Broken Windows in Dallas: Intertemporal Spillovers of Crime

by

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(Under the Direction of Gregorio Caetano)

Abstract

Crime in the past can cause crime in the future. To determine this, I use a model developed by G. Caetano and Maheshri, 2018 and the test of exogeneity by C. Caetano, 2015 to select the best possible approach of predicting crime by using past crime. By using a data set from the Dallas Police department from 2014-2021, I rule out any broken windows theory in the short run. However, I am unable to rule out any long-run effects.

Index words: [CRIME, INTERTEMPORAL SPILLOVERS, EXOGENEITY]

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Chapter 1

Introduction

Economists have been interested in crime, in its determinants, and its associated economic outcomes. Many have determined that each crime has different costs and benefits. At the moment of committing a crime, individuals have to evaluate the cost of punishment and compare it against the reward of receiving something valuable. These are usually selfish acts that come at a cost that dictates whether or not they undertake such actions. There are instances that if an individual decides to engage in this act, they are reducing productivity in their work place (if they have one) and the work place of the victim and both of their possible near futures. They are also taking valuables which could be money, goods or even the life of a person. This individual receives in exchange for all these costs a short-run benefit such as food in their mouths or something else (Anderson, 1999). Between the decision and the action of committing the crime there are indicators that will allow for this individual to reach a decision faster.

Kelling, Wilson, et al., 1982 introduce the idea that signs of vandalism, and anti-social behavior (broken windows) can signal to more serious offenders that there is no law enforcement. Eventually, criminals settle in these abandoned neighborhoods where they can commit worse felonies. Policymakers are very intrigued in this model, because it can prevent many of their cities from having future misdemeanors. In theory, this would happen if they are able to control light crime. Braga et al., 2015 find that policing disorder strategies reduces crime in New York City. However, there are not many other cities in the United States that have adopted these type of policies. In this purview, this study tries to answer what is the effect of crime today on crime tomorrow in Dallas. Dallas is an ideal setting for the study because of its vast population and high growth rate compared to many cities in the United States.

I calculate this effect using G. Caetano and Maheshri, 2018's parametric version of C. Caetano, 2015's test of exogeneity for the identification strategy. The test of exogeneity is a new tool that was developed to determine if there is endogeneity in a relationship. I can rule out any broken windows theory in the short run (one lag). However, I am unable to rule out broken windows in the long run (two or more lags) because the identification strategy does not allow it. From the first lag, we determine that many crimes have effects on themselves like larceny, robbery, burglary, auto theft, assault and light crime which had statistically significant results, but there are not many effects across different crimes. I determine these results by using the Dallas Police Public Data from June 1, 2014 till November 1, 2021 (Departments, 2022).

This research will contribute to a substantial number of published works that identified an intertemporal relationship in anti-social behavior. G. Caetano and Maheshri, 2018 identified lagged crime rate in Dallas as an exogenous variable from 2000-2007. With the test of exogeneity, they selected a feasible regression and were able to identify a causal relationship between certain crimes and lagged crime rate. In this paper, we use the test of exogeneity in similar neighborhoods, but in different time periods.

The remainder of this paper is organized as follows. Chapter 2 gives a descriptive list of literature that is used to inspire the paper. Chapter 3 describes the theoretical model that the research is based on and it contains the empirical strategy as well as a very in-depth description of the test of exogeneity. Chapter 4 contains the description of the data for the applied model using data from 2014-2021 in Dallas, Texas. It also describes the process of aggregation and classification of crimes. Chapter 5 shows the empirical results. Chapter 6 includes some robustness checks for the test of exogeneity. Chapter 7 shows the link between the results and the broken windows theory. Finally, chapter 8 provides concluding remarks.

Chapter 2

Literature Review

As mentioned before, there are many researchers who are interested in determining what are the explanatory variables for the variable crime. Some researchers conclude that it can be the changes on wages or other incentives (Freeman, 1996, Grogger, 1995, Grogger, 1998, Ehrlich, 1973). Others identify education (Lochner and Moretti, 2004), or even increased population (Glaeser and Sacerdote, 1999) as variables that have a relationship with crime. The researchers that study the labor market effects on crime determine that when real wages decrease, there are larger effects on crime. During the periods studied, 1970s and 1980s, there was a decrease in earnings which, in turn, increased the likelihood on committing a crime. Later, Lochner and Moretti, 2004 determine that as real wages increase, the opportunity cost of committing a crime increases as well; so people prefer to educate themselves rather than to commit crimes. Additionally, researchers find that cities are the center of criminal activity because there is a greater quantity of wealthier people which creates higher opportunities to receive better compensation for risking your freedom. Glaeser and Sacerdote, 1999 demonstrate this by using elasticity of deterrence which affects crime as belief of future punishment increases.

Besides all of these characteristics that determine the likelihood of future crime, there are other variables that could identify crime. Researchers adopt lagged crime rate as a common variable to use in papers about crime (Fajnzylber et al., 2002, Buonanno and Montolio, 2008, G. Caetano and Maheshri, 2018). Lagged crime rate can condition future crime because of the broken windows theory (Kelling, Wilson, et al., 1982, Coles et al., 1998). When this theory was introduced, many economists of crime added it into their research as an explanatory variable. The definition is that having aspects of lighter crime, like vandalism (graffiti or broken windows) in neighborhoods, shows criminals that the neighborhood is not watched. In the future, these vandals could return and commit the same crimes without repercussions or possibly more serious crimes. Neighborhood disorder and quality of life have a causal relationship which is explained through the fear of its residents (Chappell et al., 2011).

The basis of the paper was on G. Caetano and Maheshri, 2018's research where they explain the effect of crime on future crime. The data set is comprised of Dallas crime from 2000 to 20007 where they estimated 6 types of crime: rape, robbery, burglary, auto theft, assault, and light crime on future levels. These past crimes are aggregated in terms of geography and times to prevent confounding variables. Since confounding variables could not vary in both geography and time (Bikhchandani et al., 1992, Ellison and Fudenberg, 1995), then it is important to aggregate the variable this way. Separating crimes is also crucial to the aggregation process since it could prevent aggregation bias (Buonanno and Montolio, 2008). In this paper, I use the same process of aggregation and the same identification strategy to answer a similar question with data from later years.

Chapter 3

Model

Crime is affected by many different aspects of life. I take inspiration from Becker, 1968 and G. Caetano and Maheshri, 2018 to develop this model. Becker, 1968 wrote a static model that described behavioral relations behind the costs of crime. On the other hand, G. Caetano and Maheshri, 2018 wrote a dynamic model of crime that described the intertemporal linkages of crime. As in G. Caetano and Maheshri, 2018, $\mathbb{C}$ is defined as the set of plausible crimes that an individual i in a neighborhood j could commit. The type of crime is defined as $y \in \mathbb{C}$ and they will commit crime in week t if their marginal benefit, exceeds their marginal cost,

$$B^y_{ijt} > p^y_{jt} C^y_{ijt}, \tag{3.1}$$

where B^y_{ijt} is the total private benefit, p^y_{jt} is the probability of punishment given that the individual committed the crime, and C^y_{ijt} is the cost of punishment.

Criminal history could give certain information to individuals that are considering committing a crime like features of the environment where the crime was committed. By the attainment of past knowledge of crimes, individuals could have beliefs of p^y_{jt}. These beliefs are denoted as π^y_{ijt}. Then, the total number of crimes is denoted as the following formula

$$\text{crime}^y_{jt} = \sum_{i \in I^y_{jt}} \mathbb{1}_{b^y_{ijt} > \pi^y_{ijt}}, \tag{3.2}$$

where I^y_{jt} represents potential criminals, $\mathbb{1}$ is the indicator function, and $b^y_{ijt} = \frac{B^y_{ijt}}{C^y_{ijt}}$ represents the individual's "benefit-cost factor" of committing a crime. The assumptions for this model about individual heterogeneity within the neighborhood are: (a) $\pi^y_{ijt} = \pi^y_{jt}$ is a common prior for all individuals within a neighborhood, and (b) b^y_{ijt} is an element of the cumulative distribution $F(.;\theta^y_{jt})$. The prior, π^y_{jt}, and the parameter of the distribution, θ^y_{jt}, may vary by neighborhood, week, and type of crime. So that

$$\text{crime}^y_{jt} = I^y_{jt} * (1 - F(\pi^y_{jt}; \theta^y_{jt})). \tag{3.3}$$

The parameters, $(I^y_{jt}, \theta^y_{jt}, \pi^y_{jt})$, describe the environment of crime and they can be affected by previous crime levels. crime_{jt-1} is denoted as the vector of crimes of all types in $t-1$ (of which xth element is crime^x_{jt-1}). I can express this as

$$I^y_{jt} = I^y(\text{crime}_{jt-1}, \eta^I_{jt-1}), \tag{3.4}$$

$$\theta^y_{jt} = \theta^y(\text{crime}_{jt-1}, \eta^\theta_{jt-1}), \tag{3.5}$$

$$\pi^y_{jt} = \pi^y(\text{crime}_{jt-1}, \eta^\pi_{jt_1}), \tag{3.6}$$

where η^I_{jt-1}, η^θ_{jt-1} and η^π_{jt-1} represent other (observable and unobservable) determinants of I^y_{jt}, θ^y_{jt} and π^y_{jt}, respectively.

These three equations have different reasons for crime to continue. Many researchers have identified some of these characteristics. For example, in equation (3.4) it is represented as the incapacitation effects (e.g., Levitt, 1998) which decrease the future pool of potential criminals because it leads to more arrests. Then, equation (3.5) represents private costs and benefits of crime generated by previous criminals. This is accounted by previous experiences (Kempf, 1987), peers' experiences (Glaeser et al., 1996), and history of responses of law enforcement on other criminals conditional on arrest. Finally, equation (3.6) represents the learning process of criminals that gets updated through their prior beliefs about crime. A perfect example of this is the theory of broken windows which is the central theme of this paper where neighborhoods' appearance signals to criminals that they can commit crimes (Kelling, Wilson, et al., 1982). However, this could also mean that neighbors and police could respond more to crime if it is observed (Taylor, 1996, Weisburd and Eck, 2004).

The previous equations can be combined into the total intertemporal relationship between crimes as

$$\begin{aligned}\frac{\partial\text{crime}^y_{jt}}{\partial\text{crime}^x_{jt-1}} = & \underbrace{\frac{\partial(I^y_{jt} * (1 - F(\pi^y_{jt};\theta^y_{jt}))) * \partial I^y_{jt}}{I^y_{jt} * \partial\text{crime}^x_{jt-1}}}_{\text{channel 1}} \\ & + \underbrace{\frac{\partial(I^y_{jt} * (1 - F(\pi^y_{jt};\theta^y_{jt}))) * \partial\theta^y_{jt}}{\partial\theta^y_{jt} * \partial\text{crime}^x_{jt-1}}}_{\text{channel 2}} \\ & + \underbrace{\frac{\partial(I^y_{jt} * (1 - F(\pi^y_{jt};\theta^y_{jt}))) * \partial\pi^y_{jt}}{\partial\pi^y_{jt} * \partial\text{crime}^x_{jt-1}}}_{\text{channel 3}}\end{aligned} \tag{3.7}$$

Each type of crime has a chance of having a different response depending on past and future crimes. For instance, vandalism happens more often than crimes like aggravated assault, but it could generate little incapacitation effects compared to the rest of the crimes. Furthermore, certain crimes like murder that happen in the heat of the moment might be less affected by incapacitation effects than burglary and auto theft (Blumstein and Blumstein, 1986). It is too early to determine if $(I^y_{jt}, \theta^y_{jt}, \pi^y_{jt})$ are used correctly in equation (3.7) because of the nature of the model. Usually, those factors cannot be controlled but policies can indirectly affect them. G. Caetano and Maheshri, 2018 explain that identifying the causal effects of

$\frac{\partial \text{crime}^y_{jt}}{\partial \text{crime}^x_{jt-1}}$ for each combination of x and y is how we determine the effect of crime on future crime. In the next section, there is an explanation for the identification strategy of $\frac{\partial \text{crime}^y_{jt}}{\partial \text{crime}^x_{jt-1}}$ by isolating the component of $Cov(\text{crime}^y_{jt}, \text{crime}^x_{jt-1})$ that is not attributable to $Cov(\eta^a_{ijt}, \eta^b_{jt-1})$, where *a* and *b* may correspond to I, θ, or π.

3.1 Empirical Strategy

G. Caetano and Maheshri, 2018 test empirically whether crime affects future crime levels using the system of equations described above. It is important to note that the system of equations summarizes the co-evolution of crimes of several types in a neighborhood, so they are able to describe the intertemporal linkage once crimes are identified. The equation of motion for crime y can be written as

$$\text{crime}^y_{jt} = \sum_{x \in K} \text{crime}^x_{jt-1}\beta^{xy} + \gamma \text{controls}^y_{jt} + \text{error}^y_{jt} \tag{3.8}$$

where β^{xy} denotes the effect of a crime of type x on a future crime of type y (in the rest of the paper, y represents the dependent crime variables and x represents explanatory crime variables), controls_{jt} is a vector of observed covariates, and error^y_{jt} includes all unobserved determinants of crime. Each observation in equation (3.8) is uniquely indexed by j, t, and y. I collect these equations and represent the system of equations of motion in matrix form as

$$\text{crime}_{jt} = \beta \text{crime}_{jt-1} + \Gamma \text{controls}_{jt} + \text{error}_{jt}, \tag{3.9}$$

where crime_{jt} is a $|\mathbb{C}| \times 1$ column vector. The parameter matrix β (whose (x, y) element is equal to β^{xy}) contains the $|\mathbb{C}|^2$ treatment effects of interest: the intertemporal effects of all crimes both within and across types of crimes.

It is naive to run an OLS estimator because there could be unobserved determinants of crime that persist over time. Some examples are neighborhood amenities, neighbors' characteristics, and law enforcement routines in each neighborhood. These determinants could cause a biased estimator of β. The solution for this problem is to use instrumental variables (IV) to identify β, however IVs are hard to identify correctly because they must be both valid and relevant. Also, it is difficult to prove in this context because there are $|\mathbb{C}|$ endogenous variables, so at least $|\mathbb{C}|$ separate IVs would be needed.

Because of this problem, the equation changes to an IV regression. Under the standard exogeneity assumption, β is identified:

Assumption 1. We have $Cov(\text{crime}^x_{jt-1}, \text{error}^y_{jt} | \text{crime}^{-x}_{jt-1}, \text{controls}_{jt}) = 0$ for all x, y, where crime^{-x}_{jt-1} is the vector $\text{crime}^{x'}_{jt-1}$ for all $x' \neq x$.

This assumption can be satisfied using a detailed dataset, however there is no way of knowing ex ante which model has enough information. The model in this paper can only fulfill this assumption if equation

(3.9) is represented correctly. To prove our model can fit our data, I follow the identification strategy by G. Caetano and Maheshri, 2018 which is guided by a formal test of Assumption 1 (C. Caetano, 2015). The outline is described below:

Step 1. Leveraging institutional and theoretical knowledge, as well as unique features of the data, we begin by considering a large subset of candidate models.

Step 2. For each candidate model, we test Assumption 1 using a formal test of exogeneity.

Step 3. I present the results of the model that survives the test of exogeneity.

I omit step 4. [1]

Step 5. I perform additional robustness checks with the particular goal of detecting confounders that are undetectable by the test. I find that the surviving model above is the only one that survives all the other checks.

Step 6. From our sensitivity analysis, I systematically catalog the necessary properties that any variable must possess to bias the results from the surviving model: it (a) must be undetectable by the test of exogeneity, (b) cannot be absorbed by controls, and (c) must survive the many robustness checks performed.

In total, this procedure allows me to reach the qualified conclusion that the surviving model is appropriate for causal inference of β by OLS as it is difficult to conceive of a variable that possesses the three properties above given our empirical evidence.

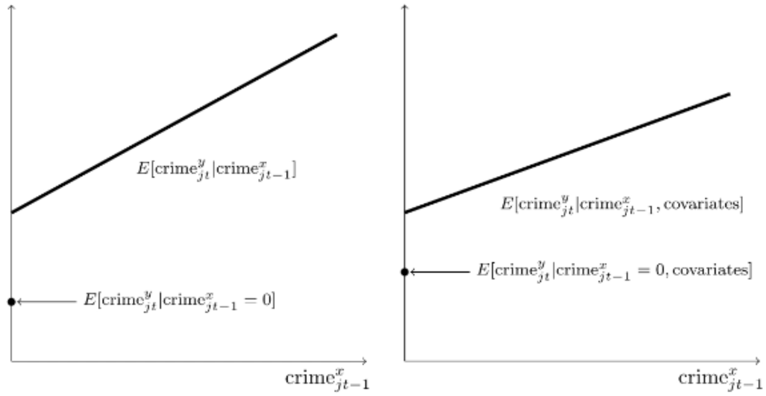
3.2 Testing the Exogeneity Assumption

There are many tests of exogeneity that have been developed over the years. Some require valid IVs (e.g., Hausman, 1978) and others require discontinuous variation of unobservable confounders around a known threshold value of the endogenous variable (C. Caetano, 2015). This paper follows the second approach on testing for the exogeneity assumption. Figure 3.1 describes the correlation of expected number of crimes y for each level of type x in a neighborhood. At the end, the test should provide the ability to describe the correlation as causation.

Assume that crime^x_{jt-1} has a continuous causal effect on crime^y_{jt} at $\text{crime}^x_{jt-1} = 0$ (this follows trivially from the specification of equation (3.9)). Then the discontinuity observed in the unconditional relationship between crime^x_{jt-1} and crime^y_{jt} (panel (a)) can be attributed to either observed covariates or unobserved confounders that vary discontinuously at $\text{crime}^x_{jt-1} = 0$. In panel b, it shows the unconditional relationship mentioned is applied on all observed covariates. Any remaining discontinuity observed at $\text{crime}^x_{jt-1} = 0$ can only be due to unobserved confounders that were not absorbed by the controls. Thus, finding a discontinuity after controlling for all covariates is equivalent to detecting endogeneity in the specification.

This test of exogeneity is easy to implement. Let d^x_{jt-1} be an indicator variable that is equal to 1 if $\text{crime}^x_{jt-1} = 0$, and let D_{jt-1} be the $|C| \times 1$ vector whose xth element is d^x_{jt-1}.

[1] Step 4. In G. Caetano and Maheshri, 2018, they presented a set of datasets, explaining that the failure to reject exogeneity does not imply exogeneity because there could still be confounders undetectable by the test that bias the results.



(a) Past crime and future crime, unconditional

(b) Past crime and future crime, conditional on covariates

Figure 3.1: Intuition of the test of exogeneity

The addition of these indicator variables in equation (3.9) helps in test Assumption 1,

$$\text{crime}_{jt} = \beta\text{crime}_{jt-1} + \Gamma\text{controls}_{jt} + \Delta D_{jt-1} + \epsilon_{jt}, \tag{3.10}$$

where Δ is a $|C|\times|C|$ matrix of parameters that represent the sizes of the discontinuities at $E[\text{crime}^y_{jt}|\text{crime}^x_{jt-1} = 0, \text{crime}^{-x}_{jt-1}, \text{controls}_{jt}]$ for all combinations of x and y. It follows that an F-test of whether all elements of Δ are equal to zero is equivalent to a test of Assumption 1.

Remark 1. The test of exogeneity requires that crime^x_{jt-1} has a continuous causal effect on crime^y_{jt} at $\text{crime}^x_{jt-1} = 0$; otherwise the parameters in Δ would incorporate the treatment effect. If this assumption did not hold, then all models would be rejected irrespective of whether they were endogenous or exogenous. Thus, the fact that some models survive the test is direct evidence that this assumption is valid. Conceptually, we believe that this assumption is valid in their context because every neighborhood crime is not necessarily observed by everyone (all neighbors, all potential criminals, etc.) and each person does not respond to this knowledge the same way. (For instance, the behavior of some potential criminals might be affected when $\text{crime}^x_{jt-1} = 1$, whereas the behavior of other potential criminals will be affected only when $\text{crime}^x_{jt-1} = 2$.) This will lead the effects we want to estimate, which will lead to represent the direct or indirect responses of these individuals to their knowledge of these crimes, to be smoothed away.

3.2.1 Power of the test

To determine that Assumption 1 is correct, the strategy is to determine the power of the test of exogeneity. The power of the test is the strongest attempt to fail to reject the null hypothesis of exogeneity.

Once this happens, it brings evidence that the null is correct. This will also ensure that we can detect endogeneity from all sources.

C. Caetano, 2015 explains that there are many settings to use the test of exogeneity. The setting for this power of the test is derived from the assumption that unobserved confounders vary discontinuously at $\text{crime}^x_{jt-1} = 0$ for some x. There will exist neighborhoods with $\text{crime}^x_{jt-1} = 0$ because they are wealthy enough to have law enforcement in that area. This would mean that there are neighborhoods that will be discontinuously different than the wealthy neighborhoods because of the crime. Having positive crime can generate bunching of neighborhoods with zero crime that may in turn lead to discontinuities in unobservable determinants of crime.

In Figure 3.2, there is an illustration of the intuition. The assumption is that the expected value of a particular unobservable for each level of a crime of type *x* is positive without loss of generality. The dashed line represents what the expected value of the unobservables would have been if it wasn't truncated at zero. This truncation shows that there is a discontinuity which aids the test in providing power to detect endogeneity from this source.

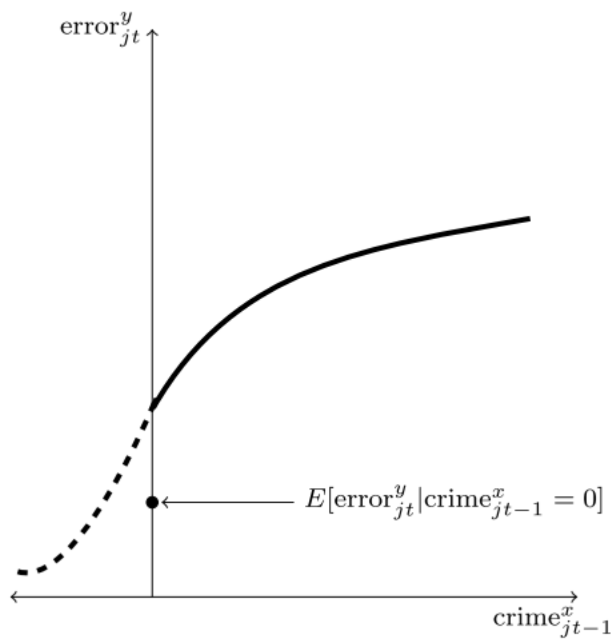


Figure 3.2: Why are unobservables discontinuous at $\text{crime}^x_{jt-1} = 0$?

Now, the test is composed of: $\mathbb{W}$ which is the set of all model confounders, defined as the set of variables w that are both correlated to crime^y_{jt-1} for some combination of x and y. Intuitively, the statistical power of the test is splitting this set into two disjoint subsets: $\mathbb{W}^D$, which contains variables that vary discontinuously at $\text{crime}^x_{jt-1} = 0$ for some *x*, and $\mathbb{W}^C$, which contains variables that vary continuously at $\text{crime}^x_{jt-1} = 0$ for all x. Subset $\mathbb{W}^D$ can be further split into $\mathbb{W}^D_1$, which contains variables that are correlated to crime^y_t when $\text{crime}^x_{t-1} = 0$ for some combination of x and y, and $\mathbb{W}^D_2$, which contains variables that are uncorrelated to $\text{crime}^x_{t-1} = 0$ for all combinations of x and y. The test of exogeneity can detect confounders only from $\mathbb{W}^D_1$, but not from the other two partitions ($\mathbb{W}^D_2$, $\mathbb{W}^C$). The significance of this is that the test can determine the power corresponds to the size $\mathbb{W}^D_1$ relative to $\mathbb{W}$. Depending on the data set, there could be "full" power of the test if all the confounders belong to $\mathbb{W}^D_1$ and the model could have a causal estimation. Figure 3.3 reflects what happens to a confounder in $\mathbb{W}^D_1$, $\mathbb{W}^D_2$, and $\mathbb{W}^C$. As the

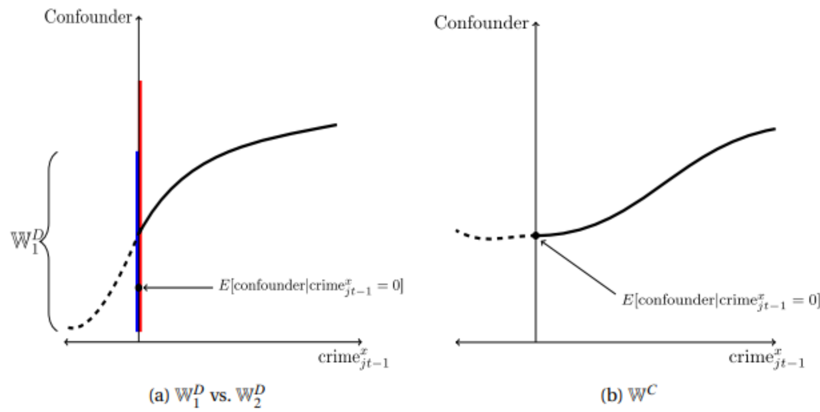


Figure 3.3: Types of confounders. $Notes$: Light gray region: Support of confounder among all observations of sample. Dark gray region: Support of confounder among all observations of sample with zero past crime.

number of crimes increases, the relative size of $\mathbb{W}^D_1$ grows while the other two partitions shrink. At the same time, the power of the test is increasing because it is using a multivariate setting. Panel (a) contains a confounder that is in either $\mathbb{W}^D_1$ or $\mathbb{W}^D_2$. The confounder at $\text{crime}^x_{jt-1} = 0$ varies discontinuously in both sets. The dashed line represents the average value of the confounder for each level of the propensity for past crime when it is negative, and the dot, as implied by the dashed line, represents the average value of this confounder across all observations with $\text{crime}^x_{jt-1} = 0$. In the left of the vertical axis, there are two regions: the light gray region which represents the support of the confounder among all observations of the sample and the dark gray region represents the support of the confounder among all observations of the sample with zero past crimes. By definition, a confounder is a variable that is correlated to crime^y_t in the light gray region (if it is also in the dark gray region, then the confounder belongs to $\mathbb{W}^D_1$). There are $|C|^2$ diagrams that follow this pattern, one for each of the combinations of x and y, and it is required that the confounder belongs only to the dark gray region, not the union of both regions (to account only for $\mathbb{W}^D_1$). Panel (b) shows a confounder belonging to $\mathbb{W}^C$. Here, the confounder is correlated to crime^x_{jt-1} but only when $\text{crime}^x_{jt-1} > 0$. When observations with $\text{crime}^x_{jt-1} > 0$ are such that $\text{crime}^{x'}_{jt-1} = 0$ for some x' and that w varies discontinuously at $\text{crime}^{x'}_{jt-1} = 0$, then the w will still be detectable by the test. The test has great power because of its multivariate setting.

Chapter 4

Data

The data set comes from the Dallas Police Public Data - RMS Incidents which begins June 1, 2014 till November 1, 2021 (Departments, 2022). Each incident contains a location (address or city block), a description of the crime, and the uniform crime reporting (UCR) classification that is given by a police officer. It also lists the times of the incident, of the report, when it was entered into the system, when the call was received, when the call was cleared and when police were dispatched. This can help construct the time sequence of crime, the police response and the neighborhood response. Also, the data is filtered by the data provider to not contain sexually oriented offenses, offenses where juveniles or children (individuals under 17 years of age) are the victim or suspect, listing of property items that are considered evidence, social service referral offenses, and identifying vehicle information in certain offenses.

4.1 Aggregation

For the strategy to work, the variables must be aggregated. The data provides individual-type data and by using neighborhood boundaries and time periods, the type of crimes can be aggregated. Even though disaggregated models can be easily interpretable and have less chance of having heterogeneity, they have a higher number of controls. More controls increase the number of models that this study has to perform. It is better to aggregate by neighborhood, type of crime and time period, so it can be easier and faster to test. With the test of exogeneity, the number of models decreases which simplifies the analysis.

4.1.1 Classification of Crimes

Crimes have many definitions in the data set; they can be very specific or very general. This brings problems to the analysis because it makes it difficult to measure the type of crime. Therefore, the solution is to use the UCR classification. Here, crimes are classified as heterogeneous subsets. The crimes that are being used for the analysis are: larceny, burglary, robbery, auto theft, assault, and light crime. Assault is defined as both aggravated and simple assault because there could be potential misclassification (Zimring, 1998). Larceny is composed of pocket-picking, purse-snatching, shoplifting, theft from building, theft from coin-operated machine or device, theft from motor vehicle, theft of motor vehicle parts or accessories and all other larceny. Light crime is composed of disorderly conduct and drunkenness.

The reason for this classification is to observe if there is a spillover effect from lighter crimes to more severe crimes. This allows us to test the broken windows theory because there are both lighter and more severe crimes. Additionally, these crimes occur more frequently than other crimes like arson. The crimes selected would have less variance than those crimes that occur less frequently.

4.1.2 Neighborhood boundaries

It was determined that neighborhoods have to be composed of areas that are large enough. This can bring spillover effects, but it is also important to have areas where $\text{crime}^{x}_{jt-1} = 0$. It can't be too broadly defined nor too finely defined, so the solution is to use the beat definition from the Dallas Police Department (DPD). Dallas is divided into seven divisions where each division is divided into sectors. Then, each sector is divided into smaller areas called beats and each beat contains neighborhoods which are called reporting areas. The data is aggregated using both beats and sectors. This is important because there can be neighborhoods that are as big as a sector or as small as a beat, so I run the test of exogeneity in both.

4.1.3 Time periods

Time has to be defined as short as possible. The reason is that it is easier to incorporate short-run intertemporal spillovers. However, there are disadvantages to this: First, there could be longer effects that are missed from cutting the time too short and it might be necessary to add lagged variables to equation (3.9) which increases the number of parameters that need to be estimated. A shorter time like an hour or 5 minutes might impede us from truly measuring the effects of local crimes from an incident database. Therefore, temporal aggregation is a week which can preserve some heterogeneity and contains many periods.

4.2 Controls

After deciding the levels of $\mathbb{C}$, j, and t, control_{jt} has to be decided. To decide, I turn once again to G. Caetano and Maheshri, 2018. They use the theory of social interactions to consider which model to use. The theory describes that there are two intertemporal links that operate in different levels of aggregation. For instance, β is identified both in fine spatial and temporal levels, however confounding effects only happen either in fine spatial or temporal levels. Crime can only spread through individual and social channels, but the spread will dissipate if the social distancing is too great. Social distance is both correlated with spatial distance (Akerlof, 1997) and temporal distance (Ellison and Fudenberg, 1995), so the causal effect stays close to where the crime happened, and it is strongest immediately after it happens. Juxtaposing confounders of crimes can be found at more aggregated levels in at least of the dimensions mentioned. For example, demographic compositions change slowly over time, and judicial institutions vary at larger geographic levels. The differences in aggregation should help to specify fixed effects that can control confounders and help to fail to reject Assumption 1.

4.3 Summary statistics

Below are the summary statistics aggregated to the sector-week level in Table 1. Larceny is the most frequent crime out of the six with 7.82 crimes reported in a sector per week. There are 61% of sector-week observations that have zero crime in this data set. There are 30 sectors and 387 weeks and there are no missing values in each week or each sector.

The average police response time is approximately 1.33 hours. Light crime has a faster response time of approximately 0.44 hours than the other crimes. On average, police respond to larceny in 1.52 hours which is similar to the time of assault. Robbery and burglary take the longest which is 2.15 hours. The police complete criminal cases at an average of approximately 3 hours. The fastest crime to be completed is larceny with 2.81 hours.

Table 4.1: Summary Statistics: 2014-2021

Variables	**Larceny**	**Robbery**	**Burglary**	**Auto Theft**	**Assault**	**Light Crime**
Avg. reported crimes in a sector per week	7.82	1.30	2.98	6.61	2.48	1.21
	(7.38)	(1.95)	(3.28)	(6.95)	(2.82)	(2.55)
Avg. police response time (hours)	1.52	0.97	1.84	2.15	1.08	0.44
	(3.62)	(3.90)	(4.31)	(4.53)	(3.84)	(2.65)
Avg. police duration (hours)	2.81	3.26	3.03	3.28	3.10	3.03
	(2.47)	(2.34)	(2.47)	(2.95)	(2.72)	(2.02)
Frac. of crimes committed at night	0.43	0.59	0.47	0.53	0.47	0.62
Frac. of crimes committed on the weekend	0.24	0.32	0.22	0.26	0.31	0.41
Total	90,814	15,089	34,634	76,750	28,817	14,008
Number of Sectors	30					
Number of Beats	192					
Number of Weeks	387					
Percentage of obs. sector-week that respective crime is 0	4.46%	41.22%	12.53%	5.54%	15.86%	48.32%
Percentage of obs. beat-week that respective crime is 0	50.8%	86.75%	70.45%	67.44%	74.15%	86.96%

Note: Standard deviations are presented in parentheses. Average police response is measured from when the police received the 911 call to when they dispatched. Average police police duration is measured by when the 911 call was received to the time the 911 call was cleared. Night time is defined as 8:00 p.m.-8:00 a.m.

Chapter 5

Empirical Results

5.1 Test of Exogeneity

First, the table of exogeneity contains six specifications where $\mathbb{C}$ = larceny, robbery, burglary, auto theft, assault, light crime, $j \in beat, sector$, and $t =$ week. $control_{jt}$. In I, there are no controls. In II, there are type-of-crime fixed effects. In III, there is an addition of year type of crime fixed effects to the second model which is close to other attempts of identifying intertemporal relationships between crimes (Funk and Kugler, 2003). In IV, there is an addition of neighborhood type of crime fixed effects to III. This is done to get neighborhood characteristics that have not changed over the sample period. In V, there is an addition of week type of crime and neighborhood type of crime fixed effects. Finally, in VI, there is an addition of both division-week type of crime and neighborhood-year type of crime fixed effects into the model.

Table 5.1: Tests of exogeneity.

	Specifications (p-Values in Parentheses)					
	I	II	III	IV	V	VI
j = beat, t = week	407.10	429.02	97.26	178.74	248.57	2.03
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
j = sector, t = week	25.04	27.45	18.14	23.97	33.68	1.18
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(**0.44**)

Note: The table shows shows the F-statistic and p-value of the tests of exogeneity for various specifications of equation (3.10). Bold represent the entries that "survived" which means that they cannot reject exogeneity at typical significance levels. Each column contains each specification: I, no fixed effects; II, fixed effects at the c level; III, fixed effects at the $year \times c$ level; IV, fixed effects at the $year \times c$ and at the $j \times c$ levels; V, fixed effects at the $j \times c$ levels; VI, fixed effects at the $J \times t \times c$ and at the $j \times T \times c$ levels, where J = division and T = year. All standard errors are clustered at the $j \times year \times c$ level.

In table 5.1, there is a better visualization of what I describe above. I represent the F-statistics and their respective p-values for each test of exogeneity. From this test, there is only one surviving model, model VI. The specification VI is

$$\mathbb{C} = \text{larceny, robbery, burglary, auto theft, assault, light crime},$$

$$j = sector,$$

$$t = week,$$

$$\text{controls}_{jt} = \Lambda_{\text{division-week type of crime}}, \Lambda_{\text{sector-year type of crime}}.$$

The $\Lambda_{\text{division-week type of crime}}$ absorbs all time-varying determinants of each crime that vary across the police divisions of Dallas, and the $\Lambda_{\text{sector-year type of crime}}$ absorbs all neighborhood-specific determinants of each crime that vary on an annual basis like demographic characteristics. In other words, the potential for omitted variable bias is very small because the variable would have to vary across weeks within a year and across sectors within a division. The test of exogeneity shows that there is no evidence of such omitted variable bias, so the estimates of this model can be interpreted as causal if the test is powerful enough.

Table 5.2: Intertemporal effects of crimes.

	Larceny$_t$	**Robbery**$_t$	**Burglary**$_t$	**Auto Theft**$_t$	**Assault**$_t$	**Light Crime**$_t$
Larceny$_{t-1}$	0.341	0.001	0.024	0.114	-0.005	0.028
	(0.049)**	(0.008)	(0.009)	(0.028)*	(0.009)	(0.014)
Robbery$_{t-1}$	0.034	0.136	0.155	0.215	0.037	-0.003
	(0.046)	(0.039)*	(0.031)**	(0.67)*	(0.030)	(0.023)
Burglary$_{t-1}$	0.031	0.060	0.223	0.126	0.001	-0.021
	(0.024)	(0.018)*	(0.025)**	(0.030)*	(0.015)	(0.014)
Auto theft$_{t-1}$	0.082	0.017	0.032	0.262	0.011	-0.001
	(0.015)**	(0.007)	(0.012)*	(0.037)**	(0.011)	(0.005)
Assault$_{t-1}$	-0.053	0.038	0.065	0.024	0.168	0.037
	(0.030)	(0.012)*	(0.020)*	(0.048)	(0.014)**	(0.008)**
Light crime$_{t-1}$	0.206	0.019	-0.102	0.041	0.107	0.710
	(0.109)	(0.019)	(0.044)	(0.031)	(0.028)*	(0.008)**
R^2				0.7897		
Number of observations				69,480		

Note: This table shows the estimated intertemporal effects of various crimes in week $t-1$ on crime levels in week t (i.e., the parameter matrix β). Fixed effects at the division-week type of crime and sector-year type of crime are included in each of the six equations, which are estimated simultaneously by seemingly unrelated regression. All errors are clustered at the sector-year type of crime level. **, significant at the 99% level; *, significant at the 95% level.

Table 5.2 contains estimates of the intertemporal effects of crime for the model that survived the test of exogeneity. There are some within-crime intertemporal effects for larceny, robbery, burglary, auto theft, and assault. This means that these crimes only generate additional a large range of the same type of crime in the following week. For instance, robbery today increases robbery tomorrow by 13.6%, all else constant. There is one piece of evidence that lighter crime has a positive effect on severe crime in a 95% confidence level. Having a statistically significant observation in the last row would mean that there is a broken windows effect. However, this is shut down in the next table. This model has a 79% explanatory power which shows a strong relationship between the variable's movements.

There is more context of these effects in Table 5.3 where they are expressed as semi-elasticities of 1-week crime elimination. There, the total spillover effect of eliminating all crimes of a given type x in an average week on each crime of type y is computed. The results are presented as a percentage of crime of type y in a single week in an average Dallas neighborhood taking into account all cumulative effects. For example, if eliminating assaults in week t will incorporate the reduction of robberies in week $t+1$, then the calculation related to the elimination of assaults in t will incorporate the reduction of burglaries in week $t+2$ that was due to the corresponding reduction of robberies in week $t+1$. In this table, the point estimates of all the semi-elasticities tend to be very small. For example, a reduction of 6.61 auto thefts will generate a total future reduction in burglary of 1.08% in a single week in the average neighborhood in Dallas. Even though these effects are close to zero, the precision allows us to rule out spillover reductions of modest intertemporal spillovers. The previous table shows that eliminating lighter crime reduces assault, however in table 5.3, the confidence interval shows evidence against that claim. The 95% confidence interval includes 0 which means that there is a reasonable possibility that there is no effect between lighter crime and assault. This allows for the elimination of sizable self-sustaining reductions in severe crimes which is part of the broken windows theory.

Table 5.3: Full long-run reduction in crime^y from a 1-week elimination of crime^x.

	Effect on crime^y					
crime^x	Larceny	Robbery	Burglary	Auto Theft	Assault	Light Crime
Eliminate 7.82 larcenies	9.06%	0.28%	-0.18%	2.19%	-1.84%	1.10%
	[5.45, 14.36]	[-0.88, 2.07]	[-1.86, 2.40]	[0.43, 4.93]	[-3.33, 0.52]	[-0.61, 4.31]
Eliminate 1.30 robberies	-0.80%	2.67%	3.30%	-2.84%	-1.38%	-0.38%
	[-6.83, 9.03]	[-0.60, 7.39]	[-1.18, 9.88]	[-7.34, 3.92]	[-5.98, 5.24]	[-3.45, 5.83]
Eliminate 2.98 burglaries	3.53%	3.18%	9.00%	1.09%	3.47%	2.06%
	[-1.07, 10.60]	[1.16, 6.15]	[5.59, 13.79]	[-2.06, 5.73]	[0.58, 7.67]	[-3.64, 1.43]
Eliminate 6.61 auto thefts	3.19%	1.89%	1.08%	5.35%	-1.40%	0.37%
	[-0.91, 9.51]	[0.22, 4.39]	[-1.43, 4.79]	[2.24, 9.78]	[-3.59, 1.95]	[-1.45, 4.04]
Eliminate 2.48 assaults	-0.24%	-0.90%	-3.35%	-1.36%	3.54%	2.49%
	[-4.36, 6.36]	[-2.80, 1.93]	[-6.19, 0.82]	[-4.42, 3.15]	[1.17, 7.20]	[0.15, 6.99]
Eliminate 1.21 light crimes	8.35%	-1.91%	-2.71%	-4.57%	1.64%	45.61%
	[-4.04, 31.05]	[-4.94, 4.42]	[-6.43, 5.53]	[-9.17, 5.15]	[-3.31, 11.43]	[23.38, 80.48]

Note: Reductions are calculated by hypothetically eliminating all crime type *x* in the average neighborhood in the sample for 1 week, computing the total number of future crimes of each type *y* that is reduced in that neighborhood, and dividing by the average number of weekly crimes of type *y* in a neighborhood in the sample. Positive values correspond to long-run reductions in crime. The 95% confidence intervals of these effects are presented in brackets.

5.1.1 Longer-run effects: Dynamic spillovers of crime

Criminal activity can have intertemporal consequences that last longer than a single period of time. In this case, the equations of motion (3.10) would be misspecified. This would affect the interpretation of β as the full intertemporal effects of crime which could also lead to an endogeneity problem. To go deeper into this, it is necessary to generalize equation (3.10) as

$$\text{crime}_{jt} = \sum_{\tau=1}^{\bar{\tau}} \beta_\tau \text{crime}_{jt-\tau} + \sum_{\tau=1}^{\bar{\tau}} \Delta_\tau D_{jt-\tau} + \Gamma \text{controls}_{jt} + \epsilon_{jt}, \tag{5.1}$$

where $\bar{\tau}$ includes the maximum amount of direct, long-run effects of crime.

In Table 5.4, it contains the results of two tests for $\bar{\tau}$ from 2 to 6 and 6 specifications of fixed effects for *j* = sector and *t* = week. The process is as follows, there is a test of exogeneity where we test whether all $|\mathbb{C}|^2 * \bar{\tau}$ elements of $\Delta_1, \ldots, \Delta_{\bar{\tau}}$ are equal to 0. After that, the *p*-value of this test is presented at the top of each test. In this case, none of the models survive this test which goes against the model in G. Caetano and Maheshri, 2018. Later, it tests if the elements of β_τ for all $\tau \leq \bar{\tau} - 1$ are the same as their counterparts in the corresponding model with only $\bar{\tau} - 1$ lags. It is represented in brackets at Table 5.4. Here, the specification VI matches with G. Caetano and Maheshri, 2018 for all the lags. The test consists of jointly testing whether any of the 36 coefficients in each of the first three lags change from one lag to the next. So, if there is a confounding omitted variable that is not detected by the test would need to be correlated to crime_{jt-6}.

Table 5.4: Sensitivity tests: longer-run effects.

	Specifications of Fixed Effects					
$\bar{\tau}$	I	II	III	IV	V	VI
$\bar{\tau} = 2$	0.00	0.00	0.00	0.00	0.00	0.00
	[0.00]	[0.00]	[0.00]	**[0.53]**	**[0.57]**	**[1.00]**
$\bar{\tau} = 3$	0.00	0.00	0.00	0.00	0.00	0.00
	[0.00]	[0.00]	[0.00]	**[1.00]**	**[1.00]**	**[0.52]**
$\bar{\tau} = 4$	0.00	0.00	0.00	0.00	0.00	0.00
	[0.01]	**[0.19]**	**[0.34]**	**[1.00]**	**[0.40]**	**[1.00]**
$\bar{\tau} = 5$	0.00	0.00	0.00	0.00	0.00	0.00
	[0.44]	**[1.00]**	**[1.00]**	**[1.00]**	**[1.00]**	**[1.00]**
$\bar{\tau} = 6$	0.00	0.00	0.00	0.00	0.00	0.00
	[1.00]	**[1.00]**	**[1.00]**	**[1.00]**	**[1.00]**	**[1.00]**

Note: The table shows shows the *p*-values of two tests associated with the generalized equation of motion of crime, equation (3.11), with various fixed effects (columns) and lags (rows). The first *p*-value listed in each cell is for the test of exogeneity for $\Delta_1, ..., \Delta_{\bar{\tau}}$ (*p*-values in bold denote surviving models for elements of $\beta_\tau, \tau = 1, ..., \bar{\tau} - 1$, in the listed model are equal to the respective elements of $\beta_\tau, \tau = 1, ..., \bar{\tau} - 1$, when the lag $\bar{\tau}$ is excluded (*p*-values in bold denote that we cannot reject that all parameters are the same at the 5% level). The specifications of controls are the same as those described in Table 4.1 for *j* = sector and *t* = week. Each one specifies fixed effects at different levels: I, no fixed effects; II, fixed effects at the *c* level; III, fixed effects at the year $\times c$ level; IV, fixed effects at the year $\times c$ and at the $j \times c$ levels; V, fixed effects at the $t \times c$ and at the $j \times c$ levels; VI, fixed effects at the $J \times t \times c$ and at the $j \times T \times c$ levels, where *J* = division, T = year. All errors are clustered at the $j \times year \times c$ level.

It is necessary to check for the long-run effects of $\bar{\tau}$ because it is not obvious ex ante if it is an appropriate value. The test consists of checking if $\bar{\tau}$ can reject that all 36 variables of $\beta_{\bar{\tau}} = 0$. Table 5.5 contains the results of these tests which shows the surviving model VI capturing all the long-run effects at $\bar{\tau} = 4$. This is only meaningful for the surviving model VI because the other models failed that first test of exogeneity. Since they failed that test, then they cannot be interpreted as causal. Even though the rest of the lags failed the test of exogeneity, I present all the models to show a complete view of the estimates. It is plausible that there are estimated spillovers from the nonsurviving models that last 6 weeks or more. This fact is essential evidence that these models are biased by persistent confounders.

Table 5.5: Should the $\bar{\tau}$th lag be included?

	Specifications of Fixed Effects					
Num. of Included Lags ($\bar{\tau}$)	I	II	III	IV	V	VI
$\bar{\tau} = 2$	0.00	0.00	0.00	0.00	0.00	0.00
$\bar{\tau} = 3$	0.00	0.00	0.00	0.00	0.00	0.00
$\bar{\tau} = 4$	0.00	0.00	0.00	0.00	0.00	0.00
$\bar{\tau} = 5$	0.00	0.00	0.00	0.00	0.00	**0.36**
$\bar{\tau} = 6$	0.00	0.00	0.00	0.00	0.00	**0.09**

Note: This table shows the p-value for a test of whether all 36 elements of $\beta_{\bar{\tau}} = 0$ for various specifications of equation 3.11 (p-values in bold denote that we cannot reject that all parameters are zero at the 5% level). These 6 specifications are the same as those described in Table 5.1 for j = sector and t = week. Each one contains fixed effects at different levels: I, no fixed effects; II, fixed effects at the c level; III, fixed effects at the year $\times c$ level; IV, fixed effects at the year $\times c$ and at the $j \times c$ levels; V, fixed effects at the $t \times c$ and at the $j \times c$ levels; VI, fixed effects at the $J \times t \times c$ and at the $j \times T \times c$ levels, where J = division, T = year. All errors are clustered at the $j \times year \times c$ level.

Table 5.6 and Table 5.7 includes the estimates of $\hat{\beta}_1, \ldots, \hat{\beta}_4$ of the surviving model. These results are fairly similar to Table 5.2. There are some statistically significant long-run effects in larceny, burglary, auto theft and light crime. Nevertheless, there is no evidence for long-run across-crime intertemporal effects. Furthermore, results show that intertemporal effects last at most 4 weeks, but still results could disappear in the 5th or 6th week and come back in the 7th. In G. Caetano and Maheshri, 2018, they explain that these results would not be economically significant.

Table 5.6: Intertemporal effects of crimes: four lags.

	Larceny_t	Robbery_t	Burglary_t	Auto Theft_t	Assault_t	Light Crime_t
			(a) $\hat{\beta}_1$			
Larceny_{t-1}	0.075	-0.004	0.005	0.032	-0.006	0.016
	(0.021)**	(0.007)	(0.010)	(0.018)	(0.009)	(0.016)
Robbery_{t-1}	-0.032	0.039	0.058	0.070	-0.025	-0.004
	(0.037)	(0.022)	(0.026)*	(0.046)	(0.026)	(0.016)
Burglary_{t-1}	-0.009	-0.003	0.077	0.010	-0.030	-0.012
	(0.030)	(0.013)	(0.017)**	(0.036)	(0.018)	(0.010)
Auto theft_{t-1}	0.039	-0.010	0.008	0.064	-0.007	-0.007
	(0.013)**	(0.006)	(0.008)	(0.019)**	(0.008)	(0.005)
Assault_{t-1}	-0.035	-0.024	0.024	-0.029	0.022	0.008
	(0.026)	(0.013)	(0.018)	(0.037)	(0.017)	(0.012)
Light Crime_{t-1}	0.090	-0.015	-0.028	0.048	0.035	0.220
	(0.043)*	(0.018)	(0.024)	(0.044)	(0.029)	(0.037)**

(*Continues*)

Table 5.7: Intertemporal effects of crimes: Continued.

	Larceny_t	Robbery_t	Burglary_t	Auto Theft_t	Assault_t	Light Crime_t
			(a) $\hat{\beta}_2$			
Larceny_{t-2}	0.022	0.007	0.011	0.018	-0.013	-0.003
	(0.020)	(0.007)	(0.011)	(0.020)	(0.008)	(0.007)
Robbery_{t-2}	0.124	-0.017	-0.005	0.052	-0.000	-0.009
	(0.037)**	(0.021)	(0.024)	(0.043)	(0.024)	0.017
Burglary_{t-2}	0.049	-0.005	-0.032	0.056	0.012	-0.015
	(0.029)	(0.013)	(0.018)	(0.030)	(0.019)	(0.011)
Auto theft_{t-2}	0.009	0.007	0.006	0.018	-0.007	0.001
	(0.015)	(0.006)	(0.008)	(0.022)	(0.008)	(0.006)
Assault_{t-2}	-0.005	0.019	0.011	0.027	-0.020	0.005
	(0.024)	(0.013)	(0.17)	(0.034)	(0.018)	(0.009)
Light Crime_{t-2}	-0.061	0.013	0.014	-0.065	0.005	0.159
	(0.037)	(0.022)	(0.024)	(0.046)	(0.029)	(0.027)**

(*Continues*)

Table 5.8: Intertemporal effects of crimes: four lags. Continued.

	Larceny_t	Robbery_t	Burglary_t	Auto Theft_t	Assault_t	Light Crime_t
			(a) $\hat{\beta}_3$			
Larceny_{t-3}	0.038	-0.008	0.007	0.050	0.002	0.005
	(0.020)	(0.007)	(0.010)	(0.021)*	(0.009)	(0.008)
Robbery_{t-3}	-0.099	-0.021	0.010	-0.093	-0.043	-0.009
	(0.049)*	(0.021)	(0.028)	(0.050)	(0.024)	(0.017)
Burglary_{t-3}	0.015	-0.005	0.036	-0.005	0.002	0.010
	(0.026)	(0.013)	(0.016)*	(0.036)	(0.016)	(0.013)
Auto theft_{t-3}	-0.004	-0.007	-0.001	-0.013	0.018	-0.008
	(0.013)	(0.006)	(0.008)	(0.016)	(0.010)	(0.005)
Assault_{t-3}	-0.008	0.013	-0.004	-0.036	0.004	-0.010
	(0.028)	(0.011)	(0.015)	(0.039)	(0.017)	(0.012)
Light Crime_{t-3}	-0.044	0.021	-0.035	0.064	0.010	0.124
	(0.053)	(0.018)	(0.033)	(0.047)	(0.022)	(0.033)**

(*Continues*)

Table 5.9: Intertemporal effects of crimes: Continued.

	Larceny_t	Robbery_t	Burglary_t	Auto Theft_t	Assault_t	Light Crime_t
			(a) $\hat{\beta}_4$			
Larceny_{t-4}	0.019	0.010	0.005	-0.003	0.004	0.011
	(0.016)	(0.008)	(0.011)	(0.020)	(0.010)	(0.007)
Robbery_{t-4}	0.062	-0.014	0.030	0.010	-0.037	0.032
	(0.041)	(0.022)	(0.024)	(0.044)	(0.025)	0.017
Burglary_{t-4}	-0.035	-0.002	-0.017	0.036	0.020	-0.004
	(0.028)	(0.012)	(0.017)	(0.035)	(0.016)	0.010
Auto theft_{t-4}	0.008	0.009	0.021	0.017	-0.005	-0.004
	(0.015)	(0.006)	(0.009)*	(0.017)	(0.007)	(0.005)
Assault_{t-4}	0.013	0.008	-0.032	0.017	0.008	0.001
	(0.31)	(0.013)	(0.018)	(0.034)	(0.019)	(0.012)
Light Crime_{t-4}	-0.031	-0.004	0.019	-0.048	0.006	0.123
	(0.041)	(0.017)	(0.020)	(0.040)	(0.027)	(0.022)**

Note: These tables show the estimated intertemporal effects of various crimes in weeks weeks *t*-1,...,*t*-4 on crime levels in week *t* (i.e., the parameter matrices $\hat{\beta}_1, \ldots, \hat{\beta}_4$ from equation (3.11)). Fixed effects at the division-week-crime type and sector-year-crime type are included in each of the six equations, which are estimated simultaneously by seemingly unrelated regression. The F-statistic for the discontinuity test over 144 indicator variables is 2.28 (p-value is 0.00); N = 45,960; R-squared = 0.825. All errors are clustered at the sector-year-crime type level. **, significant at the 99% level; *, significant at the 95% level.

Finally, Table 5.8 describes the cost-benefit analysis of various unit crime reduction policies that incorporates external information on the societal advantages of eliminating various forms of crime. On the benefits side, external studies are utilized to calculate the physical and psychological costs to victims of crime, as well as the psychological costs to society as a whole. All the advantages of crime reduction are expressed in terms of light crime reduction since external estimates of the costs of crime reduction are not accessible in the literature. This indicates that it would be efficient to target light crime reduction only if the other crimes listed are more expensive than larceny, robbery, and assault. Burglary and auto theft are cheaper than targeting lighter crime, however lighter crime contains 0 in the 95% confidence interval. This could mean that there is a possibility that burglary and auto theft are more expensive since both the confidence intervals only contain positive numbers. The lack of data regarding the costs of crime and the uncertainty of social benefits should be taken into consideration when assessing this conclusion.

Table 5.10: Estimated monetary benefits of unit crime reduction.

Crime	Total Benefits From Unit Crime Reduction ($)	Light Crime Monetary Equivalents
Larceny	295,014	12.9
	[283,071; 312,836]	
Robbery	91,672	4.0
	[66,503; 129,857]	
Burglary	16,511	0.7
	[592; 40,600]	
Auto theft	18,222	0.8
	[10,675; 29,771]	
Assault	35,262	1.54
	[18,133; 61,931]	
Light Crime	22,839	1.0
	[-14,292; 93,311]	

Note: The 95% confidence intervals for total benefits from unit crime reduction are presented in brackets. The social costs of larceny, robbery, burglary, and auto theft are taken from Heaton, 2010. The social cost of all assaults is computed by taking an average of the social cost of aggravated assault from Heaton, 2010 and the social cost of simple assault from Miller et al., 1993 weighted by the relative share of aggravated assaults in the sample. It was not possible to estimate the social cost of light crime, so it is assumed to be half of the social cost of larceny as given in Heaton, 2010. All monetary amounts are in 2021 dollars.

Chapter 6

Sensitivity Analysis

This chapter is comprised of robustness checks for the test of exogeneity, within the sense that potential sources of endogeneity that are imperceptible by the test of exogeneity can still be recognized by these assist checks.

6.0.1 Alternative specifications

Here, I explore alternative specifications to build on the models estimated in the previous section. There is an addition of variables in the data set control$_{jt}$ and later there are two tests: the test of exogeneity and a second test that measures any alteration of the parameter estimates of $\hat{\beta}_\tau$ with the enriched set of controls for $\tau \leq \bar{\tau}$. Table 6.1 shows the results of these tests for $\bar{\tau} = 1$. The first row contains the addition of 180 controls related to salience. The second row includes 72 extra control variables that try to proxy unobserved police attention in the neighborhood during period t. In the third row, there are additional variables that describe levels of each type of crime in the nearest adjacent neighborhood. Finally, the fourth row contains the nonlinear effects of crime. This is done by estimating a linear b-spline in past crimes with knots at the median levels of each type of crime. Similar to Table 5.5, the first test rejects every model. The result in the brackets represents the robustness checks and it fails to reject models IV, V, and the surviving VI. Models I, II, and III have some tests that failed to reject the second test, but models IV, V, and VI failed to reject all of them.

Table 6.1: The *p*-values for sensitivity tests under alternative specifications.

	Specifications of Fixed Effects					
	I	II	III	IV	V	VI
Add 180 salience controls (Number of each crime committed at night time, on the weekend; average police response times and durations at crime scene in period *t*-1)	0.00 [**0.50**]	0.00 [**0.83**]	0.00 [**0.72**]	0.00 [**0.98**]	0.00 [**1.00**]	0.00 [**1.00**]
Add 72 contemporaneous policing controls (Avg. police response times and durations at crime scene in period *t*)	0.00 [0.00]	0.00 [**0.90**]	0.00 [**0.95**]	0.00 [**0.99**]	0.00 [**1.00**]	0.00 [**1.00**]
Add levels of each crime in nearest adjacent neighborhood (36 variables)*	0.00 [0.00]	0.00 [**1.00**]	0.00 [**1.00**]	0.00 [**1.00**]	0.00 [**1.00**]	0.00 [**1.00**]
Nonlinear treatment (36 variables)**	0.00 [0.00]	0.00 [0.02]	0.00 [0.02]	0.00 [**0.33**]	0.00 [**0.10**]	0.00 [**0.79**]

Note: This table shows the *p*-values of two tests for various specifications of equation (3.8) as described in Table 5.1 for *j* = sector and *t* = week. All additional controls are specified additively. The first *p*-value listed in each cell is for the test of exogeneity described in Chapter 3 (*p*-values in bold denote "surviving models" for which we cannot reject exogeneity at the 5% are equal to their respective element presented in Table 5.2 (*p*-values in bold denote that we cannot reject that all of our results do not change at the 5% level). All errors are clustered at the $j \times year \times c$ level. * The first *p*-value refers to a test of whether the coefficients of D_{jt-1} and of $D_{j't-1}$ are jointly equal to zero, where j' is the nearest neighborhood to *j*. ** Nonlinear treatment effects are specified with a linear *b*-spline with a knot at the median level of each type of crime. The second *p*-value refers to a test of whether the coefficient corresponding to the portion of the support below the median is equal to the coefficient of the linear specification.

Multiple testing

There have been many tests that had been performed for several candidate models and there could be a concern related to multiple testing. This problem can be encountered in two forms: *false discovery* and *false nondiscovery*. *False discovery* is defined as the rejection of the null hypothesis by pure coincidence, even though the null hypothesis is actually correct (type 1 error). The solution to this problem is to reduce the size of the hypothesis test (Bender and Lange, 2001). On the other hand, Sarkar, 2006 defines *false nondiscovery* as a less debated problem where the null hypothesis fails to be rejected purely by chance, even though it is actually false (type 2 error). This is a possibility if the assumptions are tested in enough models, even if the models end up being all endogenous.

In this situation, there are some concerns that there could be both problems since models IV and V failed to reject on two occasions. Therefore, there is one additional robustness check that relates to both multiple testing concerns of false discovery and nondiscovery. The data will be split randomly across two subsamples of sector-years, then I estimate models I-VI in each of them, and finally perform the test of exogeneity.

Table 6.2: The *p*-values for sensitivity tests on randomly drawn subsamples.

	Specifications of Fixed Effects					
	I	II	III	IV	V	VI
Subsample 1	0.00	0.00	0.00	0.00	0.00	0.00
	[1.00]	**[1.00]**	**[0.99]**	**[0.97]**	**[0.99]**	**[0.97]**
Subsample 2	0.00	0.00	0.00	0.00	0.00	0.00
	[1.00]	**[1.00]**	**[1.00]**	**[1.00]**	**[0.98]**	**[1.00]**

Note: For each randomly drawn subsample, there is an exogeneity test and then the p-value of this test is reported for various specifications of equation (3.10) as described in Table 5.1 for *j* = sector and *t* = week. In brackets, there are the *p*-values of a test of whether at least one of the 36 coefficients of interest is different from the respective coefficient in the full sample. All errors are clustered at the $j \times year \times c$ level.

Table 6.2 contains the randomly distributed data from the original data. The *p*-values of the test of exogeneity are reported in each subsample. Later, (in brackets) the *p*-values of whether at least one of the 36 coefficients of interest differs from each of the complete samples. The process is described as follows: if there are more surviving (nonsurviving) models than the one that was determined earlier, then the testing suffered from false nondiscovery (false discovery) in each subsample. This table keeps track of the confounders that could bias the preferred estimates. For instance, let *w* be a variable that possesses the properties to bias the main estimates of the surviving model. For this *w* to bias the estimates, it must (i) belong to $\mathbb{W}$, but not be detected by the exogeneity test (it can belong to $\mathbb{W}_2^D \cup \mathbb{W}^C$); (ii) not be absorbed by sector-year-crime type and division-week-crime type fixed effects (FEs); (iii) it must be uncorrelated to prior crimes that occurred up to 6 weeks in the past; (iv) be uncorrelated to 180 controls related to specific features of crimes that reflect their salience; (v) be uncorrelated to 36 variables related to crime rates in adjacent neighborhoods; (vii) be spatially uncorrelated across neighborhoods within the six divisions of Dallas; and finally (viii) be serially uncorrelated across weeks within a calendar year (G. Caetano and Maheshri, 2018).

Chapter 7

Do Broken Windows Matter?

As mentioned earlier the broken windows theory has impacted many cities in the United States with different policing as an attempt to reduce crime. Using the current data, I try to add onto G. Caetano and Maheshri, 2018 the discussion of the theory's relationship to my findings.

The theory explains that a policy that can reduce the image of lighter crime in a neighborhood or community will reduce more severe crimes in the future (all else being equal).

Table 7.1 contains the *p*-values on whether reductions on light crime have effects on future crimes. In the first row, it contains the *p*-values from tests of whether a light crime has intertemporal spillovers across other types of crime in the future. All the lags fail to reject the test with a 95% level, however lags 2 and 3 could reject the test with a 90% level.

In the second row, there are *p*-values from tests of whether intertemporal spillovers from light crime vary by the salience of the crime. I interact each crime$^{x}_{jt-1}$ with the police's speed of arrival that occurred in the daytime and on the weekend in all periods $t-\tau, \tau = 1, ..., \bar{\tau}$ to the explanatory variables of the equations of motion which are the $180 \times \bar{\tau}$ additional control variables. Later, I test whether the coefficients on all the variables that capture the across-crime intertemporal effects of light crimes regardless of salience are equal to zero ($30 \times \bar{\tau}$ coefficients). The results show that all these models survive the test of exogeneity at the 95% confidence level.

In the third row, I report the results for *p*-values from the nonlinear intertemporal effects test by estimating a linear *b*-spline with a knot at the median number of weekly crimes for each crime type. This allows for different impacts based on whether a neighborhood has more or less than the median number of crimes of a certain kind in a given week. From the results, there is no evidence that there is a broken windows spillover in neighborhoods with high or low levels of light crime.

In the fourth row, I address the concern of the institutional response by law enforcement that could be generated by light crime. Here, I show the *p*-values from tests of whether the observed speed of arrival to and duration of stay at the crime scene by law enforcement in period t reduces the estimates of the spillovers effects of light crime. These variables proxy for neighborhood- and week-specific changes to a limited extent in the presence of law enforcement. For instance, if the police patrol near a reported crime scene, then they will respond faster. There are 72 control variables and I test whether the estimates of across-crime intertemporal effect of light crime are equal to zero. Again, there is no evidence that the estimates have an institutional response.

Table 7.1: Broken windows effect. The *p*-values for various tests of its existence.

	Num. of Included Lags ($\bar{\tau}$)					
	$\bar{\tau} = 1$	$\bar{\tau} = 2$	$\bar{\tau} = 3$	$\bar{\tau} = 4$	$\bar{\tau} = 5$	$\bar{\tau} = 6$
Baseline (across-crime intertemporal effects of light crime)	0.21	0.09	0.07	0.18	0.18	0.22
Allowing for heterogeneous effects by salience	0.26	0.21	0.20	0.51	0.26	0.51
Allowing for nonlinear light crime effects	0.24	0.09	0.06	0.15	0.33	0.35
Controlling for contemporaneous police responses	0.75	0.95	0.99	1.00	0.99	1.00

Note: This table presents *p*-values for a variety of tests of the broken windows effect for models with 1,...,6 lagged crimes of each type on the right-hand side. The first row contains *p*-values for F-tests of whether the $5 \times \bar{\tau}$ coefficients representing across-crime intertemporal effects of light crime are equal to zero. The second row contains *p*-values for F-tests of whether the $30 \times \bar{\tau}$ coefficients representing across-crime intertemporal effects of light crime, stratified by their salience, are all equal to zero. Salience in this context refers to features of crime that may be associated (positively or negatively) with its perception: whether crime occurs on the weekend, and in the daytime, and whether the police arrive quickly to the crime scene and stay longer at the crime scene. The third row contains *p*-values for F-tests of whether the $10 \times \bar{\tau}$ coefficients representing across-crime intertemporal effects of light crime, stratified by whether light crimes was higher or lower than the median level in a given week, are all equal to zero. The fourth row contains *p*-values for F-tests f whether across-crime intertemporal effects of light crime are still equal to zero. The fourth row contains *p*-values for F-tests of whether across-crime intertemporal effects of light crime scenes in t for each crime type and 36 variables referring to the average durations of the police at crime scenes in $t$for each crime type.

Chapter 8

Conclusion

Crime is an important phenomenon to study because of the impact it has to our lives. Many economists and policymakers work together to find the direct causes of crime and produce a solutions to decrease crime rate. In this study, I determine if past crime has a relationship with future crime. By using the test determined by C. Caetano, 2015, it was plausible to establish a connection with previous crime and present crime within 2014-2021 data set in the short run (Departments, 2022). This test of exogeneity assesses the model's ability to do causal identification. The surviving model for the first lag is (j = sector and t = week).

However, later in the data, all the other tests are rejected. This means that after a week of lag there is endogeneity in the model, so I am unable to rule out broken windows effects in the long run. Nevertheless, the other less powerful tests were able to fail to reject the surviving model and the other additional two models. This shows how the test of exogeneity is able to determine endogeneity more precisely than most common tests. In the confounders, it is evident that there is type I and type II error in all the specifications. Again, this shows in comparison how normal tests fail to reject the model after the second lag.

I conclude that there are no broken windows effects on neighborhoods in Dallas. By performing a regression, I determine that light crimes do not cause more severe crimes. If I was to eliminate 1.21 light crimes, then I cannot conclude that severe crimes like robbery or assault are eliminated. On the other hand, most crimes have across-crime effects. For instance, burglaries can cause future burglaries. I can also conclude that eliminating 2.98 burglaries would decrease robberies in a week.

The methodology of this paper adds onto existing empirical literature on model selection and inference. The methods that are used in this paper like model averaging and LASSO (least absolute shrinkage and selection operator) uncovered some truths in the field of economics of crime (G. Caetano and Maheshri, 2018, Durlauf et al., 2016, Belloni et al., 2014, Durlauf et al., 2010, Cohen-Cole et al., 2009). Also, this research determines that there is no evidence of broken windows effect in Dallas which can help policymakers in their strategy to target crime.

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