Do "Broken Windows" Matter? Identifying Dynamic Spillovers in Criminal Behavior

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Abstract

The "Broken Windows" theory of crime prescribes "zero-tolerance" law enforcement policies that disproportionately target light crimes with the understanding that this will lead to future reductions of more severe crimes. We provide evidence against the effectiveness of such policies using a novel database from Dallas. Our identification strategy explores detailed geographic and temporal variation to isolate the causal behavioral effect of prior crimes on future crimes and is robust to a variety of sources of potential endogeneity. We also estimate the effectiveness of alternative targeting policies to discuss the efficiency of "Broken Windows" inspired policies. *JEL Codes: K42 , R23.*

1 Introduction

Economic models of crime are built upon the notion that would-be criminals consider the benefits of committing crime, the probability of arrest and the potential costs of punishment when making decisions (Becker (1968)). In a static model, individual- and neighborhood-level heterogeneity in both the expected costs and benefits of committing various crimes along with agents' beliefs regarding these costs and benefits imply an equilibrium in which crime levels vary across neighborhoods and by different types of crimes (Fender (1999)). If, however, the expected costs and benefits of crime are determined in part by the past histories of criminal behavior in different neighborhoods, then crime must be understood as a social and dynamic phenomenon.

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The leading theory that argues for the existence of intertemporal links in criminal behavior is the "Broken Windows" (BW) theory of crime (Kelling and Coles (1998)).[1] The BW theory is developed around a social and dynamic mechanism by which the proliferation of less severe crimes (e.g., broken windows or graffiti) signals to potential criminals that enforcement, and hence punishment, is lax in the area. This leads to future crimes of increasing frequency and severity, each one signaling further to future potential criminals that enforcement is lax. Put differently, signals transmitted among potential criminals lead to herding behavior (Banerjee (1992); Bikhchandani et al. (1992)).[2] Accordingly, BW carries a strong policy implication that addressing less severe crimes today can be an effective means to reduce the future rates of more severe crimes indirectly (Kelling and Sousa (2001)). With this in mind, we refer to a BW law enforcement policy as one that disproportionally targets less severe crimes. A number of US cities have implemented BW policies in the past twenty years, notably among them New York City and Los Angeles, and such policies continue to be influential.[3] For instance the Chicago Police Department has recently subscribed to BW theory to combat their current increase in violent crime.[4]

For such an important and currently relevant policy question, it is surprising that this theory has undergone relatively little empirical validation. A few studies (Kelling and Sousa (2001), Funk and Kugler (2003) and Corman and Mocan (2005)) have attempted to analyze whether targeting less severe crimes in the present has been effective in reducing more violent crimes in the future, but as pointed out by Harcourt (1998) and Harcourt and Ludwig (2006)[5] all of them are not able to claim causal estimates.[6] We argue below that the inability

[1] James Q. Wilson is regarded as one of the originators of this theory (see Kelling, George L. and James Q. Wilson, "Broken Windows," *The Atlantic Monthly*, March 1982.)

[2] Other dynamic models of crime focus on the relationships between criminal decisions and the labor market (Davis (1988); Imai and Krishna (2004)), income inequality and crime (Fajnzlber et al. (2002)) and social networks and crime (Calvo-Armengol and Zenou (2004)). Glaeser et al. (1996) present a model of crime based on social interactions between criminals to explain geographic variation in crime rates.

[3] In a 2003 interview with the Academy of Achievement, former New York mayor Rudy Giuliani remarked, "I very much subscribe to the "Broken Windows" theory... The idea of it is that you had to pay attention to small things, otherwise they would get out of control and become much worse."

[4] From the *Chicago Tribune*, 3/12/2013: "Chicago police Superintendent Garry McCarthy said Monday that he wanted to bring a "broken windows" strategy to Chicago that would allow officers to arrest those who ignore tickets for routine offenses like gambling and public urination."

[5] Harcourt and Ludwig (2006) take advantage of a random allocation of public housing under the "Moving to Opportunity" experiment in five US cities and find no effect of neighborhood misdemeanor crime levels on the propensity to commit violent crime among those who were assigned to that neighborhood, but it is difficult to attribute this finding to neighborhood misdemeanor crime levels rather than to unobserved characteristics of the neighborhood.

[6] There does seem to exist some indirect experimental evidence in favor of the mechanism underlying BW policies. Braga and Bond (2008) randomize police efforts to reduce social disorder in certain neighborhoods of

to uncover causal estimates is likely due to a lack of crime data available at detailed levels of both geographic and temporal disaggregation. In addition, there has been no study to our knowledge that evaluates the trade-offs related to targeting less severe crimes versus alternative targeting strategies, so we know little about the relative *efficiency* of BW policies. A complete economic analysis of BW policies must address the broader issue of the opportunity cost of these policies. Such an analysis requires researchers to not only measure the effectiveness of policies that target less severe crimes but also the effectiveness of policies that target alternative types of crimes. Moreover, these measurements of policy effectiveness must be considered in the context of the costs of targeting each type of crime and the social benefits of reducing each type of crime. This paper attempts to close some of these gaps in the literature. First, we provide causal estimates of the effect of reducing a particular type of crime in the present on the levels of many different types of crime in the future. With these estimates of effectiveness, we can compute the full dynamic spillovers that are associated with various crime reduction policies. Combined with external estimates of the social benefits of reducing various types of crimes (Miller et al. (1993); Heaton (2010)), we are able to provide a more complete analysis of whether policies that preferentially aim to reduce light crime, such as those prescribed by BW theory, should be implemented.

In order to motivate our empirical analysis, it is important to define precisely the intertemporal relationship that we seek to identify because crime in the past may cause future crimes of various types through a number of mechanisms. From the perspective of a law enforcement policy maker, we argue that it is necessary to distinguish *behavioral* mechanisms from the overall intertemporal causal effect of crime, which also includes *policy based* mechanisms. Behavioral mechanisms fully characterize the dynamic process of criminality and include social learning mechanisms in addition to individual learning mechanisms and any other endogenous responses to prior crimes that are outside of the purview of law enforcement (e.g., formation of neighborhood watches by private residents).[7] In contrast, policy based mechanisms include the future responses of law enforcement agencies to changes in

Lowell, Massachusetts and find that increased policing reduces citizen calls for service for more severe crimes, though measurement error in citizen reporting may be a source of concern in their study. In addition, Keizer et al. (2008) provide evidence from field experiments that is consistent with the behavioral mechanism at the core of BW by showing that when individuals observe violations of social norms, they are more likely to violate these norms themselves. It may be difficult, however, to interpret the external validity of these highly stylized, small-scale field experiments.

[7]Several models of social learning (Gul and Lundholm (1995); Gale (1996); Bikhchandani et al. (1998)) have been developed from a rich theoretical literature on social interactions (Thibaut and Kelley (1959); Becker (1974); Manski (2000); Jackson and Watts (2002)), and they form the causal links explicitly discussed in the BW theory through which past crimes affect current and future crimes. However, past crimes may affect future crimes through non-social channels as well (e.g., learning by doing). As such, we conduct our analysis from a broader perspective without focusing on disentangling these mechanisms suggested by competing theories.

past crime levels. The main thrust of the BW theory is that in any period, a reduction in less severe crimes today will endogenously generate reductions in more severe crimes tomorrow even if future law enforcement activity is unchanged. Thus, in order to measure the effectiveness of a policy that targets light crime, it is imperative to identify the intertemporal causal effects of crime independently of changes in future law enforcement policy. We must therefore identify only those dynamic spillovers that arise from behavioral sources. This plays a particularly important role in the comparison of the effectiveness of alternative law enforcement policies. For example, if a reduction of one robbery today tends to induce a larger change in the response of police than a unit reduction of a less severe crime, then not controlling for future police response will yield a biased comparison of these crime reduction policies.

Briefly, we conduct our analysis in two stages. In the first stage, we estimate causal equations of motion for each type of crime, which summarize the short run co-evolution of all types of crimes over time. These equations describe the current levels of a given type of crime as causal functions of the previous levels of each type of crime as well as other determinants of crime. Importantly, our estimates of these causal criminal relationships only include intertemporal behavioral effects. In the second stage, we use these estimates to simulate the impulse responses of crime reduction, i.e., the long run effects of reductions in the present level of a given crime on the future levels of each crime holding all else constant.[8] We pay particular attention to the long run effects of crime reductions that would be typical of BW law enforcement policies (reductions in light crimes).

In identifying the causal, intertemporal (short run) behavioral effects in the first stage, we must address the fact that unobserved determinants of future crimes may be correlated to previous crime levels (i.e., the usual endogeneity due to omitted variables). The standard method to deal with this issue – the use of instrumental variables – is unsuitable for our task because it will necessarily identify the total reduced-form effect of previous crimes on future crime, which includes the policy based intertemporal effect of crimes.[9]

In light of these issues, we develop an identification strategy that leverages a novel, incident based dataset of crimes. Our identification strategy explores the fact that the

[8]This two stage procedure is necessary because it implicitly accounts for the fact that crime data may not be observed in long run equilibrium but rather along some trajectory. Caetano and Maheshri (2012) discuss this topic in further detail.

[9] Jacob et al. (2007) use city-wide weekly weather shocks as instruments and find a small negative within-crime intertemporal relationship. However, they focus only on within-crime effects at the city level and do not distinguish between behavioral and policy based intertemporal links between crimes, so their results are unrelated to BW policing.

behavioral effect of crime tends to be highly localized and tend to occur over short time spans, while other confounding effects, including policy responses to prior crimes, do not. Thus, we can exploit the high geographic detail and the high frequency of our data to isolate the behavioral effect from the other effects. Our identification strategy has a firm theoretical and institutional basis. As discussed in Akerlof (1997) and Ellison and Fudenberg (1995), social learning depends crucially upon the "social distance" between agents which is strongly related to both physical and temporal distance. Moreover, other endogenous responses to crime which are deemed behavioral are clearly local and at high frequency, as in the case of individual learning mechanisms (e.g., learning-by-doing, Arrow (1962)), individual specialization in criminal activity (Kempf (1987)), individual incapacitation (Levitt (1998)) and other endogenous neighborhood responses to crime (Taylor (1996)). However, other systematic determinants of crime such as neighborhood wealth levels (Flango and Sherbenou (1976)) and family structure (Sampson (1985)) vary more slowly than crime itself, and institutional knowledge allows us to conclude that the immediate policy response to crime by law enforcement is based on larger administrative boundaries that encompass multiple social and individual learning networks. Thus, by focusing on the relationship between prior crimes and current crimes within smaller neighborhoods and at shorter time scales, we can estimate intertemporal behavioral effects of crime that are plausibly independent of policy responses or other confounding effects. The richness of our data set also allows us to provide empirical support for these theoretical and institutional arguments.

We supplement this identification strategy by conducting several robustness checks that address a variety of standard empirical issues often encountered with spatial panel datasets including omitted variables, temporal and geographic misaggregation, serial correlation, spatial autocorrelation, and other forms of measurement error. As a final, novel robustness check, we directly test whether our estimates are unbiased using a formal, statistical test of exogeneity inspired by Caetano (2012) that is based on continuity conditions. We argue that with this test we are able in principle to detect endogeneity from a number of sources, including omitted variables, measurement error and unobserved future police actions. Indeed, in practice we detect endogeneity in specifications of the type that have been previously estimated in this literature, yet we do not detect endogeneity in our preferred specifications, which provides further validation of our identification strategy.

We conduct our analysis using a unique, comprehensive database that contains every police report filed with the Dallas Police Department from 2000-2007. In total, this database contains nearly 2 million unique police reports, including reported light crimes, such as broken windows and graffiti, which are not observed in most criminal data sets and play a crucial role in assessing BW policies. Each police report narrowly classifies the crime committed and contains detailed information regarding the precise location and time of the alleged crime.[10] In addition, each police report contains information regarding the speed and quality of the police response to the report, which is also rarely observed. We use this database to construct a panel data set containing the weekly levels of six types of crimes (rape, robbery, burglary, motor vehicle theft, assault and light crime) in 32 neighborhoods that span the city of Dallas.

Although we find that a reduction of one light crime leads to an additional cumulative future reduction of roughly 0.1 light crimes, this reduction is not found to generate statistically or economically significant reductions in the future levels of more severe crimes. We interpret our finding that law enforcement actions that target light crimes are ineffective in reducing more severe crimes in the future as casting considerable doubt on the claim that the dramatic reduction in the crime rate (especially the violent crime rate) in US urban areas over the past fifteen years is due to the adoption of BW or zero-tolerance law enforcement policies.[11] In sum, our findings suggest that law enforcement agencies aiming to reduce violent crimes should pursue policies that are tailored to combat those crimes.

We acknowledge that even if a law enforcement action that targets light crime is ineffective at reducing future violent crime rates, it may still be preferred to other policies. To evaluate this claim, we estimate the effectiveness of alternative targeting practices. We find that a reduction of one robbery leads to an additional cumulative future reduction of roughly 0.1 robberies, a reduction of one auto theft leads to an additional cumulative future reduction of 0.2 auto thefts, and a reduction of one burglary leads to an additional cumulative future reduction of 0.4 burglaries. Although we find that unit reductions in assaults lead to future reductions of nearly 0.005 rapes and 0.025 robberies, we find no statistically significant evidence of other dynamic spillovers across crimes of increasing severity. We do find that unit reductions of robbery, auto theft and assaults generate spillover reductions of 0.18, 0.10 and 0.05 light crimes respectively. These across-crime spillovers in the direction of *decreasing* severity are of the same order of magnitude as the within-crime spillovers associated with light crime, suggesting that actions that target more severe crimes will generate spillover benefits that strictly dominate the spillover benefits of actions that target light crimes. This stands in stark contrast to the policy prescriptions of BW theory.

To complete our analysis, we make the first attempt to evaluate the long run efficiency

[10] The precise location and time of reported crimes are rarely observed in the same dataset, at least in large, incident based criminal data sets that are relevant for this analysis such as the National Incident Based Reporting System (NIBRS).

[11] Levitt (2004) describes efforts by the media to attribute falling crime rates in New York City to innovative law enforcement policies, including "broken windows" style policies, but he argues that this conclusion is premature given other confounding changes that occurred in New York City at the same time or even before a "broken windows" policy was implemented. Our finding is consistent with this view.

of BW policies with a back of the envelope welfare calculation using external estimates of the social benefits of crime reduction from Heaton (2010) and Miller et al. (1993). We find that a BW policy is advisable only if the marginal cost of reducing a light crime is less than 25 (7) times the marginal cost of reducing a robbery (burglary).

The remainder of the paper is organized as follows. In section 2, we describe our strategy to identify intertemporal behavioral relationships between neighborhood crimes. In section 3, we describe our data set and discuss the plausibility of our identification strategy. In section 4, we present estimates of the short run intertemporal effects of crime, and in section 5 we show that those estimates withstand a variety of robustness checks. In particular, we derive a formal test of the exogeneity assumption underlying our identification strategy and use it to argue that our estimates of the intertemporal behavioral relationships between crimes are indeed unbiased.[12] In section 6 we calculate long run dynamic spillovers in criminal behavior by simulation, and we use these results to perform a back of the envelope cost-benefit analysis of various alternative law enforcement policies. We conclude in section 7.

2 Empirical Approach

Law enforcement agencies (LEAs) seek to choose and implement policies that generate the greatest net benefit in terms of crime reduction. Forward looking LEAs must explicitly consider the long run benefits and costs of law enforcement policies, hence it is useful for them to know the effects of past crimes on current and future crimes. Past crime affects current (and future) crime through two channels: directly through behavioral changes, and indirectly through future policy responses. Behavioral channels include any endogenous intertemporal responses to prior crimes. For example, social learning by criminals (Ellison and Fudenberg (1995)), learning-by-doing (Arrow (1962)), specialization in criminal activity (Kempf (1987)), incapacitation (Levitt (1998)), and neighborhood responses to crime (Taylor (1996), Bronars and Lott Jr (1998)) are all classified as behavioral channels. On the other hand, policy based channels include any current police responses to prior crimes that affect current crime levels. For example, a police crackdown (Sherman and Weisburd (1995)) and a change in the distribution of police resources due to an increase in the number of crimes (Weisburd and Eck (2004)) are classified as policy based. We depict these two causal channels in diagram (1). For a given neighborhood j, X_{jt} is a vector containing the levels of C types of crimes, and P_{jt} represents the law enforcement policy implemented in period t. The solid arrows correspond to behavioral channels, while the dashed arrows correspond to

[12] We provide theoretical and empirical support for the implementation of this test in appendix A.1.

policy based channels.

$$
\begin{tikzcd}
X_{jt-1} \arrow[r] \arrow[dr, dashed] & X_{jt} \arrow[r] \arrow[dr, dashed] & X_{jt+1} \arrow[r] \arrow[dr, dashed] & \dots \\
& P_{jt} \arrow[u, dashed] & P_{jt+1} \arrow[u, dashed] & \dots
\end{tikzcd}
\tag{1}
$$

From the perspective of a forward looking LEA, it is crucial to distinguish between these two channels when empirically evaluating law enforcement policies. In particular, LEAs must consider only the behavioral channel when evaluating the effectiveness of a law enforcement policy. To illustrate this point, suppose that a policy was available that would eliminate a single robbery in a neighborhood today. Other things equal, if this policy resulted in fewer robberies committed tomorrow than expected, then we would rightly interpret this as an effective policy. If instead this policy resulted in the same number of robberies committed tomorrow as expected in the absence of the policy, but fewer law enforcement resources were deployed tomorrow, then we should also interpret this as an effective policy. But if we did not isolate the behavioral channel from the policy based channel and instead estimated the full reduced form effect of crime today on crime tomorrow, we would be inclined to conclude – incorrectly – that this policy was ineffective. In the context of the discussion in the introduction, the BW theory conjectures that the solid arrows in diagram (1) exist, and in particular, they tightly link past light crimes with future severe crimes. It follows that testing this hypothesis requires us to disentangle the dashed arrows from the total reduced form relationship between X_{jt-1} and X_{jt}.

We formalize this intuition and develop an empirical approach to identify the intertemporal behavioral effects of crime by parametrizing these effects in a system of equations of motion which summarize the co-evolution of criminal behavior over time. Let x^c_{jt} be the c^{th} element of the vector X_{jt-1}, i.e., the number of crimes of type c in neighborhood j in period t. Then the equation of motion for crime c can be written as

$$
x^c_{jt} = X_{jt-1}\beta^c + Z^c_{jt}\gamma^c + \underbrace{P^c_{jt} + \epsilon^c_{jt}}_{\text{error}} \tag{2}
$$

where Z^c_{jt} is a vector of observed determinants of x^c_{jt} that do not absorb behavioral responses to crime from $t-1$ to t, P^c_{jt} includes all unobserved police responses to past crimes and ϵ^c_{jt} is an error term that includes other unobserved determinants of crime as well as misspecification error. In equation (2), β^c can be interpreted as the (short-run) behavioral component of the intertemporal effect of crime because it excludes the effects of X_{jt-1} on x^c_{jt} through P^c_{jt} (as well as Z^c_{jt} and ϵ^c_{jt}). Given causal estimates of β^c for all crimes c, we can calculate a long-run measure of the behavioral effects of each type of crime by recursively iterating the short-run effects over time. We refer to these long-run impacts as the *dynamic spillovers* associated with crime.

We face a distinct obstacle to identify β^c: ϵ_{jt}^c is almost certainly correlated to X_{jt-1}. For example, this endogeneity arises if ϵ_{jt}^c is serially correlated since ϵ_{jt-1}^c causes X_{jt-1} trivially. Indeed, most unobserved neighborhood amenities that determine crime, such as socioeconomic neighborhood characteristics, are likely to possess this property. The standard approach to deal with this issue is to utilize an instrumental variable (IV) to identify β^c. However, in this case, IVs will be unable to identify behavioral dynamic spillovers. Instead, IVs will identify the full reduced-form effect of X_{jt-1} on x_{jt}^c, which includes the component through the future unobserved police response P_{jt}^c to crimes in period $t-1$. Put differently, the behavioral effect is not identified by instrumental variables because any candidate IV that generates variation in X_{jt-1} (i.e., is relevant) will also generate variation in P_{jt}^c (i.e., is invalid). For intuition, we graphically depict the inability of an IV to address the endogeneity due to P_{jt}^c in diagram (3).

$$\begin{tikzcd}
 & & Z_{jt}^c \arrow[dr, "\gamma^c"] & \\
\text{IV} \arrow[r] \arrow[drr, dotted, no head] & X_{jt-1} \arrow[rr, "\beta^c"] \arrow[dr] & & x_{jt}^c \\
 & & P_{jt}^c \arrow[ur] &
\end{tikzcd} \tag{3}$$

Because of the timing of events, P_{jt}^c is caused by X_{jt-1}, which induces a correlation between a potential IV and P_{jt}^c as indicated by the dotted line. Unless we are able to fully control for P_{jt}^c (in which case the instrument may be superfluous), the instrument will be invalid. In light of this, we pursue an alternative strategy to identify β^c in equation (2).

We motivate our identification strategy with the fact that the two causal channels between X_{jt-1} and x_{jt}^c operate at different levels of aggregation. Behavioral responses to past crimes propagate along individual and social learning networks. Social learning dissipates rapidly as social distance increases. As social distance is strongly correlated to both spatial distance (Akerlof (1997)) and temporal distance (Ellison and Fudenberg (1995)), the bulk of the behavioral response to a past crime will remain close to the scene of the crime and will be strongest in its immediate aftermath as the crime will be most salient then.[13] On the other hand, as we discuss in the next section, police responses to crimes, which are based on

[13]Not only are individuals' beliefs of neighborhood crime levels likely to be subject to recency bias, but even individuals' beliefs of their own victimization have been repeatedly found to be subject to recency bias (see Block (1993) for a survey of these studies).

administrative protocols of LEAs, are likely to be consistent within administrative regions that encompass multiple individual and social learning networks. Moreover, adjustments of the allocation of LEA resources within administrative regions is likely to occur at a slower pace than behavioral responses to past crimes. In addition, other confounding causes of current crimes (ϵ^c_{jt}) also operate at more aggregated levels. For instance, the demographic composition of a neighborhood, which has been found to affect crime rates (Sampson (1985)), tends to change relatively slow over time. The same is true of judicial institutions including municipal arrest policies, criminal law, and incarceration policies. These differences in aggregation indicate an identification strategy that explores the geographic and temporal detail of the panel data to construct fixed effects that absorb all confounding factors, including unobserved police responses, without absorbing any of the treatment effect we want to measure.

Formally, let a city be composed of neighborhoods indexed by j, which are further grouped into administrative regions indexed by J. The shorter time periods t (e.g., weeks) at which crime levels are sampled can be further grouped into longer time periods T (e.g., years).[14] We can decompose the sources of error in equation (2) into three pieces:

$$P^c_{jt} + \epsilon^c_{jt} = \gamma^c_{Jt} + \gamma^c_{jT} + \eta^{cJT}_{jt} \tag{4}$$

where γ^c_{Jt} is the average high frequency varying error in an administrative region, γ^c_{jT} is the average low frequency varying error in a neighborhood, and η^{cJT}_{jt} is the remaining error that additionally depends on the levels of aggregation of J and T. For the reasons described above, police responses are not likely to vary systematically within administrative regions at a given point in time. It follows that fixed effects at the crime type, administrative region and time level (λ^c_{Jt}) will absorb the future police response P^c_{jt} (along with all other confounding causes of crime that do not vary by administrative region). Similarly, fixed effects at the crime type, neighborhood and longer time period level (λ^c_{jT}) will absorb any neighborhood specific confounding factors ϵ^c_{jt} that vary at the lower frequency T. Thus, the intertemporal behavioral effect β^c is identified under the following exogeneity assumption.

Assumption 1. $E[\eta^{cJT}_{jt} | X^c_{jt-1}, \lambda^c_{Jt}, \lambda^c_{jT}] = 0$

Intuitively, there is an implicit trade-off in our choices of J and T (and j and t by extension). Assumption 1 is more likely to be valid for smaller choices of J and T because η^{cJT}_{jt} will tend to be smaller (in absolute value) by construction. However choosing very

[14] Without loss of generality, we assume that neighborhoods are uniquely assigned to an administrative region ($j \in J$ implies $j \notin J'$ for all $J' \neq J$) and that longer time periods are evenly divisible by the shorter time periods ($t \in T$ implies $t \notin T'$ for all $T' \neq T$).

small levels of J and T may result in some of the intertemporal behavioral effects being absorbed by the fixed effects, adversely impacting our interpretation of β^r. It follows that our choices of J and T should absorb as much of the confounding factors (P^c_{jt} and ϵ^c_{jt}) as possible without absorbing any of the intertemporal behavioral effect. More formally, let $\underline{J}$ and $\underline{T}$ be the minimal levels of J and T for which no component of the behavioral effect is absorbed by the fixed effects λ^c_{Jt} and λ^c_{jT}, and let $\overline{J}$ and $\overline{T}$ be the maximum levels of J and T for which all confounding factors are absorbed by the fixed effects λ^c_{Jt} and λ^c_{jT}. Then appropriate choices of J and T should satisfy the inequalities $\underline{J} \leq J \leq \overline{J}$ and $\underline{T} \leq T \leq \overline{T}$. Before further discussing the theoretical, institutional, and empirical reasons why our choices of J and T are likely to satisfy these conditions[15], we first describe our data set.

3 Data and Preliminaries

3.1 Sample

We assemble a database encompassing every police report filed with the Dallas Police Department (DPD) from January 1, 2000 to September 31, 2007.[16] According to the FBI, Dallas held the dubious distinction of having the highest crime rate of all US metropolitan areas with at least one million persons during the sample period.[17] Although Dallas did not explicitly adopt BW policing in this period, we can still analyze the dynamics of criminal behavior using this data since we estimate these behavioral effects independently of law enforcement policy.[18] This database is uniquely suited to evaluate the effectiveness of BW policies because it includes a comprehensive catalog of all light crimes of various types that were reported.

Every report in our database lists the exact location (address or city block) of the crime and is given a five digit Uniform Crime Reporting (UCR) classification by the responding officer.[19] A full description of the complainant who called in the report is also provided, with the exception of anonymous reports. Private companies and public officials/offices may be listed as complainants. Every report also lists a series of times from which we can deduce the entire sequence of crime, neighborhood response and police response. Specifically, we

[15]In particular, we argue below why $\underline{J} < \overline{J}$ and $\underline{T} < \overline{T}$ so that this identification strategy is feasible.

[16]A small number of police reports – sexual offenses involving minors and violent crimes for which the complainant (not necessarily the victim) is a minor – are omitted from our data set for legal reasons.

[17]"New York Remains Safest Big City in US," September 19, 2006, *The Associated Press.*

[18]The coefficients β^c are inherent to the dynamic behavioral process of crime, which is, by definition, the same irrespective of the policy.

[19]If a particular complaint consists of multiple crimes (e.g., criminal trespass leading to burglary), then the report is classified only under the most severe crime (burglary) per UCR hierarchy rules developed by the FBI.

observe the time (or estimate of the time) that the crime was committed, the time at which the police were notified and dispatched, the time at which the police arrived at the scene of the crime, and the time at which the police departed the scene of the crime. This allows us to construct observable police response measures that vary geographically, temporally and by crime type and are correlated to the unobservables P^c_{jt}, which are valuable for showing that our estimates of β^c do not include (observable or unobservable) policy responses by law enforcement.

We perform our analysis on six crimes: rape, robbery, burglary, motor vehicle theft, assault and light crime. Because of potential misclassification, we define assault as both aggravated and simple assault (Zimring (1998)). We classify criminal mischief, drunk and disorderly conduct, vice (minor drug offenses and prostitution), fence (trade in stolen goods) and found property (almost exclusively cars and weapons) as light crimes.[20] Together, these six crimes comprise 55% of all police reports to the DPD during the sample period.[21]

We select this set of crimes for four reasons. First, this set of crimes includes both violent crimes and property crimes of varying levels of severity, which allows us to test for dynamic spillover effects of lighter crimes to more severe crimes. Second, these crimes are likely to signal criminals' actual beliefs of the strength of enforcement to potential criminals, which forms the basis of the social learning mechanism suggested by BW theory. In contrast, crimes such as embezzlement and gambling are less publicly observable. Third, these crimes occur relatively more frequently than other publicly observable crimes such as homicide and arson. And fourth, these crimes are relatively accurately reported in comparison with crimes such as larceny and fraud.[22]

We provide summary statistics for these reported crimes in table 1. Not surprisingly, light crime is the most prevalent crime reported, followed by assault, burglary, auto theft, robbery and rape. Police respond to crimes in approximately 80 minutes on average, although they respond to reports of rape roughly an hour slower and to reports of motor vehicle theft roughly half an hour faster. On average, police spend less than half an hour at the scene of a motor vehicle theft, but they spend up to an hour at the scenes of robberies and light

[20]As robustness checks, we replicated our full analysis defining only criminal mischief and found property as light crime, or alternatively defining criminal mischief only as light crime. In all three cases, we obtained similar results.

[21]Roughly 25% of police reports in the database do not directly correspond to criminal acts per se (i.e., they declare lost property, report missing persons, report the failure of motorists to leave identification after auto damages, etc.) so the six crimes that we consider comprise a much larger majority of total crime in Dallas during the sample period.

[22]The accuracy of reported rape statistics is admittedly poor (Mosher et al. (2010)). As an added robustness check, we replicated our full analysis excluding rapes and obtained similar results. To the extent that the propensity to misreport rape varies discontinuously at $x^{c'}_{jt-1} = 0$ for some c', the test of exogeneity described in section 5.3 will also detect endogeneity stemming from mismeasurement in rape levels.

crimes and over an hour at the scenes of reported rapes. All types of crimes occur slightly more frequently on weekends than weekdays with the exception of burglaries, which happen less frequently on weekends than weekdays. Just over half of robberies, light crimes and motor vehicle thefts occur at night, and as expected, a majority of these crimes take place outdoors. On the other hand, burglaries and assaults tend to occur during the daytime and indoors. Rapes tend to occur at night and indoors. Private businesses report approximate one fifth of robberies and light crimes and one third of burglaries, but they report very few motor vehicle thefts and no rapes or assaults.

3.2 Choosing J and T[23]

Although we observe each crime individually, the relevant variables in our model are levels of crime in pre-defined neighborhoods and time periods. As such, we must geographically and temporally aggregate our data in a careful manner that satisfies our identifying assumption and preserves sufficient intertemporal and cross-sectional variation in crime levels.

During our sample period, DPD was geographically organized into six divisions subdivided into 32 sectors, which were further subdivided into police beats.[24] In the DPD hierarchy, division deputy chiefs are given a relatively high degree of autonomy in devising rapid responses to crimes.[25] For this reason, we choose J to be the division level. Police beats range from roughly 0.5 to two square miles in area, and each sector contains five to seven beats. Ideally, we would like to define our panel at the largest geographic level that can maintain assumption 1 in order to internalize the information spillovers from observed crime levels in nearby areas. This ensures that our estimate of β^c contains as much of the intertemporal behavioral effect of crime as possible. Accordingly, we define neighborhoods our panel at the $j =$ sector level.

Because social learning may occur at high frequency, we would like to define our panel at the shortest temporal level for which we can still construct plausible crime rates. We choose $t =$ week, which preserves substantial heterogeneity in neighborhood crime rates over time and provides a long time series (402 periods). Because neighborhood level confounding

[23]Empirical justification for our choices of J and T is provided following our main results.

[24]In October 2007, DPD added a seventh division to their classification and made slight modifications to some beat and sector boundaries. We end our sample in September 2007 to ensure that the administrative boundaries in our data set are geographically consistent over the entire sample period.

[25]As depicted in the DPD Organizational Chart (http://www.dallaspolice.net/content/11/66/uploads/DPDOrgChart-4-11-13.pdf) the Patrol Bureau of the DPD, which is in charge of devising short run responses to crimes, is decentralized at the division level and led by Deputy Chiefs, who are starred commanders for each division. This decentralization is discussed in detail in the publicly available *Dallas Police Department Management and Efficiency Study*, prepared by a third party, Berkshire Advisers, Inc., for the DPD in September 2004.

determinants of crime are likely to vary slowly, we choose T to be yearly. We make this choice for two reasons. First, this choice embeds much of the previous literature on estimating intertemporal effects of crime that relies on annually varying controls (e.g., Funk and Kugler (2003); Corman and Mocan (2005)). Second, this choice allows us to estimate medium-run intertemporal behavioral effects of crime that propagate at an intermediate frequency (e.g., monthly) and may dissipate nonlinearly.

4 Estimation Results

We estimate $\beta^1, \ldots, \beta^C$ from the following system of equations

$$\begin{aligned} x^1_{jt} &= X_{jt-1}\beta^1 + Z^1_{jt}\gamma^1 + D^1\delta^1 + u^1_{it} \\ &\vdots \\ x^C_{jt} &= X_{jt-1}\beta^C + Z^C_{jt}\gamma^C + D^C\delta^C + u^C_{it}, \end{aligned} \tag{5}$$

which represent the equations of motion of all crimes ($C = 6$). Given the large number of estimated parameters, we report only the subset of the results that are most directly relevant to evaluate the effectiveness of BW policies in the body of the paper. The complete set of estimates of β^c for all types of crime are reported in the online appendix. Coefficient estimates of the effects of light crime on future levels of all other crimes are presented for various specifications in table 2. In each specification, the system of equations (5) is estimated efficiently by Seemingly Unrelated Regression (Zellner (1962)). Given that the primary source of bias is likely to be omitted determinants of crime that are positively serially correlated (e.g., neighborhood amenities), we would expect our naive estimates of β^c to be biased upward in specifications with insufficient controls.

In specification 1, we do not include any control variables. We find that an additional light crime is associated with approximately half of an additional reported light crime in the following week. Moreover, with this specification we find that an additional light crime has an intertemporal effect on more severe crimes such as assault, auto theft and burglary, which suggests that BW policies are effective in reducing more severe crimes. All coefficients are precisely estimated, and we are able to explain 81% of the variation in reported weekly neighborhood crime levels with this specification.

In specification 2, we add year-crime type fixed effects as control variables. These variables absorb any annually varying determinants of each type of crime that are common to all neighborhoods in Dallas. Previous attempts to identify intertemporal relationships between crimes (Funk and Kugler (2003)) and between crime and policing (Corman and Mocan

(2005)) are based on specifications similar to this, as they utilize only low-frequency control variables with low geographic detail (such as city-wide or national annual unemployment rates) which are absorbed by the fixed effects in specification 2. The coefficient estimates of this specification are roughly similar to our estimates from specification 1. Indeed the increase in R^2 of 0.002 from specifications 1 to 2 indicate that these control variables explain little additional variation in weekly neighborhood crime rates.

In specification 3, we add sector (j)-crime type fixed effects and week (t)-crime type fixed effects as control variables. These variables absorb any omitted neighborhood specific determinants of each crime and any omitted city-wide week specific determinants of each crime respectively. Overall the coefficient estimates are precisely estimated but decrease in magnitude relative to specifications 1 and 2, which confirms our conjecture that these omitted variables are positively correlated with criminal activity. This finding casts doubt on the results of earlier empirical studies of the effectiveness of BW policy and highlights the importance of using high frequency and geographically detailed data to identify intertemporal behavioral effects of crime. Nevertheless, reducing light crime is still found to be effective in reducing future levels of more severe crimes in this specification. With the inclusion of these fixed effects, we are able to explain 85% of the variation in reported weekly neighborhood crime levels.

In specification 4, we enrich the set of control variables by disaggregating the fixed effects by sector (j)-year (T)-crime type and division (J)-week (t)-crime type. As discussed above, these fixed effects are uniquely suited to control for endogeneity from unobserved police responses to crime as well as from other confounding factors. The first set of fixed effects absorbs all omitted neighborhood specific determinants of each crime that vary on an annual basis (e.g., demographic characteristics of the neighborhood).[26] The second set of fixed effects absorbs all time varying determinants of each crime that vary across the six police divisions of Dallas, which importantly includes high frequency division level responses to prior crimes. In short, the only potential omitted variable that could bias our estimates would have to be both sector-specific and vary across weeks within a calendar year or both week-specific and vary across sectors within a division. As in the previous specifications, all coefficients are precisely estimated. Parameter estimates in this specification are substantially smaller in magnitude than in the previous specifications. Indeed, previous light crimes are still found to cause future light crimes, but this effect is only about a third as large as in specification 3. More importantly, in this specification, we find no statistically or economically significant

[26] Sector specific unobservable amenities that are changing over time due to gentrification will be partially absorbed by these fixed effects to the extent that they vary across years in the sample.

intertemporal effect of light crime on more severe crimes of any type, so we find no evidence that targeting light crime is an effective means of reducing more severe crimes in the future.[27] This suggests that the estimates in specification 3 are biased. With these fixed effects, we are able to explain 87% of the variation in reported weekly neighborhood crime levels.

In specifications 5 and 6, we show that our estimates of β^c from specification 4 are robust to a variety of different sources of potential endogeneity. In specification 5, we enrich our set of control variables by including the shares of each type of crime reported to have been committed in the daytime and on the weekend in the previous week and the shares of each type of crime reported to have been committed outdoors in the previous week.[28] This allows us to explore if either our temporal or spatial aggregation of observations introduces endogeneity into our specification. If crimes committed during the daytime or during the weekend (outdoor) generate different intertemporal effects than crimes committed at night time or during the weekday (indoor), perhaps because they are more salient to a potential criminal, then specification 4 would be temporally (spatially) misspecified, which might bias our parameter estimates. That we find almost no change in either the coefficient estimates or the R^2 between specifications 4 and 5 suggests that our choices of j and t do not bias the results.

In specification 6 we expand the set of control variables from specification 5 by adding, for each type of crime, the average time that the police take to arrive at the crime scene in the current week, and the average duration that police remain at the crime scene in the current week.[29] We include these variables to attempt to proxy for the level of attention that the police pay each type of crime in each particular neighborhood in the current week, i.e., the police response to prior crimes. The inclusion of these variables have no discernible effect on the estimates of β^c, nor do they explain any additional variation in reported weekly neighborhood crime levels. Indeed, we a test of the hypothesis that all 72 police response and police duration coefficients are equal to zero yields a p-value of .66. We interpret this as strong evidence that the fixed effects successfully absorb unobserved determinants of police responsiveness.

[27] Gladwell (2000) has popularized the notion that BWT implies the existence of a "tipping point" level of light crime beyond which the levels of light crime and more severe crimes are on an ever increasing trajectory. Our findings that the eigenvalues of the estimated β matrix (which includes β^c for all equations of motion) are much smaller than one are inconsistent with this view (see, e.g., Lade and Gross (2012)).

[28] Given the system of 6 equations of motion, each with 6 main explanatory variables, we effectively add 36 control variables for daytime crimes, 36 control variables for weekend crimes and 36 control variables for outdoor crimes.

[29] As in specification 5, we effectively add 36 average police response and 36 average police duration variables as controls in specification 6.

5 Additional Robustness Checks

In table 2, we present evidence that the estimates of specifications 1 through 3 are biased, but we were unable to find evidence that the estimates of specifications 4 through 6 are biased. In this section, we leverage our detailed dataset to subject these specifications to stronger robustness checks that in part provide further empirical evidence in favor of our choices of J, T, j and t. Because reported crime levels have been found to suffer from non-classical measurement error (e.g., Skogan (1974, 1975, 1977)) we also conduct robustness checks that are particularly sensitive to this issue and test for spatially autocorrelated errors, serially correlated errors, and general errors due to misreporting of crime. Our results in this section are strongly consistent with the results presented in table 2 in the sense that specifications 1 through 3 repeatedly fail these additional robustness checks whereas specifications 4 through 6 do not fail any of the tests.

5.1 Spatial Autocorrelation

Determinants of crime are potentially spatially autocorrelated across neighboring regions (e.g., Morenoff and Sampson (1997)) for two reasons. First, the levels of unobserved determinants of crime in a particular neighborhood may be correlated with the levels of those determinants in nearby neighborhoods, generating positive spatial autocorrelation. Second, crime in one neighborhood may displace crime from nearby neighborhoods, generating negative spatial autocorrelation (Cornish and Clarke (1987)). If determinants of crime are spatially autocorrelated, then our estimates β^c may be biased due to endogeneity, and their standard errors may also be biased, affecting inference.

In specifications 4-6, we attempted to address this form of endogeneity by adding fixed effects at the division-week-crime type level (λ^c_{Jt}) to absorb any unobservable determinant of crime that is common across neighboring sectors. If the endogeneity problem is addressed by the control variables in our preferred specifications, then we would expect that the errors in such specifications would be spatially uncorrelated. Accordingly, we follow the suggestion of Dube et al. (2010) and re-estimate the system of equations and cluster the standard errors at a larger geographic level than our panel (by division-year-crime type as opposed to by sector-year-crime type). By doing so, we allow η^{cJT}_{jt} to be correlated with η^{cJT}_{kt}, where j and k are sectors within the same division of Dallas. In table 3, we reproduce all of the standard errors from the specifications presented in table 2 in bold. Directly below these standard errors in normal font, we present all of the standard errors clustered at the division-year-crime type. Two findings are immediate. First, the original standard errors clustered by sector-year-crime type differ substantially from the standard errors clustered by division-year-crime type in specifications 1-3.[30] Second, the standard errors clustered by division-year-crime type are nearly identical to the standard errors clustered by sector-year-crime type in specifications 4-6; that is, in these specifications, any previously estimated statistically significant intertemporal effect remains statistically significant under the broader clustering, and vice versa. These findings taken together suggest that the additional control variables in our preferred specifications effectively absorb potential spatial autocorrelation in the errors, while the control variables in specifications 1-3 do not.

We provide further evidence against spatial autocorrelation by re-estimating the system of equations with additional controls for crime in nearby neighborhoods. In particular, we include $\tilde{X}_{jt-1}$ as control variables, where $\tilde{X}_{jt-1}$ contains the crime levels of the closest sector to sector j that is within the same division. If the intertemporal behavioral effect spills over to other neighborhoods (sectors) within the same division, then we would expect the coefficients of $\tilde{X}_{jt-1}$ to be different from zero. However, an F-test of the hypothesis that all 36 coefficients of $\tilde{X}_{jt-1}$ equal zero yields a p-value of 0.24, which constitutes strong evidence against spatial autocorrelation and in favor of our claim that our choice of j = sector fully incorporates all intertemporal behavioral effects. In addition, our estimates of β^c are unchanged from before.

The results of specification 6 of table 2 and these results taken together suggest that the minimal level of J for which no component of the behavioral effect is absorbed ($\underline{J}$) is weakly smaller than a sector, and the maximum level of J for which all confounding factors are absorbed ($\overline{J}$) is weakly larger than a division. Hence, our choice of J = division is consistent with the identifying assumption (i.e., $\underline{J} < J \leq \overline{J}$).

5.2 Serial Correlation

Determinants of crime may also be serially correlated (Fajnzylber et al. (2002)), which has two implications for our empirical analysis. First, serially correlated errors may be a source of endogeneity, hence our estimates of β^c might be biased. Second, positively serially correlated errors may make inference misleading, as standard errors may be too small. In specifications 4-6, we attempted to address this form of endogeneity by adding sector-year-crime type fixed effects (λ^c_{jT}) in order to absorb neighborhood specific unobservables that are common across weeks within year.

As suggested by Angrist and Pischke (2009), we provide a further robustness check by re-clustering our standard errors at the sector-year-crime type level. This allows unobserved determinants of crime within a given sector in a particular week to be correlated with unob-

[30] Table 2 in the online appendix shows this table for all types of crime where this pattern is more striking.

served determinants of crime within that sector across all other weeks in the same year. If the endogeneity problem is addressed by the control variables in our preferred specifications, then we would expect that the errors in these specifications are serially uncorrelated. The standard errors clustered by sector-year-crime type are presented in italics in table 3. Given that in specifications 4-6 the standard errors clustered at the year-division-crime type level are similar to the ones clustered at the year-sector-crime type level from our robustness check about spatial correlation, we can use these standard errors to infer the extent of serial correlation in the remaining error. Analogous to the case of spatial autocorrelation, we find that in specifications 4-6 the two sets of standard errors are nearly identical, providing further evidence in favor of specifications 4-6.

As an additional robustness check for serial correlation, we add as control variables the reported crime levels from earlier periods $(t-2,\, t-3, \ldots, t-\tau)$. Formally, we modify the system of equations of motion of crime to

$$\begin{aligned} x_{jt}^{1} &= \sum_{k=1}^{\tau} \left(X_{jt-k}\beta_k^1 + Z_{jt-k+1}^1 \gamma_k^1 + D_k^1 \delta_k^1 \right) + u_{it}^1 \\ &\vdots \\ x_{jt}^{C} &= \sum_{k=1}^{\tau} \left(X_{jt-k}\beta_k^C + Z_{jt-k+1}^C \gamma_k^C + D_k^C \delta_k^C \right) + u_{it}^C \end{aligned} \tag{6}$$

for different values of τ. We re-estimate these systems of equations using the full set of available control variables from specification 6 and present coefficient estimates for the right hand side variable $x_{jt-1}^{\text{light crime}}$ in table 4 (coefficient estimates for the entire vector X_{jt-1} are presented in online appendix table 3). To the extent that the inclusion of these variables do not change our estimates of these coefficients, only omitted variables that are uncorrelated with crime levels $X_{jt-2}, ..., X_{jt-\tau}$ but are correlated with much earlier levels of crimes $(X_{jt-\tau-1}, ...)$ could generate endogeneity in our specification. This substantially reduces the set of potential sources of endogeneity about which we should be concerned. It is immediate that our estimates of β_1^c in specifications with higher order lags (i.e., columns 2 through 4) are statistically indistinguishable from our prior estimates, which are reproduced in the first column. As x_{jt-k}^c for $k = 2, ..., 4$ are correlated to x_{jt-1}^c for all c, the fact that the estimates of x_{jt-1}^c do not change with the inclusion of these additional lags constitutes further evidence that the β^c coefficients in specifications 4-6 in table 2 are consistent estimates of the (one-period) intertemporal behavioral effects of crime.

For brevity, we omit the large number of coefficients on higher order lagged terms in specifications with $\tau = 2$ and $\tau = 3$, but we present the full set of light crime coefficient estimates for our preferred specification with $\tau = 4$ in table 5 (coefficient estimates for the entire vectors $X_{jt-1}, \ldots, X_{jt-4}$ are presented in online appendix table 4). For any lag, we can rule out that a unit increase in light crime will increase any other type of crime by 0.034 or more units at the 95% confidence level.[31] Our finding that $\beta_1^c \neq \beta_2^c \neq \beta_3^c \neq \beta_4^c$ also serves as an additional robustness check of our choice of temporal aggregation (t). If the data generating process for reported crimes operated at the monthly level as opposed to the weekly level, then we would find these coefficient estimates to be the same across lags. The fact that they differ is evidence in support of aggregating crime rates at the weekly level. Hence, even though BW is a theory about long run variation in crime rates, testing this theory should be done at the weekly level rather than at the monthly or yearly level.

Although our choice of $\tau = 4$ is arbitrary, we do perform a sensitivity analysis and find that this assumption does not appear to have substantive implications. When we reestimate the system of equations with $\tau = 5$ and $\tau = 6$, joint F-tests of the null hypothesis that all elements of β_5^c equal zero and all elements of β_6^c equal zero yield p-values of .75 and .40, respectively. To be sure, if there exist intertemporal behavioral effects of crime that unfold over longer time scales than six weeks, they would not be included in our parameter estimates. However, such effects would need to be orthogonal to any short-run (six weeks or less) behavioral effects that we do in fact estimate. For this reason, we believe that our preferred estimates with $\tau = 4$ reasonably capture all intertemporal behavioral effects of crime.

These results also provide empirical validation for our choice of T. Indeed, they suggest that the minimal level of T for which no component of the behavioral effect is absorbed ($\underline{T}$) is weakly shorter than four weeks, and the maximum level of T for which all confounding factors are absorbed ($\overline{T}$) is weakly longer than a year, suggesting that our choice of $T =$ year is appropriate (i.e., $\underline{T} < T \leq \overline{T}$).

5.3 A Formal Test of Endogeneity

In this section, we present a formal test of exogeneity that we perform on all specifications, which is based on continuity conditions and is inspired by Caetano (2012). We first discuss the intuition behind this test and why it has statistical power to reject the identifying assumption underlying our analysis. We then describe the test more formally. Equation (2) can be rewritten as

[31] Note that our findings of higher order within-crime intertemporal effects do not contradict the consistency of the estimates of β_1^c found in specifications 4-6 in table 2. That is, from an estimation standpoint, a specification of the system of equations with a single lag is valid. However, when computing dynamic spillovers in the next section, we would like to allow for all intertemporal causal effects, even at higher lags.

$$x^c_{jt} = X_{jt-1}\beta^c + Z^c_{jt}\gamma^c + \underbrace{Q^c_{jt} + \mu^c_{jt}}_{P^c_{jt}+\epsilon^c_{jt}}, \tag{7}$$

by gathering the two potential sources of endogeneity in the equation of motion for crime c. The total error component in equation (7) is split into two terms: Q^c_{jt}, which contains all unobserved determinants of x^c_{jt} that are correlated with X_{jt-1} (conditional on Z^c_{jt}) irrespective of their source, and μ^c_{jt}, which is, by construction, an exogenous error term. We depict our causal inference problem in the following diagram:

$$\begin{tikzcd}
 & Z^c_{jt} \arrow[dr, "\gamma^c"] & \\
X_{jt-1} \arrow[rr, "\beta^c"] \arrow[d, dash, dotted] & & x^c_{jt} \\
Q^c_{jt} \arrow[urr, dashed, "?"] & \mu^c_{jt} \arrow[ur] &
\end{tikzcd} \tag{8}$$

Each arrow represents a causal link from the right hand side variables to the dependent variable. The "?" in the dashed arrow refers to the fact that this link exists only if there is endogeneity in the specification. If instead Z^c_{jt} controls for Q^c_{jt}, then the only source of error in the regression is μ^c_{jt}, which is uncorrelated to X_{jt-1} by construction.

To conduct our test, we assume that the true relationship between X_{jt-1} and x^c_{jt} is *continuous* and we assume that the relationship between X_{jt-1} and the correlated unobservables (i.e., the dotted line) is *discontinuous*. Under these assumptions, if we estimate the relationship between X_{jt-1} and x^c_{jt} to be discontinuous, then it must be the case that this discontinuity arose due to Q^c_{jt}, which is evidence that the dashed arrow exists, i.e., that X_{jt-1} is endogenous conditional on Z^c_{jt-1}. On the other hand, if we estimate the relationship between X_{jt-1} and x^c_{jt} to be continuous, then this evidence is consistent with the claim that the dashed arrow does not exist or, alternatively that the dotted line does not exist since Z^c_{jt} successfully controlled for Q^c_{jt}. In either case, this finding is consistent with X_{jt-1} being exogenous conditional on Z^c_{jt-1}.

To fix ideas, we recast this test in the notation of our particular problem. From equation (7), the conditional expectation of x^c_{jt} given the control variables can be written as

$$E[x_{jt}^c|X_{jt-1}, Z_{jt}^c] = X_{jt-1}\beta^c + Z_{jt}^c\gamma^c + E[Q_{jt}^c|X_{jt-1}, Z_{jt}^c] \tag{9}$$

We decompose the source of endogeneity in equation (7) as[32]

$$\mathrm{E}\left[Q_{jt}^c|X_{jt-1}, Z_{jt}^c\right] = \sigma_{Q,X|Z}^c(D_{jt-1}\phi^c + X_{jt-1}\pi_x + Z_{jt}^c\pi_z) \tag{10}$$

where $\sigma_{Q,X|Z}^c \equiv \mathrm{Cov}\left(Q_{jt}^c, X_{jt-1}|Z_{jt}^c\right)$ is a scalar, ϕ^c is a $C\times 1$ vector, and $D_{jt-1} \equiv \left(d_{jt-1}^1, \ldots, d_{jt-1}^C\right)$ is a row vector of dummy variables defined as

$$\begin{aligned} d_{jt-1}^c &= 1 \text{ if } x_{jt-1}^c = 0 \\ &= 0 \text{ otherwise} \end{aligned} \tag{11}$$

Our goal is to design a test of hypothesis:[33]

$$\begin{aligned} H_0 &: \sigma_{Q,X|Z}^c = 0 \text{ for all } c \\ H_1 &: \sigma_{Q,X|Z}^c \neq 0 \text{ for some } c \end{aligned}$$

Note that if Z_{jt}^c contains the fixed effects λ_{Jt}^c and λ_{jT}^c, then H_0 implies assumption 1 and H_1 implies assumption 1 does not hold.[34] Hence this test is a formal, statistical method to test the identifying assumption for specifications 4-6. We can substitute equation (10) into equation (9), which we rewrite as

$$E[x_{jt}^c|X_{jt-1}, Z_{jt}^c] = X_{jt-1}(\beta^c + \pi_x\sigma_{Q,X|Z}^c) + Z_{jt}^c(\gamma^c + \pi_z\sigma_{Q,X|Z}^c) + D_{jt-1}\underbrace{\phi^c\sigma_{Q,X|Z}^c}_{\delta^c} \tag{12}$$

According to equation (12), β^c is identified by OLS under H_0, but under H_1 least squares estimates of the coefficient on X_{jt-1} will be biased. In general, we cannot identify $\sigma_{Q,X|Z}^c$ in equation (12) in order to test H_0. However, we can identify $\delta^c \equiv \phi^c\sigma_{Q,X|Z}^c$ by simply

[32]This equation can be written more generally as $\mathrm{E}\left[Q_{jt}^c|X_{jt-1}, Z_{jt}^c\right] = \sigma_{Q,X|Z}^c(D_{jt-1}\phi^c + f^c(X_{jt-1}, Z_{jt}^c))$ where f^c is continuous in X_{jt-1}, but otherwise unrestricted.

[33]Altonji et al. (2005) describe a different approach to measure the importance of Q_{jt}^c relative to total explanatory power of X_{jt-1} and Z_{jt}^c. In particular, they offer a method to compute the ratio of the amount of selection on unobservables relative to the amount of selection-on-observables that would be required to exist if the entire estimated effect was fully attributed to endogeneity. In addition to different primitive assumptions, the notable distinction between their approach and ours is that we are able to *test* the selection-on-observables hypothesis itself. Hence, we can make statements of the form, "we cannot reject exogeneity of X_{jt-1} at the $\hat{\alpha}$ level of significance" where $\hat{\alpha}$ is the critical size of the test that we can directly estimate.

[34]Strictly speaking, the converse (H_1 implies assumption 1 does not hold) is true if Z_{jt}^c *only* contains the fixed effects λ_{Jt}^c and λ_{jT}^c as in specification 4.

including D_{jt-1} in a least squares regression of the system of equations. If an estimate of δ^c for any c contains at least one non-zero element, then $\sigma^c_{Q,X|Z} \neq 0$, and we must reject the null hypothesis of exogeneity. In contrast, if we cannot reject that $\delta^c = \vec{0}$ for all c, then by extension, we cannot reject the null hypothesis. In order to determine whether $\sigma^c_{Q,X|Z} = 0$ if we find that $\delta^c = \vec{0}$, we need to ensure that $\phi^c \neq \vec{0}$.

Assumption 2. *$\phi^c \neq \vec{0}$ for some c.*[35]

Assumption 2 states that if Q^c_{jt} exists, it will be discontinuous at $x^{c'}_{jt-1}$ for some c'. Note that if assumption 2 was not satisfied, then this test would not be able to reject the null hypothesis of exogeneity. Thus, assumption 2 provides power to the test. Intuitively, if more elements of ϕ^c are different from zero, then this test is more powerful; that is, we can be more confident that our OLS estimates are unbiased when we do not reject H_0. In the online appendix, we provide theoretical and empirical evidence in favor of assumption 2. In particular, we show how this test has power to detect endogeneity due to unobserved police responses, (non-classical) measurement error, especially due to misreporting of crime, and other omitted determinants of crime.

To test formally for whether all elements of $\delta^1, \ldots, \delta^C$ are equal to zero, we use a joint F-test. For each specification, the F-test provides statistical evidence for the (non)existence of at least one variable that is wrongly omitted from the specification among all unobserved variables that vary discontinuously at $x^{c'}_{jt-1} = 0$ for some c'.

In table 2, we present the F-statistic (and p-value) for the test of exogeneity that corresponds to each specification. In specifications 1 through 3 we reject the null hypothesis and conclude that the parameter estimates in these specifications are biased. These rejections also show trivially that our test has power to detect endogeneity. On the other hand, in specifications 4 through 6 we are unable to detect endogeneity with this test, even at high levels of significance. Finally, in table 5 we perform an even more powerful test of exogeneity by adding D_{jt-2}, D_{jt-3} and D_{jt-4} and by testing whether all (120) of the estimates of the coefficients of these indicator variables are jointly equal to zero. We are also unable to reject the null hypothesis and detect endogeneity in this specification at high levels of significance. In total, this serves as an additional piece of evidence in favor of the claim that the parameter estimates in our preferred specifications correspond to the short run intertemporal behavioral effects of crime.

[35] Strictly speaking, we only need this assumption to hold under H_1.

6 Computing Dynamic Spillovers of Crime

The coefficient estimates in table 5 present the short run effects of light crimes in one week on all reported crimes in the following 1 to 4 weeks.[36] However, prior crimes may indirectly continue to affect future crimes of all types over a longer time horizon. Any test of the effectiveness of the BW policies must consider the long run dynamic effects of crimes, especially light crimes, which include the direct and indirect intertemporal effects both within and across crimes.

In order to explore these dynamic interactions, we use our coefficient estimates to perform an experiment in which we reduce one reported crime of a given type in week 0 and then simulate the evolution of all reported crimes in weeks $1, 2, \ldots$ holding all else constant. We then compute the cumulative change in the levels of all crimes relative to how they would have evolved in the absence of the counterfactual reduction. We interpret the cumulative simulated changes in future crime levels as the dynamic spillovers that are associated with reductions in current crime levels holding all else constant except the endogenous behavioral responses to crime.[37]

We present the cumulative long run spillovers associated with unit reductions of each type of crime in figure 1 along with 95% confidence intervals.[38] The label above each panel refers to the type of crime that we hypothetically reduce by one unit, and the labels for each bar refer to the type of crime that experiences the spillover. Note that the y-axis for rapes is at a different scale from the y-axis for the other crimes, since the estimated spillovers for rapes as an explanatory variable are relatively imprecise. It is immediate that all within-crime dynamic spillovers are statistically significant (except assault) and these spillovers tend to be large relative to across-crime dynamic spillovers (except rape and assault).

Importantly, we find no statistically significant across-crime dynamic spillovers associated with reductions in light crime, which suggests that a BW policy will have little success in reducing the future levels of more severe crimes. For perspective, the dynamic spillover benefits associated with a policy that targets either robbery or auto theft strictly dominate the dynamic spillover benefits of a policy that targets light crime, as the across-crime effects of reducing robbery and auto theft on future light crimes are of the same order of magnitude as the within-crime effect of reducing light crime. A policy that targets assaults generates dynamic spillover reductions in light crime that are smaller than the within-crime spillovers associ-

[36] The full set of estimates of β_k^c are presented in the online appendix table 4.

[37] We report upper bounds on these long run dynamic spillovers as conservative estimates by assuming zero intertemporal discounting.

[38] Because our system of equations is linear in $X_{jt-1}, \ldots, X_{jt-\tau}$, the cumulative long run spillovers can be computed analytically. The standard errors for these spillovers are calculated using the delta method, which accounts for the correlations among the elements of β_k^c for all c and k.

ated with light crime reduction, but this policy also generates positive spillover reductions in future rape and robbery levels. However, this policy generates no within-crime spillover. Even though a policy that targets burglaries does not generate across-crime spillovers, it generates the largest within-crime positive spillovers of all of the crimes.[39]

Although figure 1 offers insight into the statistical significance of our results and the trade-offs involved in targeting each crime, it is difficult to glean the economic significance of these dynamic spillovers. To provide this context, we conduct a simple thought experiment. First, we consider an average neighborhood in an average week of our sample. In this neighborhood, we perform a hypothetical intervention in which we fully eliminate all crimes of type c for a week and compute the total cumulative long run spillovers within and across all crimes as $t \to \infty$. We then express this long run dynamic spillover effect on crimes as a fraction of the average weekly crime level in the neighborhood. The results of this exercise allow us to construct an upper bound on the efficacy of a targeted city-wide intervention. We present the cumulative reductions in crimes from these interventions in table 6. For example, a complete elimination of light crime in the average neighborhood for one week (a reduction of 23.15 light crimes) generates a total future spillover reduction in rapes equal to only 9.5% of the average number of weekly rapes in the neighborhood (a cumulative reduction of 0.03 future rapes).

Three results are immediate from table 6. First, within-crime dynamic spillovers tend to be relatively large (except assault) and are precisely estimated. Second, across-crime dynamic spillovers from light crime reduction are both small and statistically insignificant, as even a full elimination of light crimes generates at most modest future reductions in more severe crimes (less than 10%). Third, across crime spillovers from policies that reduce other crimes are small and largely statistically insignificant.

In sum, these findings suggests that a BW law enforcement policy based on aggressively targeting light crimes will fail to reduce the future rates of more severe crimes in an economically significant way.

6.1 Cost-Benefit Analysis

Even if a BW law enforcement policy that targets light crimes is not effective in reducing future severe crime rates, it may still be an optimal policy from a cost-benefit perspective. We expand on this point by performing a back of the envelope evaluation of the monetary benefits

[39] We are hesitant to assess the benefits of a hypothetical policy that targets rape due to imprecision in our estimates of the dynamic spillovers associated with such a policy. The inclusion of rape in our analysis is important because we are able to precisely estimate the intertemporal effects of other crimes on rape. The results do not change when we drop rape from the analysis.

of various crime reduction policies. In table 7, we present estimates of the monetary benefits of a unit crime reduction.[40] In the first column, we list the social benefits of reducing one unit of each type of crime that we adapt from Heaton (2010) and supplement with Miller et al. (1993).[41] These estimates of the social costs of each type of crime are designed to account for both tangible and intangible costs of crime. Tangible costs include direct financial costs to individuals, businesses and governments including productivity losses. Intangible costs include losses in quality of life due to fear of crime and the psychological costs of victimization.

In the second column of table 7, we compute the total monetary benefits that are associated with a law enforcement policy that reduces one unit of a particular type of crime. We calculate these by simulating the dynamic spillover changes in all crimes associated with a unit reduction of a particular type of crime, valuing them according to the figures in column 1, and adding them to the direct benefit of the unit reduction. For example, a policy that reduces one robbery generates roughly \$86,000 in total social benefits in present value. On the other hand, a BW law enforcement policy that reduces one unit of light crime generates only \$3,341 in total social benefits in present value.[42]

A simple comparison of the benefits of crime reduction policies is incomplete without a concomitant consideration of the costs of implementing these policies. Unfortunately, we are unable to find external estimates of the marginal costs of abating specific crimes.[43] Nevertheless, we can still offer a rough policy prescription. In the third column of table 7, we present the total current and future benefits of unit crime reduction policies in terms of the same benefits associated with a unit light crime reduction policy. Unless the marginal cost of reducing robbery is more than 25.88 times the marginal cost of reducing light crime, a policy targeting robberies is preferable from a cost-benefit perspective to a BW law enforcement policy, and unless the marginal cost of reducing burglary is more than 7.06 times the marginal cost of reducing light crime, a policy targeting burglaries is preferable to a BW law enforcement policy. Similar results for the remaining crimes are presented in the table.

[40] All monetary values are presented in 2012 dollars.

[41] Details of the construction of these cost estimates can be found in the footnote to table 7.

[42] These estimated benefits are based on an analysis of only six types of crime; to the extent that reductions in these six crimes generate dynamic spillovers across other types of crimes (e.g., murder) in the future, we will underestimate the social benefits of any crime reduction. However, we believe these results are (if anything) biased in favor of finding support for BW policies because we expressly selected those crimes for which social learning is likely to matter.

[43] We believe this inability highlights the lack of attention to the net benefits of law enforcement policies in the literature so far.

7 Conclusion

The "Broken Windows" theory of crime has influenced urban law enforcement policy over the past twenty years in many cities. Although there has been a vibrant debate in the policy arena over its desirability and efficacy, surprisingly little work has been done to empirically validate such theory. In this paper, we offer robust empirical evidence against this theory.

The primary policy content of BW follows from the notion that less severe crimes will endogenously lead to more severe crimes being committed in the future. Under this assumption, a law enforcement agency utilizing BW policing techniques ought to divert policing attention and resources preferentially from more severe crimes to less severe crimes in order to take advantage of this dynamic spillover. Our analysis casts substantial doubt on the effectiveness of such a policy in abating more severe crimes, as we consistently find no evidence that a reduction of light crime leads to future reductions in the levels of more severe crimes. Instead, if an agency aims to reduce the levels of severe crimes, our analysis supports law enforcement policies that aggressively target severe crimes. Indeed, the evidence of dynamic spillovers from higher intensity crimes to light crime that we find suggests that the spillover benefits of such policies will strictly dominate the spillover benefits of a BW policy. We also find in a cost-benefit analysis that unless the marginal costs of combating severe crimes exceed the marginal costs of combating light crimes by a factor of at least 25 for robbery and 7 for burglary, BW policing is likely inefficient in addition to being ineffective.

We conclude by noting that further inquiry into the costs and benefits of targeted law enforcement policies is well warranted. Such information would allow for a fuller welfare analysis of law enforcement policy and could provide more precise and comprehensive prescriptions to policymakers.

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Table 1: Summary Statistics: 2000-2007

Variable	Rape	Robbery	Burglary	Auto Theft	Assault	Light Crime
Average reported crimes in a sector per week	0.42 (0.69)	4.58 (3.18)	13.30 (7.56)	10.71 (6.01)	21.29 (12.07)	23.15 (10.15)
Average police response time (hours)	2.37 (1.45)	1.29 (1.00)	1.39 (0.73)	0.88 (0.66)	1.38 (0.74)	1.41 (0.72)
Average police duration (hours)	1.08 (1.61)	0.97 (1.73)	0.59 (0.81)	0.41 (0.68)	0.67 (0.71)	0.62 (0.61)
Share of crimes committed at night	0.62	0.55	0.36	0.50	0.45	0.43
Share of crimes committed outdoors	0.26	0.59	0.02	0.79	0.33	0.58
Share of crimes committed on the weekend	0.35	0.33	0.24	0.30	0.35	0.29
Share of crimes reported by private businesses	0.00	0.20	0.34	0.06	0.00	0.10
Total reported crimes	5,439	59,015	171,506	138,086	274,586	298,520

Notes: Standard deviations are presented in parentheses where relevant. Average police response time is measured in hours from dispatch time to officer's arrival. Average police duration is measured in hours from officer's arrival to the scene of the crime to their departure. Night crimes occur between 8:00PM and 8:00AM of the following day.

Table 2: Intertemporal Behavioral Effects of Light Crime

Dep. Var. (t)	(1)	(2)	(3)	(4)	(5)	(6)
Rape	-0.006	0.002**	0.002	0.002	0.002	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Robbery	0.013	0.017**	0.018**	0.003	0.003	0.003
	(0.014)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
Burglary	0.088**	0.085**	0.045**	0.006	0.007	0.007
	(0.010)	(0.010)	(0.008)	(0.009)	(0.009)	(0.009)
Auto Theft	0.088**	0.095**	0.040**	0.009	0.008	0.008
	(0.009)	(0.010)	(0.008)	(0.008)	(0.009)	(0.008)
Assault	0.148**	0.159**	0.101**	0.016	0.016	0.015
	(0.012)	(0.013)	(0.012)	(0.010)	(0.010)	(0.010)
Light Crime	0.436**	0.412**	0.182**	0.066**	0.065**	0.065**
	(0.017)	(0.017)	(0.014)	(0.014)	(0.014)	(0.014)
All other crimes in $t-1$ included?	Yes	Yes	Yes	Yes	Yes	Yes
Year-Crime type FE included?	No	Yes	No	No	No	No
Sector-Crime type FE included?	No	No	Yes	No	No	No
Week-Crime type FE included?	No	No	Yes	No	No	No
Sector-Year-Crime type FE included?	No	No	No	Yes	Yes	Yes
Division-Week-Crime type FE included?	No	No	No	Yes	Yes	Yes
Controlled for frac. of each crime type at $t-1$ at daytime?	No	No	No	No	Yes	Yes
Controlled for frac. of each crime type at $t-1$ on the weekend?	No	No	No	No	Yes	Yes
Controlled for frac. of each crime type at $t-1$ outdoors?	No	No	No	No	Yes	Yes
Average police response time at t for each crime type included?	No	No	No	No	No	Yes
Average police duration at t for each crime type included?	No	No	No	No	No	Yes
R^2	0.810	0.812	0.851	0.867	0.867	0.867
Discontinuity test F-statistic (P value)	11.14**	1.85**	1.46*	0.90	0.85	0.87
	(0.00)	(0.00)	(0.05)	(0.62)	(0.70)	(0.67)
Number of observations	12,864	12,864	12,864	12,864	12,864	12,864

Notes: This table shows the coefficient estimates of light crime in period $t-1$ for each dependent variable in period t, as in equation (5). Heteroskedasticity robust standard errors clustered by sector-year-crime type are presented in parentheses. *: significant at 5% level. **: significant at 1% level. The other coefficient estimates of the regression are presented in table 1 in the online appendix.

Table 3: Standard Error Estimates for Specifications in Table 2 at Various Levels of Clustering

Dep. Var. (t)	(1)	(2)	(3)	(4)	(5)	(6)
Rape	**0.001****	**0.001****	**0.001**	**0.001**	**0.001**	**0.001**
	0.001**	0.001**	0.001	0.001	0.001	0.001
	*0.001***	*0.001*	*0.001*	*0.001*	*0.001*	*0.001*
Robbery	**0.013**	**0.005****	**0.004****	**0.004**	**0.004**	**0.004**
	0.004	0.004**	0.005**	0.005	0.005	0.005
	*0.003***	*0.003***	*0.004***	*0.005*	*0.005*	*0.005*
Burglary	0.010**	**0.010****	**0.008****	**0.009**	**0.009**	**0.009**
	0.011**	0.011**	0.007**	0.009	0.009	0.009
	*0.007***	*0.007***	*0.007***	*0.009*	*0.009*	*0.009*
Auto Theft	**0.009****	**0.010****	**0.008****	**0.008**	**0.009**	**0.008**
	0.010**	0.010**	0.009**	0.009	0.009	0.009
	*0.006***	*0.006***	*0.006***	*0.008*	*0.008*	*0.008*
Assault	**0.012****	**0.013****	**0.012****	**0.010**	**0.010**	**0.010**
	0.010**	0.009**	0.010**	0.010	0.010	0.010
	*0.009***	*0.010***	*0.009***	*0.011*	*0.011*	*0.011*
Light Crime	**0.017****	**0.017****	**0.014****	**0.014****	**0.014****	**0.014****
	0.017**	0.018**	0.013**	0.016**	0.016**	0.016**
	*0.009***	*0.010***	*0.010***	*0.013***	*0.013***	*0.013***
Discontinuity test P-value	**0.00****	**0.00****	**0.05***	**0.62**	**0.70**	**0.67**
	0.00**	0.002*	0.02*	0.43	0.47	0.49
	*0.00***	*0.001***	*0.09*	*0.88*	*0.91*	*0.91*

Notes: Standard error estimates are presented for coefficients estimated in table 2. The standard errors in **bold** are reproduced from table 2 and are clustered at the sector-year-crime type level. The standard errors in normal font are clustered at the division-year-crime type level. The standard errors in *italics* are clustered at the division-week-crime type level. *: coefficient significant at 5% level. **: coefficient significant at 1% level. The other standard error estimates of the regression are presented in table 2 in the online appendix.

Table 4: Robustness: Intertemporal Behavioral Effects of Light Crime - Additional Lags

	Number of Lagged Periods of Included Explanatory Variables			
Dep. Var. (t)	1	2	3	4
Rape	0.002	0.002	0.002	0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Robbery	0.003	0.002	0.002	0.002
	(0.004)	(0.004)	(0.004)	(0.004)
Burglary	0.007	0.003	0.003	0.003
	(0.009)	(0.008)	(0.008)	(0.008)
Auto Theft	0.008	0.007	0.007	0.006
	(0.008)	(0.008)	(0.008)	(0.008)
Assault	0.015	0.014	0.011	0.014
	(0.010)	(0.010)	(0.010)	(0.010)
Light Crime	0.065**	0.062**	0.062**	0.062**
	(0.014)	(0.013)	(0.013)	(0.013)
R^2	0.867	0.867	0.867	0.867
Discontinuity test F-statistic	0.87	0.83	1.18	1.15
(p-Value)	(0.67)	(0.82)	(0.13)	(0.13)
Number of Observations	12,864	12,864	12,864	12,864

Notes: This table shows the coefficient estimates of light crime in period $t-1$ for each dependent variable in period t, for each specification. All specifications include sector-year-crime type and division-week-crime type fixed effects. All specifications also control for the fraction of each crime type committed in the daytime, the fraction of each type of crime committed on the weekend, the fraction of each crime type committed outdoors, the average police response time and the average police duration for periods $t-1$ through $t-k$ where k is the specification number. Heteroskedasticity robust standard errors clustered by sector-year-crime type are presented in parentheses. *: significant at 5% level. **: significant at 1% level. The other coefficient estimates of the regression are presented in table 3 in the online appendix.

Table 5: Intertemporal Behavioral Effects of Light Crime - 4 Lags

Dep. Var. (c)	β_1^c	β_2^c	β_3^c	β_4^c
Rape	0.002	-0.001	0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Robbery	0.002	0.003	0.001	-0.008
	(0.004)	(0.005)	(0.004)	(0.004)
Burglary	0.003	0.001	0.018*	0.005
	(0.008)	(0.008)	(0.008)	(0.007)
Auto Theft	0.006	0.006	-0.004	0.002
	(0.008)	(0.007)	(0.006)	(0.007)
Assault	0.014	0.008	0.002	0.005
	(0.010)	(0.011)	(0.011)	(0.010)
Light Crime	0.062**	0.025*	0.018	0.013
	(0.013)	(0.011)	(0.011)	(0.012)
R^2	0.867			
Discontinuity test F-statistic	1.15			
(p-value)	(0.13)			
Number of Observations	12,864			

Notes: Column s refers to the coefficients of x_{t-s}^6 in our preferred specification, which includes sector-year-crime type and division-week-crime type fixed effects, and also controls for the fraction of each crime type committed in the daytime, the fraction of each type of crime committed on the weekend, the fraction of each crime type committed outdoors, the average police response time and the average police duration for periods $t-1$ through $t-4$. Heteroskedasticity robust standard errors clustered by sector-year-crime type are presented in parentheses. *: significant at 5% level. **: significant at 1% level. The other coefficient estimates of the regression are presented in table 4 in the online appendix.

Table 6: Cumulative Reduction in Crimes from Various Crime Elimination Policies

	Cumulative Reduction in Crimes from Elimination of:					
Crime	Rape	Robbery	Burglary	Auto Theft	Assault	Light Crime
Rape	-7.8%*	-6.3%	10.9%	-4.9%	24.5%*	9.5%
Robbery	2.0%	13.1%**	7.3%	-3.4%	11.9%*	-0.6%
Burglary	2.6%	2.4%	38.1%**	4.7%	-0.5%	7.2%
Auto Theft	-0.9%	1.5%	6.1%	20.9%**	-1.9%	3.5%
Assault	0.2%	2.1%	1.2%	2.4%	2.2%	3.8%
Light Crime	-0.4%	3.7%**	4.8%	4.9%*	4.8%*	13.9%**

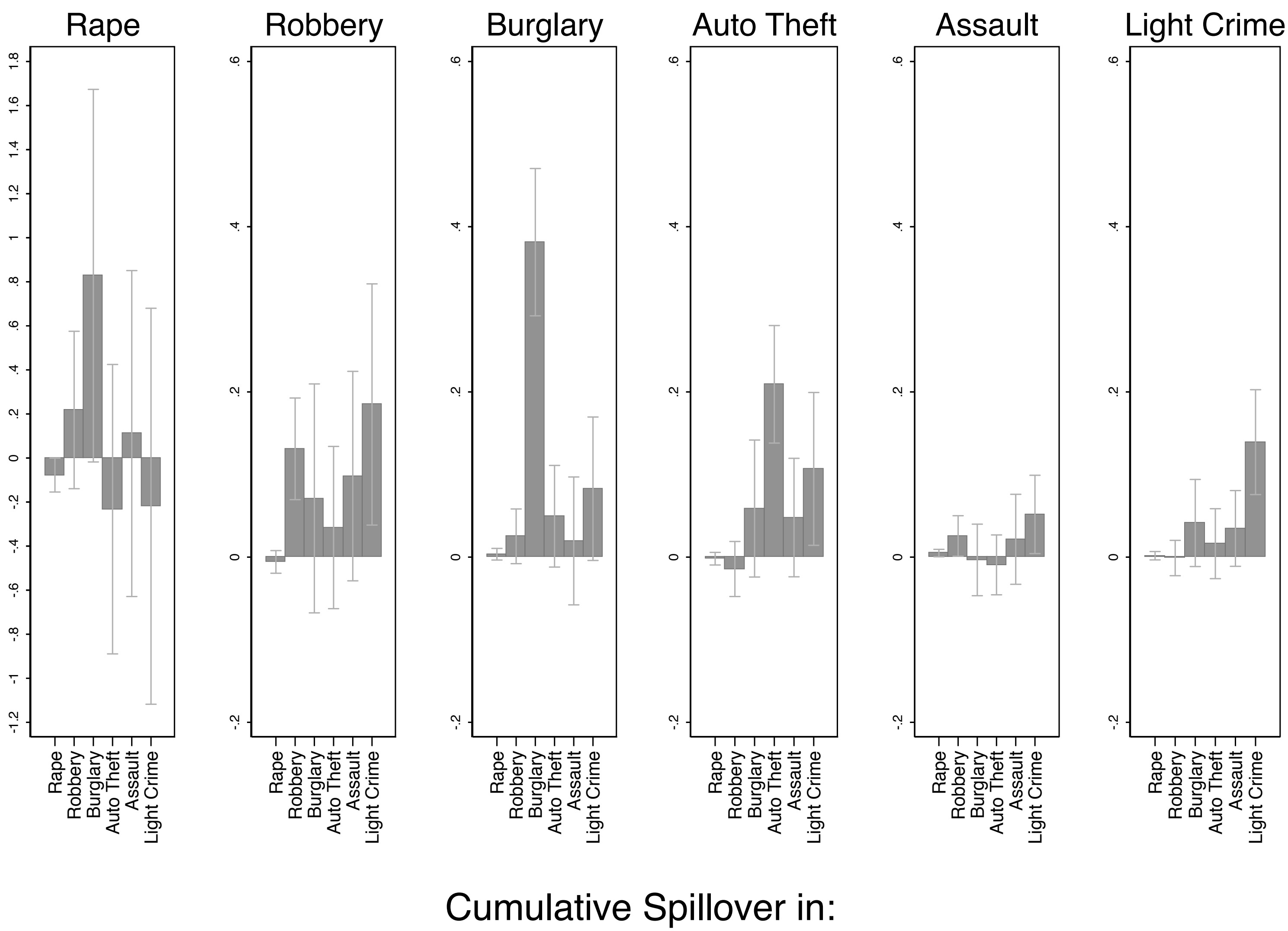
Note: Reductions are calculated by hypothetically eliminating one type of crime in the average neighborhood in the sample for a week, computing the total number of future crimes of each type that is reduced in that neighborhood and dividing by the average number of weekly crimes of each type in a neighborhood in the sample. For example, eliminating light crime in the average neighborhood for a week will generate a future reduction in rapes equal to 9.5% of the average number of weekly rapes in a neighborhood in the sample. *: coefficient significant at 5% level. **: coefficient significant at 1% level.

Table 7: Estimated Monetary Benefits of Unit Crime Reduction

Crime	Social Benefit of Unit Crime Reduction (Miller et al. (1993); Heaton (2010))	Total Benefits from Unit Crime Reduction ($)	Light Crime Monetary Equivalents
Rape	239,653	249,357	74.64
Robbery	74,005	86,462	25.88
Burglary	14,406	23,588	7.06
Auto Theft	9,987	12,621	3.78
Assault	22,944	26,429	7.91
Light Crime	1,176	3,341	1

Notes: The social costs of rape, robbery, burglary and auto theft are taken from Heaton (2010). We compute the social cost of all assaults by taking an average of the social cost of aggravated assault ($95,962) from Heaton (2010) and the social cost of simple assault ($13,457) from Miller et al. (1993) and weighting by the relative share of aggravated assaults in our sample (22.83%). We are unable to obtain estimates of the social cost of light crime, so we assume it to be half of the social cost of larceny as given in Heaton (2010). All monetary amounts are in 2012 dollars.

Figure 1: Long Run Cumulative Spillovers From Unit Crime Reductions



Note: Inside each panel s, the bars represent the long run cumulative spillover effect for each crime s' of a reduction of one unit in crime s. 95% confidence intervals for spillovers (calculated via the delta method) are also shown.

A APPENDIX

A.1 Detecting Endogeneity

In this appendix we provide theoretical and empirical support for assumption 2 by arguing that unobservable determinants of crime, measurement error and unobserved police responses to prior crimes (as well as other potential confounders) all vary discontinuously with $x_{jt-1}^{c'}$ at $x_{jt-1}^{c'} = 0$. In section A.1.1, we provide theoretical reasons for this discontinuity to occur, based on a mechanical bunching of observations at $x_{jt-1}^{c'} = 0$. In section A.1.2, we supplement this argument with indirect empirical evidence in favor of assumption 2. Taken together, this ensures that our test has power to detect endogeneity from several different sources.

A.1.1 Theoretical Support for Assumption 2

Our general theoretical argument for how discontinuities in Q_{jt}^{c} arise is based on the simple fact that that reported crimes are truncated at zero.[44] That is, there cannot be a negative number of reported crimes in any neighborhood. In general, this truncation will generate bunching of latent variables at the threshold of $x_{jt-1}^{c'} = 0$. To develop this argument and connect it to our application, we separately consider why omitted endogenous variables of three specific types will vary discontinuously at $x_{jt-1}^{c'} = 0$ for some c'. However, we should note that this is not an exhaustive list of sources of endogeneity that our test can detect, as an analogous argument may also work for other sources of endogeneity.

Figure 1 illustrates a hypothetical relationship between a generic explanatory variable, $x_{jt-1}^{c'}$, and a particular unobservable determinant of past crime, e.g., the average neighborhood wealth in period $t-1$, which is included in ϵ_{jt-1}^{c}. From the first panel of the figure, poorer neighborhoods are expected to have higher levels of reported crime, but in wealthier neighborhoods reported crime is expected to be lower. When the level of wealth is $\epsilon^{\star}$, no crimes are reported in expectation. For any neighborhood with an average wealth larger than $\epsilon^{\star}$, the expected level of reported crime will still be 0, as it cannot be negative. Conversely, we can plot the expected value of neighborhood wealth for each level of reported crime, as in the second panel of the figure. Here, we find a mechanically generated discontinuity in expected wealth when no crimes are reported: neighborhoods with no reported crimes include not only those with $\epsilon_{jt-1}^{c} = \epsilon^{\star}$ but also those with $\epsilon_{jt-1}^{c} > \epsilon^{\star}$. Intuitively, there are

[44] Note that this is distinct from the statement that reported crimes are *censored* at zero. Indeed, no observed variables in our analysis are censored at any value. Caetano (2012) discusses this distinction in detail.

neighborhoods that are so wealthy that even if they were slightly poorer, no crimes would be reported in expectation.

As another example, we illustrate a hypothetical relationship between $x^{c'}_{jt-1}$ and the propensity of neighborhood residents to misreport crime in period $t-1$ in figure 2. When a crime occurs, residents decide whether or not to report it to the police, hence this variable is an important determinant of measurement error. If poorer neighborhoods are more distrustful of law enforcement, or if property crimes committed in poorer neighborhoods are of lower value, then there would be greater misreporting in poor neighborhoods than in richer neighborhoods (Skogan (1977)). In conjunction with the previous example, this suggests that the propensity to misreport crime may be positively correlated with the level of reported crime. In the first panel of figure 2, neighborhoods that are more likely to misreport crime are shown to suffer from higher expected levels of reported crime. When the propensity to misreport is low enough (i.e., $\epsilon^c_{jt-1} = \epsilon^\star$), no crimes are reported in expectation. For any neighborhood with a lower propensity to misreport than $\epsilon^\star$, the expected reported level of crime will still be zero. This implies a discontinuity in the conditional expectation of the unobservable at the level of reported crime equals to 0, as shown in the second panel. Intuitively, in some neighborhoods nearly every crime that is committed is reported, and even if a few people began to harbor distrust of the police, the lack of actual crime in the neighborhood would still lead to no crimes being reported.

We add three remarks about the generality of the argument illustrated by these two examples. First, because our system of equations are specified on a panel with geographic and temporal dimensions, any bunching of *observations* (as opposed to bunching of neighborhoods) at $x^{c'}_{jt-1} = 0$ for some c' will generate a discontinuity similar to the one described in assumption 2. For instance, if a particular neighborhood has a higher level of some unobserved amenity in the first week of every month (say, because of a monthly farmer's market), then this unobservable amenity will vary discontinuously at $x^{c'}_{jt-1} = 0$ for some c'. Second, the relationship between the unobservable and $x^{c'}_{jt-1}$ need not be monotonic as illustrated in these figures, nor does the discontinuity need to lie in the direction of the slope of $x^{c'}_{jt-1}$ near $x^{c'}_{jt-1} = 0$. Finally, a sufficient (but not necessary) condition for assumption 2 to hold is that Q^c_{jt} is caused by some unobservable that causes $x^{c'}_{jt-1}$.[45] This guarantees that the bunching of observations with no reported crime ($x^{c'}_{jt-1} = 0$) will generate a discontinuity in Q^c_{jt} in expectation.[46]

[45]In figure 1, ϵ^c_{jt-1} is not included in Q^c_{jt}. However, Q^c_{jt} contains ϵ^c_{jt}, so any serial correlation in neighbors' wealth will generate a discontinuity in Q^c_{jt}.

[46]The only instance in which assumption 2 does not hold is if Q^c_{jt} is a discontinuous function of the unobservable at precisely the point at which that unobservable causes $x^{c'}_{jt-1} = 0$ (denoted as $\epsilon^\star$ in figures 1 and 2) and that the size of this discontinuity exactly offsets the original discontinuity of the unobservable.

Following the logic of the examples above, the police response to past crimes, P^c_{jt}, may be discontinuous at $x^{c'}_{jt-1} = 0$ if it is caused by P^c_{jt-1}. Indeed, the presence of police may affect the reporting of crime directly by deterrence or indirectly by mitigating measurement error, so any inertia in the allocation of police resources would imply a discontinuity in P^c_{jt} at $x^{c'}_{jt-1} = 0$. The police response may be discontinuous for a second reason: P^c_{jt} itself may be caused by X_{jt-1} discontinuously at $x^{c'}_{jt-1} = 0$. Consider for instance the level of attention that the police give to a neighborhood. Prior reported crimes may cause a change in this unobservable (e.g., due to a police crackdown). Hence, we would expect a positive relationship between $x^{c'}_{jt-1}$ for some c' and the unobservable police response P^c_{jt}, as illustrated in figure 3. When fewer crimes are reported, the police tend to reduce their attention in the following period.[47] Because the police observe only reported crimes as opposed to actual crimes, the response of the police to prior reported crimes could be discontinuously different when there is no crime reported versus when there is one crime reported simply because the lack of reported crime in a neighborhood may leave it "under the radar" for that week.

A.1.2 Empirical Support for Assumption 2

In addition to the theoretical arguments laid out in support of assumption 2, we provide indirect empirical evidence in the form of discontinuity plots (figure 4).[48] Each point in these plots represents the mean of the variable on the y-axis (some other potential determinant of current crime levels, such as another element of X_{jt-1} or an element of Z^c_{jt-1} for some c) conditional on a given level of $x^{c'}_{jt-1}$ for some c'. The dashed curve represents a third order local polynomial regression of all of these points for which $x^{c'}_{jt-1} > 0$, and the shaded region represents the 95% confidence region for this regression, with an out of sample prediction at $x^{c'}_{jt-1} = 0$.[49] Finally, the hollow point represents the observed mean value of the variable on the y-axis conditional on $x^{c'}_{jt-1} = 0$. If the hollow point lies outside of the shaded region, then this implies that the observed variable on the y-axis is discontinuous at $x^{c'}_{jt-1} = 0$ with

[47]There may also be a substitution effect as resources are re-allocated to prevent other types of crime. In this case, we may observe a negative relationship between prior reported crime and policing for $c \neq c'$. Regardless, our test is motivated only by the existence of this relationship, not by its sign.

[48]This empirical evidence in support of assumption 2 is analogous to the evidence supplied in a standard regression discontinuity (RD) framework in support of the identifying assumption that unobservables vary continuously at the threshold. Although researchers operating in an RD framework are unable to show that *all* unobservable determinants of the outcome vary continuously at the threshold, in practice they show that many observable determinants of the outcome vary continuously at the threshold. This constitutes indirect evidence that unobservables also vary continuously at the threshold. Similarly, in order to support assumption 2, we show that a variety of observables vary discontinuously at the threshold.

[49]For each regression, we use the Epanechnikov kernel with bandwidths of five for the kernel and the standard error calculation.

at least 95% confidence, which is evidence in support of the validity of assumption 2. Indeed, we show that there is ample evidence of discontinuities for various potential determinants of x^{c}_{jt}.[50] Although we cannot formally assess why the discontinuities arise in each figure, our test only requires that they exist, irrespective of the reason. Nevertheless, we offer suggestive explanations for these discontinuities with the intention of providing intuition for our test.[51]

In panels 4a and 4b we plot the shares of various reported crimes that occur on the weekend against reported crime levels in the same week. In the first panel, we find that assault occurs discontinuously more frequently on weekends in weeks with no reported burglaries, and in the second panel, we find that robbery occurs discontinuously less frequently on weekends in weeks with no reported auto thefts. Similarly, in panels 4c and 4d we plot the shares of various reported crimes that occur during the daytime against reported crime levels in the same week. We find that light crime and rape occurs discontinuously more frequently during the daytime in weeks with no reported auto thefts and burglaries respectively. All four of these discontinuities could reflect different levels of within-week intertemporal substitutibility between these various types of crimes. In panels 4e and 4f, we find analogous discontinuities in the shares of assaults and light crimes that occur outdoors against reported robberies and burglaries respectively. These discontinuities could reflect different levels of within-neighborhood spatial substitutibility between these various types of crimes. The discontinuities found in panels 4a-4f suggest that our test has power to detect endogeneity stemming from misspecification related to temporal and spatial aggregation (i.e., to our choices of j and t).

In panels 4g-4j we plot the average police response time to reports of various crimes in week t against reported crime levels in week $t-1$. In the first two panels, we find that police respond discontinuously faster to burglaries and light crimes after weeks in which no auto thefts are reported. This discontinuity may arise because police shift their attention away from auto thefts and towards other property crimes.[52] In panel 4i we find that police respond discontinuously slower to light crime after weeks in which no burglaries are reported, and in panel 4j, we find that police spend discontinuously less time at the scenes of reported rapes after weeks in which no auto thefts are reported. In all four cases, we find indirect evidence

[50]To be sure, our running variables, $x^{c'}_{jt-1}$, are discrete. Given the fact that they take on a wide variety of values, we treat them as continuous in order to test for discontinuities. This approach is commonly taken in regression discontinuity studies (e.g., Lee and McCrary (2005), Card et al. (2008)).

[51]In our sample there are no observations with zero units of light crime, so we cannot search for discontinuities in unobservables at $x^{\text{light crime}}_{t-1} = 0$. However, our test still has power against unobservables correlated to light crime to the extent that they are also correlated to other crimes for which we do find discontinuities (see, for example, panels 4c, 4f, 4h, 4i and 4k).

[52]In the case of light crimes (panel 4h), it may be that a discontinuously higher number of light crimes last week (see panel 4c) causes the police to pay more attention to these crimes the following week.

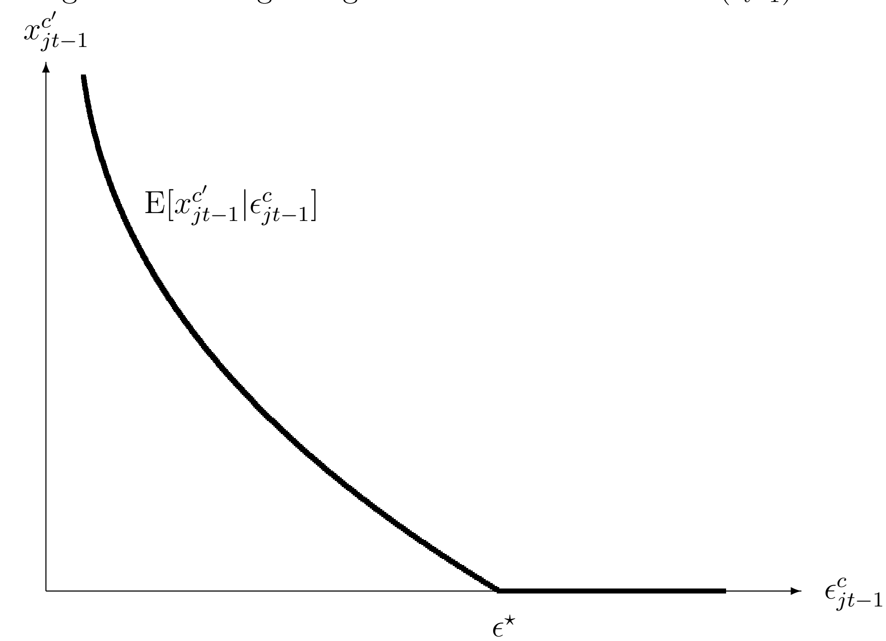
that P_{jt}^{c} varies discontinuously with some explanatory variables of interest. These results suggest that our test has power to detect endogeneity stemming from changes in future police actions.

In panel 4k we plot the share of light crimes that are reported by private businesses in week $t-1$ against reported burglaries in week $t-1$, and in panel 4l we plot the share of auto thefts reported by public employees in week $t-1$ against reported robberies in week $t-1$. In both cases, we find that when no burglaries and robberies are reported, the share of light crimes reported by private businesses and the share of auto thefts reported by public employees are discontinuously lower, respectively. This may reflect the propensity of non-individuals to under report less severe property crimes when more severe property crimes are absent. These discontinuities in reporting behavior suggest that our test has power to detect endogeneity stemming from measurement error in crime data.

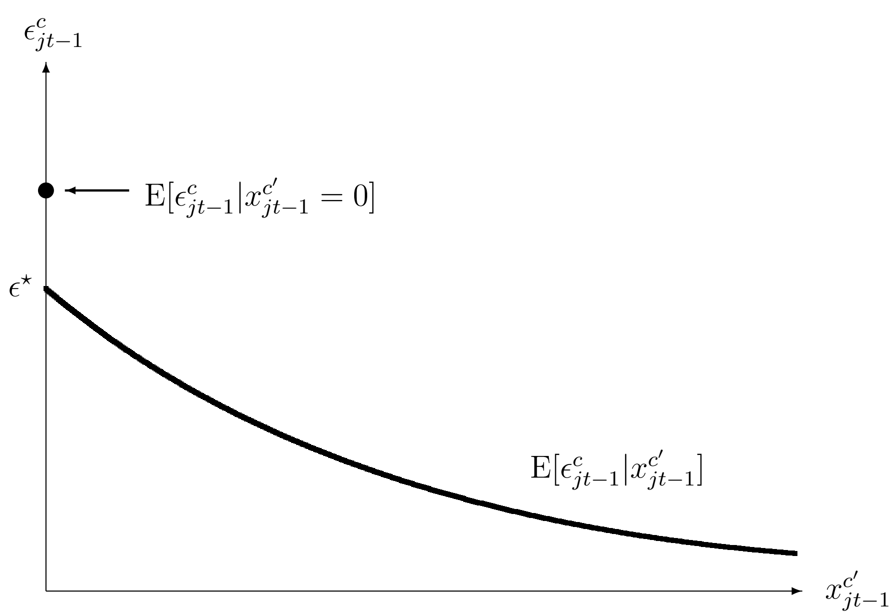
In totality, these discontinuity plots illustrate the variety of sources of endogeneity against which we have power to test.

A.2 Appendix Figures

Figure 1: Average Neighborhood Wealth in $t-1$ (ϵ_{t-1})

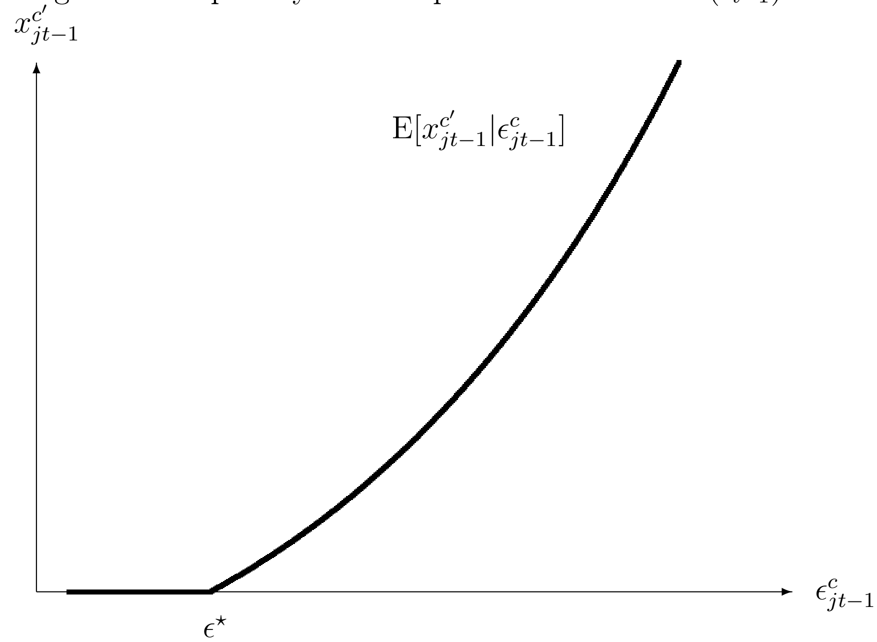


(a) Past Crime as a Function of Wealth

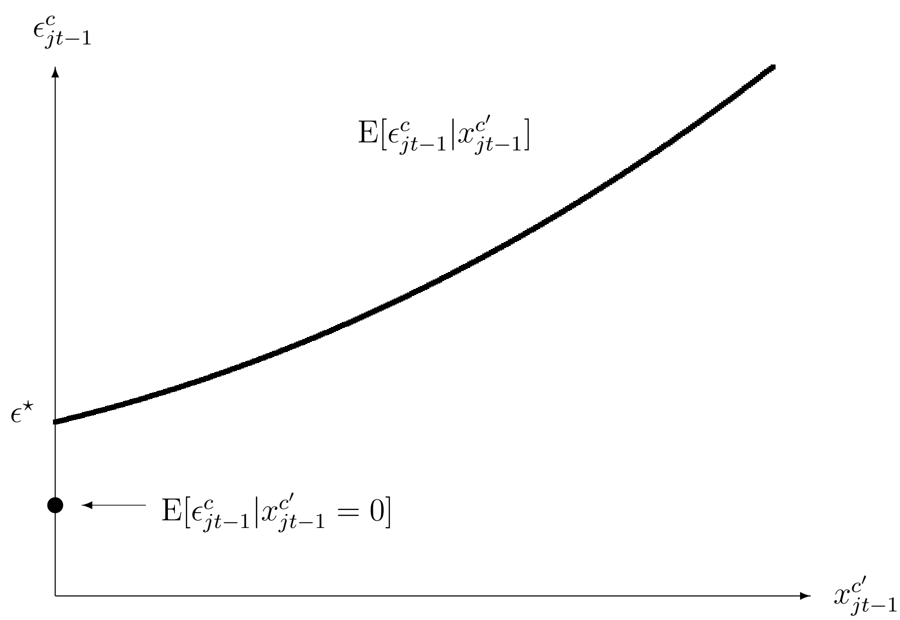


(b) Wealth as a Function of Past Crime

Figure 2: Propensity to Misreport Crime in $t-1$ (ϵ_{t-1})



(a) Past Crime as a Function of the Propensity to Misreport Crime



(b) Propensity to Misreport Crime as a Function of Past Crime

Figure 3: Unobserved Police Response (P_t) as a Function of Past Crime

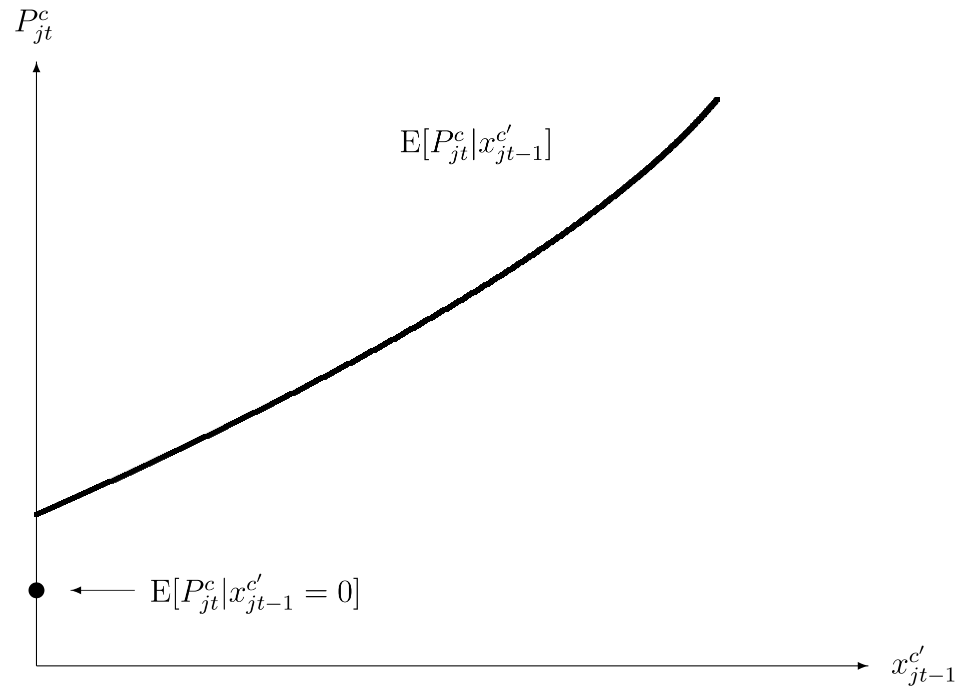
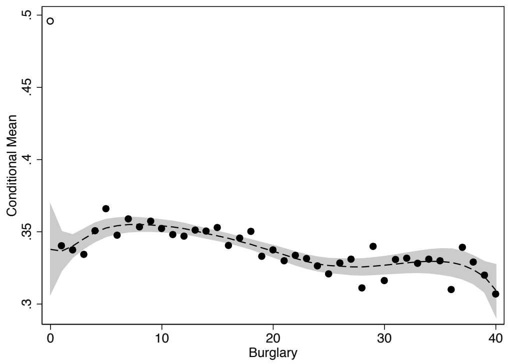
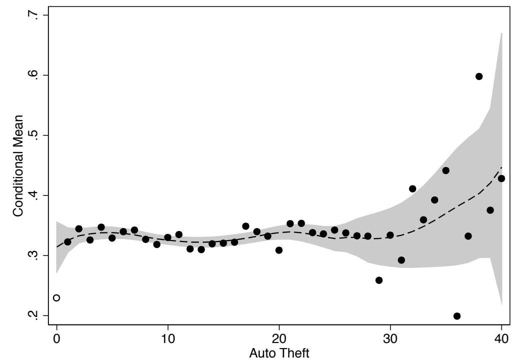


Figure 4: Discontinuity Plots of Determinants of Crime on Reported Crime Levels in week $t-1$ (1 of 2)

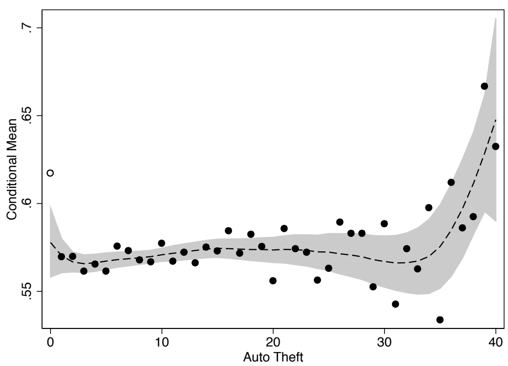
(a) Share of Weekend Assaults in week $t-1$



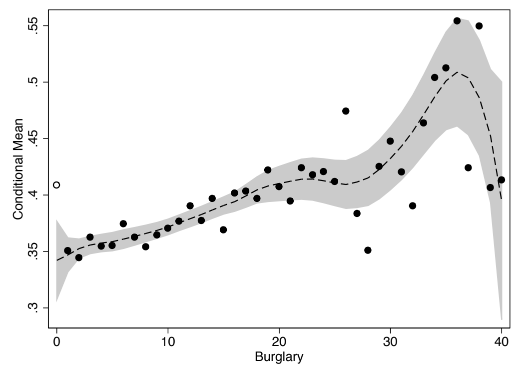
(b) Share of Weekend Robberies in week $t-1$



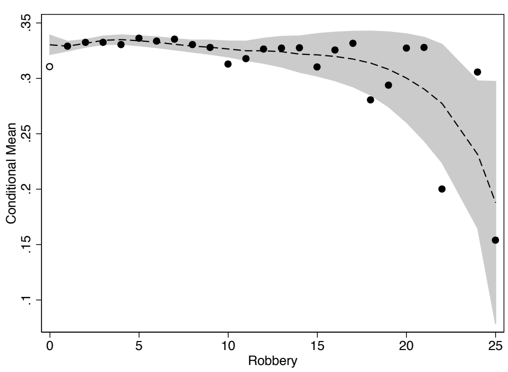
(c) Share of Daytime Light Crimes in week $t-1$



(d) Share of Daytime Rapes in week $t-1$



(e) Share of Outdoor Assaults in week $t-1$



(f) Share of Outdoor Light Crimes in week $t-1$

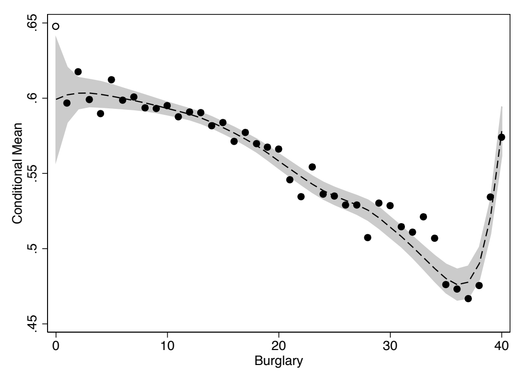
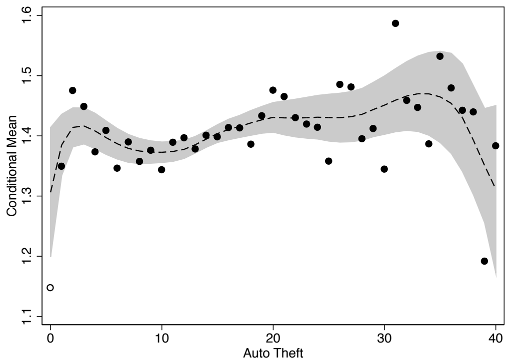
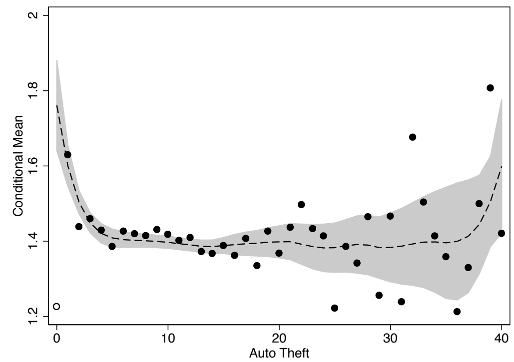


Figure 4: Discontinuity Plots of Determinants of Crime on Reported Crime Levels in week $t-1$ (2 of 2)

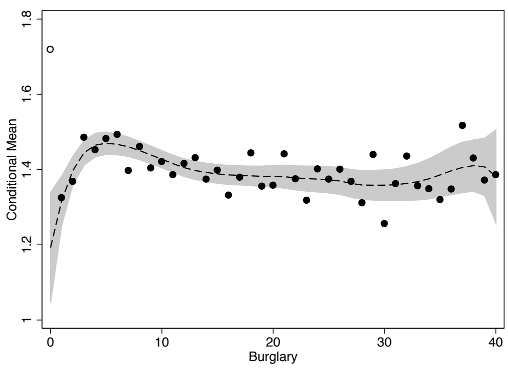
(g) Police Response to Burglaries in week t (in hours)



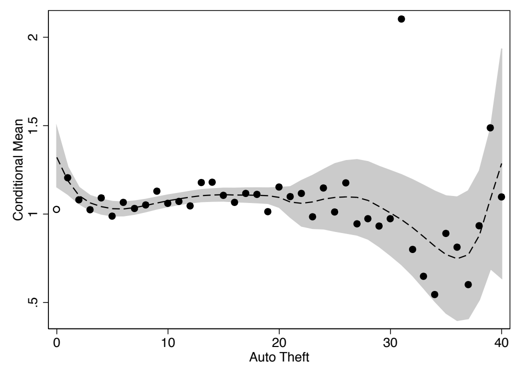
(h) Police Response to Light Crime in week t (in hours)



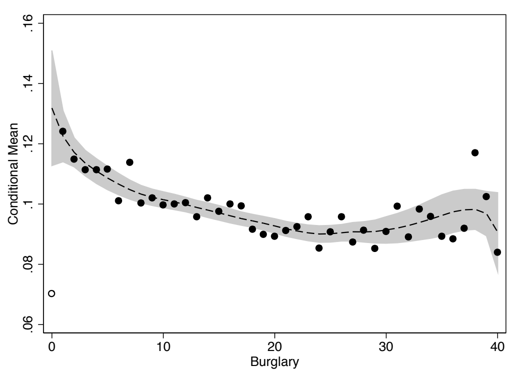
(i) Police Response to Light Crimes in week t (in hours)



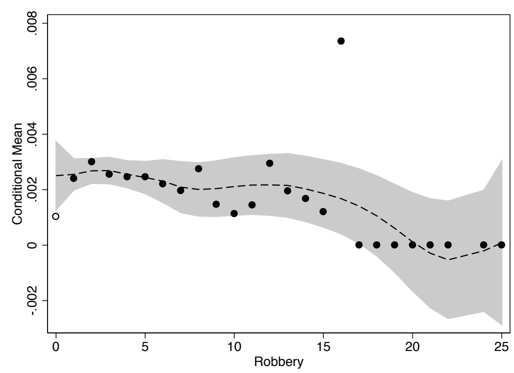
(j) Police Duration at Rape Crime Scenes in week t (in hours)



(k) Share of Light Crimes Reported by Businesses in week $t-1$



(l) Share of Auto Thefts Reported by Public Employees in week $t-1$



Note: Each point represents the mean of the variable on the y-axis conditional on the number of reports of the crime on the x-axis. The dashed curve represents a third order local polynomial regression with an Epanechnikov kernel and bandwidth equals to 5 for both the kernel and the standard error calculation using all points for which the explanatory variable is positive. The shaded region represents a 95% confidence region for this regression. The hollow dot represents the mean of the variable on the y-axis for which there are zero crimes on the x-axis reported.

A.3 Appendix Tables

This online appendix contains the estimates from the same tables in the paper, but for all types of crimes, including light crime.

Table 1: Intertemporal Behavioral Effects of All Crimes (1 of 3)

Dep. Var. (t)	RHS Var. ($t-1$)	(1)	(2)	(3)	(4)	(5)	(6)
Rape	Rape	-0.575**	0.044*	0.026	0.010	0.009	0.009
		(0.066)	(0.021)	(0.021)	(0.026)	(0.026)	(0.026)
	Robbery	-0.011**	0.001	-0.003	-0.005	-0.005	-0.005
		(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
	Burglary	-0.001	0.003*	0.000	-0.001	-0.000	-0.000
		(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
	Auto Theft	-0.001	0.003*	0.001	-0.001	-0.001	-0.001
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
	Assault	0.007**	0.008**	0.003**	0.002	0.002	0.002
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	Light Crime	-0.006	0.002**	0.002	0.002	0.002	0.002
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Robbery	Rape	-0.219*	0.126	0.092	0.086	0.088	0.087
		(0.093)	(0.082)	(0.073)	(0.074)	(0.074)	(0.074)
	Robbery	0.323**	0.325**	0.133**	0.061**	0.061**	0.061**
		(0.016)	(0.015)	(0.011)	(0.012)	(0.013)	(0.013)
	Burglary	0.026**	0.027**	0.025**	0.010	0.010	0.010
		(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
	Auto Theft	0.056**	0.059**	0.018**	-0.004	-0.004	-0.004
		(0.008)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)
	Assault	0.041**	0.043**	0.025**	0.010*	0.009	0.009
		(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
	Light Crime	0.013	0.017**	0.018**	0.003	0.003	0.003
		(0.014)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)

Table 1: Intertemporal Behavioral Effects of All Crimes (2 of 3)

Dep. Var. (t)	RHS Var. ($t-1$)	(1)	(2)	(3)	(4)	(5)	(6)
Burglary	Rape	0.014	0.181	-0.007	-0.123	-0.112	-0.111
		(0.141)	(0.147)	(0.013)	(0.145)	(0.145)	(0.144)
	Robbery	0.074**	0.076**	0.068**	0.013	0.014	0.014
		(0.026)	(0.025)	(0.021)	(0.024)	(0.024)	(0.024)
	Burglary	0.535**	0.512**	0.284**	0.154**	0.153**	0.153**
		(0.019)	(0.018)	(0.020)	(0.014)	(0.014)	(0.014)
	Auto Theft	0.057**	0.086**	0.030**	0.013	0.014	0.014
		(0.015)	(0.015)	(0.014)	(0.013)	(0.013)	(0.013)
	Assault	0.091**	0.105**	0.016	-0.005	-0.005	-0.005
		(0.010)	(0.009)	(0.008)	(0.007)	(0.007)	(0.007)
	Light Crime	0.088**	0.085**	0.045**	0.006	0.007	0.007
		(0.010)	(0.010)	(0.008)	(0.009)	(0.009)	(0.009)
Auto Theft	Rape	0.021	0.091	-0.043	-0.073	-0.066	-0.069
		(0.126)	(0.125)	(0.115)	(0.128)	(0.128)	(0.128)
	Robbery	0.163**	0.165**	0.071**	0.027	0.029	0.029
		(0.020)	(0.021)	(0.015)	(0.016)	(0.016)	(0.016)
	Burglary	0.051**	0.075**	0.028**	0.008	0.008	0.009
		(0.012)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)
	Auto Theft	0.471**	0.440**	0.243**	0.088**	0.088**	0.088**
		(0.015)	(0.015)	(0.015)	(0.012)	(0.012)	(0.012)
	Assault	0.037**	0.022**	0.047**	0.002	0.001	0.001
		(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
	Light Crime	0.088**	0.095**	0.040**	0.009	0.008	0.008
		(0.009)	(0.010)	(0.008)	(0.008)	(0.009)	(0.008)

Table 1: Intertemporal Behavioral Effects of All Crimes (3 of 3)

Dep. Var. (t)	RHS Var. ($t-1$)	(1)	(2)	(3)	(4)	(5)	(6)
Assault	Rape	-0.030	0.228	0.129	0.081	0.081	0.089
		(0.181)	(0.216)	(0.195)	(0.194)	(0.195)	(0.196)
	Robbery	0.189**	0.203**	0.115**	0.056	0.056	0.057
		(0.035)	(0.033)	(0.031)	(0.030)	(0.031)	(0.031)
	Burglary	0.166**	0.194**	0.044**	0.009	0.009	0.008
		(0.017)	(0.015)	(0.014)	(0.015)	(0.015)	(0.015)
	Auto Theft	0.075**	0.043*	0.107**	0.029	0.030	0.029
		(0.020)	(0.021)	(0.019)	(0.017)	(0.017)	(0.017)
	Assault	0.611**	0.593**	0.206**	0.017	0.016	0.016
		(0.016)	(0.014)	(0.016)	(0.012)	(0.011)	(0.011)
	Light Crime	0.148**	0.159**	0.101**	0.016	0.016	0.015
		(0.012)	(0.013)	(0.012)	(0.010)	(0.010)	(0.010)
Light Crime	Rape	1.536**	0.09	-0.034	0.032	0.020	0.028
		(0.172)	(0.201)	(0.181)	(0.195)	(0.196)	(0.197)
	Robbery	0.164**	0.132**	0.131**	0.081**	0.083**	0.084**
		(0.033)	(0.034)	(0.027)	(0.028)	(0.028)	(0.028)
	Burglary	0.194**	0.177**	0.097**	0.038**	0.038**	0.038**
		(0.018)	(0.018)	(0.014)	(0.015)	(0.015)	(0.015)
	Auto Theft	0.233**	0.234**	0.112**	0.024	0.024	0.024
		(0.022)	(0.023)	(0.019)	(0.019)	(0.019)	(0.019)
	Assault	0.162**	0.166**	0.103**	0.012	0.011	0.011
		(0.011)	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)
	Light Crime	0.436**	0.412**	0.182**	0.066**	0.065**	0.065**
		(0.017)	(0.017)	(0.014)	(0.014)	(0.014)	(0.014)
Year-Crime type FE included?		No	Yes	No	No	No	No
Sector-Crime type FE included?		No	No	Yes	No	No	No
Week-Crime type FE included?		No	No	Yes	No	No	No
Sector-Year-Crime type FE included?		No	No	No	Yes	Yes	Yes
Division-Week-Crime type FE included?		No	No	No	Yes	Yes	Yes
Controlled for frac. of each crime type at $t-1$ at daytime?		No	No	No	No	Yes	Yes
Controlled for frac. of each crime type at $t-1$ on the weekend?		No	No	No	No	Yes	Yes
Controlled for frac. of each crime type at $t-1$ outdoors?		No	No	No	No	Yes	Yes
Average police response time at t for each crime type included?		No	No	No	No	No	Yes
Average police duration at t for each crime type included?		No	No	No	No	No	Yes
R^2		0.810	0.812	0.851	0.867	0.867	0.867
Discontinuity test F-statistic (P value)		11.14**	1.85**	1.46*	0.90	0.85	0.87
		(0.00)	(0.00)	(0.05)	(0.62)	(0.70)	(0.67)
Number of observations		12,864	12,864	12,864	12,864	12,864	12,864

Notes: Heteroskedasticity robust standard errors clustered by sector-year-crime type are presented in parentheses. *: significant at 5% level. **: significant at 1% level.

Table 2: Standard Error Estimates for Specifications in Table 1 at Various Levels of Clustering (1 of 3)

Dep. Var. (t)	RHS Var. ($t-1$)	(1)	(2)	(3)	(4)	(5)	(6)
Rape	Rape	**0.066****	**0.021***	**0.021**	**0.026**	**0.026**	**0.026**
		0.082**	0.023	0.023	0.032	0.032	0.032
		*0.048***	*0.021**	*0.021*	*0.027*	*0.027*	*0.027*
	Robbery	**0.003****	**0.002**	**0.003**	**0.003**	**0.003**	**0.003**
		0.003**	0.003	0.003	0.004	0.004	0.004
		*0.003***	*0.002*	*0.003*	*0.003*	*0.003*	*0.003*
	Burglary	**0.002**	**0.001***	**0.001**	**0.002**	**0.002**	**0.002**
		0.002	0.001*	0.001	0.002	0.002	0.002
		0.001	*0.001**	*0.001*	*0.002*	*0.002*	*0.002*
	Auto Theft	**0.002**	**0.002***	**0.002**	**0.002**	**0.002**	**0.002**
		0.002	0.002	0.002	0.002	0.002	0.002
		0.002	*0.001**	*0.002*	*0.002*	*0.002*	*0.002*
	Assault	**0.001****	**0.001****	**0.001**	**0.001**	**0.001**	**0.001**
		0.001**	0.001**	0.001**	0.001	0.001	0.001
		*0.001***	*0.001***	*0.001***	*0.001*	*0.001*	*0.001*
	Light Crime	**0.001****	**0.001****	**0.001**	**0.001**	**0.001**	**0.001**
		0.001**	0.001**	0.001	0.001	0.001	0.001
		*0.001***	*0.001*	*0.001*	*0.001*	*0.001*	*0.001*
Robbery	Rape	**0.093***	**0.082**	**0.073**	**0.074**	**0.074**	**0.074**
		0.113	0.06*	0.049	0.065	0.066	0.066
		0.069**	0.079	*0.073*	*0.085*	*0.085*	*0.085*
	Robbery	**0.016****	**0.015****	**0.011****	**0.012****	**0.013****	**0.013****
		0.013**	0.014**	0.011**	0.014**	0.014**	0.014**
		*0.010***	*0.010***	*0.010***	*0.013***	*0.013***	*0.013***
	Burglary	**0.007****	**0.007****	**0.006****	**0.006**	**0.006**	**0.006**
		0.008**	0.008**	0.006**	0.008	0.007	0.008
		*0.005***	*0.005***	*0.005**	*0.006*	*0.006*	*0.006*
	Auto Theft	**0.008****	**0.008****	**0.007****	**0.007**	**0.007**	**0.007**
		0.008**	0.009**	0.006**	0.007	0.007	0.007
		*0.005***	*0.006***	*0.006***	*0.008*	*0.008*	*0.008*
	Assault	**0.005****	**0.005****	**0.005****	**0.004***	**0.004***	**0.004***
		0.004**	0.00**	0.005**	0.004*	0.004*	0.004*
		*0.003***	*0.003***	*0.004***	*0.005*	*0.005*	*0.005*
	Light Crime	**0.013**	**0.005****	**0.004****	**0.004**	**0.004**	**0.004**
		0.004	0.004**	0.005**	0.005	0.005	0.005
		*0.003***	*0.003***	*0.004***	*0.005*	*0.005*	*0.005*

Table 2: Standard Error Estimates for Specifications in Table 1 at Various Levels of Clustering (2 of 3)

Dep. Var. (t)	RHS Var. ($t-1$)	(1)	(2)	(3)	(4)	(5)	(6)
Burglary	Rape	0.141	0.147	0.013	0.145	0.145	0.144
		0.165	0.150	0.134	0.146	0.146	0.146
		0.110	0.148	0.141	0.160	0.160	0.160
	Robbery	0.026**	0.025**	0.021**	0.024	0.024	0.024
		0.024**	0.024**	0.025**	0.030	0.031	0.030
		0.019**	0.019**	0.019**	0.023	0.023	0.023
	Burglary	0.019**	0.018**	0.020**	0.014**	0.014**	0.014**
		0.020	0.018**	0.021**	0.017**	0.017**	0.017**
		0.010**	0.010**	0.011**	0.013**	0.013**	0.013**
	Auto Theft	0.015**	0.015**	0.014**	0.013	0.013	0.013
		0.020**	0.019**	0.014*	0.017	0.016	0.017
		0.019**	0.011**	0.012**	0.014	0.014	0.014
	Assault	0.010**	0.009**	0.008	0.007	0.007	0.007
		0.011**	0.009**	0.007*	0.008	0.008	0.008
		0.006**	0.006**	0.008*	0.010	0.010	0.010
	Light Crime	0.010**	0.010**	0.008**	0.009	0.009	0.009
		0.011**	0.011**	0.007**	0.009	0.009	0.009
		0.007**	0.007**	0.007**	0.009	0.009	0.009
Auto Theft	Rape	0.126	0.125	0.115	0.128	0.128	0.128
		0.145	0.110	0.102	0.121	0.121	0.121
		0.098	0.130	0.120	0.140	0.140	0.140
	Robbery	0.020**	0.021**	0.015**	0.016	0.016	0.016
		0.021**	0.021**	0.013**	0.015	0.016	0.015
		0.016**	0.016**	0.016**	0.019	0.019	0.019
	Burglary	0.012**	0.011**	0.010**	0.010	0.010	0.010
		0.015**	0.014**	0.009**	0.013	0.013	0.013
		0.007**	0.007**	0.008**	0.010	0.010	0.010
	Auto Theft	0.015**	0.015**	0.015**	0.012**	0.012**	0.012**
		0.014**	0.015**	0.013**	0.014**	0.014**	0.014**
		0.009**	0.009**	0.010**	0.012**	0.012**	0.012**
	Assault	0.008**	0.007**	0.007**	0.007	0.007	0.007
		0.009**	0.010*	0.008**	0.007	0.007	0.007
		0.005**	0.005**	0.006**	0.008	0.008	0.008
	Light Crime	0.009**	0.010**	0.008**	0.008	0.009	0.008
		0.010**	0.010**	0.009**	0.009	0.009	0.009
		0.006**	0.006**	0.006**	0.008	0.008	0.008

Table 2: Standard Error Estimates for Specifications in Table 1 at Various Levels of Clustering (3 of 3)

Dep. Var. (t)	RHS Var. ($t-1$)	(1)	(2)	(3)	(4)	(5)	(6)
Assault	Rape	**0.181**	**0.216**	**0.195**	**0.194**	**0.195**	**0.196**
		0.222	0.210	0.211	0.222	0.222	0.222
		0.147	*0.208*	*0.189*	*0.209*	*0.210*	*0.210*
	Robbery	**0.035****	**0.033****	**0.031****	**0.030**	**0.031**	**0.031**
		0.032**	0.032**	0.036**	0.037	0.038	0.038
		*0.025***	*0.025***	*0.024***	*0.029*	*0.030*	*0.030*
	Burglary	**0.017****	**0.015****	**0.014****	**0.015**	**0.015**	**0.015**
		0.019**	0.016**	0.013**	0.011	0.011	0.011
		*0.012***	*0.012***	*0.012***	*0.011*	*0.015*	*0.015*
	Auto Theft	**0.020****	**0.021***	**0.019****	**0.017**	**0.017**	**0.017**
		0.021**	0.023	0.018**	0.018	0.018	0.018
		*0.014***	*0.014***	*0.014***	*0.017*	0.017	*0.017*
	Assault	**0.016****	**0.014****	**0.016****	**0.012**	**0.011**	**0.011**
		0.026**	0.020**	0.020**	0.013	0.013	0.013
		*0.009***	*0.009***	*0.010***	*0.013*	*0.013*	*0.013*
	Light Crime	**0.012****	**0.013****	**0.012****	**0.010**	**0.010**	**0.010**
		0.010**	0.009**	0.010**	0.010	0.010	0.010
		*0.009***	*0.010***	*0.009***	*0.011*	*0.011*	*0.011*
Light Crime	Rape	**0.172****	**0.201**	**0.181**	**0.195**	**0.196**	**0.197**
		0.199**	0.222	0.217	0.228	0.228	0.228
		*0.142***	*0.199*	*0.187*	*0.214*	*0.214*	*0.214*
	Robbery	**0.033****	**0.034****	**0.027****	**0.028****	**0.028****	**0.028****
		0.028**	0.030**	0.028**	0.032*	0.032*	0.032*
		*0.023***	*0.023***	*0.024*	*0.030***	*0.030***	*0.030***
	Burglary	**0.018****	**0.018****	**0.014****	**0.015****	**0.015****	**0.015****
		0.018**	0.019**	0.012**	0.015**	0.015**	0.015**
		*0.011***	*0.011***	*0.012***	*0.015**	*0.015**	*0.015**
	Auto Theft	**0.022****	**0.023****	**0.019****	**0.019**	**0.019**	**0.019**
		0.022**	0.023**	0.020**	0.025	0.025	0.025
		*0.014***	*0.014***	*0.015***	*0.018*	*0.018*	*0.018*
	Assault	**0.011****	**0.011****	**0.011****	**0.010**	**0.010**	**0.010**
		0.011**	0.012**	0.011**	0.009	0.009	0.009
		*0.007***	*0.007***	*0.010***	*0.012*	*0.012*	*0.012*
	Light Crime	**0.017****	**0.017****	**0.014****	**0.014****	**0.014****	**0.014****
		0.017**	0.018**	0.013**	0.016**	0.016**	0.016**
		*0.009***	*0.010***	*0.010***	*0.013***	*0.013***	*0.013***
Discontinuity test	P-value	**0.00****	**0.00****	**0.05***	**0.62**	**0.70**	**0.67**
		0.00**	0.002*	0.02*	0.43	0.47	0.49
		*0.00***	*0.001***	*0.09*	*0.88*	*0.91*	*0.91*

Notes: Standard error estimates are presented for coefficients estimated in table 1. The standard errors in **bold** are reproduced from table 1 and are clustered at the sector-year-crime type level. The standard errors in normal font are clustered at the division-year-crime type level. The standard errors in *italics* are clustered at the division-week-crime type level. *: coefficient significant at 5% level. **: coefficient significant at 1% level.

Table 3: Robustness: Intertemporal Behavioral Effects of All Crimes - Additional Lags (1 of 3)

		Number of Lagged Periods of Included Explanatory Variables			
Dep. Var. (t)	RHS Var. ($t-1$)	1	2	3	4
Rape	Rape	0.009	0.008	0.008	0.011
		(0.026)	(0.026)	(0.026)	(0.026)
	Robbery	-0.005	-0.005	-0.005	-0.005
		(0.003)	(0.003)	(0.003)	(0.003)
	Burglary	-0.000	-0.001	-0.001	-0.001
		(0.002)	(0.002)	(0.002)	(0.002)
	Auto Theft	-0.001	-0.002	-0.000	-0.001
		(0.002)	(0.002)	(0.002)	(0.002)
	Assault	0.002	0.002	0.002	0.002
		(0.001)	(0.001)	(0.001)	(0.001)
	Light Crime	0.002	0.002	0.002	0.002
		(0.001)	(0.001)	(0.001)	(0.001)
Robbery	Rape	0.087	0.082	0.082	0.079
		(0.074)	(0.074)	(0.074)	(0.07)
	Robbery	0.061**	0.059**	0.058**	0.057**
		(0.013)	(0.013)	(0.013)	(0.013)
	Burglary	0.010	0.008	0.007	0.007
		(0.006)	(0.006)	(0.007)	(0.007)
	Auto Theft	-0.004	-0.005	-0.004	-0.004
		(0.007)	(0.007)	(0.007)	(0.007)
	Assault	0.009	0.008	0.008	0.007
		(0.004)	(0.004)	(0.004)	(0.004)
	Light Crime	0.003	0.002	0.002	0.002
		(0.004)	(0.004)	(0.004)	(0.004)

Table 3: Robustness: Intertemporal Behavioral Effects of All Crimes - Additional Lags (2 of 3)

		Number of Lagged Periods of Included Explanatory Variables			
Dep. Var. (t)	RHS Var. ($t-1$)	1	2	3	4
Burglary	Rape	-0.111	-0.099	-0.099	-0.102
		(0.144)	(0.143)	(0.143)	(0.143)
	Robbery	0.014	0.009	0.005	0.004
		(0.024)	(0.024)	(0.024)	(0.024)
	Burglary	0.153**	0.138**	0.134**	0.132**
		(0.014)	(0.012)	(0.012)	(0.012)
	Auto Theft	0.014	0.013	0.011	0.011
		(0.013)	(0.013)	(0.013)	(0.013)
	Assault	-0.005	-0.007	-0.007	-0.008
		(0.007)	(0.008)	(0.008)	(0.008)
	Light Crime	0.007	0.003	0.003	0.003
		(0.009)	(0.008)	(0.008)	(0.008)
Auto Theft	Rape	-0.069	-0.084	-0.080	-0.064
		(0.128)	(0.127)	(0.128)	(0.130)
	Robbery	0.029	0.028	0.026	0.027
		(0.016)	(0.016)	(0.016)	(0.016)
	Burglary	0.009	0.006	0.005	0.004
		(0.010)	(0.010)	(0.010)	(0.010)
	Auto Theft	0.088**	0.083**	0.081**	0.079**
		(0.012)	(0.011)	(0.011)	(0.011)
	Assault	0.001	0.000	-0.001	-0.001
		(0.007)	(0.006)	(0.006)	(0.006)
	Light Crime	0.008	0.007	0.007	0.006
		(0.008)	(0.008)	(0.008)	(0.008)

Table 3: Robustness: Intertemporal Behavioral Effects of All Crimes - Additional Lags (3 of 3)

		Number of Lagged Periods of Included Explanatory Variables			
Dep. Var. (t)	RHS Var. ($t-1$)	1	2	3	4
Assault	Rape	0.089	0.075	0.072	0.074
		(0.196)	(0.197)	(0.198)	(0.198)
	Robbery	0.057	0.055	0.054	0.054
		(0.031)	(0.031)	(0.031)	(0.031)
	Burglary	0.008	0.003	0.000	0.001
		(0.015)	(0.015)	(0.014)	(0.014)
	Auto Theft	0.029	0.029	0.028	0.026
		(0.017)	(0.017)	(0.017)	(0.017)
	Assault	0.016	0.016	0.016	0.017
		(0.011)	(0.011)	(0.011)	(0.012)
	Light Crime	0.015	0.014	0.011	0.014
		(0.010)	(0.010)	(0.010)	(0.010)
Light Crime	Rape	0.028	0.047	0.043	0.035
		(0.197)	(0.198)	(0.197)	(0.199)
	Robbery	0.084**	0.083**	0.085**	0.083**
		(0.028)	(0.028)	(0.028)	(0.028)
	Burglary	0.038**	0.035**	0.035**	0.034**
		(0.015)	(0.014)	(0.015)	(0.014)
	Auto Theft	0.024	0.021	0.021	0.020
		(0.019)	(0.019)	(0.019)	(0.018)
	Assault	0.011	0.010	0.009	0.007
		(0.010)	(0.010)	(0.010)	(0.010)
	Light Crime	0.065**	0.062**	0.062**	0.062**
		(0.014)	(0.013)	(0.013)	(0.013)
R^2		0.867	0.867	0.867	0.867
Discontinuity test F-statistic		0.87	0.83	1.18	1.15
(p-Value)		(0.67)	(0.82)	(0.13)	(0.13)
Number of Observations		12,864	12,864	12,864	12,864

Notes: All specifications include sector-year-crime type and division-week-crime type fixed effects. All specifications also control for the fraction of each crime type committed in the daytime, the fraction of each type of crime committed on the weekend, the fraction of each crime type committed outdoors, the average police response time and the average police duration for periods $t-1$ through $t-k$ where k is the specification number. Heteroskedasticity robust standard errors clustered by sector-year-crime type are presented in parentheses. *: significant at 5% level. **: significant at 1% level.

Table 4: Intertemporal Behavioral Effects of All Crimes - 4 Lags (1 of 3)

Dep. Var. (c)	RHS Var	β_1^c	β_2^c	β_3^c	β_4^c
Rape	Rape	0.011	-0.033	-0.030	-0.035
		(0.026)	(0.020)	(0.022)	(0.021)
	Robbery	-0.005	0.002	0.001	-0.004
		(0.003)	(0.003)	(0.003)	(0.003)
	Burglary	-0.001	0.003	0.001	-0.000
		(0.002)	(0.002)	(0.002)	(0.002)
	Auto Theft	-0.001	-0.002	0.001	-0.000
		(0.002)	(0.002)	(0.002)	(0.002)
	Assault	0.002	0.002	-0.000	0.002
		(0.001)	(0.001)	(0.001)	(0.001)
	Light Crime	0.002	-0.001	0.001	-0.000
		(0.001)	(0.001)	(0.001)	(0.001)
Robbery	Rape	0.079	0.089	0.081	-0.060
		(0.07)	(0.075)	(0.088)	(0.079)
	Robbery	0.057**	0.023	0.027*	0.009
		(0.013)	(0.012)	(0.011)	(0.013)
	Burglary	0.007	0.005	0.004	-0.001
		(0.007)	(0.006)	(0.006)	(0.006)
	Auto Theft	-0.004	0.010	-0.006	-0.012
		(0.007)	(0.007)	(0.007)	(0.007)
	Assault	0.007	0.003	0.004	0.007
		(0.004)	(0.005)	(0.004)	(0.005)
	Light Crime	0.002	0.003	0.001	-0.008
		(0.004)	(0.005)	(0.004)	(0.004)

Table 4: Intertemporal Behavioral Effects of All Crimes - 4 Lags (2 of 3)

Dep. Var. (c)	RHS Var	β_1^c	β_2^c	β_3^c	β_4^c
Burglary	Rape	-0.102	0.237	0.251	0.273
		(0.143)	(0.126)	(0.155)	(0.149)
	Robbery	0.004	0.023	0.021	-0.004
		(0.024)	(0.020)	(0.022)	(0.022)
	Burglary	0.132**	0.088**	0.034**	0.016
		(0.012)	(0.011)	(0.012)	(0.011)
	Auto Theft	0.011	0.011	0.009	0.004
		(0.013)	(0.012)	(0.013)	(0.013)
	Assault	-0.008	0.018*	-0.010	-0.008
		(0.008)	(0.008)	(0.008)	(0.008)
	Light Crime	0.003	0.001	0.018*	0.005
		(0.008)	(0.008)	(0.008)	(0.007)
Auto Theft	Rape	-0.064	-0.030	-0.120	-0.023
		(0.130)	(0.128)	(0.132)	(0.128)
	Robbery	0.027	-0.001	-0.013	0.009
		(0.016)	(0.017)	(0.020)	(0.019)
	Burglary	0.004	0.010	0.012	0.003
		(0.010)	(0.010)	(0.009)	(0.009)
	Auto Theft	0.079**	0.040**	0.032**	0.019
		(0.011)	(0.011)	(0.011)	(0.012)
	Assault	-0.001	0.001	0.005	-0.013*
		(0.006)	(0.007)	(0.007)	(0.006)
	Light Crime	0.006	0.006	-0.004	0.002
		(0.008)	(0.007)	(0.006)	(0.007)

Table 4: Intertemporal Behavioral Effects of All Crimes - 4 Lags (3 of 3)

Dep. Var. (c)	RHS Var.	β_1^c	β_2^c	β_3^c	β_4^c
Assault	Rape	0.074	0.134	-0.019	-0.081
		(0.198)	(0.194)	(0.170)	(0.181)
	Robbery	0.054	0.001	0.003	0.021
		(0.031)	(0.028)	(0.024)	(0.025)
	Burglary	0.001	0.024	0.036*	-0.052**
		(0.014)	(0.013)	(0.015)	(0.014)
	Auto Theft	0.026	-0.003	-0.013	0.027
		(0.017)	(0.020)	(0.016)	(0.016)
	Assault	0.017	-0.000	0.002	-0.001
		(0.012)	(0.012)	(0.011)	(0.013)
	Light Crime	0.014	0.008	0.002	0.005
		(0.010)	(0.011)	(0.011)	(0.010)
Light Crime	Rape	0.035	-0.060	0.082	-0.327
		(0.199)	(0.178)	(0.194)	(0.210)
	Robbery	0.083**	-0.012	0.016	0.046
		(0.028)	(0.031)	(0.026)	(0.028)
	Burglary	0.034**	0.014	0.010	-0.010
		(0.014)	(0.015)	(0.015)	(0.015)
	Auto Theft	0.020	0.040**	-0.007	0.023
		(0.018)	(0.015)	(0.016)	(0.019)
	Assault	0.007	0.014	0.014	0.008
		(0.010)	(0.011)	(0.010)	(0.011)
	Light Crime	0.062**	0.025*	0.018	0.013
		(0.013)	(0.011)	(0.011)	(0.012)
R^2			0.867		
Discontinuity test F-statistic			1.15		
(p-value)			(0.13)		
Number of Observations			12,864		

Notes: Each column refers to the lagged coefficients of our preferred specification, which includes sector-year-crime type and division-week-crime type fixed effects, and also controls for the fraction of each crime type committed in the daytime, the fraction of each type of crime committed on the weekend, the fraction of each crime type committed outdoors, the average police response time and the average police duration for periods $t-1$ through $t-4$. Heteroskedasticity robust standard errors clustered by sector-year-crime type are presented in parentheses. *: significant at 5% level. **: significant at 1% level.