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 NO_x emissions and ozone concentrations in participating states. Employing a triple-difference estimator, we find robust evidence that the cap-and-trade market statistically significantly reduced violent crimes in participating states, whereas property crimes were less affected. Instrumental variable estimates suggest that lowering pollution emissions may play an important role in reducing violent 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 criminal activities are driven by air pollution—in the present 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 (pollution emissions) on criminal behaviors.

The NBP was a cap-and-trade system aimed at reducing ozone concentrations.[2] 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.[3] Nineteen Eastern and Midwestern states, together with Washington, DC, were included in this program (see Figure 1). 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 variation, we use a triple-difference estimator to examine the relationship between air pollution and criminal acts.

By compiling county-season-level crime data with pollution emission and weather information, we find that the NBP market statistically significantly lowered violent crime rate in participating states by 2.9%. To put this figure into perspective, Chalfin and McCrary (2013) found that violent crimes decrease by around 0.4% once police officers

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

[2] Details about the NBP market are provided in Deschênes, Greenstone and Shapiro (2017) and Curtis (2017).

[3] 2004 is an exception. The NBP operated from June to September in that year.

increase by 1%. Based on our estimate, the effect of the NBP on violent crimes is equivalent to increasing the size of the police force by about 7% during the sample period. To be specific, rates for assault, robbery, and rape statistically significantly decreased by 2.5%, 7.9%, and 6.6%, respectively. Although the NBP's effect on murders is not statistically significant at the traditional level, the magnitude is not negligible; Rate for murder fell by 2.1%. On the other hand, the effects of the NBP market on property crimes are not statistically significant and the magnitudes are relatively small.

To validate the assumption of the triple-difference estimator, we use two 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 might be 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.

Our study contributes to the environment economics literature in several ways. First, this paper, along with a concurrent study by Herrnstadt et al. (2016), is the first to identify the causal relationship between air pollution (pollution emissions) and criminal activities. 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 emission monitors to counties shrink estimates towards zero. Without considering the endogeneity problem of pollution emissions, our fixed-effect estimates indicate that air pollution does not drive any criminal behaviors—but the instrumental variable estimates demonstrate that air pollution (pollution emissions) is indeed a determinant of violent 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 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 (pollution emissions) 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. 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 pollution emission reduction, and thus have important policy implications.

The rest of the paper is organized as follows. The second section describes mechanisms that may explain the association between air pollution and criminal activities. 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 Possible 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.

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 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.[4] 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.[5] 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 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

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

[5] 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.

clustered at the state level.

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:

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

where ΔY_{it} represents the differences in criminal activities between summer and winter within a county-year cell. $\mathbf{1}(NBP)_i$ is a dummy indicating those counties in the NBP states. 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.

3.3 Instrumental variable estimation

Next, we use *DDD* in Equation (1) as an instrument variable to measure the effects of NO_x emissions on criminal behaviors. The causal interpretation of the IV estimates is straightforward: as the direct controlled pollutant NO_x, the estimated effects on criminal activities should result from reductions in NO_x emissions caused by the NBP.[6] Specifications for the two-stage-least-square estimation are as follow:

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

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

where $\widehat{NOx_{ist}}$ is the predicted NO_x emissions 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 NO_x emissions. As can be seen in Table A.3, NO_x emissions decreased by about 33.1% of 1997-2002 mean summer emissions in NBP states. Although reductions in SO_2 emissions are statistically significant, the effect is relatively small (see Table A.4). Another 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

[6]In our sample, 645 out of 1,412 counties had positive NO_x emissions. To avoid any potential selection bias, we include all counties in IV and fixed-effect estimates. For those without positive NO_x emissions, we treat them as zero emitted pollution. Tables A.1 and A.2 show estimates based on only counties with positive NO_x emissions. The results do not change much.

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. Moreover, given the positive relationship between unemployment rate and criminal activities (e.g., Raphael and Winter-Ebmer 2001), the unemployment changes caused by the NBP would likely bias our estimates downwards. In other words, our estimates on the effects of pollution on criminal activities are lower bound.

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.[7]

Crime data. Crime data are extracted from the Uniform Crime Reporting (UCR) 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 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,

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

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.[8] 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.[9] Following Ranson (2014), we drop all county-year cells with a population of fewer than 1,000 people.

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.[10] Such data availability constrains us from comparing summer versus winter. However, another pollution reduction program, called the Acid 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, SO_2, and CO_2 for 1,734 firms in 645 counties in the 49 states. 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 (Deschênes, 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.[11]

In the analysis, we exclude non-continental states—i.e., Alaska and Hawaii—and

[8] Most cases are from counties in Alabama and Florida.

[9] Most of these cases are in Illinois counties.

[10] One exception is that in 2004, the NBP initiated from the end of May.

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

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.[12] 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 1,412 counties in 37 states.

4.2 Descriptive analysis

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

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

where NOx_{ist} denotes NO_x emissions 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 NO_x emissions 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 NO_x emissions on violent crimes are statistically indifferent from zero, except for rapes. The magnitudes are small, which suggests that NO_x emissions have little influence on violent crimes. Similarly, columns (6) through (9) in Table 2 present estimates on property crimes. The effects on larcenies and motor vehicle thefts are not statistically significant from zero, and although the estimate on larcenies is statistically significant at the 5% level, the magnitude is not large.

[12] In sensitivity analysis, we include these states as the control or treatment group. Our results remain stable (see Tables A.5 and A.6).

To summarize, the fixed-effect estimates provide little evidence that NO_x emissions have 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 emission 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.[13] We expect that by using IV methods, the coefficients of NO_x emissions would become larger if air pollution did affect criminal behaviors.

5 Main results

This section first summarizes estimates of the reduced-form effects of the NBP on violent and property crimes. Next, we employ two methods to check the validity of the identification assumption for this triple-difference setting. In addition, using an IV approach, we measure the effects of NO_x emissions on criminal activities and compare them to fixed-effect estimates. Finally, we present several sensitivity analyses.[14]

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 ef-

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

[14]As Deschênes, Greenstone and Shapiro (2017) have proved that the NBP significantly decreased NO_x emissions and ozone and NO_2 concentrations, we do not emphasize these results in this study.

fects at first. The coefficient is statistically insignificant at the conventional 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 2.9%.

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 2004-2005 and 2007-2008 are statistically significant at the 5% 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 2.5%. The estimate is statistically significant at the 1% 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 2003-2008 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 7.9% (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 statistically significantly 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 6.6%. The estimate is statistically significant at the 1% 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, but they are not statistically significant at the conventional level. Correspondingly, as can be seen in panel (e) of Figure 2, there is no statistically significant 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 2.9%. Specifically, the effects on assault, rape, and robbery are 2.5%, 7.9%, and 6.6%, respectively. However, the estimate for the murder rate is not statistically at the traditional level, although the sign of the coefficient is negative. 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.

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 was not statistically significant at the traditional 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 5% level.[15] The common trend assumption for burglary rate may hold. After the market's initiation, the coefficients are not statistically significant different from zero. More importantly, after 2003 the coefficients do not depict a clear trend, suggesting no clear evidence that the property crime rate declined.

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 did not impose a statistically significant impact on the larceny rate. 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, the coefficients in 2003 through 2008 fluctuate and are not significant at the 5% level.

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 relatively small—a reduction of 0.9 burglaries per 1,000 people—and the estimate is not statistically significant at the traditional 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, the coefficients are not statistically significant different from zero, suggesting no clear evidence that the burglary rate declined.

Motor vehicle theft. Panel (D) of Table 4 reports reduced-form effects of the NBP

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

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. The sign of the coefficient is even positive. 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.

To summarize, in this section we find that the effects of the NBP market on property crimes are relatively small and statistically insignificant. The results indicate that the mechanism—air pollution may increase the probability of successfully committing a crime and escaping undetected—we outline in the second section may not be an important one. Using event graph studies, no clear evidence demonstrates any meaningful differences in the trend in summertime property crimes between participating and non-participating states before the market's initiation.

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.

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, most 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. One point worth noting is that some pre-existing differences seem to exist in the rape rate; However, the estimate is only statistically significant at the 10% level. In general, we conclude that only a small pre-existing trend, if any, presents in our triple-difference setting.

5.4 IV estimates

In this part, we first estimate the NBP's effects on pollution emissions, which would enable us to check the validity of the IV estimation. As can be seen below, our results are consistent with those of Deschênes, Greenstone and Shapiro (2017). Next, we employ two-stage-least-square estimation to measure the effects of NO_x emissions on criminal activities. Furthermore, we compare the IV results to fixed-effect estimates.

Table A.3 statistically reports the NBP's effect on NO_x emissions. Compared to the average emissions in NBP states in advance of the market's initiation, NO_x emissions statistically significantly fell by about 33.1%. The magnitude is similar to that of Deschênes, Greenstone and Shapiro (2017).[16] Additionally, in Table A.4, we find that SO_2 emissions statistically significantly decreased by around 7.1%. However, the effect on CO_2 emissions is statistically insignificant. Although there are some reductions in SO_2 emissions, the magnitude is relatively economically small.

The effects of NO_x emissions on violent crimes are shown in columns (1)-(5) in Ta-

[16] In Deschênes, Greenstone and Shapiro (2017), they assigned emissions of zero to counties with no recorded emissions, while we do not include these counties. This is because NO_x can travel long distances. The NO_x concentrations in these counties are not necessary to be zero. Although the total reduction of NO_x emissions estimated in our study are higher than that in Deschênes, Greenstone and Shapiro (2017), the reduction percentages are similar to each other.

ble 6. The results indicate a evident association between NO_x emissions and violent crimes; the effects on assaults, robberies, and rapes are statistically significant at the 1% level. A 1,000-ton reduction in NO_x emissions lowers assault, rape, and robbery rates by 2.5%, 8.0%, and 6.7%, respectively. Although the estimate on murder is not precise, the sign is positive. Columns (6)-(9) in Table 6 present the estimates on property criminal activities. Overall, the coefficients are smaller than those for violent crimes, and the sign for motor vehicle thefts is even negative. The findings therefore suggest that NO_x emissions affect violent criminal behaviors but not property crimes.

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 NO_x emissions have 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 NO_x's effects on violent crimes become larger.

5.5 Sensitivity analysis

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 7. As can be seen, the effects on assault, robbery, and rape rates remain statistically significant. On the other hand, the effects on property crimes remain statistically insignificant. To sum up, after taking multiple hypotheses tests into account, our main inferences remain stable.

Falsification test. Based on the potential mechanisms that explain the relationships between air pollution and criminal activities, we expect that manslaughters should not be affected much by air pollution. This is because manslaughter is the killing of another person through gross negligence. Table 8 presents the impact of the NBP on manslaughters. The estimates in columns (1) through (3) indicate that the NBP's effect on the manslaughter rate is not statistically significantly different from zero. These results provide a reassuring placebo test.

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 murders are not statistically significant; and the NBP's effects on property crimes, except for larcenies, are not statistically significant across different 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.5 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.6; 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 NO_x emissions and ozone concentrations in participating states. Using a triple-difference method, we find that violent crimes in the participating states statistically significantly decreased. However, property crimes were less affected by the NBP. Instrumental variable estimates suggest that NO_x emissions are positively correlated with violent criminal behaviors, indicating that lowering pollution emissions may play an important role in reducing violent 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, and murder are \$66,888, \$42,310, \$240,776, and \$9 million, respectively.[17] According to our estimates, this cap-and-trade market decreased assaults, robberies, and rapes in the Eastern U.S. by 11,652.1, 3,611.5, and 671.1 cases per year, respectively.[18] In total, the NBP saved around \$558 million per year in societal costs. If we further take murders into account, total societal costs saved by the NBP reach about \$1,094 million.[19] Relative to the costs of the NBP (\$400-700 million per year as estimated by Deschênes, Greenstone and Shapiro (2017)), these benefits were non-negligible.

[17]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.

[18]Murders were reduced by 36.0 cases per year, although the estimated effects are not statistically significant.

[19]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)	29,834	85.85	175.44	1.04	2,335.39
The NBP county	1,412	0.51	0.50	0.00	1.00
Actual offenses per 1,000 people					
Num. of violent crimes	29,834	6.36	4.83	0.00	166.82
Num. of assaults	29,834	5.98	4.58	0.00	165.01
Num. of robberies	29,834	0.23	0.36	0.00	4.67
Num. of rapes	29,834	0.14	0.15	0.00	8.44
Num. of murders	29,834	0.02	0.04	0.00	1.83
Num. of property crimes	29,834	13.50	8.44	0.41	245.96
Num. of larcenies	29,834	9.34	6.08	0.00	173.98
Num. of burglaries	29,834	3.26	2.31	0.00	69.43
Num. of motor vehicles thefts	29,834	0.90	0.92	0.00	44.04
Pollution emissions (1,000 tons)					
NO_x Emissions	10,089	3.18	4.87	0.00	58.79
SO_2 Emissions	10,089	7.58	13.36	0.00	127.11
CO_2 Emissions	10,089	1,798.52	2,275.16	0.00	14,046.22
Weather					
Average temperature (^{0}F)	29,834	58.69	15.08	20.01	88.05
Average precipitation (mm)	29,834	2.09	1.46	0.00	10.47
Average dew point (^{0}F)	29,834	46.23	15.09	0.00	74.00

Note: The crime-data sample includes 1,412 counties in 37 states. Each observation represents a county×year×season cell. Crimes are totals per 1,000 people per county-season-year. Pollution emissions are mean values in each county-year-season cell. Winter emissions are multiplied by 5/7, so all values are summer-equivalent. Means are across counties (i.e., not weighted). The sample covers the period from 1998 through 2008.

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

VARIABLES	(1) Violent	(2) Assault	(3) Robbery	(4) Rape	(5) Murder	(6) Property	(7) Larceny	(8) Burglary	(9) Motor
NO_x emissions	0.000	-0.000	0.001	0.007*	-0.001	0.001	0.003**	-0.003	0.002
	(0.001)	(0.001)	(0.004)	(0.003)	(0.004)	(0.001)	(0.001)	(0.002)	(0.002)
Observations	29,603	29,834	29,834	29,834	29,834	29,834	29,834	29,834	29,834
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.

Table 3: Impact of the NBP on violent crimes

	(1)	(2)	(3)
A. Total violent crimes	-0.012	-0.030***	-0.029***
	(0.009)	(0.010)	(0.010)
B. Assault	-0.008	-0.026***	-0.025***
	(0.009)	(0.009)	(0.009)
C. Robbery	-0.065***	-0.076***	-0.079***
	(0.018)	(0.019)	(0.019)
D. Rape	-0.045**	-0.064***	-0.066***
	(0.019)	(0.020)	(0.019)
E. Murder	-0.022	-0.020	-0.021
	(0.020)	(0.020)	(0.020)
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$\times$year$\times$season cell. The dependent variables are shown in the leftmost column. The number of observations are 29,834 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.

Table 4: Impact of the NBP on property crimes

	(1)	(2)	(3)
A. Total property crimes	-0.004	-0.013	-0.012
	(0.007)	(0.008)	(0.008)
B. Larceny	-0.008	-0.017	-0.016
	(0.010)	(0.011)	(0.010)
C. Burglary	0.001	-0.011	-0.009
	(0.009)	(0.009)	(0.009)
D. Motor vehicle theft	0.009	0.001	-0.006
	(0.013)	(0.016)	(0.015)
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 36,632 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.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Violent	Assault	Robbery	Rape	Murder	Property	Larceny	Burglary	Motor
NBP× year	-0.002	-0.004	0.002	0.016*	-0.006	-0.001	-0.003	0.005	0.003
	(0.004)	(0.005)	(0.007)	(0.008)	(0.009)	(0.003)	(0.005)	(0.008)	(0.008)
Observations	6,657	6,657	6,657	6,657	6,657	6,657	6,657	6,657	6,657
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$\times$*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.

Table 6: Impacts of NO_x emissions on violent and property criminal activities (IV estimates)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Violent	Assault	Robbery	Rape	Murder	Property	Larceny	Burglary	Motor
NO_x emissions	0.029***	0.025**	0.080***	0.067***	0.023	0.016	0.021	0.012	-0.004
	(0.010)	(0.010)	(0.021)	(0.017)	(0.022)	(0.010)	(0.015)	(0.013)	(0.014)
Observations	29,834	29,834	29,834	29,834	29,834	29,834	29,834	29,834	29,834
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). NO_x emissions 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.

Table 7: 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	-0.025***	-0.079***	-0.066***	-0.021	-0.016	-0.009	0.006
	(0.008)	(0.000)	(0.001)	(0.159)	(0.195)	(0.231)	(0.655)
Observations	29,834	29,834	29,834	29,834	29,834	29,834	29,834
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

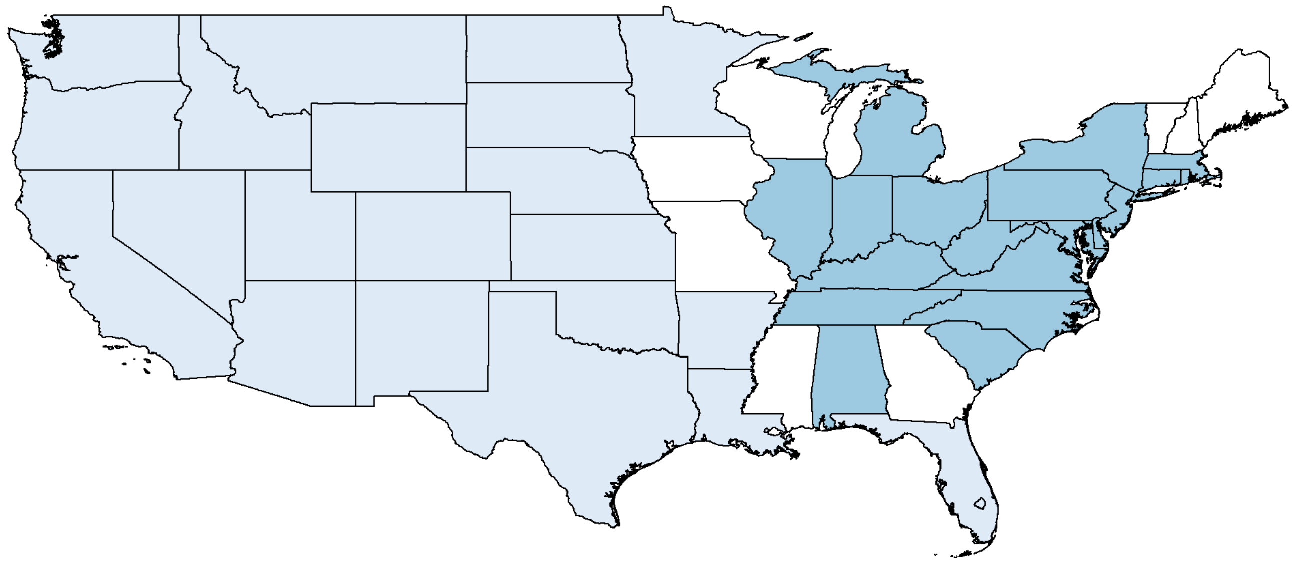
Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each observation represents a county$\times$year$\times$season cell. Dependent variables are the number of criminal activities per 1,000 people per county-season-year (in logarithm). **DDD** is the triple-difference estimator, which equals to 1 for all counties 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.

Table 8: Impact of the NBP on manslaughters

VARIABLES	(1) Manslaughter	(2) Manslaughter	(3) Manslaughter
DDD	-0.006	-0.002	-0.003
	(0.022)	(0.023)	(0.023)
Observations	29,834	29,834	29,834
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

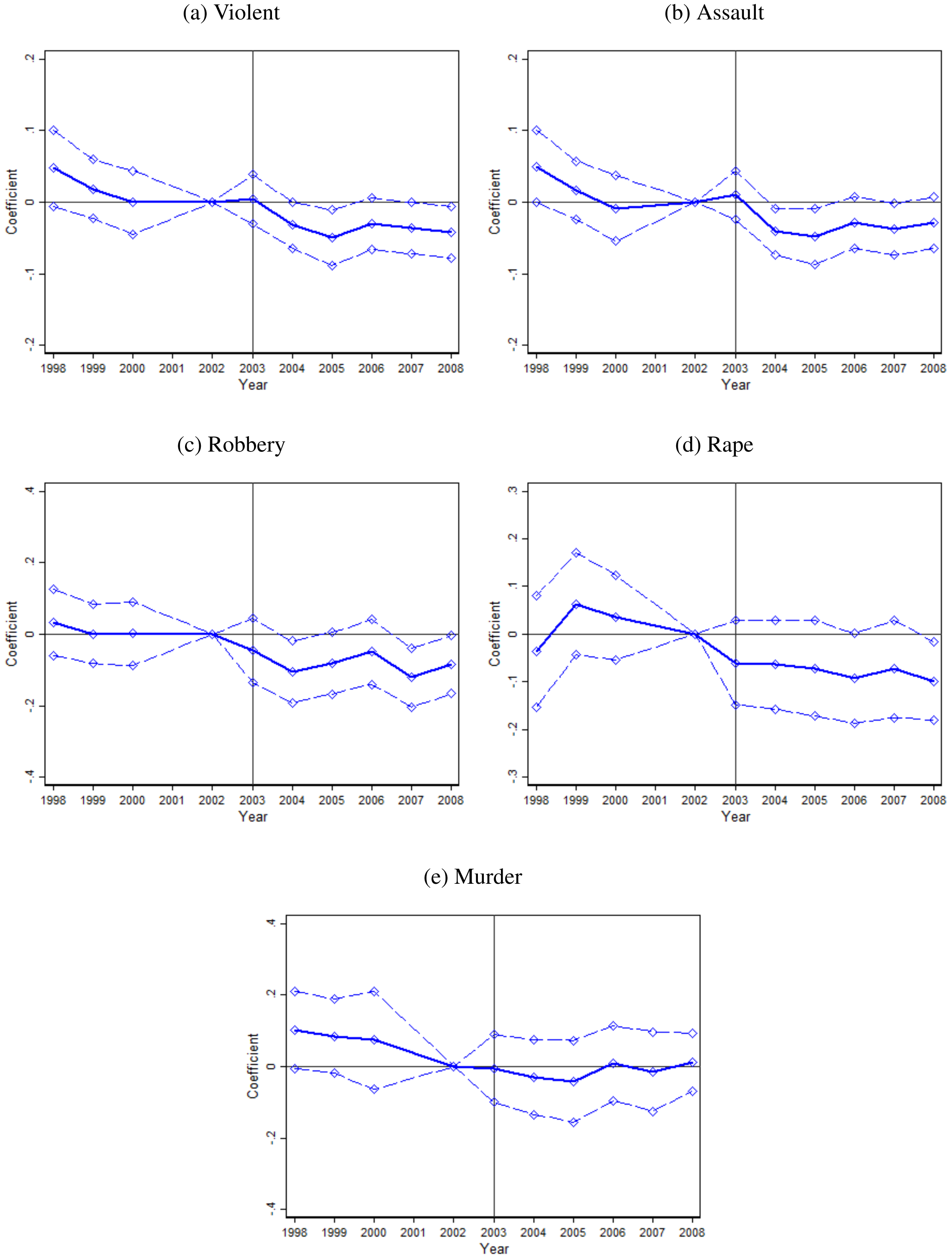
Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each observation represents a county×year×season cell. Dependent variable is the number of manslaughters per 1,000 people per county-season-year (in logarithm). **DDD** is the triple-difference estimator, which equals to 1 for all counties belonging to NBP 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.

Figure 1: NBP regions



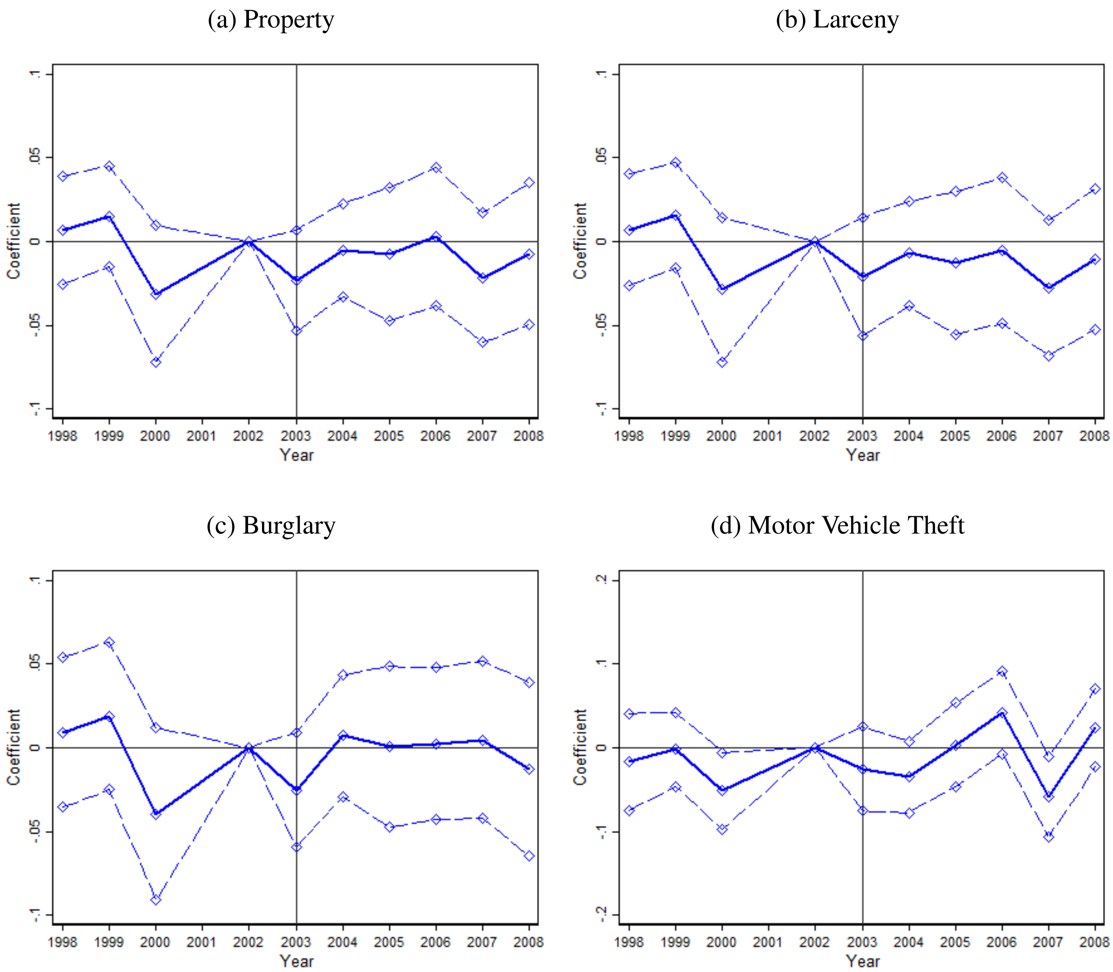
Note: Dark blue states are those participating in the NBP during the 2003-2008 period (the NBP states). Light blue states are not participating (non-NBP states). White states, which did not participate in the NBP but are adjacent to NBP states, are: Georgia, Iowa, Maine, Mississippi, Missouri, New Hampshire, 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



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: Fixed-effect estimates using counties with positive NO_x emissions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Violent	Assault	Robbery	Rape	Murder	Property	Larceny	Burglary	Motor
NO_x emissions	-0.001	-0.001	0.002	0.004	0.001	0.000	0.001	-0.003	0.001
	(0.002)	(0.002)	(0.004)	(0.004)	(0.006)	(0.001)	(0.001)	(0.002)	(0.003)
Observations	19,293	19,293	19,293	19,293	19,293	19,293	19,293	19,293	19,293
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.

Table A.2: IV estimates using counties with positive NO_x emissions

VARIABLES	(1) Violent	(2) Assault	(3) Robbery	(4) Rape	(5) Murder	(6) Property	(7) Larceny	(8) Burglary	(9) Motor
NO_x emissions	0.031**	0.026**	0.069***	0.059**	0.042	0.022	0.023	0.015	0.003
	(0.012)	(0.011)	(0.023)	(0.023)	(0.031)	(0.016)	(0.015)	(0.014)	(0.016)
Observations	19,293	19,293	19,293	19,293	19,293	19,293	19,293	19,293	19,293
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). NO_x emissions 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.

Table A.3: Impact of the NBP on NO_x emissions

VARIABLES	(1) NO_x	(2) NO_x	(3) NO_x
DDD	-1.687***	-1.692***	-1.703***
	(0.294)	(0.287)	(0.382)
Pre-2003 mean	5.144	5.144	5.144
Observations	10,089	10,089	10,089
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. 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.

Table A.4: Impact of the NBP on SO_2 and CO_2 emissions

VARIABLES	(1) SO_2	(2) SO_2	(3) SO_2	(4) CO_2	(5) CO_2	(6) CO_2
DDD	-1.019***	-0.964***	-1.019***	-36.659	-41.757*	-47.461
	(0.235)	(0.245)	(0.307)	(26.614)	(22.292)	(29.993)
Pre-2003 mean	14.411	14.411	14.411	2136.117	2136.117	2136.117
Observations	10,089	10,089	10,089	10,089	10,089	10,089
County-by-Season FE	Yes	Yes	Yes	Yes	Yes	Yes
Season-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Year FE	Yes	Yes	No	Yes	Yes	No
County-by-Year FE	No	No	Yes	No	No	Yes
Flexible Weather Controls	No	Yes	Yes	No	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. 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.

Table A.5: Sensitivity analysis (including adjacent states as the control group)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Violent	Assault	Robbery	Rape	Murder	Property	Larceny	Burglary	Motor
DDD	-0.018**	-0.016**	-0.044***	-0.053***	-0.024	-0.008	-0.012	-0.005	0.012
	(0.007)	(0.007)	(0.017)	(0.016)	(0.019)	(0.006)	(0.009)	(0.008)	(0.009)
Observations	40,574	40,574	40,574	40,574	40,574	40,574	40,574	40,574	40,574
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$\times$year$\times$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.

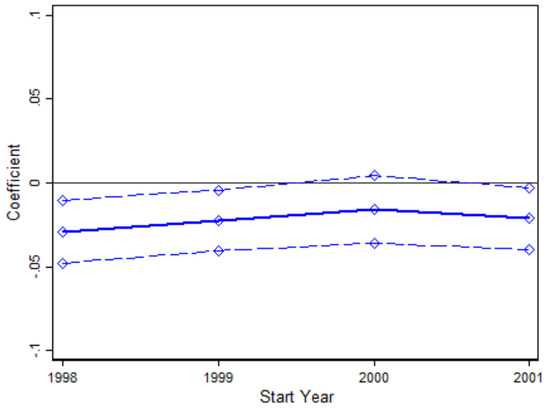
Table A.6: Sensitivity analysis (including adjacent states as the treatment group)

VARIABLES	(1) Violent	(2) Assault	(3) Robbery	(4) Rape	(5) Murder	(6) Property	(7) Larceny	(8) Burglary	(9) Motor
DDD	-0.023***	-0.019**	-0.071***	-0.060***	-0.019	-0.006	-0.009	-0.005	0.009
	(0.009)	(0.008)	(0.018)	(0.017)	(0.017)	(0.006)	(0.007)	(0.008)	(0.010)
Observations	40,574	40,574	40,574	40,574	40,574	40,574	40,574	40,574	40,574
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

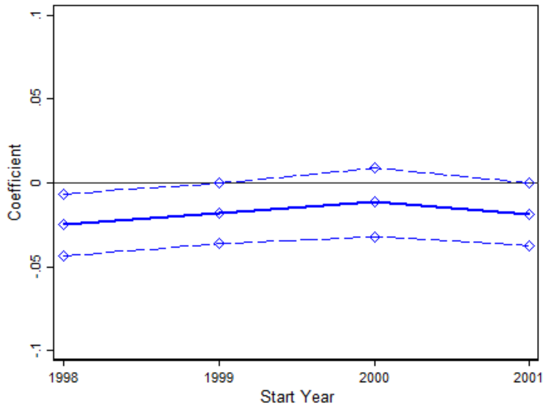
Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each observation represents a county×year×season cell. Dependent variables are the number of criminal activities per 1,000 people per county-season-year (in logarithm). **DDD** is the triple-difference estimator, which equals to 1 for all counties belonging 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.

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

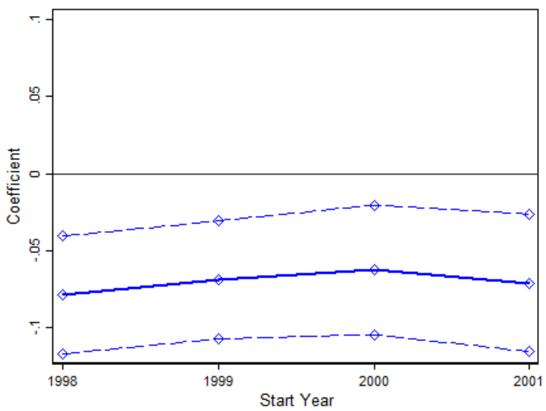
(a) Violent



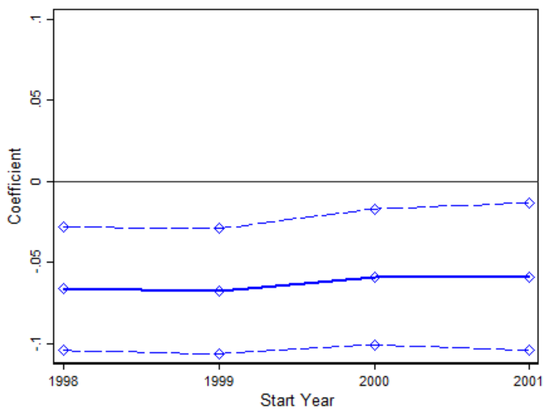
(b) Assault



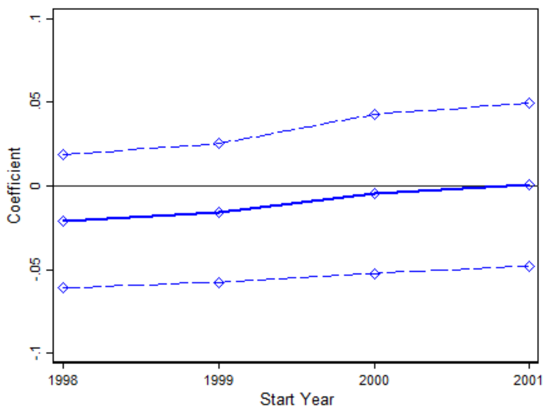
(c) Robbery



(d) Rape

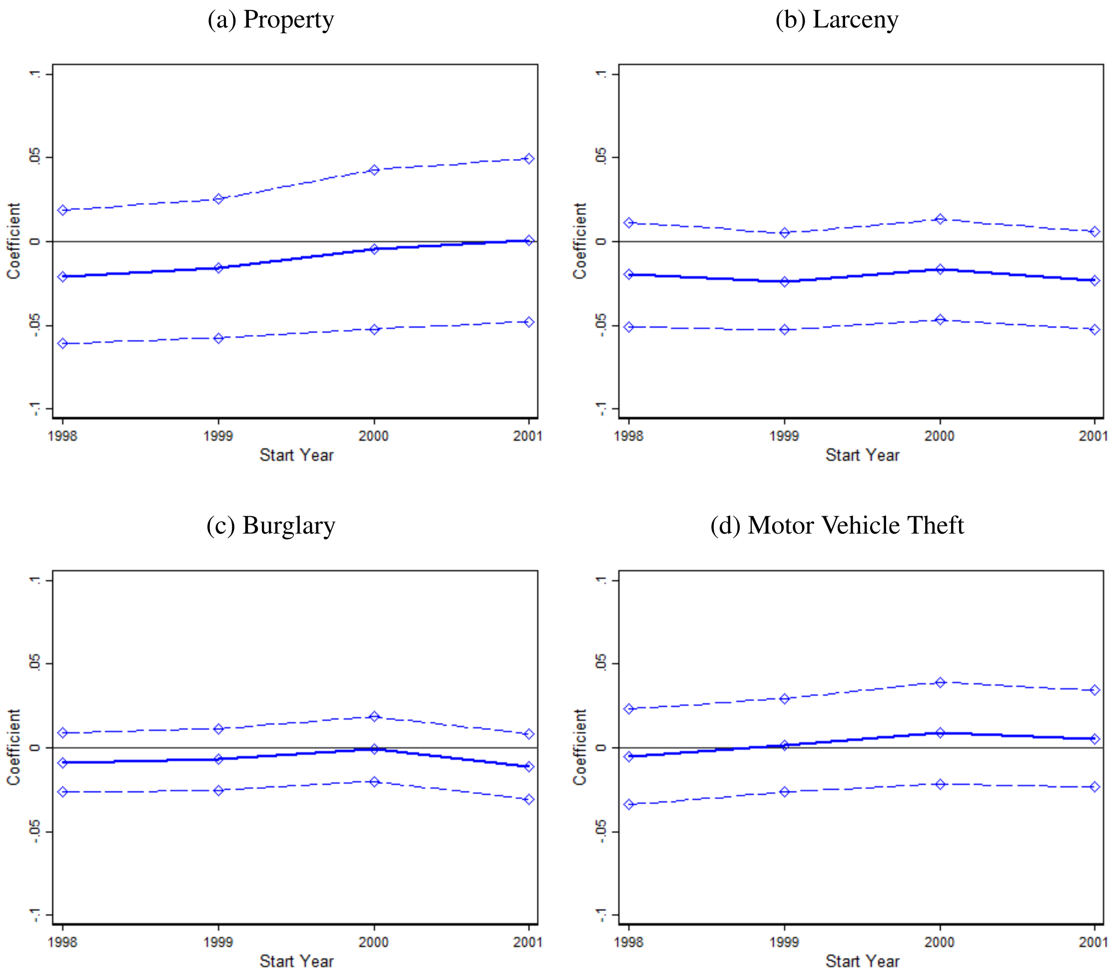


(e) Murder



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.