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Shooting up Liquidity:

The Effect of Crime on Real Estate

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Abstract

We combine real estate data with various types of crime data using time and geospatial information to detect discontinuities in transaction densities and pricing around crime events in Rochester, NY. Discontinuities in transaction densities invalidate causal inference for price responses implied by the regression discontinuity design (RDD) approach. However, these discontinuities also capture the liquidity response to crimes and, together with the commonly emphasized price response, provide a richer picture of how crime affects housing valuation. A calibrated match-and-bargain model reveals that house valuations decrease between 6% and 25% after a crime, depending on the type of crime. These predictions are manifolds of the estimated effect on prices documented in this paper and in the literature. The welfare effects of crime are not uniform across market participants and can elicit considerable disappointment to uninformed buyers that move into a high-crime neighborhood.

Keywords: crime, real estate, liquidity effects, density discontinuity

JEL codes: C31, R21, R23, R30

1 Introduction

Residential property is an important indicator of economic conditions. We estimate the impact various types of crime have on real estate and quantify the concomitant welfare implications. Three stylized facts motivate our study. First, an individual's risk of being subject to crime depends on residential location. According to the National Crime Victimization Survey (2008) conducted by the Bureau Of Justice Statistics, 64% of property crimes and 34% of violent crimes between 2004 and 2008 in the U.S. occurred near or inside the victim's home. This leads to the following causal link: An increase in perceived risk of becoming a victim of a crime in the neighborhood prompts residents to reduce their reservation price to sell their house and move to a safer place. Second, residential property is often a prominent asset in a household's financial portfolio (Bertaut and Starr, 2000; Wolff, 2016). Primary residence represents more than 60% of total wealth for a typical U.S. household in the middle of the wealth distribution. Since crime can affect housing valuations and, as a consequence, household savings, home sale prices become a first-order source of information to determine the cost of crime. Third, the real estate market is notoriously illiquid. We define liquidity as the "expected time to sell an asset", as in Lippman and McCall (1986). The typical home was between 65 and 93 days on the market in 2018 according to Zillow, an online real estate and rental marketplace. This gives rise to an identification issue, as the timing of a house sale is endogenous. Estimates of pricing changes might not accurately reflect the true cost to residential welfare.

We contribute to the literature along various dimensions. First, crime and real estate have an endogenous relationship (Ihlanfeldt and Mayock, 2010a,b). Specifically, does crime devalue housing, or does cheap housing attract individuals more prone to crime? This adds another identification issue that calls for a careful analysis regarding the direction of causality. To this end, we consider the housing market shortly before a crime is reported as a control and as treated thereafter. This procedure is largely inspired by a regression discontinuity design (RDD) when time becomes the running or sorting variable (Hahn, Todd and Van der

Klaauw, 2001; Lee, 2008; Hausman and Rapson, 2018). Most recent studies on crime evaluate panel data with yearly frequencies (Bowes and Ihlanfeldt, 2001; Gibbons, 2004; Ihlanfeldt and Mayock, 2010*b*; Tita, Petras and Greenbaum, 2006). While their estimates mix short-run and long-run effects of crime levels, we identify the immediate effect of individual crimes and find that the effects vanish within a few months and a few hundred meters from the crime location.

Second, while RDD motivates our econometric approach, we explain why this setup does not warrant causal inference. In particular, our pricing and density estimations aim to detect a discontinuity around crime events. However, since the density in general displays a discontinuity, the estimated pricing coefficients do not warrant a causal interpretation.

Third, by quantifying the effect of crime on market liquidity, we add an important twist to the crime-housing relationship, which has been neglected in the literature. For this purpose, we estimate density functions and interpret jumps in the frequency of house sales as a boost or reduction in liquidity. Hence, our statistical approach can be characterized as detecting local discontinuities in outcomes (McCrary, 2008; Otsu, Xu and Matsushita, 2013).

Fourth, rather than using citywide housing and crime indices, as is often used in related work, we employ geospatial data that account for the granularity of housing markets. Our smallest estimation windows contain only transactions that are no further away than 300 meters from the location of a crime, and no more than 30 days before or after the crime had been committed. This avoids mixing the targeted effects with the long-run effects of individual crimes on future crime and of a house sale on the real estate market, as well as any further interaction effects between crime and real estate. Some previous studies also employ geospatial data, but their application of panel data methods requires spatial and temporal aggregation. As far as we know, only Gibbons (2004) and Ihlanfeldt and Mayock (2010*b*) used a dataset with similar spatial granularity.

Fifth, we account for delayed market responses due to strategic considerations in the real-estate market. In particular, transaction-relevant information is transmitted only slowly, coordination frictions give rise to delays, and the bargaining process takes time. According to Zillow, a closing period of a house sale lasts between 30 and 45 days. This adds an important dimension to the role time plays in this market.

Sixth, we propose a structural model that replicates our estimates and, unlike direct estimates, yields meaningful and interpretable conclusions of how crime impinges on residential welfare. In the Appendix, we provide an analysis of two counterfactual policies, one involving crime fighting and the other crime prevention.

Seventh, we shed light on which type of crime matters most. Previous research has either used a single crime variable or, when more than one crime variable was used, yields mixed results (Ihlanfeldt and Mayock, 2010*a*). We examine four types of crime – shootings, assaults, burglaries, and robberies – with data from the city of Rochester, New York, and for the period from 2009 to 2017.

Four novel results emerge from our statistical analysis, and two economic insights follow from our model calibration. The four statistical facts can be summarized as follows. (i) Crime is a determining factor for both prices and liquidity. In general, the relative liquidity response is a manifold of the price response. This first stylized fact provides three take-aways. First, price responses do not capture the full effect of crime. Second, two-dimensional responses need to be collapsed into a single response to assess the welfare implications for local residents. We address this with a structural model calibration. Third, a causal interpretation for price responses obtained from an RDD is unlikely to hold. (ii) The sign of the liquidity effect depends on the type of crime. While liquidity increases immediately after a shooting, hinting at fire sales from sellers wanting to leave the affected area, the other three types of crime depress market liquidity on impact. This suggests immediate market freezes with occupants sitting out a temporary crime spell. (iii) Violent crimes eventually lead to a delayed market freeze. The liquidity boosting effect of shootings is replaced by an opposite response after 30 days, while the existing market freeze exacerbates after an assault. (iv) Liquidity effects die out when we expand the estimation windows. This is an important extension relative to papers that estimate the effect of crime using annual frequencies and larger spatial spheres of influence. While these papers provide aggregate effects by compounding the initial direct effect from an individual crime with long-run effects, our estimates pin down the short-run effects that are attributable to individual crimes.

The two economic insights from the model calibration are as follows: (1) Information asymmetries and learning can explain the differences in market response between shootings and the other three crimes. The alternative explanation of time-consuming bargaining, where a house owner accepts a purchasing offer received before a crime occurred, does not accommodate the immediacy with which liquidity slumps in its wake. (2) The welfare losses arising from price and liquidity effects are considerable. Shootings reduce aggregate welfare by approximately 25% of housing values. Assaults and robberies decrease welfare by 14% and burglaries by 6%. This is equivalent to a reduction in the price for an average house of USD 19,400 after shootings, USD 12,000 after assaults, USD 5,500 after robberies, and USD 13,300 after burglaries. In comparison, the price response to crimes per square meter in the direct estimation is much lower. For example, for shootings, the price response is only 0.44%. This sizable difference between estimates and calibration is attributable to the neglect of liquidity effects. From the calibration, we conservatively infer an annual national welfare loss of USD 2,324 billion from burglaries, USD 1,775 billion from robberies, USD 1,599 billion from assaults, and USD 419 billion from shootings.

The remainder of the paper is organized as follows. Section 2 nests our approach in the existing literature. Section 3 describes the data, followed by the estimation in section 4. Section 5 presents the concomitant welfare implications. Section 6 concludes the paper. Additional results can be found in the Appendix.

2 Related Literature

The protection of personal well-being and private property is essential to a well-functioning society. Accordingly, crime features high on the list of social ills (Helsley and Strange, 1999). One negative effect of crime repeatedly shown in empirical studies is on house prices or values. A question still open for debate is what determines crime in the first place. Levitt (2004) surveys the validity of a wide variety of crime determinants. Dills, Miron and Summers (2008) argue that economists know little about the effectiveness of factors such as arrest rates and punishment. Reverse causality, where policy responds to crime, creates an endogeneity bias when crime variables are regressed on policy variables. This type of bias was largely ignored in early studies.[1]

Critical for our identification strategy is whether cheap housing attracts residents more prone to commit crimes. One possible reverse causality in our setup is that poverty and inequality increase crime rates. This claim is supported by strain theory (Merton, 1938), social disorganization theory (Shaw and McKay, 1942), and the economic theory of crime (Becker, 1968).[2] All three theories can tie cheap housing to crime, for which there is corroborating empirical evidence, for example, by Land, McCall and Cohen (1990) and Kelly (2000) based on aggregate measures of inequality and poverty. Freeman (1996), Grogger (1998), and Machin and Meghir (2004) associate a decline in wages in the low-wage labor market with an increase in crime, whereas the evidence on the relationship between unemployment and crime is mixed (Chiricos, 1987; Eide, Aasness and Skjerpen, 1994; Freeman, 1983, 1995).[3]

[1]For example, Ehrlich (1972) and Wilson and Boland (1978) focused on the cross-sectional association between police and crime.

[2]The strain theory argues that the relative success of others puts pressure on the less successful members of society. The social disorganization theory predicts a rise in crime whenever mechanisms of social control weaken. Poverty weakens the ability of a society to self-regulate because of social instability and residential mobility. According to the economic theory of crime successful members of society have goods that are worth stealing whereas unsuccessful ones face lower opportunity cost to become criminals.

[3]Looking at educational outcomes, Aizer (2008) found that the causal effect of crimes can be overstated because children who are more likely to be exposed to crimes also face other disadvantages, such as the lack of good educational opportunities.

A counterargument for cheap housing causing crime is that theft in poor neighborhoods is less rewarding. Adam Smith (1776, p. 670) already observed that crime and the demand for protection from it are both motivated by the accumulation of property. Some authors found that crime correlates positively with property values, for example, Case and Mayer (1996) in the Boston area and Lynch and Rasmussen (2001) in Jacksonville, Florida. However, this does not rid an empirical analysis of the endogeneity issue. Instead, reverse causality becomes nonmonotone in property values, making instrumenting difficult.

Studies about crime face potential omitted variable problems in both the cross section and time series dimensions (Aliyu et al., 2016). In the first dimension, crime rates may covary with other geographic amenities that researchers cannot adequately control for. Second, crime rates may change as the composition and characteristics of neighborhoods change. Reductions in crime levels may be associated with other changes that increase property values (Linden and Rockoff, 2008; Gibbons, 2004). Hence, various endogeneity issues have to be dealt with, which have led to a predominance of papers using panel data techniques such as first-differencing or quasi-experimental techniques such as instrumental variables and differences-in-differences. While the endogeneity of crime is widely recognized, from 19 studies reviewed by Ihlanfeldt and Mayock (2010*a*; 2010*b*), only six treated crime as an endogenous variable, and only one fully validated the choice of instrumental variables.[4]

A direct quantitative comparison of our analysis with studies using panel data and various instruments is difficult because all panel data studies are subject to some form of aggregation, usually regressing average property values on average crime rates. Some studies pool different types of crime (Bowes and Ihlanfeldt, 2001).

A further critical issue is that crime is undercounted, resulting in a "dark figure" (MacDonald, 2001) that can affect panel data approaches. Lower-income, younger, and male victims are in fact more likely to underreport than homeowners (Skogan, 1986). Further-

[4]Ihlanfeldt and Mayock (2010*a*; 2010*b*) mention at least five mechanisms making crime endogenous in a housing price model. Three would result in a positive crime-housing price relationship, the other two in a negative relationship.

more, violent crimes are more likely to be reported than property crimes (National Crime Victimization Survey, 2008). This implies that a variation in reported crimes over time can reflect changes in reporting frequency rather than changes in crime rates.[5]

These caveats notwithstanding, panel-data approaches have examined various issues. While Bowes and Ihlanfeldt (2001) looked at the nexus of residential property as well as access to public transport and crime and found a decrease in housing prices in high-crime areas, Gibbons (2004) reported that vandalism, graffiti and arson (but not burglaries) had a significant negative impact on house prices in London. Ihlanfeldt and Mayock (2010*b*) employed a distributed-lag panel data model for various crimes – murder, robbery, aggravated assault, burglary, auto theft, larceny, and vandalism – and concluded that only an increase in the density of aggravated assaults and robberies lowered housing values in the neighborhood.

How does our evidence compare with that reported in Ihlanfeldt and Mayock (2010*a*,*b*)? A direct comparison is not possible because we do not examine robberies and aggravated assaults based on a distributed-lag panel data model. However, these authors' findings that burglaries do not affect property values contrasts with our results. This discrepancy is surprising. A single crime event can trigger a downward spiral in the socioeconomic composition of communities, referred to as "urban flight" in the literature. As a consequence, crime-averse neighbors leave the area and are replaced by new residents who, arguably, could be more prone to criminal behavior. The latter is usually referred to as "contagion" which is derived from social interaction models where individual behavior depends not only on individual incentives but also on the behavior of peers and neighbors. Urban flight finds large empirical support.[6] While the empirical evidence also supports the contagion hypothesis,[7] contagious effects seem to have a limited scope (Ludwig and Kling, 2007). Taken together, the cumulative effects from urban flight and contagion as measured in a distributed lag

[5] Buonanno, Montolio and Raya-Vílchez attempt to bypass the underreporting problems by relying on victimization surveys to estimate the effect of crime perception on housing prices in the City of Barcelona from 2004 to 2006.

[6] See Liska and Bellair (1995); Morenoff and Sampson (1997); Dugan (1999); Cullen and Levitt (1999); Ellen and O'Regan (2009).

[7] See Case and Katz (1991); Ludwig, Duncan and Hirschfield (2001); Zenou (2003).

model, as in Ihlanfeldt and Mayock (2010*a*,*b*), are likely to bias the estimates even more downward than in our case.

Another study related to ours is George E. Tita, Tricia L. Petras and Robert T. Greenbaum (2006), who link 43,000 house sales from Columbus, Ohio, to crimes at the census tract level for 189 tracts between 1995 and 1998. To mitigate the effect of reverse causality, the authors use instruments and classify neighborhoods as "low," "medium," and "high" income. Violent crimes are shown to have a stronger impact on housing prices than property crimes. However, Ihlanfeldt and Mayock (2010*a*,*b*) point out that the ad hoc nature of instrumentation makes it difficult to interpret their results.

Two further studies that have some bearing with us examine whether living close to a convicted sex offender reduces house prices. Both find that having a registered sex offender moving into a house close-by reduces house prices by 2% (Pope, 2008) and 3% to 4% (Linden and Rockoff, 2008). In both studies, these effects are localized and quickly decline with the locational distance to the offender.

3 Data

Our analysis uses data from the city of Rochester, New York, from 2009 to 2017. This choice is simply motivated by the availability of their data. Rochester has approximately 206,000 inhabitants and is the seat of Monroe County that counts approximately 742,000 inhabitants (U.S. Census Bureau). The county-wide median household income was USD 60,240 in 2018, below the national level of USD 63,179. Not only income but also house prices are (considerably) lower than those across the country. The local median house price is USD 135,000, which compares with the national median of USD 227,000. At the same time, Monroe County exhibits a relatively low crime rate, ranking 9th among the 143 counties in New York State for which the FBI (2014) reports violent crime rates for 2014 and 5th for property crimes. While the crime rates of Monroe County are among the lowest in the state, the city of Rochester reports a higher rate of burglaries (for every 100,000 inhabitants) than any countywide rate in New York State.

We collected data from two different sources and connected the observations using geospatial and time information. The first dataset was provided by the Department of Taxation and Finance of New York State and sums up to 1,491,244 individual property transactions in New York State between May 6, 2008 and May 15, 2018.[8] The dataset includes the transaction date, the total sales price, and the size of each property. We omitted transactions with a total sale price below USD 50,000 and above USD 3,000,000 as well as properties smaller than 100 square meters or larger than 10,000 square meters. The dataset further classifies each house and transaction into categories. We selected sales involving "one family year-round residences", which are done "at arm's length". The latter excludes foreclosures and transactions between relatives. We used the services of OpenCage to convert the house address into longitude and latitude information.

The second source is the Rochester Police Department's Open Data Portal, which includes two different datasets. The first lists 3,655 shootings between January 15, 2000 and October 23, 2018 for the city of Rochester, NY. The shooting dataset defines when and where a crime was committed. The second dataset lists 89,897 crimes between January 1, 2011, and December 31, 2017. These data have observations for the other three types of crime considered in this paper, that is, burglaries, robberies and aggravated assault.

We analyzed the various types of crime separately. Let C denote the total number of crimes of a certain type and individual crimes indexed by c. A date realization is indicated by T_c and the geospatial information by $L_c = [L_{c1}\, L_{c2}]$ where L_{c1} (L_{c2}) represents the latitude (longitude) measurement. H denotes the number of house sales indexed by h. Time and geospatial data of sales are denoted similar to crimes. In addition, the property size is indicated by S_h and is measured in (the logarithm of) square meters. P_h mirrors the (logarithm of the) total sales price, while $\overline{P}_h$ is the (logarithm of the) price per square meter

[8]Excluding transaction data from the five boroughs that make up New York City: Bronx, Kings, New York, Queens, and Richmond.

(PPSM), which is the sales price normalized by the property size, or $\overline{P}_h = P_h - S_h$. We transformed the geospatial information into distance by linking crimes and sales pairwise into $C \times H = I$ crime-sales observations indexed by i. In particular, we characterize the time between crime c and sale h by $T_i = T_h - T_c$ so that $T_i > 0$ if sale i occurred after a crime. Furthermore, our measure for distance (in km) between crimes and house sales, $D_i = \sqrt{(L_{c1} - L_{h1})^2 + (L_{c2} - L_{h2})^2}$, has a direct interpretation in terms of locational distance. Finally, $X_i = \{T_i, D_i, S_i, P_i\}$ stands for realization i with distribution $F_i(X)$ with the probability density function $f_i(X)$.

Figure 1 shows the spatial distribution of crime events in Rochester, NY, against locations of house sales in the vicinity of the crimes. All crimes are registered inside the city limits. Arguably, the effect of crime is not contained by city limits. For this reason, we also included house transactions in neighboring townships. The blue dots display house transactions that are either in the city limits or within 600 meters of a crime. Family year-round residences form a donut-like shape around the city center with a small bite on the southwest side where the Rochester international airport is marked by a dark gray area as a point of reference. Crimes concentrate on the city center where fewer single-family residences are found and are shown in red. The relevant dataset eventually contains 19,148 house transactions, 15,035 burglaries, 5,201 robberies, 6,447 assaults, and 1,744 shootings.

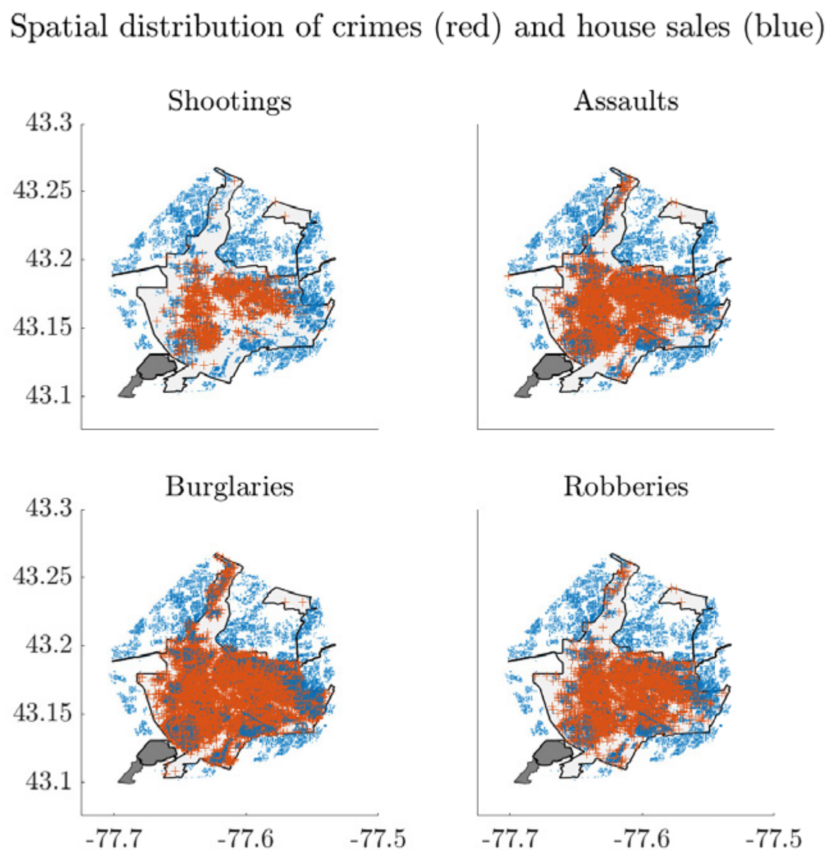

Figure 1: Geospatial distribution of crime and house transaction events in Rochester city. Red crosses represent individual crime events against the distribution of house transactions represented by blue dots. The horizontal axis displays longitude information, and the vertical axis displays latitude information. The dark gray area in the southwest highlights the location of the airport as a point of reference. The lines mark the city limits.

4 Estimation

This section describes our estimation. A preliminary analysis of the data is provided in subsections 4.1 and 4.2. Econometric issues are discussed in subsection 4.3, and the statistical specifications are in subsection 4.4. In subsection 4.5 we explain why causal inference ï¿œ la RDD is inappropriate for our setup although it motivates much of the statistical toolbox we apply. The results for liquidity and price responses to the various types of crimes are summarized in subsections 4.6 and 4.7, respectively.

4.1 Preliminary analysis

In this subsection, we discuss the pattern of key variables around crimes and summarize them in Table 1. The main insights are as follows: First, the number of house transactions increases after shootings and decreases after burglaries and robberies. Second, there is some evidence that assaults temporarily delay transactions. Both observations are evidence of fire sales and market freezes and are in line with our estimation results provided in Table 2. Third, price changes attributable to crime are negligible or insignificant. We put this down to the large heteroscedasticity among houses. Fourth, we distinguish between house sales that are finalized before and those after a crime. The former observations belong to the control group and the latter to the treatment group. We find some significant changes in covariates around crime events but in general small absolute differences between the control and the treated observations.

We discard observations outside an estimation window to focus on the immediate effect. This avoids mixing in long-run effects that might arise because of policy responses or a change in a neighborhood's composition. In particular, let v_T denote the maximum number of days between a crime and a house transaction and v_D the maximum locational distance. Hence, the bandwidth $\nu = \{\nu_T, \nu_D\}$ defines an estimation window. There is no information regarding the timing of each event during the day. Hence, we dismissed house transactions that were conducted on the same day a crime was perpetrated.

Table 1 compares the control and the treatment groups for a large ($\overline{\nu} = \{60 \text{ days}, 0.6 \text{ km}\}$) and a small ($\underline{\nu} = \{30 \text{ days}, 0.3 \text{ km}\}$) estimation window. The inequality signs highlight whenever the mean outcomes of the control and the treatment groups are significantly different from each other. We allowed the variance in the control and the treatment groups to be different in the two-sample t test. Because the number of days is negative in the control group and positive in the treatment group, the test compares the distributions for absolute realizations. Note that all observations are equally weighted in Table 1, while most estimations downweight house transactions further away from crimes using a triangular kernel. See our discussion in subsection 4.4.

	Estimation window	Large				Small			
	Group	**Pool**	**Control**		**Treatment**	**Pool**	**Control**		**Treatment**
Shootings	Number of obs	3,463	1,738		1,725	429	186	$<^{***}$	243
	Mean of T_i	-0.05	-30.58		30.71	2.00	-15.84		15.66
	Mean of P_i	11.25	11.25		11.25	11.25	11.25		11.25
	Mean of $\overline{P}_i$	5.04	5.05		5.04	5.06	5.03		5.08
	Mean of D_i	0.41	0.42	$>^{***}$	0.40	0.20	0.20		0.20
	Mean of S_i	6.21	6.20		6.21	6.19	6.22		6.17
Assaults	Number of obs	18,632	9,317		9,315	2,304	1,152		1,152
	Mean of T_i	0.29	-29.61	$<^{**}_{abs}$	30.19	0.43	-14.74	$<^{**}_{abs}$	15.60
	Mean of P_i	11.39	11.39		11.39	11.36	11.36		11.36
	Mean of $\overline{P}_i$	5.19	5.19		5.20	5.18	5.19		5.18
	Mean of D_i	0.41	0.41		0.41	0.20	0.20		0.20
	Mean of S_i	6.19	6.19		6.19	6.18	6.17		6.18
Burglaries	Number of obs	62,128	31,828	$>^{***}$	30,300	8,223	4,280	$>^{***}$	3,943
	Mean of T_i	-0.72	-30.20		30.26	-0.54	-15.21		15.37
	Mean of P_i	11.43	11.44	$>^{*}$	11.43	11.42	11.42		11.43
	Mean of $\overline{P}_i$	5.23	5.23		5.23	5.23	5.23		5.23
	Mean of D_i	0.39	0.39	$<^{*}$	0.40	0.20	0.20		0.20
	Mean of S_i	6.21	6.21		6.20	6.20	6.19		6.20
Robberies	Number of obs	20,459	10,376	$>^{***}$	10,083	2,659	1,382	$>^{**}$	1,277
	Mean of T_i	-0.35	-30.16		30.33	-0.46	-15.45		15.77
	Mean of P_i	11.46	11.47		11.46	11.44	11.46	$>^{*}$	11.43
	Mean of $\overline{P}_i$	5.26	5.26		5.27	5.25	5.25		5.25
	Mean of D_i	0.40	0.40		0.40	0.20	0.20		0.20
	Mean of S_i	6.20	6.21	$>^{**}$	6.19	6.20	6.21	$>^{*}$	6.18

Table 1: Summary statistics for key variables around crime events. T_i refers to the time difference between a house sale and a crime, P_i is the (log of the) total sales price, $\overline{P}_i$ is the (log of the) price per square meter, D_i is the locational distance between a house sale and a crime, and S_i is the (log of the) property size. Note that $\underline{\nu} = \{30 \text{ days}, 0.3 \text{ km}\}$ and $\overline{\nu} = \{60 \text{ days}, 0.6 \text{ km}\}$ define the data window. The inequalities indicate that the null hypothesis of a two-sample t test, applied to the absolute realizations, can be rejected with a type I error of 10% (*), 5% (**), or 1% (***).

We start by characterizing the mean outcomes for shootings. The first two lines (in each block) highlight the number of house transactions and the number of days between a crime and a house transaction. Arguably, these variables capture the liquidity effect of crime. The number of sales increases substantially and significantly after shootings for the small estimation window. The mean (absolute) number of days between shootings and sales does not vary substantially between the control and the treatment groups. However, the average for the pooled small window is 2, which appears tilted to the right. Arguably, crime-averse owners want to leave the area and fire-sales boost house sales, that is, liquidity.

The next two lines show pricing outcomes. We expected that a crime would lead to a decrease in the total sales price and the price per square meter (PPSM) as the crime event depresses the expectations of residents and potential buyers regarding the quality of life in the neighborhood. However, pricing outcomes are statistically indistinguishable between the control and treatment groups for shootings. Arguably, the idiosyncrasy of house properties is large, making a direct comparison between the control and the treatment groups inadequate.

The last two lines show the locational distance between crimes and the houses involved in a transaction as well as property sizes. For shootings, there is a notable decrease in the physical distance for the large estimation window. Postcrime house purchases are eventually 20 meters closer to the location of crime. The house transactions are pooled over all crimes so that neighborhoods from different parts of Rochester are pooled. The sparsity of houses across neighborhoods can vary so that we are unsure about the interpretation of this measurement.

There is little to no change in the number of sales observations after an assault. Rather, the (absolute) number of days between such an event and a transaction increases significantly. Arguably, the real estate market freezes for a couple of days, while purchases are simply delayed and not called off completely. The remaining outcomes for prices, locational distance, and property sizes reveal no substantial change between the control and the treatment groups.

Burglaries, in contrast to shootings, are followed by a significant reduction in the number of transactions, depressing liquidity. Owners seem to sit out a temporary increase in crime awareness that might depress housing prices, for which we found some (mild) evidence, although only for total sales prices in the large estimation window and at the 10% significance level. House transactions occur approximately 10 meters further away after a burglary occurs. Again, the change is only significant for the large estimation window and at the 10% level.

Robberies are also followed by a significant reduction in the number of sales. There is again (mild) evidence that total sales prices are lowered in the small estimation window. There is also some evidence that transacted houses are generally smaller. The property size changes range from 2% to 3% for the large and the small estimation windows, respectively. A possible interpretation is that there are fewer sales of larger houses after a robbery.

4.2 Graphical Analysis

To convey a better impression of how crime affects house sales, we display the detrended and recentered transaction data in Figure 2.[9] To simplify the discussion, we next introduce our baseline estimation model. We estimate discontinuities using a linear model with a threshold interaction, denoted $m^T(X, \boldsymbol{\delta})$, whose conditional expectation function reads

$$m^T(X, \boldsymbol{\delta}) = \delta + \delta_T T + (\Delta + \Delta_T T)\, \mathbb{I}(T > 0) \tag{1}$$

where $\boldsymbol{\delta}$ collects the coefficients $\{\delta, \delta_T, \Delta, \Delta_T\}$. δ and δ_T capture the level and trend over time if no crime had been committed. The second part, $(\Delta + \Delta_T T)\, \mathbb{I}(T > 0)$, captures the change after crime. Our parameter of interest is Δ and represents the immediate jump in the endogenous variable at the point of time the crime was committed. The second postcrime parameter Δ_T describes how a variable trends after a crime. However, its main purpose is to mitigate the impact of observations that come in late in the estimation window and be subject to indirect long-run effects of crime. Hence, we consider $\{\delta, \delta_T, \Delta_T\}$ to be nuisance parameters.[10]

The data manipulation leading to Figure 2 can be summarized as follows. We dropped

[9] Compare Imbens and Lemieux (2008); Lee and Lemieux (2010).
[10] They are not reported but are available upon request.

observations outside the closer vicinity of crimes ($\underline{\nu} = \{30 \text{ days}, 0.3 \text{ km}\}$), similar to the preferred estimation below, assigned house sales to 20 bins of equal size along the time dimension, and created bin-wide averages. For densities, we divided the number of observations in a bin by the total number of observations and took the logarithm. Similarly, distances were first transformed by the logarithm, and then all averages were rotated around the time trend line and recentered around zero. The black dots represent the detrended and recentered bin averages, while the horizontal black line displays the trend before a crime. Hence, a re-estimation of Equation (1) would yield $\delta = \delta_T = 0$. The vertical black lines at $T = 0$ display the discontinuity jump Δ around the time of crime. We colored the jump green if it was positive and in red if negative and significantly different from zero at the 10% significance level for a one-sided t test. The yellow lines show the trend after a crime and are derived from a linear estimation that appears to be nonlinear because of the rescaling of the vertical axis. Unlike in the full estimation below, the distance to the location of crime is not considered in these calculations.

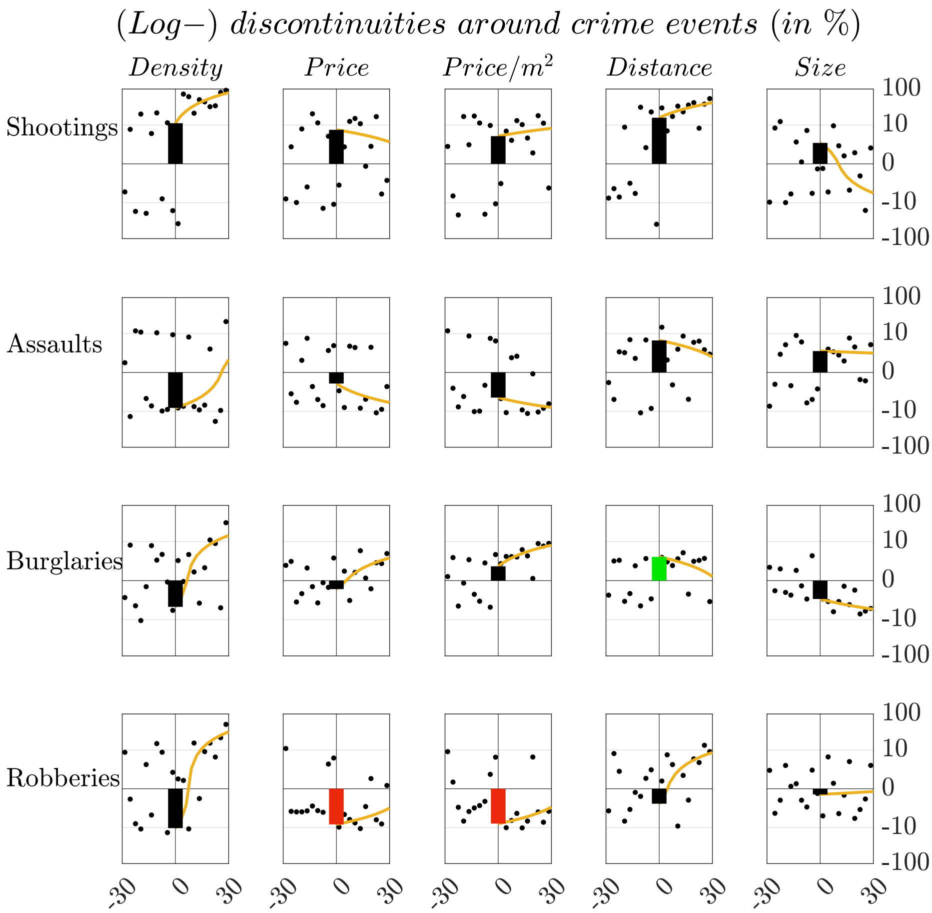

Figure 2: (Dis)continuities in house sales around crime with time plotted on the horizontal axis. The four rows correspond to different types of crime. Each column corresponds to a different outcome variable denoted in the column header. The 20 black dots display detrended and recentered equiwide bin averages, where 10 lead up (follow) the corresponding offense. The horizontal black line exhibits the trend in the absence of a crime. The vertical black lines at $T = 0$ display the discontinuity jump Δ around the time a crime occurs. Green implies a significant positive jump, and red implies a negative jump around the time of crime. The yellow lines to the right of $T = 0$ are the postcrime trends based on the linear specification in Equation (1). The vertical axis is rescaled so that the postcrime trend does not reflect its linear nature visually. The bandwidth ($\overline{\nu} = \{30 \text{ days}, 0.3 \text{ km}\}$) corresponds to the small estimation window used in Tables 2 and 3.

We emphasize three issues. First, the directions of the discontinuity jumps in the densities are in line with the full estimates shown in Table 2. Nonetheless, none of the jumps are significant although binning biases the noise estimates downward. Second, and in line with our discussion above, we detect jumps in the pricing process across the board. However, only robberies seem to significantly depress total sales prices. The other six jumps are not significant and, in fact, point equally often to a price decrease as well as an increase. Third, the two covariates – the distance between the crime to the location of the house and a property's size – do not show significant systematic variation between the control and the treatment groups except for burglaries. Immediately after a burglary, houses are purchased further away than in the control window, which is in line with our initial analysis for the large estimation window.

4.3 Econometric Issues

We want to understand how crimes affect local residents by eliciting their responses in the real estate market. Market prices reveal housing valuations in general. However, there are several caveats in a direct estimation strategy that we address next.

First and foremost, illiquid markets suggest a strategic component to market valuations whereby sellers reject an offer for a better one in the future. Hence, price changes incorporate immediate changes to residential utility as well as changes to (future) resale option valuations. This calls for an analysis of possible delays in market reactions to crimes. We consider a delay $\xi \geq 0$ in our estimations, exclude observation i if $0 < T_i < \xi$ and, accordingly, extend the estimation window.[11]

Second, as mentioned, a fundamental issue is the direction of causality. As cheap real estate may attract individuals prone to crime, crime rates and real estate prices may correlate across neighborhoods in the long run. Since we compare the real estate market before and after a crime, serial dependence in the market or in the underlying crime process can introduce a long-run bias if the estimation window is too large.[12] We address serial dependence

[11] Compare the right panel of Figure 4 in the online Appendix for an example of a delayed response in the density function.

[12] For example, assume the instantaneous price effect is negative ($\Delta < 0$) and prices depreciate at rate α upon an observed price drop independent of its cause. A large estimation window compounds the initial crime effect with the subsequent self-exciting effect of a price drop. A similar line of thought applies to a self-exciting effect in crimes. Assume crime is followed by more crime while the instantaneous price effect is still $\Delta < 0$. The estimated Δ is magnified (in absolute terms) by the subsequent crimes if the estimation window is too large. If, on the other hand, an initial crime gives rise to effective policing, and we might not be able to pick up any effect.

in four ways: (i) we allow for a change in the effect of time captured by the coefficient Δ_T in Equation (1), (ii) downweight observations that are further away from the crime event in time and location, (iii) keep the estimation windows small, and (iv) test explicitly for the self-exciting effects of each type of crime and the self-exciting effect of house sales.

Tables 8 and 9 in the Appendix summarize the results. Accordingly, the frequencies of assaults, burglaries, and robberies decrease significantly immediately after such an event. Both estimations for responses to shootings are also negative but insignificant. The estimates for Δ show a sizable and significant reduction in the density of crimes following an event if we allow for a 30-day delay. House sales depress the density of future sales immediately and with a 30-day delay. A house sale increases the total sales price immediately and with a delay but depresses the PPSM in our preferred estimation, which uses a partly linear model. We motivate this preference below.

Third, another critical question is whether the focus on housing valuations captures the whole welfare effect of crime. It is well known that crime varies across locations. Grogger and Willis (2000) show that city centers are more crime ridden than suburbs. As discussed in the data section, this also applies to our data. The fact that crime varies locally supports our assumption that the main obstacle for moving from a crime-ridden to a safer neighborhood can be traced back to transaction costs linked to real estate. In contrast, changes to other amenities, such as access to educational, professional, or other location-bound opportunities, arguably factor in less. Households could just move to another neighborhood and keep enjoying the advantages offered by their old neighborhood. Hence, changes in house owners' reservation prices arguably capture the largest part of the welfare cost from crime.

Fourth, we assume that crime events are predetermined in a statistical sense. There are incentives to commit or incite a crime to buy a house cheaper. We cannot rule out such a possibility but deem it unlikely that our sample is dominated by such instances.

4.4 Specifications

In this subsection, we describe technical choices that apply to discontinuity estimates in densities and to pricing results. First, the trade-off from a larger estimation window is a gain in efficiency against a potential bias that may arise from long-run effects. Standard procedures to pick an asymptotically optimal bandwidth, as described by Imbens and Kalyanaraman (2012), are inappropriate in our context, as we have distance as a second dimension to account for. Hence, our bandwidth choices are ad hoc but informed by preliminary data analysis and motivated by the question of how quickly effects vanish when the estimation window is expanded. For this reason, we report a variety of preferred bandwidths. To keep the estimates for the liquidity and the price response comparable, we employ similar bandwidth choices. In particular, for each crime, we use a small ($\underline{\nu} = \{\underline{\nu}_T, \underline{\nu}_D\} = \{30 \text{ days}, 0.3 \text{ km}\}$) and a large ($\overline{\nu} = \{\overline{\nu}_T, \overline{\nu}_D\} = \{60 \text{ days}, 0.6 \text{ km}\}$) estimation window.

Observations further away from the crime event in terms of time and location are downweighted. In particular, we employ the triangular kernel for $\mathbb{K}_T(u) = \max(0, 1 - |u|)$ and an upweighted triangular kernel for $\mathbb{K}_D(u) = 2\max(0, 1 - |u|)$ so that both integrate to one, in line with McCrary (2008). Let υ_T and υ_D denote the bandwidth with respect to the temporal dimension in days and the spatial dimension in kilometers; then,

$$w(X) = \frac{1}{\upsilon_T \upsilon_D} \mathbb{K}_B\left(\frac{T}{\upsilon_T}\right) \mathbb{K}_D\left(\frac{D}{\upsilon_D}\right) \tag{2}$$

assigns a nonnegative weight to a sale at time T with distance D.

Finally, we introduce a second model to accommodate the effect of property sizes. Similar to Equation (1) it distinguishes between precrime and postcrime observations and reads

$$m^T(X, \boldsymbol{\delta}) = \delta + \delta_T T + \delta_S S + (\Delta + \Delta_T T + \Delta_S S)\,\mathbb{I}(T > 0) \tag{3}$$

We abstain from using locational distance between a crime and a house sale as a controlling covariate. The causal interpretation of δ_D would be difficult. On the one hand, $\delta_D > 0$ in a density estimation can suggest that a crime was anticipated, and potential house buyers avoided moving into the affected area. On the other hand, as shown in Figure 1, crimes in Rochester are committed mostly in the city center, where "one family year-round residences" are sparsely distributed. Consequently, this selection might be driven by confounding factors. Instead, we downweight observations that are further away as described above and leave property size as an explanatory variable that arguably correlates monotonically with socioeconomic factors. To lend credence to our analysis, we provide a placebo estimation where we synthetically create crime events. Location and season effects are controlled for by moving the crime dates one year ahead: $T_c^{placebo} = T_c - 365$.

4.5 The (Lack of) Applicability of RDD

The specifications and issues discussed in subsections 4.3 and 4.4 can be motivated by the RDD approach. In this subsection, we discuss why we cannot conclude that price changes capture the full cost of crime.[13]

RDD identification compares the mean outcome of the control group with the mean outcome of the treated group, yielding the average treatment effect (ATE) (ATE; Hahn, Todd and Van der Klaauw, 2001). This is in line with our estimation specifications (1) so that Δ captures the ATE. RDD identification rests on two assumptions (Compare discussion in Lee, 2008). First, the outcome function must be continuous in the assignment (or running) variable around the threshold that assigns an observation to the control or the treatment group. The running variable in our setup is time, and the threshold is given by the point in time a crime is committed. Second, the distribution function must also be continuous around the threshold. Neither of these assumptions can be tested directly, but both provide testable predictions. In particular, covariates (if present) should have a continuous outcome around the threshold. A failure of this prediction suggests that outcomes are not continuous around a crime event.

[13]The online Appendix explains the (lack of) applicability of an RDD strategy to our setup in detail.

We observe property sizes and the locational distance between crime and real estate transactions. We show and discuss estimates using specification (1) in Table 7 in the Appendix. The size of transacted properties does not significantly change after crime events. This suggests that socioeconomic factors do not play a decisive role in the response to crime. In particular, an increase in property sizes would suggest that owners of larger properties, who are arguably better off and less likely to be liquidity-constrained, are leaving the area. This does not seem to be the case.

In contrast, houses are further away from the location of the crime after shootings and robberies, at least when we allow for a 30-day delay. Such an increase can complement our conclusion regarding the presence of liquidity effects. For example, an increase in the distance measurements in the treatment group (compared to the control group) suggests that fewer houses are sold in the vicinity of a crime, and houses further away are purchased as close but safer substitutes. This corresponds to a market freeze around the location of a crime. However, an increase in the distance can also be attributable to a change in the composition of neighborhoods. Arguably, posh neighborhoods are composed of larger properties so that houses might be more dispersed because house owners flee from crime. Overall, we are unsure about how to interpret this result.

The second testable prediction in Lee (2008) is addressed directly with the density estimations below. Simply put, RDD identification requires that the composition of the ex ante and ex post means do not change. However, density jumps in our data suggest that this composition changes following a crime. Arguably, the ex post mean contains more crime-averse sellers than the ex ante mean. Crime-averse owners want to leave the area after a shooting fire sale, which boosts liquidity. The other types of crime examined in this paper immediately depress liquidity, which suggests that house owners sit out a temporary price slump.

These two explanations with opposing predictions show a potential pitfall that may arise in similar RDD analyses. A failure to detect a discontinuity in the density can result from the two possible responses to crime – fire-selling and sitting-it-out – offsetting each other through different parts of the population. Hence, testing for discontinuity in the density function is necessary if one pursues the RDD identification strategy. However, the density estimates provide a direct economic interpretation by determining the liquidity in the housing market in general, and discontinuities (Δ) show liquidity jumps following a crime in particular. Hence, detecting a nontrivial manipulation of the density around crimes invalidates the causal inference of the price response. At the same time, liquidity exhibits the observed jump.

4.6 Liquidity Estimates

Next, we address the impact of crime on the density of house sales as a measure of market liquidity. As mentioned, this can provide validity to the causal inference of the price effect. A nonrejection of the null hypothesis that the densities do not experience a jump suggests that the assumption according to which the outcome is continuous in the assignment variable around the threshold holds. However, it also yields interpretable insights.

Since we do not observe the density of sales directly, we cannot apply the usual regression techniques. McCrary (2008) suggests to bin observations as a remedy. Counting the frequency of observations in each bin creates a regressand variable usable for estimation. We report point and error estimates as laid out by McCrary (2008) along our tailored results. Otsu, Xu and Matsushita (2013) highlight three issues with the Wald-type test in McCrary (2008) that are relevant in our context. First, the variance estimate depends on functional choices of the weighting function. Second, the confidence interval for Δ in McCrary (2008) is not automatically generated but requires plugins that might not always be reliable. Empirical likelihood, as suggested by Otsu, Xu and Matsushita (2013), or bootstrapping, are remedies to both of these issues. We apply both methods to our binning estimates below. Finally, McCrary (2008) regresses the level of the density for the binning approach, while we use the logarithmic transformation of the density to avoid predictions with negative densities.

Copas (1995), Loader et al. (1996), Hjort and Jones (1996), and Park et al. (2002), among others, developed a likelihood-based approach for local density estimations. This does not require a (global) parametric functional form for the density but is based on a local (linear) approximation, such as Equations (1) or (3). Otsu, Xu and Matsushita (2013) also propose empirical likelihood-based tests for this approach. Empirical likelihood-based tests have several advantages.[14] More importantly, the simulation study in Otsu, Xu and Matsushita (2013) demonstrates finite-sample behavior for the local-linear empirical-likelihood estimator that is superior to the binning estimator, regardless of whether the error inference is based on the bootstrap or the empirical likelihood approach. We present a small simulation study in the online Appendix whose results are in line with the general findings in Otsu, Xu and Matsushita (2013).[15]

Nonetheless, a simpler alternative than binning and local likelihood is a maximum likelihood estimator whose (global) density is described by the estimation window and Equations (1) or (3). The advantage of this approach is the (wider) familiarity with maximum likelihood in the literature and the fact that the global density integrates to one, which, in turn, determines one parameter directly. The disadvantage is that maximum likelihood estimators are generally not robust against misspecifications.

Surprisingly, we found that the maximum likelihood estimator generally outperformed the other three density estimators in terms of (absolute) bias and dispersion of estimates. This applies even when the data generating process provides a misspecification to the estimator. Hence, we report all results. More reliable estimates obtained from empirical-likelihood and maximum likelihood principles are presented in the main text. In contrast, the binning estimates exhibit a much larger degree of uncertainty, prompting us to report the results in Table 6 in the Appendix as part of a wider effort to show negative effects. We employ the

[14]See Otsu, Xu and Matsushita (2013) for a discussion.

[15]There are two minor differences between the estimators proposed by Otsu, Xu and Matsushita (2013) and the one used here. First, Otsu, Xu and Matsushita (2013) only employ and test for a discontinuity in the running variable while we also use distance. Second, and similar to McCrary (2008), Otsu, Xu and Matsushita (2013) who use the level of the density for the binning approach while we employ the logarithmic transformation of the density.

estimates of Δ from the maximum likelihood estimator with a global density described by Equation (3) for our model calibration below. Our preferred estimates are summarized in Table 2.

The first row block in Table 2 summarizes our estimations for shootings. The first row uses only data from the small estimation window ($\underline{\nu} = \{\underline{\nu}_T, \underline{\nu}_D\} = \{30 \text{ days}, 0.3 \text{ km}\}$) centered around the recorded crimes ($\omega = 0$ days) without any delay ($\xi = 0$ days). Shootings are relatively scarce so the small estimation window yields relatively few observations. The empirical likelihood-based estimates are positive but insignificant. The maximum-likelihood estimates become larger and turn significant. The point estimates range from 0.21 – when we do not control for the property size – to 0.33 when we do. Their standard errors suggest that they are significantly different from zero at the 5% level, which leads us to infer that assumption (11) does not hold.[16] This supports our first and second stylized statistical facts: (i) Shootings do affect house sales and, more precisely, (ii) increase their frequency.

The second row (of the first row block concerning shootings) uses the small data window without any delays around synthetic crimes created $\omega = 365$ days before the recorded crimes at their respective locations. All estimates for Δ are insignificant, which lends plausibility to our method.

The third row imposes a delay of 30 days ($\xi = 30$ days). That is, the first 30-day observations after a crime are excluded, and observations in the subsequent 30 days are included. All estimates turn out to be significantly negative, which supports our third stylized fact according to which (iii) violent crimes lead to a market freeze after some time.

The fourth row expands the data window ($\overline{\nu} = \{60 \text{ days}, 0.6 \text{ km}\}$) around recorded crimes ($\omega = 0$ days) without any delay ($\xi = 0$ days). All significant point estimates for Δ from the first row decrease while they remain significant. This supports our last stylized fact that (iv) the effects of crime die out when we expand the estimation windows.

[16] An earlier version of this paper excluded houses that were sold for less than USD 100,000 (rather than houses sold for less than USD 50,000). The point estimates ranged between 0.432 and 0.8436 for the linear model without controlling for property sizes (Equation (1)) using both binning estimators as well as the local-linear empirical likelihood estimator. All estimates were positive and significantly different from zero.

The order of the rows in the following row blocks are organized similarly. We use the small estimation window first, use the synthetic crimes for a placebo test, impose a delay on the recorded (factual) crimes, and enlarge the estimation window.

The second row block in Table 2 summarizes the estimates for assaults. All estimates for the immediate response using the small estimation window are negative and significantly different from zero (stylized fact (i)). Hence, house transactions become less frequent immediately after an assault, which contrasts with our results for shootings where we found that sales immediately increase after a shooting (stylized fact (ii)). None of the estimates around synthetic crimes is significant. A delay of 30 days further depresses liquidity. All point estimates are negative, significant, and larger (in absolute terms, stylized fact (iii)). Finally, all estimates from the immediate small estimation window decrease (in absolute terms) when we expand the estimation window (stylized fact (iv)) in the last row of the assault block.

The third row block reports estimation results for burglaries. The estimates for the immediate Δ are negative and significant in the first row (stylized facts (i) and (ii)). Surprisingly, the estimates around the synthetic crimes are significant, albeit one is positive and the other three are negative. There is no clear interpretation of this result, which states that house sales decline a year before a burglary is committed. Residents might already receive early signals of a deteriorating neighborhood. A delay shows little absolute change in the estimates. Hence, burglaries do not produce an excessive delayed effect (stylized fact (iii)). Finally, expanding the estimation window decreases all estimates in absolute terms when compared to the small estimation window (stylized fact (iv)).

The last row block reports the density responses to robberies. The estimators show an immediate and significant decrease in the density after robbery (stylized facts (i) and (ii)). The synthetic robberies show no irregularities. The delayed effect is milder for the empirical likelihood-based estimators, while the maximum likelihood estimators increase compared to the immediate effect. The larger estimation window again supports our stylized fact that the effects of crimes die out quickly (stylized fact (iv)).

In summary, we find overwhelming evidence that the density of house transactions changes at the moment a crime is committed. This supports our first statistical stylized fact insofar as the densities of house transactions change following a crime event. Shootings significantly increase the frequency of house transactions – liquidity. The other three types of crime lead to fewer sales, with six out-of-nine point estimates being significant. This lends weight to our second statistical stylized fact according to which liquidity can go up or down after crimes. The delayed response to shootings and assaults, the two violent types of crime examined in this paper, is negative, significant, and larger in magnitude than the immediate response. This result supports our third statistical stylized fact. Finally, all immediate estimates for Δ become either smaller (in absolute terms), less significant, or both once we lengthen the data window. This lends support to our fourth statistical stylized fact that measurable effects die out quickly.

From our results, we can also infer that the distribution function is not continuous around the threshold, which invalidates the RDD approach. By extension, our estimates for the response of prices to crimes do not capture the full effect. Furthermore, the opposing responses of liquidity to shootings compared with the other three types of crime explain why a direct estimation strategy can suggest that causal identification of a crime effect on house prices is valid when in fact it is not. The responses from different parts of the population can offset each other. The null hypothesis of no jumps in the transaction density function would not be rejected while the composition of treated and untreated observations changes. Admittedly, the case where different reactions exactly offset each other appears special but can be relevant for small sample sizes.

								Error inference:	Empirical Likelihood		Maximum Likelihood	
								Density approximation:	Local		Global	
								Control for property size:	no	yes	no	yes
	ν_D	ν_B	ξ	ω	$\|I\|$	$\|C\|$	$\|H\|$	Δ_{McCrary}	Δ			
Shootings	0.3	30	0	0	429	268	332	-0.4393	0.0985	0.0986	0.2144**	0.3321**
								(0.5122)	(0.2785)	(0.2784)	(0.0926)	(0.1428)
	0.3	30	0	365	302	211	239	-0.2563	-0.3369	-0.3344	-0.0403	-0.0739
								(0.3787)	(0.3011)	(0.3008)	(0.1217)	(0.2457)
	0.3	30	30	0	403	248	319	0.1616	-0.5527*	-0.5520*	-0.3266***	-0.8966*
								(0.3681)	(0.3134)	(0.3133)	(0.1216)	(0.5231)
	0.6	60	0	0	3463	977	1818	0.0496	0.1176	0.1176	0.0983**	0.1700***
								(0.1093)	(0.0934)	(0.0934)	(0.0403)	(0.0645)
Assaults	0.3	30	0	0	2325	1187	1609	-0.0882	-0.2888***	-0.2889***	-0.1015**	-0.2045**
								(0.1218)	(0.1089)	(0.1089)	(0.0471)	(0.1030)
	0.3	30	0	365	2121	1234	1507	-0.0224	-0.0377	-0.0377	0.0534	0.0941
								(0.1149)	(0.1188)	(0.1188)	(0.0461)	(0.0835)
	0.3	30	30	0	2233	1185	1564	-0.1299	-0.3075***	-0.3078***	-0.1333***	-0.2832**
								(0.1183)	(0.1131)	(0.1130)	(0.0475)	(0.1128)
	0.6	60	0	0	18632	3922	4855	0.0603	-0.1112***	-0.1112***	-0.0317*	-0.0620*
								(0.0437)	(0.0394)	(0.0394)	(0.0164)	(0.0339)
Burglaries	0.3	30	0	0	8281	4069	3873	0.0500	-0.2072***	-0.2072***	-0.1212***	-0.2540***
								(0.0579)	(0.0593)	(0.0593)	(0.0250)	(0.0625)
	0.3	30	0	365	8219	4264	3746	0.0722	2.5159***	-0.1562***	-0.0646***	-0.1291**
								(0.0551)	(0.0361)	(0.0599)	(0.0243)	(0.0563)
	0.3	30	30	0	8157	4065	3859	-0.0185	-0.2061***	-0.2062***	-0.1201***	-0.2608***
								(0.0511)	(0.0597)	(0.0597)	(0.0250)	(0.0600)
	0.6	60	0	0	62128	10602	6839	0.0248	-0.1054***	-0.1054***	-0.0701***	-0.1344***
								(0.0196)	(0.0214)	(0.0214)	(0.0105)	(0.0212)
Robberies	0.3	30	0	0	2730	1120	1854	-0.0074	-0.2436**	-0.2435**	-0.1264***	-0.2683**
								(0.1085)	(0.1020)	(0.1020)	(0.0441)	(0.1075)
	0.3	30	0	365	2516	1173	1765	0.0627	-0.0885	-0.0887	-0.0158	-0.0415
								(0.1027)	(0.1071)	(0.1071)	(0.0273)	(0.0881)
	0.3	30	30	0	2662	1094	1788	0.0322	-0.1942*	-0.1943*	-0.1282***	-0.2809**
								(0.1202)	(0.1046)	(0.1046)	(0.0416)	(0.1100)
	0.6	60	0	0	20459	3258	5128	0.0172	-0.0816**	-0.0817**	-0.0437***	-0.0936***
								(0.0361)	(0.0377)	(0.0377)	(0.0137)	(0.0341)

Table 2: Estimates for density jumps around four types of crime. The first row in each row block uses the small data window centered around crimes. The second row uses a small data window but centered around synthetic crime events created $\omega = 365$ days prior to recorded crimes. The third row imposes a delay of $\xi = 30$ days after a factual crime. The fourth row uses the large data window centered around factual crimes. $|C|$ ($|H|$) refers to the number of crimes (houses) involved that yield the $|I|$ observation used in the estimation. The estimates (with standard errors in parentheses beneath them) refer to the jump coefficient Δ. Estimates of the other coefficients are available upon request. *, **, and *** indicate that the value is different from zero with 10%, 5%, and 1% probability, respectively.

4.7 Price Estimates

Finally, this subsection discusses the price response estimations. We applied a simple local linear estimator as discussed in Imbens and Lemieux (2008) and used both approximation models, (1) and (3). Furthermore, we downweighted observations using Equation (2). Reported standard errors follow conventional arguments of asymptotic inference. The estimates are subject to significant noise, which is remindful of the motto that real estate is about "location, location, location", leading us to employ the partly linear estimator proposed by Robinson (1988),

$$P_i = X_i \delta + n(L_i) + \epsilon_i \tag{4}$$

where the linear part $X_i \delta$ contains a linear trend around the crime as well as a change in the intercept and the time trend for $T_i > 0$[17] as well as yearly and quarterly dummies. The non-linear Part $n(L_i)$ employs the geospatial information. The assumption on the error term is $\mathbb{E}[\epsilon_i | X_i, L_i] = 0$ and we allow for heteroscedasticity $\mathbb{E}[\epsilon_i | X_i = x, L_i = l] = \sigma^2(x, l)$. The conditional expectations of a variable z before the crime associated with observation i occurred is defined by $g_z(l) = \mathbb{E}_{i,T_i<0}[z | L_i = l]$. Subtracting the conditional expectations from Equation (4) yields

$$P_i - g_P(L_i) = (X_i - g_X(L_i))\,\delta + \mu_i + \epsilon_i \tag{5}$$

where $\mu_i = n(L_i) - \mathbb{E}_{i,T_i<0}[n(L_i) | L_i]$ is the innovation to a price index attributable to location L_i. The transformed Equation (5) immediately suggests an infeasible (linear) estimator for δ. Feasibility is achieved by casting μ_i into the residual term and using the Nadaraya-Watson non-parametric estimator for $g_x(l)$.[18]

[17] The estimator does not allow for a constant.

[18] It is suggested to trim observations with a smaller density. Furthermore, the bandwidths are determined by the estimation windows. Reported standard errors are robust. The function $g_x(L_i)$ uses all house transaction observations in our real estate dataset, subject to the spatial distance of house i and transactions needed to be conducted before the crime occurred. The average number of houses K can be found in Table 3.

A critical element for the inference of Δ is that μ_i is uncorrelated with the innovations in the regressor variable in location L_i, $\mathbb{I}(T_i > 0) - g_{\mathbb{I}(T_i>0)}(L_i)$, where $g_{\mathbb{I}(T_i>0)}(L_i)$ controls for the expected crime at location L_i. Hence, μ_i represents postcrime changes in house prices that we attribute to location L_i. Arguably, these changes are small given the relatively short estimation windows. Furthermore, time dummies in X_i accommodate seasonality and long-run trends. The advantage of introducing the nonlinear function $n(L_i)$ is that location summarizes housing valuation quite well, not just because of locational amenities, such as tax regimes and the quality of schools but also because houses in a similar neighborhood have similar intrinsic characteristics.

In sum, we obtained a couple of interesting results. Only four of the 48 estimates of Δ using either Model (1) or (3) are significantly different from zero. Note that Model (3) explicitly controls for property size. Only nine estimations have less than 1,000 observations, while 15 of the non-placebo estimations have more than 8,000 observations. All significant estimates are linked to changes in the total house sale price, and two imply a jump in prices around shootings. One estimate is positive in the first row, suggesting a price increase, while the other is negative and rather large, implying an average price decrease of approximately 98%. We conclude that house prices are subject to unobserved heterogeneity.

The picture changes dramatically when we employ specification (4). Only two of the 18 nonplacebo point estimates for assaults, burglaries, and robberies are not significant. The 16 significant estimates are all negative and of reasonable size, ranging from 5.2% to 10.3% price drops. In contrast, estimates around shootings are all insignificant. We attribute this to the small number of shootings in our sample and do not interpret this result as evidence that shootings do not impinge on real estate prices. We conclude from these results that our point estimates of price responses using the partly linear model described in (4) are relatively precise and reliable.

	ν_D	ν_B	ξ	ω	Explaining: $\lvert I \rvert$	Model K	Price (1) Δ	Price (3) Δ	Price (4) Δ	Price/m^2 (1) Δ	Price/m^2 (4) Δ
Shootings	0.3	30	0	0	429	61.2	0.2285*	-4.1517**	0.0668	0.1238	-0.0044
							(0.1358)	(1.8680)	(0.0423)	(0.1543)	(0.0548)
	0.3	30	0	365	302	36.3	-0.2551	1.4605	-0.0065	-0.0743	-0.0641
							(0.2252)	(1.0936)	(0.0505)	(0.2939)	(0.0668)
	0.3	30	30	0	403	66.7	-0.0259	0.0579	0.0335	-0.0810	-0.0515
							(0.0999)	(1.0817)	(0.0490)	(0.1501)	(0.0719)
	0.6	60	0	0	3463	124.3	0.0781	0.4942	-0.0141	0.0658	-0.0345
							(0.0529)	(0.6173)	(0.0163)	(0.0622)	(0.0229)
Assault	0.3	30	0	0	2325	126.6	-0.0679	-0.7455	-0.0733***	-0.0876	-0.0322
							(0.0678)	(0.8149)	(0.0246)	(0.0865)	(0.0326)
	0.3	30	0	365	2121	74.4	-0.0672	0.2168	-0.0014	-0.0853	0.0401
							(0.0699)	(0.8376)	(0.0266)	(0.0920)	(0.0351)
	0.3	30	30	0	2233	119.8	0.1109	-0.5326	-0.0929***	-0.0293	-0.0680**
							(0.0845)	(0.9450)	(0.0251)	(0.0979)	(0.0342)
	0.6	60	0	0	18632	260.8	-0.0139	-0.1088	-0.0885***	-0.0065	-0.0798***
							(0.0246)	(0.2769)	(0.0095)	(0.0310)	(0.0123)
Burglary	0.3	30	0	0	8281	121.5	-0.0705*	0.0355	-0.0643***	-0.0228	-0.0218
							(0.0377)	(0.4093)	(0.0123)	(0.0497)	(0.0166)
	0.3	30	0	365	8219	79.4	0.0531	0.3041	-0.0080	0.0135	-0.0155
							(0.0344)	(0.3537)	(0.0123)	(0.0446)	(0.0160)
	0.3	30	30	0	8157	119.9	-0.0013	0.1870	-0.0614***	0.0257	-0.0511***
							(0.0439)	(0.4331)	(0.0130)	(0.0518)	(0.0167)
	0.6	60	0	0	62128	250.9	-0.0191	-0.1281	-0.0569***	-0.0190	-0.0638***
							(0.0132)	(0.1504)	(0.0051)	(0.0168)	(0.0065)
Robbery	0.3	30	0	0	2730	188.2	-0.0850	-0.9345	-0.1091***	-0.1212	-0.0536*
							(0.0700)	(0.7139)	(0.0253)	(0.0823)	(0.0311)
	0.3	30	0	365	2516	125.5	-0.0057	-0.2568	-0.0200	-0.0724	-0.0312
							(0.0764)	(0.6540)	(0.0263)	(0.0881)	(0.0308)
	0.3	30	30	0	2662	182.5	0.0263	-1.2597	-0.1063***	-0.0181	-0.0528*
							(0.0914)	(0.9599)	(0.0261)	(0.0832)	(0.0317)
	0.6	60	0	0	20459	316.2	-0.0455*	-0.1413	-0.0842***	-0.0347	-0.0556***
							(0.0251)	(0.2679)	(0.0097)	(0.0304)	(0.0121)

Table 3: Estimates for jumps in the (log-) sales price and the (log-) price per square meter around crime events. The four row blocks correspond to the different types of crime. The first row in each row block uses the small data window centered around crimes. The second row uses a small data window but centered around synthetic crime events created $\omega = 365$ days prior to recorded crimes. The third row imposes a delay of $\xi = 30$ days after a factual crime. The fourth row uses the large data window centered around factual crimes. The numbers of observations are identical to those in Table 2. The estimates (with heteroskedasticity-robust standard errors in parentheses beneath them) refer to the jump coefficient Δ. Estimates of the other coefficients are available upon request. *, **, and *** indicate that the value is different from zero with 10%, 5%, and 1% probability, respectively.

5 A Structural Model

This section aims to elicit the effects of crime on welfare using a structural model that is fitted to the data points from the previous section. We motivate the use of a structural model as follows. First, price responses do not capture the full effect of crimes. Rather, crimes also elicit quantity, that is, liquidity, responses. Second, the question arises how a policy maker can interpret two-dimensional responses. A structural approach collapses the price and quantity dimensions into a single dimension. To this end, we develop in subsection 5.1 a dynamic model that rationalizes our empirical results and enables us to draw consolidated answers about the welfare costs of crime. In subsection 5.2, we calibrate the model to estimated moments and discuss the findings in subsection 5.3.

The intuition for the model is as follows: Assume that house owners are well informed about the current (and persistent) criminal state of their neighborhood. The majority of buyers are out-of-towners and receive information only with delay. As a result, sellers accept offers from buyers they would have rejected had a crime not been committed that boosts the market's liquidity. Transactions decrease as buyers gradually learn about the level of criminality in the neighborhood. This scenario replicates the empirical pattern around shootings well. In contrast, if the fraction of (potential) buyers consisting of local, well-informed residents is large enough, the price offers adjust immediately downward, and some residents avoid the area altogether. House owners sit out the valuation slump and wait for prices to recover. As out-of-towners also gradually learn about the true state of the neighborhood, the initial market freeze can be exacerbated. This scenario replicates the empirical pattern around assaults, burglaries, and robberies.

The initial response to a switch to a high-crime state is determined by the fraction of well-informed local home buyers. Well-informed buyers immediately adjust their price offers, while out-of-towners only learn about the current crime state through local news. Sellers can take advantage of out-of-towners' lack of knowledge by accepting their offers and leaving the area. This leads to a fire-sale phenomenon. The immediate adjustment of the price offers by local buyers, on the other hand, leads to a market freeze.

We propose local news as the transmission channel to explain our statistical findings. Local news is not the only way information about crime is spread but plays a prominent role in the media landscape. Local television news is dominated by crime (Klite, Bardwell and Salzman, 1997), and viewing it raises the perceived risk of crime (Romer, Jamieson and Aday, 2003). Specific to our context, the Pew Research Center (2020) reports that 43% of adults in Rochester, NY, obtain their local news via television, and 39% report that news about local crime is important to their daily life, ranking second after the weather.

Our explanation for the different liquidity responses to different crimes is based on information asymmetries in the real estate market. Kurlat and Stroebel (2015) detect a positive correlation between the informedness among buyers, proxied by the fraction of real estate agents and individuals who moved to a neighborhood from a nearby location, and subsequent price increases.

One objection to our setup could be that house sales pick up after shootings because home-owners accept standing offers that they received before a crime had been committed. However, this does not fit our data when the frequency drops. This also applies to the three other crimes examined in this paper. One could argue that the buyer, after receiving an offer from the home-owner before a crime, rejects the offer to submit a lower counteroffer. However, this would suggest that the type of crime coincides with whom holds an offer, the buyer or the seller. We deem this unlikely.

5.1 Search-and-Match Model

We describe in this subsection a search-and-match model we developed with the aim of rationalizing the empirical estimates found in section 4. Time is continuous, continuous forever, and denoted by t. Agents discount the future with the rate ρ. There is a unit mass of house owners who sell their property at the “right price” and a unit mass of (potential) buyers.

The economy is characterized by two different states that determine whether crimes affect residents or not. The true state at time t is denoted by c_t. $c_t = h$ is the high-crime state, and $c_t = l$ is the low-crime state. All residents are subject to disutility if $c_t = h$, but their experiences vary. Each agent possesses an idiosyncratic and time-invariant measure of crime aversion denoted b for buyer and s for seller. Both b and s are drawn from an exponential distribution with parameter σ. This disutility captures the direct cost residents face in a high-crime state. The switch between the two different states is governed by Poisson processes. A low-crime state switches to a high-crime state with intensity θ_h, while a high-crime state switches to a low-crime state with intensity θ_l. To keep the distribution of b and s stationary, we let buyers leave the game after making an offer, regardless of whether they purchased a house or not. Sellers, on the other hand, only leave after a sale and are replaced so that the economy remains stationary. In other words, while sellers can wait for a better offer, buyers have the outside option to move to a different area that we normalize to zero. This leads sellers to pursue a reservation price strategy. That is, buyers submit their valuation privately to the homeowner whenever they match according to a Poisson process with intensity λ. Transaction p creates a linear cost to the buyer and utility to the seller. Hence, the payment good corresponds to the log-price estimates discussed above.

We further distinguish between informed and uninformed buyers of whom there are η and $1 - \eta$, respectively. Local buyers (and sellers) have better information about their neighborhood than out-of-town buyers. Residents are the collective term for sellers who have not yet sold their houses, buyers who purchased a house in the past, and local potential buyers who have not yet purchased a house. Residents are perfectly informed about the crime state. Out-of-town buyers believe the true crime state is high with probability x_t and low with probability $1 - x_t$. They learn about the true state through news that arrives according to a Poisson process with intensity μ. This leads the state variable x_t to evolve as summarized by the equation

$$\dot{x}_t = \begin{cases} -x_t\mu & \text{if } c_t = l \\ (1-x_t)\,\mu & \text{if } c_t = h \end{cases} \tag{6}$$

There is no other way with which buyers update their beliefs. In particular, we assume buyers make private one-shot offers when submitting a price that equals their reservation value. This assumption implies that buyers cannot learn from posted prices or from the rejection of their previous offers.

Possessing a house yields u units of utility per period to a seller and u_b utility to a (successful) buyer. We ensure gains from trade by imposing $u_b > u$. The valuation of a home buyer in a low-crime or high-crime state is therefore

$$\rho W_l\,(b) = u_b + \theta_h\,(W_h\,(b) - W_l\,(b)) \tag{7}$$

$$\rho W_h\,(b) = u_b - b + \theta_l\,(W_l\,(b) - W_h\,(b)) \tag{8}$$

respectively. Note that buyers do not have a resell option. The values W_l and W_h are determined by their respective flow payoffs u_b and $u_b - b$ and their likelihood and valuation difference to switch to the other state. The valuations by out-of-town buyers are a weighted average of these two values $W_x\,(b) = xW_h\,(b) + (1-x)\,W_l\,(b)$ because crime aversion is linear. Buyers offer their reservation value but can always opt out so that $p_i\,(b) = \max\{W_i\,(b)\,, 0\}$.

The valuation of a homeowner depends on the utility of home ownership, the disutility from crime, and two additional events. First, as described above, the crime state changes with intensities θ_l and θ_h. Second, buyers present a purchasing offer that arrives with intensity λ dependent on their crime aversion and the information at hand. For example, if the current crime state is low and the aggregate belief of out-of-town buyers is x, a seller's valuation with crime aversion s for the two events is given by

$$Q_l(s,x) = \frac{\theta_h}{\theta_h+\lambda} V_h(s,x) + \frac{\lambda}{\theta_h+\lambda}\eta \int \max\{V_l(s,x), p_l(b)\} dF(b) + \frac{\lambda}{\theta_h+\lambda}(1-\eta) \int \max\{V_l(s,x), p_x(b)\} dF(b) \tag{9}$$

where $V_h(s,x)$ is the continuation value when the crime state switches to "high". This occurs with probability $\theta_h/(\theta_h+\lambda)$ given that an event occurred. With complementary probability $\lambda/(\theta_h+\lambda)$, the homeowner receives a purchasing offer. With probability η, this offer comes from a local buyer who (correctly) believes the current economy is in a low-crime state. With probability $1-\eta$, the offer comes in from an out-of-town buyer whose valuation is $W_x(b)$. In either case, the purchasing offer $p_i(b) = W_i(b)$ for $i \in \{l, x\}$ depends on the buyer's parameter of crime aversion. The homeowner will accept the offer if and only if it represents a better value than continuing to live in the house given the true crime state $c_t = l$. A similar explanation applies to the event valuation of a homeowner with crime aversion s in a high-crime economy.

The (indirect) utility of a homeowner with crime aversion s in the high-crime state with belief x by out-of-town buyers is

$$V_h(s,x) = \int_0^\infty (\theta_l+\lambda) e^{-(\theta_l+\lambda)t} \left[\int_0^t e^{-\rho\tau}(u-s)\, d\tau + e^{-\rho t} Q_h(s, x(t))\right] dt$$

We motivate this expression by starting with the terms inside the square brackets. First, the homeowner enjoys the house but suffers from crime, $u-s$, until the next (joint) event arrives at time t, and the flow utility is discounted continuously by $e^{-\rho\tau}$. The next switch (to a low crime state) or price offer is valued $Q_h(s, x(t))$ and is realized according to a Poisson process with intensity $\theta_l+\lambda$. Note that the aggregate belief x is subject to the learning process described above.

What determines trading outcomes? Sellers have three advantages: (i) they remain in the game and can wait for a higher offer, (ii) have better information than out-of-town buyers, and (iii) know their crime aversion, whereas buyers submit their reservation values.

5.2 Calibration

Next, we calibrate the model laid out in the previous subsection to the estimates documented above, which allows us to extract the factual welfare costs caused by crime. In addition, the model calibration makes it possible to calculate counterfactual scenarios. We provide the results for these counter-factual calculations in the Appendix to this paper. We target several moments given by our estimates using the parameters $\{u, u_b, \sigma, \mu, \eta, \lambda, \theta_l, \theta_h, \rho\}$. To reduce the number of parameters, we set $\rho = 0.01$ as an approximation to a yearly discount rate. Unfortunately, the literature does not provide more guidance on this issue.

With the aim of increasing the number of moments, we also target the average (log-) PPSM available in our data. We also divided the total number of house transactions by the average number of housing units (1 unit, detached) as reported by the census, which yielded a probability of selling a house over the course of a year of approximately 4%.

The estimation based on (log-) sale prices yielded implausible (but also insignificant) positive responses to shootings. The calibration might not succeed, as a large deviation from the targeted price-response estimates could compromise other targeted estimates as well. Although the estimates for the postshooting responses in the (log-) PPSM are similarly imprecise as (log-) sale prices, they are in a more reasonable range when compared to the other crimes. Furthermore, we target immediate and delayed price responses in the small estimation window. All responses in the (log-) PPSM to assaults, burglaries, and robberies in Table 3 are smaller (in absolute terms) than the respective responses in the (log-) sale prices. Hence, we err on the side of caution eliciting the cost of crime when we calibrate the model moments to the smaller price responses.

The price and liquidity responses are determined as the difference measured between a point of time $t = 0$ when the economy is in a low crime state ($c_t = l$) and all out-of-towners (correctly) believe they are about to purchase a house in a low crime economy ($x_t = 0$). We then transformed the immediate and delayed price and liquidity responses to capture the percentage deviation after a crime. Table 4 summarizes the results from our calibration.

PARAMETERS	**Shootings**	**Assaults**	**Burglaries**	**Robberies**
u **(homeowner)**	0.0509	0.0509	0.0506	0.0506
u_b **(buyer)**	0.0509	0.0510	0.0507	0.0511
σ **(crime aversion)**	396.3	5.2	3.7	2.7
μ **(learning)**	34.10	3.51	658.95	0.17
θ_h **(switch to high crime)**	0.000001	0.000161	0.000559	0.000754
θ_l **(switch to low crime)**	0.000001	1.406300	4.493762	2.658492
η **(locals)**	0.0000	0.7417	0.8798	0.8697
λ **(offer arrival)**	0.1327	0.0419	0.0642	0.0488

MOMENTS		$\boldsymbol{\xi}$	**Data**	**Model**	**Data**	**Model**	**Data**	**Model**	**Data**	**Model**
Frequency	Level		4.03	8.22	4.03	3.24	4.03	4.28	4.03	3.77
	Δ (in %)	0	33.21	32.30	-20.45	-20.56	-25.40	-25.19	-26.83	-27.38
	Δ (in %)	30	-89.66	-88.42	-28.32	-28.40	-26.08	-26.23	-28.09	-27.55
Log-price per square meter	Level		5.04	5.04	5.04	5.04	5.04	5.04	5.04	5.04
	Δ (in %)	0	-0.44	-1.82	-3.22	-5.02	-2.18	-2.90	-5.36	-5.90
	Δ (in %)	30	-5.15	-4.61	-6.80	-5.67	-5.11	-2.93	-5.28	-5.92

Table 4: Calibration results. The top block reports the calibrated parameters, while the bottom block shows the targeted and predicted moments.

While all parameters have an effect on all computed moments, we can establish some first-order relationships. First, u and u_b jointly determine the average housing valuation, and the difference between u and u_b determines the probability a house is sold in each meeting. The offer arrival parameter λ determines the frequency of house sales. The fraction of locals η determines the immediate liquidity response, while the persistence parameters θ_l and θ_h as well as the learning parameter μ determine the delayed responses.

The calibration fits the targeted moments rather well for a model that allows for asymmetric responses. The qualitative differences in the model's responses stem from the size of η, the fraction of informed buyers. The immediate and delayed liquidity effects as well as the steady state prices are close fits to the targeted moments.

However, the calibration presents some caveats. First, the model predicts a sales volume that deviates from the data. In particular, the shooting calibration yields more sales, while assaults and robberies exhibit fewer sales. Second, we use six moments to calibrate a nonlinear model with eight parameters.[19] Unfortunately, we lack extra moments, so we are unable to compare outcomes that were not targeted. Third, the statistical estimates for the self-exciting effect suggest that crimes are (mostly) self-depressing (compare Table 8 in the Appendix). Our calibration allows for such quick reversals explicitly. In particular, the estimates would suggest a large θ_l. The calibrated model suggests that high-crime states are quite persistent, particularly for shootings. Again, we emphasize that disutility does not necessarily arise from being a victim of a crime but solely from the fear of becoming a victim. Finally, the average degree of crime aversion, $1/\sigma$, for shootings is much smaller (≈ 0.0025) than for assaults (≈ 0.1923), burglaries (≈ 0.2703), and robberies (≈ 0.3704). However, this is offset by the much longer expected time until the economy switches back to a low crime state. We discuss the welfare implications below.

These caveats notwithstanding, several plausible features of the results speak in favor of our calibration. Most calibrated moments closely capture the empirical moments. All frequency responses match the targeted moments quite well. In particular, the shooting calibration exhibits an immediate increase, followed by a large market freeze after 30 days. Some price responses also match the data quite well even when the estimates show some variability between the immediate and delayed responses.

5.3 Model Dynamics and Welfare Implications

Table 5 summarizes the model dynamics and the welfare calculations for the factual calibration.

[19] One could calibrate the model for all scenarios simultaneously, leaving crime-irrelevant parameters $\{u, u_b, \lambda\}$ constant across crime types. However, these parameters are already very close, and a joint calibration is very time-consuming.

FACTUAL CALIBRATION		ξ	Shootings	Assaults	Burglaries	Robberies
Buyers with high beliefs (in %)	All	0	8.92	74.42	98.02	86.98
		30	93.94	80.65	100.00	87.15
Meetings resulting in sale (in %)	All	pc	61.93	77.33	66.60	77.28
	Out-of-towners	0	85.55	98.89	55.67	98.09
		30	25.58	80.48	51.24	97.31
	Locals	0	24.29	50.45	51.24	52.88
		30	24.32	50.45	51.24	52.89
Price changes	Out-of-towners	0	-1.82%/-1,398$	-0.22%/-192$	-2.67%/-2,461$	-0.41%/-393$
		30	-4.61%/-3,546$	-2.74%/-2,425$	-2.93%/-2,693$	-0.58%/-548$
	Locals	0	-4.65%/-3,571$	-6.69%/-5,918$	-2.93%/-2,693$	-6.72%/-6,374$
		30	-4.65%/-3,576$	-6.69%/-5,919$	-2.93%/-2,693$	-6.72%/-6,374$
Welfare effects	Total	0	-7.46%/-5,733$	-13.41%/-11,855$	-5.93%/-5,457$	-13.85%/-13,132$
	Housing	0	-25.23%/-19,393$	-13.61%/-12,038$	-5.97%/-5,496$	-13.98%/-13,261$
	Resale	0	17.77%/13,660$	0.21%/182$	0.04%/38$	0.14%/129$
	Ex-post	0	-15.88%/-12,205$	-13.06%/-11,553$	-0.29%/-267$	-13.11%/-12,438$

Table 5: Results from the welfare analysis. Note that $\xi =$ pc refers to precrime results when out-of-towners believe $x = 0$.

5.3.1 Model Dynamics

The first block of Table 5 displays the fraction of buyers who believe the economy is in a high-crime state immediately after the economy switches to a high crime state ($\xi = 0$) after a 30-day delay (ξ). Remember that all out-of-towners believe the economy was in a low-crime state ($x_0 = 0$). The fraction of buyers with high-crime beliefs is rather low for the shooting calibration, as out-of-towners dominate the population of buyers. In contrast, assaults, burglaries, and robberies start on a higher level of informedness about the true state of the economy. After 30 days, the majority of buyers are well informed, and in fact, more buyers know about the high-crime state in the shooting calibration (94%) than for robberies (87%) and assaults (81%).

The second block documents that the asymmetric responses derive from the number of informed buyers. In the precrime period ($\xi = pc$), the majority of meetings between a house owner and a potential buyer lead to a house sale. However, there is a stark difference between out-of-towners and local buyers immediately after a crime ($\xi = 0$). In all but one calibration, the rate of success in these meetings increases. Largely uninformed out-of-towners submit relatively high offers. Only in the burglary calibration do we observe a decline in the success rate, which we attribute to the steep learning curve. The average time until an out-of-towner changes his or her belief from a low-crime state to a high-crime state is only approximately half a day ($1/\mu * 365 \approx 0.55$). This contrasts with locals who adjust their offers according to the true state, which leads to a universal decline in the success rate immediately after the economy switches from a low-crime to a high-crime state ($\xi = 0$). In fact, the success rate remains relatively stable for local buyers ($\xi = 30$), while the analog number for out-of-towners decreases as they learn about the true state of the economy.

The third block complements our story with pricing dynamics. The offers made by out-of-towners drop slightly immediately after a crime, $\xi = 0$. The lower bound for these price drops is given by those we observe for locals. The relative size of price drops by out-of-towners is determined by the learning process and crime aversion. It is, for example, largest in the burglary calibration. While the price change (compared to the precrime offers) remains largely unchanged in the local population after 30 days, the price offers made by out-of-towners continue to decline.

5.3.2 Welfare in Calibrated Model

The fourth block of Table 5 documents the welfare effects from the calibrated model. We emphasize two elements. First, we dissect the immediate welfare effects for homeowners into two components, the quality of life and the resale option value. Second, we document the cost of disappointment an uninformed buyer experiences when purchasing a house in a high-crime state. The first line, labeled "total", shows that crime uniformly depresses the indirect utility that homeowners derive from their property. We define this measure by the expected difference between a homeowner in a high-crime and a low-crime state, $\mathbb{E}_s \left[V_h(s) - V_l(s)\right]$. Surprisingly, the welfare effect is not largest for shootings. The highest cost arises from robberies, amounting to approximately 14% of a house value, or a monetary value of USD

13,100 for an average house in the vicinity of a robbery (compare Table 1). This is closely followed by the loss attributable to an assault that costs the homeowner approximately 13% of a house's value (monetary value USD 11,900). Shootings "only" cost approximately 7% (monetary value USD 5,700). The smallest effect is caused by burglaries with approximately 6% of house value (monetary value USD 5,500).

This total welfare effect is a composition of two underlying valuation changes. First, living in a neighborhood after a crime is less enjoyable; residents suffer from fear and anxiety. Second, a house also loosens in terms of its resale option value, which is affected by the search friction of the real estate market. With the onset of a high-crime state, the reservation prices for homeowners shift accordingly so that market value becomes a substitute for retaining property. We next dissect and discuss these elements.

From the second line of the welfare block, labeled "housing", we can infer that crime uniformly depresses the residential experience, which we define as the difference between a local living in a high-crime state and a low-crime state, $\mathbb{E}_b \left[W_h\left(b\right) - W_l\left(b\right)\right]$.[20] The loss is steepest for shootings with a welfare loss equivalent to approximately 25% of the house value (monetary value USD 19,400). Both assaults and robberies give rise to a welfare loss of approximately 14% of house value, while the monetary value is slightly higher for robberies (monetary value USD 13,300) than for assaults (monetary value USD 12,000). Finally, the loss in quality of life after a burglary amounts to approximately 6% of house value (monetary value USD 5,500).

The third line captures the role of the real estate market in the allocation of crime disutility. We determine this resale option measure as the difference between the total and housing welfare effects, $\mathbb{E}_s \left[V_h\left(s\right) - V_l\left(s\right)\right] - \mathbb{E}_b \left[W_h\left(b\right) - W_l\left(b\right)\right]$. Noticeably, the liquidity influx after shootings shifts the crime disutility to uninformed out-of-towners who are unaware of the current crime state. Hence, the fundamental welfare loss attributable to the change in the residential experience just discussed is absorbed by a reallocation of the housing unit. The

[20]Note that the residential utility cancels.

absorption value is quite large and amounts to approximately 18% for shootings (monetary value USD 13,700). The other types of crime are not accompanied by the same resale option value to homeowners. Here, most potential home buyers are well informed about the state of the economy so that prices reflect a fair valuation. Hence, the offset in the resale option value is largely attributable to the gains from trade.

The final line in the welfare block, labeled "ex post", highlights the disappointment an average out-of-town buyer experiences from bidding for a house immediately after a crime has been committed. The buyer realizes he or she bought a house in a high-crime neighborhood. The buyer's offer succeeds if the ex ante valuation ($W_{x_0}(b)$) is larger than the homeowner's reservation value ($V_h(s, x_0)$). However, the out-of-town buyer now becomes aware of living in a high-crime state $W_h(b)$ which is lower than the expected utility derived from living in the new house.[21] The disappointment is largest in the case of shootings, which lead to a loss of approximately 16% of a house value (monetary value USD 12,200). It does not equate the gains from the resale option a homeowner has because, by assumption, buyers derive a higher residential utility than home-owners, $u_b > u$. Disappointment is also high for assaults and robberies, amounting to approximately 13% of a house value in both cases, but there are fewer uninformed offers in both calibrations. Losses are smallest for burglaries, which we attribute to the shorter average duration an agent remains in the high-crime state.

5.4 Comparison with Other Studies

How do our estimates compare to others reported in the literature? One of the first empirical studies that analyzed the effect of crime on real estate was by Thaler (1978). Thaler (1978) also applied a hedonic-pricing model to data from Rochester, New York, and came up with

[21]Hence, the formula is

$$\int\int \left(W_h(b) - W_{x_0}(b)\right) \mathbb{I}\left[W_{x_0}(b) \geq V_h(s, x_0)\right] dF(s)\, dF(b)$$

where $\mathbb{I}[A]$ is the indicator function, which is equal to 1 if statement A is true and zero otherwise. This is remindful of the "winner's curse" in common-value auctions.

an average cost of property crime of USD 2,560. Anderson (1999) provided a comprehensive estimation of the annual burden of all crimes in the U.S. of more than USD 1 trillion, while Lynch and Rasmussen (2001) estimated that high crime areas in Jacksonville, Florida, had real estate prices discounted by up to 40 percent (or USD 50,000).

An aggregation of estimates requires some care. Simply multiplying the cost per crime by the number of crimes and the number of houses is misleading, as crimes only affect houses in their direct vicinity. Aggregation has further implications. For example, Tita, Petras and Greenbaum (2006) estimated both the effects of the level of criminal activity and changes to that level on average house prices for whole census tracts in Columbus, Ohio[22], from 1994 to 1998. To compare our results with those of Tita, Petras and Greenbaum (2006), we use their data, holding constant the socioeconomic and physical characteristics of the origin of the estimate. Specifically, we calculate the total cost of a single crime as follows: An additional violent (property) crime lowers the average house price by 0.163% (0.009%) for an average census tract with 3,848 inhabitants.[23] The average household has 2.64 members according to the U.S. Census, so that a census tract has approximately 1.457 houses, which is the number of houses affected by an average crime in Tita, Petras and Greenbaum (2006). The FBI reports 1,206,836 violent (7,196,045 property) crimes in 2018 and the Federal Reserve of St. Louis reports a median house price of approximately USD 325,000. This sums up to a total annual cost of violent crimes of USD 931 billion and for property crime of USD 307 billion.

How does this compare with our aggregate welfare cost? We register a total of 19,148 housing transactions with an average price of USD118,600 for 16,418 unique houses. The U.S. Census, on the other hand, documents an average of 43,684 detached single-unit houses between 2010 and 2017. The factor of total houses over houses in our dataset is 2.66. We calculated the weighted number of houses in the vicinity of an average crime by $\frac{2.66}{C}\sum_{c=1}^{C}\sum_{h=1}^{H}\frac{1}{v_D}\mathbb{K}_D\left(\frac{D}{v_D}\right)$.

[22]Columbus is the capital of a midwestern state with just below 900,000 inhabitants.

[23]An aggregation would also require knowing changes of violent and property crimes on a very granular level. We only highlight the level effect here and ignore the change effect.

This metric provides us with the number of houses that are affected by an average crime, while it also downweights observations that are further away as they are less affected, in accordance with our estimation approach. Hence, the number of affected houses is 0.83 for shootings, 1.25 for assaults, 1.93 for burglaries, and 1.78 for robberies. Shootings occur in comparatively sparse regions, while burglaries are perpetrated in more densely populated areas. The average number of crimes per year is 195 for shootings, 915 for assaults, 1,967 for burglaries, and 697 for robberies. Multiplying these numbers together with the welfare loss in the housing utility and the average house price in our sample provides us with a back-of-the-envelope estimate for the annual cost for Rochester city residents. Accordingly, residents lose a monetary value of approximately USD 4.9 million attributable to shootings, USD 18.5 million to assaults, USD 26.9 million to burglaries, and USD 20.6 million to robberies. Finally, an aggregation to the U.S. as a whole using the population count ratio suggests an annual welfare cost of approximately USD 419 billion due to shootings, USD 1,599 billion from assaults, USD 2,324 billion from burglaries, and USD 1,775 billion from robberies. Note that these estimates are rather conservative, as we use the price level in Rochester, NY, which is considerably lower than the US-wide level, as mentioned above. Our estimates of the welfare costs caused just by burglaries or robberies alone are much higher than the total cost of property crimes based on the data used by Tita, Petras and Greenbaum (2006). Furthermore, the cost of assaults also far exceeds the numbers reported by Tita, Petras and Greenbaum (2006). Only shootings remain inside the estimate of violent crimes found in Tita, Petras and Greenbaum (2006). Our estimates are also higher than the burden of all crimes found by Anderson (1999).

6 Conclusions

Crime and the fear it generates are important determinants of individual welfare. Crime causes much pain in society – physical, psychological and pecuniary. One area where the real estate market – of which (il)liquidity is just a reminder – represent the main inhibitor for moving to a safer neighborhood. Learning about the willingness to move highlighted by reduced housing prices is only one part. The other important measurement is the degree to which crime affects liquidity. This is the main motivation for this study.

In detail, we contribute to the literature along various dimensions. First, to establish directional identification, we consider the housing market shortly before a crime is reported as a control and treated observations thereafter. Second, we broaden the scope beyond the price impact of crime on the housing market on which previous work has dwelled by estimating the liquidity effects of crime. Third, we employ geospatial data that account for the granularity of housing markets. Fourth, we account for delayed responses to crime. Fifth, while RDD motivates our econometric approach, we show why this setup does not warrant causal inference. Sixth, we propose a structural model that replicates our empirical estimates and, unlike direct estimates, provides meaningful and interpretable conclusions about residential welfare resulting from crime. Finally, we study the impact of four different types of crime – shootings, assaults, burglaries, and robberies.

Five results arise from the analysis. First, the main novel contribution we provide to the existing literature is that price adjustments are not the most important indicator of how crime affects real estate. The primary response to crimes is instead observed in transactions' frequency – the density of house sales – which we interpret as liquidity.

Second, a technical implication of the evidence that housing responds mainly through transaction frequencies and not prices invalidates the applicability of an RDD approach, however appealing it may be. Our estimations show that the liquidity responses for shootings are opposite to those from the other three types of crimes, leading to fire sales in the former and market freezes in the latter. A test that suggests the absence of discontinuity jumps is non-conclusive in an RDD approach. The densities for two different segments of observations can shift in opposing directions, effectively offsetting each other in the aggregate. In our

Hence, typical ATE inference becomes invalid. These caveats notwithstanding, the estimated moments can be used for the calibration of a structural model. Buyers and sellers have a heterogeneous and unobservable degree of crime aversion. Our model also accommodates the observed change in the economic responses when we allow for a delay. Local buyers post prices that respond immediately to a crime, while out-of-town buyers respond only with a delay. Asymmetric information can explain both liquidity responses. House transactions increase after a crime if the group of uninformed buyers is large and decrease when this group is small enough, which is what we observe in the data around shootings and burglaries.

Third, we show that the direct market impact is most likely of short duration. Expanding the estimation windows yields more observations but renders most responses smaller (in absolute terms) or statistically insignificant.

Fourth, we provide aggregate welfare implications of crime and uncover distributional aspects associated with it. According to our calibrations, crime negatively affects house owners whose loss in direct residential utility can be offset if buyers are not informed about the current state of the neighborhood. Out-of-town buyers ignorant of a high-crime environment offer relatively high prices to sellers.

Fifth, our estimates stand out in the literature in terms of granularity.

We test our estimates with varying geographical and temporal bandwidths for robustness. It turns out that the effects vanish rather quickly over both time and space. This suggests that our results represent an upper bound on the effects crime exerts on housing property.

One limitation of our study is that estimates for the price effects are rather noisy. Our approach pools real estate market observations around and across different crime events. Each individual event is based on only 1.5 to 3.5 house sales on average. This means that some crimes have no untreated house transactions, while others have no treated house transactions. This does not matter for our density estimates, but it makes control for locational properties difficult in the estimation of price changes. We remedy this by controlling for the location explicitly through a partly linear model.

Several venues for future research follow from our paper. Our study addresses how crime affects residential valuations and decisions. As such, it looks at the fringe of the topic of urban flight whose literature focuses on the social composition of neighborhoods. This leads to the question of what factors are responsible for a neighborhood's configuration and what exactly changes after a crime. Answering this question can help us better understand several important topics, in particular how costs and benefits of crime-related moves among the population are distributed, to what extent these costs are borne by property owners versus renters, how city and suburban tax bases are affected, whether highly educated residents are more responsive to crime, and whether urban flight leaves behind a population with a greater dependency on city-provided public services.

One critical avenue for answering these questions resides in tackling identification and matching issues. Our dataset contains the names of the head of the household buying and selling the property. Unfortunately, house owners who sold their house to flee crime do not necessarily buy a new one. Rather, they may rent, which reduces the sample size and creates a selection issue. Similarly, a crime might bring forward the decision of a renter to buy a property in another part of town. Hence, an important extension of our dataset would complement it with rental agreements.

A question closely related to ours is whether a drop in the perceived risk of being subject to a crime leads to an increase in property values. Information about such perceptions is hard to come by, in particular on a larger scale with granular information regarding location and time. One potential promising remedy would be to incorporate a (one-sided) density estimate of crime in house pricing estimates.

A further related issue is whether housing prices bear on crime. An estimated (one-sided) house price trend could help explain crime frequencies. However, panel data with synthetic cross sections create noise through aggregation so that using local polynomial approximations appears more suitable.

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Appendix

Data Description

Figure 3 shows the temporal distribution of all events. The left panel shows the distribution over years, while the right panel shows the distribution within a representative year. House sales in Rochester dipped in 2011, which we attribute to the Great Recession. Only burglaries show a clear downward trend, while other crimes fluctuate throughout the years. The right panel of the left block shows the seasonal variation. Approximately 14% of house sales occur in June, which is well above the expected value of 8% given by a uniform distribution. Crimes also spike over the summer month, but there is a smaller peak around the change of the year.

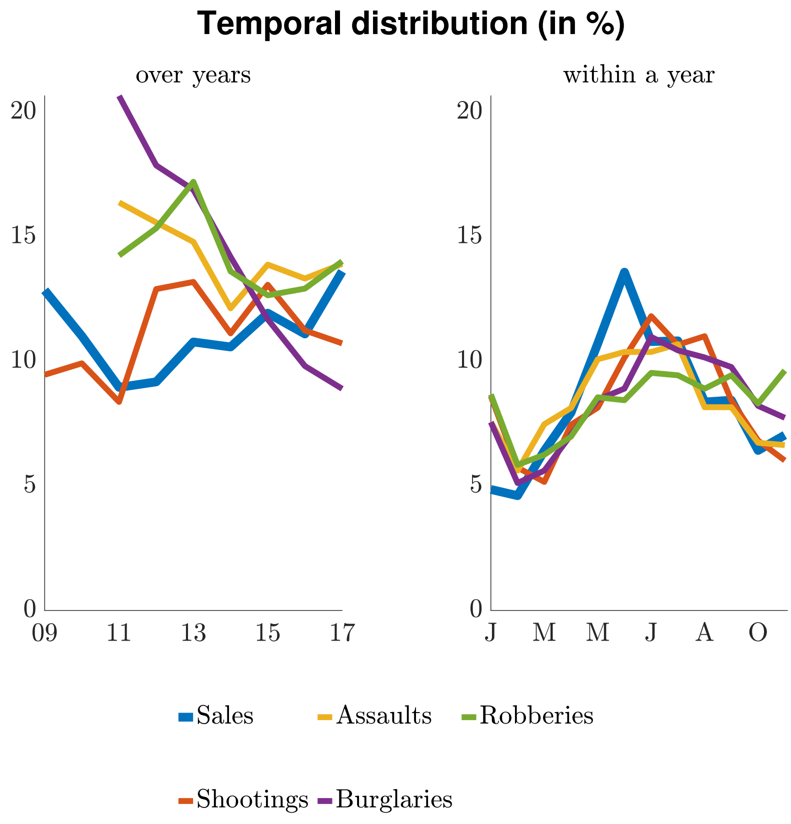

Figure 3: Temporal distribution of crime and sales events in Rochester city. The left (right) panel shows the normalized distribution across years (within a year).

Further Liquidity Estimates

Table 6 reports the estimates for the density discontinuity using the binning approximation to the density.

	Error inference:					Bootstrap		Empirical Likelihood	
	Density approximation:					Binning			
	Control for property size:					no	yes	no	yes
	ν_D	ν_B	ξ	ω	Δ_{McCrary}	Δ			
Shootings	0.3	30	0	0	0.2276	-6.1111	-1.0631	-3.1426	-0.3603
					(0.4975)	(5.9800)	(1.0595)	(6.0003)	(0.4637)
	0.3	30	0	365	0.1967	-9.3992	-2.4653***	-1.8220	-0.2064
					(0.3223)	(6.1594)	(0.9145)	(5.0489)	(0.2949)
	0.3	30	30	0	0.5919	-13.5731**	-1.2250	-8.9266	-0.6315
					(0.5233)	(5.6431)	(0.9870)	(5.5076)	(0.4141)
	0.6	60	0	0	0.2587**	0.2689	-0.7035	-0.1874	0.5203
					(0.1293)	(0.8679)	(1.1345)	(4.6003)	(0.8171)
Assaults	0.3	30	0	0	0.3080**	-0.5173	-1.0493	-8.9351	-1.3977*
					(0.1312)	(3.7888)	(1.2995)	(6.0580)	(0.7476)
	0.3	30	0	365	0.2642**	-0.3321	1.7077	-0.2051	-0.3844
					(0.1132)	(4.8636)	(1.1801)	(6.6162)	(0.7115)
	0.3	30	30	0	0.3480***	-2.8311	-1.7827	-5.0770	-0.6214
					(0.1253)	(2.5646)	(1.2891)	(5.6173)	(0.7899)
	0.6	60	0	0	0.0020***	-0.0233	-0.8810	0.0664	-0.3497
					(0.0424)	(0.0822)	(1.3075)	(0.5170)	(1.2748)
Burglaries	0.3	30	0	0	0.4466***	-0.0936	-0.4688	3.6845	-0.3801
					(0.0674)	(0.1419)	(1.4145)	(3.5238)	(1.1499)
	0.3	30	0	365	0.4070***	-0.1333	-2.4155*	-2.6321	-1.4394
					(0.0651)	(0.1848)	(1.2933)	(3.9311)	(1.1428)
	0.3	30	30	0	0.4027***	-0.0260	0.6309	0.8703	-0.1473
					(0.0696)	(0.1213)	(1.4081)	(4.5044)	(1.1472)
	0.6	60	0	0	0.1816***	-0.0439	0.3848	-0.0910	-1.9607
					(0.0217)	(0.0412)	(0.9368)	(0.3354)	(1.3971)
Robberies	0.3	30	0	0	0.4441***	0.1520	0.2751	6.0186	-0.5304
					(0.1296)	(3.1062)	(1.2686)	(5.6262)	(0.8373)
	0.3	30	0	365	0.4060***	0.1926	1.2733	5.0191	0.3870
					(0.1200)	(1.6329)	(1.4547)	(4.2889)	(0.8906)
	0.3	30	30	0	0.4074***	0.0732	1.5303	1.9448	-0.1063
					(0.1216)	(3.2329)	(1.4128)	(5.8726)	(0.8455)
	0.6	60	0	0	0.1582***	0.0112	0.2764	0.1404	0.5701
					(0.0370)	(0.0715)	(1.0716)	(0.3742)	(1.3456)

Table 6: Estimates for density jumps around crime events using the binning-based approximation. The four row blocks correspond to the types of crime. The first row in each row block uses the small data window centered around crimes. The second row uses a small data window but centered around synthetic crime events created $\omega = 365$ days prior to recorded crimes. The third row imposes a delay of $\xi = 30$ days after a factual crime. The fourth row uses the large data window centered around factual crimes. The numbers of observations are identical to those in Table 2. The estimates (with standard errors in parentheses beneath them) refer to the jump coefficient Δ. Estimates of the other coefficients are available upon request. *, **, and *** indicate that the value is different from zero with 10%, 5%, and 1% probability, respectively.

Locational Distance and Property Size as a Covariate

The assumption that the outcome is continuous around crimes is critical for RDD to apply. This assumption is not testable directly but provides testable predictions around predetermined covariates X_i (proposition 2c in Lee, 2008.). Specifically, covariates should have a continuous outcome around the threshold. We have two covariates. Our datasets allow us to measure the distance between the location of a crime and a house transaction. Furthermore, the dataset also includes measurements regarding the property size. Hence, we can ask whether these covariates indicate that there is some degree of manipulation around crime events. In particular, a change in average property size between the control and the treatment groups would point to socioeconomic factors at play. If houses are on average larger after a crime, then richer households arguably move away, a possibility that poorer individuals might not have. A change in the distance measurements is supportive of the liquidity estimates. For example, an increase in the distance measurements in the treatment group (compared to the control group) suggests that houses in the direct vicinity of the crime are less likely to be sold. This corresponds with a decrease in the density estimates, that is, a market freeze. On the other hand, a decrease in the distance measurements goes hand in hand with fire sales. We summarize that there were no meaningful changes in the property size around any type of crime event. There are some significant differences looking at the distance measurements.

We follow the procedure of the main analysis, which elicits jumps in the pricing observations, albeit we will not use the partly linear model, as distance is not a generic observation such as prices. We start with the summary statistics exhibited in Table 1 in the main text. Postshooting house transactions involve houses that are significantly closer to the crime location in the large estimation window. This does not apply in the small estimation window. There are no differences between distance or property size before and after assaults in either the large or the small estimation window. For burglaries, we find an increase in the distance in the large estimation window, albeit at the 10% significance level. Robberies, on the other hand, seem to lead to a decrease in property sizes.

Figure 2 plots the distance (to a crime) and the (log-) size of properties involved in a transaction around the respective crime for the small estimation window ($\overline{\nu} = \{30 \text{ days}, 0.3 \text{ km}\}$). Only burglaries show a significant increase for the distance measure. This suggests that houses that are further away appear in the treatment group more often in line with our main results where burglaries lead to a market freeze.

A specification test regarding the continuity of the two covariates regresses the distance and the property size using model $m^T(X)$, yielding a misspecification by a simple t test with the null $H_0 : \Delta = 0$. Table 7 summarizes the results for all crimes, estimation windows, and delay specifications.

We discuss the dynamics of distance to a crime location around crime events first. Two of the eight estimates for Δ are significant. The coefficients can be interpreted as changes in average distances after a crime measured in km. None of the coefficients are larger (in absolute terms) than 0.03, suggesting that if there is a discontinuity, it is merely a change of approximately 30 meters. As mentioned above, all but one significant jump corresponds to a significant estimate of a jump in the density.

Is there a discontinuity in the property sizes involved in house transactions? No.

	Explained variable				Distance	Property Size
	ν_D	ν_B	ξ	ω	Δ	
Shootings	0.3	30	0	0	0.0109 (0.0150)	0.0185 (0.0728)
	0.3	30	30	0	0.0299** (0.0149)	-0.0276 (0.0793)
Assaults	0.3	30	0	0	0.0025 (0.0059)	0.0008 (0.0354)
	0.3	30	30	0	0.0029 (0.0060)	0.0471 (0.0356)
Burglaries	0.3	30	0	0	0.0051 (0.0031)	-0.0285 (0.0178)
	0.3	30	30	0	-0.0001 (0.0032)	-0.0138 (0.0181)
Robberies	0.3	30	0	0	0.0036 (0.0055)	0.0110 (0.0329)
	0.3	30	30	0	0.0131** (0.0054)	-0.0127 (0.0332)

Table 7: Distance between house transactions and crime and log property sizes around crime events. The four row blocks correspond to the types of crime. The first row in each row block uses the small data window centered around crimes. The second row imposes a delay of $\xi = 30$ days after a factual crime. The estimation employs the linear model in Equation (1). The estimates refer to the jump coefficient Δ. Estimates of the other coefficients are available upon request. Asymptotic standard errors are in parentheses. *, **, and *** indicate that the value is different from zero with 10%, 5%, and 1% probability, respectively.

Serial Dependence of Events

Hausman and Rapson (2018) discuss the problem of serial dependencies when time is used as a running variable in RDD. There can be two issues in our data. First, crime can be self-exciting or, because of increased policing, decrease in frequency immediately after a crime. Second, house sales can also display serial dependency. For example, a house sale signals that a new owner is committed to staying and takes his or her house off the market. Alternatively, a house sale might encourage other neighbors to leave the area as well because of severed social ties to the neighbors who leave.

Crime on Crime

We discuss our analysis of the effect of crime on crime first. Note that we analyze the relationship between crimes, which addresses a factual statistical relationship. This is different from how individuals are affected by crimes. The main analysis focuses on the reactions of individuals to past crimes and not on the objective probability of becoming a victim of a crime that is rationally updated because of a crime event. As crimes are clustered geographically, the sample sizes increase drastically, so that we employ a tighter bandwidth $\{\nu_T, \nu_D\} = \{10 \text{ days}, 0.1 \text{ km}\}$.

Table 8 shows overwhelming evidence that crime frequency reduces after a crime. The only exception is immediately following a shooting, yet the delayed estimation window allows us to conclude that shootings eventually taper off. Policing might just require some time. We observe a similar response in the delayed density estimates for almost all other crime types.

									Error inference:	Empirical Likelihood	Maximum Likelihood
									Density approximation:	Local	Global
	ν_D	ν_B	ξ	ω	$\|I\|$	$\|C\|$	$\|H\|$	θ_{McCrary}		Δ	
Shootings	0.1	10	0	0	7149	2333	2331	1.7182*** (0.1076)		-0.2037 (0.1674)	-0.0665 (0.0737)
	0.1	10	30	0	6845	2253	2251	1.5526*** (0.1014)		-0.4415** (0.1798)	-0.2655*** (0.0699)
Assaults	0.3	30	0	0	904	583	583	0.2158 (0.2017)		-0.4069*** (0.1329)	-0.1412** (0.0566)
	0.3	30	30	0	845	525	535	0.0055 (0.1807)		-0.3749*** (0.1357)	-0.1047* (0.0552)
Burglaries	0.15	15	0	0	1467	1204	1205	0.6247*** (0.1621)		-0.5982*** (0.1111)	-0.2038*** (0.0462)
	0.15	15	30	0	1466	1212	1223	0.5924*** (0.1486)		691.4587 (1224.9274)	-0.6357*** (0.0497)
Robberies	0.1	10	0	0	2250	1901	1902	1.5874*** (0.1699)		-0.3992** (0.1684)	-0.1311* (0.0708)
	0.1	10	30	0	2037	1790	1770	1.1487*** (0.1497)		-0.8081*** (0.1788)	-0.3649*** (0.0718)

Table 8: Explaining the density around crime events. *, **, and *** indicate that the value is different from zero with 10%, 5%, and 1% probability, respectively. The four blocked rows correspond to the types of crime. The first row in each row block uses the small data window centered around crimes. The second row imposes a delay of $\xi = 30$ days after a crime. Standard errors are in parentheses.

Sales on Sales

Table 9 shows evidence that the housing market is self-depressing in frequency and pricing. The top left panel exhibits a large and significant drop in transaction frequencies immediately after a house sale. The effect becomes slightly smaller (in absolute terms) with a 30-day delay but remains significant. The bottom panel of Table 9 shows that total sales prices increase immediately and with a delay. The price per square meter, on the other hand, drops. We are not sure how to interpret this observation.

				Error inference:		Empirical Likelihood		Maximum Likelihood	
				Density approximation:		Local		Global	
				Control for property size:		no	yes	no	yes
ν_D	ν_B	ξ	ω	$\|I\|$	θ_{McCrary}	Δ			
0.1	10	0	0	7149	1.7182***	-0.6161***	-0.6162***	-0.2393***	-0.5649***
					(0.1076)	(0.0601)	(0.0601)	(0.0240)	(0.0791)
0.1	10	30	0	6845	1.5526***	-0.5595***	-0.5594***	-0.2266***	-0.4672***
					(0.1014)	(0.0592)	(0.0592)	(0.0260)	(0.0059)

Explained variable				**Price**			
Model				Linear		Partly linear	
Control for property size:				no	yes	no	yes
ν_D	ν_B	ξ	ω	Δ			
0.3	30	0	0	-0.0091	-0.0183	-0.0274***	0.2057***
				(0.0186)	(0.0244)	(0.0065)	(0.0149)
0.3	30	30	0	0.0104	-0.0046	-0.0256***	0.2661***
				(0.0189)	(0.0248)	(0.0069)	(0.0153)

Explained variable				**Price /m²**			
Model				Linear		Partly linear	
Control for property size:				no	yes	no	yes
ν_D	ν_B	ξ	ω	Δ			
0.3	30	0	0	0.0133	0.0266	-0.0799***	-0.6464***
				(0.0211)	(0.0278)	(0.0088)	(0.0159)
0.3	30	30	0	-0.0247	-0.0060	-0.0997***	-0.5838***
				(0.0222)	(0.0290)	(0.0092)	(0.0162)

Table 9: Explaining the density (top) and the normalized log price (middle and bottom panel) around house sales. *, **, and *** indicate that the value is different from zero with 10%, 5%, and 1% probability, respectively. The first row in each block uses the small data window centered around crimes. The second row imposes a delay of $\xi = 30$ days after a crime. Standard errors are in parentheses.

Counterfactual Calculations

As counterfactuals, we discuss two possible policy responses. First, the police, the local government, and the community at large can install measures that prevent crime. In particular, an increase in police patrols, the start of neighborhood watch groups, the installment of streetlights, and an influx of social workers might dampen the prospect of residents falling prey to crime in the future. Any of these measures contributes to the perception that a crime might occur. We translate such policy measures by decreasing the parameter that determines a switch to a high-crime state by 10%.

Second, a crime-contingent but immediate response to crime can lead to shortening the perception of duration a neighborhood remains in a high-crime state. A key ingredient, in addition to the efforts mentioned above, is arguably offenders' apprehension. As a result, the economy switches back to a low-crime state faster. To discuss the consequences of such a change in the environment, we increase the parameter that determines the switch back to a low-crime state, θ_l, by 10%.

We start by discussing the response from offenders' arrest first, which we dub "fight crime", followed by our discussion about the other policy responses, which we call "prevent crime". To facilitate the discussion, changes compared to the factual calibration are displayed in parentheses.

Fight Crime

The commitment to fight crime after it has occurred increases the fraction of meetings that lead to a house sale before it actually occurs. Only shootings show no sizable effect. We attribute this to the fact that θ_l in the shooting calibration is much smaller compared to the other types of crimes. The reason for an increase in successful meetings in the precrime period is that the comparative advantage between buyers and sellers regarding their willingness to live in a high-crime state becomes less important so that homeowners are more likely to accept an offer.

Table 10 summarizes the model dynamics and the welfare calculations for the two counterfactual scenarios. There is no sizable change in how prices respond to shootings; the relative change to θ_l is again insufficient to make a noticeable difference. Additionally, postcrime prices are largely determined by uninformed out-of-towners. However, there are some sizable premiums in price responses to the other crimes, which amount to almost USD 600 for locals after robberies. Hence, fighting crime leads to conspicuous reductions in price effects.

The precrime welfare changes compared to the factual results vary only little. The largest increase in housing welfare is USD 91 for robberies. Fighting crime matters less when it does not occur, but given that, arguably, crimes are relatively sparse, the rather small gain accumulates over many transactions.

However, the policy-induced change becomes sizable when we look at the welfare responses after a crime compared with those of the factual results discussed above. While housing welfare responses do not change after a shooting, fighting crime reduces the welfare loss from robberies by approximately USD 1,200, by approximately USD 1,100 from assaults, and still by approximately USD 500 from burglaries. Similarly, out-of-towners are less affected by the rude awakening of buying a house right after an assault or a robbery. The monetary recoup from fighting crime is over USD 1,000 in both cases. However, fighting crime has little effect after shootings and burglaries.

Prevent Crime

Similar to fighting crime, preventing crime generally increases housing liquidity even in the absence of crimes. A 10% decrease in θ_h leads to a stronger increase in house sales than a 10% increase in θ_l across all types of crimes. Unsurprisingly, the price responses at the onset of a crime are largely unaffected. The strongest change is a USD 55 increase in the price response immediately after a robbery.

Similar to fighting crime, the precrime welfare effects on residential utility are negligible. The strongest precrime welfare change in comparison with the factual calibration is observed for robberies with an increase of USD 100. The prevention of crime yields no welfare improvement when a crime is committed.

FIGHT CRIME		ξ	Shootings	Assaults	Burglaries	Robberies
Meetings resulting in sale (in %)		pc	61.93 (0.00)	79.16 (1.83)	68.43 (1.83)	79.13 (1.84)
Price changes	Out-of-towners	0	-1.82%/-1,398$ (0.00%/0$)	-0.19%/-171$ (0.02%/21$)	-2.43%/-2,234$ (0.25%/227$)	-0.36%/-340$ (0.06%/53$)
	Locals	0	-4.65%/-3,571$ (0.00%/0$)	-6.09%/-5,384$ (0.60%/534$)	-2.66%/-2,445$ (0.27%/248$)	-6.10%/-5,785$ (0.62%/589$)
Welfare effects	Housing	pc	(0.00%/0$)	(0.02%/18$)	(0.03%/28$)	(0.10%/91$)
	Housing	0	-25.23%/-19,393$ (0.00%/0$)	-12.38%/-10,950$ (1.23%/1,087$)	-5.43%/-4,997$ (0.54%/499$)	-12.71%/-12,059$ (1.27%/1,201$)
	Ex-post	0	-15.88%/-12,205$ (0.00%/0$)	-11.90%/-10,521$ (1.17%/1,032$)	-0.26%/-244$ (0.03%/23$)	-11.97%/-11,355$ (1.14%/1,083$)
PREVENT CRIME		ξ	**Shootings**	**Assaults**	**Burglaries**	**Robberies**
Meetings resulting in sale (in %)		pc	64.24 (2.31)	79.36 (2.03)	68.63 (2.03)	79.33 (2.04)
Price changes	Out-of-towners	0	-1.82%/-1,398$ (0.00%/0$)	-0.20%/-181$ (0.01%/12$)	-2.67%/-2,459$ (0.00%/2$)	-0.36%/-338$ (0.06%/55$)
	Locals	0	-4.65%/-3,571$ (-0.00%/-0$)	-6.69%/-5,919$ (-0.00%/-1$)	-2.92%/-2,692$ (0.00%/1$)	-6.72%/-6,376$ (-0.00%/-2$)
Welfare effects	Total	pc	(0.00%/0$)	0.01%/13$)	(0.02%/17$)	(0.07%/66$)
	Housing	pc	(0.00%/0$)	(0.02%/19$)	(0.03%/31$)	(0.11%/100$)
	Resale	pc	(-0.00%/-0$)	-0.01%/-6$)	(-0.01%/-14$)	(-0.04%/-34$)
	Total	0	-7.46%/-5,733$ (-0.00%/-0$)	-13.41%/-11,855$ (-0.00%/-0$)	-5.93%/-5,457$ (-0.00%/-0$)	-13.85%/-13,132$ (-0.00%/-0$)
	Housing	0	-25.23%/-19,394$ (-0.00%/-0$)	-13.61%/-12,038$ (-0.00%/-0$)	-5.97%/-5,496$ (-0.00%/-0$)	-13.98%/-13,261$ (-0.00%/-0$)
	Resale	0	17.77%/13,660$ (0.00%/0$)	0.21%/182$ (-0.00%/-0$)	0.04%/38$ (-0.00%/-0$)	0.14%/129$ (-0.00%/-0$)
	Ex-post	0	-15.88%/-12,206$ (-0.00%/-1$)	-13.10%/-11,581$ (-0.03%/-29$)	-0.29%/-268$ (-0.00%/-1$)	-13.24%/-12,560$ (-0.13%/-122$)

Table 10: Results from the welfare analysis for the counterfactual calculations. Note that $\xi = \text{pc}$ refers to precrime results when out-of-towners believe $x = 0$. The results in brackets refer to the change in comparison with the factual model calibration.

Online Appendix

The RDD Approach

A house sale i is either untreated $(W = 0)$ or treated $(W = 1)$, depending on whether it occurred before $(T_i \leq 0)$ or after $(T_i > 0)$ a crime, respectively.

Some characteristics of a house sale are unobservable. In particular, heterogeneity in crime aversion among buyers and sellers is critical for our analysis. Hence, we index outcomes by i so that

$$P_i(W_i) = \alpha_i + \beta_i [T_i > 0] \tag{10}$$

where $\alpha_i = P_i(0)$ and $\beta_i = P_i(1) - P_i(0)$. The ATE of crimes on prices is labeled β and is a weighted average of β_i over all i.

Formally, identification requires that

$$F(P_i(0)|T_i = \tau) \quad \text{and} \quad F(P_i(1)|T_i = \tau) \quad \text{are continuous in } \tau \text{ at } 0 \tag{11}$$

$$\mathbb{E}[P_i(0)|T_i = \tau] \quad \text{and} \quad \mathbb{E}[P_i(1)|T_i = \tau] \quad \text{are continuous in } \tau \text{ at } 0 \tag{12}$$

$\forall i$ according to Hahn, Todd and Van der Klaauw (2001).[24] Simply put, the outcomes and the distribution of outcomes of each observation i are continuous in time when the crime occurs. Here, an observation consists of a combination of a seller, a buyer and a house, which we call a sale. The limiting difference between values before and after a crime

$$\beta = \lim_{\tau \downarrow 0} \mathbb{E}[P|T = \tau] - \lim_{\tau \uparrow 0} \mathbb{E}[P|T = \tau] \tag{13}$$

yields the ATE.

Lee (2008) refines assumptions (11) and (12). We discuss the relaxation regarding as-

[24]See Imbens and Lemieux (2008) or Lee and Lemieux (2010) for discussions.

sumption (11) next and address assumption (12) further below.

First, identification of the ATE is preserved even when the running variable is partially manipulated. The RDD identification can become invalid if test subjects have deterministic control over whether they belong to the control or the treatment group. This can change the composition of treated and untreated observations, a phenomenon called "manipulation". It suggests that test subjects know the threshold location and can control the running variable. We assume crimes are pre-determined in a statistical sense but unanticipated by market participants. The latter assumption is at odds with the notion of manipulation as usually described in the RDD literature, that is students know their exam score immediately and stop as soon as they reach the critical threshold for a prize. But manipulation is simply a change in the statistical assignment rule which coincides with our interpretation that house sales are more or less likely after a crime.

A toy model can illustrate this idea. Assume (potential) buyers and sellers are randomly paired into matches, labeled i. The net surplus of a house purchase is $S_b = u_b - bW_i - P_i$ for a potential buyer where P_i is the payment for the house, u_b the gains from trade, and b the disutility because of a crime treatment. Similarly, the net surplus of a seller is $S_s = sW_i + P_i$ where leaving the area now creates utility s if match i is treated. Assume a transaction is only executed when the joint welfare $S_i = u_b + (s - b)\, W_i$ is non-negative, and the payment $P_i = \frac{1}{2}\left(u_b - (s - b)\, W_i\right)$ splits the welfare gains evenly. Every untreated match leads to a transaction with $P_i\,(0) = \frac{1}{2}$ for $u_b = 1$. Treated matches, on the other hand, only trade when $S_i \geq 0 \Leftrightarrow u_b + s \geq b$, or when the crime aversion of the seller (buyer) is sufficiently large (small). Compare the left panel of Figure 4. This is the case for about $\frac{4}{5}$ of all matches if both parameters for crime-aversion, b and s, are exponentially distributed with rate $\lambda = 1$. The observed average payment is $P^{obs}\,(1) \approx -\frac{2}{5}$ while the average payment valuation for all matches is $P^{true}\,(1) = -\frac{1}{2}$. We measure $\beta^{obs} = -\frac{9}{10}$ while the true causal effect of a crime on the house price is $\beta^{true} = -1$. Compare the middle panel of Figure 4. Note that the (unobservable) heterogeneity in the pricing function is necessary to invalidate the inference

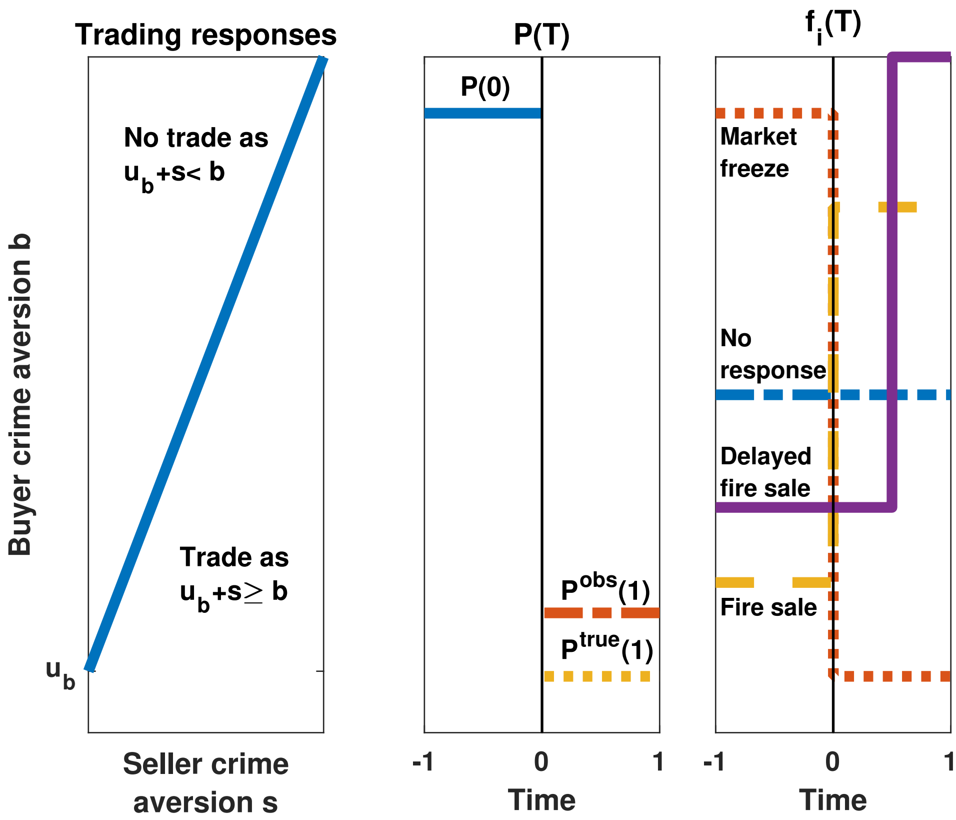

Figure 4: All trades are executed when a sale is not treated. However, as shown in the left panel, only trades underneath the line $b = u_b + s$ are executed when a crime is perpetrated. The middle panel shows the untreated payment before the crime as well as the observed and true treated payment after the crime that occurs at $t = 0$. The right panel displays different scenarios in the sale-specific density function $f_i\left(T\right)$.

of β. In fact, we can identify the causal effect for each sale as $\beta_i = P_i(1) - P_i(0) = \frac{b+s}{2}$ if we were to observe b and s.

Identification under (partial) manipulation is preserved if there is a positive probability for every sale i to be either treated or untreated, or $0 < F_i(T_i = 0) < 1\,\forall i$ (condition 2b in Lee, 2008). However, our toy model suggests that $F_i(T_i = 0) = 1$ for $u + s < b$.

A violation of assumption (11) can be detected by testing for a discontinuity in the density function.[25] Let $f_i(T)$ denote the density of sale i over time. We would like to test whether $\theta_i = 0$ directly where

$$\theta_i = \lim_{T\uparrow 0} \log(f_i(T)) - \lim_{T\downarrow 0} \log(f_i(T)) \tag{14}$$

However, we only observe one realization per sale. Hence, we pool observations and generate a distribution F with density f to test whether $\theta = 0$ where

$$\theta = \lim_{T\uparrow 0} \log(f(T)) - \lim_{T\downarrow 0} \log(f(T)) \tag{15}$$

In other words, we try to detect a non-trivial aggregate manipulation of the running variable time around crime events. A rejection of the null hypothesis can suggest the identification of the ATE on prices is invalid because the price averages below and above the threshold are composed of different kinds of sales.[26]

The second relaxation in Lee (2008) addresses a possible direct impact of time on the

[25] Detecting anomalies in distribution has a wide range of applications in economics. For example, bunching exploits discontinuities in an incentive schedule to elicit economically meaningful parameters. Kleven (2016) provides a recent survey. Similarly, RDD identification rests on the assignment to a treatment group when a running variable is either above or below some threshold. But this is where the similarities end. Bunching relies on the assumption that the outcome variable is subject to choice while RDD requires that the running variable cannot be completely manipulated.

[26] There are two caveats to this test. We detect a false positive if, for example, $b = s + c + \epsilon$ where c is a constant and ϵ is normal noise. Then $\beta^{obs} = \beta^{true} = c$ yet

$$Prob(S_i \geq 0) = Prob(u \geq c + \epsilon) < 1$$

We can also detect a false negative where two segments of sales react in opposing directions. We present a more elaborate model below and showcase this idea.

outcome (condition 1c in Lee, 2008). We include time as an explanatory variable in equation (10) in order to reduce the estimation bias of β due to housing market trends (Fan and Gijbels (1996)). Higher-order polynomials tend to accommodate jumps. Hence, we arrive at specifications (1) and (3).

Finally, a peculiarity of our analysis is that we use time as the running variable. This does not necessarily invalidate the RDD approach. In particular, the assumptions in RDD are concerned with continuity and assignment probabilities. Inference is valid as long as observations have a positive probability to occur in either the control and the treatment group. But letting time sort observations into the control group and the treatment group and using the RDD approach bears similar risks as it does in panel data regressions. In particular, three issues arise in this context as discussed by Hausman and Rapson (2018). First, serial dependence in the real estate market and an underlying crime process can create a bias if the estimation window is too large. For this reason we use relatively small estimation windows. Second, equi-distanced time series makes it impossible to test for manipulation. This does not apply to our data so that we can explicitly test for manipulation. Third, the use of observations far away from the threshold can introduce a long-run bias. We avoid the latter pitfall by using varying bandwidths that are reasonably small.

The Binning Estimator

In the following we describe the binning estimate for the log-density. Effectively, we do not observe the density of sales directly. McCrary (2008) suggests binning observations. This procedure generates regressands by counting the number of observations in each bin.

Let ϕ_T describe the uniform bin width for the time dimension, and ϕ_D the uniform bin width for the distance dimension. We create a grid over time, $\left\{T_j^{bin}\right\}_{j=1}^{J} = \left\{(2j - J - 1)\,\phi_T,\right\}$, and distance, $\left\{D_k^{bin}\right\}_{k=1}^{K} = \{(2k-1)\,\phi_D,\}$, where J and K are even.[27] Then we calculate

$$Y_{jk}^{bin} = \frac{1}{I}\frac{1}{\phi_T\phi_D}\sum_i \mathbb{I}\left(T_j^{bin} - \phi_T \leq T_i < T_j^{bin} + \phi_T \wedge D_k^{bin} - \phi_D \leq D_i < D_k^{bin} + \phi_D\right)$$

as the density of bin $\{j,k\}$ with midpoint $X_{jk}^{bin} = \left\{T_j^{bin}, D_k^{bin}\right\}$. Denote $y_{jk}^{bin} = \log\left(\epsilon + Y_{jk}^{bin}\right)$ where ϵ bounds the density away from zero.

The problem becomes

$$\min_\delta\left\{\sum_{j,k} w\left(X_{jk}^{bin}\right)\left(y_{jk}^{bin} - v\left(X_{jk}^{bin},\delta\right)\right)^2\right\} = \min_\delta\left\{\sum_{j,k} s\left(y_{jk}^{bin}, X_{jk}^{bin},\delta\right)\right\} \tag{16}$$

Underlying this objective function is the assumption that there is a γ_0 so that

$$\mathbb{E}\left[s\left(y_{jk}^{bin}, X_{jk}^{bin},\delta_0\right)\right] \leq \mathbb{E}\left[s\left(y_{jk}^{bin}, X_{jk}^{bin},\delta\right)\right] \tag{17}$$

and the inequality becomes strict for $\delta \neq \delta_0$.

McCrary (2008) derives a cookbook recipe for good choices for the binning grid and the bandwidth but emphasizes to take these parameters as suggestive starting values only. We employ $J = K = 20$ throughout.

Equation (16) suggests using simple least-squares formulae. But the associated standard formulae for the standard deviations are inappropriate as they neglect the noise captured in $\left\{Y_{jk}^{bin}\right\}_{jk}$. McCrary (2008) also develops a t-statistic-styled test for $\theta = 0$, or whether the running variable is manipulated around the threshold. He estimates the level density from both sides of the discontinuity point, and the estimate of θ combines those two separate regressions.

Our setup suggests to estimate both sides jointly after a logarithmic transformation of the density. This has the advantage that we avoid negative point estimates for the density.

[27] For example, for $J = 4$ and $\phi_T = 1$ the sequence would be $\left\{T_j^{bin}\right\}_{j=1}^{4} = \{-3,-1,1,3\}$. Similarly, let $K = 4$ and $\phi_D = 1$. Then $\left\{D_k^{bin}\right\}_{k=1}^{4} = \{1,3,5,7\}$

Further, the formulation of the t-statistic-styled test of McCrary (2008) hinges on the choice of the weighting function, $w(T, D)$, as pointed out by Otsu, Xu and Matsushita (2013). We avoid these complications by applying a pairwise bootstrap with 1000 draws in a direct least-squares estimation. This is the BinBtStp estimator.

Further, we derive moment conditions to apply the empirical likelihood method described below. This is the BinEL estimator. The sample analogue objective function (16) has moment conditions that are equal to zero at the true parameters. Let

$$\mathbb{E}\left[g_{jk}^{bin}(\delta_0)\right] = \mathbb{E}\left[\frac{\partial s\left(y_{jk}^{bin}, X_{jk}^{bin}, \delta_0\right)}{\partial \delta}\right] = 0 \tag{18}$$

be a $l \times 1$ when δ has l elements.

The Local Likelihood-based Estimator

Let f be globally continuous with one exception at X_0. We approximate the density f using $\widehat{f}$. The Kullback-Leibler divergence between those two densities is given by

$$D_{KL} = \int f(X) \log\left(\frac{f(X)}{\widehat{f}(X)}\right) dX \tag{19}$$

$$= \int f(X)\left(\log(f(X)) - \log\left(\widehat{f}(X)\right)\right) - 1 + \widehat{f}(X)\, dX \tag{20}$$

where the second line applies because the approximation $\widehat{f}$ integrates to one. A modification that puts more weight on observations close to a value X_0 is given by a localized Kullback-Leibler distance

$$D_{LKL} = \int w(X)\left(f(X)\left(\log(f(X)) - \log\left(\widehat{f}(X)\right)\right) - 1 + \widehat{f}(X)\right) dX \tag{21}$$

The Kullback-Leibler distance is the dual representation of the likelihood function; while the first is minimized at the true density, the latter is maximized. Therefore, the asymptotic local log-likelihood representation is

$$LL\left(\widehat{f}\right)=\int w\left(X\right)\log\left(\widehat{f}\left(X\right)\right)dF\left(X\right)-\int w\left(X\right)\widehat{f}\left(X\right)dX \tag{22}$$

where elements that are not affected by a maximization are dropped. The sample analogue is given by

$$LL\left(X_I,\boldsymbol{\delta}\right)=\frac{1}{I}\sum_i w\left(X_i\right)v\left(X_i,\boldsymbol{\delta}\right)-\int w\left(X\right)\exp\left(v\left(X,\boldsymbol{\delta}\right)\right)dX \tag{23}$$

The discontinuity splits the numerical integration in two parts. The sample analogue objective function (23) provides the following l population moment conditions

$$\mathbb{E}\left[g_i^{LL}\left(\delta_0\right)\right]=\mathbb{E}\left[\frac{\partial LL\left(X_i,\boldsymbol{\delta}_0\right)}{\partial\delta}\right]=0 \tag{24}$$

when $\boldsymbol{\delta}$ has l elements. Applying the EL approach yields the ELL estimator. We describe the EL approach that employs the moment conditions (18) and (24) next.

Empirical Likelihood-based Functions

We proceed by describing the empirical likelihood estimator for a generic moment condition $\mathbb{E}\left[g_i\left(\boldsymbol{\delta}_0\right)\right]=0$ with I observations.[28] There are $J\times K$ observations for the binning estimator. We place probability p_i on observation i so that $\sum_i^I p_i=1$ and $p_i\geq 0$ are the only constraint for a maximum likelihood estimator with I unknowns. With I observations this model is just-identified. This is equivalent to maximizing

$$\widetilde{\mathcal{L}}\left(p,\mu\right)=\sum_i^I\log\left(p_i\right)-\mu\left(\sum_i^I p_i-1\right)$$

[28]See Hansen (2018, chapter 19) for a great exposition.

The MLE yields $p_i = \frac{1}{I}$. The additional moment conditions in (24) or (18) produce a Lagrangian objective function

$$\mathcal{L}(\boldsymbol{\delta}, p, \mu, \lambda) = \sum_i^I \log(p_i) - \mu\left(\sum_i^I p_i - 1\right) - I\lambda'\left(\sum_i^I p_i g_i(\boldsymbol{\delta})\right) \tag{25}$$

which has the following first-order conditions

$$\begin{array}{ll} (p_i) & \dfrac{1}{p_i} = \mu + I\lambda' g_i(\boldsymbol{\delta}) \\ (\mu) & \displaystyle\sum_i^I p_i = 1 \\ (\lambda) & \displaystyle\sum_i^I p_i g_i(\boldsymbol{\delta}) = 0 \end{array}$$

We find $\mu = I$ and

$$p_i = \frac{1}{I(1 + \lambda' g_i(\boldsymbol{\delta}))}$$

which can be substituted back in (25) to find the sample analogue

$$L(\boldsymbol{\delta}, \lambda) = -I\log(I) - \sum_i^I \log(1 + \lambda' g_i(\boldsymbol{\delta})) \tag{26}$$

where λ are Lagrange-style multipliers for the moment condition (24).[29] Then λ minimizes (26) for a given $\boldsymbol{\delta}$, or

$$\hat{\lambda}(\boldsymbol{\delta}) = \arg\min_{\lambda}\{LL(\boldsymbol{\delta}, \lambda)\}$$

The solution is well-defined as $LL(\boldsymbol{\delta}, \lambda)$ is convex in λ, but must be solved numerically. The estimate for $\boldsymbol{\delta}$ is obtained for maximizing

$$\hat{\boldsymbol{\delta}} = \arg\max_{\boldsymbol{\delta}}\left\{LL\left(\boldsymbol{\delta}, \hat{\lambda}(\boldsymbol{\delta})\right)\right\}$$

[29]In other words, the maximum likelihood function defined in equation (23) can be used as a Type 1 and a a Type 2 ML estimator (Davidson, MacKinnon et al. (2004, page 404)), but we are employing the first-order conditions in equation (24) to feed the empirical likelihood function in equation (26).

which also requires a numerical optimization. Hence, the estimation of $\boldsymbol{\delta}$ requires a nested optimization. The interior optimization finds a λ that minimizes (26) while an exterior optimization finds an $\boldsymbol{\delta}$ that maximizes that value function.

Define $G_i(\boldsymbol{\delta}) = \frac{\partial g_i(\boldsymbol{\delta})}{\partial \boldsymbol{\delta}'}$, $G = \mathbb{E}[G_i(\boldsymbol{\delta})]$, and $\Omega = \mathbb{E}\left[g_i(\boldsymbol{\delta})\, g_i(\boldsymbol{\delta})'\right]$, then the estimator is well-behaved and

$$\sqrt{I}\left(\hat{\boldsymbol{\delta}} - \delta\right) \xrightarrow{d} N\left(0, \left(G'\Omega^{-1}G\right)^{-1}\right)$$

and

$$\sqrt{I}\hat{\lambda} \xrightarrow{d} N\left(0, \Omega - G\left(G'\Omega^{-1}G\right)^{-1}G'\right)$$

where $\sqrt{I}\left(\hat{\boldsymbol{\delta}} - \boldsymbol{\delta}\right)$ and $\sqrt{I}\hat{\lambda}$ are asymptotically independent under some regulatory conditions.

Monte Carlo Study

We test the four density estimators (Binning with bootstrapped errors, binning with empirical likelihood, local likelihood with empirical likelihood, and maximum likelihood). The bi-variate data generating process can be described by two linear density functions which correspond to the control and treatment window, respectively. In line with our estimation, the realization $t \leq 0$ assigns an observation to the control group while the observation is treated if $t > 0$. The second variable d measures the distance from the point of interest. The density is determined by

$$f(t,d) = \begin{cases} A + sd & \text{if } -1 \leq t \leq 0 \text{ and } 0 \leq d \leq 1 \\ A' + s'd & \text{if } 0 \leq t \leq 1 \text{ and } 0 \leq d \leq 1 \\ 0 & \text{otherwise} \end{cases}$$

where $\Delta = A'/A$ is the discontinuity at the point of interest $d = 0$, $k = \log(s/A) + 1$ determines the increase of the density between $d = 0$ to $d = 1$ for the control window, and

$h = \log\left(s'/A + \exp\left(\Delta\right)\right) - k$. Note that the estimators assume a uniform density in d so that $k \neq 0$ provides a mis-specification, albeit one that is uniform along any value of d if $h = 0$.

Further, k provides a (stronger) measure of mis-specification. If $h = 0.2$, as in the simulation study below, then the density jump between the control and the treatment window increases in d. This motivates our use of a weighting function to focus on the density jumps at $d = 0$.

We denote the estimate of Δ for the simulated sample s by Δ_s, and the squared bias of Δ_s by $\Delta_{bias^2} = \frac{1}{S}\sum\left(\Delta_s - \Delta\right)^2$. The standard error is $\Delta_{se} = \frac{1}{S}\sum\left(\Delta_s - \Delta_{mean}\right)^2$ where $\Delta_{mean} = \frac{1}{S}\sum \Delta_s$. The mean squared error (MSE) is $\Delta_{mse} = \Delta_{se} + \Delta^2_{bias}$.

Table 11 contains summary statistics. We summarize the results in three main points. First, the binning estimator performs very bad in small samples throughout. Second, the maximum likelihood estimator outperforms all other estimators in small and large samples, regardless of whether the data generating process fits the global density or whether there is a light or strong mis-specification. Third, the local likelihood specification using the empirical likelihood inference can be considered a good second choice.

Mis-spefication	Density approximation	Error inference	$\|I\|$	No Jump ($d = 0$) Δ_{bias^2}	No Jump ($d = 0$) Δ_{se^2}	No Jump ($d = 0$) Δ_{mse}	Jump ($d = 0.05$) Δ_{bias^2}	Jump ($d = 0.05$) Δ_{se^2}	Jump ($d = 0.05$) Δ_{mse}
No ($k = 0, h = 0$)	Binning	Bootstrapping	300	15.41	15.41	30.82	15.44	15.37	30.81
			1000	0.04	0.04	0.08	0.05	0.05	0.09
		Empirical Likelihood	300	25.29	25.28	50.57	26.15	26.03	52.18
			1000	0.06	0.06	0.12	0.11	0.11	0.21
	Local		300	0.09	0.09	0.18	0.09	0.09	0.18
			1000	0.03	0.03	0.05	0.03	0.03	0.05
	Global	Maximum Likelihood	300	0.03	0.03	0.06	0.03	0.03	0.06
			1000	0.01	0.01	0.02	0.01	0.01	0.02
Light ($k = 0.2, h = 0$)	Binning	Bootstrapping	300	18.32	18.33	36.65	17.67	17.65	35.33
			1000	0.04	0.04	0.08	0.05	0.05	0.10
		Empirical Likelihood	300	31.69	31.69	63.38	30.53	30.47	61.00
			1000	0.06	0.06	0.12	0.08	0.08	0.16
	Local		300	0.09	0.09	0.18	0.09	0.09	0.18
			1000	0.03	0.03	0.05	0.03	0.03	0.05
	Global	Maximum Likelihood	300	0.03	0.03	0.06	0.03	0.03	0.06
			1000	0.01	0.01	0.02	0.01	0.01	0.02
Strong ($k = 0, h = 0.2$)	Binning	Bootstrapping	300	18.93	18.87	37.80	18.60	18.34	36.94
			1000	0.05	0.04	0.09	0.04	0.04	0.08
		Empirical Likelihood	300	31.57	31.58	63.14	30.91	30.77	61.68
			1000	0.08	0.08	0.15	0.06	0.06	0.11
	Local		300	0.09	0.09	0.18	0.10	0.09	0.19
			1000	0.03	0.03	0.06	0.03	0.03	0.05
	Global	Maximum Likelihood	300	0.04	0.03	0.06	0.03	0.03	0.06
			1000	0.01	0.01	0.02	0.01	0.01	0.02

Table 11: Summary statistics for estimates of Δ using 1,000 simulations each.

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