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

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May 29, 2022

Abstract

We find that PM2.5 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 a PPML control function approach, we estimate a PM2.5 elasticity of the expected crime rate more than four times as large as studies of cleaner cities. Among crime types, robbery and high-stakes property crime are most affected by PM2.5. These results support theory that air pollution induces higher discounting as well as aggressive behavior.

JEL classification: Q53, K42

Keywords: Air Pollution; PM2.5; Crime

*We thank Gani Aldashev, Alisher Aldashev, James Anton, Leslie Marx, David McAdams, Ryan McDevitt, Thierry Post and Jozef Konings for feedback, as well as all seminar participants at the Duke Fuqua School of Business, Nazarbayev University Graduate School of Business, and the Applied Economics Workshop of the International School of Economics. We especially thank Amina Abilkassym, Kamola Ismoilova, and Rustam Merekenov for sharing data and insights. Funding from the Nazarbayev University Social Policy Grant is gratefully acknowledged. Any errors are the responsibility of the authors.

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1 Introduction

The adverse effects of air pollution on human health are now widely known, but an alarming recent finding is the negative short-term effects of air pollution on human behavior (see, for a recent survey, Aguilar-Gomez et al. 2022). One manifestation of these negative consequences is air pollution increasing crime. Much of what we know about air pollution increasing crime comes exclusively from high-income countries, such as in the United States (U.S.) and the United Kingdom (U.K.), where the effects are evident even at low levels of pollution.

Observed effects of air pollution on crime at even healthy levels of air pollution raise the question of how air pollution impacts human behavior in urban areas with abnormally high levels of air pollution, but addressing this topic presents several challenges. We cannot extrapolate from studies of cleaner cities whether the worst marginal effects of air pollution are concentrated at low levels or whether harmful marginal effects extend through the range of air pollution common to dirtier cities of developing economies. Credible measurement of these effects requires sufficiently disaggregated information for air pollution and crime, and such data is often less readily available outside of high-income countries. Further obscuring the harmful effects of air pollution at the highest levels is the possibility that individuals may engage in pollution avoidance behavior. Lastly, disentangling the effects of air pollution on crime requires a reliable identification strategy, given the web of common causes for both air pollution and crime.

The city of Almaty, the largest in Kazakhstan, has unique features that help us overcome the challenges in identifying the effects of abnormal air pollution on crime. 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 fine particulate matter of diameter 2.5 microns or less (PM2.5). Almaty's mountains are a blessing and a curse for the city's air: winds blowing from the mountains are a source of clean air, but mountains also function as a barrier that traps 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. We suggest that 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 over 8-hour periods from both crime reports and PM2.5 sensors during Almaty's winter and exploit the advantages of Almaty's mountains for our identification.

Our study is the first to assess that air pollution increases criminal activity in a city where air pollution regularly exceeds international safety standards. We estimate an elasticity of the expected crime rate with respect to PM2.5 air pollution equal to 0.38, which is more than 4 times as large as elasticity estimates from studies of other air pollutants in cleaner cities like Chicago and London. Our finding contrasts with evidence that air pollution can reduce crime due to avoidance behavior in a high-pollution setting such as India (Singh and Visaria 2021). But for the range of air pollution and the population that we observe in Almaty, our identification strategy allows us to estimate that PM2.5 air pollution increases crime, and this finding suggests that high levels of PM2.5 cause substantial disruption to human behavior.

The second contribution of our study is its unique findings that air pollution affects robbery and high-stakes property crime—two results crucial for understanding the mechanisms through which air pollution distorts behavior. Our findings contrast with three U.S. studies (Jones 2022; Herrnstadt et al. 2021; Burkhardt et al. 2019) who 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, but our results suggest that the absence of property crime effects is specific to the U.S. context. We anticipate that Almaty is a strong candidate among global cities to measure effects on property crimes given the prevalence of Almaty's property crime enforcement and its rapid decline in homicides over the last decade. Our results support the relevance of the alternative mechanism, proposed by Bondy et al. (2020), that air pollution causes higher discounting in intertemporal choices. Our discussion of potential mechanisms considers an expanded interpretation of higher discounting, newly informed by literature on criminal perceptions from criminology and criminal deterrence from economics. We expect to find the largest effects of air pollution on robbery and high-stakes property crimes, and our data allows us to distinguish high-stakes property crime from petty crime. By finding effects specifically for high-stakes property crime and robbery, we provide the strongest empirical support to date for the mechanism that air pollution causes higher discounting.

The third contribution of our study is methodological through applying a pseudo-Poisson maximum likelihood (PPML) control function approach to study the effects of air pollution on crime. For our setting with a non-negative count-related dependent variable (the crime rate) and an endogenous explanatory variable (air pollution measured by PM2.5), recent work by Lin and Wooldridge (2019) endorses using the PPML control function approach that we apply. A broader literature further supports that for non-linear estimation, the control function approach of including residuals in the second-stage yields consistent estimates, unlike alternatives (Terza et al. 2008; Cameron and Trivedi 2013). For our results, the PPML control function approach leads to wider standard errors than two-stage least squares (TSLS) estimation, which does not take into account the count-derived features of our data. Other authors relating air pollution and crime apply PPML fixed effects models but not the PPML control function approach.[1] So, our use of the PPML control function approach meaningfully extends applications of the econometric toolbox.

Our paper proceeds as follows. Section 2 surveys the potential mechanisms through which pollution could affect crime and discusses how results on crime types could be informative about the relevant mechanisms. Section 3 provides background for our setting of Almaty, Kazakhstan, which we argue is a highly-polluted city in winter with a substantial property-crime problem. Section 4 discusses our data sources for crime, air pollution, and weather controls—all of which we aggregate across Almaty regions and 8-hour intervals over the winter of 2019-20. Section 5 details our empirical strategy, which ultimately focuses on the PPML control function approach, using mountain winds and temperature inversions as IV. Section 6 presents our results for air pollution's effect on the total crime rate, and we provide thorough arguments that our finding is robust. Section 7 discusses our results for individual crime types and crime aggregates, highlighted by our finding that air pollution affects robbery and high-stakes property crime, and Section 8 concludes.

2 Mechanisms for air pollution affecting crime

For this section's discussion of mechanisms through which air pollution could affect crime, we focus first on mechanisms that operate through potential perpetrators' preferences. We discuss first the hypotheses that have been the initial focus of the empirical literature relating air pollution to crime: aggressive behavior and higher discounting. We bring into the discussion the vast literature on criminal deterrence from economics and criminal perceptions from criminology. This body of work suggests a broader interpretation of higher discounting than the one proposed by prior air pollution literature.

The mechanism with the best supporting 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), and a "taste for violence" can be integrated into economic models of criminals' preferences (Baysan et al. 2019).

1. Specifically, Jones (2022) and the appendix of Herrnstadt et al. (2021) apply PPML only in the context of measuring reduced-form effects of weather variables on crime without using them to instrument for air pollution. Bondy et al. (2020) apply an IV approach distinct from the control function approach.

A distinct mechanism that can increase both 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.

Applying modern literature on crime deterrence and criminal perceptions, we suggest a broader interpretation of *higher discounting* than an increase in the discount rate within a standard intertemporal choice model. A leading crime deterrence survey by Chalfin and McCrary (2017) emphasizes that the key consensus of the previous two decades of empirical literature is that greater severity of sanctions causes limited deterrence of crime, while tactics like higher surveillance that increases chances of being caught do deter crime. Informed by the empirical evidence, recent economic theory of crime goes beyond Becker (1968) to consider heterogeneous time preferences among criminals and hyperbolic discounting of criminals. Chalfin and McCrary (2017) also direct economists to consider insights from criminologists on criminal perceptions. A recent criminology survey by Apel (2022) discusses experimental evidence 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. To align with modern theory and evidence, we propose that higher discounting caused by air pollution could take the form of disregarding consequences within the context of any of these richer forms of intertemporal preferences. We continue using the phrase *higher discounting* as its literal meaning is suitable to encompass broad forms of discounting. Our work cannot empirically distinguish among the various types of discounting, but considering all relevant microfoundations for discounting is essential for a full understanding of how air pollution distorts decisions.

To explore whether higher discounting is a relevant mechanism, we assess whether air pollution increases the highest-stakes property crimes in a location with strong enforcement against property crimes. Since the crime literature suggests discounting the immediate consequences of being caught is most important, higher discounting would be a larger shock to higher-stakes crimes where enforcement is stronger. Provided there is not a relatively large density of lower-stakes crimes vs. higher-stakes crimes on the margins, such discounting would have a larger effect on higher-stakes crimes. Assessing effects on higher-stakes property crimes rather than violent crimes is a priority for isolating the mechanism of higher discounting, because effects on violent crimes could be a consequence of aggression rather than higher discounting. Though we know from Bondy et al. (2020) that air pollution can increase property crime and that higher discounting is a plausible mechanism, the authors suggest no particular pattern in their results for which property crimes are affected by air pollution. Finding an effect of high-stakes property crimes and no effect on low-stakes property crimes would then provide strong evidence that aggression is not the exclusive mechanism for air pollution affecting crime. Alternatively, finding no effects of air pollution on high-stakes property crime would be strong support that aggression is the exclusive mechanism.

A location suitable for testing the hypothesized effect on high-stakes property crime must be chosen carefully, because null results could also result from large underreporting of property crime, and we argue in the next section that Almaty is relatively suitable. Though the U.S. is the most-studied location in the nascent air pollution and crime literature, there are several reasons to doubt that the U.S. is a suitable location to explore this hypothesis. To find an effect on high-stakes property crime, there must be a priority for enforcement of property crime, but over two-thirds of U.S. property crimes are not reported to the police (e.g. Dilulio 1996). The unique problem of household gun ownership in the United States creates a unique problem for violent crime (Duggan 2001) that U.S. policing must address, and gun interests have unique political power in the U.S. (Bouton et al. 2021). Racial bias in policing property crimes in the U.S. (e.g. Hoekstra and Sloan 2022) is an additional factor in muting the effects of air pollution on property crimes in the U.S.

An additional challenge for identifying ill effects of air pollution is the possibility that avoidance behavior mitigates the ill effects. Avoidance behavior from all economic actors involved in producing a crime report is plausibly important: pollution avoidance could reduce crime opportunities or limit crime enforcement. A recent working paper by Singh and Visaria (2021) studying India suggests the mechanism of lower crime reporting is relevant for property crimes in higher-pollution settings. Our focus though is on distortions in the criminal's behavior, and we seek to minimize other potential channels through the location we choose to study. The world's most-polluted cities in India and Pakistan may exhibit too much visible air pollution to be suitable. Similarly, a country like China with a strong culture of masking to combat air pollution (see, e.g., Liu et al. 2018) would be relatively unattractive to assess our main hypothesis of interest. We proceed to argue that Almaty, a city with circumstantial evidence of limited avoidance behavior, is suitable for exploring the effects of air pollution on property crime.

3 Background

The city of Almaty has a combination of useful characteristics for our study: a downward trend in homicide, a substantial property crime problem, high average winter air pollution, and large air pollution variation within winter. Centralized coal-powered heating in Almaty's winter contributes to the abnormal air pollution, though there is substantial variation in pollution due to weather patterns. Mountains just southeast of the city's center serve as both a clean-air source and a dirty-air barrier.

Before delving into details of Almaty's crime and air pollution, we confirm concisely that Almaty is an important national and regional urban economic center, with a standard of living on par with well-known eastern European cities. Almaty, with a population of about 2 million, is the largest city in Kazakhstan. The city was Kazakhstan's capital before 1998, and it remains an economic center. In global city comparisons, Almaty typically ranks in the bottom third and on par with cities like Istanbul and Belgrade.[2]

3.1 Crime in Almaty

Trends in crime reporting for both Almaty and Kazakhstan imply success in reducing the homicide rate, while broader crime remains a problem. In this section, we report crime rates annualized per 100,000 population, as is common practice. Kazakhstan's homicide rate was twice the global average for most years in between 1993-2004, but there was a steady decline in homicide from 11.4 in 2008 to 4.9 in 2015, well below the world average of 5.9; but as the homicide rate declined, there was a trend increase in reported crime, though the trend in convictions was relatively flat (UNODC 1993-2015). Almaty has shown similar trends as the nation as a whole; the murder rate fell from 12.7 in 2005 to 5.7 in 2015 (UNODC 2005-2015). Similarly, the data in our sample over the 2019-20 winter reflect a murder rate of 4.0 for Almaty. Crime overall remains a problem for Almaty, despite the fall in homicide. The bulk of the increase in Kazakhstan's reported crime is from Almaty and the new capital Astana (e.g. Zhumakanova 2018).

We cannot infer how much of the observed crime increase results from better reporting, but other evidence points to substantial problems of crime in Almaty despite the falling homicide rate. Like many other middle-income countries, Kazakhstan states ambitions to improve the rule of law and reduce corruption to attract foreign investment (e.g., OECD 2017), though low police officer salaries offer few incentives to solve crimes. In 2018, the city budget allocated 10.7 billion Kazakhstan tenge (KZT) equivalent to 28.8 million U.S. dollars

2. For example, in the EIU's 2015 liveability rankings, Almaty ranks 100th of 140 cities, just below Belgrade and well above Istanbul (EIU 2015).

(USD)[3] for the protection of public order and security in the city within the Department of Internal Affairs, but with a city police staff of 4,560 people, the average monthly police salary is at most 234 thousand tenge (624 USD), according to data from Tengrinews.kz (2018). Numbeo's crowdsourced data ranks Almaty 29th-highest in its crime index among 374 cities in 2020 (Numbeo 2020). To compare to U.S. cities, this rank places Almaty in between St. Louis and Milwaukee.

We find that in the winter of 2019-20, a large share of crime reports in Almaty are for property crimes. In our data, the five most common property crimes account for 93.8% of total crimes. If we include robbery as property crime rather than violent crime, as some studies do, the share increases to 96.4%. In contrast, the share of property crime out of total property and violent crime in the U.S. is 85% in 2019 (FBI 2019). The corresponding share for Chicago from 2001-12 is 88% based on the summary statistics of Herrnstadt et al. (2021), and this study classifies robbery as a property crime.

The headline crime statistics suggest that Almaty is a worthwhile setting to explore the effects of air pollution on property crime. The high ratio of property-to-violent crime reporting could to some extent reflect Almaty's underreporting of particular violent crimes.[4] Still, the high share of property crimes and the declining need to police homicide imply that Almaty offers some prospect of observing effects on property crime.

3.2 Air pollution in Almaty

Almaty is among the most polluted cities in the world based on air pollution from PM2.5, whose potential for harm is by now well-documented. For average PM2.5 pollution in the year 2021, the firm IQAir ranks Almaty 340th among 6475 cities globally (94th percentile). But this statistic masks through aggregation Almaty's clean summers and dirty winters. For the month of January 2021, Almaty's average PM2.5 pollution of $96.4\mu g/m^3$ ranks 77th in the world (98th percentile). A World Bank report confirms that this seasonal pattern in Almaty pollution carries back to 2017 (Zlatev et al. 2021, Figure 5).

We and many other researchers worldwide focus on PM2.5 air pollution given its high potential for harm not just outdoors, but also indoors, no matter the ventilation mode. Cyrys et al. (2004) find that more than 75 percent of daily indoor variation of PM2.5 is explained by its outdoor variation. Mounting evidence suggests that air pollution might impose significant economic costs in middle-income and low-income countries. For example,

3. We convert using the U.S. Treasury's 2018 average exchange rate of 375.15 KZT/USD.

4. Evidence for such underreporting comes from one major state and UN-sponsored survey, which reports that 17% of women in Kazakhstan experience domestic or sexual violence from an intimate partner in their lifetime (UNFPA 2017).

Barwick et al. (2021) estimate that a $10\mu g/m^3$ reduction in PM2.5 would decrease healthcare spending by approximately $9 billion in China, which roughly corresponds to 1.5% of annual healthcare spending. Fu et al. (2021) estimate that a 1% nationwide fall in PM2.5 in China would increase gross domestic product by 0.04% due to negative short-run effects of air pollution on productivity.

The abnormal levels of pollution in Almaty's winters are a result of the city's temperate climate and need for winter heating. The city's combined heat and power (CHP) plants, a legacy of the Soviet centralized heating system, still rely heavily on burning bituminous coal. There are three CHP plants around the region, and two of them are within or near city borders: the Almaty-2 CHP (510 MW) within Alatau district to the city's northwest, and the Almaty-1 CHP (145 MW) right outside of Turksib district to the city's northeast. Zlatev et al. (2021) reports that CHPs are responsible for 60% of heat production, and 70% of CHP production is coal-fueled. However dirty the CHP plants may be, small residential stoves and boilers are worse. Zlatev et al. (2021) adds that only one-third of Kazakhstan households have district heating, while two-third use smaller stoves and boilers that have low stacks and no filters, so their emissions tend to reach urban centers. 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.[5]

Circumstantial evidence suggests that Almaty's public engages in limited air pollution avoidance behavior. Because air pollution is so concentrated in winter, air pollution can be more easily ignored in Almaty compared to a city where pollution is year-round. The founders of the Anti Smog air pollution mask company highlight "lack of awareness regarding the pollution problem as a key challenge in their business."[6] One recent United Nations report observes, "Kazakhstan has no national policy on air protection, nor does it have specific air quality programmes" (UN 2019). In contrast to high-income countries like the U.S., where air pollution is easily available in daily forecasts and there are smog alert systems (such as the system studied by Zivin and Neidell 2009), there is no such public alert system in Almaty. Zlatev et al. (2021) confirm that real-time data on PM2.5 air pollution in Almaty was scarce until 2017. We acknowledge these points are at best suggestive evidence that air pollution avoidance behavior is limited in Almaty. But this evidence is enough to motivate

5. "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/.

6. 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/.

us to select Almaty for a rigorous study of the ill effects of air pollution on criminal behavior, given the prospect of limited avoidance behavior in the city.

Importantly for our empirical analysis, Almaty exhibits substantial variation in air pollution due to its location on the foothills of Northern Tian Shan mountain range (also known as Trans-Ili Alatau), closest to the southeast of the city. 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 5.3.

4 Data

Our study gathers data on air pollution, crime, and weather for Almaty over the winter of 2019-20. Though the setting of Almaty offers many advantages for our research approach, straightforward access to high-frequency data is not one of them. We focus our data collection efforts on one winter due to the abnormal pollution particular to Almaty winter that we detailed in the preceding section. Our pollution data originates from web-crawling our pollution source in real time, and our crime data originates from scraping the city crime-reporting map. Both tasks, especially the latter, are labor-intensive, and they were completed by students under our supervision. High-frequency weather data, in contrast, are easily downloadable.

A key decision in our data approach is the level of time aggregation. The first studies of air pollution and crime aggregate data to the daily level, though hourly data is available. The daily studies (e.g., Herrnstadt et al. 2021) argue that spillovers of effects across time are one reason to aggregate to the daily level. A second reason is potential gaps between crime occurrence and time reporting. However, our high-pollution setting also exhibits substantial patterns of variation in pollution within the day, motivating a finer level of aggregation than one day. Our solution to focus on aggregation to 8-hour time periods reflects a balance between these competing considerations.

A second important data aggregation decision is spatial. We aggregate crimes to 8 city divisions, known as rayons. The rayon is a unit we will use throughout our paper and analysis. Almaty's rayon borders are visible in Figure 1. Similar to the time aggregation decision, the spatial aggregation decision balances a tradeoff between finer geographic data and reducing the chance of spillovers across units. By aggregating our data sources to 8-hour intervals at the rayon level, we have a stable unit of analysis.

4.1 Data sources

Our data on air pollution comes from AirKaz.org (2019-20), an independent public 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 is the only source for PM2.5 data in Almaty. The AirKaz network reports real-time PM2.5 data on the website using PM2.5 sensors across Almaty.[7] Our students ran a web crawler for a time period spanning December 21, 2019 to March 31, 2020. They collected the PM2.5 measurements from all the sensors in Almaty and constructed the average eight-hour pollution measures for the 7 out of 8 rayons which contain sensors. Though there are lapses in collecting data from the sensors beyond our control, the final data set consists of 1702 observations of air pollution across the 7 rayons and 8-hour time periods. Though this is a smaller sample than similar air pollution studies, we find it to be large enough for reliable inference in our setting.

Our crime data comes from Qamqor (2019-20), 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, time of day, and location. Our students scraped the webpage and aggregated the crime data to rayons and 8-hour time periods to match our air pollution data. We report data for 4 violent crime types (murder, robbery, serious harm, and medium harm) and 5 property crime types (major theft, major fraud, petty theft/fraud, drugs and hooliganism). A useful feature of our data source is that it separates theft and fraud into major categories from a petty category. The maximum value of property for petty crime is just under 70 USD during our whole sample period.[8]

We use the online weather resource Reliable Prognosis, rp5.kz (Raspisaniye Pogodi Ltd. 2012-20) for wind direction, historical temperatures, and most of our time-varying weather controls.[9] 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

7. For the position of the sensors at the beginning of our study, see https://web.archive.org/web/20191221025903/https://airkaz.org/.

8. The cutoff for petty theft is indexed annually and rose from 25,250 tenge in 2019 to 26,510 tenge to 2020. This amounts to a small change from 66 USD to 69 USD, using the 2019 average KZT/USD exchange rates of 381.18 from the US Treasury.

9. We also determine daylight hours using data from Time and Date AS (2019-20).

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 city weather data. The site provides weather observations from the station for every half hour. We average weather data across 8-hour intervals to use as time-varying controls to match our crime and pollution data. To construct historical temperatures, we use the station's data from December 2012 to March 2019. Table 1 lists all the specific weather variables we consider as controls.

We use a second weather station in Almaty (Raspisaniye Pogodi Ltd. 2019-20) to obtain precipitation data and high-elevation temperature data. This station reports weather every 3 hours. To recognize temperature inversions, we compare temperatures from the two weather stations at the two different elevations. We discuss the wind data and temperature inversion data further when detailing our instrumental variables in section 5.3.

4.2 Descriptive Statistics

The first section of our summary statistics in Table 1 establish the key facts 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), in order to ease comparison between our sample and other studies or standards. Our sample's mean PM2.5 concentration of 91.4 $\mu g/m^3$ is more than 18 times the annual concentration of 5 recommended by the World Health Organization (WHO). Our sample's average AQI of 160.82 is within the range that the EPA considers unhealthy for all groups. Comparing our data to samples from the leading air pollution and crime studies, our sample's average PM2.5 level is more than 3 times the average PM10 level 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 in London from 2004-05 is the 17th percentile of the AQI distribution for our Almaty data. Notably, both of these studies rely on PM10 data, since the samples are from periods before PM2.5 monitoring became widespread.

The second part of the table reports crime rates for total crime and 9 crime types as they are used in our analysis, per 100K rayon population and 8-hour period. The table also reports the aggregate of the 5 most common property crime types. We see major theft and major fraud are the two most common crime types.[10] The 9 individual crime types

10. 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.

summarized here cover 99% of all crimes in our sample.

5 Empirical strategy

Our preferred empirical strategy applies the PPML control function approach to properly model the dependent variable, the crime rate, and employs mountain winds and temperature inversions as instrumental variables for the endogenous explanatory variable of air pollution. 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 rayon fixed effects, various time fixed effects, time-varying weather controls, and time-varying seasonal controls. We first focus on estimations using the total crime rate as the dependent variable to assess the overall effects of air pollution, and we then proceed to consider additional estimations using individual and various crime types as the dependent variables to distinguish between the possible mechanisms (described in Section 2) through which air pollution can affect crime.

To build toward our main control function approach, we first discuss methodological choices common to all our approaches, and then discuss a parsimonious but limited PPML fixed effects model. Our main concern about relying on a fixed effects approach without IV is that unobservables causing variation in both pollution and crime and cannot easily be addressed via our weather data or fixed effects.[11] We argue such unobservables may be particularly problematic in a high-pollution middle-income country.

5.1 General methodological issues

We discuss here methodological choices common to all approaches in the literature. These include our focus on PPML, our choice to use the log transformation of PM2.5, our choice of fixed effects, and our choice of controls.

We focus on PPML estimation over alternative approaches due to our non-negative dependent variable and desirable properties of PPML. Our dependent variable, the total crime rate, is non-negative with 21% zero observations, so a model that restricts the conditional mean to be non-negative is naturally desirable. Poisson fixed effects regression is fully robust in the 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

11. Measurement error and reverse causality are other maladies that an instrumental variables approach can also address.

necessary for consistency.[12] PPML is also special among non-linear models in avoiding the incidental parameters problem and is computationally simple.[13] PPML is thus well-suited among nonlinear approaches for fixed effects estimation, which we rely on heavily.

We consider log(PM2.5) rather than PM2.5 pollution as the endogenous explanatory variable for several reasons. One reason is that our sample's PM2.5 distribution exhibits substantial right skew, with a mean of 91 and a median of 72 $\mu g/m^3$. Upon the log transformation, the mean and median (rounded) are both 4.3, and the distribution is much more symmetric. Since we will ultimately apply a linear first stage estimation, this transformation improves the efficiency of our estimations. A second reason to consider log(PM2.5) is that the coefficient of the PPML estimation has a simple interpretation as the elasticity of PM2.5 on the expected mean crime rate. Lastly, evidence from past literature suggests a constant elasticity of PM2.5 is a suitable parametric approach.[14]

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 pollution.

Various time fixed effects allow us to control for unobservables that are constant across specific time categories. Because the instrumental variables we ultimately consider are largely constant across rayons within 8-hour time periods, we do not partial out fixed effects for 8-hour periods for any specifications or 24-hour periods for most specifications. We instead use time fixed effects liberally at higher frequencies. For our preferred specification, we include fixed effects for each of the 7 days of the week, the 3 times-of-day (0-8h, 8-16h, or 16-24h), months, and weeks. The day-of-week and times-of-day fixed effects are essential for unobservable periodic activity that may cause both pollution and crime, while week and month effects address common seasonal trends in both pollution and crime.

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 measured over 8-hour intervals as controls that are common across our rayons. The list of weather

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

13. 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.

14. Specifically, Table 3 of Bondy et al. (2020) reports estimates of marginal effects of AQI that increase for higher levels of AQI, and these increasing estimates are roughly consistent with a constant AQI elasticity of crime.

controls includes temperature indicators for bins of size 5-degrees Celsius, relative humidity, wind velocity, total precipitation, and barometric pressure.

We also include controls that address seasonality at an even higher frequency than week fixed effects. We include average historical temperatures for each 8-hour period of each date from December 2012 to March 2019, as these are the past observations we have available from our weather-data source. Additionally, we determine the hours of daylight observed for each 8-hour period in our sample. Given the distinct patterns of daylight variation for each of the times of day (e.g. the 8h-16h has a range of 7-8 hours while other times of day are mostly dark), we allow the slope to differ for each of the times of the day.

The controls and fixed effects just detailed are all common to other air pollution and crime studies, though we note a few exceptions. Because our data's period is 8 hours rather than 24-hour days, the 3 times-of-day fixed effects are essential given periodic patterns in pollution or crime that depend on the times of the day. The inclusion of daylight hours by 8-hour time period is also less common. But we argue that daylight hours is a good control rather than a bad control (as in e.g., Angrist and Pischke 2009; Cinelli et al. 2020). There are relevant causal paths (e.g., through economic activity) from daylight to air pollution and from daylight to crime, so our empirical strategy should address these paths. Importantly, because daylight hours are determined by nature, daylight hours are not plausibly a direct or indirect outcome of air pollution or crime such that adding it to our models would introduce collider bias. As for other weather and seasonal controls that are common in the existing literature, we believe that they remain good controls in our setting.

5.2 PPML fixed effects approach

The PPML fixed effects models we estimate for the effects of air pollution on the total crime rate, aggregated across all crimes for rayon i within 8-hour period t, takes the form

$$Crimerate_{it} = exp\Big[log(PM2.5)_{it}\beta + X_t\xi^{(0)} + T_t\tau^{(0)} + \alpha_i^{(0)}\Big] + \varepsilon_{it}. \quad (1)$$

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 reporting estimates, we build up the model by first including only $log(PM2.5)_{it}$ as an explanatory variable, and then we gradually add in rayon fixed effects, day-of-week and times-of-day fixed effects, controls, and month/day fixed effects. We also estimate a straightforward extension of the model allowing the coefficient on $log(PM2.5)_{it}$ to vary across the three times of the day (0h-8h, 8h-16h, and 16h-24h).

Our main parameter of interest is β, which reflects the effect of air pollution on crime. For the extensions where we allow the effect of $log(PM2.5)_{it}$ to vary across our three times of day, we conduct the Wald test that all three coefficients are jointly zero. For inference, our baseline approach is to cluster standard errors by date.[15]

Even with our best available fixed effects and controls, the PPML fixed effects approach can still suffer from unobserved heterogeneity idiosyncratic to both time and place that affects both pollution and crime, and we suspect this problem is paramount in high-pollution settings in winter. For example, an idiosyncratic shock that leads households to stay at home increases demand for coal-fueled central heating or home burning of fuels in a middle-income urban center like Almaty. The shock then increases air pollution, while reducing social interactions that lead to crime, thus downward biasing our estimate of air pollution's effect on crime. Conversely, an idiosyncratic shock that increases social and economic activity within a region could increase both air pollution and crime opportunities within the region, so such a shock would upward-bias any estimated effect of air pollution on crime.

To address these problems inherent with the PPML fixed effects approach, we pursue an instrumental variables strategy. We argue that an IV approach is superior to alternatives. An alternative approach to address the unobserved heterogeneity would be to gather more time-varying rayon-level data at 8-hour intervals. But such data, even if readily available, would still be unlikely to fully control for all possible idiosyncratic shocks. Moreover, any available time-varying rayon-level data is likely to reflect economic decisions that could be indirectly related to both pollution and crime, so adding such controls would introduce collider bias. Another approach would be to add fixed effects at higher frequencies or interact location and time, but no amount of fixed effects can perfectly address idiosyncratic factors that affect both pollution and crime. The best approach, which we pursue, is to find variables that induce variation in air pollution.

5.3 Instrumental variables strategy

The terrain of Almaty makes the city particularly 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

15. We discuss our baseline clustering approach and alternative standard error approaches in section A.2.

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 31% of the observations in our sample. The mountains make Almaty a strong candidate for our identification strategy.

We design our mountain wind instrument to a priori allow for the strongest inducement of air pollution in our sample within the southern rayons closest to the mountains. We define our instrument to be the [0,1] share of weather observations within each 8-hour 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 focus on where we anticipate the instrument to induce the strongest treatment in air pollution, and so we remain agnostic about the effects of other wind directions in our main specifications. We exclude the northern rayons in defining the instrument, because the northern rayons are more distant from the mountains, and there is no reason a priori 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' inducing of air pollution changes in 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.

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 an 8-hour period all low-elevation temperature readings are less than the high-elevation temperature readings, when measured at the same time. By relying on multiple observations within the 8-hour period, we better confirm that cold air is trapped below the warm air.

To show that our instruments satisfy the requirement of relevance, we must demonstrate that they satisfy a weak instrument test which is valid in the presence of heteroscedasticity. For all subsequent F statistics, we estimate the effective F-statistic of Montiel Olea and Pflueger (2013).[16]

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, seasonal 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 and seasonal controls, and various 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, there is no obvious pathway between mountains winds or inversions and outcomes of criminal activity. We 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 that suggests relationships of mountain winds or inversions other than through air pollution. So we see no clear violation for an indirect relationship between air pollution and crime, after conditioning on our controls and fixed effects.

As an additional check of our exclusion restriction, we consider reduced-form estimates of the effects of our instrumental variables on the crime rate. We expect if our exclusion restriction and reasoning above are satisfied, we should observe evidence that mountain winds decrease the crime rate and temperature inversions increase the crime rate. The reduced-form results alone cannot confirm the validity of our approach, but they offer additional evidence that we are not missing any major pathways between our instruments and the crime rate that would, on net, change the expected sign when estimating the effect of our instruments on the crime rate.

5.4 Control function approach

For our main results, we follow the PPML control function approach of Lin and Wooldridge (2019), which we argue is the best method for our context of a non-negative dependent variable and endogenous explanatory variable. Our approach is a two-stage estimation strategy that instruments for the endogenous explanatory variable of log(PM2.5), while proposing

16. The recent survey of Andrews et al. (2019) on weak instruments endorses the effective F-statistic and rule-of-thumb of 10 for suitable instrument strength in our context of heteroscedastic errors. The effective F-statistic may be lower than other F-statistics more often reported in empirical work. We apply the implementation of Pflueger and Wang (2015).

an exponential conditional mean for our crime rate dependent variables and adjusting for various forms of time and region-constant heterogeneity. With this approach, we estimate a typical first-stage linear regression of log(PM2.5) on our instrumental variables (mountain winds and temperature inversions) and controls, while absorbing fixed effects. We use Z_{it} to denote the instruments. Our main approach includes day-of-week, times-of-day, month, and week fixed effects in T_t.

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

The second-stage PPML model for crime rate under the control function approach includes the estimated first-stage residuals $\hat{u}_{it}$ in order to control for variation in air pollution unrelated to the instruments. The model includes weather controls, seasonal controls, and fixed effects while excluding the first-stage instruments.

$$Crimerate_{it} = exp\Big[log(PM2.5)_{it}\gamma + X_t\xi^{(2)} + T_t\tau^{(2)} + \alpha_i^{(2)} + \hat{u}_{it}\rho\Big] + e_{it}. \tag{3}$$

A large econometrics literature supports the control function approach of including first-stage residuals in the second stage for consistent estimation, while the common 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).

Our main hypothesis of interest is that for the second stage estimation, $H_0 : \gamma = 0$. The null hypothesis is that there is no effect of air pollution on the total crime rate, when we consider only the variation in air pollution through our excluded instruments of mountain wind and temperature inversions.

For assessing the effects of air population on crime rates for individual crime types or subsets of total crime, we estimate similar specifications. Specifically, we estimate using the alternative crime rates as the dependent variable rather than the total crime rate.

Proper inference requires standard errors that adjust for the estimation error from the first-stage residuals, and cluster as appropriate to adjust for panel correlation and relevant correlation within rayons. We bootstrap the two-stage estimation to address the first-stage estimation error, as suggested by Lin and Wooldridge (2019), and we cluster by day.[17]

17. Our bootstrapped standard errors are each based on 500 full resamplings of size equal to the number of day clusters in the data, which is 96. We conclude that clustering by day is adequate for our inference,

6 Results for total crime rate

Our results confirm economically and statistically significant effects of air pollution increasing crime. We first present results from the PPML fixed effects model, which suggests some relationship between air pollution and crime but leaves concerns about endogeneity. A PPML control function approach, using mountain wind and inversion as instruments, provides greater confidence in identifying the effect of air pollution on crime. We estimate large and positive PM2.5 elasticities of the expected mean crime rate.

6.1 PPML fixed effects estimation

Table 2 gradually builds up our model by adding fixed effects and controls, so we can better understand sources of bias and endogeneity that would motivate the need for an IV strategy. The results suggest some relationship between air pollution and crime, even after inclusion of controls and fixed effects, but there is still reason to be skeptical about any causal relationship. Aside from the usual concern about the limited knowledge of the residual sources of variation in log(PM2.5) in a fixed effects model, the heterogeneous effects of air pollution over times-of-day raise additional endogeneity concerns.

Column 1 is the simple pooled PPML estimation of the relationship between log(PM2.5) air pollution and the total crime rate. This shows an overall positive relationship, but it may be a consequence of many possible types of unobservables that affect both pollution and crime.

Including rayon fixed effects in column 2 raises the log(PM2.5) coefficient. This suggests that rayon-constant unobservables causing negative bias (e.g. crime targeting higher-income, lower-pollution rayons) outweigh unobservables causing positive bias (e.g. elevated local crime in lower-income, higher-pollution rayons).

The last three columns of Table 2 consider heterogeneous coefficients of air pollution across the 3 times of day. In column 3, we add two forms of time fixed effects, for the 7 days of the week and three times of the day, and we allow the air pollution effect to vary across the 3 times of day. The pollution coefficients are positive for all 3 times of day, and the coefficient is largest during the day. Column 4, our first specification to add time-varying weather and seasonal controls, suggests that omitting these controls positively biases all coefficients. We interpret that we are controlling for bias (rather than introducing it with colliders) because weather and seasonal factors are common causes of air pollution and crime. Lastly in column 5, we add month and week fixed effects, which can address unobserved seasonal variation. Including controls and these seasonal fixed effects reduces the pollution coefficients for all 3

after considering various alternative standard error approaches in section A.2.

times of day. The Wald test for no effect for all three pollution coefficients is still significant at the 5% level for column 4 and the 10% level for column 5. In both column 4 and column 5, the pollution coefficient is highly negative overnight, and the average of the coefficients is negative.

The coefficient heterogeneity in the last three columns is important for motivating our instrumental variables strategy. One plausible explanation for the lower or negative coefficients for evening (16h-24h) and overnight (0h-8h) is that there is idiosyncratic heterogeneity not absorbed by fixed effects that biases coefficients downward. Any reason other than pollution that keeps people at home could lower opportunities for crime and increase demand for home heating, causing higher local air pollution. These endogeneity concerns are likely to be stronger in low or middle-income countries than high-income countries, so studies from cleaner cities in high-income countries (Herrnstadt et al. 2021; Bondy et al. 2020) are more likely to find a positive relationship between pollution and crime in fixed effects estimation without using IV. In contrast, an IV strategy is essential to addressing such unobservables for our setting of Almaty.

An alternative possibility worth discussing is that causal relationships drive the overall negative pollution-crime relationships in the last two columns, but we ultimately rule out this explanation. One source of a negative causal relationship could be air pollution avoidance, though we expect this relationship to be weak in Almaty given the lack of public awareness about air pollution (discussed in background section 3). Another explanation for a lower elasticity at night is that there are fewer potential crime targets overnight. Our IV strategy can help us sort whether causal factors or non-causal factors are what drive the negative relationship in the fixed effects estimation. Our control function results, which we discuss next, reveal a positive effect of air pollution on crime, so we can conclude that the negative pollution-crime relationship in the last two columns is not due to causal factors.

6.2 PPML control function approach

Using the mountain wind and inversion instrumental variables and the PPML control function approach, we confirm a large and positive air pollution elasticity of the total crime rate. Our tests for instrument strength confirm that mountain wind and inversion induce substantial variation in air pollution, and our tests for the endogeneity of log(PM2.5) broadly confirm the need for the IV approach. In addition, a reduced-form PPML model shows that our instruments have the effects we anticipate on the total crime rate, so the reduced-form results provide further support for our estimation strategy.

We report all results from our control function specification with our main parameter estimates in Table 3. In the appendix, we include additional estimates from the first stage in Table A1 and the second stage controls in Table A2.

We first discuss the large pollution coefficient in Table 3, column 1, which reports results for a parsimonious specification with fixed effects but no weather or seasonal controls. This specification includes fixed effects for rayons, day-of-week, and times-of-day. The effective F-statistic of 50.06 suggests a strong relationship between our instrumental variables and air pollution that allows us to identify the second-stage estimate relating air pollution to the total crime rate. This specification requires some caution in interpretation, because the seasonal unobservables or weather that are not included may be common causes of our instruments and the crime rate. Such common causes lead to exclusion restriction violations that bias our second-stage estimate of the log(PM2.5) coefficient and invalidate the test of endogeneity based on the t-statistic of the residual coefficient. Still, we include the results of this specification to build toward our preferred model.

For our preferred specification in column 2 of Table 3, we report a statistically significant effect of air pollution on the total crime rate, and auxiliary statistics that support the validity of our estimate. Building on column 1, the column 2 specification adds seasonal month/week fixed effects, current weather controls, and seasonal controls. In column 2, we reject the null hypothesis of no effect of air pollution on crime at the 5% significance level. Based on the statistically significant residual coefficient, we also reject the null hypothesis of the exogeneity of log(PM2.5) air pollution. The F-statistic falls from column 1 but is still well above the threshold for weak instruments concern. We can see from Table A1 that the controls and seasonal fixed effects reduce the relationship between inversions and air pollution, but this is a necessary correction since the seasonal effects on inversions may also be related to seasonal effects on the crime rate that would violate the exclusion restriction. Notably, the pseudo R-squared increases from 0.132 to 0.183, suggesting we are including important sources of variation in the crime rate that could relate to exclusion restriction violations.

Column 3 and column 4 of Table 3 show that results are robust and stable to including more fixed effects in the model, though we maintain column 2 as our preferred specification. In column 3, we include Week*Times-of-day fixed effects to adjust for seasonal variation that may vary by times of day, though we expect our seasonal fixed effects and seasonal controls varying by an 8-hour period may already be adequate. In column 4, we add Rayon*Day-of-week effects, which may adjust for periodic patterns in rayon activity that are common causes for both air pollution and crime, such as different commuting patterns on weekends versus weekdays. Neither set of fixed effects leads to a substantial qualitative change in our results, neither adds much to the Pseudo R-squared, and both increase standard errors. To be conservative in our approach, while maintaining fixed effects that are most common in the pollution-crime literature, we focus on only column 2 as our preferred specification for further analysis.

Reduced-form PPML estimations, which relate our instrumental variables directly to the crime rate, provide further support for our empirical strategy. Table 4 shows that for our preferred set of controls and fixed effects in column 2, our mountain wind instrument has a negative effect on the crime rate and temperature inversion has a positive effect on the crime rate, each statistically significant at the 5% level. These are the results we would expect from our identifying assumption that the instruments are related to the crime rate only through air pollution. These results are robust across all the same patterns of controls and fixed effects that we consider in Table 3. By evaluating the reduced-form effects of weather variation on the crime rate, we are following a similar reduced-form approach as Herrnstadt et al. (2021) evaluating the effects of wind blowing from highways or Jones (2022) evaluating the effects of dust storms. Likewise, the survey of Aguilar-Gomez et al. (2022) broadly recommends such reduced-form estimation to assess the overall effects of pollution beyond just one air pollutant. The results here consider all possible paths between our instruments and the crime rate, and thus, provide a further check on our exclusion restriction. The comparison of the large changes in the inversion coefficient between column (1) and column (2) further supports our understanding that the column (1) specification with no controls and no seasonal fixed effects violates the exclusion restriction, since the seasonal controls and fixed effects address common causes of temperature inversions and crime. The other three columns do not eliminate the possibility of an exclusion restriction violation, but they rule out the possibility of exclusion restriction violations that would on net reduce (in absolute terms) the effects of our instrumental variables on the crime rate. Consequently, the reduced-form estimates give us greater confidence in all our control function results.

Our preferred estimates from column 2 of Table 3 suggest an air pollution elasticity of crime rate that is larger than other leading pollution-crime estimates. With a PPML model for the effect of log(PM2.5) on the crime rate, we can interpret our point estimate $\hat{\gamma} = 0.38$ for the coefficient of log(PM2.5) as the air pollution elasticity of the expected crime rate, so a 10% increase in PM2.5 pollution (e.g. an increase from 91 to 100 $\mu g/m^3$ at the sample's mean PM2.5) would increase the expected crime rate by roughly 4%. 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.[18] For London, Bondy et al. (2020) report an AQI elasticity of the total crime rate equal to 0.08 for their preferred IV estimates (see

18. 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.

p. 571).

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) $\sigma = 0.72$ (which roughly corresponds to doubling the PM2.5 air pollution) would then increase the expected crime rate by a factor of $exp(\hat{\gamma}\sigma)$ equal to a 31% increase in the expected mean crime rate. Since our sample's mean crime rate roughly equals the sample's standard deviation, this change also corresponds to an increase of 0.31 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 more than half 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 31% figure 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 common in low or middle-income countries. Unfortunately, we find even larger PM2.5 elasticities of the crime rate at the levels of pollution that we observe in Almaty.

6.3 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. Results from TSLS estimation provide additional checks on our estimation strategy. Our work suggests TSLS inference yields smaller standard errors than our control function approach, thus reinforcing the importance of our control function approach. A variety of approaches that allow for greater possibilities of serial correlation find no substantial increase in standard errors, so these alternatives reinforce that our main results already address serial correlation adequately. Alternative instrumental variable specifications, alternative sets of controls, and coarser forms of aggregation all yield similar results. Reduced-form PPML results for current and lagged treatments suggest our results are adequate in considering only contemporaneous treatment. Lastly, placebo specifications find no statistically significant effects of air pollution from irrelevant past and future periods on the current crime rate.

7 Results for crime types

A final contribution of our paper is in exploring the mechanisms of how air pollution affects crime by estimating effects across crime types. As we explain in Sections 2 to 4, our setting in Almaty is appropriate for identifying an effect on high-stakes theft and high-stakes fraud relative to low-stakes theft and low-stakes fraud: we have data to distinguish the two crime types, and we argue Almaty is less likely to have high-stakes property crime go unreported. A rejection of a null effect on high-stakes property crime would be evidence to support our main hypothesis that air pollution causes criminals to discount the consequences of their actions, rather than an exclusive role for air pollution causing aggression. We explore our hypothesis first by considering the four most common crime types, and then we consider various crime type aggregates.

The first three columns of Table 5 suggest evidence for the hypothesis that results are driven by higher-stakes crimes. The effect on major theft, the most common crime, is statistically significant at the 10% level, and the effect on fraud is statistically significant at the 5% level. Meanwhile, the point estimate for petty theft and petty fraud is smaller, and we fail to reject the hypothesis of no effect of air pollution. The results are consistent with the hypothesis that higher discounting matters for the effect of air pollution on crime, and inconsistent with the hypothesis that aggression is the exclusive mechanism.

Our finding in column 4 of an effect on robbery, significant at the 5% level, is notable in the pollution-crime literature. Because robbery is a violent crime that differs from theft only by the threat of violence, we would expect in theory it could have large effects due to air pollution increasing either aggression or discounting, and we confirm a large effect on robbery. Consistent with the related literature, we also estimate a larger standard error for robbery.[19] Our data and approach though allow us to identify the statistically significant effect on robbery that theory anticipates.

Table 6 presents results for various crime type aggregates of interest. Column 1 considers

19. Neither Bondy et al. (2020) in London nor Herrnstadt et al. (2021) find a statistically significant effect on robbery, though the large standard errors they estimate suggests underpower for this crime type. Jones (2022) for U.S. dust storms finds a relatively large point estimate for robbery but even larger standard errors, and the robbery test p-value is just above 0.1.

an aggregate of high-stakes property crime which includes major theft and major fraud. Robbery is a high-stakes property crime in some classifications, but we exclude it because it contains a violent element, by definition. We want to consider only property crimes that are not driven by aggression. Column 2 considers an aggregate of low-stakes property crimes that include petty theft and fraud, drug use, and hooliganism. Column 3 considers an aggregate of our sample's most common violent crimes, including robbery, murder, serious bodily harm, and medium bodily harm.

Columns 1 and 2 present key results in support of our main hypothesis regarding high-stakes property crime and low-stakes property crime. For high-stakes property crime, the effect of air pollution is statistically significant at the 5% level with a p-value of 0.015. As this aggregate excludes violent crime including robbery, the result argues against an exclusive role for aggression in driving the effects of air pollution on crime, and supports our hypothesis that higher discounting is an important mechanism. Further support for our hypothesis is in column 2, which fails to reject the null hypothesis of an effect of air pollution on low-stakes property crime. Introducing drug use and hooliganism into the broader measure of low-stakes property crime even drives the coefficient to be negative. A larger effect of air pollution on high-stakes property crime than on low-stakes property crime is consistent with the mechanism of higher discounting.

The wide standard errors on petty crime and low-stakes property crime raise the question of whether the differences between high-stakes and low-stakes crime are statistically significant. Consequently, we consider a TSLS model of the difference between major theft/fraud and petty theft/fraud. We use a linear model, since we can no longer consider an exponential-mean PPML model for a difference. We focus on this difference rather than alternatives because this difference compares types of crime that only differ in the value of the property involved. So, this difference offers the purest test available of whether results are driven by the different values of property at stake. Plus, we stack the deck against ourselves by excluding drug use and hooliganism that introduce negative effects into our broader aggregate of low-stakes property crime. To show the robustness of this result, we consider a pattern of introducing fixed effects similar to our main tables. Taking the difference between the two counts may provide additional control for common unobservables that affect both crime types. As a final check for robustness, we consider date fixed effects to further address positive or negative shocks to the difference that may be common within a date.

The results of our TSLS model of major theft/fraud less petty theft/fraud in Table A3 confirm our interpretation of our main results. There is indeed a statistically meaningful difference between the effects on high-stakes theft/fraud and low-stakes theft/fraud, though we interpret this result with some caution. Results are statistically significant at the 5% level across the four specifications, and the difference is statistically significant at the 1% level in the column 2 model which has fixed effects for date, times-of-day, and rayons. We acknowledge that higher misreporting of low-stakes petty theft/fraud vs. high-stakes theft/fraud could contribute to our finding, yet our result though is still informative. A failure to find a significant difference in our setting, which we argue is among the best possible settings to identify an effect on high stakes theft/fraud, would have been a flat rejection of the theory that air pollution causes higher discounting.

In the final two columns of Table 6, we expect to find large effects, because the aggregates include violent crimes, which are well-reported and potentially driven by either aggression or discounting. For the violent crimes, the effect of air pollution is statistically significant at the 1% level, though we acknowledge this effect is largely driven by robbery.[20] Lastly, we consider a dependent variable of high-stakes crime that aggregates violent crimes and high-stakes property crimes. This is the subset of crimes where we anticipate the effects of air pollution to be the largest. The effect of air pollution on this crime rate is statistically significant at the 1% level.

8 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 high-stakes property crimes and robbery and not for low-stakes property crime, we strengthen support for the theory that air pollution leads to higher discounting, and we find evidence against the possibility that higher aggression is the exclusive channel through which air pollution affects crime. We advance methodology within the pollution-crime literature by applying the PPML control function approach.

Our work, naturally, motivates further research questions and works along the lines of our contributions. Though we use our results to distinguish between competing hypotheses about the mechanisms through which air pollution affects crime, we still lack direct evidence of the mechanisms we emphasize as relevant. Further work could clarify which of the microfoundations for higher discounting is most relevant. Though we apply the PPML control function approach well for our purposes, there is still further opportunity to extend econometric methods for this approach.

20. If we consider a dependent variable that aggregates the three most common violent crimes other than robbery, we obtain a positive point estimate of 0.685 but a standard error of 0.707. We also note that the TSLS coefficient for log(PM2.5) affecting murder is 0.0080 with a s.e. of 0.0033, but this result we interpret with caution since the data is too sparse for us to estimate using the PPML control function approach.

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) 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.

A Appendix: robustness and placebo tests

A.1 TSLS estimations

TSLS estimation provides an additional check on our results. Though we stand by our main PPML control function approach, any vast differences from TSLS results would require some explanation.

Table A4 reports TSLS coefficient estimates that correspond to our control function approaches in Table 3, and the results are qualitatively similar. Note that TSLS coefficients represent constant marginal effects of log(PM2.5) on the expected crime rather than elasticities, so they are not directly comparable. To better facilitate comparisons, we also report in Table A2 the average marginal effects (AME) of log(PM2.5) that we determine from our PPML control function parameter estimates. We can derive these average marginal effects analytically by multiplying the control function approach's estimated elasticities by the sample mean of the total crime rate.

The results suggest that by ignoring count features of the data, TSLS may underestimate true standard errors. Comparing the average marginal effects from the PPML control function estimation against TSLS, we find TSLS yields smaller point estimates and narrower confidence intervals, which are strictly within our PPML control function confidence intervals. The point estimates for column 1 are similar, but the control function AME standard errors are 11% larger. For our preferred fixed effects and controls specification in column 2, the 95% confidence interval for TSLS of [0.13, 0.72] is strictly within the 95% confidence interval for the AME of the PPML control function approach, [0.10, 0.94]. Similar conclusions hold true for columns 3 and 4. As the PPML control function approach properly considers the count-derived features of our data, and TSLS does not, we trust that the confidence intervals from the PPML control function approach are appropriately conservative.

TSLS offers an additional check on the overidentifying restrictions for our model with two excluded instruments and one endogenous variable. One advantage of TSLS relative to the exactly-identified PPML control function estimation is that TSLS permits the Sargan-Hansen J-test of overidentifying restrictions. As Roodman (2009) notes, low J-test p-values below 0.25 suggest concerns about the correlation between instruments and dependent variable residuals that may reflect exclusion restriction violations, while p-values near 1 may be implausibly high. The p-values for our J tests are within a comfortable range.

TSLS also offers an additional check on our instrument strength. Since LIML is less unbiased, though less efficient than TSLS, Angrist and Pischke (2009) suggest comparing LIML estimates to TSLS estimates to check instrument strength. Though judging by our Montiel Olea and Pflueger (2013) effective F-statistic results, we expect further confirmation that our instruments are sufficiently strong. Indeed, we find that all LIML point estimates for our air pollution parameter estimate are within 0.7% (less than a percent) of our TSLS estimates. We do not report LIML estimates in any greater detail because of their redundancy.

TSLS offers easy opportunity to explore heterogeneous effects of air pollution on the crime rate, though our results here are inconclusive. One possibility we consider is heterogeneity in the log(PM2.5) coefficient between the 8h-16h time of day and other times of day. The other possibility is heterogeneity in the log(PM2.5) below and above the sample median of PM2.5. Both of these models have two endogenous pollution variables which creates greater challenges for identification. We fail to reject the hypothesis of heterogenous effects in either case, though we acknowledge that this result may be a result of underpower. Given these results are inconclusive, we do not report them in any greater detail.

As a final check of our results using the TSLS approach, we confirm that we obtain similar results from an exactly-identified TSLS model with one instrument. Recent econometric literature supports robustness properties of exactly-identified linear models (e.g., Angrist and Kolesár 2021), and there is concern about models with multiple IV aggregating effects of treatment on vastly different populations of interest (e.g., Ch. 9 of Morgan and Winship 2014). To address such concerns, we report in Table A5 results for TSLS with identification coming only from the mountain wind instrument. We obtain qualitatively similar results to the overidentified model, though point estimates are somewhat (14-21%) smaller. Given the similar results, we continue to focus on results from our model with both the mountain wind and inversion IV.

A.2 Standard errors

Our main specifications cluster by date. Because we rely on instrumental variables and weather controls that are constant across rayons, the most important error correlation to consider is across the rayons, following the clustering guidance of Abadie et al. (2017). We cluster by date rather than 8-hour period to be conservative in allowing for error correlation across time periods within dates. Our 96 dates are an adequate number of clusters for asymptotics (see, e.g., Cameron and Miller 2015).

Several alternative standard error estimation approaches reinforce that our approach of clustering by date is adequate for our preferred specifications, though the possibility of serial correlation is important to consider. Indeed, we find that our models without seasonal controls or seasonal (week/month) fixed effects do not adequately adjust for serial correlation. But for our preferred specifications including such seasonal controls and effects, the alternative standard error approaches reduce standard errors. Consequently, we conclude that our main approach of clustering by date is both adequate and conservative.

For our TSLS estimates in Table A4, we can easily implement standard errors of Driscoll and Kraay (1998) that allow for arbitrary correlation within panels (effectively clustering by each 8-hour panel) and also permit serial correlation of a chosen bandwidth like Newey and West (1987). We pick a bandwidth of 26 periods that derives from the Newey and West (1987) optimal bandwidth for the dataset collapsed to a single time series dimension. A bandwidth of 26 corresponds to just under 10 days given that we have 3 time periods per day.

Our results for Driscoll-Kraay standard errors in the bottom section of Table A4 suggest that our original standard errors were adequate in addressing serial correlation. Though the standard errors slightly rise (by 9%) for column 1 without seasonal controls and seasonal fixed effects, the Driscoll-Kraay approach actually reduces the standard errors for the other specifications.

For our PPML estimates, an alternative well-developed approach to Driscoll-Kraay standard errors is two-way clustering.[21] Notice we cannot follow the simple approach of two-way clustering by rayon and by date, because we have too few rayons to cluster by rayon. An alternative possibility then is to two-way cluster by date and by rayon-week.[22] This approach allows for correlation of errors within rayons for each week, and the number of rayon-weeks of 98 is adequate for clustering asymptotics.

Two-way clustering by date and rayon-week for our single-stage PPML estimates in Table 2 and Table 4 yields a similar pattern as the Driscoll-Kraay TSLS estimates. For brevity, we do not report the specific results in our main tables but describe the relevant details here. For Table 4 of our reduced-form estimates, the two-way clustering decreases the estimated standard error for the mountain wind coefficient in all four columns. For the inversion coefficient, the estimated standard error for column 1 (again, lacking seasonal effects and controls) increases by 13% relative to one-way date clustering, but the standard error falls relative to one-way date clustering for the last three columns. Similarly, for Table 2, the standard errors increase by 20% for each of the first three columns, which lack seasonal controls or seasonal fixed effects. But for the last 3 columns which include either seasonal controls or seasonal fixed effects, the average change in standard errors across the three log(PM2.5) coefficients is never more than 1%.

So across a variety of specifications and estimation approaches, we find that standard

21. We are unaware of econometric theory that explicitly extends the Driscoll-Kraay approach to PPML estimation, particularly two-stage PPML control function estimation.

22. Clustering along interactions of the time and panel dimension is less common as of late, but not new. For example, an early example of two-way clustering cited by the survey of Cameron and Miller (2015) is Acemoglu and Pischke (2003), who cluster by individual and by region-time.

error methods that permit greater within-rayon correlation than our baseline approach do not increase our standard errors. We then conclude that our baseline approach of clustering by date is adequate to address correlation within rayons across time.

A.3 Alternative model specifications

Table A6 confirms our results are robust to alternative specifications of the instrumental variables. The first column considers in place of our wind instrument two equal-sized bins ranging from above 0 to the midpoint of the variable's range (0.375), and from the range midpoint to the max (0.75). This specification checks that our results are not overly sensitive to a few high-leverage values of the mountain wind instrument. The second column 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. In the third column, we divide the other wind directions (aside from the wind directions that we define as mountain wind) into three bins and estimate using all four wind-direction shares as instruments, so the omitted category is weather reports with no wind direction identified. In the last column, we use an alternative temperature inversion instrument which requires that the high-elevation temperature exceeds the low-elevation temperature all day, and thus we consider only long-lasting temperature inversions. Across the four alternatives, there is some variation in the point estimates around our preferred specification, but the alternative IV specifications do not substantially alter our results.

Table A7 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. Because daylight hours is a less common control in the literature, we consider in column 3 a specification without our 3 daylight hour controls. Column 4 considers no controls. Column 5 adds more controls common elsewhere in the literature 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 results suggest somewhat smaller point estimates than our preferred specification. The largest reduction in the point estimate is for the specification with no controls. 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 collider bias, because our choice of controls largely 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 also consider including as a control a clear visibility indicator from our weather sources, since we would like to control for the mechanism that air pollution affects crime through visibility rather than criminal behavior. Including visibility as a control increases our estimate for the effect of air pollution on crime. However, we expect visibility may be a bad control because poor visibility is also an outcome of pollution, and controlling for it would introduce a colliding path if, for instance, unobserved visibility variables affect both crime and our measured visibility. To avoid confusion, we omit results with this visibility control from any of our tables.

We confirm in table A8 that our results are robust to alternative aggregations, which serve as an important check on our methods. Our choice to focus on 8-hour periods rather than 24-hour periods clearly motivates a check that the higher time frequency does not somehow drive our results. More broadly, large spillovers between time or location would be substantial violations of stable unit value treatment, and we want to ensure our results are not an artifact of disaggregation. We consider estimation under two alternative aggregations. Column 1 collapses our data set from 8-hour time periods to 24-hour time periods, and we see little effect on the result. Column 2 collapses our rayon-level data into two regions, the southern rayons close to the mountains and the northern rayons far from the mountains. This approach leads to a substantial loss of information, so the standard error is much larger. Still, the effect is statistically significant at the 10% level and the point estimate is larger.

A natural question for our time frame is whether the onset of the COVID-19 pandemic in March 2020 could affect our 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 and month fixed effects capture important unobservables related to the pandemic. Still, to address the concern, we estimate our sample dropping all observations from March, so we use only data between December and February. In column 3 of A8, we see the estimated elasticity remains above 0.3 and statistically significant at the 5% level, despite the smaller sample.

Lastly, 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 data. 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 from Table 4. We see from the results in Table A9 that including 1, 2, or 3 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 the control function 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.

All of the placebo tests in Table A10 fail to reject the null hypothesis of no effect from the irrelevant-date pollution on the current crime rate. The irrelevant dates we consider are +/- 7 days, 14 days, and 31 days (i.e., 1 week, 2 weeks, and 1 month). We pick these periods also to confirm that our results are not some artifact of the periodicity of our data. The effective F-statistics for our first-stages are still large, given that we instrument for irrelevant-date pollution using irrelevant-date instruments and controls, so our failure to reject is not a consequence of weak instruments. The standard errors for the placebo effects are of similar magnitude as our main results, so our failure to reject the placebo tests is not due to imprecision or underpower in estimating the effects of the irrelevant day pollution. Thus, our placebo specifications confirm that our main findings thus far are not an artifact of our estimation strategy.

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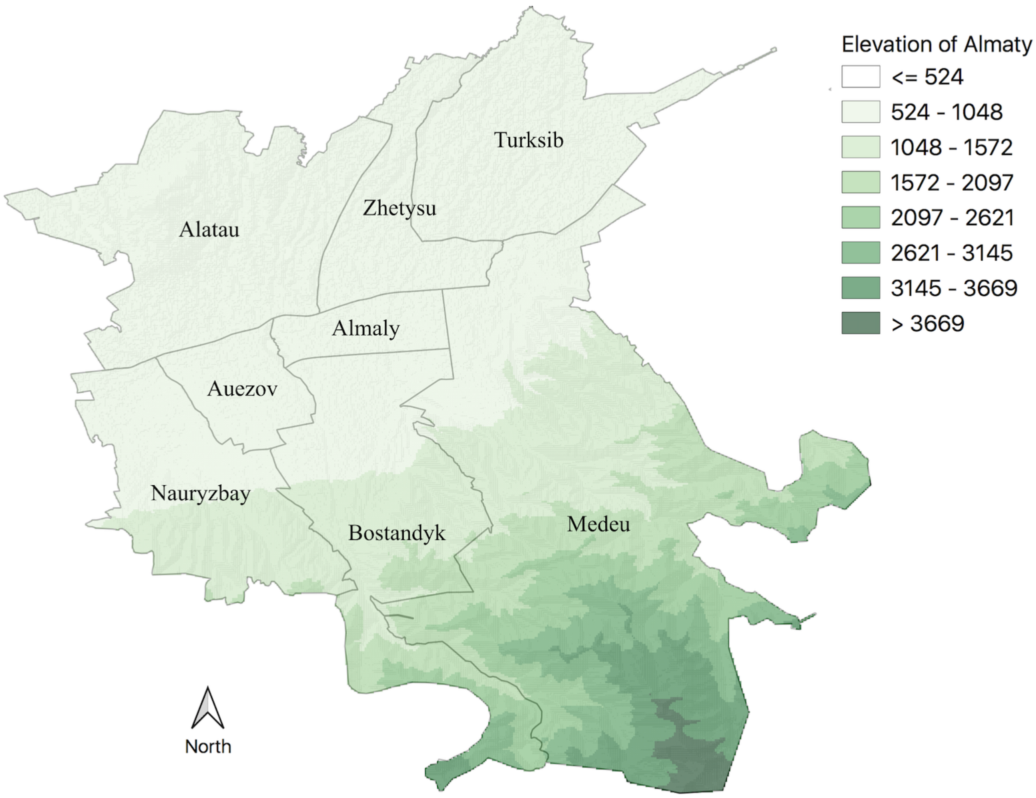

Figure 1: Districts and elevation of Almaty (in meters).

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

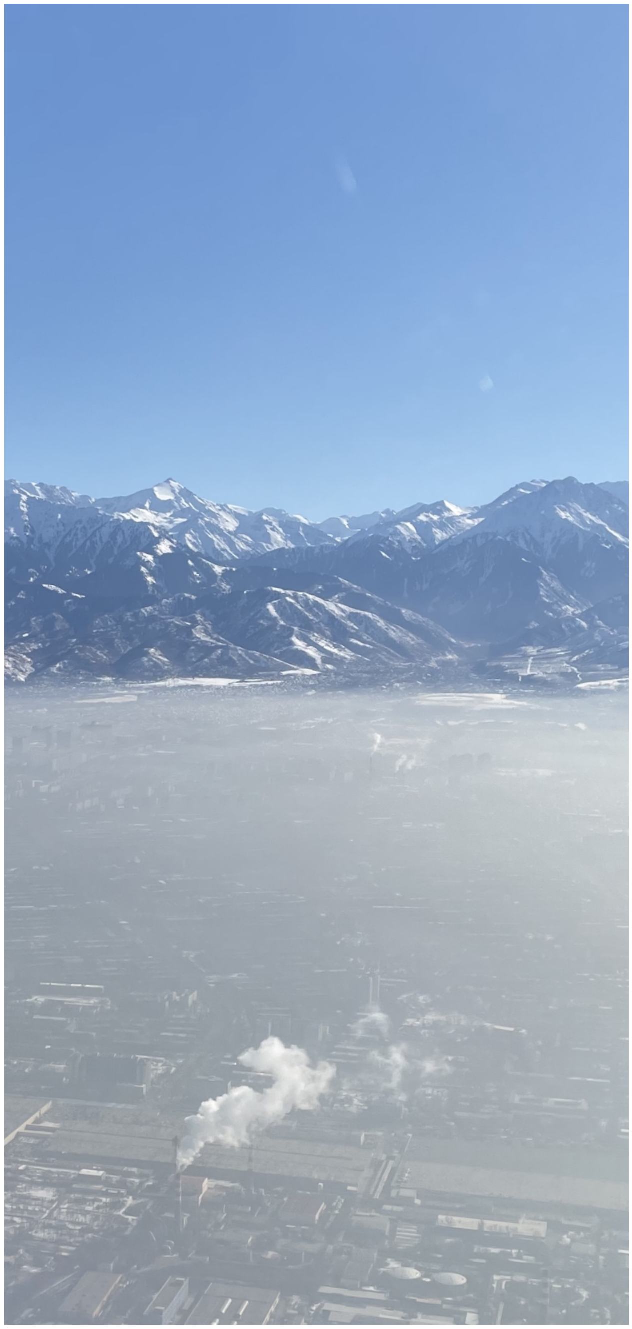

Figure A1: Temperature inversion in Almaty (our photo).

Table 1: Summary Statistics

Variables	Mean	Std. Dev.	Min.	Max.
Pollution measures				
PM2.5 ($\mu g/m^3$)	91.43	72.48	2.00	822.50
Log(PM2.5)	4.27	0.72	0.69	6.71
Air Quality Index	160.82	62.81	8.33	500.00
Crime rates				
Total	1.36	1.37	0.00	8.73
Property crimes	1.28	1.33	0.00	8.73
Major theft	0.93	1.00	0.00	6.49
Major fraud	0.21	0.41	0.00	3.34
Petty theft/fraud	0.09	0.28	0.00	6.99
Robbery	0.04	0.14	0.00	1.56
Hooliganism	0.02	0.11	0.00	1.02
Drug-related crime	0.02	0.10	0.00	0.95
Medium bodily harm	0.01	0.09	0.00	0.95
Serious bodily harm	0.01	0.05	0.00	0.78
Murder	0.00	0.04	0.00	0.48
Instrumental variables				
Mountain wind	0.07	0.13	0.00	0.75
Inversion	0.31	0.46	0.00	1.00
Control variables				
Temperature (C)	0.52	6.46	-12.94	18.50
Humidity (%)	77.04	18.41	22.56	100.00
Wind velocity (m/s)	2.42	1.01	0.75	6.13
Total precipitation (mm)	0.47	1.53	0.00	14.00
Atmospheric pressure (mmHg)	704.31	4.05	694.66	715.71
Historical temperature (2012-19)	-0.83	6.23	-15.26	14.19
Hours of daylight (0:00-8:00)	0.39	0.45	0.00	1.42
Hours of daylight (8:00-16:00)	7.88	0.16	7.60	8.00
Hours of daylight (16:00-24:00)	2.34	0.58	1.35	3.28

Notes: The sample size is 1702 observations for all variables above, except for the three daylight hours interactions (the sum of the three interaction sample sizes is 1702). All crime rates are reported per 100K rayon population and 8-hour time periods.

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

	(1)	(2)	(3)	(4)	(5)
Log(PM2.5)	0.161***	0.235***	0.351***	0.190*	0.127
	(0.0418)	(0.0466)	(0.0718)	(0.0978)	(0.0821)
Log(PM2.5)*I(0h-8h)			-0.205**	-0.175	-0.0902
			(0.0844)	(0.109)	(0.107)
Log(PM2.5)*I(16h-24h)			-0.191**	-0.339***	-0.274***
			(0.0857)	(0.111)	(0.104)
Rayon FE	no	yes	yes	yes	yes
Day-of-Week FE	no	no	yes	yes	yes
8h Times-of-Day FE	no	no	yes	yes	yes
Controls	no	no	no	yes	yes
Week/Month FE	no	no	no	no	yes
Wald test p-value			<0.001	0.023	0.053
Observations	1702	1702	1702	1702	1702
Pseudo R-Squared	0.00569	0.0724	0.133	0.164	0.183

Notes: The Wald test p-value is for the null hypothesis that the three log(PM2.5) coefficients are all zero. Standard errors are clustered by day. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, barometric pressure, historical temperatures (averaged from 2012-19), and hours of daylight. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table 3: PPML control function approach,
Mountain wind & inversion IVs,
Log(PM2.5) effect on total crime rate

	(1)	(2)	(3)	(4)
Log(PM2.5)	0.373***	0.379**	0.359**	0.360**
	(0.109)	(0.157)	(0.162)	(0.167)
Residuals	-0.200	-0.408**	-0.391**	-0.398**
	(0.122)	(0.169)	(0.175)	(0.183)
Rayon FE	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes
Times-of-Day FE	yes	yes	yes	yes
Controls	no	yes	yes	yes
Month/Week FE	no	yes	yes	yes
Times-of-Day*Week FE	no	no	yes	yes
Rayon*DoW FE	no	no	no	yes
First Stage F-Statistic	50.06	19.19	23.42	24.08
Observations	1702	1702	1702	1702
Pseudo R-Squared	0.132	0.183	0.187	0.193

Notes: Standard errors are derived by bootstrapping two-stage PPML CF coefficient estimates. Our bootstraps use 500 replications of resampled day clusters. We report the Montiel-Pflueger effective F-statistic for the first stage. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, barometric pressure, historical temperatures (averaged from 2012-19), and hours of daylight. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

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

	(1)	(2)	(3)	(4)
Mountain wind	-0.463*	-0.461**	-0.497**	-0.506**
	(0.249)	(0.195)	(0.232)	(0.233)
Inversion	0.259***	0.129**	0.108*	0.102*
	(0.0703)	(0.0608)	(0.0615)	(0.0609)
Rayon FE	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes
Times-of-Day FE	yes	yes	yes	yes
Controls	no	yes	yes	yes
Month/Week FE	no	yes	yes	yes
Times-of-Day*Week FE	no	no	yes	yes
Rayon*DoW FE	no	no	no	yes
Observations	1702	1702	1702	1702
Pseudo R-Squared	0.129	0.183	0.187	0.193

Notes: Standard errors are clustered by day. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, barometric pressure, historical temperatures (averaged from 2012-19), and hours of daylight. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table 5: PPML control function approach,
Mountain wind & inversion IVs,
Log(PM2.5) effect on four most common crime types

	(1) Major Theft	(2) Major Fraud	(3) Petty Theft/Fraud	(4) Robbery (Violent)
Log(PM2.5)	0.282*	0.917**	0.181	1.530**
	(0.161)	(0.428)	(0.557)	(0.701)
Residuals	-0.343**	-0.863*	0.0584	-1.644**
	(0.170)	(0.461)	(0.590)	(0.761)
Controls	yes	yes	yes	yes
Rayon FE	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes
Times-of-Day FE	yes	yes	yes	yes
Month/Week FE	yes	yes	yes	yes
Observations	1702	1702	1667	1702
Pseudo R-Squared	0.151	0.213	0.199	0.0875

Notes: Standard errors are derived by bootstrapping two-stage PPML CF coefficient estimates. Our bootstraps use 500 replications of resampled day clusters. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, barometric pressure, historical temperatures (averaged from 2012-19), and hours of daylight. The Montiel-Pflueger effective (first stage) F-statistic for all columns is 19.19. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table 6: PPML control function approach,
Mountain wind & inversion IVs,
Log(PM2.5) effect on crime aggregates

	(1) High-Stakes Property	(2) Low-Stakes Property	(3) All Violent	(4) High-Stakes Violent/Property
Log(PM2.5)	0.416**	-0.156	1.097***	0.459***
	(0.171)	(0.398)	(0.414)	(0.162)
Residuals	-0.459**	0.201	-1.163**	-0.500***
	(0.180)	(0.409)	(0.458)	(0.174)
Controls	yes	yes	yes	yes
Rayon FE	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes
Times-of-Day FE	yes	yes	yes	yes
Week/Month FE	yes	yes	yes	yes
Observations	1702	1702	1702	1702
Pseudo R-Squared	0.184	0.135	0.0781	0.176

Notes: Column 4 aggregates Columns 1 and 3. Standard errors are derived by bootstrapping two-stage PPML CF coefficient estimates. Our bootstraps use 500 replications of resampled day clusters. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, barometric pressure, historical temperatures (averaged from 2012-19), and hours of daylight. The Montiel-Pflueger effective (first stage) F-statistic for all columns is 19.19. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A1: First stage results,
Linear fixed effects estimates for instruments' effect on log(PM2.5)

	(1)	(2)	(3)	(4)
Mountain wind	-1.601***	-1.442***	-1.511***	-1.511***
	(0.366)	(0.289)	(0.279)	(0.272)
Inversion	0.658***	0.176***	0.222***	0.219***
	(0.0734)	(0.0619)	(0.0635)	(0.0641)
Humidity (%)		0.00525**	0.00689**	0.00689**
		(0.00260)	(0.00271)	(0.00273)
Wind velocity (m/s)		-0.171***	-0.170***	-0.170***
		(0.0208)	(0.0206)	(0.0204)
Total precipitation (mm)		-0.0699***	-0.0761***	-0.0764***
		(0.0211)	(0.0186)	(0.0186)
Atmospheric pressure (mmHg)		-0.0239***	-0.0220***	-0.0221***
		(0.00670)	(0.00629)	(0.00637)
Historical temperature (2012-19)		0.0310***	0.0261**	0.0268**
		(0.0112)	(0.0104)	(0.0104)
Hours of daylight (0:00-8:00)		0.229	0.729	0.729
		(0.165)	(1.099)	(1.106)
Hours of daylight (8:00-16:00)		0.456	-1.661	-1.640
		(0.388)	(1.615)	(1.618)
Hours of daylight (16:00-24:00)		0.167	0.304	0.304
		(0.106)	(0.817)	(0.821)
Rayon FE	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes
Times-of-Day FE	yes	yes	yes	yes
Controls	no	yes	yes	yes
Week/Month FE	no	yes	yes	yes
Times-of-Day*Week FE	no	no	yes	yes
Rayon*DoW FE	no	no	no	yes
Montiel-Pflueger F-Statistic	50.06	19.19	23.42	24.08
Observations	1702	1702	1702	1702
R-Squared	0.386	0.616	0.638	0.651

Notes: The instrumental variables excluded in the second stage are bolded. Standard errors are clustered by day. Controls include temperature bins (of width 5 degrees C). The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A2: PPML control function approach,
Mountain wind & inversion IVs, Log(PM2.5) effect on total crime rates,
Expanded results with control parameters and marginal effects

	(1)	(2)	(3)	(4)
Log(PM2.5)	0.373***	0.379**	0.359**	0.360**
	(0.109)	(0.157)	(0.162)	(0.167)
Residuals	-0.200	-0.408**	-0.391**	-0.398**
	(0.122)	(0.169)	(0.175)	(0.183)
Humidity (%)		-0.00436	-0.00464	-0.00462
		(0.00390)	(0.00398)	(0.00400)
Wind velocity (m/s)		0.0384	0.0250	0.0255
		(0.0405)	(0.0432)	(0.0435)
Total precipitation (mm)		0.0251	0.0251	0.0237
		(0.0269)	(0.0241)	(0.0241)
Atmospheric pressure (mmHg)		0.0205*	0.0190	0.0192
		(0.0117)	(0.0122)	(0.0123)
Historical temperature (2012-19)		-0.0231*	-0.0165	-0.0164
		(0.0135)	(0.0147)	(0.0148)
Hours of daylight (0:00-8:00)		0.0441	1.226	1.223
		(0.222)	(1.637)	(1.642)
Hours of daylight (8:00-16:00)		-0.237	3.643	3.620
		(0.453)	(3.620)	(3.631)
Hours of daylight (16:00-24:00)		-0.0661	0.527	0.504
		(0.151)	(1.499)	(1.512)
Rayon FE	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes
Times-of-Day FE	yes	yes	yes	yes
Controls	no	yes	yes	yes
Week/Month FE	no	yes	yes	yes
Times-of-Day*Week FE	no	no	yes	yes
Rayon*DoW FE	no	no	no	yes
Log(PM2.5) marginal effects	0.509***	0.517**	0.491**	0.491**
	(0.149)	(0.214)	(0.222)	(0.228)

Notes: See Table 3 for table notes.

Table A3: Two-stage least squares estimates
Log(PM2.5) effect on major theft/fraud minus petty theft/fraud

	(1)	(2)	(3)	(4)
Log(PM2.5)	0.399***	0.346***	0.357**	0.355**
	(0.110)	(0.129)	(0.142)	(0.145)
Rayon FE	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes
Times-of-Day FE	yes	yes	yes	yes
Controls	no	yes	yes	yes
Date FE	no	yes	yes	yes
Times-of-Day*Week FE	no	no	yes	yes
Rayon*DoW FE	no	no	no	yes
First Stage F-Statistic	50.06	21.01	25.33	26.38
J-stat p-value	0.383	0.658	0.337	0.343
Observations	1702	1702	1702	1702

Notes: The baseline standard errors are clustered by day. We report the Montiel-Pflueger effective F-statistic for the first stage. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, barometric pressure, historical temperatures (averaged from 2012-19), and hours of daylight. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A4: Two-stage least squares estimates,
Mountain wind & inversion IVs,
Log(PM2.5) effect on total crime rate

	(1)	(2)	(3)	(4)
Log(PM2.5)	0.510***	0.427***	0.411**	0.411**
	(0.134)	(0.149)	(0.158)	(0.163)
Rayon FE	no	no	no	yes
Day-of-Week FE	yes	yes	yes	yes
Times-of-Day FE	yes	yes	yes	yes
Controls	yes	yes	yes	yes
Month/Week FE	no	yes	yes	yes
Times-of-Day*Week FE	no	yes	yes	yes
Rayon*DoW FE	no	no	yes	yes
Driscoll-Kraay SEs	(0.146)	(0.142)	(0.150)	(0.156)
First Stage F-Statistic	50.06	19.19	23.42	24.08
J-stat p-value	0.571	0.305	0.325	0.364
Observations	1702	1702	1702	1702

Notes: The baseline standard errors are clustered by day. We report the Montiel-Pflueger effective F-statistic for the first stage. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, barometric pressure, historical temperatures (averaged from 2012-19), and hours of daylight. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A5: Two-stage least squares estimates,
Mountain wind IV only,
Log(PM2.5) effect on total crime rate

	(1)	(2)	(3)	(4)
Log(PM2.5)	0.401**	0.358**	0.340**	0.346**
	(0.199)	(0.149)	(0.163)	(0.169)
Rayon FE	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes
Times-of-Day FE	yes	yes	yes	yes
Controls	no	yes	yes	yes
Week/Month FE	no	yes	yes	yes
Times-of-Day*Week FE	no	no	yes	yes
Rayon*DoW FE	no	no	no	yes
First Stage F-Statistic	45.82	28.37	33.15	34.99
Observations	1702	1702	1702	1702

Notes: The baseline standard errors are clustered by day. We report the Montiel-Pflueger effective F-statistic for the first stage. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A6: Robustness to alternative instrumental variable specifications, Log(PM2.5) effect on total crime rate

	(1) Binned Wind	(2) Above Average Wind	(3) Estimate All Wind Directions	(4) All Day Inversion
Log(PM2.5)	0.431**	0.388**	0.326**	0.428***
	(0.186)	(0.168)	(0.134)	(0.154)
Residuals	-0.461**	-0.418**	-0.358**	-0.464***
	(0.190)	(0.183)	(0.150)	(0.165)
Controls	yes	yes	yes	yes
Rayon FE	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes
Times-of-Day FE	yes	yes	yes	yes
Week FE	yes	yes	yes	yes
First Stage F-Statistic	12.10	17.87	12.69	25.56
Observations	1702	1702	1702	1702
Pseudo R-Squared	0.183	0.183	0.183	0.183

Notes: Columns headings summarize deviations from the standard wind and inversion IVs, using the same controls and fixed effects (those of Table 3 Column 2). Here Column 1 replaces the Mountain Wind IV with two equal-sized mountain bin indicator IVs. Column 2 uses as mountain wind instrument only the share of mountain winds in excess of the sample average mountain winds (roughly 10 percent). Column 3 uses as instruments four different wind directions (effects estimated relative to no wind). Column 4 uses as the inversion instrument an indicator for the temperature inversion lasting all day, rather than just the 8-hour period. Standard errors are derived by bootstrapping two-stage PPML CF coefficient estimates. Our bootstraps use 500 replications of resampled day clusters. We report the Montiel-Pflueger effective F-statistic for the first stage. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, barometric pressure, historical temperatures (averaged from 2012-19), and hours of daylight. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A7: Robustness to alternative control variables,
PPML control function approach,
Log(PM2.5) effect on total crime rate

	(1) 10-Bin All Covariates	(2) Unbin Temp.	(3) No Daylight	(4) No Controls	(5) More Controls	(6) Lead/Lag Pollution
Log(PM2.5)	0.324**	0.370**	0.371**	0.231*	0.359**	0.361**
	(0.137)	(0.157)	(0.158)	(0.129)	(0.154)	(0.153)
Residuals	-0.352**	-0.381**	-0.401**	-0.259*	-0.395**	-0.409**
	(0.140)	(0.168)	(0.171)	(0.136)	(0.168)	(0.174)
Controls	yes	yes	yes	yes	yes	yes
Rayon FE	yes	yes	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes	yes	yes
Times-of-Day FE	yes	yes	yes	yes	yes	yes
Week FE	yes	yes	yes	yes	yes	yes
First Stage F-Stat.	33.93	19.22	18.19	11.38	21.83	20.14
Observations	1702	1702	1702	1702	1702	1640
Pseudo R-Squared	0.193	0.181	0.183	0.180	0.183	0.183

Notes: This table considers alterations in controls while otherwise preserving the fixed effects and controls from Table 3, Column 2. 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. Columnn 3 no longer uses daylight also controls. 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. Standard errors are derived by bootstrapping two-stage PPML CF coefficient estimates. Our bootstraps use 500 replications of resampled day clusters. We report the Montiel-Pflueger effective F-statistic for the first stage. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, barometric pressure, historical temperatures (averaged from 2012-19), and hours of daylight. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A8: Robustness to alternative aggregations or subsamples, PPML control function approach, Log(PM2.5) effect on total crime rate

	(1) Aggregate to Rayon-Days	(2) Aggregate Rayons	(3) Drop March 2020
Log(PM2.5)	0.310**	0.520*	0.321**
	(0.145)	(0.283)	(0.148)
Residual	-0.321*	-0.491	-0.338**
	(0.165)	(0.305)	(0.171)
Controls	yes	yes	yes
Rayon FE	yes	yes	yes
Day-of-Week FE	yes	yes	yes
Times-of-Day FE	yes	yes	yes
Week/Month FE	yes	yes	yes
First Stage F-Statistic	21.57	16.87	14.98
Observations	547	609	1124
Pseudo R-Squared	0.142	0.147	0.149

Notes: Columns headings summarize specifications where variables are aggregated or subsetted, using similar controls and similar fixed effects as Table 3 Column 2. Here Column 1 collapses data by times-of-day, so each observation is a rayon-day. Column 2 collapses data by northern and southern rayons, so we estimate over 2 regions rather than 7 districts. Column 3 estimates without using any data from March 2020. Standard errors are derived by bootstrapping two-stage PPML CF coefficient estimates. Our bootstraps use 500 replications of resampled day clusters. We report the Montiel-Pflueger effective F-statistic for the first stage. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, barometric pressure, historical temperatures (averaged from 2012-19), and hours of daylight. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A9: Reduced-form effects of IVs and lagged IVs on total crime rate, PPML fixed effects estimates

	(1) 1 Period of Lagged IVs	(2) 2 Periods of Lagged IVs	(3) 3 Periods of Lagged IVs
Mountain wind	-0.461**	-0.443**	-0.459**
	(0.196)	(0.197)	(0.193)
Inversion	0.133**	0.126**	0.134**
	(0.0610)	(0.0617)	(0.0603)
Wind, sum of lags	0.0690	0.267	0.273
	(0.135)	(0.272)	(0.357)
Inversion, sum of lags	-0.0128	-0.0362	0.0468
	(0.072)	(0.104)	(0.121)
Controls	yes	yes	yes
Rayon FE	yes	yes	yes
Day-of-Week FE	yes	yes	yes
Times-of-Day FE	yes	yes	yes
Week FE	yes	yes	yes
Observations	1697	1697	1697
Pseudo R-Squared	0.183	0.183	0.184

Notes: Standard errors are clustered by day. We report the Montiel-Pflueger effective F-statistic for the first stage. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, barometric pressure, historical temperatures (averaged from 2012-19), and hours of daylight. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table A10: Placebo tests for irrelevant log(PM2.5) effect on crime rate,
PPML control function approach

	(1) -31 days	(2) -14 days	(3) -7 days	(4) +7 days	(5) +14 days	(6) +31 days
Log(PM2.5)	-0.111	-0.0589	-0.180	0.0542	0.0124	0.147
	(0.119)	(0.174)	(0.135)	(0.133)	(0.148)	(0.169)
Residuals	0.114	0.102	0.194	0.0232	-0.00326	-0.0367
	(0.134)	(0.175)	(0.140)	(0.148)	(0.161)	(0.170)
Controls	yes	yes	yes	yes	yes	yes
Rayon FE	yes	yes	yes	yes	yes	yes
Day-of-Week FE	yes	yes	yes	yes	yes	yes
Times-of-Day FE	yes	yes	yes	yes	yes	yes
Week FE	yes	yes	yes	yes	yes	yes
First Stage F-Statistic	16.30	21.53	20.52	24.88	23.63	20.65
Observations	1051	1372	1514	1514	1372	1051
Pseudo R-Squared	0.156	0.174	0.177	0.190	0.196	0.196

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. Standard errors are derived by bootstrapping two-stage PPML CF coefficient estimates. Our bootstraps use 500 replications of resampled day clusters. Controls include temperature bins (of width 5 degrees C), humidity, wind velocity, precipitation, barometric pressure, historical temperatures (averaged from 2012-19), and hours of daylight. We report the Montiel-Pflueger effective F-statistic for the first stage. The *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.