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IMPACTS OF BUSINESS IMPROVEMENT DISTRICTS ON CRIME OUTCOMES IN THE CITY OF PHILADELPIHA: DYNAMIC PANEL DATA ANALYSES

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ABSTRACT

During the last few decades, business improvement districts (BIDs), self-assessment districts formed by property owners in designated areas, have become an increasingly popular urban governance mechanism for providing local public goods. BIDs are self-financing public service delivery mechanisms. BID managers levy assessments on all property owners in their designated areas once they are established and use the money to deliver services in them. BIDs have become popular means to address the free-rider problems in local economies. They have become an increasingly popular topic of study among scholars.

It has been suggested that BIDs could be effective mechanisms for preventing and reducing crime in their designated areas. So far, only a few researchers have investigated the impacts of BIDs on crime outcomes. A major limitation of previous empirical studies is that they did not investigate the dynamic relationship between BIDs and crime. In other words, they did not consider the possibility that the previous rates of crime could affect the current BID establishment and crime rates.

The purpose of my dissertation is to investigate the impact of BIDs on crime rates in the city of Philadelphia using panel data from 1998 to 2009. I examine particularly the dynamic relationship between the existence of a BID and crime rate. I factor in the possibility that the current BID establishment is influenced by previous levels of crime rate; this approach can improve estimating the causal effect of BIDs on crime rates. More specifically I address the dynamic panel bias, which has been neglected in the previous studies, by using the Anderson-Hsiao estimators.

Overall, I found in my analyses that BIDs are effective in reducing a limited number of crime outcomes. I found a negative association between BIDs and the aggravated assault rate, showing that an approximately 26% reduction in the aggravated assault rate was due to the existence of a BID in an area. I also found that areas with five years or less of BID operation were 26% lower in the aggravated assault rate in comparison with non-BID areas.

The results of my dissertation indicate that crime reduction cannot be achieved simply by establishing a BID in a neighborhood. The results may indicate that understanding the area around BID neighborhoods and their relationships with BIDs, and the structure and implementation strategies of BIDs, are more important for crime reduction. This is an important issue that warrants further investigation as it can provide valuable information for local governments and neighborhood property owners in managing and redesigning BIDs to be effective urban mechanisms to reduce and prevent crime.

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Chapter 1. INTRODUCTION

During the past decades, business improvement districts (BIDs) have become increasingly popular as promising mechanisms for addressing various urban problems and have gained considerable attention from various scholars and practitioners (Becker, 2010; Davies, 1997; Hoyt, 2005; MacDonald et al., 2010; Mitchell, 2001; Morçöl, Vasavada, & Kim, 2014). BIDs have been formed in various countries including the United States, Canada, United Kingdom, Germany, Netherlands, and South Africa (Morçöl, G., & Gautsch, 2013). The most recent estimate of the number of BIDs showed that there were over 1,002 of them in the U.S. (IDA, 2011). BIDs are especially popular in large metropolitan areas such as Los Angeles, New York City, and Philadelphia in the U.S. (Brooks, 2008; Cook & MacDonald, 2011; Morçöl, Hoyt, Meek, & Zimmermann, 2008).

BIDs have become important in urban governance because of their organizational structures, their increasing numbers, and the economic and governmental powers some of the BIDs have. It is important to understand that there are variations among BIDs in their organizational structures and the legal powers they have (Morçöl & Wolf, 2010). Although there are variations in the definition of BIDs, BIDs can be generally conceptualized as "self-assessment districts that are usually initiated and governed by property or business owners, enabled by state laws, and authorized by local governments to provide public services in designated urban and suburban areas" (Morçöl & Wolf, 2010, p. 906).

The proliferation of BIDs is, in part, due to their ability to solve those collective action problems that cannot be satisfactorily solved by voluntary organizations (Cook & MacDonald, 2011).[1] One of the main difficulties associated with collective action is individuals' tendency to free-ride. Due to the free-rider problem, it becomes difficult for groups of individuals to accomplish their collective goals. One of the typical solutions for the free-rider problem is for the government to impose taxation and provide public goods to all members of a community. It is sometimes possible that these governmentally provided public goods may not satisfy some groups of individuals. For instance, property owners in neighborhoods may not be satisfied with the local government's provision of local public goods. Although property owners are not satisfied with the level of provision of local public goods in their neighborhoods, they may be reluctant to invest their resources as they are concerned that others may free-ride on their effort (Brooks, 2008). BIDs can address the free-rider problem through their power to levy assessments on all the neighborhood property owners in designated areas once they are formed. In other words, all the neighborhood property owners within the BID areas are legally responsible for paying for assessments, regardless of whether or not they originally advocate establishment of the BID (Brooks, 2008; Cook & MacDonald, 2011; Ellen, Schwartz, & Voicu, 2007). The mechanism that BIDs have to levy assessments enables them to address the free-rider problem as all the property owners in the designated areas are legally bound to pay an additional assessment.

The feasibility of extra-governmental provision of public goods has long been discussed in social sciences (Demsetz, 1970; Ostrom, 1990; Ostrom, 2007). To local officials, BIDs may be regarded as a means of near-free funding of local area improvement at the expense of a small cession of sovereignty. Local officials are also likely to consider the activities of BIDs as a

[1] The term "collective action problems" refer to situations in which groups of individuals would benefit from group action that may not happen since individual incentives hinder individuals from engaging in the group action (Schelling, 1960).

promising alternative to the unsatisfactorily evaluated performance of government funded and directed revitalization policies (Brooks, 2008). To property owners, BIDs can facilitate local autonomy by managing their neighborhoods to solve problems in a way they want to while the assessment is levied on them (Sutton, 2014). However, there is also a concern about BIDs because they can cause a number of problems for residents of the local neighborhoods (Briffault, 1999; Hochleutner, 2003; Hoyt, 2004). Critics of the BID model argue that BIDs threaten political accountability (Garodnick, 2000) and lead to economic and social inequalities through the different levels of service provision (Briffault, 1999). Since BIDs operate for the interests of residents in the designated areas, it is not difficult to think that they work while ignoring the needs and interests of residents who reside around BID areas (Briffault, 1999). Given the criticisms on the BID model, however, there is still a strong belief among many scholars and practitioners that BIDs can make an area more attractive beyond their boundaries (Ellen et al., 2007; MacDonald, 1996, Mitchell, 2001).

City officials and neighborhood property owners have been increasingly supportive of BIDs as effective mechanisms for maintaining social order and facilitating economic development (Ellen et al., 2007; Sutton, 2014). BIDs are created to provide supplementary local public services including street cleaning, security, and place marketing. Some BIDs provide a wider range of services, such as employment programs, school-based youth activities, and homeless outreach (Stokes, 2002).

BIDs are especially attractive mechanisms for preventing and reducing crime in their neighborhoods (Cook & MacDonald, 2011). Some earlier researchers found that BID areas were lower in crime in comparison with non-BID areas (Brooks, 2008; Cook & MacDonald, 2011; Hoyt, 2004). One of the ultimate goals for establishing BIDs for property owners is to promote an economic development in the designated areas (MacDonald et al., 2010). BIDs provide various services such as beautification, street cleaning, security, and business support to increase overall economic performance in the areas (Sutton, 2014). Crime can decrease property values and keeps customers away from commercial areas, which hinders the economic development of the areas. Thus, it is not surprising that BIDs frequently spend their budgets to facilitate public safety in their designated areas (Brooks, 2008) to attract customers, which can enhance economic well-being of the areas in turn (Brooks, 2006).

Although it is argued that BIDs are effective in reducing crime, there are a limited number of studies that empirically examined their effects on crime (Brooks, 2008; Calanog, 2006; Hoyt, 2004; MacDonald, Golinelli, Stokes, & Bluthenthal, 2010; Cook & Macdonald, 2011; MacDonald, Stokes, Grunwald, & Bluthenthal, 2013). Various estimation techniques were adopted in previous empirical studies to estimate the effects of BIDs on crime. Most these studies show that BIDs reduce crime, but not every study finds a statistically significant association between BIDs and crime. One of the most rigorous studies on the impact of BIDs on crime by Brooks (2008) showed that BIDs were negatively associated with crime in Los Angeles. Similarly, Cook and MacDonald (2011) found a negative association between BIDs and crime.

While the previous researchers adopted various strategies to better estimate the impact of BIDs on crime, there is one major limitation that has not been addressed: they did not investigate the dynamic relationship between BIDs and crime. This neglect limits the possibility that the previous level of crime could affect the current decision on BID establishment and the current level of crime. It is important to examine the dynamic relationship between BIDs and crime as a decision to form a BID in an area could be influenced by previous crime experience in the area (Hoyt, 2005). Neglecting this dynamic relationship between BIDs and crime can lead to a biased estimate of BIDs' effects on crime. There is only one study that investigated the dynamic relationship between BIDs and crime (Calanog, 2006). Even in this study, the author did not correct for dynamic panel bias.[2] Once researchers include the lagged dependent variable (in this case, crime) as an explanatory variable in a model and estimate the model by OLS, its estimate is biased by construction (Baum, 2006). In other words, the lagged crime variable is correlated with the error term in the model that can bias an estimate of the lagged crime variable on the current crime outcome. This can further bias an estimate of BIDs on the current crime outcome if there is correlation between the lagged crime variable and BIDs.

In my dissertation, I attempt to answer the question: Do BIDs reduce crime rates in their designated areas? It is difficult to answer this research question since BIDs are not randomly distributed across the neighborhoods in urban areas. In other words, the formation of BIDs involves a neighborhood decision, instead of randomization. Brooks (2006), Brooks and Strange (2011), Hoyt (2005), and Melzer (2012) found that there are various factors including crime, presence of effective organization, badly decayed infrastructure, and voting behaviors of property owners, that influence the establishment of BIDs in urban areas. The selective formation of BIDs yields methodological challenges in estimating their impacts on crime. If there are unmeasured factors that affect the formation of BIDs and crime, the effect of BIDs on crime is biased and inconsistent as their effect include the effects of other unobserved factors on crime. As I mentioned earlier, the previous empirical studies on the association between BIDs and crime used various estimation techniques to improve the causal effect of BIDs on crime.

[2] Once the lagged dependent variable is included as an explanatory variable in a regression model, the lagged dependent variable is correlated with the error term in the model, which is known as dynamic panel bias (Nickell, 1981).

However, they, except for Calanog's study (2006), assume that the current BID establishment status is totally independent of past experiences of crime, which is an unrealistic assumption and could bias the estimations of the effect of BIDs on crime.

I evaluate the impact of BIDs on crime rates in the city of Philadelphia using panel data from 1998 to 2009 in my dissertation. I allow the possibility that the current BID establishment status is influenced by the previous levels of crime rates that can improve the causal effect of BIDs on crime rates. I further address dynamic panel bias, which has been a neglected issue on the empirical studies on relationship between BIDs and crime. My dissertation is organized as follows: In chapter 2, I discuss the concept of collective action problems and how BIDS can address these problems. I also discuss the theoretical linkage between BIDs and crime. I also review and evaluate the previous empirical studies on the association between BIDs and crime. I discuss the methods I used for my dissertation in chapter 3. I present and discuss the findings in chapter 4. I conclude my dissertation in chapter 5.

Chapter 2. LITERATURE REVIEW

2.1 Collective Action Problems and BIDs

Under the assumption of rationality, individuals are assumed to be self-interested. In his seminal work, Olson (1965) illustrated the difficulty related to a group of rational individuals achieving common goals. He argued that rational individuals are motivated to free-ride on other's contributions, which make difficult for them to collaborate for their common goals. He further argued that:

> Unless there is coercion or some other special device to make individuals act in their common interest, *rational, self-interested individuals* will not act to achieve their common or group interests. (p. 2, emphasis in original)

In other words, Olson (1965) maintained that if there is no coercion or some other special ways to make individuals work towards their common goal, they will not work towards accomplishing their group interests. This is because individuals are willing to free-ride on the efforts made by other group members.

One of the standard solutions adopted by governments to address the free-rider problem is to impose taxation and provide public goods for everyone (Brooks, 2008). A problem arises when people in a neighborhood are not satisfied by governmentally provided public goods. Although a property owner is dissatisfied by public goods provided by the local government, they may hesitate to invest their resources in their neighborhood since they worry that other property owners may free-ride on them (Brooks, 2008). The proliferation of BIDs is, in part, due to their ability to solve the free-rider problem that cannot be successfully addressed by voluntary organizations (Cook & MacDonald, 2011). Olson (1965) argue that unless there is coercion or the group size is small, it is likely that a group of individuals may not act together to achieve their common goals. Individuals tend to free-ride on others. BIDs have become a popular urban institution providing local public services that can address the free-rider problem by utilizing the coercive mechanism of levying assessments, which are taxes for all practical purposes (Mitchell, 2001).

BIDs may be able to resolve the free-rider problem because they have the power to levy assessments to all the property owners in their designated areas. BIDs are generally established when neighborhood property owners decide, by majority vote, to levy an additional assessment on themselves to finance supplementary local public goods provision. Once BIDs are established, the local government levies and collects the additional assessment from all property owners within the BID areas, even from those who did not sign the original petition or endorse the establishment of the BID (Brooks, 2008; Ellen et al, 2007). The authority of BIDs to levy additional assessments on all property owners in the district enables them to address the free-rider problem (Meltzer, 2011).

2.2 Linking BIDs to Crime

One of the areas BIDs are considered to be effective is crime reduction. The literature on BIDs and crime shows that there is some relationship between the existence of BIDs and reduced crime. This observed relationship is consistent with one of the major stated goals of establishing BIDs in urban areas: to deter crime and enhance public safety (Brooks, 2008; Meltzer, 2011). BIDs frequently allocate substantial percentages of their budgets to providing private security to their areas, which can be expected to reduce crime.

Although there is no specific conceptual mechanism linking BIDs to crime outcomes, the theoretical foundation of BIDs' influence on crime can be found from the social disorganization theory that is closely related to theories such as broken windows (Wilson & Kelling, 1982) and routine activities (Cohen & Felson, 1979). Shaw (1931) pointed out that poverty, family disruption, and ethnic heterogeneity are common structural characteristics shared by disorganized neighborhoods. Shaw and Mckay (1942) further maintained that these characteristics prevent civic engagement and social networks among residents that further lower trust among neighbors. As a result of the lack of civic engagement, social networks, and trust among neighbors, informal social control diminishes, which leads to high crime rates. The main thesis of the social disorganization theory is that structural disorganization in a community leads to lower informal social control, which increases crime rates in turn (Roh & Lee, 2013).[3]

Many empirical studies supported the social disorganization theory. For instance, Rice and Smith (2002) examined various factors proposed by the social disorganization theory using census data and found that they were associated with crime rates in a southeastern mid-size city in the U.S. Using data from U.S. Census and Houston Police Department, Lee, Lee, and Hoover (2013) also found that factors related to the social disorganization theory, such as concentrated disadvantage, residential stability, and racial heterogeneity were associated with crime rates.

Overall, the social disorganization theory emphasizes the importance of various neighborhood characteristics, such as concentrated disadvantage and residential mobility and

[3] Informal social control is generally defined as the capacity to realize common problems and behave collectively to address them within neighborhoods (Shaw & McKay, 1942). Examples include, but are not limited to, collectively overseeing public areas, residents watching out for each other's property, or sharing norms among residents (Drakulich & Crutchfield, 2013).

heterogeneity. Its basic assumption is that individual attributes such as age and gender alone cannot explain variances in crime (Sampson & Groves, 1989).

The empirical studies on the establishment of BIDs supported that they were more likely to be formed in disorganized neighborhoods as crime activities are an important concern in such neighborhoods. For instance, Brooks (2006) found that BID areas were higher in various heterogeneity measures, such as the dissimilarity measure and the fragmentation index in comparison with non-BID areas in the state of California. This result and the evidence that BIDs are more likely to be established in areas with higher crime rates (Hoyt, 2005) are in line with the core thesis of the social disorganization theory. The main functions of most, if not all, BIDs, such as street maintenance, cleaning, and beautification are methods for facilitating informal social control in disorganized neighborhoods that in turn can lower crime rates suggested by the social disorganization theory.

According to the broken windows theory, physical signs of decay and disorder in a neighborhood, such as litter, vacant houses, abandoned cars, loitering, and graffiti signal that residents do not care about their neighborhoods. Such signs can also erode the collective sense of community, which may cause the residents to have less commitment to engaging in informal social control (Wilson & Kelling, 1982). Meanwhile, potential offenders feel less restricted by social norms, and the likelihood of them to commit crime in the neighborhood will increase since they perceive that the likelihood of detection is low (Cullen, Agnew, & Wilcox, 2014). Wilson and Kelling (1982) further argue that the police forces have to be the key agents in reestablishing social control. The presence of police officers via increased visibility by walking the beat in the community and coming to know and talking to the residents can increase informal social control, decrease fear of crime, and in turn prevent crime.

Empirical studies tend to support the argument of the broken widows theory. For instance, Keizer, Lindenberg, and Steg (2008) found that graffiti or trash increased the probability of littering and other types of disorder in Netherlands. Milam, Furr-Holden, Harrell, Whitaker, and Leaf (2012) also found that the count of juvenile drug arrests increased in the disordered neighborhoods in the city of Baltimore, Maryland.

BIDs can reduce crime by actively investing in sanitation, street cleaning, and physical beautification, which enhance physical appearances of neighborhoods (Sutton, 2014). BIDs also facilitate various local economic activities, which may fill commercial, industrial, and residential properties in a community, which reduces signs of neglect and decay that invite crime. These efforts made by BIDs are a means to reduce crime from the perspective of the broken windows theory.

According to the routine activity theory, existence of effective guardians facilitates the collective supervision of public areas and pressures offenders to evaluate potential targets more carefully (Cohen & Felson, 1979). BIDs frequently hire private guards and install electronic surveillance equipment enhancing the level of guardianship in communities (Hoyt, 2004), which together prevent potential offenders from committing crime as suggested by the routine activity theory (Felson & Cohen, 1980). BIDs also deter crime by properly organizing the areas. For instance, BIDs typically invest in street cleaning and physical beautification (Billings & Leland, 2009; Brooks & Strange, 2011; Stokes, 2006), which send a signal to motived offenders that the areas are being controlled and cared for. These activities make the areas less attractive for motivated offenders (Felson, 1987). Physically improved BID areas can increase the attractiveness of the region, social interactions, and social bonding among residents and property owners in them, which in turn can facilitate the surveillance system.

Overall, the broken windows and routine activity theories, which can be encompassed by the social disorganization theory, emphasize the role of environment, such as physical structure and social control, in relation to crime. Although a specific mechanism linking an environment to crime is different among these theories, they all suggest that properly designed environments can increase social control, which can deter crime. Many of the activities of BIDs, such as beautification, physical improvements, and hiring private guards are in line with these criminological theories.

2.3 Empirical Evidence on the Association between BIDs and Crime

Despite the proliferation of BIDs and the widespread support for them, only a limited number of empirical studies have examined their effectiveness on crime reduction to date (Brooks, 2008; Calanog, 2006; Cook & MacDonald, 2011; Hoyt, 2004; MacDonald et al., 2010; MacDonald et al., 2013). In this section, I review these studies. A summary of the following discussions on the previous studies is presented in Appendix I.

Hoyt (2004) examined the impact of BIDs on crime in Philadelphia. She mainly used the data from the Philadelphia City Planning Commission and the Philadelphia Police Department. She conducted linear regression analysis to examine the association between the presence of BID security and property crimes while adjusting for a limited number of control variables, including median household income, zoning designation (nonresidential versus residential), and number of businesses. She found that the presence of BID security was negatively associated with property crimes.

Calanog (2006) investigated the influence of BIDs on crime outcomes in the city of Philadelphia, using monthly panel data from 1997 to 2002 at the census-tract level. He adopted the fixed effects model to adjust for time-invariant unobserved factors, such as distance to the highway. This study did not find any significant association between BIDs and relatively less mobile crime, such as murder, rape, and aggravated assault. However, he found significant negative associations between BIDs and relatively more mobile crimes, including robbery, theft, and auto theft.

Brooks (2008) investigated the effect of BIDS on 21 types of crimes and 27 varieties of arrests in Los Angeles. She used the data from the Los Angeles Police Department (LAPD) between 1990 and 2002 at the “crime reporting district” level, which is equivalent to a census tract. She notes that the establishment of a BID is a community decision, rather than random assignment, which can cause a selection bias. To address the selection bias, she adopted multiple estimation strategies, including a fixed effects model and propensity score matching. After using these estimation strategies, she found that BID adoption generally reduced both serious crimes and less serious crimes.

Similarly, Cook and MacDonald (2011) used data from the LAPD between 1994 and 2005 to examine the impacts of BIDs on crime and arrests. They adopted the fixed effects model to adjust for time-invariant factors that may affect both the establishment of BIDs, crime, and arrests at the “police reporting district.” They found that establishment of BIDs reduced total crime, robbery, assault, burglary, and auto theft. They also found that BIDs with greater expenditures on private security had greater decreases in crime and arrests. Using the same data, MacDonald et al. (2010) examined the association between BIDs and violent felony crimes, including homicide, rape, robbery, and aggravated assault at the police reporting district level between 1994 and 2005. They relied on a pre-post intervention design, which only used the BID areas and compared crime outcomes between before and after the BID formation. They adopted a hierarchical model and found negative associations between the implementation of BIDs and the robbery count and the total violent crime count.

Finally, MacDonald et al. (2013) investigated the effect of BIDs on violent victimization among adolescents in Los Angeles using survey data. They conducted their telephone interviews with one adult and one youth (ages 14-17) in each household between October 2006 and February 2007. They used a multilevel analysis that took into account the multilevel structure of data. Specifically, theirs was a three-level model in which household respondents (level 1) were nested within interview waves (level 2), which were in turn nested within census tracts (level 3). They failed to find a significant association between BIDs and youth victimization.

2.4 Methodological Consideration

It is not an easy task to estimate the causal effect of BIDs on crime. If researchers were able to randomly assign neighborhoods in cities into the treatment (in this case, BIDs) and control groups (in this case, non-BIDs), there would be no observed factors that affect both the establishment of the BID and crime (Hannan, 2006). Thus, the observed association between a BID’s existence and reduced crime rate could be interpreted as causal. The previous empirical studies were based on observational study designs, so BID areas and non-BID areas were not randomly distributed across the cities investigated. Thus, it is possible that there may have been unobserved factors that affected both the independent and dependent variables: existence of BID (i.e., the decisions of property owners to form a BID in an area) and crime. Depending on how these decisions are made, the effect of BIDs on crime may underestimate or overestimate the true causal effect.

To solve the problems of overestimation or underestimation, the researchers in the previous empirical studies adopted various strategies. For instance, Brooks (2006) used multiple linear regression analysis while adjusting for several control variables. Three studies (Brooks, 2008; Calanog, 2006; Cook & MacDonald, 2011) adopted the fixed effects model to reduce a certain form of endogeneity problem of BIDs.[4] The fixed effects model has an advantage in that it allows time-invariant factors to be correlated with the establishment of BIDs. In other words, if there is only a time-invariant factor that affects both the formation of BIDs and crime, the fixed effects model still yields a consistent estimate of BIDs unlike conventional regression analysis. Two studies (MacDonald et al. 2010; MacDonald et al. 2013) adopted the random-effects model. MacDonald et al.'s (2013) study is especially noteworthy, because it controlled for various control variables at multiple levels to reduce an endogeneity problem.

Brooks (2008) additionally used several matching estimators, including propensity score analysis. She adopted propensity score analysis, whichhelped reduce the selection biases in her studyaccording to observable pretreatment characteristics. Generally, propensity score analysis creates a statistical comparison group using a model of the probability of receiving the treatment based on observed variables (Khandker, Koolwal, & Samad, 2010; Rosenbaum & Rubin, 1983; Sianesi, 2004). This method enables researchers to reduce selection bias by constructing counterfactual control groups obtained from baseline characteristics observed prior to BID adoption. Specifically, Brooks (2008) used several variables, including pre-BID annual levels of crime, crime trend, median income, median rents, and median home price to calculate the probability of receiving treatment (in this case, BID).

[4] Endogeneity refers to the presence of an explanation variable in a regression model that is associated with the error term (Wooldridge, 2012).

A major limitation of previous studies on the association between BIDs and crime is that they did not investigate the dynamic relationship between BIDs and crime. The previous researchers assumed that the current status of a BID establishment is completely independent of the past levels of crime; indeed, a decision to establish a BID in an area could be a reaction to previous crime experience (Hoyt, 2005).[5] Additionally, given the fact that one of the most influential factors contributing to the current level of an outcome variable is likely to be its previous level, neglecting this dynamic relationship between BIDs and crime can create an endogeneity problem.

This problem could be addressed by studying the dynamic relationships between BID establishments and crime using panel data. Unfortunately, even those previous studies that are based on panel data (Brooks, 2008; Cook & MacDonald, 2011) did not examine the dynamic relationship between BIDs and crime.[6] It should be noted that inclusion of the lagged dependent variable as an explanatory variable in a regression model creates another problem, known as dynamic panel bias (Nickell, 1981). If researchers include the lagged dependent variable as an explanatory variable in a regression model and estimate it by OLS, its estimate of the lagged dependent variable will be inconsistent. This is because the lagged dependent variable is associated with the error term in the regression model by construction (Baum, 2006). Dynamic panel bias is a serious concern as it is a bias not only in the estimate of the lagged crime variable on the current crime outcome, but also in the coefficient of BIDs on the current crime outcome, when there is a correlation between the lagged crime variable and BIDs. Given the fact that the

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[5] In other words, the previous researchers ignored the possibility that the current level of an explanatory variable can be a function of the past levels of crime.

[6] Indeed, there is one study (Calanog, 2006) that examined the dynamic relationship between BIDs and crime. However, he applied a fixed effects model, which is inconsistent with using a lagged dependent variable as an explanatory variable. Calanog did not make any corrections for this bias.

reason I controlled for the lagged crime is a correlation between the current status of a BID establishment and the past level of crime, a coefficient of BIDs on crime is likely to be biased.

In my dissertation, I investigated the possibility that the previous level of crime could affect the current establishment of BIDs and the current level of crime; I did so by including the lagged crime as an explanatory variable in the model. I further adopted the Anderson-Hsiao estimators (1981/1982) to address dynamic panel bias, which I will discuss in the following section. In doing so, I used crime rate as the dependent variable because each area has a different population number unlike the previous studies (e.g., Brooks, 2008; Cook & McDonald, 2011; Hoyt, 2004) that used crime counts as dependent variables,

Chapter 3. METHODS

3.1 Data

In my dissertation, I used secondary panel data about the city of Philadelphia.[7] The first official BID in Philadelphia is known as the Center City District. It was established in 1990 when property owners and commercial entities, as well as the City of Philadelphia, agreed to form a private-sector business improvement district with the purpose of keeping Philadelphia's downtown clean, safe, beautiful, and fun. It provides various services, such as security, cleaning streets, and installing and maintaining lighting. The Center City District is considered as one of the world's largest and most prominent BIDs (Hoyt, 2005). Since the creation of the Center City District, other areas in the city began establishing BIDs (Calanog, 2006). As of 2013, there were 15 BIDs in existence in the city of Philadelphia. The popularity of BIDs in the city of Philadelphia is expected to keep increasing, in part because there is a belief that BIDs can reduce crime rates, strengthen retail sales, and enhance property values (Hoffman & Houstoun, 2010).

There are two major reasons for studying the BIDs in Philadelphia. The first reason is that Philadelphia is one of the cities that has the largest number of BIDs in the U.S. (Mitchell, 1999), which could be used in analyses to compare the crime rates in BID areas and non-BID areas at the census tract level. At the end of the study period in 2009, there were 381 census tracts in the city of Philadelphia and 88 (23.10%) of them were within BID areas.

The second reason for studying the BIDs in Philadelphia was that detailed crime data were available at the census tract level for this city. I obtained the data I used in the study from the CrimeBase database of the Philadelphia Neighborhood Information System (NIS) (Cartographic Modeling Lab, 2014). The Philadelphia NIS utilizes Geographic Information Systems and the

[7] Two other studies had been conducted on Philadelphia: Canalog (2006) and Hoyt (2005).

CrimeBase database, which contain variables on crime that were provided by the Philadelphia Police Department from 1998 to 2009 in Philadelphia at the census tract level. I investigated Philadelphia City County files and websites for BIDs, and when it was necessary, I contacted city officials and BID administrators to obtain the establishment dates and boundary maps for all BIDs in Philadelphia. After I obtained the maps for all BIDs, I matched the CrimeBase database of the Philadelphia NIS with the BID boundaries by using the GIS capability of the database. Table 3.1 presents the list of BIDs in Philadelphia during the study period. None of the BIDs were terminated during the study period between 1998 and 2009.

Table 3.1. List of BIDs in Philadelphia

BID	Establishment year
Aramingo Avenue Shopping District	2008
Center City District	1991
Chestnut Hill BID	2003
City Avenue Special Services District	1996
East Passyunk Avenue BID	2002
Frankford Special Services District[a]	1995
Germantown Special Services District	1995
Greater Cheltenham Avenue BID	2007
Manayunk Special Services District	1996
Mt. Airy BID	2007
Old City Special Services District	1998
Port Richmond Industrial Development Enterprise	1998
Roxborough Neighborhood Improvement District	2003
South Street/Headhouse District	1993
Sports Complex Special Services District	1995
University City Improvement District	1997

Note. a. Frankford Special Services District was terminated in 2011.

Source: Dilworth (2010)

3.2 Measures

Crime outcomes. I used the following crime measures: minor disturbance, property vandalism, aggravated assault, theft, robbery, and burglary. I analyzed them as crime rates, not as crime counts. I calculated the rates by dividing crime counts in census tracts by their residential population numbers, which helped adjust for the different sizes of population. In other words, I used minor disturbance, property vandalism, aggravated assault, theft, robbery, and burglary rates, each calculated per 1,000 population. I used the natural logarithm transformation for all crime rates. The natural logarithm transformation is used to change a highly skewed variable into one that is approximately normal (Andreß, Golsch, & Schmidt, 2013). This type of transformation is commonly used for rate variables in applied studies.[8]

BIDs. I created a dummy variable to differentiate between census tracts that have BIDs and others that do not have BIDs. I coded the variable $BID_{it} = 1$ if census tract *i* encompasses a BID boundary in the year *t*, and 0 if otherwise. A list of census tracts of BID and non-BID areas is presented in Appendix II.

3.3 Statistical Analyses

As I discussed in the previous section, previous empirical studies neglected the dynamic relationship between BIDs and crime. To investigate the dynamic relationship between BIDs and

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[8] Consider a model that includes a dependent variable that is natural logarithm transformed, $\log(y_i)$, with one explanatory variable, $X : \log(y_i) = a + \beta X_i + \varepsilon_i$. The interpretation of estimated regression coefficient $\hat{\beta}$ is that a one-unit increase in X multiplies the expected value of y by $\exp(\hat{\beta})$. In terms of percent change, we can use the following approximation: $100 \cdot [\exp(\hat{\beta}\Delta X) - 1]$ (Wooldridge, 2012). For example, when $\hat{\beta} = 0.1$, $\exp(0.1) \approx 1.10$, for a one-unit change in X, $\Delta X = 1$, we would expect to see an approximately 10% increase in y.

crime, the following dynamic panel model can be specified by including the lagged crime as an explanatory variable in the model:

$$y_{it} = \delta y_{it-1} + \beta BID_{it} + a_i + v_t + \varepsilon_{it} \qquad (3.1)$$

where y_{it} is a crime rate of census tract *i* in year *t*, y_{it-1} is one year lagged level of y_{it}, a_i is a census tract fixed effect, v_t is a year fixed effect, and ε_{it} is a random error term that varies with census tracts and years. BID_{it} is a dummy variable that has a value of 1 if census tract *i* has implemented BID in year *t*, and 0 otherwise.

There are some advantages to estimating the above model using the fixed effects model.[9] First, it can reduce a certain type of endogeneity problem of BIDs by including the census tract fixed effect, a_i.[10] It permits a_i to be correlated with BID_{it}. Thus, if there are only unmeasured time-invariant factors, such as distance to the highway and distance to the police station that are associated with the establishment of BID and crime, the fixed effects model can yield a consistent effect of BIDs on crime.[11] Second, the year fixed effect term, v_t, in the above model can account for trends of crime rates in the study areas.[12] Finally, by including the lagged level of crime rates, y_{it-1}, it can account for the possibility that the establishment of BIDs is affected by the past level of crime rates. However, the fixed effects model is inconsistent once the lagged dependent variable is included as a regressor (Cameron & Trivedi, 2005). In other words, the

[9] See Appendix III to confirm what the fixed effects model does.
[10] It should be noted the fixed effects model only uses within-group variation.
[11] I conducted the robust Hausman test (Wooldridge, 2002) for each crime outcome model to check whether or not a_i is correlated with BID_{it}. The results showed rejection of the null hypothesis that the random effects model provides a consistent estimate of BID_{it} for all crime models. In other words, BID_{it} is correlated with a_i the fixed effects model is preferable to the random effects model.
[12] The year fixed effect term, v_t ,was included as a set of year dummy variables. The year fixed effect term allows crime rates to vary over time, which captures trends in crime rates.

lagged dependent variable is correlated with the error term in the model, which is known as dynamic panel bias (Nickell, 1981).

Although the fixed effects model eliminates a_i, the within-transformed lagged crime variable, $y_{it-1} - \bar{y}_i$, is correlated with the within-transformed error, $\varepsilon_{it} - \bar{\varepsilon}_i$. This is because y_{it-1} is correlated with ε_{it-1} and thus $\bar{\varepsilon}_i$, even if ε_{it} is not serially correlated (Cameron & Trivedi, 2005).[13] If the BID variable is correlated with the lagged crime rate to some extent, the effect of BIDs on the current level of crime rate is likely to be biased as well (Baum, 2006).[14] It is important to note that I controlled for the lagged crime rate because there is a relationship between the previous level of crime and the decision on BID establishment.[15] Accordingly, it is likely that the effect of BIDs on the current level of crime rate is biased once I included the lagged crime rate as an explanatory variable. Moreover, instrumental variables estimation using lags would not be feasible, since any lag of the dependent variable will also be related to $\bar{\varepsilon}_i$ and thus, to $\varepsilon_{it} - \bar{\varepsilon}_i$.[16] By contrast, the first difference (FD) model with appropriate instrumental variables yields a consistent estimate of the lagged dependent variable (Cameron & Trivedi, 2005).

The first difference (FD) model is expressed as:

[13] The within-transformed lagged dependent variable, $y_{it-1} - \bar{y}_i$, is correlated with the within-transformed error $\varepsilon_{it} - \bar{\varepsilon}_i$ since y_{it-1} is correlated with $\bar{\varepsilon}_i$. This is because $\bar{\varepsilon}_i$ includes ε_{it-1}, which is correlated with y_{it-1}. Additionally, $\bar{y}_i$ is correlated with ε_{it}, because $\bar{y}_i$ includes y_{it}, which is obviously correlated with ε_{it} (Baltagi, 2013).

[14] This is because the estimated coefficient of each variable is affected by the other variables in a model (Dohoo, Martin, & Stryhn, 2012).

[15] Results on a series of Pearson correlation analyses between BIDs and lagged crime rate variables were shown in Appendix IV.

[16] A standard solution, when there is an endogenous explanatory variable, is to use an instrumental variables estimator (Wooldridge, 2002).

$$\Delta y_{it} = \delta \Delta y_{it-1} + \beta \Delta BID_{it} + \Delta v_t + \Delta \varepsilon_{it} \quad (3.2)$$

The FD estimator eliminates the census tract fixed effect, a_i, as the within estimator does. In other words, it allows the correlation between time-invariant factors and the BID establishment just like the fixed effects model does. Thus, if there are only unmeasured time-invariant factors, the FD estimator leads to a consistent estimate of BIDs on crime rates. However, the FD estimator is still inconsistent once the lagged dependent variable is included as an explanatory variable (Roodman, 2009). Namely, the lagged dependent variable is still endogenous as it is correlated with the error term in the model. This is because Δy_{it-1} is correlated with $\Delta \varepsilon_{it}$. In other words, the FD model of $\Delta y_{it-1} = y_{it-1} - y_{it-2}$ is correlated with $\Delta \varepsilon_{it} = \varepsilon_{it} - \varepsilon_{it-1}$, because y_{it-1} is correlated with ε_{it-1}.[17] At the same time, unlike the within estimator, instrumental variables estimators of the FD model can yield consistent parameter estimates with appropriate lags of regressors (Cameron & Trivedi, 2005). Anderson-Hsiao (1981/1982) suggested instrumental variables estimation using either the second lag of the first difference of the dependent variable, $\Delta y_{it-2} = y_{it-2} - y_{it-3}$, or the second lag of the dependent variable, y_{it-2}, as an instrumental variable for the endogenous explanatory variable, $\Delta y_{it-1} = y_{it-1} - y_{it-2}$, as both instrumental variables are correlated with Δy_{it-1} but uncorrelated with $\Delta \varepsilon_{it}$.[18] I adopted the Anderson- Hsiao estimators to address dynamic panel bias.[19]

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[17] There is a correlation between the term ε_{it-1} in $\Delta \varepsilon_{it} = \varepsilon_{it} - \varepsilon_{it-1}$ and the y_{it-1} term in $\Delta y_{it-1} = y_{it-1} - y_{it-2}$ (Roodman, 2009).

[18] It is recommended to use the Anderson-Hsiao estimator that uses levels y_{it-2} for instrumental variables over the Anderson-Hsiao estimator that uses differences (Δy_{it-2}) for instrumental variables, as the former maximizes sample size and thus is more accurate (Roodman, 2009). Arellano (1989) also pointed out that the Anderson-Hsiao estimator that uses differences Δy_{it-2} for instrumental variables are more imprecise than the Anderson-Hsiao estimator that uses levels y_{it-2} for instrumental variables. Thus, if there is a discrepancy on the results from the two

Equations 1 and 2, which treat the influences of the BIDs on crime rates, are the same over time. However, it was reported that the effects of BIDs on crime rates can vary depending on years of BID operation (Brooks, 2008; Cook & McDonald, 2011). To distinguish the effects of BIDs on crime rates according to their operational years, I created a set of three dummy variables, which were applied to any given year: five years of BID operation or less; 6 to 10 years of BID operation; and BID operation over 10 years.[20] The following FD model with a set of BID dummy variables was specified:

$$\Delta y_{it} = \delta \Delta y_{it-1} + \beta_1 \Delta BID1_{it} + \beta_2 \Delta BID2_{it} + \beta_3 \Delta BID3_{it} + \Delta v_t + \Delta \varepsilon_{it} \quad (3.3)$$

$BID1_{it}$, $BID2_{it}$, and $BID3_{it}$ are a series of dummy variables. Five years of BID operation or less ($BID1_{it}$) takes on value 1 if census tract *i* has operated a BID for five years or less in year *t*, and 0 otherwise. Six to 10 years of BID operation ($BID2_{it}$) takes on value 1 if census tract *i* has operated a BID for 6 to 10 years in year *t*, and 0 otherwise. BID operation over 10 years ($BID3_{it}$) takes on value 1 if census tract *i* has operated a BID over 10 years in year *t*, and 0 otherwise. I also used the Anderson-Hsiao estimators to address dynamic panel bias to estimate Equation 3.

For each crime outcome model, I developed and estimated the models in the following sequence.[21] First, I presented the results based on the pooled OLS estimator, which is a baseline

estimators, the results based on the Anderson-Hsiao estimator that uses levels y_{it-2} for instrumental variables is preferable.

[19] I further tested models with different specifications of lagged variable, Δy_{it-1}. For example, I fitted autoregressive models of order 2 and order 3, which additionally include Δy_{it-2}, and Δy_{it-2} and Δy_{it-3} as explanatory variables, respectively. I found that the basic results remained the same as models including only Δy_{it-1} as an explanatory variable. I presented the results based on autoregressive models of order 1.

[20] Non-BID census tracts were used as the reference category.

[21] Other than the Anderson-Hsiao estimators that need to include the lagged crime outcome as an explanatory variable, I present the results based on before and after the inclusion of the lagged crime outcome as an explanatory variable to see whether or not the inclusion of the lagged crime outcome as an explanatory variable changes the results.

model in my dissertation. The pooled OLS estimator does not allow both unobserved time variant and time invariant factors to be associated with the establishment of BIDs (Baum, 2006). Thus, the pooled OLS estimation of the effect of BIDs on crime is biased, even when there are only unobserved time invariant factors that are correlated with the BID establishment and the crime rate, unlike the fixed effects model. In other words, the pooled OLS estimator assumes that the establishment of BIDs are completely exogenous, which is a strong assumption for studies based on observational designs. Additionally, the pooled OLS estimator does not address dynamic panel bias. Second, I presented the results based on the fixed effects model to compare results with the previous empirical studies (e.g., Brooks, 2008; Calanog, 2006; Cook & MacDonald, 2011). It should be also noted that the fixed effects model does not address dynamic panel bias. Third, I presented the results based on the Anderson-Hsiao estimators that address dynamic panel bias.[22] Fourth, I presented the results of the effects of BIDs over their operational years on the crime rates based on the Anderson-Hsiao estimators that address dynamic panel bias. I computed the cluster-robust standard errors at the census-tract level for all the models except for the Anderson-Hsiao estimators, because random error terms are likely to be correlated within a census tract. Additionally, I estimated all the models based on the crime counts as dependent variables as well to compare the results with the previous empirical studies that used the crime counts (e.g., Brooks. 2008; Cook & MacDonald, 2011; Hoyt, 2004); to check whether or not different specifications alter the result although using crime rates, reflecting the different sizes of population, are more appropriate. I performed all the statistical analyses using Stata version 13 (StataCorp LP, College Station, TX).

[22] Up to this point, I used the dummy variable of BIDs (BIDs versus non-BIDs).

Chapter 4. RESULTS AND DISCUSSIONS

In this chapter, I present the results on the association between BIDs and crime outcomes in the following sequence: the association between BIDs and minor disturbance, the association between BIDs and property vandalism, the association between BIDs and aggravated assault, the association between BIDs and theft, the association between BIDs and robbery, and the association between BIDs and burglary. For each crime outcome, I present the results based on the crime rate followed by the crime count. I present the following models on the association between a dummy variable of BIDs and crime in sequences for each crime outcome model; the pooled OLS regression model, the fixed effects model, the Anderson-Hsiao estimators. Finally, I present the results on the impacts of BIDs over their operational years on crime outcomes based on the Anderson-Hsiao estimators.

4.1 Minor Disturbance

Minor disturbance rate. In Table 4.1, I present the results of the association between BIDs and the minor disturbance rate using the pooled OLS regression analysis while including the time-fixed effects. In Model A, I did not include the lagged disturbance rate as an explanatory variable. I did not find a significant association between BIDs and minor disturbance rate. In Model B, I included the lagged minor disturbance rate as an explanatory variable. Although I did not find a significant association between BIDs and the minor disturbance rate, I found a positive association between the lagged minor disturbance rate and the outcome measure (i.e., the current level of minor disturbance). This indicates that a 10% increase in the lagged minor disturbance rate increases the current minor disturbance rate by 9.6%. The limitation of the pooled OLS estimator is that it does not control for both time-invariant factors and time-variant factors.

Additionally, once the lagged dependent variable (in this case, minor disturbance rate) is introduced as an explanatory variable, the pooled OLS estimator leads to a biased estimate of the lagged minor disturbance rate on the current minor disturbance rate. This is because the lagged minor disturbance rate is correlated with the error term. Thus, the estimated coefficient of the lagged minor disturbance rate on the current minor disturbance rate could be biased. In the following models, I used the fixed effects model to control for time-invariant factors.

Table 4.1. Pooled OLS estimations of the minor disturbance rate on BIDs.[23]

	Model A	Model B
BIDs	-.076 (.103)	.001 (.006)
Lagged dependent variable (t-1)	…	.966 (.013)*
Constant	4.615 (.057)*	-.004 (.061)
Time FE	Yes	Yes
Census-tract FE	No	No
Observations	4327	3897
Number of census tracts	372	370

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses. The natural logarithm of the minor disturbance rate is used.

* p <0.05, p < 0.01, ** p < 0.001.

[23] There is a discrepancy between the number of observations used in the two models. This is because one model (Model B) includes the lagged dependent variable as an explanatory variable, which reduces the sample size. The same logic is applied to all the models for other crime outcome models.

In Table 4.2, I used the census-tract fixed effects model, which allows BIDs to be associated with time-invariant factors, and estimated the association between BIDs and the minor disturbance rate. In Model C, I did not include the previous level of the minor disturbance rate as an explanatory variable. I did not find a significant association between BIDs and the minor disturbance rate. In Model D, I included the previous level of the minor disturbance rate as an explanatory variable. I found a significant and negative association between BIDs and the minor disturbance rate (B = -.060, $p < 0.05$). This indicates that BID areas were approximately 6% lower in the minor disturbance rate compared to non-BID areas. The estimated BID effect in this model can be interpreted as the BID effect when it is adjusted for the minor disturbance rate in the previous year. I also found a significant and positive association between the lagged minor disturbance rate and the current minor disturbance rate. This means that that a 10% increase in the lagged minor disturbance rate increases the current minor disturbance rate by 3.4%.

Although I found a negative association between BIDs and the minor disturbance rate in Model D, the estimate of BIDs on the minor disturbance rate is still not unbiased. This is because the within group estimator does not address dynamic panel bias. I further addressed this issue by adopting instrumental variables estimators suggested by Anderson and Hsiao in the following models.

Table 4.2. Fixed effects models of the association between BIDs and the minor disturbance rate

	Model C	Model D
BIDs	-.017 (.043)	-.060 (.027)*
Lagged dependent variable (t-1)	…	.360 (.026)**
Constant	4.606 (.019)**	2.806 (.124)**
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	4327	3897
Number of census tracts	372	370

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses. The natural logarithm of the minor disturbance rate is used.

* p <0.05, p < 0.01, ** p < 0.001.

In Table 4.3, I used instrumental variables estimators proposed by Anderson and Hsiao to address dynamic panel bias. In Model E, I used the Anderson-Hsiao approach with the second lag of the minor disturbance rate as an instrumental variable. I did not find a significant association between BIDs and the minor disturbance rate. In Model F, I used the Anderson-Hsiao approach with the second lag of the first difference of the minor disturbance rate as an instrumental variable. Again, I did not find any significant association between BIDs and the minor disturbance rate. I found no practical difference on the estimate of BIDs between the Anderson-Hsiao approach with the second lag of the minor disturbance rate as an instrumental variable and the Anderson-Hsiao approach with the second lag of the first difference of the minor disturbance rate as an instrumental variable. Additionally, I found no significant association between the lagged minor disturbance rate and the current minor disturbance rate in Model E and F unlike Model D in Table 4.2. Overall, the results indicate that addressing dynamic panel bias is important. After I addressed dynamic panel bias using the Anderson-Hsiao estimators, the effect of BIDs on the minor disturbance rate was no longer significant. Without eliminating dynamic panel bias, researchers may wrongly conclude that BIDs have a negative effect on the minor disturbance rate.

Table 4.3. Anderson-Hsiao estimators of the association between BIDs and the minor disturbance rate[24]

	Model E	Model F
BIDs	-.028 (.040)	-.029 (.041)
Lagged dependent variable (*t*-1)	-.069 (.305)	.019 (.095)
Constant	-.150 (.035)*	-.096 (.010)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3499	3121
Number of census tracts	365	359

Notes**.** Model E: The Anderson-Hsiao approach with the second lag of the minor disturbance rate as an instrumental variable. Model F: The Anderson-Hsiao approach with the second lag of the first difference of the minor disturbance rate as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses. The natural logarithm of the minor disturbance rate is used.

* *p* <0.05, ** *p* <0.01, *** *p* <0.001.

[24] There is a discrepancy between the number of observations used in the two models. This is because Model F uses Δy_{it-2} as an instrumental variable, which is generally not available until $t = 4$ reduces the total sample size (Roodman, 2009).

So far, I set the effect of BIDs on the minor disturbance rate as the same over time. I relaxed this assumption by creating a set of BID dummy variables in Table 4.4. I used the Anderson-Hsiao estimators. I did not find any significant association between the BID dummy variables and the minor disturbance rate in either the Anderson-Hsiao approach with the second lag of the minor disturbance rate as an instrumental variable or the Anderson-Hsiao approach with the second lag of the first difference of the minor disturbance rate as an instrumental variable. Overall, the results indicate that BIDs do not have significant impact on the minor disturbance rate based on the Anderson-Hsiao estimators.

Table 4.4. Anderson-Hsiao estimators of effects of BIDs on the minor disturbance rate over the years of operation

	Model G	Model H
Lagged dependent variable (*t*-1)	.254 (.082)*	.018 (.094)
BIDs (5 years or less)	-.023 (.046)	-.030 (.041)
BIDs (6-10 years)	-.073 (.054)	-0.43 (.048)
BIDs (over10 years)	-.103 (.067)	-.091 (.064)
Constant	-.114 (.016)**	-.096 (.010)**
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3499	3121
Number of census tracts	365	359

Notes**.** Model G: The Anderson-Hsiao approach with the second lag of the dependent variable as an instrumental variable. Model H: The Anderson-Hsiao approach with the second lag of the first difference of the dependent variable as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses. The natural logarithm of the minor disturbance rate is used.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Minor disturbance count. In table 4.5, I present the results of the pooled OLS regression analysis between BIDs and the minor disturbance count while including the time-fixed effects, enabling control for minor disturbance count trends in the study regions. In Model A, I did not include the lagged disturbance count as an explanatory variable. I did not find a significant association between BIDs and the minor disturbance count. In Model B, I included the lagged minor disturbance count as an explanatory variable. I did not find a significant association between BIDs and minor disturbance count, but I did find a positive association between the lagged minor disturbance count and the current minor disturbance count (B = 0.767, p <0.001). This means that a one-unit increase in the lagged minor disturbance count increases the current minor disturbance count by 0.7. The limitation of the pooled OLS estimator is that it does not yield control for both time-invariant factors and time-variant factors. Additionally, once the lagged minor disturbance count is included as an explanatory variable, the pooled OLS estimator does yield a consistent estimate of the lagged minor disturbance count on the current minor disturbance count. It is because the lagged minor disturbance count is associated with the error term. Thus, the estimated coefficient of the lagged minor disturbance count on the current minor disturbance count is biased. In the following models, I used the census tract fixed effects model to control for time-invariant factors.

Table 4.5. Pooled OLS estimations of the minor disturbance count on BIDs

	Model A	Model B
BIDs	3.700 (21.224)	5.822 (3.236)
Lagged dependent variable (*t*-1)	…	0.767 (.015)*
Constant	420.628*	19.717 (5.455)*
Time FE	Yes	Yes
Census-tract FE	No	No
Observations	4572	4191
Number of census tracts	381	381

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

I used the census tract fixed effects model, which allows BIDs to be correlated with time-invariant factors, and estimated the association between BIDs and the minor disturbance count in Table 4.6. In Model C, I did not include the lagged minor disturbance count as an explanatory variable. I did not find a significant association between BIDs and the minor disturbance count. In Model D, I included the lagged minor disturbance rate as an explanatory variable. Again, I did not find a significant association between BIDs and the minor disturbance count. However, I found a significant and positive association between the lagged minor disturbance count and the current minor disturbance count (B = 0.556, $p < 0.001$). This means that a one-unit increase in the lagged minor disturbance count increases the current minor disturbance count by 0.5. Although I found a positive association between the lagged minor disturbance count and the current minor disturbance count, the estimate of the lagged minor disturbance count on the current minor disturbance count is still biased. This is because the fixed effects model does not address dynamic panel bias. I addressed dynamic panel bias by using the Anderson-Hsiao estimators in the following models.

Table 4.6. Fixed effects models of the association between BIDs and the minor disturbance count

	Model C	Model D
BIDs	17.754 (20.456)	-6.760 (7.810)
Lagged dependent variable (*t*-1)	…	0.556 (.022)*
Constant	418.599 (12.235)*	110.378 (9.214)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	4572	4191
Number of census tracts	381	381

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

I used instrumental variables approaches proposed by Anderson and Hsiao to address dynamic panel bias in Table 4.7. In Model E, I used the Anderson-Hsiao approach with the second lag of the minor disturbance count as an instrumental variable. I did not find a significant association between BIDs and the minor disturbance count, but I did find a significant and positive association between the lagged minor disturbance count and the current minor disturbance count ($B = 0.550$, $p < 0.001$). This means that a one-unit increase in the lagged minor disturbance count increases the current minor disturbance count by 0.5.

In Model F, I used the Anderson-Hsiao approach with the second lag of the first difference of the minor disturbance count as an instrumental variable. Again, I did not find any significant association between BIDs and the minor disturbance count. I also did not find a significant association between the lagged minor disturbance count and the current minor disturbance count. The different results with regard to the significance of the effect of the lagged minor disturbance count on the current minor disturbance count between Model E and Model F is due to the differences in sample sizes used in the two models. When there is a discrepancy, the Anderson-Hsiao approach with the second lag of the dependent variable as an instrumental variable is preferable as it maximizes sample size, and its estimate is more accurate than the Anderson-Hsiao approach with the second lag of the first difference of the dependent variable as an instrumental variable.[25] So far, I restrict the effect of BIDs on the minor disturbance count as the same over time. I relaxed this restriction by creating a set of BID dummy variables over BID operational years in the following models.

[25] This logic is applied to other models as well. I no longer mention this in the rest of the section.

Table 4.7. Anderson-Hsiao estimators of the association between BIDs and the minor disturbance count

	Model E	Model F
BIDs	-7.684 (5.768)	-4.116 (35.345)
Lagged dependent variable (*t*-1)	0.550 (0.429)*	-5.101 (10.284)
Constant	-11.670 (1.860)*	-137.098 (241.236)
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3810	3429
Number of census tracts	381	381

Notes**.** Model E: The Anderson-Hsiao approach with the second lag of the minor disturbance count as an instrumental variable. Model F: The Anderson-Hsiao approach with the second lag of the first difference of the minor disturbance count as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

In Table 4.8, I used the Anderson and Hsiao estimators. In Model G, I found a significant and negative association between areas with six to ten years of BID operation and the minor disturbance count (B = -31.31, $p < 0.01$). This implies that areas with six to ten years of BID operation were approximately 31 less in the minor disturbance count in comparison with non-BID areas. I also found a significant and negative association between areas with over ten years of BID operation and the minor disturbance count (B = -40.66, $p < 0.01$). This indicates that areas with over ten years of BID operation were approximately 40 less in the minor disturbance count compared to non-BID areas. I also found a significant and positive association between the lagged minor disturbance count and the current minor disturbance count (B = 0.55, $p < 0.001$). This means that a one-unit increase in the lagged minor disturbance count increases the current minor disturbance count by 0.5.

In Model H, I used the Anderson-Hsiao approach with the second lag of the first difference of the minor disturbance count as an instrumental variable. In this model, I did not find any significant association between the BID dummy variables and the minor disturbance count. I also did not find a significant association between the lagged minor disturbance count and the current minor disturbance count.

The different results between Model G and Model H are attributed to the difference in sample sizes in these models. The results also suggested that it may be too restrictive to assume that BIDs have the same effect on the minor disturbance count regardless of their operational years, as their effects on the minor disturbance count can be different according to their operational years. Overall, I concluded that BIDs have a negative effect on the minor disturbance count in their later years (i.e., over five years of operation) based on the Anderson-Hsiao approach with the second lag of the dependent variable as an instrumental variable.

Table 4.8. Anderson-Hsiao estimators of effects of BIDs on the minor disturbance count over the years of operation

	Model G	Model H
Lagged dependent variable (*t*-1)	0.551 (.042)**	-4.815 (9.225)
BIDs (5 years or less)	-8.018 (5.785)	-3.335 (34.302)
BIDs (6-10 years)	-31.313 (9.485)*	114.91 (252.659)
BIDs (over10 years)	-40.667 (11.778)*	84.376 (198.720)
Constant	-10.992 (1.915)**	-131.407 (218.504)
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3810	3429
Number of census tracts	381	381

Notes**.** Model G: The Anderson-Hsiao approach with the second lag of the minor disturbance count as an instrumental variable. Model H: The Anderson-Hsiao approach with the second lag of the first difference of the minor disturbance count as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

4.2 Property Vandalism

Property vandalism rate. In Table 4.9, I present the results of the Pooled OLS regression analysis between BIDs and the property vandalism rate while including the time-fixed effects to control for minor disturbance count trends in the study regions. In Model A, I did not include the lagged property vandalism rate as an explanatory variable. I did not find a significant association between BIDs and the minor disturbance count. In Model B, I included the lagged property vandalism rate as explanatory variable. I found a significant and negative association between BIDs and the property vandalism rate (B = -0.018, $p < 0.05$). This means that BID areas were approximately 2% lower in the property vandalism rate compared to non-BID areas. I also found a significant and positive association between the lagged property vandalism rate and the current property vandalism rate (B = 0.0925 $p < 0.001$).This means that a 10% increase in the lagged property vandalism rate increases the current property vandalism rate by 1%.

In Models A and B, I did not control for time-invariant factors in this model. Moreover, once the lagged property vandalism rate is introduced as an explanatory variable (Model B), the pooled OLS estimator is no longer a consistent estimate of the lagged property vandalism rate on the current property vandalism rate. Thus, the estimated coefficient of the lagged property vandalism rate on the current property vandalism rate is biased. In the following models, I used the fixed effects model, which controls for time-invariant factors.

Table 4.9. Pooled OLS estimations of vandalism of the property vandalism rate on BIDs

	Model A	Model B
BIDs	-.038 (.081)	-.018 (.008)*
Lagged dependent variable (*t*-1)	…	.925 (.019)**
Constant	2.775 (.054)**	.037 (.052)
Time FE	Yes	Yes
Census-tract FE	No	No
Observations	4328	3898
Number of census tracts	372	371

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses. The natural logarithm of the property vandalism rate is used.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

In Table 4.10, I used the census-tract fixed effects model to control for time-invariant factors and examined the association between BIDs and the property vandalism rate. In Model C, I did not include the lagged property vandalism rate as an explanatory variable. I did not find a significant association between BIDs and the property vandalism rate. In Model D, I included the lagged property vandalism as an explanatory variable. Again, I did not find a significant association between BIDs and the property vandalism rate. However, I found a significant and positive association between the lagged property vandalism rate and the current property vandalism rate (B = 0.097, P < 0.001). This means that a 10% increase in the lagged property vandalism rate increases the current property vandalism rate by 1%. However, the estimated effect of the lagged property vandalism rate on the current property vandalism rate can be biased due to dynamic panel bias. I corrected this bias by adopting the Anderson-Hsiao estimators in the following models.

Table 4.10. Fixed effects models of the association between BIDs and the property vandalism rate

	Model C	Model D
BIDs	-.013 (.040)	.022 (.033)
Lagged dependent variable (*t*-1)	…	.097 (.025)*
Constant	2.767 (.015)*	2.257 (.068)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	4328	3898
Number of census tracts	372	371

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses. The natural logarithm of the property vandalism rate is used.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

In Table 4.11, I addressed dynamic panel bias by adopting the Anderson-Hsiao estimators and estimated the association between BIDs and the property vandalism rate. I did not find a significant association between BIDs and the property vandalism rate in either the Anderson-Hsiao approach with the second lag of the property vandalism rate as an instrumental variable or the Anderson-Hsiao approach with the second lag of the first difference of the property vandalism rate as an instrumental variable (see Model E and Model F). The lagged property vandalism rate was not significantly associated with the current property vandalism rate in both Model E and Model F. So far, I fixed the impact of BIDs on the property vandalism rate as the same over time. I relaxed this restriction by creating a set of BID dummy variables in the following models.

Table 4.11. Anderson-Hsiao estimators of the association between BIDs and the property vandalism rate

	Model E	Model F
BIDs	.096 (.059)	.094 (.059)
Lagged dependent variable (*t*-1)	-.108 (.321)	-.011 (.060)
Constant	-.131 (.021)*	-.019 (.002)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3499	3121
Number of census tracts	365	359

Notes**.** Model E: The Anderson-Hsiao approach with the second lag of the property vandalism rate as an instrumental variable. Model F: The Anderson-Hsiao approach with the second lag of the first difference of the property vandalism rate as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses. The natural logarithm of the property vandalism rate is used.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

In Table 4.12, I relaxed the restriction that the effects of BIDs on the property vandalism rate are the same in each year after the BID operation by including a set of BID dummy variables. I used the Anderson-Hsiao approach with the second lag of the property vandalism rate as an instrumental variable (Model G) and the Anderson-Hsiao approach with the second lag of the first difference of the property vandalism rate (Model H) as an instrumental variable. I did not find a significant association between BID dummy variables and the property vandalism rate in either Model G or Model H. I also did not find a significant association between the lagged property vandalism rate and the current property vandalism rate in either Model G or Model H. Overall, I concluded that BIDs do not have an effect on the property vandalism rate based on the Anderson-Hsiao estimators.

Table 4.12. Anderson-Hsiao estimators of effects of BIDs on the property vandalism rate over the years of operation

	Model G	Model H
Lagged dependent variable (*t*-1)	.041 (.057)	-.011 (.060)
BIDs (5 years or less)	.090 (.061)	.093 (.059)
BIDs (6-10 years)	.057 (.080)	.049 (.084)
BIDs (over10 years)	.023 (.093)	.022 (.104)
Constant	-.118 (.015)*	-.018 (.002)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3499	3121
Number of census tracts	365	359

Notes**.** Model G: The Anderson-Hsiao approach with the second lag of the property vandalism rate as an instrumental variable. Model H: The Anderson-Hsiao approach with the second lag of the first difference of the property vandalism rate as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses. The natural logarithm of the property vandalism rate is used.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Property vandalism count. I present the results on the association between BIDs and the property vandalism count using the pooled OLS regression analysis in Table 4.13. I controlled for property vandalism count trends in the study areas by including the census tract time-fixed effects. In Model A, I did not include the lagged property vandalism count as an explanatory variable. I did not find a significant association between BIDs and the property vandalism count. In Model B, I included the lagged property vandalism count as an explanatory variable. I also did not find a significant association between BIDs and the property vandalism count. However, I found a significant and positive association between the lagged property vandalism count and the current property vandalism count (B = 0.872, p < 0.001). This means that a one-unit increase in the lagged property vandalism count increases the current property vandalism count by 0.8. In Models A and B, I did not control for time-invariant factors. Moreover, once the lagged property vandalism count is included as an explanatory variable (Model B), the pooled OLS estimator is no longer a consistent estimate of the lagged property vandalism count on the current property vandalism count. In the following models, I used the census tract fixed effects model, which controls for time-invariant factors.

Table 4.13. Pooled OLS estimations of the property vandalism count on BIDs

	Model A	Model B
BIDs	1.597 (3.652)	-0.672 (.474)
Lagged dependent variable (t-1)	…	0.872 (.007)*
Constant	57.383 (1.928)*	-1.266 (.726)
Time FE	Yes	Yes
Census-tract FE	No	No
Observations	4572	
Number of census tracts	381	

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

In Table 4.14, I used the census-tract fixed effects model to control for time-invariant factors and investigated the association between BIDs and the property vandalism count. In Model C, I did not include the property vandalism count as an explanatory variable. I did not find a significant association between BIDs and the property vandalism count. In Model D, I included the lagged property vandalism count as an explanatory variable. Although I did not find a significant association between BIDs and the property vandalism count in this model, I found a significant and positive association between the lagged property vandalism count and the current property vandalism count ($B = 0.074$, $P < 0.001$). This means that a one-unit increase in the lagged property vandalism count increases the current property vandalism count by 0.07. However, the estimated effect of the lagged property vandalism count on the current property vandalism count is biased due to dynamic panel bias. I addressed this panel dynamic bias by using the Anderson-Hsiao estimators in the following models.

Table 4.14. Fixed effects models of the association between BIDs and the property vandalism count

	Model C	Model D
BIDs	.511 (1.336)	1.111 (1.272)
Lagged dependent variable (*t*-1)	…	.074 (.021)*
Constant	57.540 (.673)*	50.905 (1.192)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	4572	4191
Number of census tracts	381	381

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

I addressed dynamic panel bias by using the Anderson-Hsiao estimators and estimated the association between BIDs and the property vandalism count in Table 4.15. In Model E, I used the Anderson-Hsiao approach with the second lag of the property vandalism count as an instrumental variable. I did not find a significant association between BIDs and the property vandalism count. In contrast, I found a significant and positive association between the lagged property vandalism count and the current property vandalism count ($B = 0.090$, $p < 0.01$). This means that a one-unit increase in the lagged property vandalism count increases the current property vandalism count by 0.08.

In Model F, I used the Anderson-Hsiao approach with the second lag of the first difference of the property vandalism count as an instrumental variable. I did not find a significant association between BIDs and the property vandalism count. I also did not find a significant association between the lagged property vandalism count and the property vandalism count. The difference between Model E and Model F in terms of the significance of the lagged property vandalism count is due to the difference in sample sizes used in both models. So far, I restricted the impact of BIDs on the vandalism of property count as the same over time. I relaxed this restriction by using a set of BID dummy variables according to different operational years in the following models.

Table 4.15. Anderson-Hsiao estimators of the association between BIDs and the property vandalism count

	Model E	Model F
BIDs	1.658 (2.182)	2.278 (1.954)
Lagged dependent variable (t-1)	0.090 (.034)*	-0.082 (.044)
Constant	-5.399 (.637)**	-0.997 (.111)**
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3810	3429
Number of census tracts	381	381

Notes**.** Model E: The Anderson-Hsiao approach with the second lag of the property vandalism count as an instrumental variable. Model F: The Anderson-Hsiao approach with the second lag of the first difference of the property vandalism count as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

In Table 4.16, I relaxed the assumption that the impact of BIDs on the property vandalism count is the same in each year after the BID operation by including a set of BID dummy variables. I used the Anderson-Hsiao approach with the second lag of the property vandalism count as an instrumental variable in Model G. I did not find any significant association between a series of BID dummy variables and the property vandalism count. In contrast, I found a significant and positive association between the lagged property vandalism count and the current property vandalism count ($B = 0.089$, $p < 0.01$). This means that a one-unit increase in the lagged property vandalism count increases the current property vandalism count by 0.08.

In Model H, I used the Anderson-Hsiao approach with the second lag of the first difference of the property vandalism count as an instrumental variable. I did not find any significant association between BID dummy variables and the property vandalism count. I also did not find a significant association between the lagged property vandalism count and the current property vandalism count in this model. The different results on the effect of the lagged property vandalism count on the present property vandalism count between Models G and H were due to different sample sizes used in both models. Overall, I concluded that BIDs do not have an effect on the property vandalism count based on the Anderson-Hsiao estimators.

Table 4.16. Anderson-Hsiao estimators of effects of BIDs on the property vandalism count over the years of operation

	Model G	Model H
Lagged dependent variable (*t*-1)	0.089 (.034)*	-0.082 (.044)
BIDs (5 years or less)	1.642 (2.183)	2.257 (1.956)
BIDs (6-10 years)	1.286 (3.332)	1.963 (3.300)
BIDs (over10 years)	-0.526 (4.037)	-0.233 (4.052)
Constant	-5.362 (.645)**	-0.965 (0.121)**
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3810	3429
Number of census tracts	381	381

Notes**.** Model G: The Anderson-Hsiao approach with the second lag of the dependent variable as an instrumental variable. Model H: The Anderson-Hsiao approach with the second lag of the first difference of the dependent variable as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

4.3 Aggravated Assault

Aggravated assault rate. In Table 4.17, I present the results on the association between BIDs and the aggravated assault rate using the pooled OLS estimator. I controlled for aggravated assault rate trends in the study regions by including the time-fixed effects. In Model A, I found a negative association between BIDs and the aggravated assault (B = -.467, $p < 0.001$). This means that BID areas were approximately 38% lower in the aggravated assault rate in comparison with non-BID areas. In Model B, I included the lagged aggravated assault rate as an explanatory variable. I also found a negative association between BIDs and the aggravated assault rate (B = -0.070, $p < 0.001$). This means that BID areas were approximately 6.8% lower in the aggravated assault rate compared to non-BID areas. The estimated effect of BIDs on the aggravated assault rate was much smaller once I included the lagged aggravated assault rate as an explanatory variable. I also found a positive association between the lagged aggravated assault rate and the current aggravated assault rate (B = 0.879, $p < 0.001$). This means that a 10% increase in the lagged aggravated assault rate increases the current aggravated assault rate by 8%.

The limitation of the pooled OLS regression is that it is not a consistent estimate of BIDs on the aggravated assault rate if there are time-invariant factors and time-variant factors that are associated with the establishment of BIDs. Moreover, once the lagged aggravated assault rate is included as an explanatory variable, the pooled OLS regression does not yield a consistent estimate of the lagged aggravated assault rate on the current aggravated assault rate. Thus, coefficients of the lagged aggravated assault rate and BIDs are likely to be biased. In the following models, I used the fixed effects model that allows a correlation between BIDs and time-invariant factors.

Table 4.17. Pooled OLS estimations of the aggravated assault rate on BIDs

	Model A	Model B
BIDs	-.467 (.112)*	-.070 (.017)*
Lagged dependent variable (*t*-1)	…	.879 (.017)*
Constant	1.540 (.071)*	.392 (.034)*
Time FE	Yes	Yes
Census-tract FE	No	No
Observations	372	371
Number of census tracts	4328	3898

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses. The natural logarithm of the aggravated assault rate is used.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

In Table 4.18, I used the census-tract fixed effects model to examine the association between BIDs and the aggravated assault rate to control for time-invariant factors. In Model C, I did not include the lagged aggravated assault rate as an explanatory variable. I found a negative association between BIDs and the aggravated assault rate ($B = -0.200$, $p < 0.01$). This means that BID areas were approximately 18.2% lower in the aggravated assault rate compared to non-BID areas.

In Model C, I included the lagged aggravated assault rate as an explanatory variable. I found a negative association between BIDs and the aggravated assault rate ($B = -0.186$, $p < 0.001$). This means that BID areas were approximately 17% lower in the aggravated assault rate in comparison with non-BID areas. The magnitude of coefficient of BIDs was slightly lower in Model D compared to Model C. I also found a positive association between the lagged aggravated assault rate and the current aggravated assault rate ($B = 0.090$, $p < 0.05$). This means that a 10% increase in the lagged aggravated assault rate increases the current aggravated assault rate by 1%.

Compared to the results from the pooled OLS regression (Model B in Table 4.17), the effect of BIDs on the aggravated assault rate were higher in Model D. However, the fixed effects model still does not address dynamic panel bias. The estimated effect of the lagged aggravated assault rate on the current aggravated assault rate is likely to be biased, which, in turn, leads to a biased estimate of BIDs on the current aggravated assault rate. I addressed this issue by adopting the Anderson-Hsiao estimators in the following models.

Table 4.18. Fixed effects models of the association between BIDs and the aggravated assault rate

	Model C	Model D
BIDs	-.200 (.066)**	-.186 (.025)***
Lagged dependent variable (t-1)	…	.090 (.072)*
Constant	1.503 (.025)***	1.583 (.042)***
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	372	371
Number of census tracts	4328	3898

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses. The natural logarithm of the aggravated assault rate is used.

* p <0.05, ** p < 0.01, *** p < 0.001.

In Table 4.19, I addressed dynamic panel bias by using the Anderson-Hsiao estimators and estimated the dynamic relationships between BIDs and the aggravated assault rate. In Model E, I used the Anderson-Hsiao approach with the second lag of the aggravated assault rate as an instrumental variable. I found a negative association between BIDs and the aggravated assault rate (B = -0.308, p <0.05). This means that BID areas were approximately 26.6% lower in the aggravated assault rate compared to non-BID areas. In Model F, I used the Anderson-Hsiao approach with the second lag of the first difference of the aggravated assault rate as an instrumental variable. I found a negative association between BIDs and the aggravated assault rate (B = -0.301, p <0.05). This means that BID areas were 26% lower in the aggravated assault rate in comparison with non-BID areas. The magnitude of coefficients on BIDs were similar between the Anderson-Hsiao approach with the second lag of the aggravated assault rate as an instrumental variable and the Anderson-Hsiao approach with the second lag of the first difference of the aggravated assault rate as an instrumental variable.

Overall, I found a negative association between BIDs and the aggravated assault rate. The results also suggest that the pooled OLS estimator underestimates the effect of BIDs on aggravated assault rate and the fixed effects model overestimates the effect of BIDs on the aggravated assault rate in comparison with the Anderson- Hsiao estimators. In the following models, I relaxed the assumption that the effects of BIDs on the aggravated assault rate are the same over years.

Table 4.19. Anderson-Hsiao estimators of the association between BIDs and the aggravated assault rate

	Model E	Model F
BIDs	-.308 (.124)*	-.301 (.120)*
Lagged dependent variable (t-1)	.103 (.098)	-.005 (.049)
Constant	-.108 (.028)**	-.015 (.009)
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3499	3121
Number of census tracts	365	359

Notes**.** Model E: The Anderson-Hsiao approach with the second lag of the aggravated assault rate as an instrumental variable. Model F: The Anderson-Hsiao approach with the second lag of the first difference of the aggravated assault rate as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses. The natural logarithm of the aggravated assault rate is used.

* p <0.05, ** p < 0.01, *** p < 0.001.

In Table 4.20, I included a series of BID dummy variables to differentiate the effects of BIDs on the aggravated assault rate according to operation years. In Model G, I found a negative and significant association between five years or less of BID operation and the aggravated assault rate (B = -0.309, $p < 0.05$). This indicates that areas with five years or less of BID operation were approximately 26% lower in the aggravated assault rate in comparison with non-BID areas. I also found a negative and significant association between areas with over 10 years of BID operation and the aggravated assault rate (B = -0.347, $p < 0.05$). This indicates that areas with over 10 years of BID operation were approximately 29% lower in the aggravated assault rate compared to non-BID areas. I also found a positive and significant association between the lagged aggravated assault rate and the current aggravated assault rate (B = 0.105, $p < 0.05$). This means that a 10% increase in the lagged aggravated assault rate increases the current lagged aggravated assault rate by 1%.

In Model H, I found a negative and significant association between five years or less of BID operation and the aggravated assault rate (B = -0.302, $p < 0.05$). This indicates that five years or less of BID operation areas were approximately 26% lower in the aggravated assault rate in comparison with non-BID areas. However, I did not find a significant association between areas with over10 years of BID operation and the aggravated assault rate, unlike Model G. I also did not find a significant association between the lagged aggravated assault rate and the current aggravated assault rate. The discrepancy between Model G and Model H is due to a difference in sample size. When there is a discrepancy, the Anderson-Hsiao approach with the second lag of the dependent variable as an instrumental variable is preferable. This is because it maximizes sample size and its estimate is more accurate than the Anderson-Hsiao approach with the second lag of the first difference of the dependent variable as an instrumental variable. Overall, I found a negative and significant association between the BID variable for five years or less of operation and the aggravated assault rate. I also found that areas with over 10 years of BID operation were lower in the aggravated assault rate in comparison with non-BID areas based on the Anderson-Hsiao estimator with the second lag of the dependent variable as an instrumental variable.

Table 4.20. Anderson-Hsiao estimators of BIDs on the aggravated assault rate over the years of operation

	Model G	Model H
Lagged dependent variable (*t*-1)	.105 (.049)*	-.004 (.049)
BIDs (5 years or less)	-.309 (.124)*	-.302 (.120)*
BIDs (6-10 years)	-.274 (.152)	-.178 (.153)
BIDs (over10 years)	-.347 (.166)*	-.317 (.176)
Constant	-.107 (.028)**	-.015 (.009)
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3499	3121
Number of census tracts	365	359

Notes**.** Model G: The Anderson-Hsiao approach with the second lag of the aggravated assault rate as an instrumental variable. Model H: The Anderson-Hsiao approach with the second lag of the first difference of the aggravated assault rate as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses. The natural logarithm of the aggravated assault rate is used.

* p <0.05, ** p < 0.01, *** p < 0.001.

Aggravated assault count. I present the results on the association between BIDs and the aggravated assault count using the pooled OLS estimator in Table 4.21. I controlled for aggravated assault count trends in the study regions by including the time-fixed effects. In Model A, I found a negative association between BIDs and the aggravated assault count (B = -8.032, p < 0.001). This means that BID areas were approximately 8 lower in the aggravated assault count in comparison with non-BIDs. In Model B, I included the lagged aggravated assault count as an explanatory variable. I did not find a significant association between BIDs and the aggravated assault count. However, I found a positive significant and positive association between the lagged aggravated assault count and the current aggravated assault count (B = 0.903, p < 0.001). This means that a one-unit increase in the lagged aggravated assault count increases the current aggravated assault count by 0.9.

The limitation of the pooled OLS estimator is that it is not a consistent estimate of BIDs on the aggravated assault count if there are time-invariant factors and time-variant factors that are associated with the establishment of BIDs. Moreover, once the lagged aggravated assault count is introduced as an explanatory variable, the pooled OLS regression does not yield a consistent estimate of the lagged aggravated assault count on the current aggravated assault count. I used the fixed effects model that allows BIDs to be correlated with time-invariant factors in the following models.

Table 4.21. Pooled OLS estimations of the aggravated assault count on BIDs

	Model A	Model B
BIDs	-8.032 (2.204)*	-0.581 (.317)
Lagged dependent variable (*t*-1)	…	0.903 (.008)*
Constant	22.306 (1.221)*	6.235 (.504)*
Time FE	Yes	Yes
Census-tract FE	No	No
Observations	4572	4191
Number of census tracts	381	381

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses.

* p <0.05, ** p < 0.01, *** p < 0.001.

In Table 4.22, I used the census-tract fixed effects model to control for time-invariant factors and examined the association between BIDs and the aggravated assault count. I did not include the lagged aggravated assault rate as an explanatory variable in Model C. I did not find a significant association between BIDs and the aggravated assault count. In Model D, I included the lagged aggravated assault count as an explanatory variable. I did not find a significant association between BIDs and the aggravated assault count. However, I found a significant and positive association between the lagged aggravated assault count and the current aggravated assault count (B = 0.187, p < 0.001).This means that a one-unit increase in the lagged aggravated assault count increases the current aggravated assault count by 0.1.

It should be noted that the fixed effects model still does not address dynamic panel bias. Thus, the estimated effect of the lagged aggravated assault count on the current aggravated assault count is likely to be biased. As a result, the effect of BIDs on the current aggravated assault count can be biased as well. I addressed this dynamic panel bias by using the Anderson-Hsiao estimators in the following models.

Table 4.22. Fixed effects models of the association between BIDs and the aggravated assault count.

	Model C	Model D
BIDs	-1.155 (.640)	-0.719 (.955)
Lagged dependent variable (*t*-1)	…	.187 (.015)*
Constant	21.313 (.429)*	21.400 (.542)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	4572	4191
Number of census tracts	381	381

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses.

* p <0.05, ** p < 0.01, *** p < 0.001.

In Table 4.23, I addressed dynamic panel bias by using the Anderson-Hsiao estimators and estimated the dynamic relationships between BIDs and the aggravated assault count. In Model E, I used the Anderson-Hsiao approach with the second lag of the aggravated assault count as an instrumental variable. I did not find a significant association between BIDs and the aggravated assault count, but I did find a significant and positive association between the lagged aggravated assault count and the current aggravated assault count ($B = 0.240$, $p < 0.001$). This means that a one-unit increase in the lagged aggravated assault count increases the current aggravated assault count by 0.2.

In Model F, I used the Anderson-Hsiao approach with the second lag of the first difference of the aggravated assault count as an instrumental variable to address dynamic panel bias. I did not find a significant association between BIDs and the aggravated assault count in this model. I also did not find a significant association between the lagged aggravated assault count and the current aggravated assault count. The different results on the significance of the lagged aggravated assault count from the Anderson-Hsiao estimators were due to the different number of observations used in two estimators. In the following models, I relaxed the restriction that the effects of BIDs on the aggravated assault count are the same over years.

Table 4.23. Anderson-Hsiao estimators of the association between BIDs and the aggravated assault count

	Model E	Model F
BIDs	-2.63 (1.555)	-1.950 (1.212)
Lagged dependent variable (t-1)	0.240 (.053)**	-.076 (.047)
Constant	-2.514 (.520)**	-0.438 (.165)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3810	3429
Number of census tracts	381	381

Notes**.** Model E: The Anderson-Hsiao approach with the second lag of the dependent variable as an instrumental variable. Model F: The Anderson-Hsiao approach with the second lag of the first difference of the dependent variable as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses.

* p <0.05, ** p < 0.01, *** p < 0.001.

In Table 4.24, I included a set of BID dummy variables to differentiate the effects of BIDs on the aggravated assault count according to operation years. In Model G, I used the Anderson-Hsiao approach with the second lag of the aggravated assault count as an instrumental variable. I did not find any significant association between BID dummy variables and the aggravated assault count. By contrast, I found a significant and positive association between the lagged aggravated assault count and the current aggravated assault count ($B = 0.239$, $p < 0.001$). This means that a one-unit increase in the lagged aggravated assault count increases the current aggravated assault count by 1.2.

In Model H, I used the Anderson-Hsiao approach with the second lag of the first difference of the aggravated assault count as an instrumental variable. I did not find a significant association between BID dummy variables and the aggravated assault count. I also did not find a significant association between the lagged aggravated assault count and the current aggravated assault count. The different results on the effect of the lagged aggravated assault count on the present aggravated assault count between Model G and H were due to different sample sizes used in both models. Overall, I concluded that BIDs do not have an effect on the aggravated assault count based on the Anderson-Hsiao estimators.

Table 4.24. Anderson-Hsiao estimators of effects of BIDs on the aggravated assault count over the years of operation

	Model G	Model H
Lagged dependent variable (*t*-1)	0.239 (.053)**	-0.075 (.047)
BIDs (5 years or less)	-2.631 (1.555)	-1.934 (1.213)
BIDs (6-10 years)	-1.523 (2.019)	.306 (1.604)
BIDs (over10 years)	-2.140 (2.161)	-0.417 (2.009)
Constant	-2.526 (.523)**	-0.453 (.168)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3810	3429
Number of census tracts	381	381

Notes**.** Model G: The Anderson-Hsiao approach with the second lag of the aggravated assault count as an instrumental variable. Model H: The Anderson-Hsiao approach with the second lag of the first difference of the aggravated assault count as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses.

* p <0.05, ** p < 0.01, *** p < 0.001.

4.4 Theft

Theft rate. In Table 4.25, I estimated the association between BIDs and the theft rate using the pooled OLS regression. I controlled for aggravated theft rate trends in the study regions by including the time-fixed effects. In Model A, I did not include the lagged theft rate as an explanatory variable. I found a significant and positive association between BIDs and the theft rate (B = 0.346, p <0.01). This means that that BID areas were approximately 41% higher in the theft rate compared to non-BID areas. In Model B, I included the lagged theft rate as an explanatory variable. BIDs were no longer significantly associated with the theft rate. I also found a positive and significant association between the lagged theft rate and the current theft rate (B = 0.951, p <0.001). This means that a 10% increase in the theft rate increases the current theft rate by 9%. The limitation of the pooled OLS regression is that it does not control for both time-invariant and time-variant factors. Additionally, once the lagged aggravated assault rate is introduced as an explanatory variable, the pooled OLS estimator does not yield a consistent estimate of the lagged theft rate on the current theft rate. Accordingly, the estimated effect of the lagged theft rate on the current theft rate is biased. In the following models, I used the fixed effects model to control for time-invariant factors.

Table 4.25. Pooled OLS estimations of the theft rate on BIDs

	Model A	Model B
BIDs	.346 (.119)*	.005 (.009)
Lagged dependent variable (*t*-1)	…	.951 (.011)**
Constant	3.434 (.062)**	.174 (.040)**
Time FE	Yes	Yes
Census-tract FE	No	No
Observations	4464	4092
Number of census tracts	372	372

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses. The natural logarithm of theft rate is used.

* p <0.05, ** p < 0.01, *** p < 0.001.

In Table 4.26, I used the fixed effects model to estimate the association between BIDs and the theft rate to control for time-invariant factors. In Model C, I did not include the lagged theft rate as an explanatory variable. I found a negative and significant association between BIDs and the theft rate (B = -0.103, $p < 0.05$). This means that BID areas were approximately 10% lower in the theft rate compared to non-BID areas. In Model D, I included the lagged theft rate as an explanatory variable. I found a negative and significant association between BIDs and the theft rate (B = -0.084, $p < 0.05$). This means that BID areas were approximately 9% lower in the theft rate compared to non-BID areas. I also found a positive association between the lagged theft rate and the current theft rate (B = 0.126, $p < 0.01$). This indicates that a 10% increase in the theft rate increases the current theft rate by 1.2%.

The limitation of the fixed effects model is that it does not address dynamic panel bias. Thus, estimated effect of the lagged theft rate on the current theft rate is likely to be biased, which leads to a biased estimate of BIDs on the current theft assault rate in turn. I addressed dynamic panel bias by using the Anderson- Hsiao estimators in the following models.

Table 4.26. Fixed effects models of the association between BIDs and the theft rate

	Model C	Model D
BIDs	-.103 (.042)*	-.084 (.035)*
Lagged dependent variable (*t*-1)	…	.126 (.024)***
Constant	3.498 (.015)***	2.835 (.081)***
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	372	372
Number of census tracts	4464	4092

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses. The natural logarithm of the theft rate is used.

* p <0.05, ** p < 0.01, *** p < 0.001.

I used the Anderson-Hsiao estimators to address dynamic panel bias in Table 4.27. I did not find a significant association between BIDs and the theft rate in Model E using the Anderson-Hsiao approach with the second lag of the theft rate as an instrumental variable. I also did not find a significant association between the lagged theft rate and the current theft rate.

In Model F, I used the Anderson-Hsiao approach with the second lag of the first difference of the theft rate as an instrumental variable. Again, I did not find a significant association between BIDs and the theft rate. I also did not find a significant association between the lagged theft rate and the current theft rate. In the following models, I relaxed the assumption that the effects of BIDs on the theft rate are the same over years.

Table 4.27. Anderson-Hsiao estimators of the association between BIDs and the theft rate

	Model E	Model F
BIDs	-.027 (.047)	-.026 (.047)
Lagged dependent variable (t-1)	.070 (.066)	.083 (.076)
Constant	-.076 (.015)*	-.024 (.002)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	372	372
Number of census tracts	3720	3348

Notes**.** Model E: The Anderson-Hsiao approach with the second lag of the theft rate as an instrumental variable. Model F: The Anderson-Hsiao approach with the second lag of the first difference of the theft rate as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses. The natural logarithm of the theft rate is used.

* p <0.05, ** p < 0.01, *** p < 0.001.

In Table 4.28, I relaxed the restriction that the effects of BIDs on the theft rate are the same over years by creating a set of BID dummy variables. In Model G, I used the Anderson-Hsiao approach with the second lag of the theft rate as an instrumental variable. None of the BID dummy variables were significantly associated with the theft rate. The lagged theft rate was also not significantly association with the current theft rate in this model.

In Mode H, I did not find any significant association between a set of BID dummy variables and the theft rate. I also did not find a significant association between the lagged theft rate and the current theft rate. Overall, I concluded that BIDs do not have an impact on the theft rate based on the Anderson-Hsiao estimators.

Table 4.28. Anderson-Hsiao estimators of effects of BIDs on the theft rate over the years of operation

	Model G	Model H
Lagged dependent variable (*t*-1)	.077 (.066)	.087 (.076)
BIDs (5 years or less)	-.026 (.047)	-.025 (.048)
BIDs (6-10 years)	-.025 (.061)	.004 (.064)
BIDs (over10 years)	.028 (.088)	.091 (.097)
Constant	-.077 (.015)*	-.025 (.002)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3720	3348
Number of census tracts	372	372

Notes**.** Model G: The Anderson-Hsiao approach with the second lag of the theft rate as an instrumental variable. Model H: The Anderson-Hsiao approach with the second lag of the first difference of the theft rate as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses. The natural logarithm of the theft rate is used.

* p <0.05, ** p < 0.01, *** p < 0.001.

Theft count. I examined the association between BIDs and the theft count using the pooled OLS regression in Table 4.29. I controlled for aggravated theft count trends in the study regions by including the time-fixed effects. In Model A, I did not include the lagged theft count as an explanatory variable. I found a significant and positive association between BIDs and the theft count (B = 78.082, p < 0.01). This implied that BID areas were approximately 78 higher in the theft count compared to non-BID areas. In Model B, I included the lagged theft count as an explanatory variable. I found a significant and positive association between BIDs and theft count (B = 6.728, p < 0.01). This indicated that BID areas were approximately 6 higher in the theft count in comparison with non-BID areas. The magnitude of BIDs on theft count was hugely reduced from Model A to Model B. I also found a positive and significant association between the lagged theft rate and the current theft count (B = 0.850, p <0.001). This means that a one-unit increase in the lagged theft count increases the current theft count by 0.8.

The limitation of the pooled OLS regression is that it does not control for both time-invariant and time-variant factors. Moreover, once the lagged aggravated assault count is included as an explanatory variable, the pooled OLS regression does not yield a consistent estimate of the lagged theft count on the current theft count. In the following models, I used the fixed effects model that controls for time-invariant factors.

Table 4.29. Pooled OLS estimations of the theft count on BIDs

	Model A	Model B
BIDs	78.082 (20.507)**	6.728 (2.093)*
Lagged dependent variable (*t*-1)	…	0.850 (.014)**
Constant	118.662 (5.029)**	16.446 (2.432)**
Time FE	Yes	Yes
Census-tract FE	No	No
Observations	4572	4191
Number of census tracts	381	381

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses.

* p <0.05, ** p < 0.01, *** p < 0.001.

In Table 4.30, I used the fixed effects model to investigate the association between BIDs and the theft count while controlling for time-invariant factors. In Model C, I did not include the lagged theft count as an explanatory variable. I did not find a significant association between BIDs and the theft count. In Model D, I included the lagged theft count as an explanatory variable. I did not find a significant association between BIDs and the theft count. By contrast, I found a significant and positive association between the lagged theft count and the current theft count ($B = 0.257$, $p < 0.001$). This means that a one-unit increase in the lagged theft count increases the current theft count by 0.2. Overall, once I applied the fixed effects model, BIDs did not show a significant effect on the theft count.

The limitation of the fixed effects model is that it still does not solve dynamic panel bias. Thus, the estimated effect of the lagged theft count on the current theft count is likely to be biased, which leads to a biased estimate of BIDs on the current theft assault count in turn. I addressed dynamic panel bias by using the Anderson-Hsiao estimators in the following models.

Table 4.30. Fixed effects models of the association between BIDs and the theft count.

	Model C	Model D
BIDs	0.644 (4.489)	-0.589 (3.139)
Lagged dependent variable (t-1)	…	0.257 (.038)*
Constant	129.841 (2.752)*	94.584 (5.270)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	4572	4191
Number of census tracts	381	381

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses.

* p <0.05, ** p < 0.01, *** p < 0.001.

In Table 4.31, I used the Anderson and Hsiao estimators to address dynamic panel bias. In Model E, I used the Anderson-Hsiao approach with the second lag of the theft count as an instrumental variable. I did not find a significant association between BIDs and theft count. By contrast, I found a significant and positive association between the lagged theft count and the current theft count ($B = 0.369$, $p < 0.001$). This means that a one-unit increase in the lagged theft count increases the current theft count by 0.3.

In Model F, I used the Anderson-Hsiao approach with the second lag of the first difference of the theft count as an instrumental variable. Again, I did not find a significant association between BIDs and the theft count. I also did not find a significant association between the lagged theft rate and the current theft count. The different results on the significance of the lagged theft count from the Anderson-Hsiao estimators are due to different sample sizes used in both models. In the following models, I relaxed the restriction that the effects of BIDs on the theft count are the same over years.

Table 4.31. Anderson-Hsiao estimators of the association between BIDs and the theft count.

	Model E	Model F
BIDs	0.777 (4.830)	0.195 (4.008)
Lagged dependent variable (t-1)	0.369 (.067)*	0.156 (.119)
Constant	-8.129 (1.499)*	-2.178 (.386)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3810	3429
Number of census tracts	381	381

Notes**.** Model E: The Anderson-Hsiao approach with the second lag of the theft count as an instrumental variable. Model F: The Anderson-Hsiao approach with the second lag of the first difference of the theft count as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses

* p <0.05, ** p < 0.01, *** p < 0.001.

In Table 4.32, I relaxed the assumption that the effects of BIDs on the theft count are the same over years and examined the effects of BIDs on the theft count according to their operational year. In Model G, I used the Anderson-Hsiao approach with the second lag of the theft count as an instrumental variable. I did not find any significant association between BID dummy variables and the theft count. By contrast, I found a significant and positive association between the lagged theft count and the current theft count ($B = 0.363$, $p < 0.001$). This means that a one-unit increase in the lagged theft count increases the current theft count by 0.3.

In Mode H, I used the Anderson-Hsiao approach with the second lag of the first difference of the theft count as an instrumental variable. I did not find any significant association between BID dummy variables and the theft count. I also did not find a significant association between the lagged theft count and the current theft count. The different results on the significance of the lagged theft count from the Anderson-Hsiao estimators were due to differences in sample sizes used in the two models. Overall, I concluded that BIDs do not have an impact on the theft count based on the Anderson-Hsiao estimators.

Table 4.32. Anderson-Hsiao estimators of effects of BIDs on the theft count over the years of operation

	Model G	Model H
Lagged dependent variable (*t*-1)	0.363 (.067)*	0.157 (.119)
BIDs (5 years or less)	0.646 (4.813)	0.148 (4.018)
BIDs (6-10 years)	-6.835 (7.643)	-5.926 (7.133)
BIDs (over10 years)	-10.576 (11.892)	-4.423 (10.975)
Constant	-7.881 (1.524)*	-2.126 (.397)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3810	3429
Number of census tracts	381	381

Notes**.** Model G: The Anderson-Hsiao approach with the second lag of the theft count as an instrumental variable. Model H: The Anderson-Hsiao approach with the second lag of the first difference of the theft count as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

4.5 Robbery

Robbery Rate. In Table 4.33, I estimated the association between BIDs and the robbery rate using the pooled OLS estimator. I controlled for aggravated robbery rate trends in the study regions by including the time-fixed effects. In Model A, I did not include the lagged robbery rate as an explanatory variable. I did not find a significant association between BIDs and the robbery rate. In Model B, I included the lagged robbery rate as an explanatory variable. Although I did not find a significant association between BIDs and the robbery rate, I did find a significant association between the lagged robbery rate and the current robbery rate (B = 0.891, p < 0.01). This means that a 10% increase in the lagged robbery rate increases the current robbery rate by 8%.

The pooled OLS estimator does not control for both time-invariant factors and time-variant factors. In other words, the pooled OLS estimator is not a consistent estimate of BIDs on the robbery rate if there are time-invariant or time-variant factors that are correlated with the formation of BIDs. Additionally, the pooled OLS estimator is not a consistent estimate of the lagged robbery rate on the current robbery rate. Thus, the estimated coefficient of the lagged robbery rate on the current robbery rate count is biased. In the following models, I used the fixed effects model to control for time-invariant factors.

Table 4.33. Pooled OLS estimations of the robbery rate on BIDs.

	Model A	Model B
BIDs	-.002 (.116)	-.022 (.015)
Lagged dependent variable (*t*-1)	…	.891 (.015)*
Constant	1.868 (.068)*	.144 (.033)*
Time FE	Yes	Yes
Census-tract FE	No	No
Observations	4344	3917
Number of census tracts	372	371

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses. The natural logarithm of the robbery rate is used.

* p <0.05, ** p < 0.01, *** p < 0.001.

In Table 4.34, I used the census tract fixed effects model to control for time-invariant factors and estimated the association between BIDs and the robbery rate. In Model C, I did not include the lagged robbery rate as an explanatory variable. I did not find a significant association between BIDs and the robbery rate. In Model D, I included the lagged robbery rate as an explanatory variable. Although I did not find a significant association between BIDs and the robbery rate, I did find a positive and significant association between the lagged robbery rate and the current robbery rate ($B = 0.110$, $p < 0.01$). This means that a 10% increase in the lagged robbery rate increases the current robbery rate by 1%.In Models C and D, I did not address dynamic panel bias in this model. Thus, the estimated effect of the lagged robbery on the current robbery rate can be biased, which may cause a biased estimate of BIDs on the robbery rate in turn. I solve dynamic panel bias in the following models.

Table 4.34. Fixed effects models of the association between BIDs and the robbery rate.

	Model C	Model D
BIDs	.034 (.051)	.062 (.049)
Lagged dependent variable (*t*-1)	…	.110 (.022)*
Constant	1.843 (.022)*	1.576 (.048)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	4344	3917
Number of census tracts	372	371

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses. The natural logarithm of the robbery rate is used.

* p <0.05, ** p < 0.01, *** p < 0.001.

In Table 4.35, I addressed dynamic panel bias by using the Anderson-Hsiao estimators. In Model E, I used the Anderson-Hsiao approach with the second lag of the robbery rate as an instrumental variable. I did not find a significant association between BIDs and the robbery rate in this model. Additionally, I did not find a significant association between the lagged robbery rate and the current robbery rate. In Model F, I used the Anderson-Hsiao approach with the second lag of the first difference of the robbery rate as an instrumental variable. Again, I did not find a significant association between BIDs and the robbery rate. Additionally, I did not find a significant association between the lagged robbery rate and the current robbery rate. In the following models, I relaxed the restriction that the effects of BIDs on the theft rate are the same over years by including a set of BID dummy variables.

Table 4.35. Anderson-Hsiao estimators of the association between BIDs and the robbery rate

	Model E	Model F
BIDs	-.059 (.184)	.049 (.080)
Lagged dependent variable (*t*-1)	1.603 (.849)	.007 (.056)
Constant	.068 (.082)	-.012 (.003)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3518	3136
Number of census tracts	365	363

Notes**.** Model C: The Anderson-Hsiao approach with the second lag of the robbery rate as an instrumental variable. Model D: The Anderson-Hsiao approach with the second lag of the first difference of the robbery rate as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses. The natural logarithm of the robbery rate is used.

* p <0.05, ** p < 0.01, *** p < 0.001.

In Table 4.36, I included a series of BID dummy variables to differentiate the effects of BIDs on the robbery rate over BID operation years. In Model G, I used the Anderson-Hsiao approach with the second lag of the robbery rate as an instrumental variable to address dynamic panel bias. I did not find any significant association between BID dummy variables and the robbery rate in this model. However, I did find a significant association between the lagged robbery rate and the current robbery rate ($B = 0.188$, $p < 0.001$). This means that a 10% increase in the lagged robbery rate increases the current robbery rate by 1%.

In Model H, I used the Anderson-Hsiao approach with the second lag of the first difference of the robbery rate as an instrumental variable. Again, I did not find any significant association between BID dummy variables and the robbery rate. I also did not find a significant association between the lagged robbery rate and the current robbery rate. The different results on the significance of the lagged robbery rate from the Anderson-Hsiao estimators were due to differences in sample sizes used in the two models. Overall, I concluded that BIDs do not have an effect on the robbery rate based on the Anderson-Hsiao estimators.

Table 4.36. Anderson-Hsiao estimators of effects of BIDs on the robbery rate over the years of operation

	Model G	Model H
Lagged dependent variable (*t*-1)	.188 (.051)**	.005 (.055)
BIDs (5 years or less)	.027 (.086)	.049 (.080)
BIDs (6-10 years)	.082 (.127)	.077 (.125)
BIDs (over10 years)	-.052 (.140)	-.032 (.141)
Constant	-.026 (.027)	-.011 (.004)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3518	3136
Number of census tracts	364	363

Notes**.** Model G: The Anderson-Hsiao approach with the second lag of the robbery rate as an instrumental variable. Model H: The Anderson-Hsiao approach with the second lag of the first difference of the robbery rate as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses. The natural logarithm of the robbery rate is used.

* p <0.05, ** p < 0.01, *** p < 0.001.

Robbery count. In Table 4.37, I examined the association between BIDs and the robbery count using the pooled OLS estimator. I controlled for robbery count trends in the study regions by including the time-fixed effects. In Model A, I did not include the lagged robbery count as an explanatory variable. I did not find a significant association between BIDs and the robbery count. In Model B, I included the lagged robbery count as an explanatory variable. Again, I did not find a significant association between BIDs and the robbery count. By contrast, I found a significant and positive association between the lagged robbery count and the current robbery count (B = 0.874, $p < 0.001$). This means that a one-unit increase in the lagged robbery count increases the robbery count by 0.8.

The limitation of the pooled OLS estimator is that it does not control for both time-invariant factors and time-variant factors. Namely, the pooled OLS estimator is not a consistent estimate of BIDs on the robbery count if there are time-invariant or time-variant factors that are correlated with the formation of BIDs. Additionally, the pooled OLS estimator is not a consistent estimate of the lagged robbery count on the current robbery count. This is because the lagged robbery count is correlated with the error term. I used the fixed effects model to control for time-invariant factors in the following models.

Table 4.37. Pooled OLS estimations of the robbery count on BIDs

	Model A	Model B
BIDs	2.456 (2.690)	0.056 (.369)
Lagged dependent variable (t-1)	…	0.874 (.010)*
Constant	28.524 (1.387)*	2.091 (.489)*
Time FE	Yes	Yes
Census-tract FE	No	No
Observations	4572	4191
Number of census tracts	381	381

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses.

* p <0.05, ** p < 0.01, *** p < 0.001.

I used the census tract fixed effects model to control for time-invariant factors and to examine the association between BIDs and the robbery count in Table 4.38. In Model C, I did not include the lagged robbery count as an explanatory variable. I found a significant and positive association between BIDs and the robbery count ($B = 2.149$, $p < 0.05$). This indicates that BID areas are approximately 2 higher in the robbery count in comparison with non-BID areas. In Model D, I included the lagged robbery count as an explanatory variable. I found a significant and positive association between BIDs and the robbery count ($B = 1.404$, $p < 0.05$). This means that BID areas are approximately 1 higher in the robbery count compared to non-BID areas. I also found a significant and positive association between the lagged robbery count and the current robbery count ($B = 0.249$, $p < 0.01$). This means that a one-unit increase in the lagged robbery count increases the current robbery count by 0.2

The limitation of the fixed effects model is that it does not address dynamic panel bias. Thus, the estimated effect of the lagged robbery count on the current robbery count is likely to be biased, which may cause a biased estimate of BIDs on the robbery count in turn. I address dynamic panel bias in the following models.

Table 4.38. Fixed effects models of the association between BIDs and the robbery count

	Model C	Model D
BIDs	2.149 (.999)*	1.404 (.693)*
Lagged dependent variable (*t*-1)	…	0.249 (.027)**
Constant	28.568 (.552)**	19.960 (.732)**
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	4572	4191
Number of census tracts	381	381

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses.

* p <0.05, ** p < 0.01, *** p < 0.001.

In Table 4.39, I addressed dynamic panel bias by using the Anderson and Hsiao estimators. In Model E, I used the Anderson-Hsiao approach with the second lag of the robbery count as an instrumental variable. I did not find a significant association between BIDs and the robbery rate in this model. By contrast, I found a significant and positive association between the lagged robbery count and the current robbery count ($B = 0.288$, $p < 0.001$). This means that a one-unit increase in the lagged robbery count increases the current minor robbery count by 0.2.

In Model F, I used the Anderson-Hsiao approach with the second lag of the first difference of the robbery count as an instrumental variable. Again, I did not find a significant association between BIDs and the robbery count. I also did not find a significant association between the lagged robbery count and the current robbery count. The different results on the significant of the lagged robbery count from Model E and Model F are derived from the different number of observations used in the Anderson and Hsiao models. I relaxed the assumptions that the effects of BIDs on the theft count are the same over years in the following models.

Table 4.39. Anderson-Hsiao estimators of the association between BIDs and the robbery count

	Model E	Model F
BIDs	1.531 (1.934)	1.571 (1.583)
Lagged dependent variable (*t*-1)	0.288 (.059)***	0.006 (.050)
Constant	-1.124 (.510)*	-0.343 (.073)***
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3810	3429
Number of census tracts	381	381

Notes**.** Model C: The Anderson-Hsiao approach with the second lag of the robbery count as an instrumental variable. Model D: The Anderson-Hsiao approach with the second lag of the first difference of the robbery count as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses.

* p <0.05, ** p < 0.01, *** p < 0.001.

I included as a series of BID dummy variables to differentiate the effects of BIDs on the theft count over BID operation years in Table 4.40. In Model G, I used the Anderson-Hsiao approach with the second lag of the robbery count as an instrumental variable to address dynamic panel bias. I did not any significant association between BID dummy variables and the robbery count in this model. By contrast, I found a significant association between the lagged robbery count and the current robbery count ($B = 0.278$, $p < 0.001$). This means that a one-unit increase in the lagged robbery count increases the current minor robbery count by 0.2.

In Model H, I used the Anderson-Hsiao approach with the second lag of the first difference of the robbery count as an instrumental variable. Again, I did not find any significant association between BID dummy variables and the robbery count. I also did not find a significant association between the lagged robbery count and the current robbery count. The discrepancy with regard to the significance of the lagged robbery count in Model G and Model H was due to the differences in sample sizes used in the Anderson-Hsiao estimators. Overall, I concluded that BIDs do not have an effect on the robbery count based on the Anderson-Hsiao estimators.

Table 4.40. Anderson-Hsiao estimators of effects of BIDs on the robbery count over the years of operation

	Model G	Model H
Lagged dependent variable (*t*-1)	0.278 (.059)**	0.004 (.050)
BIDs (5 years or less)	1.482 (1.922)	1.540 (1.579)
BIDs (6-10 years)	1.791 (2.414)	2.070 (2.099)
BIDs (over10 years)	-3.936 (2.937)	-1.858 (2.66)
Constant	-1.040 (.512)*	-0.299 (.079)**
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3810	3429
Number of census tracts	381	381

Notes**.** Model G: The Anderson-Hsiao approach with the second lag of the robbery count as an instrumental variable. Model H: The Anderson-Hsiao approach with the second lag of the first difference of the robbery count as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses.

* p <0.05, ** p < 0.01, *** p < 0.001.

4.6 Burglary

Burglary rate. In Table 4.41, I examined the association between BIDs and the burglary rate by using the pooled OLS estimator. I controlled for burglary rate trends in the study regions by including the time-fixed effects. In Model A, the lagged burglary rate was not included as an explanatory variable. I did not find a significant association between BIDs and the burglary rate. In Model B, I included the lagged burglary rate as an explanatory variable. Although I did not find a significant association between BIDs and the burglary rate, I did find a significant and positive association between the lagged burglary rate and the current burglary rate ($B = 0.862$, $p < 0.001$). This means that a 10% increase in the lagged burglary rate increases the current burglary rate by 8%.

The limitation of the pooled OLS regression is that it does not control for both time-variant and time-invariant factors. It is also a biased estimate once the lagged dependent variable is introduced as an explanatory variable. Thus, the estimated a coefficient of the lagged burglary rate on the current burglary rate could be biased. In the following models, I used the fixed effects model to control for the time-invariant factor.

Table 4.41. Pooled OLS estimations of the burglary rate on BIDs

	Model A	Model B
BIDs	.004 (.086)	-.010 (.014)
Lagged dependent variable (t-1)	…	.862 (.035)**
Constant	2.342 (.050)**	.219 (.076)*
Time FE	Yes	Yes
Census-tract FE	No	No
Observations	4435	4044
Number of census tracts	372	372

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses. The natural logarithm of the burglary rate is used.

* p <0.05, ** p < 0.01, *** p < 0.001.

In Table 4.42, I used the fixed effects model to control for time-invariant factors. In Model C, I did not include the lagged burglary rate as an explanatory variable. I did not find a significant association between BIDs and the burglary rate. In Model D, I included the lagged burglary rate as an explanatory variable. Although I did not find a significant association between BIDs and the burglary rate, I did find a significant and positive association between the burglary rate and the current burglary rate ($B = 0.058$, $p < 0.05$). This means that a 10% increase in the lagged burglary rate increases the current burglary rate by 0.5%.

The limitation of the fixed effects model is that it does not address dynamic panel bias. Accordingly, the estimated effect of the lagged burglary rate on the current burglary rate is likely to be biased, which leads a biased estimate of BIDs on the burglary rate in turn. I address dynamic panel bias in the following models by using the Anderson-Hsiao estimators.

Table 4.42. Fixed effects models of the association between BIDs and the burglary rate

	Model C	Model D
BIDs	-.045 (.049)	.005 (.045)
Lagged dependent variable (t-1)	…	.058 (.023)*
Constant	2.354 (.021)**	1.829 (.050)**
Time FE	Yes	Yes
Census-tract FE	No	No
Observations	4435	
Number of census tracts	372	

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses. The natural logarithm of the burglary rate is used.

* p <0.05, ** p < 0.01, *** p < 0.001.

In Table 4.43, I investigated the dynamic relationship between BIDs and the burglary rate while addressing dynamic panel bias by using the Anderson and Hsiao estimators. In Model E, I used the Anderson-Hsiao approach with the second lag of the burglary rate as an instrumental variable. Although I did not find a significant association between BIDs and the burglary rate, I did find a positive and significant association between the lagged burglary rate and the current burglary rate ($B = 0.168$, $p < 0.01$). This means that a 10% increase in the lagged burglary rate increases the current burglary rate by 1%.

In Model F, I used the Anderson-Hsiao approach with the second lag of the first difference of the burglary rate as an instrumental variable. I did not find a significant association between BIDs and the burglary rate. I also did not find a significant association between the lagged burglary rate and the current burglary rate in this model. The discrepancy between Model E and Model F regarding the significance of the lagged burglary rate is due to the sample size difference.

So far, I imposed an assumption that the effect of BIDs on the burglary rate is the same regardless of the years of BID operation. I relaxed this assumption by creating a set of BID dummy variables according to the years of BID operation in the following models.

Table 4.43. Anderson-Hsiao estimators of the association between BIDs and the burglary rate

	Model E	Model F
BIDs	-.010 (.086)	.039 (.076)
Lagged dependent variable (t-1)	.168 (.048)*	.077 (.065)
Constant	-.214 (.028)**	-.012 (.003)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3659	3279
Number of census tracts	372	372

Notes**.** Model C: The Anderson-Hsiao approach with the second lag of the burglary rate as an instrumental variable. Model D: The Anderson-Hsiao approach with the second lag of the first difference of the burglary rate as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses. The natural logarithm of the burglary rate is used.

* p <0.05, ** p < 0.01, *** p < 0.001.

In Table 4.44, I estimated effects of BIDs on the burglary rate according to the years of BID operation. In Model G, I used the Anderson-Hsiao approach with the second lag of the burglary as an instrumental variable. I did not find any significant association between BID dummy variables and the burglary rate. I found a significant and positive association between the lagged burglary rate and the current burglary rate ($B = 0.167$, $p < 0.01$). This means that a 10% increase in the lagged burglary rate increases the current burglary rate by 1%.

In Model H, I used the Anderson-Hsiao approach with the second lag of the first difference of the burglary rate as an instrumental variable. Again, I did not find any significant association between BID dummy variables and the burglary rate. Overall, I concluded that BIDs do not affect the burglary rate, based on the Anderson-Hsiao estimators.

Table 4.44. Anderson-Hsiao estimators of effects of BIDs on the burglary rate over the years of operation

	Model G	Model H
Lagged dependent variable (*t*-1)	.167 (.048)*	.080 (.066)
BIDs (5 years or less)	-.008 (.086)	.042 (.076)
BIDs (6-10 years)	.069 (.108)	.127 (.108)
BIDs (over10 years)	.139 (.131)	.299 (.261)
Constant	-.217 (.028)**	.196 (.025)**
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3659	3279
Number of census tracts	372	372

Notes**.** Model G: The Anderson-Hsiao approach with the second lag of the burglary rate as an instrumental variable. Model H: The Anderson-Hsiao approach with the second lag of the first difference of the burglary rate as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses. The natural logarithm of the burglary rate is used.

* p <0.05, ** p < 0.01, *** p < 0.001.

Burglary count. In Table 4.45, I examined the association between BIDs and the burglary count by using the pooled OLS regression, which does not control for both time-invariant and time-variant factors. In Model A, the lagged burglary count was not included as an explanatory variable. I did not find a significant association between BIDs and the burglary count. In Model B, I included the lagged burglary count as an explanatory variable. On the one hand, I did not find a significant association between BIDs and the burglary count. On the other hand, I found a significant and positive association between the lagged burglary count and the current burglary count (B = 0.788, p < 0.001). This means that a one-unit increase in the lagged burglary count increases the current burglary count by 0.7.

The limitation of the pooled OLS estimator is that it does not control for both time-variant and time-invariant factors. It also yields a biased estimate on the effect of the lagged burglary count on the current burglary count. Accordingly, the estimated coefficient of the lagged burglary count on the current burglary count is likely to be biased. I used the fixed effects model to control for time-invariant factor in the following models.

Table 4.45. Pooled OLS estimations of the burglary count on BIDs

	Model A	Model B
BIDs	2.957 (2.436)	0.179 (.454)
Lagged dependent variable (*t*-1)	…	0.788 (.014)*
Constant	38.725 (1.464)*	4.344 (.813)*
Time FE	Yes	Yes
Census-tract FE	No	No
Observations	4572	4191
Number of census tracts	381	381

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

I used the fixed effects model to control for time-invariant factors in Table 4.46. In Model C, I did not include the lagged burglary count as an explanatory variable. I controlled for burglary count trends in the study regions by including the time-fixed effects. I did not find a significant association between BIDs and the burglary count. In Model D, I included the lagged burglary count as an explanatory variable. I did not find a significant association between BIDs and the burglary count. By contrast, I found a significant and positive association between BIDs and the lagged burglary count ($B = 0.151$, $p < 0.001$). This means that a one-unit increase in the lagged burglary count increases the current burglary count by 0.1.

The estimated effect of the lagged burglary count on the current burglary count from the fixed effects model is likely to be biased due to dynamic panel bias, which may lead a biased estimate of BIDs on the burglary count in turn. In the following models, I address dynamic panel bias by using the Anderson-Hsiao estimators.

Table 4.46. Fixed effects models of the association between BIDs and the burglary count.

	Model C	Model D
BIDs	-0.116 (1.958)	1.491 (1.548)
Lagged dependent variable (t-1)	…	.151 (.024)*
Constant	39.168 (.791)*	29.072 (.998)*
Time FE	Yes	Yes
Census-tract FE	No	No
Observations	4572	4191
Number of census tracts	381	381

Notes**.** FE: Fixed effect. Cluster-robust standard errors at the census-tract level are presented in parentheses.

* p <0.05, ** p < 0.01, *** p < 0.001.

In Table 4.47, I investigated the dynamic relationship between BIDs and the burglary count while addressing dynamic panel bias by utilizing the Anderson-Hsiao estimators. In Model E, I used the Anderson-Hsiao approach with the second lag of the burglary count as an instrumental variable. I did not find a significant association between BIDs and the burglary count. I also did not find a significant association between the lagged burglary count and the current burglary count. In Model F, I used the Anderson-Hsiao approach with the second lag of the first difference of the burglary count as an instrumental variable. I did not find a significant association between BIDs and the burglary count. I also did not find a significant association between the lagged burglary count and the current burglary count in this model.

So far, I assumed that the effect of BIDs on the burglary count is same regardless of the years of BID operation. I relaxed this restriction by allowing BIDs' effects on the burglary count to be different according to the years of BID operation in the following models.

Table 4.47. Anderson-Hsiao estimators of the association between BIDs and the burglary count.

	Model E	Model F
BIDs	0.212 (.535)	1.856 (1.722)
Lagged dependent variable (t-1)	1.087 (1.766)	-0.043 (.044)
Constant	-5.808 (.782)**	-0.257 (.088)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3810	3429
Number of census tracts	381	381

Notes**.** Model C: The Anderson-Hsiao approach with the second lag of the burglary count as an instrumental variable. Model D: The Anderson-Hsiao approach with the second lag of the first difference of the burglary count as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses.

* p <0.05, ** p < 0.01, *** p < 0.001.

In Table 4.48, I estimated effects of BIDs on burglary count according to the years of BID operation. I used the Anderson-Hsiao approach with the second lag of the burglary count as an instrumental variable in Model G. I did not find any significant association between a series of BID dummy variables and the burglary count. By contrast, I found a significant and positive association between the lagged burglary count and the current burglary count ($B = 0.210$, $p < 0.001$). This means that a one-unit increase in the lagged burglary count increases the current burglary count by 0.2.

In Model H, I used the Anderson-Hsiao approach with the second lag of the first difference of the burglary count as an instrumental variable. I did not find any significant association between a set of BID dummy variables and the burglary count. I also did not find a significant association between the lagged burglary count and the current burglary count. The difference between Model G and Model H with regard to the significance of the lagged burglary count is due to different numbers of samples used in the two models. Overall, I concluded that BIDs do not affect the burglary count based on the Anderson-Hsiao estimators.

Table 4.48. Anderson-Hsiao estimators of effects of BIDs on the burglary count over the years of operation

	Model G	Model H
Lagged dependent variable (*t*-1)	0.210 (.034)**	-.042 (.044)
BIDs (5 years or less)	1.091 (1.763)	1.892 (1.722)
BIDs (6-10 years)	2.853 (2.409)	3.419 (2.480)
BIDs (over10 years)	0.779 (3.313)	6.180 (5.964)
Constant	-5.801 (.787)**	-.309 (.096)*
Time FE	Yes	Yes
Census-tract FE	Yes	Yes
Observations	3810	3429
Number of census tracts	381	381

Notes**.** Model G: The Anderson-Hsiao approach with the second lag of the burglary count as an instrumental variable. Model H: The Anderson-Hsiao approach with the second lag of the first difference of the burglary count as an instrumental variable. FE: Fixed effect. Heteroskedasticity-robust standard errors are presented in parentheses.

* p <0.05, ** $p < 0.01$, *** $p < 0.001$.

4.7 Summary of Findings

Table 4.49 presents the summary of the results on the dynamic relationships between BIDs and the crime rates over the different estimators. In terms of the minor disturbance rate, only the fixed effects model showed a significant and negative association with BIDs. However, I did not find a significant association between BIDs and the minor disturbance rate after addressing dynamic panel bias. The results suggested that it is important to address panel bias. If I did not address dynamic panel bias, I could have concluded that BIDs have an impact on the minor disturbance rate. Only the pooled OLS estimator showed a negative and significant association between BIDs and the property vandalism rate. In terms of the aggravated assault rate, I found a significant and negative association with BIDs across all the estimators. Compared to the Anderson-Hsiao estimators, the effect of BIDs on the aggravated assault rate was lower from the pooled OLS estimator and fixed effects model. In other words, the pooled OLS estimator and fixed effects model underestimated the effect of BID on the aggravated assault rate. I found a significant and negative association between BIDs and the theft rate on in the fixed effects model. However, I did not find a significant association between BIDs and the theft rate after addressing dynamic panel bias. Again, the results showed that it is important to address dynamic panel bias. Otherwise, I could have concluded that BIDs have effect on the theft rate. I did not find any significant associations between BIDs and the robbery or burglary rates over all the estimators.

Table 4.49. Dynamic relationships between BIDs and crime rates across different models

Crime rate	Pooled OLS[a]	Fixed Effects[b]	AH-1[c]	AH-2[d]
Minor disturbance[e]	X	-.060*	X	X
Property vandalism[e]	-.018*	X	X	X
Aggravated assault[e]	-.070**	-.186**	-.308*	-.301*
Theft[e]	X	-.084*	X	X
Robbery[e]	X	X	X	X
Burglary[e]	X	X	X	X

Notes. X: no significant association. a. Pooled OLS estimator; b. Fixed effects model; c. The Anderson-Hsiao approach with the second lag of the dependent variable as an instrumental variable; d. The Anderson-Hsiao approach with the second lag of the first difference of the dependent variable as an instrumental variable; and e. the natural logarithm transformation is used for all the dependent variables.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 4.50 presents the summary of the results on the dynamic relationships between BIDs and crime counts over the different estimators. On the one hand, I did not find any significant association between BIDs and minor disturbance, property vandalism, or aggravated assault counts in any of the estimators. On the other hand, I found a significant and positive association between BIDs and the theft count when I used the Pooled OLS estimator. I also found a significant and positive association between BIDs and the robbery count when I used the fixed effects model. Overall, I did not find any significant association between BIDs and crime counts when I addressed dynamic panel bias.

Table 4.50. Dynamic relationships between BIDs and crime counts

Crime count	Pooled OLS[a]	Fixed Effects[b]	AH-1[c]	AH-2[d]
Minor disturbance	X	X	X	X
Property vandalism	X	X	X	X
Aggravated assault	X	X	X	X
Theft	6.728**	X	X	X
Robbery	X	1.404*	X	X
Burglary	X	X	X	X

Notes. X: no significant association. a. Pooled OLS estimator; b. Fixed effects model; c. The Anderson-Hsiao approach with the second lag of the dependent variable as an instrumental variable; and d. The Anderson-Hsiao approach with the second lag of the first difference of the dependent variable as an instrumental variable.
* p <0.05, ** p < 0.01, *** p < 0.001.

The different results between crime rate and crime count suggest that it is important to adjust for the different numbers of population at the study areas (in this case, census tracts). Crime rates can better reflect the number of people at being risk in an area than crime counts. As the result showed, using crime counts may lead to a different conclusion than using crime rates. In the current case, if I used an aggravated assault count as a dependent variable, I was not able to find a significant association between BIDs and the aggravated assault rate.

In Table 4.51, I present the results on the dynamic relationships between BIDs and crime rates according to the years of BID operation only for the Anderson-Hsiao estimators. I did not find any significant associations between BID dummy variables on the one hand and minor disturbance and property vandalism rate variables on the other. I did find a significant and negative association between five years or less of BID operation and over 10 years of BID operation on the one hand and the aggravated assault rate on the other hand, when I used the Anderson-Hsiao approach with the second lag of the aggravated assault rate as an instrumental variable. I only found a significant and negative association between five years or less of BID operation and the aggravated assault rate when I used the Anderson-Hsiao approach with the second lag of the first difference of the aggravated assault rate as an instrumental variable.

Table 4.51. Dynamic relationships between BIDs and crime rates over the years of operation

Crime rate		AH-1[a]	AH-2[b]
	BIDs (5 years or less)	X	X
Minor disturbance[c]	BIDs (6-10 years)	X	X
	BIDs (over10 years)	X	X
	BIDs (5 years or less)	X	X
Property vandalism[c]	BIDs (6-10 years)	X	X
	BIDs (over10 years)	X	X
	BIDs (5 years or less)	-.309*	-.302*
Aggravated assault[c]	BIDs (6-10 years)	X	X
	BIDs (over10 years)	-.347*	X
	BIDs (5 years or less)	X	X
Theft[c]	BIDs (6-10 years)	X	X
	BIDs (over10 years)	X	X
	BIDs (5 years or less)	X	X
Robbery[c]	BIDs (6-10 years)	X	X
	BIDs (over10 years)	X	X
	BIDs (5 years or less)	X	X
Burglary[c]	BIDs (6-10 years)	X	X
	BIDs (over10 years)	X	X

Notes. X: no significant association. a. The Anderson-Hsiao approach with the second lag of the dependent variable as an instrumental variable; b. The Anderson-Hsiao approach with the second lag of the first difference of the dependent variable as an instrumental variable; and c. the natural logarithm transformation is used for all dependent variables.
* p <0.05, ** p < 0.01, *** p < 0.001.

The different results are derived from different sample sizes used for the two estimators. If two approaches are inconsistent, the results from the Anderson-Hsiao approach with the second lag of the aggravated assault rate as an instrumental variable are preferable. This is because it maximizes sample size and is more precise than the Anderson-Hsiao estimator with the second lag of the first difference of the dependent variable as an instrumental variable (Arellano, 1989; Roodman, 2009).

In Table 4.52, I present the summary of the results on the dynamic relationships between BIDs and crime counts according to the years of BID operation only for the Anderson-Hsiao estimators. I found a significant and negative association between the six to 10 years of BID operation and over10 years of BID operation on the one hand and the minor disturbance, property vandalism, aggravated assault, theft, robbery, or burglary count variables on the other hand, when I used the Anderson-Hsiao approach with the second lag of the aggravated assault count as an instrumental variable. I did not find any significant association between any of the BID dummy variables and the minor disturbance count when I used the Anderson-Hsiao approach with the second lag of the first difference of the aggravated assault count as an instrumental variable.

Table 4.52. Dynamic relationships between BIDs and crime counts over the years of operation

Crime count		AH-1[a]	AH-2[b]
	BIDs (5 years or less)	X	X
Minor disturbance[c]	BIDs (6-10 years)	-31.313 **	X
	BIDs (over10 years)	-40.667 **	X
	BIDs (5 years or less)	X	X
Property vandalism[c]	BIDs (6-10 years)	X	X
	BIDs (over10 years)	X	X
	BIDs (5 years or less)	X	X
Aggravated assault[c]	BIDs (6-10 years)	X	X
	BIDs (over10 years)	X	X
	BIDs (5 years or less)	X	X
Theft[c]	BIDs (6-10 years)	X	X
	BIDs (over10 years)	X	X
	BIDs (5 years or less)	X	X
Robbery[c]	BIDs (6-10 years)	X	X
	BIDs (over10 years)	X	X
	BIDs (5 years or less)	X	X
Burglary[c]	BIDs (6-10 years)	X	X
	BIDs (over10 years)	X	X

Notes. X: no significant association. a. The Anderson-Hsiao approach with the second lag of the dependent variable as an instrumental variable; and b. The Anderson-Hsiao approach with the second lag of the first- difference of the dependent variable as an instrumental variable.
* p <0.05, ** p < 0.01, *** p < 0.001.

The different results are due to different sample sizes used for two estimators. The Anderson-Hsiao estimator with the second lag of the aggravated assault count as an instrumental variable is preferable to the Anderson-Hsiao approach with the second lag of the first difference of the aggravated assault count as an instrumental variable. This is because the former maximizes sample size and its estimate is more accurate (Arellano, 1989; Roodman, 2009).

I also found that the results are different depending on whether I used crime count or rate as outcome variables. The results indicate that it is important to adjust for the different sizes of population at the study regions (in this case, census tracts). This is because each region has the different numbers of population at being risk, and crime counts cannot reflect this variation. As the results showed, one may reach a different result if they use crime counts instead of crime rates. In this case, if I used the aggravated assault count as a dependent variable, I was not able to find a significant association between BIDs and the aggravated assault rate. Additionally, I may conclude that BIDs have a significant effect on minor disturbance, if I used the minor disturbance count instead of the minor disturbance rate.

Chapter 5. CONCLUSIONS

During the past decades, BIDs have become increasingly popular urban governance mechanisms for providing local public goods, such as public safety. The self-financing nature of BIDs has made the mechanism popular as a means to address the free-rider problem in many communities and they attracted the attention of several scholars (Brooks & Strange 2008; Sutton, 2014). There is the widespread anecdotal belief among local managers and practitioners that BIDs are effective in reducing and deterring crime (MacDonald et al., 2013).

There are also theories that can explain why BIDs are an effective mechanism for crime deterrence and reduction. Both broken windows (Wilson & Kelling, 1982) and routine activity (Cohen & Felson, 1979) theories, which are closely related to the broader social disorganization theory, can be applied to explain the effects of BIDs on crime reduction. The broken windows theory argues that signs of decay, disorder neglect, including, vacant houses, abandoned cars, loitering, and graffiti attract crime, and that the presence of uniformed officers can facilitate informal social control, reduce fear of crime, and prevent crime (Wilson & Kelling, 1982). According to the routine activity theory, criminal acts result from the convergence in place and time of the presence of a motivated offender, a suitable target, and an absence of effective guardians, and that the effective guardians can facilitate the collective supervision of public areas and pressure the motivated offender to consider the potential targets less attractive (Cohen & Felson, 1979). The social disorganization theory, encompassing both broken windows and routine activity theories, suggests that the structural disadvantages of a neighborhood weakens informal social control, which in turn leads to crime acts (Shaw & Mckay, 1942). Various efforts of BIDs, including beautification, investment in sanitation and street cleaning, and hiring private guards are consistent with the mechanisms of deterring crime suggested by these criminological theories.

The theoretical mechanisms linking BIDs to crime have been investigated in several empirical studies (Brooks, 2008; Calanog, 2006; Cook & MacDonald, 2011; Hoyt, 2004; MacDonald et al., 2010; MacDonald et al., 2013). These previous researchers adopted various strategies to better estimate the effect of BIDs on crime. They investigated the possibility that the previous experience of crime could influence the current decision on BID formation and the current level of crime, but they did not investigate the dynamic relationship between BIDs and Crime. It should be noted that examining the dynamic relationship between BIDs and crime is important since a decision to establish a BID area could be influenced by the previous level of crime in the area (Hoyt, 2005). Ignoring this dynamic relationship between BIDs and crime can cause a biased estimate of BIDs on crime. To date, only Calanog's (2006) study examined the dynamic relationship between BIDs and crime (Calanog, 2006). But even Calanog did not correct for potential dynamic panel biases.

With the problems of these previous studies in mind, I investigated the impact of BIDs on crime using panel data from 1998 to 2009 in the city of Philadelphia in my study. Investigated crime outcomes were minor disturbance, property vandalism, aggravated assault, theft, robbery, and burglary. I expanded the previous literature by investigating the dynamic relationship between BIDs and crime by including the previous level of crime as an explanatory variable in the model, which opens the possibility that the previous experience of crime could affect the current decision on BID establishment and the current level of crime. At the same time, I addressed dynamic panel bias by using the Anderson-Hsiao estimators (1981/1982).

When I fixed the impacts of the BIDs on crimes as the same over time, I found a negative association between BIDs and the aggravated assault rate using the Anderson-Hsiao estimators (Table 4.19). It was estimated that BID areas were approximately 26% lower in the aggravated assault rate in comparison with non-BID areas. I also distinguished the effects of BIDs on crime rates according to their operational years since it was argued that the effects of BIDs on crime can be different depending on years of BID operation (Brooks, 2008; Cook & McDonald, 2011). I found a negative association between five years or less of BID operation and over 10 years of BID operation on the one hand, and the aggravated assault rate on the other hand, using the Anderson-Hsiao estimator with the second lag of the aggravated assault rate as an instrumental variable (Table 4.20). Areas with five years or less of BID operation were approximately 26% lower in the aggravated assault rate compared to non-BID areas. Additionally, areas with over 10 years of BID operation were approximately 29% lower in the aggravated assault rate in comparison with non-BID areas. In terms of other crime rate variables, minor disturbance, property vandalism, aggravated assault, theft, robbery, and burglary, I did not find any significant association with BIDs using the Anderson-Hsiao estimators.

It is difficult to synthesize the findings of the previous empirical studies, because there are differences in their study areas, crime measures, research designs, and statistical methods. Generally, the previous studies can be categorized into two: studies that were based on non-panel data analyses and those based on panel data analyses. In her non-panel data analyses, Hoyt (2004), used multiple regression analysis and found a negative association between BID security services and crime and a positive association between BID sanitation services and crime in Philadelphia. Using survey data in Los Angeles for their multi-level analyses, MacDonald and his colleagues (2013) investigated the association between BIDs and violent crime outcomes for adolescents. They did not find a significant association between BIDs and violent crime.

Those previous empirical studies that were based on panel data are more comparable to my dissertation than the ones based on non-panel data analyses. Among the three studies based on panel data analyses, Calnong (2006)'s study is the only one that investigated the dynamic relationship between BIDs and crime outcomes from 1997 to 2002 in Philadelphia. He found significant associations between BIDs and robbery, theft, and auto theft. However, he did not find a significant association between BIDs on the one hand and murder, rape, aggravated assault, and burglary on the other. It should be noted that he did not address the dynamic panel bias in his analyses. Brooks (2008) investigated the impact of BIDs on various crime outcomes, such as robbery, burglary, and auto burglary and theft between 1990 and 2002 in Los Angeles. She found negative associations between BIDs and various crime outcomes using the fixed effects model and matching estimators. MacDonald and his colleagues (2010), used a hierarchical model on the crime data between 1994 and 2005 in Los Angeles; they did not find any significant associations between BIDs and robbery or violent crimes. Using panel data between 1994 and 2005 and fixed effects models, Cook and MacDonald (2011) investigated the impacts of BIDs on crime. They found negative associations between BIDs and robbery, assault, burglary, and auto theft. .

Overall, two studies (Brooks, 2008; Cook & MacDonald, 2011) found significant and negative impacts of BIDs on crime. One study (Calanog, 2006) found a significant negative association between BIDs and a portion of crime outcomes. Two studies (Macdonald et al., 2010; MacDonald et al., 2013) did not find any significant associations between BIDs and crime. Additionally, there is one study (Hoyt, 2004) that found a mixed result in that different types of BID services affect crime differently.

The results of my dissertation show that BIDs only have a negative impact on the aggravated assault rate. It should be also noted that some of the previous studies (e.g., Brooks, 2008; Cook & MacDonald, 2011), which found a significant and negative association between BIDs and crimes, used crime counts instead of crime rates. In my study, crime rates were more likely to be associated with BIDs than crime counts. The results show that it is important to adjust for the different numbers of population at a study area by using crime rates better reflect the number of people at risk in the study area than crime counts. As the results of my dissertation show, using counts may lead to a different conclusion than using crime rates. It is possible that the previous studies found a spurious effect of BIDs on crime outcomes, as they used crime counts instead of crime rates.

In conclusion, the results of my dissertation partially support that BIDs had some effects on crime outcomes in the city of Philadelphia from 1998 to 2009, after the dynamic relationship between BIDs and crime was taken into account. Among the six crime outcomes I investigated BIDs only have a negative effect on the aggravated assault rate. The results show that BID areas were approximately 26% lower aggravated assault rates compared to non-BID areas based on the Anderson-Hsiao estimators. I also found that the effects of BIDs on the aggravated assault rate vary depending on their operational years, based on the Anderson-Hsiao estimator, with the second lag of the aggravated assault rate as an instrumental variable. Areas with five years or less of BID operation were approximately 26% lower aggravated assault rates compared to non-BID areas and areas with over 10 years of BID operation were approximately 29% lower aggravated assault rates in comparison with non-BID areas.

The results of my dissertation should not be disappointing to those practitioners and researchers who think that BIDs are effective in crime reduction. My results may indicate that simply establishing a BID may not be enough to achieve such a goal. It was reported that the impact of BIDs on crime can vary depending on the amount of money allocated to crime reduction (Brooks, 2008; Cooks & MacDonald, 2011). Moreover, there are variations among BIDs with regard to the organizational and legal structures, and functions, and their surrounding environments (Gross, 2005). Furthermore, it was argued that some BIDs in Philadelphia had internal and external issues, and thus not all the BIDs in the city function as well as originally intended (Hoffman & Houstoun, 2010). All in all, the results may indicate that understanding the area around BID neighborhoods and their relationships with BIDs, and the structure and implementation strategies of BIDs, are more important for understanding crime reduction. This is an important issue that warrants further investigation as it can provide valuable information for local governments and neighborhood property owners in managing and redesigning BIDs to be effective urban mechanisms to reduce and prevent crime.

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Appendix A: Previous studies on the impact of BIDs on crime

Author	Area of study	Study period	Unit of analysis	Statistical model	Crime outcomes	Results
Hoyt (2004)	Philadelphia	1998-2001 (aggregate measure)	Individual crime occurrence	Multiple linear regression	Property crime counts	BID security services have a negative effect on property crime. BID sanitation services have a positive effect on property crime.
Calanog (2006)	Philadelphia	1997-2002	Census tract	Fixed effects model	Murder, rape, aggravated assault, robbery, theft, auto theft, and burglary counts.	BIDs have no impact on murder, rape, and aggravated assault. BIDs have effect on robbery, theft, and auto theft.
Brooks (2008)	Los Angeles	1990-2002	LAPD police-reporting district	Fixed effects model Matching estimators: (1) Matching BIDs with almost-BID-forming neighborhoods; (2) Propensity score matching: Matching treated reporting districts (BID areas) with untreated reporting districts (non-BID areas) with similar pre-BID attributes; (3)Geographic matching: Comparing changes in crime in BID areas with their neighbors' changes in crime	Violent crimes of murder and non-negligent manslaughter, forcible rape, robbery, and aggravated assault, along with the non-violent crimes of burglary, larceny-theft, and motor vehicle theft counts.	Generally, BIDs have an effect on reduction in crime outcomes
MacDonald,	Los Angeles	1994-2005	BID areas	Bayesian hierarchical	Robbery and violent crime	BIDs were associated

Golinelli, Stokes, & Bluthenthal (2010)				Poisson model	counts	with reduction in robbery and violent crime but they were not statistically significant
Cook & MacDonald (2011)	Los Angeles	1994-2005	LAPD police-reporting district	Fixed effects model	Crime and arrest counts	BIDs have a negative effect on crime and arrest counts.
MacDonald, Stokes, Grunwald, & Bluthental (2013)	Los Angeles	A survey of 2006 and 2007	Level 3: census tract Level 2: interview wave Level 1: Respondent	Multilevel analysis	Self-reported violent crime	BIDs have no effect on self-reported crime.

Appendix B: Census tracts of BID and non-BID areas in the City of Philadelphia

BID census tracts	100	200	300	400	500	600	700	800	900	1000	1200	1500	1600	1700
	2300	2400	2800	2900	3800	3901	3902	4001	4500	4700	4800	4900	7400	7600
	7700	7800	7900	8600	8700	8800	8900	9000	9100	9200	11600	11700	12000	12100
	12200	12400	12500	12600	17900	18000	18500	18600	18700	18800	18900	20900	21000	21100
	21200	21300	21400	21500	21600	21700	22700	22800	23100	23200	23300	23700	23800	24000
	24100	24600	24700	25200	25300	25500	25600	25700	25900	26000	26301	26302	26500	26600
	26700	26800	29400	30000										
Non-BID census tracts	1100	1300	1400	1800	1900	2000	2100	2200	2500	2600	2700	3000	3100	3200
	3300	3400	3500	3600	3701	3702	4002	4101	4102	4201	4202	4300	4400	4600
	5000	5100	5200	5400	5500	5600	5700	5800	5900	6000	6100	6200	6300	6400
	6500	6600	6700	6800	6900	7000	7100	7200	7300	7500	8000	8100	8200	8301
	8302	8400	8500	9300	9400	9500	9600	9700	9800	9900	10000	10100	10200	10300
	10400	10500	10600	10700	10800	10900	11000	11100	11200	11300	11400	11500	11800	11900
	12300	12700	12800	12900	13000	13100	13200	13300	13400	13500	13600	13700	13800	13900
	14000	14100	14200	14300	14400	14500	14600	14700	14800	14900	15000	15100	15200	15300
	15400	15500	15600	15700	15800	15900	16000	16100	16200	16300	16400	16500	16600	16700
	16800	16901	16902	17000	17100	17200	17300	17400	17500	17601	17602	17700	17800	18100
	18200	18300	18400	19000	19100	19200	19300	19400	19500	19600	19700	19800	19900	20000
	20100	20200	20300	20400	20500	20600	20700	20800	21800	21900	22000	22100	22200	22300
	22400	22500	22600	22900	23000	23400	23500	23600	23900	24200	24300	24400	24500	24800
	24900	25000	25100	25400	25800	26100	26200	26400	26900	27000	27100	27200	27300	27400
	27500	27600	27700	27800	27900	28000	28100	28200	28300	28400	28500	28600	28700	28800
	28900	29000	29100	29200	29300	29500	29600	29700	29800	29900	30100	30200	30300	30400
	30500	30600	30700	30800	30900	31000	31100	31200	31300	31400	31500	31600	31700	31800
	31900	32000	32100	32200	32300	32400	32500	32600	32700	32800	32900	33000	33100	33200
	33300	33400	33500	33600	33700	33800	33900	34000	34100	34200	34300	34400	34500	34600
	34701	34702	34801	34802	34803	34900	35100	35200	35301	35302	35400	35500	35600	35700
	35800	35900	36000	36100	36201	36202	36203	36301	36302	36303	36400	36500	36600	

Appendix C: Fixed effects model

To see what the fixed effects model does, consider a model with one explanatory variable (Cameron & Trivedi, 2005): for each *i*,

$$y_{it} = \beta x_{it} + a_i + \varepsilon_{it}, \quad t = 1,2,3,\ldots,T. \qquad \text{(III.1)}$$

Then, by averaging the above equation over time for each *i*, we get

$$\bar{y}_i = \beta \bar{x}_i + a_i + \bar{\varepsilon}_i. \qquad \text{(III.2)}$$

Now, if we subtract Equation III.2 from Equation III.1 for each *i*, we would get a within-transformed model:

$$(y_{it} - \bar{y}_i) = \beta(x_{it} - \bar{x}_i) + (\varepsilon_{it} - \bar{\varepsilon}_i), \quad t = 1,2,3,\ldots,T. \qquad \text{(III.3)}$$

We can confirm that the time invariant effect, a_i, has been eliminated. Thus, the fixed effects model can yield a consistent estimate of β, even if a_i is correlated with x_{it}.

Appendix D: Pearson correlations between BIDs and lagged crime rate variables

Lagged crime rate variables	BIDs
Minor disturbance	-0.049*
Property vandalism	-0.013
Aggravated assault	-0.151***
Theft	0.107***
Robbery	-0.001
Burglary	-0.001

* p <0.05, ** p < 0.01, *** p < 0.001.

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