estimates, mountain wind & inversion IV

	(1)	(2)	(3)	(4)	(5)
Log(PM2.5)	0.171**	0.460**	0.354**	0.392***	0.422***
	(0.0738)	(0.196)	(0.169)	(0.106)	(0.146)
Rayon FE	yes	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes	yes
Controls	no	yes	yes	yes	yes
Day-of-Year FE	no	no	yes	yes	yes
Week-Year FE	no	no	no	yes	yes
Rayon-Day-of-Year FE	no	no	no	no	yes
1st Stage F-Stat	74.18	23.69	23.59	30.22	15.36
J-stat p-value	0.289	0.650	0.797	0.851	0.164
Observations	2275	2275	2275	2275	2249

Notes: We report Driscoll-Kraay standard errors for all coefficients. The dependent variable is the total crime rate normalized by dividing by the mean total crime rate of the sample. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, and barometric pressure. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table 4: Reduced-form effects of IVs on total crime rate,
Linear fixed effects estimates

	(1)	(2)	(3)	(4)	(5)
Mountain Wind	-0.296***	-0.239**	-0.322***	-0.278***	-0.204**
	(0.109)	(0.107)	(0.0929)	(0.0788)	(0.101)
Inversion	0.102***	0.115***	0.0889***	0.0753***	0.0755***
	(0.0340)	(0.0379)	(0.0342)	(0.0199)	(0.0239)
Rayon FE	yes	yes	yes	yes	yes
Day-8h-of-Week FE	yes	yes	yes	yes	yes
Controls	no	yes	yes	yes	yes
Day-8h-of-Year FE	no	no	yes	yes	yes
Week-Year FE	no	no	no	yes	yes
Rayon-Day-8h-of-Year FE	no	no	no	no	yes
R-Squared	0.458	0.467	0.525	0.571	0.736
Observations	8736	8736	8736	8736	8712

Notes: The time dimension is 8-hour periods: 0-8h, 8h-16h, and 16h-24h for each day of the sample. We report Driscoll-Kraay standard errors for all coefficients. The dependent variable is the total crime rate normalized by dividing by the mean total crime rate of the sample. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, and barometric pressure. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table 5: Log(PM2.5) effect on 4 common crime types and 2 aggregates, IV estimates, mountain wind & inversion IVs

	(1) Theft (Crime)	(2) Petty (Crime)	(3) Fraud (Crime)	(4) Robbery (Crime)	(5) Property (Crimes)	(6) Violent (Crimes)
Log(PM2.5)	0.322***	0.663	0.467*	0.253	0.389***	-0.0178
	(0.117)	(0.456)	(0.258)	(0.325)	(0.108)	(0.237)
Rayon FE	yes	yes	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
Day-of-Year FE	yes	yes	yes	yes	yes	yes
Week-Year FE	yes	yes	yes	yes	yes	yes
J-stat p-value	0.838	0.702	0.269	0.887	0.913	0.366
Observations	2275	2275	2275	2275	2275	2275

Notes: The estimated log(PM2.5) coefficients can be interpreted as the PM2.5 elasticities of the crime rate for the crime type, evaluated at the mean of that crime rate. We report Driscoll-Kraay standard errors for all coefficients. The first-stage F-statistic for all columns is 30.22. Each dependent variable is a crime type or aggregate that has been normalized by dividing by the mean total crime rate of the sample. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, and barometric pressure. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table 6a: IV estimates
of Log(PM2.5) effect on major crimes and minor crimes,
mountain wind and inversion IVs

	(1) Major (All) Crimes	(2) Minor (All) Crimes	(3) Major Property Crimes	(4) Minor Property Crimes	(5) Major Theft/ Fraud	(6) Minor Theft/ Fraud
Log(PM2.5)	0.327***	0.686***	0.321***	0.773***	0.308***	0.613**
	(0.107)	(0.253)	(0.110)	(0.278)	(0.112)	(0.268)
Rayon FE	yes	yes	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
Day-of-Year FE	yes	yes	yes	yes	yes	yes
Week-Year FE	yes	yes	yes	yes	yes	yes
J-stat p-value	0.950	0.807	0.965	0.852	0.958	0.974
Observations	2275	2275	2275	2275	2275	2275

Notes: The estimated log(PM2.5) coefficients can be interpreted as the PM2.5 elasticities of the crime rate for the crime type, evaluated at the mean of that crime rate. We report Driscoll-Kraay standard errors for all coefficients. The first-stage F-statistic for all columns is 30.22. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, and barometric pressure. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table 6b: Reduced-form linear FE estimates
of IV effects on major crimes and minor crimes

	(1) Major (All) Crimes	(2) Minor (All) Crimes	(3) Major Property Crimes	(4) Minor Property Crimes	(5) Major Theft/ Fraud	(6) Minor Theft/ Fraud
Mountain Wind	-0.255***	-0.396***	-0.238***	-0.453**	-0.244***	-0.459**
	(0.0850)	(0.148)	(0.0877)	(0.181)	(0.0909)	(0.183)
Inversion	0.0705***	0.111***	0.0690***	0.168***	0.0685***	0.138***
	(0.0212)	(0.0423)	(0.0224)	(0.0503)	(0.0230)	(0.0491)
Rayon FE	yes	yes	yes	yes	yes	yes
Day-8h-of-Week FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
Day-8h-of-Year FE	yes	yes	yes	yes	yes	yes
Week-Year FE	yes	yes	yes	yes	yes	yes
R-Squared	0.526	0.341	0.514	0.281	0.506	0.294
Observations	8736	8736	8736	8736	8736	8736

Notes: The time dimension is 8-hour periods: 0-8h, 8h-16h, and 16h-24h for each day of the sample. We report Driscoll-Kraay standard errors for all coefficients. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, and barometric pressure. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

A Appendix: robustness and placebo tests

A.1 PPML estimation

Estimating PPML models offers additional validity to our main results that used linear models. Though we have relatively few zeroes in our daily data (5 of 2275 observations), 8.5% of observations are zero for our 8-hour sample. PPML is better described functionally as an exponential model of the expected conditional mean, which is then guaranteed to be non-negative. Poisson fixed effects regression is fully robust in a sense that the estimator requires only the assumption that the conditional mean is specified correctly, and neither the Poisson error assumption, nor conditional independence is necessary for consistency.[30] PPML is also special among non-linear models in avoiding the incidental parameters problem and is computationally simple.[31] PPML is thus well-suited among nonlinear approaches for fixed effects estimation.

We first estimate the following equation with log(PM2.5) presumed to be exogenous.

$$Crimerate_{it} = exp\Big[log(PM2.5)_{it}\beta + X_t\xi^{(0)} + T_t\tau^{(0)} + \alpha_i^{(0)}\Big] + \varepsilon_{it}^{0},$$

where we abuse some redundancy in notation between on our main text and this appendix. For a PPML model, the expected mean of the dependent variable conditional on the explanatory variables is equal to the exponential term above, hence PPML often being referred to as an exponential mean model. We add ε_{it}^{0} as a mean-zero error term outside the exponentiation to reflect deviations between the dependent variable and the exponential mean.

We estimate standard errors that are two-way clustered to allow for correlation across rayons within any date, and to allow for correlation within rayons over time within months. As with our linear models, we cannot simply two-way cluster by rayon and date (as e.g. Bondy et al. 2020, do for PPML estimation), because 7 rayons is too few clusters to satisfy the necessary asymptotics for clustering. Ideally, there would be an extension to PPML of the approach Driscoll and Kraay (1998) that addresses both serial and panel correlation with a small panel dimension, but we are not aware of theory or computation that has done so. As an alternative, we cluster by rayon-month, which allows for 84 clusters and addresses serial correlation within months.

The baseline PPML model results are in Table A3a. Results should be comparable to

30. Random effects or alternative nonlinear approaches such as negative binomial lack these robustness properties (Wooldridge 1999).

31. One dimension of fixed effects based on a small sample can be partialed out without relying on asymptotics (e.g., Cameron and Trivedi 2013) while for other dimensions of fixed effects we can rely on large-T asymptotics for consistency (e.g., Fernández-Val and Weidner 2018). Our results apply the PPML fixed effects implementation of Correia et al. (2020) and report McFadden's pseudo R-squared.

our linear fixed effects results where log(PM2.5) is presumed to be exogenous (in Table 2). Coefficients in the PPML estimation can be interpreted as the PM2.5 elasticity of the expected mean crime rate for any observation, whereas the coefficients in our linear estimation are the PM2.5 elasticity of crime rate evaluated at the sample mean crime rate. Comparing the two sets of results, we see an identical pattern of coefficients and statistical significance. Estimated elasticities and standard errors are especially similar for columns 1, 4, and 5. The similarity of coefficients and standard errors between the linear and PPML approaches offers some confidence that our results are not being distorted by the distinct approaches to serial correlation.

The second PPML model that we estimate applies a control function approach to address the potential endogeneity of log(PM2.5), following Lin and Wooldridge (2019). With this approach, we estimate the same first stage as in our paper's baseline IV model. The second-stage PPML model for crime rate under the control function approach then includes the estimated first-stage residuals $\hat{u}_{it}$ in order to control for variation in air pollution unrelated to the instruments.

$$Crimerate_{it} = exp\Big[log(PM2.5)_{it}\gamma + X_t\xi^{(2)} + T_t\tau^{(2)} + \alpha_i^{(2)} + \hat{u}_{it}\rho\Big] + e_{it}.$$

A large econometrics literature supports the control function approach of including first-stage residuals in the second stage for consistent estimation, while an alternative approach of substituting in the fitted value of the endogenous explanatory variable into the second stage typically leads to inconsistent estimates for nonlinear models (e.g., Cameron and Trivedi 2013) and specifically models with an exponential mean (Terza et al. 2008). An additional advantage of the control function approach is that the hypothesis $H_0 : \rho = 0$ offers a test for idiosyncratic exogeneity, one that is fully robust to distributional misspecification and arbitrary serial dependence (Lin and Wooldridge 2019). To estimate standard errors, we bootstrap the two-stage estimation to address the first-stage estimation error, as suggested by Lin and Wooldridge (2019). Our bootstrapping clusters by day and estimates standard errors based on 500 full resamplings of size equal to the number of day clusters.[32]

The baseline PPML control function results are in Table A3b. Results should be comparable to our baseline IV results (in Table 3). As in our previous comparison, we see the same pattern in the ranking of point estimate magnitudes across the specifications. Standard errors are somewhat larger for columns 4 and 5 than in our IV estimates. We also report in

32. We acknowledge two alternative approaches for inference for the PPML control function approach that could be worthwhile, beyond the scope of this appendix. One approach we suggest is to calculate the standard errors of the two-stage estimation analytically, building on the approach of Papke and Wooldridge (1996), and this would make bootstrapping unnecessary. A second approach would be to implement two-way cluster bootstrapping, an area where theory is developing rapidly (e.g., Menzel 2021).

this table the effective F-statistic of Montiel Olea and Pflueger (2013) for the first stages as additional confirmation of our instrument strength.[33]

The third PPML approach that we consider is the reduced-form estimation of the instruments' effects on the total crime rate. The reduced-form is especially worth considering with PPML since there are zeroes for 8.5% of the observations using the 8-hour data, so we may expect to see more evidence of misspecification from the linear model. The estimating equation is as follows:

$$Crimerate_{it} = exp\Big[Z_{it}\omega + X_t\xi^{(3)} + T_t\tau^{(3)} + \alpha_i^{(3)}\Big] + \upsilon_{it},$$

Standard errors are clustered by date and by rayon-month, like our earlier PPML fixed effects estimations. Comparing results between the reduced-form PPML estimation A3c and our linear reduced-form estimation (Table 4), results again are quite similar. The coefficients here between PPML and linear models are somewhat distinct in interpretation, e.g., the column 4 estimate of 0.0626 for the inversion coefficient implies that the expected mean crime rate is 6.46% (i.e. $exp(0.0626) - 1$) higher when there is an inversion for any parameters, whereas the column 4 inversion coefficient of 0.0753 for the linear model implies that the expected mean crime rate increases by 7.53% of the sample mean crime rate for any observation.

To summarize, the results of our three PPML appendix tables broadly align with our main results. The PPML results do not reveal any obvious shortcomings that dissuade us from focusing on linear fixed effects models and IV for our main results.

A.2 Alternative PM2.5 specifications

Table A4a provides results for IV using the level of PM2.5 as the endogenous explanatory variable measuring pollution, rather than log(PM2.5), Though we have justified our use of the log transformation through the skewness of PM2.5, the robustness check is worth considering given that much of the pollution-crime literature to date has used the level rather than the log transformation. We normalize PM2.5 by dividing by its sample mean, so the coefficients can be interpreted as the PM2.5 elasticity of the total crime rate when evaluated at the mean PM2.5 and mean crime rate. Compared to our results for log(PM2.5) and IV (Table 3), the estimated elasticities using PM2.5 are lower and more precisely estimated, and the first stage F-statistics are larger. For our specifications with week-year fixed effects (columns 4 and 5), 0.08 remains outside of the 95% confidence interval. Table A4b provides a corresponding

33. The reason that we report the F-statistics of Kleibergen and Paap (2006) for our IV results rather than the effective F of Montiel Olea and Pflueger (2013) is that the implementation of Pflueger and Wang (2015) does not extend to Driscoll-Kraay standard errors. But we can see that the F-statistics are similar between the approaches by comparing Table 3 and Table A3b.

check for the PPML control function approach, and likewise, coefficients are lower than in log(PM2.5) estimation (Table A3b.)

We also consider more flexible non-linear models of how PM2.5 affects crime, but with our data we were unable to identify distinct effects of PM2.5 at different levels, and we do not report these results any further.

A.3 Other alternative model specifications

For remaining tables in this appendix, we estimate using as a baseline of controls with rayon, day-of-week, day-of-year, and week-year fixed effects (column 4 in our usual progression of specifications), and we consider deviations from this baseline.

Table A5 confirms the effect of pollution on crime is robust to alternative specifications of the instrumental variables. The first column considers in place of our baseline wind instrument a dummy variable when any mountain wind is observed, rather than the share of observations within the time period exhibiting mountain wind. The second column considers in place of our baseline wind instrument two equal-sized bins ranging from above 0 to the midpoint of the variable's range. This specification checks that our results are not overly sensitive to a few high-leverage values of the mountain wind instrument. The third column "above average wind" considers only values of wind above the variable's sample average as treatment, and robustness to this specification suggests our main results are not driven by small variations in the wind instrument with below-average mountain winds. These three columns all provide similar results as the baseline estimate (column 4 of Table 3).

The fourth column of Table A5, already discussed in the main text, uses a flexible wind instrument specification similar to one common in the air pollution literature (e.g., Deryugina et al. 2019; Bondy et al. 2020). We estimate 8 wind instrumental variables reflecting the share of 4 different wind directions affecting the northern rayons and 4 wind directions affecting the southern rayons. Compared to the baseline estimate, the point estimate here is lower though still statistically significant at the 5% level. The first stage F-statistic is lower when considering the expanded set of instruments, and the p-value of Hansen's J-statistic is 0.03, which is evidence against the validity of the overidentifying restrictions with the expanded set of instruments.

The fifth column of Table A5 considers as an alternative inversion instrument the average temperature difference between the high-elevation and low-elevation Almaty weather stations, which is an alternative approach in the literature (e.g., Bondy et al. 2020). Compared to our baseline, the first stage F-statistic is higher, and the coefficient is lower. The 95% confidence interval for the PM2.5 elasticity of total crime is [0.054, 0.295] which suggests more modest effects of pollution's effect on crime. The higher first stage F-statistic may at first glance suggest that the temperature difference is a superior instrument, but there is a tradeoff. Our preferred inversion instrument aligns more closely with the inversion characteristics we would ideally measure—because our instrument indicator considers only inversions that last for every temperature observation over the day, there is a greater chance that pollution is truly trapped, so our measure is capturing strong inversions. Though the temperature difference captures some information on inversion strength that relates to pollution, the temperature difference could also capture irrelevant temperature variation when there is no inversion present, and this irrelevant information could lead to more substantial violations of the exclusion restriction, for our context. We do see that the J-statistic of 0.383 is much lower than J-statistic of our baseline model.[34] So overall, we do not conclude that the more modest point estimates with temperature difference instrument must be more reliable than estimates with our inversion instrument in our context. The results do suggest some sensitivity in the magnitude of our results based on the identification strategy, as we noted when we summarized the appendix results in the main text.

Table A6 confirms our preferred specification is robust to several alternative controls (or lack thereof) that are natural to consider for sensitivity. Column 1 replaces each of our control variables with indicators covering 10 equal sized bins. Column 2 goes to the opposite extreme of covariate modeling and uses the temperature level as a control rather than our baseline specification of 5°C temperature bins. Column 3 considers month-year fixed effects rather than week-year fixed effects. Column 4 considers no controls. Column 5 adds more controls common elsewhere in the literature (Bondy et al. 2020) such as cloud cover, dewpoint, humidity-squared, and a temperature-humidity interaction. Column 6 adds a lead and lag of log(PM2.5) pollution as controls. Most of these checks result in slightly smaller point estimates than our baseline specification. The largest reduction in the point estimate is for the specification with no controls. But as we discuss earlier in the paper, controls may be necessary for the exclusion restriction to hold, since some short-term weather could have a relationship to our instruments and also affect the crime rate. Omitting controls can conceivably then downward bias the effects. This possibility seems more plausible than the controls introducing a positive collider bias, because our choice of controls follows existing literature, and we select our controls carefully. Regardless, the log(PM2.5) coefficient for the specification with no controls is still statistically significant at the 10% level.

We confirm in Table A7 that our results are robust to alternative aggregations and subsamples, which serve as an important check on our methods. Column 1 collapses our

34. Moreover, when we estimate a specification (not included in the appendix tables) with the temperature difference as an instrument and adding rayon-day-of-week effects, we find a J-statistic of 0.025.

rayon-level data into two regions, the southern rayons close to the mountains and the northern rayons far from the mountains. This collapses the number of observations to 728 and a lower point estimate. The coefficient remains statistically significant at the 1% level. Column 2 drops all observations in March of 2018, 2019, or 2020. There are multiple motives for this robustness check. One is the question of whether we would get distinct results if we had chosen our sample to be meteorological winter and considered just January, February, and December. A second motive is that we do observe some common downward trend of pollution and crime in March 2020 from Figure 3 and that raises a question of whether the onset of the COVID-19 pandemic affects results. Kazakhstan was among the last nations to report a COVID case, the country did not have strict enforcement of lockdowns within city borders, and we expect week-year fixed effects to capture important unobservables related to the pandemic. But we see that results are still relatively stable after dropping over 500 observations from March. Our third column drops Nauryzbay rayon. Our sensor map in Figure 1 shows that Nauryzbay has the worst coverage, and it is also the smallest rayon by population. We see that dropping Nauryzbay from our sample has negligible impact on our results.

The remaining three columns of Table A7 are robustness checks for the reduced-form estimation (so the relevant baseline specification is column 4 of Table 4). Column 4 estimates the reduced-form at the daily level of aggregation rather than the 8-hour aggregation. Compared to the baseline, coefficient estimates are similar, though there is some loss of precision in the mountain wind estimate as a result of the aggregation, but the coefficient is still statistically significant at the 10% level. Column 5 estimates the reduced-form using an alternative definition of inversions that is defined as maintaining an inversion over an 8-hour period rather than daily. This results in losing about 1000 observations because of some missing evening data from the high-elevation weather station during the winter of 2017-18. We see though that the coefficient estimate for mountain wind is still relatively stable. The inversion coefficient falls, as would be expected since this alternative IV has a weaker standard in what constitutes an inversion. The inversion coefficient remains statistically significant at the 5% level. Column 6 is motivated by the potential concern that the pattern of missing pollution observations in our daily data is nonrandom and affecting our results. To assess this possibility, we include a dummy for the missing pollution observations in our reduced-form estimation and also interact it with the instruments. We can see that relative to the baseline reduced-form estimation, our mountain wind and inversion coefficients are entirely stable to including this dummy variable, and we see that each individual coefficient involving the dummy variable is not statistically significant. Jointly testing that all three coefficients involving the missing pollution dummy are zero, we fail to reject the null with a p-value of 0.4845. So we interpret the column 6 results as providing evidence that missing pollution data is not substantially impacting results in our daily data.

In Table A8, we consider the possibility that air pollution could have persistent effects that we should be modeling. Per Herrnstadt et al. (2021) and citations within, estimated effects from air pollution tend to be short-lived. However, our study may be more likely to encounter lagged effects because of the relatively short 8-hour frequency of our reduced-form estimation. Because we know our instrumental variables affect air pollution, a simple approach to explore persistent effects is to add lagged instruments to our reduced-form estimation. We see from the results in Table A8 that including 1, 2, or 3 days of lags of instruments does not affect our estimates for the contemporaneous instruments, and there is no statistically significant effect nor any particular pattern in the total effects of the lagged instruments.

A.4 Placebo tests for air pollution on irrelevant dates

As a final check on our results for the total crime rate, we consider placebo specifications where we estimate the effects of air pollution on irrelevant dates for current crime, similar to approaches of Ebenstein et al. (2016) and Bondy et al. (2020). Precisely-estimated null results here should confirm that our finding of an effect of current air pollution on current crime is not the result of a flaw in our estimation strategy.

To implement the placebo estimations for our IV approach, we must be careful to properly instrument for the irrelevant-date air pollution. To do so, we include as controls (in both first stage and second stage) the matrix of contemporaneous controls and the matrix of controls from the irrelevant date. We use the irrelevant-date IVs in the first stage estimation and exclude them from the second stage. Contemporaneous IVs are not related to irrelevant-date air pollution, so they are not part of the estimation. With this approach, we expect the contemporaneous controls to have no effect on the first-stage estimation, and we expect the irrelevant-date controls to have no effect on the second-stage estimation. We then have valid IV estimates for the effect of the irrelevant-date pollution on the current crime rate, and we expect this effect to be zero and precisely estimated, provided that the irrelevant-date instruments are sufficient strong for the irrelevant-date pollution.

All of the placebo tests in Table A9 fail to reject the null hypothesis of no effect from the irrelevant-date pollution on the current crime rate. We first discuss the irrelevant dates of +/- 7 days and +/- 31 days (i.e., 1 week and 1 month). We pick these periods also to confirm that our results are not some artifact of the periodicity of our data. The first stage F-statistics for our first-stages are still large, given that we instrument for irrelevant-date pollution using corresponding irrelevant-date instruments and controls. These four coefficients are estimated about as precisely as our main IV results, so the statistical insignificance of these estimates for 1 week or 1 month is not a consequence of weak instruments or imprecision. Thus, these placebo specifications confirm that our main findings are not an artifact of our estimation strategy.

We also attempt placebo specifications of +/- 183 days (i.e. 6 months). In other words, we use summer pollution instrumented by summer instruments to predict winter crime. What this specification reveals, through the low F-statistics, is that our summer instruments are weakly related to summer pollution. In fact, our inversion measure, which equals 1 for over 35% of observations in winter, is always 0 for the shifted summer months.[35] The remaining instrument mountain wind is predictably weak because, as we discussed in Section 2 there is less dirty air for the mountain wind to clean up in the summer. The placebo tests find no statistically significant effect of pollution on crime, though the coefficients are imprecisely estimated due to the weak instruments. The weakness of the summer instruments back up the rationale for our focus on winter months in Section 3.

A.5 Models of major minus minor crime

We assess whether the elasticities we have estimated for major and minor crimes (in Tables 6a and 6b) are statistically distinct from one another within categories. We use as dependent variables the differences between major and minor crime rates, where each rate has been normalized by dividing by its own mean. We consider this difference, first, within all crimes, second, within property crimes, and third, within theft and fraud. We estimate IV models in Table A10a and reduced-form models in Table A10b, using our usual controls and our preferred set of fixed effects. We discuss results in Section 5.5 of the main text.

Lastly, we assess whether there is a statistical difference in the contributions of major and minor crimes to the increase in the total crime rate. To do so, we run IV regressions using as the dependent variable the difference in major and minor crime rates, now normalized by dividing though by the total crime rate. We can then interpret the coefficients of this regression as differences in the contributions of major and minor crimes to the total crime rate. We find that for each of the three crime subsamples considered, major crimes have the larger contribution to total crime. The larger contribution is statistically significant at the 10% level when considering the difference within all crimes or within property crimes, and the difference is statistically significant at the 5% level within major theft and fraud.

35. We estimate additional placebo specifications (not reported in the tables) using an alternative inversion measure with more variation in summer months—the temperature difference that we consider in column 5 of A5. Even then, the first stage F-statistics each round to 4 so the IV remain weak.

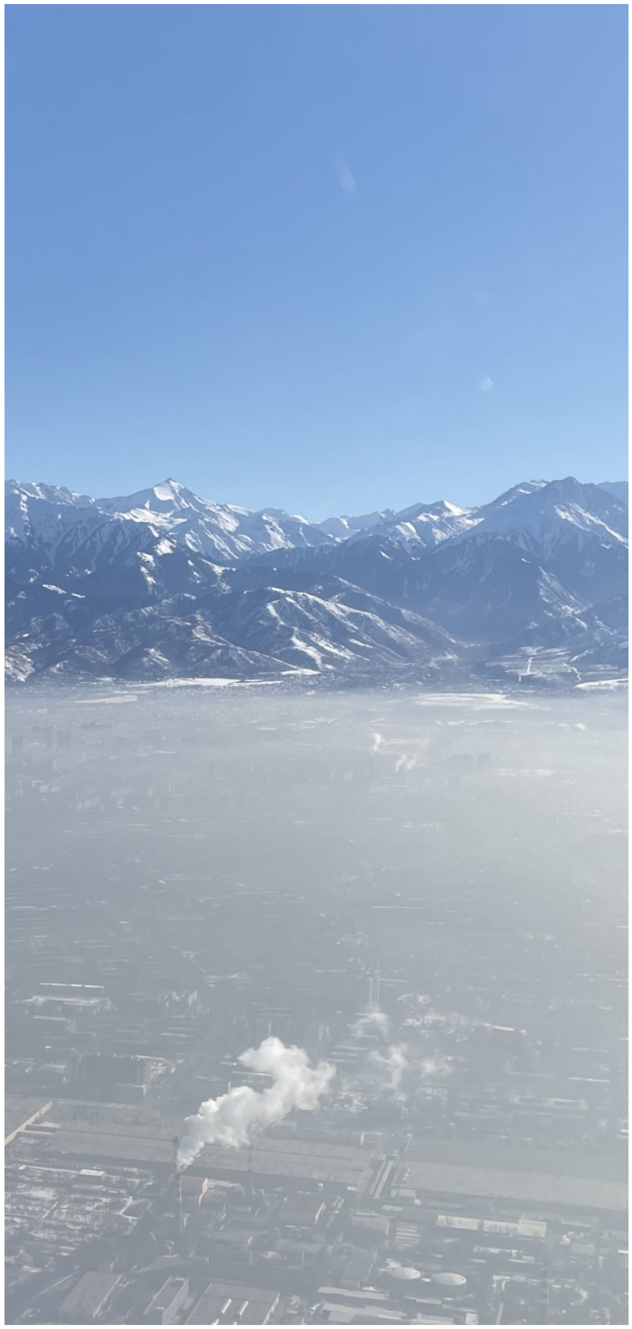

Figure A1: Temperature inversion in Almaty (our photo).

Table A1: Summary Statistics of Air Pollution

	(1) PM2.5	(2) AQI
Our paper (Almaty, Kazakhstan)	89.7 (55.4)	162.1* (49.3)
Jones (2022) (30 counties, USA)	8.39 (6.40)	
Burkhardt et al. (2019) (397 counties, USA)	9.96 (5.7)	
Zárate-Barrera (2022) (Mexico City, Mexico)	22.49 (10.06)	90.51 (34.03)
Herrnstadt et al. (2021) (Chicago, USA)	27.7** (14.4)	
Bondy, Roth, Sager (2020) (London, UK)	28.05** (10.35)	30.06 (9.18)
Singh and Visaria (2021) (Bihar, India)	150.36 (96.04)	

Notes: The table shows the mean and standard deviation of PM2.5 (in $\mu g/m^3$) and AQI from comparable studies. We include published economics studies of the pollution-crime relationship and two working papers focused on the pollution-crime relationship in emerging markets. The * indicates our own estimates based solely on PM2.5. The ** designates measurements of PM10, not PM2.5.

Table A2a: First stage results
Linear fixed effects estimates for instruments' effect on log(PM2.5)

	(1)	(2)	(3)	(4)	(5)
Mountain wind	-0.628**	-0.511**	-0.785***	-0.740***	-0.454*
	(0.290)	(0.205)	(0.204)	(0.181)	(0.252)
Inversion	0.610***	0.224***	0.224***	0.225***	0.221***
	(0.0555)	(0.0357)	(0.0373)	(0.0354)	(0.0428)
Humidity (%)		-0.00136	-0.00544***	-0.00395*	-0.00377
		(0.00199)	(0.00185)	(0.00205)	(0.00250)
Wind velocity (m/s)		-0.236***	-0.257***	-0.223***	-0.221***
		(0.0211)	(0.0237)	(0.0230)	(0.0271)
Total precipitation (mm)		-0.0891***	-0.0921***	-0.0719***	-0.0694***
		(0.0146)	(0.0166)	(0.0145)	(0.0175)
Atmospheric pressure (mmHg)		-0.0103**	-0.00235	-0.00391	-0.00452
		(0.00516)	(0.00557)	(0.00517)	(0.00633)
Rayon FE	yes	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes	yes
Controls	no	yes	yes	yes	yes
Day-of-Year FE	no	no	yes	yes	yes
Week-Year FE	no	no	no	yes	yes
Rayon-Day-of-Year FE	no	no	no	no	yes
1st Stage F-Stat	74.18	23.69	23.59	30.22	15.36
R-squared	0.386	0.642	0.702	0.739	0.799
Observations	2275	2275	2275	2275	2249

Notes: The instrumental variables excluded in the second stage are bolded. Controls also include temperature bins (of width 5 degrees C). We report Driscoll-Kraay standard errors for all coefficients. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A2b: Expanded results with control parameters,
Log(PM2.5) effect on total crime rate
Linear IV estimates, mountain wind & inversion IV

	(1)	(2)	(3)	(4)	(5)
Log(PM2.5)	0.171**	0.460**	0.354**	0.392***	0.422***
	(0.0738)	(0.196)	(0.169)	(0.106)	(0.146)
Humidity (%)		0.00476*	0.00644**	0.000504	0.000699
		(0.00259)	(0.00263)	(0.00136)	(0.00172)
Wind velocity (m/s)		0.0934	0.0974	0.103***	0.109***
		(0.0579)	(0.0601)	(0.0287)	(0.0363)
Total precipitation (mm)		0.0659***	0.0627***	0.0576***	0.0626***
		(0.0251)	(0.0224)	(0.0149)	(0.0197)
Atmospheric pressure (mmHg)		0.00799	-0.00184	0.00870**	0.00772
		(0.00636)	(0.00539)	(0.00411)	(0.00544)
Rayon FE	yes	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes	yes
Controls	no	yes	yes	yes	yes
Day-of-Year FE	no	no	yes	yes	yes
Week-Year FE	no	no	no	yes	yes
Rayon-Day-of-Year FE	no	no	no	no	yes
1st Stage F-Stat	74.18	23.69	23.59	30.22	15.36
J-stat p-value	0.289	0.650	0.797	0.851	0.164
Observations	2275	2275	2275	2275	2249

Notes: We report Driscoll-Kraay standard errors for all coefficients. The dependent variable is the total crime rate normalized by dividing by the mean total crime rate of the sample. Controls also include temperature bins (of width 5 degrees C) The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A2c: Log(PM2.5) effect on total crime rate
Limited information maximum likelihood (LIML) estimates,
Mountain wind & inversion IV

	(1)	(2)	(3)	(4)	(5)
Log(PM2.5)	0.172**	0.461**	0.354**	0.393***	0.430***
	(0.0741)	(0.197)	(0.169)	(0.106)	(0.150)
Rayon FE	yes	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes	yes
Controls	no	yes	yes	yes	yes
Day-of-Year FE	no	no	yes	yes	yes
Week-Year FE	no	no	no	yes	yes
Rayon-Day-of-Year FE	no	no	no	no	yes
1st Stage F-Stat	74.18	23.69	23.59	30.22	15.36
J-stat p-value	0.289	0.650	0.797	0.851	0.165
Observations	2275	2275	2275	2275	2249

Notes: We report Driscoll-Kraay standard errors for all coefficients. The dependent variable is the total crime rate normalized by dividing by the mean total crime rate of the sample. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, and barometric pressure. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A3a: PPML fixed effects estimates,
Log(PM2.5) effect on total crime rate

	(1)	(2)	(3)	(4)	(5)
Log(PM2.5)	0.117**	0.277***	0.192***	0.0761***	0.0949***
	(0.0502)	(0.0517)	(0.0445)	(0.0224)	(0.0247)
Rayon FE	yes	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes	yes
Controls	no	yes	yes	yes	yes
Day-of-Year FE	no	no	yes	yes	yes
Week-Year FE	no	no	no	yes	yes
Rayon-Day-of-Year FE	no	no	no	no	yes
Pseudo R-Squared	0.0706	0.0769	0.0922	0.113	0.125
Observations	6825	6825	6825	6825	6825

Notes: Standard errors are two-way clustered to allow for correlation in errors across rayons for any date and across time for any rayon for each month-year. The dependent variable is the total crime rate normalized by dividing by the mean total crime rate of the sample. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, and barometric pressure. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A3b: PPML control function approach,
Mountain wind & inversion IVs, log(PM2.5) effect on total crime rate

	(1)	(2)	(3)	(4)	(5)
Log(PM2.5)	0.188**	0.414**	0.324*	0.334**	0.365*
	(0.0867)	(0.178)	(0.171)	(0.131)	(0.212)
Residuals	-0.116	-0.290	-0.220	-0.318**	-0.339
	(0.0921)	(0.183)	(0.178)	(0.136)	(0.218)
Rayon FE	yes	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes	yes
Controls	no	yes	yes	yes	yes
Day-of-Year FE	no	no	yes	yes	yes
Week-Year FE	no	no	no	yes	yes
Rayon-Day-of-Year FE	no	no	no	no	yes
First Stage F-Statistic	24.85	25.96	30.51	27.63	14.18
Observations	2275	2275	2275	2275	2249
Pseudo R-Squared	0.0797	0.0816	0.0949	0.113	0.127

Notes: Standard errors are derived by bootstrapping two-stage estimation using 500 replications of resampled day clusters. We report the Montiel-Pflueger effective F-statistic.

Table A3c: PPML fixed effects estimates,
Reduced form effects of IVs on total crime rate

	(1)	(2)	(3)	(4)	(5)
Mountain Wind	-0.217*	-0.146	-0.211**	-0.180**	-0.203***
	(0.111)	(0.105)	(0.0871)	(0.0734)	(0.0719)
Inversion	0.102**	0.120***	0.0896***	0.0626***	0.0626***
	(0.0423)	(0.0403)	(0.0336)	(0.0179)	(0.0180)
Rayon FE	yes	yes	yes	yes	yes
Day-8h-of-Week FE	yes	yes	yes	yes	yes
Controls	no	yes	yes	yes	yes
Day-8h-of-Year FE	no	no	yes	yes	yes
Week-Year FE	no	no	no	yes	yes
Rayon-Day-8h-of-Year FE	no	no	no	no	yes
Pseudo R-Squared	0.144	0.146	0.163	0.180	0.214
Observations	8736	8736	8736	8736	8631

Notes: Standard errors are two-way clustered to allow for correlation in errors across rayons for any date and across time for any rayon for each month-year. The dependent variable is the total crime rate normalized by dividing by the mean total crime rate of the sample.

Table A4a: PM2.5 effect on total crime rate
Linear IV estimates, mountain wind & inversion IV

	(1)	(2)	(3)	(4)	(5)
PM2.5	0.160**	0.336**	0.241**	0.301***	0.287***
	(0.0714)	(0.146)	(0.123)	(0.0809)	(0.109)
Rayon FE	yes	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes	yes
Controls	no	yes	yes	yes	yes
Day-of-Year FE	no	no	yes	yes	yes
Week-Year FE	no	no	no	yes	yes
Rayon-Day-of-Year FE	no	no	no	no	yes
1st Stage F-Stat	60.22	25.79	27.23	28.81	18.55
J-stat p-value	0.216	0.972	0.488	0.723	0.0480
Observations	2275	2275	2275	2275	2249

Notes: The daily PM2.5 has been normalized by dividing by the average daily PM2.5 in the sample. The dependent variable is the total crime rate normalized by dividing by the mean total crime rate of the sample. We report Driscoll-Kraay standard errors for all coefficients.

Table A4b: PM2.5 effect on total crime rate,
PPML control function estimation, mountain wind & inversion IVs

	(1)	(2)	(3)	(4)	(5)
PM2.5	0.160**	0.314**	0.228*	0.266**	0.261*
	(0.0754)	(0.131)	(0.129)	(0.110)	(0.157)
Residuals	-0.118	-0.252*	-0.176	-0.254**	-0.234
	(0.0789)	(0.133)	(0.135)	(0.115)	(0.163)
Rayon FE	yes	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes	yes
Controls	no	yes	yes	yes	yes
Day-of-Year FE	no	no	yes	yes	yes
Week-Year FE	no	no	no	yes	yes
Rayon-Day-of-Year FE	no	no	no	no	yes
First Stage F-Statistic	24.85	25.96	30.51	27.63	14.18
Observations	2275	2275	2275	2275	2249
Pseudo R-Squared	0.0795	0.0812	0.0946	0.113	0.127

Notes: Standard errors are derived by bootstrapping two-stage estimation using 500 replications of resampled day clusters. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, and barometric pressure. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A5: Robustness to alternative IV specifications,
Linear IV estimates,
Log(PM2.5) effect on total crime rate

	(1) Wind Dummy	(2) Binned Wind	(3) Above Average Wind	(4) Estimate All Wind Directions	(5) Temperature Difference
Log(PM2.5)	0.408***	0.449***	0.402***	0.194**	0.175***
	(0.131)	(0.115)	(0.109)	(0.0814)	(0.0612)
Rayon FE	yes	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes
Day-of-Year FE	yes	yes	yes	yes	yes
Week-Year FE	yes	yes	yes	yes	yes
First Stage F-Statistic	20.40	22.77	29.05	19.64	46.02
J-stat p-value	0.989	0.765	0.944	0.0308	0.383
N	2275	2275	2275	2275	2275

Notes: Column headings summarize deviations from the standard wind and inversion IVs, using the same controls and fixed effects (those of Table 3 Column 4). Column 1 replaces our baseline Mountain Wind IV with a dummy variable equal to 1 when the baseline Mountain Wind IV is positive. Column 2 replaces the Mountain Wind IV with two equal-sized mountain bin indicator IVs. Column 3 uses as mountain wind instrument only the share of mountain winds in excess of the sample average mountain wind. Column 4 estimates an IV specification with inversions and eight wind instruments (4 wind direction instruments for northern rayons, 4 wind direction instruments for southern rayons, and no wind is the omitted category). Column 5 uses as an inversion measure the temperature difference between the daily averages for the high-elevation and low-elevation weather station. We report Driscoll-Kraay standard errors for all coefficients. The dependent variable is the total crime rate normalized by dividing by the mean total crime rate of the sample. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, and barometric pressure. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A6: Robustness to alternative control variables,
Linear IV estimates,
Log(PM2.5) effect on total crime rate

	(1) 10-Bin All Covariates	(2) Unbin Temp.	(3) Month-Year FEs	(4) No Controls	(5) More Controls	(6) 1 Lead/Lag Log(PM2.5)
Log(PM2.5)	0.362***	0.387***	0.319***	0.0954*	0.400***	0.422***
	(0.115)	(0.103)	(0.0930)	(0.0516)	(0.105)	(0.154)
Rayon FE	yes	yes	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	no	yes	yes
Day-of-Year FE	yes	yes	yes	yes	yes	yes
Week-Year FE	yes	yes	no	yes	yes	yes
First Stage F-Statistic	29.82	28.77	24.06	70.46	30.53	18.38
J-stat p-value	0.359	0.590	0.866	0.180	0.634	0.877
N	2275	2275	2275	2275	2275	2208

Notes: This table considers specific alterations to controls or fixed effects from Table 3, Column 4. Here Column 1 controls using 10 equal-sized bins for all covariates, including temperature. Column 2 uses continuous temperature rather than 5-degree Celsius bins. Column 3 uses month-year FEs in lieu of week-year FEs. Column 4 uses no controls. Column 5 adds additional controls: cloud cover, dewpoint, squared humidity, and a humidity-temperature interaction. Column 6 adds as controls 1 lead and 1 lag of Log(PM2.5) as controls, i.e. pollution for the previous and subsequent 8-hour periods. We report Driscoll-Kraay standard errors for all coefficients. The dependent variable is the total crime rate normalized by dividing by the mean total crime rate of the sample. Baseline Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, and barometric pressure. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A7: Aggregation and subsample checks,
Effects on total crime rate

	(1) Aggregate Rayons IV	(2) Drop March IV	(3) Drop rayon Nauryzbay IV	(4) R.-form Daily agg. Linear FE	(5) R.-form 8-hour Inversion	(6) R.-form Missing PM2.5
Log(PM2.5)	0.312***	0.369***	0.401***			
	(0.0933)	(0.129)	(0.107)			
Mountain wind				-0.274*	-0.245***	-0.286***
				(0.152)	(0.0776)	(0.0820)
Inversion				0.0863***	0.0392**	0.0800***
				(0.0212)	(0.0187)	(0.0219)
Missing(PM2.5)						0.0297
						(0.0277)
Missing*M.wind						0.150
						(0.169)
Missing*Inv.						-0.0213
						(0.0318)
Rayon FE	yes	yes	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
Day-of-Year FE	yes	yes	yes	yes	yes	yes
Week-Year FE	yes	yes	yes	yes	yes	yes
First Stage F-Statistic	29.12	19.30	29.85			
J-stat p-value	0.738	0.612	0.585			
R-Squared				0.674	0.565	0.571
Observations	728	1694	2012	2912	7720	8736

Notes: Columns headings summarize specifications where variables are aggregated or subsetted, using similar controls and similar fixed effects as Table 3 Column 4. Here Column 1 collapses data by northern and southern rayons, so we report IV estimates over 2 regions rather than 7 districts. Column 2 reports IV estimates without using any data from March 2018, March 2019, or March 2020, so only months following the standard metereological definition of winter from December-February remain. Column 3 drops from our sample Nauryzbay Rayon, the smallest district by population in our sample. Column 4 estimates the reduced-form fixed effects specification aggregating to daily data. Column 5 estimates the reduced-form fixed effects specification with the inversion measure defined at an 8-hour frequency rather than a daily frequency. Column 6 adds to the reduced-form 8h-period estimation a dummy for missing PM2.5 observations in the daily data and also interacts the dummy with the IVs. Column 5 and 6 is designed to be compared to Column 4 of Table 4 and includes the same fixed effects with 8h rather than daily frequency We report Driscoll-Kraay standard errors for all coefficients. The dependent variable is the total crime rate normalized by dividing by the mean total crime rate of the sample. Baseline Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, and barometric pressure. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A8: Reduced-form effects of IVs,
including effects of Lagged IVs on total crime rate,
Linear FE estimates

	(1) 1 Day of Lagged IVs	(2) 2 Days of Lagged IVs	(3) 3 Days of Lagged IVs
Mountain Wind	-0.230***	-0.226***	-0.223***
	(0.0794)	(0.0784)	(0.0809)
Inversion	0.0683***	0.0699***	0.0633***
	(0.0201)	(0.0211)	(0.0210)
Wind Sum of Lags	0.00340	0.0401	0.206
	(0.146)	(0.214)	(0.281)
Inversion Sum of Lags	0.0109	-0.00127	-0.0244
	(0.023)	(0.030)	(0.038)
Rayon FE	yes	yes	yes
Day-8h-of-Week FE	yes	yes	yes
Controls	yes	yes	yes
Day-8h-of-Year FE	yes	yes	yes
Week-Year FE	yes	yes	yes
R-Squared	0.573	0.575	0.576
Observations	8664	8592	8520

Notes: The time dimension is 8-hour periods: 0-8h, 8h-16h, and 16h-24h for each day of the sample. We report Driscoll-Kraay standard errors for all coefficients. Each dependent variable is a crime type or aggregate that has been normalized by dividing by the mean total crime rate of the sample. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, and barometric pressure. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A9: Placebo tests for irrelevant log(PM2.5) effect on crime rate,
Linear IV estimates,
Mountain Wind and Temperature Differences as IVs

	(1) -183 days	(2) -31 days	(3) -7 days	(4) +7 days	(5) +31 days	(6) +183 days
Log(PM2.5)	-0.417	0.0163	0.0260	0.122	0.0595	0.136
	(0.763)	(0.109)	(0.121)	(0.0957)	(0.113)	(0.579)
Rayon FE	yes	yes	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
Day-of-Year FE	yes	yes	yes	yes	yes	yes
Week-Year FE	yes	yes	yes	yes	yes	yes
First Stage F-Statistic	1.911	35.15	23.09	28.69	26.23	1.604
Observations	2012	2184	2236	2229	2193	2068

Notes: Placebo specifications use irreleveant log(PM2.5) shifted by the amount in the column heading, and instruments from the shifted period, and covariates from both the current period and the shifted period. We report Driscoll-Kraay standard errors for all coefficients. The dependent variable is the total crime rate normalized by dividing by the mean total crime rate of the sample. Baseline Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, and barometric pressure. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A10a: IV estimates reflecting the difference in PM2.5 elasticities of major crime rates and minor crime rates, Mountain wind and inversion IVs

	(1) Major-Minor All Crimes	(2) Major-Minor Property Crimes	(3) Major-Minor Theft/ Fraud
Log(PM2.5)	-0.360	-0.453	-0.305
	(0.252)	(0.279)	(0.272)
Rayon FE	yes	yes	yes
Day-of-Week FE	yes	yes	yes
Controls	yes	yes	yes
Day-of-Year FE	yes	yes	yes
Week-Year FE	yes	yes	yes
J-stat p-value	0.822	0.829	0.992
Observations	2275	2275	2275

Notes: Each dependent variable here is a difference between dependent variables from column pairs of Table 6a. The coefficients here can be interpreted as differences in the PM2.5 elasticities of major vs. minor crime rates. We report Driscoll-Kraay standard errors for all coefficients. The first-stage F-statistic for all columns is 30.22. Each dependent variable is a crime type or aggregate that has been normalized by dividing by the mean total crime rate of the sample. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, and barometric pressure. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A10b: Reduced-form estimates of effects of IVs on difference of major minus minor normalized crime rates, Linear FE estimates

	(1) Major-Minor All Crimes	(2) Major-Minor Property Crimes	(3) Major-Minor Theft/ Fraud
Mountain Wind	0.141	0.215	0.215
	(0.156)	(0.191)	(0.196)
Inversion	-0.0403	-0.0992**	-0.0691
	(0.0435)	(0.0500)	(0.0488)
Rayon FE	yes	yes	yes
Day-8h-of-Week FE	yes	yes	yes
Controls	yes	yes	yes
Day-8h-of-Year FE	yes	yes	yes
Week-Year FE	yes	yes	yes
R-Squared	0.123	0.114	0.120
Observations	8736	8736	8736

Notes: Each dependent variable here is a difference between dependent variables from column pairs of Table 6b. The time dimension is 8-hour periods: 0-8h, 8h-16h, and 16h-24h for each day of the sample. We report Driscoll-Kraay standard errors for all coefficients. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, and barometric pressure. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A11: Linear IV estimates reflecting difference in contributions to total crime rate of major and minor crimes, Mountain wind and inversion IVs

	(1) Major-Minor All Crimes	(2) Major-Minor Property Crimes	(3) Major-Minor Theft/ Fraud
Log(PM2.5)	0.154*	0.163*	0.164**
	(0.0885)	(0.0852)	(0.0825)
Rayon FE	yes	yes	yes
Day-of-Week FE	yes	yes	yes
Controls	yes	yes	yes
Day-of-Year FE	yes	yes	yes
Week-Year FE	yes	yes	yes
J-stat p-value	0.968	0.909	0.965
Observations	2275	2275	2275

Notes: Each dependent variable here is a difference between major and minor crime rates, normalized by the mean total crime rate. The coefficients here can be interpreted as differences in the contribution to the total crime rate of major vs. minor crimes for the subsample specified in the column heading. We report Driscoll-Kraay standard errors for all coefficients. The first-stage F-statistic for all columns is 30.22. Each dependent variable has been normalized by dividing by the mean total crime rate.