The Effect of Air Pollution on Criminal Activities: Evidence from the NO_x Budget Trading Program

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Abstract

This paper examines the impacts of air pollution on criminal activities by exploiting three dimensions of variations in a rich quasi-experiment: the NO_x Budget Trading Program. This program has been well documented to decrease air pollution concentrations in participating states. Employing a triple-difference estimator, we find robust evidence that the cap-and-trade market statistically significantly reduced violent and property crimes in participating states by about 3.7% and 2.9%, respectively. Instrumental variable estimates suggest that lowering air pollution may play an important role in reducing criminal behaviors.

Keywords: Air pollution, criminal activities, NO_x Budget Trading Program

JEL classification: K42, Q51, Q58

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1 Introduction

Previous studies have provided strong evidence that air pollution affects human well-being in many aspects—e.g., infant mortality (Chay and Greenstone 2003; Currie, Neidell et al. 2005); life expectancy (Chen et al. 2013); worker productivity (Zivin and Neidell 2012); academic performance (Ebenstein, Lavy and Roth 2016); and so forth. We explore a new dimension—how air pollution affects criminal activities—in this study. Epidemiological literature shows that poor air quality can cause people to behave aggressively due to anxiety, tension, anger, or depression, which suggests that air pollution may be associated with violent crimes (e.g., Rotton 1983).[1] In this study, we employ a well-known quasi-experiment—the NO_x Budget Trading Program (hereafter NBP)—to identify the causal effects of air pollution on criminal behaviors.

The NBP was a cap-and-trade system aimed at reducing ozone concentrations. It was initiated in 2003 and ended in 2008. As ozone concentrations are generally high in the summer, the NBP only operated from May to September.[2] Nineteen Eastern and Midwestern states, together with Washington, DC, were included in this program. Therefore, this quasi-experiment provides three dimensions of variations. The first is the difference in criminal activities between NBP and non-NBP states. The second difference arises from before versus after the market's initiation, and summer versus winter is the last dimension. Employing these three sources of variations, we use a triple-difference framework to examine the relationship between air pollution and criminal acts.

By compiling county-season-level crime data with air pollution and weather information, we find that the NBP market statistically significantly lowered violent crime rate in participating states by 3.7%. To put this figure into perspective, Chalfin and McCrary (2013) found that violent crimes decrease by around 0.4% once police officers increase by 1%. Based on our estimate, the effect of the NBP on violent crimes is equivalent to

[1] Possible mechanisms that explain the relationship between air pollution and criminal activities are detailed in Section 2.

[2] 2004 is an exception. The NBP operated from June to September in that year.

increasing the size of police forces by about 9% during the sample period. Specifically, rates for assault, robbery, rape, and murder statistically significantly decreased by 2.5%, 7.9%, 6.6%, and 6.8% respectively. The NBP's effects on property crimes are slightly smaller. Specifically, the NBP market statistically significantly reduced property crime rate in participating states by 2.9%. Rates for larceny and burglary fell by 3.3% and 2.9%, respectively. But the NBP's effect on motor vehicle theft is not statistically significant at the traditional level.

To validate the assumption of the triple-difference estimator, we use three methods to examine the presence of pre-existing trends. First, we plot the impacts of the NBP on criminal activities across years. These event-time graphs show that before the market's initiation, there were no meaningful differences in the trend in summertime criminal activities between participating and non-participating states. As the event-time-study method requires large samples to get the precisely estimated effect for each year, one concern is that the statistically insignificant differences in the 1998-2002 period are due to the lack of statistical power. To address this concern, we conduct another pre-existing trend test. Instead of separately estimating the coefficient for each year, we allow NBP and non-NBP states to have their own linear trends. Again, we find no clear pre-existing trends in our triple-difference setting. Lastly, we conduct a falsification test by using the sample period from 1998 to 2002 and setting 2000 as the start time of the placebo NBP. The results pass the falsification test.

Next, we employ an IV approach to examine the effects of air pollution, measured by air quality index, on criminal activities. The IV estimates suggest that air pollution significantly affects violent and property criminal behaviors. Compared to the IV estimates, the fixed-effect estimates show little evidence that air pollution has a strong relationship with criminal activities. It indicates that our IV approach may correct omitted variable and attenuation bias suffered by the fixed-effect estimates. Additionally, we show that the NBP has no impact on unemployment rate, worker earnings, or electricity demand, validating the exclusion restriction assumption of the IV approach.

We conduct a series of heterogeneous and robustness tests. First, we examine the heterogeneous effect of the NBP by NO_x emissions before the NBP. We find that the NBP effects on crimes are mainly driven by counties with high NO_x emission level. By contrast, the effects are much smaller and less statistically significant for counties with low NO_x emission level. Second, we test multiple hypotheses simultaneously to make our inferences more conservative. The inferences of our main results remain stable. Lastly, we investigate whether our results are robust to exclude the adjacent states by randomly assigning them to treatment or control groups. The results are qualitatively and quantitatively similar to those in the main analysis.

Our study contributes to the environment economics literature in several ways. First, this paper, along with two concurrent studies by Herrnstadt et al. (2016) and Bondy, Roth and Sager (2019), identifies the causal relationship between air pollution and criminal activities.[3] Estimating this relationship is challenging, because it is confounding from economic activities that may bias standard estimates. For perspective, local economic activities not only affect criminal acts (e.g., Raphael and Winter-Ebmer 2001; Gould, Weinberg and Mustard 2002), but are also related to air pollution concentrations. Another challenge is that measurement errors in assigning pollution monitors to counties shrink estimates towards zero. Without considering the endogeneity problem of air pollution, our fixed-effect estimates indicate that air pollution does not drive any criminal behaviors—but the instrumental variable estimates demonstrate that air pollution is indeed a determinant of crimes.

Previous psychological studies have also examined the association between air pollution and criminal activities. Strahilevitz, Strahilevitz and Miller (1979) found that psychiatric disturbances increased as air pollution levels went up. Rotton and Frey (1985), using archival data for Dayton, Ohio, documented that family disturbances and assaults were

[3]We acknowledge that we cannot pin down the exact air pollutants related to crimes, as the NBP also reduced other air pollutants besides NO_x and ozone. Therefore, we conclude that the effects of NBP on criminal activities are through lowering "general" air pollution.

affected by ozone, smoke, and meteorological factors. On the other hand, using cross-sectional data, Lave and Seskin (1978) did not find any significant relationships between outdoor air pollution and crimes in the U.S.—e.g., rapes, robberies, assaults, burglaries, and auto thefts. However, these studies either are based on cross-sectional data or employ a small set of controls. As a result, they have limited ability to address endogeneity issues.

Second, we exploit the seasonal variations of crime and show the relatively long-term effects of air pollution on criminal activities. To compare, Herrnstadt et al. (2016) exploited daily variations in air pollution and violent crimes in Los Angeles and Chicago and found that air pollution statistically significantly increased violent crimes. Bondy, Roth and Sager (2019) employed daily administrative data in London and found that elevated levels of air pollution have a positive and statistically significant impact on overall crime. Jacob, Lefgren and Moretti (2007), in contrast, found that crime rates were negative serial correlated over a span of weeks. Therefore, the daily link between air pollution and criminal activities may not be capable of reflecting the long-term effects (Ranson 2014). Last, our findings provide evidence of the potential benefits of air pollution abatement, and thus have important policy implications.

The rest of the paper is organized as follows. The second section describes mechanisms and the NBP. Section 3 introduces the empirical framework. Section 4 summarizes the data sources and presents the descriptive analysis. The main findings and sensitivity analysis are presented in Section 5, and Section 6 presents implications of our findings and concludes.

2 Mechanisms and the NBP Market

2.1 Mechanisms

In this section, we summarize the potential mechanisms proposed in psychological, biological, and economics literature that may support the relationship between air pollution and criminal activities.

First, based on laboratory experiments, researchers have found that a number of negative psychological symptoms are associated with air pollution—e.g., anxiety, tension, anger, and depression (for instance, Evans et al. 1987; Zeidner and Shechter 1988). These symptoms may directly influence human judgment and may be reflected explicitly as human aggression. In a laboratory study by Rotton et al. (1979), individuals who were exposed to unpleasant odors delivered higher levels electric shocks, on average, to their confederates as punishment for making errors on a learning task, compared to their counterparts under clean air. With regard to ozone pollution, Petruzzi et al. (1995) found that continuous exposure to ozone markedly influenced a number of items of aggressive behavior for adult mice. More recently, scientists provided evidence that ozone pollution reduces serotonin in the brains, which considered a natural mood stabilizer (Murphy et al. 2013). It reduces depression and regulates anxiety. Cases et al. (1995) showed that decreased serotonin is associated with aggressive behaviors.

Second, the respiratory system is well documented to be directly affected by air pollution. For instance, oxidative stress responses have been consistently observed when people were exposed to ozone pollution (Chuang et al. 2007; Corradi et al. 2002; Valavanidis et al. 2013). In addition, air pollution is linked with neuro-inflammation (Block and Calderón-Garcidueñas 2009; Levesque et al. 2011). Both oxidative stress and neuro-inflammation can cause anxiety and have possible links with aggressive behaviors (Rammal et al. 2008).

Third, as argued by Ranson (2014), environmental factors may play a role in Becker's (1968) production function for crime. In the canonical model of crime, Becker suggested that implementation of criminal activities is based on the benefits and costs. Air quality conditions may change the benefits and costs. For perspective, when air pollution is high, police officers may have lower "productivity", e.g., spending more time on staying indoors instead of on patrolling, thereby increasing the probability of successfully committing a crime and escaping undetected (Zivin and Neidell 2012).

To summarize, the first two strands of literature indicate that air pollution may increase violent, but not property, crimes. The third explanation suggests that police officers may be less productive under poor air quality, in which case both violent and property crimes would increase. According to the possible mechanisms, in this study we expect that the NBP reduces violent crimes; property crimes may be affected as well.

2.2 The NBP Emission Market

The NBP was a U.S. emission market that limited NO_x emissions in eastern states. It originated from the Ozone Transport Commission (OTC), an organization of Northeast States formed in the 1990s. OTC (1998) found that the Northeast regions had high ozone levels partly because winds transported NO_x from the industrial Midwest to the Northeast, where it produced ozone. Thus OTC initiated an early version of the NBP that operated in 1999-2002 and decreased summer NO_x emissions slightly.[4] Then the OTC implemented a more stringent version of the NBP which began in 2003 and operated until 2008.[5] The emission market only operated from May to September since ozone concentrations are normally high in the summer and low in the winter.[6] According to the U.S. Environmental Protection Agency (USEPA 2009), 2,500 electricity generating units and industrial boilers were enrolled in this cap-and-trade market. Among them, 700 coal-fired plants produced around 95% of the NOx emissions on the market.

The emission market included eight northeastern states in addition to Washington,

[4]The early version of the NBP operated in May-September. OTC (2003) documented that the program reduced summer NO_x emissions by 76,000 tons. By contrast, NO_x emissions fell by 504,000 tons between 2002 and 2005, about 6.6 times more than the early version market (Deschênes, Greenstone and Shapiro 2017). Theoretically, this early version market could be a source of confounding variation for the crime regressions which started in 1998. But the crime regressions beginning in 2000 or 2001 display similar signs and significance (see Figures A.1 and A.2). Additionally, Table A.1 shows that after excluding participating states in the early version program, the estimates remain stable relative to those in our main results.

[5]In 2009, the Clean Air Interstate Rule (CAIR) replaced the NBP. In 2010, the EPA created a Transport Rule, combining the NO_x emission market with a market for SO_2 emissions. In July 2011, the EPA replaced this market with the Cross-State Air Pollution Rule, regulating power plant emissions in 27 states to reduce ozone and particulate levels.

[6]In 2004, the NBP operated from June to September.

D.C.[7] In 2004, another 11 states joined the program.[8] Figure 1 displays the division of states by NBP participation status.

The EPA allocated about 150,000 tons of NO_x allowances in 2003, 650,000 tons in 2004, and about 550,000 tons in each of the years 2005-2008. During the period, each state received a set of permits and decided on how to distribute those permits to regulated sources. Once permits were distributed to the sources, they could trade them through open markets. Regulated sources are allowed to bank allowances for future years. USEPA (2009) reported that in each year of the NBP market, about 250,000 tons of allowances were saved unused for subsequent years. Each source has to give the EPA one allowance for each ton of NO_x emitted at the end of each regulated season.

3 Empirical framework

As discussed above, the quasi-experiment—the NBP—provides three dimensions of variations in pollution emissions and criminal activities. Specifically, the first is to contrast the periods before and after the program's operation. The NBP started in 2003 and covered eight states and Washington, DC. Another 11 states joined in 2004. Participating versus non-participating states is the second dimension, and the third dimension is the NBP's operating season, i.e., from May 1 to September 31.

3.1 Main specification

To isolate the causal effects of the emission market on criminal activities, we employ the triple-difference (DDD) specification, similar to that of Deschênes, Greenstone and

[7]The eight states are Connecticut, Delaware, Maryland, Massachusetts, New Jersey, New York, Pennsylvania, and Rhode Island.

[8]The 11 states are Alabama, Illinois, Indiana, Kentucky, Michigan, North Carolina, Ohio, South Carolina, Tennessee, Virginia, and West Virginia. Only a few counties in Alabama and Michigan entered the market. Also, one region in Missouri participated in 2007.

Shapiro (2017). In particular:

$$Y_{ist} = \beta \mathbf{1}(DDD)_{ist} + W_{ist}^{'}\gamma + \mu_{it} + \lambda_{st} + \eta_{is} + \varepsilon_{ist}, \tag{1}$$

where i, s, and t denote county, season, and year, respectively. Two seasons—summer and winter—constitute a year.[9] The NBP only operated in summer, during which ozone pollution generally remains high. The dependent variables, Y_{ist}, are the log of total number of criminal activities per 1,000 people in each county-year-season cell, including assaults, robberies, rapes, murders, larcenies, burglaries, and motor vehicle thefts.[10] The variable of interest, $\mathbf{1}(DDD)_{ist}$, is defined as follows: When a state participated in the NBP in 2003 (or 2004), we set $\mathbf{1}(DDD)_{ist} = 1$ for all counties in that state in summertime in 2003 (or 2004) through 2008.

As meteorological factors are correlated with criminal activities, we should add them to our regressions (Ranson 2014). Following Ranson (2014), we use 11 bin indicators to model the daily distribution of average temperatures within a county-season-year cell: $(-\infty, 10°F]$, $(10, 20°F]$, $(20, 30°F]$, $(30, 40°F]$, $(40, 50°F]$, $(50, 60°F]$, $(60, 70°F]$, $(70, 80°F]$, $(80, 90°F]$, $(90, 100°F]$, and $(100, +\infty°F)$. Precipitation is divided into four categories: 0mm, $(0, 5mm]$, $(5, 15mm]$, and $(15, +\infty mm)$. Dew point temperature covers nine groups: $(-\infty, 10°F]$, $(10, 20°F]$, $(20, 30°F]$, $(30, 40°F]$, $(40, 50°F]$, $(50, 60°F]$, $(60, 70°F]$, $(70, 80°F]$, and $(80, +\infty°F)$.

Three sets of two-way fixed effects are further added to the main specification, i.e., county-year (μ_{it}), season-year (λ_{st}), and county-season fixed effects (η_{is}). First, county-year fixed effects capture nonlinear changes in the determinants of criminal activities within a county-year cell, e.g., the local unemployment rate and police officer recruitment. Second, by controlling year-by-season fixed effects, we partial out common shocks across

[9] In 2004, summertime is from June to September. In other years, summertime is from May to September.

[10] There are county-year-season cells in which some specific type crimes are zero. To account for that, we use a transformation with a logarithm of the number of crimes plus one.

season by year, e.g., summer vacation and the Christmas holiday (McDowall, Loftin and Pate 2012; Miron 1996). Third, county-specific seasonality patterns of criminal behaviors are controlled by county-season fixed effects. ε_{ist} denotes an idiosyncratic random error term. To allow for potential temporal and spatial autocorrelations, standard errors are clustered at the state and year level (two-way clustering).

3.2 Identification assumption

The validity of this triple-difference estimator relies on the parallel trend assumption. In this context, it requires that without the policy intervention, NBP and non-NBP states have the same trends on seasonal differences of criminal activities. The following specification, which was also applied by Deschênes, Greenstone and Shapiro (2017), tests this assumption:

$$Y_{ist} = \sum_{t=1998}^{2008} \beta_t \mathbf{1}(NBP\ State\ and\ Summer)_{is} + W_{ist}^{'}\gamma + \mu_{it} + \lambda_{st} + \eta_{is} + \varepsilon_{ist}. \qquad (2)$$

Here, for all summer-NBP observations for all years, $\mathbf{1}(NBP\ State\ and\ Summer)_{is} = 1$. Other notations are the same as those in Equation (1). In practice, we set 2002 as the omitted group, i.e., $\beta_{2002} = 0$. If coefficients for 1998 through 2001 are not statistically significantly different from zero, this may imply that there is no evidence of clear differences in the trend of criminal acts in summertime between NBP and non-NBP states before 2003. In addition to assessing the common trend assumption, this specification can estimate the policy effect for each year after the market's operation as well.

One potential flaw in this method is that it requires large samples to get the precisely estimated effect for each year; Otherwise, insignificant effects may be due to the lack of statistical power. To overcome this problem, we employ the following specification to double check the common trend assumption:

$$\Delta Y_{it} = \rho_1 \mathbf{1}(NBP)_i * t + \sigma_i + \tau_t + \xi_{it}, \tag{3}$$

where ΔY_{it} represents the differences in criminal activities between summer and winter within a county-year cell. $\mathbf{1}(NBP)_i * t$ is the NBP county dummy times linear time trend. County and year fixed effects are denoted by σ_i and τ_t, respectively. Using the data from 1998 to 2002, we test whether there is a significantly different pre-trend between the NBP and non-NBP states. The null hypothesis is $\rho_1 = 0$. Standard errors are clustered at the state level.

Finally, to take care of the small-sample problem discussed above we also conduct a falsification test by using the sample period from 1998 to 2002 and setting 2000 as the start time of the placebo NBP. Hypothetically, the effect of the placebo NBP has no impact on criminal activities.

3.3 Instrumental variable estimation

Next, we use *DDD* in Equation (1) as an instrument variable to measure the effects of air pollution on criminal behaviors. As the NBP reduced multiple air pollutants (e.g., NO_x, ozone, $PM_{2.5}$), the exclusion restriction assumption will not hold if we use one single pollutant as the endogenous variable. Instead, we use air quality index (hereafter, AQI) to measure the combined effects of all the air pollutants affected by the NBP. The causal interpretation of the IV estimates is straightforward: as the overall air pollution measure, the estimated effects on criminal activities should result from reductions in AQI caused by the NBP. Specifications for the two-stage-least-square estimation are as follow:

$$\textbf{First stage}: AQI_{ist} = \beta \mathbf{1}(DDD)_{ist} + W_{ist}^{'}\gamma + \mu_{it} + \lambda_{st} + \eta_{is} + \varepsilon_{ist}, \tag{4}$$

$$\textbf{Second stage}: Y_{ist} = \alpha \widehat{AQI_{ist}} + W_{ist}^{'}\gamma + \mu_{it} + \lambda_{st} + \eta_{is} + \varepsilon_{ist}, \tag{5}$$

where $\widehat{AQI_{ist}}$ is the predicted AQI within a county-year-season cell from the first stage. Other notations are the same as those in Equation (1).

The IV estimation requires the validity of the exclusion restriction assumption, i.e., $\mathbf{1}(DDD)_{ist}$ affects criminal activities only through air pollution measured by AQI. One concern about the validity of the exclusion restriction assumption is that local economic conditions may also be affected by the NBP market. For instance, Curtis (2017) found that hiring rates in the manufacturing sector decreased as a result of the NBP market. However, the effects documented by Curtis (2017) are annual-based. As firms in the participating states realized that the policy would be executed in every summer, they may hire fewer workers not only in summertime but also in wintertime. Therefore, the county-year fixed effects might capture the unemployment changes caused by the NBP. Additionally, in the results section we formally test whether the NBP affected economic activities by examining the effects on unemployment rate, earnings, and electricity consumption. The results show that none of the outcomes were affected by the NBP.

4 Data and descriptive analysis

4.1 Data sources

To assess the impacts of air pollution on criminal activities, we compile a rich set of data on crime, pollution emissions, and meteorology for the period 1998-2008.[11]

Crime data. Crime data are extracted from the Uniform Crime Reporting (UCR, hereafter) Program operated by the US Federal Bureau of Investigation (FBI). Based on around 17,000 local enforcement agencies' monthly reports, the data cover about 3,000 counties in the 49 continental states, representing 97.4% of the US population (Federal Bureau of Investigation 2011). Data for criminal activities are submitted voluntarily by

[11] Our conclusions are not sensitive to the choice of the start year of the sample period (see Figures A.1 and A.2).

city, county, and state law enforcement agencies. The FBI is responsible for checking the completeness and accuracy of the reports. If the FBI detects an unusual fluctuation in an agency's criminal activities, it will contact the local enforcement agency to explain or correct the figures. Therefore, the UCR data should contain few errors.

Monthly reports typically include the number of two types of reported offenses—violent and property crime. Specifically, the violent crime includes assaults, robberies, rapes, and murders. Larcenies, burglaries, and motor thefts constitute the property crime.

In the dataset, a number of counties report criminal activities only once, twice, or four times a year.[12] To maintain our sample balance, these observations are deleted. What's more, in some cases, agencies provide only the total number of violent and property crimes instead of the number for each type of offense. We drop these cases as well. Furthermore, we eliminate all records in which total criminal activities is zero for every month within a year.[13] Following Ranson (2014), we drop all county-year cells with a population of fewer than 1,000 people.

Air quality index. Air quality index data are obtained from the EPA's detailed Air Quality System. It is a composite measure of air pollution that ranks air quality based on the associated health risks as a means to facilitate comprehensibility by the public. Air quality index is computed based on six main pollutants—*ozone*, NO_2, SO_2, $PM_{2.5}$, PM_{10}, and CO. The pollutant that has the highest index is referred to as the primary pollutant and determines the AQI of the day. To ensure the accuracy of the AQI readings, we assign the closest pollution monitor with distances to one county's centroid of less than 100km. Counties without a pollution monitor within 100km are excluded in the analysis.

Pollution emissions. Pollution emissions data are obtained from the EPA's Clean Air Markets Division. Firms in the NBP report pollution emissions only during the summer, i.e., from May 1 to September 30. Such data availability constrains us from comparing summer versus winter. However, another pollution reduction program, called the Acid

[12]Most cases are from counties in Alabama and Florida.
[13]Most of these cases are in Illinois counties.

Rain Program (ARP), provides firms emissions data for the entire year. Almost all the firms enrolled in the NBP are also in the ARP; it provides total daily emissions of NO_x for 1,734 firms in 645 counties in 49 states. USEPA (2009) documents that in 2008, 97% emissions came from firms with continuous emissions monitoring systems. Therefore, for the other 652(=1297-645) counties without monitored firms, we impute their pollution emissions as zero. As those firms are enrolled in the cap-and-trade market and monitored by the EPA, the measurements on pollution emissions are supposed to have few errors (Deschenes, Greenstone and Shapiro 2017).

Weather data. Weather data are provided by the National Oceanic and Atmospheric Administration (NOAA) and include 1,761 different weather stations across the U.S. The weather variables include daily average temperature, total daily precipitation, and dew point temperature. To ensure the accuracy of weather readings, we select all weather stations that are less than 50 km from the county's centroid to construct the meteorological variables, using a inverse-distance-weighted average. The use of alternative distance thresholds, such as 100 km and 150 km, does not change our main results.[14]

In the analysis, we exclude non-continental states—i.e., Alaska and Hawaii—and Puerto Rico. As Deschênes, Greenstone and Shapiro (2017) argued, states adjacent to NBP states were also likely to benefit from pollution reduction, given that air pollution can cross state borders. These states—Georgia, Iowa, Maine, Mississippi, Missouri, New Hampshire, Vermont, and Wisconsin—are also excluded.[15] Moreover, only certain counties in Michigan participated the NBP, and we do not include it. Sample statistics are summarized in Table 1. The final sample contains 1,297 counties in 37 states.

[14]Due to limited space, results are not included but are available on request.

[15]In sensitivity analysis, we include these states as the control or treatment group. Our results remain stable (see Tables A.2 and A.3).

4.2 Descriptive analysis

To estimate the relationship between pollution emissions and criminal activities, we begin with a preliminary analysis in which AQI is treated as exogenous. In other words, we estimate Equation (4) directly by a fixed-effect model instead of instrumenting AQI with an exogenous policy change. The specification is as follows:

$$Y_{ist} = \alpha AQI_{ist} + W_{ist}^{'}\gamma + \mu_{it} + \lambda_{st} + \eta_{is} + \varepsilon_{ist}, \tag{6}$$

where AQI_{ist} denotes the average of daily AQI in each county-year-season cell. Other notations are the same as those in Equation (4).

Table 2 statistically reports fixed-effect estimates of the effects of AQI on criminal activities. Columns (1) to (5) in Table 2 show that after partialling out county-by-season, season-by-year, and county-by-year fixed effects and flexible meteorological factors, the effects of AQI on violent, assault, and rape crimes are statistically significant, but their magnitudes are small. For instance, a 10 unit rise in AQI increase violent crimes by 1%. The effects on robberies and murders are not statistically or economically significant. Columns (6) through (9) in Table 2 present estimates on property crimes. The effects on all the property crimes are not statistically significantly different from zero, and the magnitude is limited.

To summarize, the fixed-effect estimates provide little evidence that air pollution has a strong relationship with criminal activities. However, these estimates could suffer from omitted variable bias, even though we have controlled three sets of two-way fixed effects. In Equation (6), after partialling out county-by-season, season-by-year, and county-by-year fixed effects, variations are at the county-year-season level. At this variation level, local economic conditions may still be an important omitted variable that is closely related to both criminal activities and pollution emissions; it may bias our estimates towards zero. Another possibility is that measurement errors in assigning pollution monitors to counties shrink the fixed-effect estimates towards zero. Therefore, to correct the bias we employ an exogenous policy change—the NBP—that is supposed to be unrelated to local economic conditions.[16] We expect that after correcting the omitted-variable and attenuation bias by using IV methods, the coefficients of AQI would become larger.

5 Main results

This section first summarizes estimates of the reduced-form effects of the NBP on violent and property crimes. Next, we employ three methods to check the validity of the identification assumption for this triple-difference setting. Then, using an IV approach we measure the effects of air pollution measured by AQI on criminal activities and compare them to fixed-effect estimates. Finally, we present several heterogeneous and sensitivity analyses.[17]

5.1 Violent crimes

Panel (A) in Table 3 statistically summarizes the effect of the NBP on total violent crimes. In column (1), we control county-by-season, season-by-year, and state-by-year fixed effects at first. The coefficient is statistically significant at the 10% level. As discussed before, meteorological factors are correlated with both air pollution concentrations and criminal activities; therefore, we add flexible weather controls in the second column. As can be seen, the coefficient becomes statistically significant at the 1% level. Column (3) is the richest specification, in which state-by-year fixed effects are replaced with county-by-year fixed effects. We notice that compared to that in column (2), the estimate in column (3) changes only slightly. The coefficient indicates that the NBP market statistically significantly decreased the violent crime rate in NBP counties by 3.7%.

[16]The validity of the instrumental variables estimates is discussed in the empirical framework section.

[17]As Deschênes, Greenstone and Shapiro (2017) have proved that the NBP significantly decreased NO_x emissions and other pollution concentrations, we do not emphasize these results in this study.

Panel (a) of Figure 2 performs an event-time study for log of the violent crime rate. As Equation (2) shows, the corresponding graph displays the changes of the coefficient on the variable $\mathbf{1}(NBP\ State\ and\ Summer)_{is}$ across years. We set 2002 as the omitted group and normalize the coefficient for 2002 to be zero. The figure shows that before the market's initiation the coefficients are statistically insignificant, which indicates that there are no clear differences in the trend in summertime violent crime rates between NBP and non-NBP states. More importantly, after the start of the program, coefficients gradually decrease, indicating that our regression estimates are not driven by some specific years. Among them, the coefficients in 2005 and 2007-2008 are statistically significant at the 10% level. Compared to Equation (1), Equation (2) has more coefficients to estimate. It is not surprising that the coefficients in some years are not precisely estimated. But the trend of policy effects derived from Figure 2 is informative.

Assault. Panel (B) of Table 3 reports the estimates on the effect of the NBP on the assault rate. Column (3), the most stringent specification, shows that the assault rate fell by 3.2%. The estimate is statistically significant at the 10% level. Panel (b) of Figure 2 displays the event study graph for the assault rate. For the period 1998-2002, the coefficients are not statistically significantly different from zero, suggesting that there may not be a different trend for the assault rate in summertime between the NBP and non-NBP states. Similar to that in panel (a) of Figure 2, the effects on the assault rate for the period 2004-2008 are consistently negative.

Robbery. Panel (C) of Table 3 reports the NBP's effects on robbery rates. As can be seen, the cap-and-trade market has a statistically significantly negative effect on the robbery rate. Column (3) indicates that the NBP reduced the robbery rate by 7.4% (significant at the 1% level). Panel (c) of Figure 2 displays a clear pattern for the NBP's effect on the robbery rate across years. Before 2003, the coefficients for 1998 through 2002 are around zero and far from statistically significant at the traditional level, which implies that the parallel assumption holds. Since 2003, most coefficients are consistently negative.

Rape. Panel (D) of Table 3 examines the impact of the NBP market on rapes. Columns

(1) through (3) shows that the NBP market decreased the rape rate significantly. Specifically, the most stringent specification, in column (3), indicates that the rape rate fell by 6.5%. The estimate is statistically significant at the 5% level. The event-time study for log of the rape rate is displayed in panel (d) of Figure 2. Overall, the rape rate seems decreases after 2003.

Murder. The effect of the cap-and-trade market on murders is presented in panel (E) of Table 3. The coefficients are negative and relatively stable across specifications, and they are statistically significant at the 1% level. Column (3) demonstrates that the murder rate fell by 6.8%. Correspondingly, as can be seen in panel (e) of Figure 2, there is a persistent effect across years after 2003.

We summarize the above regression results as follows. We find that the NBP market reduced violent crimes. The overall effect is about 3.7%. Specifically, the effects on assault, rape, robbery, and murder are 3.2%, 7.4%, and 6.5%, and 6.8%, respectively. Employing event time studies, we do not find any evidence showing meaningful differences in the trend in summertime violent crimes between participating and non-participating states before 2003. One point worth mentioning is that we cannot pin down the exact air pollutants related to crimes, as the NBP also reduced other air pollutants besides NO_x and ozone. Therefore, we treat our estimates as "general" air pollution effects, similar to that in Bondy, Roth and Sager (2019).

5.2 Property crimes

The NBP market's effect on the property crime rate is shown in panel (A) of Table 4. Columns (1) through (3) replicates the specifications in Table 3. The coefficient in column (3) indicates that the NBP's impact on property crimes is statistically significant at the 10% level. Panel (a) of Figure 3 presents the impact of the NBP market on property crimes across years. Before 2003, the coefficients are not statistically significant at the conventional level.[18] The pattern indicates that common trend assumption for property crime rate holds. After the market's initiation, the coefficients gradually decrease. Importantly, after 2003 the coefficients depict a clear downward-sloping trend, suggesting that the property crime rate declined by the NBP.

Larceny. The influence of the NBP market on the larceny rate is shown in panel (B) of Table 4. Columns (1) through (3) indicate that the NBP imposed a statistically significant impact on the larceny rate. Column (3) shows that the assault rate fell by 3.3%. The event time study for log of the larceny rate is exhibited in panel (b) of Figure 3. During the period 1998-2002, the coefficients are close to zero, indicating no meaningful differences in the trend in summertime between the NBP and non-NBP states. As can be seen, most coefficients in 2003 through 2008 remain consistently negative.

Burglary. The NBP market's effect on the burglary rate is shown in panel (C) of Table 4. The coefficient in column (3) indicates that the NBP's impact on burglaries was non-negligible—a reduction of 2.9 burglaries per 1,000 people—and the estimate is statistically significant at the 5% level. Panel (c) of Figure 3 presents the impact of the NBP market on burglaries across years. As can be seen, there is no meaningful trend differences between participating and non-participating states. After the market's initiation, most coefficients are statistically significantly negative.

Motor vehicle theft. Panel (D) of Table 4 reports reduced-form effects of the NBP market on motor vehicle thefts. Column (3) shows that the NBP's effect on motor vehicle theft rate is not statistically significantly different from zero. We notice that all coefficients are statistically insignificant across specifications, and magnitudes are not stable. Panel (d) of Figure 3 exhibits the impact of the NBP market on motor vehicle thefts across years. Similar to the patterns for other property crimes, the pattern for motor vehicle thefts shows that in advance of the NBP's initiation, we cannot reject the parallel trend assumption. After 2003, the coefficients fluctuate around zero. There is no strong evidence that the motor vehicle theft rate fell.

[18] The coefficient for 2000 is almost significant at the 5% level.

To summarize, in this section we find that the effects of the NBP market on property crimes are non-negligible and statistically significant, except for motor vehicle thefts. The results suggest that the mechanism—air pollution may increase the probability of successfully committing a crime and escaping undetected—we outline in the second section may be an important one. But compared to violent crimes, property crimes are less affected by the NBP. We propose one possibility that may explain this finding. According to the hierarchical classification approach adopted by the FBI's UCR Program, violent crimes have a higher hierarchy than property crimes. The hierarchical rule requires scoring of only the highest-level offense and ignoring all others in the incident. Consequently, reports for property crimes are biased downwards as they are assigned as a low hierarchy.[19] As the NBP reduces violent crimes in our settings, property crimes would be less likely to be accompanied by violent crimes. Thus it may result in an increase in property crime reports. Such an effect could potentially offset a decrease in property crimes due to the direct effect of the emission market. Therefore, given the nature of the UCR program, our estimated effect of the NBP on property crimes is a lower bound.

5.3 Validity of the identification assumption

Validity of the triple-difference estimator requires that in the absence of the NBP, the difference in criminal activities between the treatment—NBP states in summertime—and the control group is constant over time. We first employ Equation (2)—the event time study—to check trends both in advance of and after the market's initiation. As we find in the preceding subsections, Figures 2 and 3 indicate the absence of more than a slight pre-existing trend between NBP and non-NBP states before the market's initiation. However, the event-time-study method requires large enough samples to get precisely estimated effects for each year. In other words, the statistically insignificant coefficients in the 1998-2002 period may be due to the lack of statistical power.

[19]For instance, if a burglar breaks into a house, steals several items, and hurts the homeowner, the incident is classified as violent crime, although it also includes property crime.

Next, we provide another pre-existing trend test using Equation (3). In this model, instead of separately estimating the coefficient for each year, we combine data from 1998 to 2002 and test whether there is a significantly different linear trend between the NBP and non-NBP states before 2003. Again, the null hypothesis is $\rho_1 = 0$. As Table 5 shows, all estimates on the pre-existing differences between NBP and non-NBP states before 2003 are far from statistically significant, consistent with the event time studies. We conclude that little pre-existing trend presents in our triple-difference setting.

Finally, we conduct a falsification test by using the sample period from 1998 to 2002 and setting 2000 as the start time of the placebo NBP. Table A.4 presents the estimates. As can be seen, the coefficients are not statistically significant, and their signs do not display a consistent pattern. Therefore, the results provide a reassuring falsification test.

5.4 IV estimates

In this part, we first provide the first-stage results by estimating the NBP's effects on air pollution measured by AQI. Next, we employ a two-stage-least-square estimation to measure the effects of AQI on criminal activities. Furthermore, we compare the IV results to fixed-effect estimates.

Table A.5 statistically reports the NBP's effect on AQI. Compared to the average AQI in NBP states in advance of the market's initiation, AQI statistically significantly fell by about 7.6%. The magnitude is similar to that of Deschênes, Greenstone and Shapiro (2017).

The effects of AQI on violent crimes are shown in columns (1)-(5) in Table 6. The results indicate an evident positive association between air pollution and violent crimes; the effects on assaults, robberies, rapes, and murders are statistically significant at least at the 10% level. A 1-unit reduction in AQI lowers assault, rape, robbery, and murder rates by 0.7%, 1.7%, 1.5%, and 1.6% respectively. Columns (6)-(9) in Table 6 present the estimates on property criminal activities. Overall, the coefficients are smaller than those for violent crimes. A 1-unit reduction in AQI lowers larceny rates by 0.8%. Although the effects on burglary and motor vehicle theft rates are not statistically significant, their signs are positive. The findings, therefore, suggest that air pollution affects violent and property criminal behaviors.

Next, we compare the IV estimates to the fixed-effect estimates, which are shown in Table 2. The fixed-effect estimates provide little evidence that air pollution has a strong relationship with criminal activities. As we discussed before, this is possibly because the fixed-effect estimates may suffer from omitted variable and attenuation bias. As can be seen in IV estimates, the effects of air pollution on violent and property crimes become larger.

The IV estimation requires the validity of the exclusion restriction assumption, i.e., $\mathbf{1}(DDD)_{ist}$ affects criminal activities only through air pollution measured by AQI. One concern about the validity of the exclusion restriction assumption is that local economic conditions may also be affected by the NBP market. To test whether the NBP affected economic activities, we examine the effects of the NBP on unemployment rate, earnings, and electricity consumption.

First, we obtain the unemployment data at state-year-month level from the Local Area Unemployment Statistics program at the U.S. Department of Labor. Unemployment data at county-year-month level are not publicly available. Given the nature of the unemployment data, we replace county-by-year and county-by-season with state-by-year and state-by-season fixed effects in Equation (1) to study the effect of the NBP on unemployment rate. The results are summarized in column (1) in Table 7. The coefficient of the three-way interaction is far from statistically significant. Moreover, compared to the baseline mean, the magnitude of the coefficient is small. The result indicates that the NBP did not affect unemployment rate.

Second, we examine the effect of the NBP on average worker earnings. We obtain worker earnings data from the Quarterly Workforce Indicators (QWI). The underlying microdata for the QWI is the Longitudinal Employer Household Dynamics (LEHD) program at the U.S. Census Bureau. The dataset provides state-quarter level average earnings for new hired workers as well as for all workers. One point worth mentioning is that in the second quarter May and June are in the treatment season, while April is in the non-treatment season. Considering the ambiguous treatment status, we exclude the second quarter across years from our analysis sample. Similar to column (1), we replace county-by-year and county-by-season with state-by-year and state-by-season fixed effects in our main specification to study the effect of the NBP on worker earnings. Columns (2) and (3) in Table 7 reports the effects of the NBP on average earnings for new hired workers and all workers, respectively. As can be seen, the coefficients are neither statistically nor economically significant. The results demonstrate that the NBP did not affect average worker earnings.

Lastly, we investigate the effect on electricity consumption. Our electricity consumption data are derived from the Energy Information Administration (EIA) form 923. The data contains electricity consumption in each year-month for 6,155 electricity generating units across the U.S. In the analysis, we replace county-by-year and county-by-season with plant-by-year and plant-by-season fixed effects in our main specification. Table 8 presents the estimates. The dependent variable is electricity consumption (in logarithm). The coefficient is far from statistically significant at the conventional level, indicating that the NBP did not affect electricity consumption.

To sum up, we do not find any evidence supporting that economic activities were affected by the NBP, validating our exclusion restriction assumption.

5.5 Heterogeneous and sensitivity analysis

Heterogeneous analysis by NO_x emission level. For counties with high NO_x emission level before the NBP, they experienced a larger decline in NO_x emission by the NBP (see Table A.6). Correspondingly, we would expect that for those counties, the NBP effects on crimes may be more salient as well. To examine the heterogeneous effect, we divide our regression sample into two subsamples by the median summertime NOx emission level in the period 1998-2002. For the 1,297 counties in our regression sample, the median summertime NOx emissions were 200 tons. Table 9 presents the results. By comparing the estimates in panels A and B, we find that the NBP effects on crimes mainly driven by counties with high NO_x emissions. By contrast, the effects are much smaller and less statistically significant for counties with low NO_x emissions.

Multiple hypotheses testing. The control of the increased type I error when testing multiple hypotheses simultaneously makes inferences more conservative. Given that we have a range of outcomes, it would be useful to recompute p-values for our core coefficients by accounting for multiple hypotheses tests. We employ the method proposed by Benjamini and Yekutieli (2001) to derive conservative p-values. The results are shown in Table 10. As can be seen, the effects on assault, robbery, rape, murder, larceny, and burglary rates remain statistically significant. On the other hand, the effects on motor vehicle thefts remain statistically insignificant. To sum up, after taking multiple hypotheses tests into account, our main inferences remain stable.

Alternative start year. In general, the triple-difference estimator requires two-period (two-year) observations before the policy's initiation, based on which we can check the parallel trend assumption. By adding extra period observations in advance of the policy's initiation, the coefficients are likely to be estimated more precisely. The main conclusions should not change, however, with the selection of pre-treatment sample periods. Figures A.1 and A.2 plot the coefficients of interest based on different sample periods for violent and property crimes, respectively. The graphs show that our main conclusions are not sensitive to the choice of sample period start year. Specifically, in the four distinct sample periods, the NBP's effects on rapes and robberies are significantly negative (at the 5% level); the coefficients for assaults are statistically significant at the 10% level, except for 2000; the coefficients for assaults are statistically significant at the 5% level, except for 2001; and the NBP's effects on property crimes, except for motor vehicle thefts, are statistically significant across most sample periods.

Adjacent states. In the main results, states adjacent to NBP states are excluded because the treatment status is unclear. As winds can blow air pollution far away, it is possible that air pollution concentrations in states adjacent to NBP states also decrease. To check whether our results are robust to this step, we first include these states as the control group. Table A.2 shows the corresponding results. In each column, county-by-season, season-by-year, county-by-year fixed effects, and flexible weather controls are added in regressions. By comparing these estimates to our main results, we find that our conclusions remain almost unchanged. Next, we designate these states adjacent to NBP states as the treatment group. The corresponding results are presented in Table A.3; again, coefficients change only slightly relative to our main results. To sum up, our main results are insensitive to such changes.

6 Concluding remarks and implications

This paper examines the causal effects of air pollution on criminal activities, employing a well-known quasi-experiment—the NO_x Budget Trading Program, which has been documented to dramatically reduce air pollution concentrations in participating states. Using a triple-difference method, we find that violent and property crimes in the participating states statistically significantly decreased by about 3.7% and 2.9%, respectively. Instrumental variable estimates suggest that air pollution (measured by AQI) are positively correlated with violent and property criminal behaviors, indicating that lowering air pollution may play an important role in reducing crimes. In comparison, fixed-effect estimates show that the effects are negligible.

We end by using our estimates to conduct a back-of-the-envelope calculation, with a view to drawing implications from these results. McCollister, French and Fang (2010) reported the potential social costs, both tangible and intangible, for each crime type in 2008 dollars. Specifically, the social costs for a case of assault, robbery, rape, murder, larceny, and burglary are \$66,888, \$42,310, \$240,776, \$9 million, \$3,532, and \$6,462, respectively.[20] According to our estimates, this cap-and-trade market decreased assaults, robberies, rapes, murders, larcenies, and burglaries in the Eastern U.S. by 10,968, 2,520, 493, 83, 18,813, and 5,193 cases per year, respectively. In total, the NBP saved around $1,809 million per year in societal costs.[21] Relative to the costs of the NBP ($400-700 million per year as estimated by Deschênes, Greenstone and Shapiro (2017)), these benefits were substantial.

[20] The social costs for assaults are the mean of that for aggravated and simple assaults. Following Ranson (2014), we value the costs for simple assaults as 25% of that for aggravated assaults.

[21] Estimated savings heavily depend on how to value the social cost of each crime type. The figure we calculate here, therefore, is merely a rough magnitude.

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Table 1: Descriptive statistics

Variables	(1) N	(2) Mean	(3) Std.Dev.	(4) Minimum	(5) Maximum
Population (1000)	23,803	100.32	192.47	1.04	2,335.39
The NBP county	1,297	0.50	0.50	0.00	1.00
Actual offenses per 1,000 people					
Num. of violent crimes	23,803	6.62	4.82	0.00	139.72
Num. of assaults	23,803	6.21	4.57	0.00	132.41
Num. of robberies	23,803	0.26	0.38	0.00	4.67
Num. of rapes	23,803	0.14	0.14	0.00	8.44
Num. of murders	23,803	0.02	0.04	0.00	1.83
Num. of property crimes	23,803	13.94	8.56	0.41	245.96
Num. of larcenies	23,803	9.65	6.17	0.00	173.98
Num. of burglaries	23,803	3.35	2.32	0.00	66.97
Num. of motor vehicles thefts	23,803	0.95	0.95	0.00	44.04
Air quality index					
AQI	23,803	48.77	18.23	1.00	124.60
Weather					
Average temperature (^{0}F)	23,803	58.76	14.76	21.35	87.24
Average precipitation (mm)	23,803	2.23	1.45	0.00	10.47
Average dew point (^{0}F)	23,803	46.41	15.00	0.00	74.00

Note: The crime-data sample includes 1,297 counties in 37 states. Each observation represents a county$\times$year$\times$season cell. Crimes are totals per 1,000 people per county-season-year. Air quality index and meteorological factors are mean values in each county-year-season cell. The sample covers the period from 1998 through 2008.

Table 2: Impacts of air pollution on violent and property criminal activities (fixed-effect estimates)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Violent	Assault	Robbery	Rape	Murder	Property	Larceny	Burglary	Motor
AQI	0.001**	0.001***	-0.000	0.001**	-0.000	0.000	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	23,803	23,803	23,803	23,803	23,803	23,803	23,803	23,803	23,803
County-by-Season FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flexible Weather Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each observation represents a county×year×season cell. Dependent variables are the number of crimes per 1,000 people per county-season-year (in logarithm). Regressions are GLS with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by state and year.

Table 3: Impact of the NBP on violent crimes

	(1)	(2)	(3)
A. Total violent crimes	-0.023*	-0.039***	-0.037**
	(0.013)	(0.012)	(0.017)
B. Assault	-0.018	-0.034***	-0.032*
	(0.013)	(0.012)	(0.017)
C. Robbery	-0.066***	-0.072***	-0.074***
	(0.015)	(0.017)	(0.024)
D. Rape	-0.046**	-0.063***	-0.065**
	(0.018)	(0.019)	(0.027)
E. Murder	-0.064***	-0.065***	-0.068***
	(0.013)	(0.015)	(0.019)
County-by-Season FE	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes
State-by-Year FE	Yes	Yes	No
County-by-Year FE	No	No	Yes
Flexible Weather Controls	No	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each observation represents a county×year×season cell. The dependent variables are shown in the leftmost column. The number of observations are 23,803 for all regressions. Regressions are GLS with weight equal to square root of population in a given county-year-season cell. Standard errors in parentheses, clustered by state and year.

Table 4: Impact of the NBP on property crimes

	(1)	(2)	(3)
A. Total property crimes	-0.020	-0.030**	-0.029*
	(0.013)	(0.012)	(0.016)
B. Larceny	-0.025	-0.036**	-0.033**
	(0.018)	(0.017)	(0.016)
C. Burglary	-0.018	-0.030***	-0.029**
	(0.012)	(0.011)	(0.014)
D. Motor vehicle theft	-0.001	-0.006	-0.009
	(0.018)	(0.019)	(0.024)
County-by-Season FE	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes
State-by-Year FE	Yes	Yes	No
County-by-Year FE	No	No	Yes
Flexible Weather Controls	No	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each observation represents a county×year×season cell. The dependent variables are shown in the leftmost column. The number of observations are 23,803 for all regressions. Regressions are GLS with weight equal to square root of population in a given county-year-season cell. Standard errors in parentheses, clustered by state and year.

Table 5: Validity checks on the identification assumption for the triple-difference estimator

VARIABLES	(1) Violent	(2) Assault	(3) Robbery	(4) Rape	(5) Murder	(6) Property	(7) Larceny	(8) Burglary	(9) Motor
NBP× year	-0.009	-0.009	-0.006	-0.007	-0.011	-0.001	-0.005	0.012	-0.009
	(0.006)	(0.006)	(0.010)	(0.015)	(0.009)	(0.005)	(0.006)	(0.008)	(0.010)
Observations	5,261	5,261	5,261	5,261	5,261	5,261	5,261	5,261	5,261
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State specific linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables are the differences of log of criminal activities per 1,000 people between summer and winter within a county-year cell. NBP×*year* is the interaction between a NBP-county dummy and linear trend term. Ordinary least squares estimates for all columns. Standard errors in parentheses, clustered by state and year.

Table 6: Impacts of air pollution on violent and property criminal activities (IV estimates)

VARIABLES	(1) Violent	(2) Assault	(3) Robbery	(4) Rape	(5) Murder	(6) Property	(7) Larceny	(8) Burglary	(9) Motor
AQI	0.009*	0.007*	0.017*	0.015*	0.016***	0.007	0.008*	0.007	0.002
	(0.005)	(0.004)	(0.009)	(0.008)	(0.006)	(0.005)	(0.005)	(0.004)	(0.006)
Observations	23,803	23,803	23,803	23,803	23,803	23,803	23,803	23,803	23,803
County-by-Season FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flexible Weather Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each observation represents a county×year×season cell. Dependent variables are the number of crimes per 1,000 people per county-season-year (in logarithm). AQI is the endogenous variable, which is instrumented by *DDD*. Regressions are two-stage least squares with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by state and year.

Table 7: Impact of the NBP on unemployment rate and worker earnings

VARIABLES	(1) Unemployment rate (%)	(2) Earnings of new hires ($)	(3) Earnings of all workers ($)
DDD	-0.003	-17.0	-12.1
	(0.017)	(26.6)	(32.2)
Pre-2003 mean	4.262	1,988.0	1,996.5
Observations	678	544	544
State-by-Season FE	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes
State-by-Year FE	Yes	Yes	Yes
Flexible Weather Controls	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each observation represents a state-year-season cell. Mean represents 1998-2002 summer in NBP areas. **DDD** is the triple difference estimator, which equals to 1 for all NBP states in summertime in 2003 (or 2004) through 2008. Standard errors in parentheses, clustered by state and year.

Table 8: Impact of the NBP on electricity consumption

VARIABLES	(1) Electricity consumption (in logarithm)
DDD	0.021
	(0.046)
Observations	214,908
Plant-by-Season FE	Yes
Season-by-Year FE	Yes
Plant-by-Year FE	Yes
Flexible Weather Controls	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each observation represents a plant-year-season cell. **DDD** is the triple difference estimator, which equals to 1 for all electricity generating units belonging to NBP states in summertime in 2003 (or 2004) through 2008. Standard errors in parentheses, clustered by state and year.

Table 9: Heterogeneous analysis by NO_x emission level

VARIABLES	(1) Violent	(2) Assault	(3) Robbery	(4) Rape	(5) Murder	(6) Property	(7) Larceny	(8) Burglary	(9) Motor
				Panel A. Counties with high NO_x emissions					
DDD	-0.046**	-0.040*	-0.081***	-0.050	-0.119***	-0.047***	-0.052***	-0.044**	-0.021
	(0.022)	(0.021)	(0.028)	(0.035)	(0.034)	(0.015)	(0.015)	(0.020)	(0.031)
Observations	12,200	12,200	12,200	12,200	12,200	12,200	12,200	12,200	12,200
				Panel B. Counties with low NO_x emissions					
DDD	-0.023*	-0.020	-0.067**	-0.081**	0.010	-0.004	-0.007	-0.010	0.004
	(0.014)	(0.015)	(0.027)	(0.031)	(0.044)	(0.009)	(0.009)	(0.012)	(0.021)
Observations	11,132	11,132	11,132	11,132	11,132	11,132	11,132	11,132	11,132
County-by-Season FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flexible Weather Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

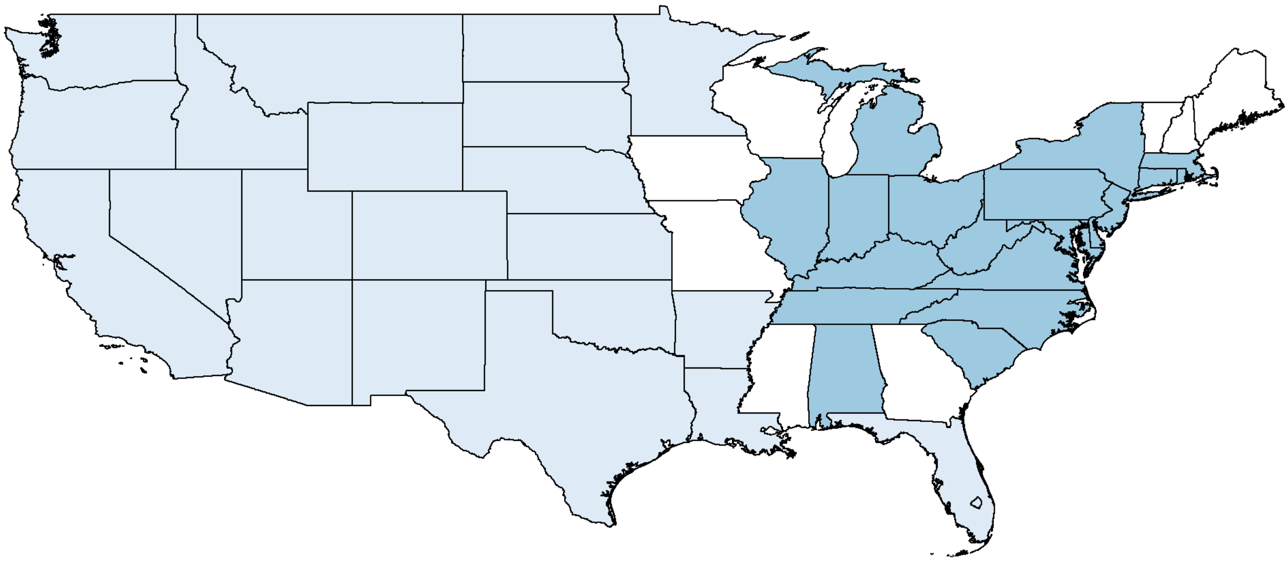
Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Panels A and B report the estimates for two subsamples divided by the median summertime NO_x emission level in the period 1998-2002. For the 1,297 counties in our regression sample, the median summertime NO_x emissions were 200 tons. Each observation represents a county×year×season cell. Regressions are GLS with weight equal to square root of population in a given county-year-season cell. Standard errors in parentheses, clustered by state and year.

Table 10: Recomputing p-values using the method by Benjamini and Yekutieli (2001)

VARIABLES	(1) Assault	(2) Robbery	(3) Rape	(4) Murder	(5) Larceny	(6) Burglary	(7) Motor vehicle theft
DDD	-0.032*	-0.074***	-0.065**	-0.068***	-0.033**	-0.029**	-0.009
	(0.065)	(0.003)	(0.020)	(0.001)	(0.039)	(0.044)	(0.723)
Observations	23,803	23,803	23,803	23,803	23,803	23,803	23,803
County-by-Season FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flexible Weather Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each observation represents a county×year×season cell. Dependent variables are the number of criminal activities per 1,000 people per county-season-year (in logarithm). **DDD** is the triple-difference estimator, which equals to 1 for all counties belonging to NBP states in summertime in 2003 (or 2004) through 2008. Regressions are GLS with weight equal to square root of population in a given county-year-season. Corrected **p-values** in parentheses.

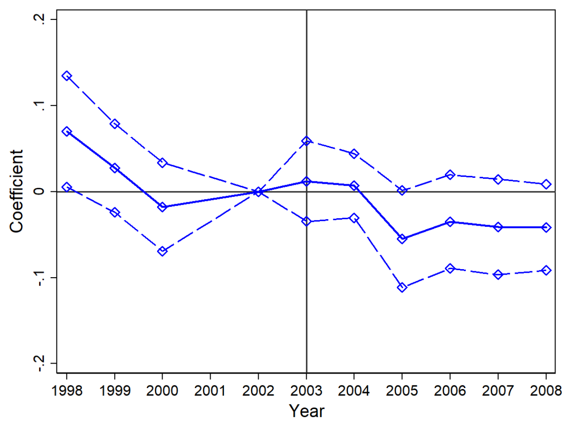
Figure 1: NBP regions



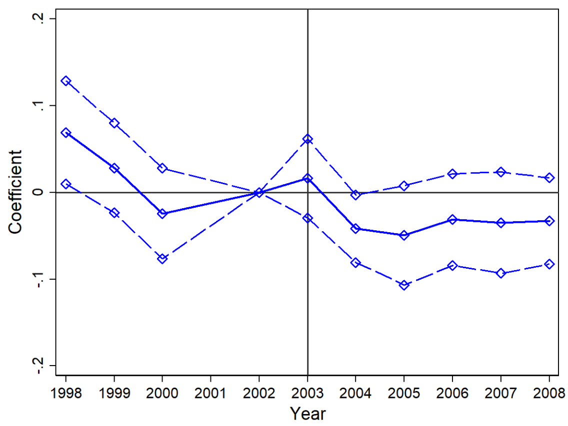
Note: Dark blue states are those participating in the NBP during the 2003-2008 period (the NBP states). Light blue states are not participating (non-NBP states). White states, which did not participate in the NBP but are adjacent to NBP states, are: Georgia, Iowa, Maine, Mississippi, Missouri, New Hampshire, Vermont, and Wisconsin. We exclude these states in our analysis. Additionally, non-continental states (Alaska and Hawaii) and Puerto Rico are also not included. Alabama, Florida, and Illinois are also deleted because their crime data do not satisfy the requirements for the present analysis (see details in data section). As only a few counties in Michigan participated the NBP, we do not include it in the analysis.

Figure 2: Impacts of the NBP on violent crimes across years

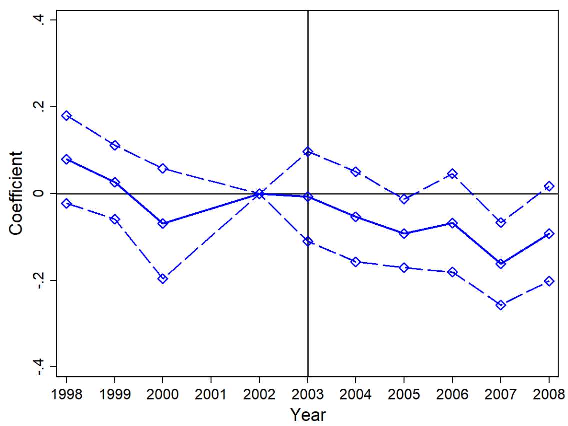
(a) Violent



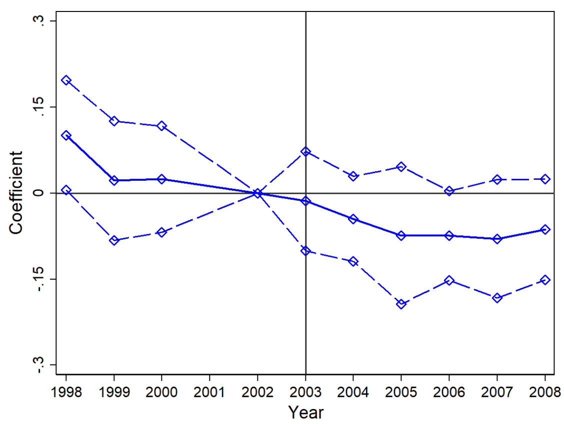
(b) Assault



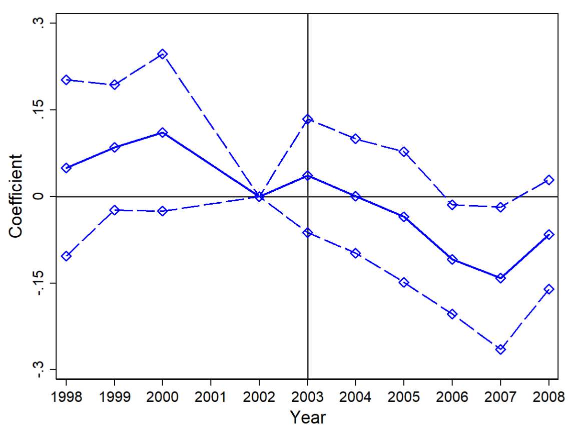
(c) Robbery



(d) Rape

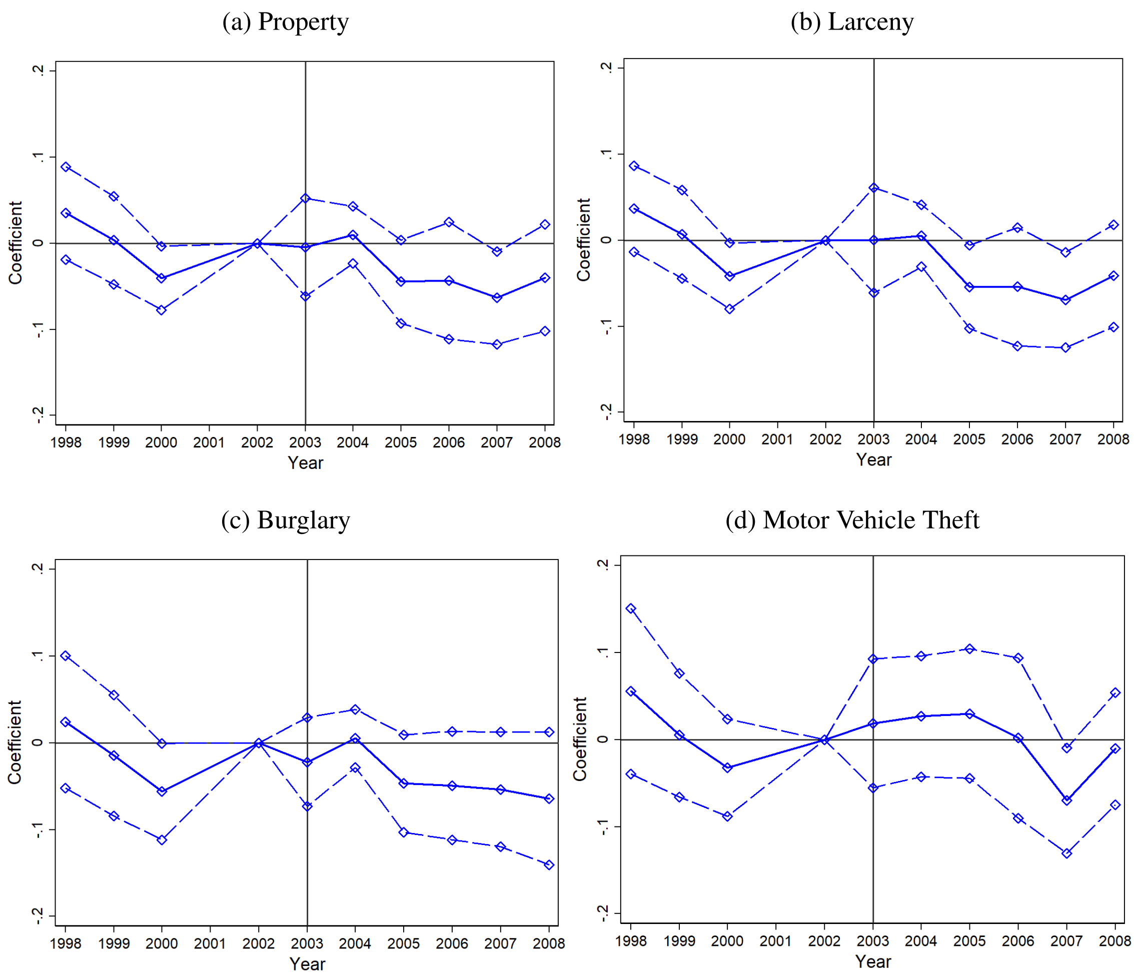


(e) Murder



Note: Solid lines denote estimated coefficients. Dash lines represent upper and lower bounds for the 95% confidence interval.

Figure 3: Impacts of the NBP on violent and property crimes across years



Note: Solid lines denote estimated coefficients. Dash lines represent upper and lower bounds for the 95% confidence interval.

A Appendix

Table A.1: Sensitivity analysis: Excluding participating states in the early version program

VARIABLES	(1) Violent	(2) Assault	(3) Robbery	(4) Rape	(5) Murder	(6) Property	(7) Larceny	(8) Burglary	(9) Motor
DDD	-0.044*	-0.040*	-0.082***	-0.073**	-0.094***	-0.023**	-0.031***	-0.014	0.003
	(0.022)	(0.022)	(0.030)	(0.035)	(0.021)	(0.011)	(0.011)	(0.016)	(0.028)
Observations	19,787	19,787	19,787	19,787	19,787	19,787	19,787	19,787	19,787
County-by-Season FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flexible Weather Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each observation represents a county×year×season cell. Dependent variables are the number of crimes per 1,000 people per county-season-year (in logarithm). The regression sample excludes the states which participated in the early version of the NBP, including Connecticut, Delaware, District of Columbia, Maryland, Massachusetts, New Jersey, New York, Pennsylvania, and Rhode Island. Regressions are GLS with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by state and year.

Table A.2: Sensitivity analysis (including adjacent states as the control group)

VARIABLES	(1) Violent	(2) Assault	(3) Robbery	(4) Rape	(5) Murder	(6) Property	(7) Larceny	(8) Burglary	(9) Motor
DDD	-0.014*	-0.012	-0.037**	-0.046***	0.001	-0.011	-0.016*	-0.008	0.014
	(0.008)	(0.008)	(0.016)	(0.017)	(0.020)	(0.008)	(0.008)	(0.010)	(0.011)
Observations	35,592	35,592	35,592	35,592	35,592	35,592	35,592	35,592	35,592
County-by-Season FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flexible Weather Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each observation represents a county×year×season cell. Dependent variables are the number of criminal activities per 1,000 people per county-season-year (in logarithm). **DDD** is the triple-difference estimator, which equals to 1 for all counties belonging to NBP states in summertime in 2003 (or 2004) through 2008. Regressions are GLS with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by state and year.

Table A.3: Sensitivity analysis (including adjacent states as the treatment group)

VARIABLES	(1) Violent	(2) Assault	(3) Robbery	(4) Rape	(5) Murder	(6) Property	(7) Larceny	(8) Burglary	(9) Motor
DDD	-0.020**	-0.015	-0.055***	-0.051***	-0.038*	-0.017*	-0.021**	-0.015	0.007
	(0.010)	(0.010)	(0.019)	(0.019)	(0.021)	(0.010)	(0.011)	(0.011)	(0.013)
Observations	35,592	35,592	35,592	35,592	35,592	35,592	35,592	35,592	35,592
County-by-Season FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flexible Weather Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each observation represents a county×year×season cell. Dependent variables are the number of criminal activities per 1,000 people per county-season-year (in logarithm). **DDD** is the triple-difference estimator, which equals to 1 for all counties belonging to NBP states in summertime in 2003 (or 2004) through 2008. Regressions are GLS with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by state and year.

Table A.4: Falsification test

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Violent	Assault	Robbery	Rape	Murder	Property	Larceny	Burglary	Motor
DDD	-0.025	-0.027	-0.038	0.035	-0.031	-0.001	-0.013	0.025	0.024
	(0.017)	(0.017)	(0.032)	(0.032)	(0.047)	(0.016)	(0.015)	(0.023)	(0.024)
Observations	12,992	12,992	12,992	12,992	12,992	12,992	12,992	12,992	12,992
R-squared	0.979	0.979	0.958	0.886	0.911	0.978	0.978	0.962	0.957
County-by-Season FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flexible Weather Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each observation represents a county×year×season cell. Dependent variables are the number of crimes per 1,000 people per county-season-year (in logarithm). In this table, we use the sample period from 1998 to 2002 and setting 2000 as the start time of the placebo NBP. Regressions are GLS with weight equal to square root of population in a given county-year-season. Standard errors in parentheses, clustered by state and year.

Table A.5: Impact of the NBP on air pollution

VARIABLES	(1) AQI	(2) AQI	(3) AQI
DDD	-3.777***	-5.022***	-5.244***
	(0.686)	(0.700)	(1.036)
Pre-2003 mean	69.026	69.026	69.026
Observations	23,803	23,803	23,803
County-by-Season FE	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes
State-by-Year FE	Yes	Yes	No
County-by-Year FE	No	No	Yes
Flexible Weather Controls	No	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each observation represents a county-year-season cell. Mean represents 1998-2002 summer in NBP areas. **DDD** is the triple difference estimator, which equals to 1 for all counties belonging to NBP states in summertime in 2003 (or 2004) through 2008. Standard errors in parentheses, clustered by state and year.

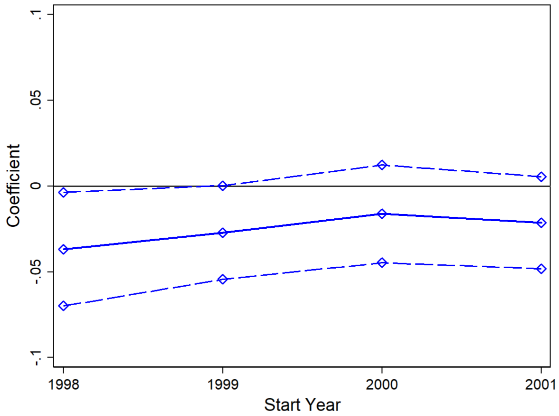
Table A.6: Impact of the NBP on NO_x emissions by NO_x emission level before the NBP

VARIABLES	(1) Counties with high NO_x emissions	(2) Counties with low NO_x emissions
DDD	-1.733***	-0.093
	(0.529)	(0.078)
Pre-2003 mean	5.526	0.010
Observations	12,258	11,074
County-by-Season FE	Yes	Yes
Season-by-Year FE	Yes	Yes
County-by-Year FE	Yes	Yes
Flexible Weather Controls	Yes	Yes

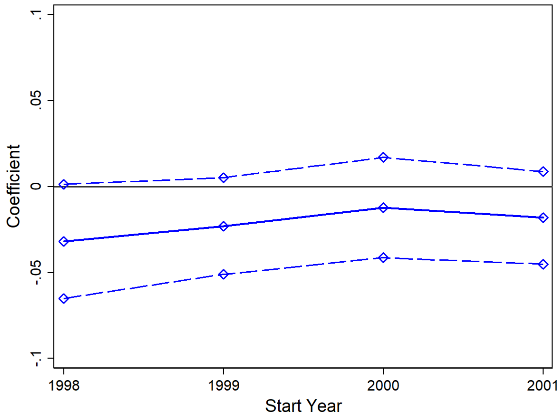
Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Columns (1) and (2) report the estimates for two subsamples divided by the median summertime NO_x emission level in the period 1998-2002. For the 1,360 counties in our regression sample, the median summertime NO_x emissions were 200 tons. Each observation represents a county-year-season cell. Winter emissions are multiplied by 5/7, so all values are summer-equivalent. Response variable measured in thousands of tons. Mean represents 1998-2002 summer in NBP areas. **DDD** is the triple difference estimator, which equals to 1 for all counties belonging to NBP states in summertime in 2003 (or 2004) through 2008. Standard errors in parentheses, clustered by state and year.

Figure A.1: Estimates based on alternative sample periods (violent crimes)

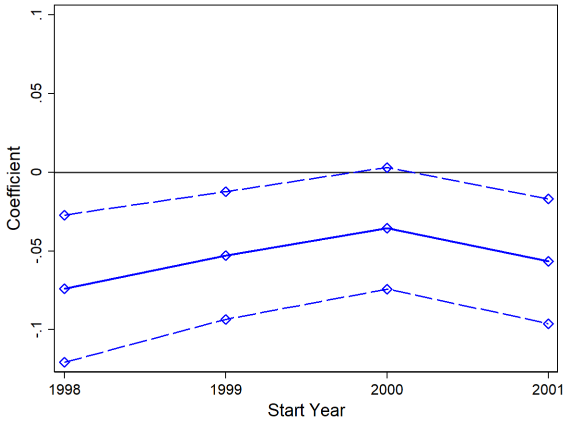
(a) Violent



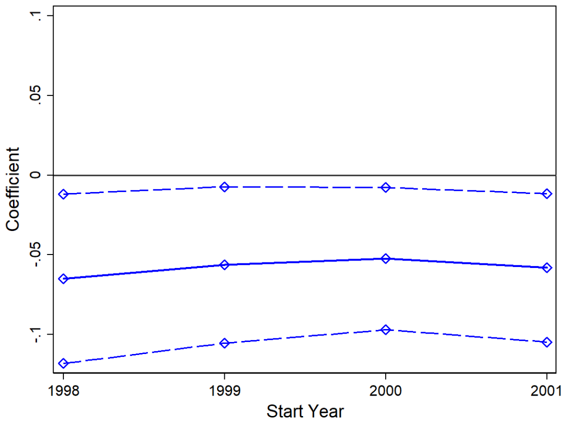
(b) Assault



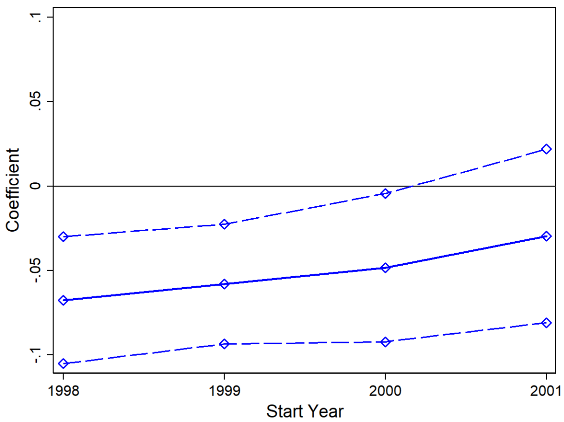
(c) Robbery



(d) Rape

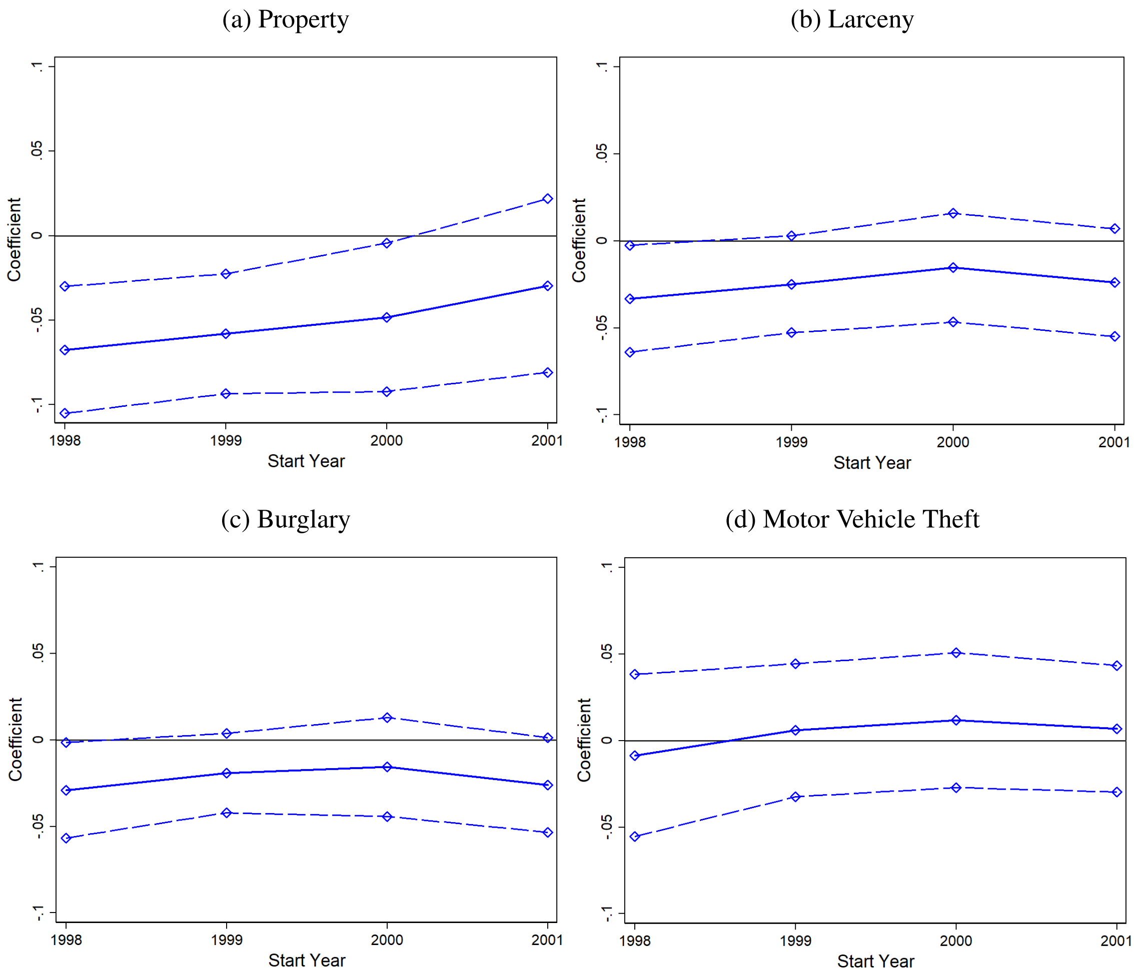


(e) Murder



Note: Solid lines denote estimated coefficients. Dash lines represent upper and lower bounds for the 95% confidence interval.

Figure A.2: Estimates based on alternative sample periods (property crimes)



Note: Solid lines denote estimated coefficients. Dash lines represent upper and lower bounds for the 95% confidence interval.