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How bad is crime for business? Evidence from consumer behavior☆

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ABSTRACT

Understanding how consumers respond to crime offers evidence of how safety perception impacts individuals daily choices and has important implications for economic development of communities. This paper investigates the impact of local crime on subsequent consumer visits to food and entertainment retails using a novel longitudinal dataset with point-specific crime and consumer visit data. We leverage the richness of our data to account for unobserved heterogeneity and time variant confounders through temporal and geographical variation. Our results show that consumers respond more strongly to property and street crimes. The response concentrates on the venue visit decision rather than the intensity of consumption (i.e. duration) in the venue.

1. Introduction

Theoretical and empirical arguments suggest that the fear of victimization causes consumers, workers and entrepreneurs to alter their routine activities (Hamermesh, 1999; Wilcox et al., 2018). Crime and its resulting behavior changes increase the cost of doing business in a locality and ultimately affects the development trajectory of the whole neighborhood (Greenbaum and Tita, 2004). The economics literature has barely devoted attention to studying whether and how crime impacts business activities through its effect on consumer behavior. This paper aims to fill the gap by directly measuring consumers' sensitivity to criminal activities.

We leverage point-specific crime and consumer visit data to investigate the impacts of different crimes on subsequent consumer visits to restaurants, entertainment and retail establishments, a subset of businesses that are highly sensitive to actual and perceived levels of safety (Rosenthal and Ross, 2010). Our findings suggest that consumers respond to property and street crimes. However, the response is only on the extensive margin measured by number of visits and number of consumers, not on the intensive margin measured by venue dwell time.

Understanding consumers' sensitivity to local crime is crucial for businesses, city planners and policy makers. The importance of customers for a business's success is self-evident. By attracting more customers, businesses secure revenue and increase the likelihood of survival. In recent times, the presence of thriving small local businesses like coffee shops, grocery stores and bars has emerged as a symbol of neighborhood development and gentrification (Papachristos et al., 2011; Glaeser et al., 2018). Thus, by measuring consumer response to local crime this study helps policy makers and city planners to understand how crime might affect economic development efforts and if crime control can be a viable economic development tool. The findings of this paper may also contribute to debates on city zoning and public finance of local jurisdictions. If businesses substantially lose potential consumers as a result of crime, the public administration could, for instance, create tax credit programs such as Opportunity Zones, or Business Improvement Districts (BID) where businesses are required to pay an additional tax in order to fund projects that promote local improvements and public safety.

The vast majority of the literature on the effect of crime on business activities approaches the topic from the supply side focusing on business inception, closure or relocation. Greenbaum and Tita (2004) investigate the impact of local homicide levels upon job creation and destruction caused by changes in business status. Their findings suggest that firms adapt to violence surges within their operating environments. They observe no significant impacts of violence on business closures. Similarly, Bates and Robb (2008) find that young firms operating in high-crime

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niches in urban areas of the United States are not disadvantaged by crime. Lastly, De la Roca et al. (2016) conclude that certain environmental factors like crime are not significant in explaining firm inception or survival once they control for time invariant characteristics of a neighborhood.

On the contrary, Hipp et al. (2019) report that higher prevalence of violent and property crimes are significantly associated with both business failure and relocation. Lens and Meltzer (2016) find that neighborhood crime reduces commercial property values, used as proxy for economic activity. Rozo (2018) studies abrupt reductions in violence driven by government's expenditures in security and finds that when firms face higher violence their output prices fall more than the prices of inputs. This drives firms to reduce production, and eventually some firms exit the market.

Therefore, at this stage there is not a clear consensus on the effect of crime on business activities and most of the empirical results in the literature still lack causal interpretations. Furthermore, most likely owing to the dearth of detailed data, much less attention has been devoted to the consequences of crime on the demand side of business activities. This study begins to fill this gap. To the best of our knowledge, we provide the first empirical evidence of how routine consumer activities are affected by local crime.

Our analysis focuses on Chicago, the third most populous city in the United States. Incident level crime data are provided by the Chicago Police Department and publicly available at the city of Chicago data portal. The consumer visit data are drawn from the SafeGraph business venue database, which catalogs the dynamic human mobility patterns of over 45 million mobile devices in the United States.[1] The SafeGraph data for Chicago contain daily counts of visits to about 15,000 food and entertainment venues from January 2017 to September 2019. We combine crime and consumer visit data to form a longitudinal dataset by matching detailed local-area crime statistics to each venue in our sample.

To estimate the local impacts of crime on subsequent consumer visits, we face two important concerns. First, with two databases detailed in multiple dimensions, there are several plausible ways to aggregate the data, i.e. how should we classify crimes and define "local" and "subsequent"? Second, identifying the effects of interest is challenging. The main difficulty lies in handling the unobserved determinants of consumer visits that are also correlated with local crime, such as foot-traffic, neighborhood amenities and trends in local economy.

We utilize a conservative approach to account for time invariant and variant confounders. The approach starts with the intuition that the impacts of local crime occur at fine levels of geography and time, whereas most confounders, such as weather and neighborhood socioeconomic status, only vary at fine levels of geography or time, but not both (Caetano and Maheshri, 2018). In light of this, we specify fixed effects varying at different temporal and geographical levels from our variables of interest. Given several practical trade-offs, our variables of interest, local crimes of different categories, are aggregated to monthly counts at the block group level. The fixed effects we specify are at the levels of tract by month and block group by year. The former captures all time-varying unobservables that vary at a larger geographic area than a block group (e.g. weather) and the latter absorbs neighborhood-specific confounders such as wealth level that changes more slowly than crime. To alleviate the endogeneity concerns from venue specific confounders and confounders varying at the same temporal and geographical level as crime, we add venue fixed effects and lagged consumer visits aggregated at the block group level.

[1] SafeGraph data (https://www.safegraph.com) have been widely used by researchers and the Centers for Disease Control and Prevention examining the COVID-19 impacts (see for example Allcott et al., 2020, Dave et al., 2020a, Dave et al., 2020b and Dave et al., 2020c).

Our research design is bolstered by multiple robustness checks. First, the results vary little when adding more control variables measuring local economic development (number of active business licenses and number of building permits), venue attractiveness (median venue dwell time and median distance from home travelled by visitors) and crime spillover (crimes in the nearest adjacent neighborhood). Second, we confirm that our estimates are not likely to suffer from endogeneity via an exogeneity test developed by Caetano (2015). Finally, we perform a falsification test using incidents that happened in residences and are classified as domestic-related. These validity checks lend credibility that our empirical strategy recovers plausibly causal estimates, close enough to the true effects of local crimes on consumer visits.

We find that the effects of property crimes and street crimes on consumer visits in the following month are negative, meaningful and strongly significant. One additional property crime incident near a venue results in 1.13 fewer visits to that venue in the following month, which is a 12% reduction in consumer visits with one standard deviation increase in property crime. Further exploration suggests that theft is the subcategory of crime driving this result. The estimated effect for violent crime is also negative, though not statistically significant. We also look at the crime effects by place of occurrence. One additional crime in streets near a venue results in about three fewer visits to that venue in the following month, a 10% reduction in consumer visits with one standard deviation increase in street crime. Notably, while the effect is large and significant for incidents that occur in public spaces, crimes that occur within residences do not have a statistically significant effect on subsequent consumer visits. Residential crime is less visible to consumers and usually not random, which precludes fear of victimization. However, residential crime is highly correlated to neighborhood trends. The fact that we do not find a statistical significant result for residential crime suggests that unobserved factors are not likely driving the negative association between outdoor crimes and consumers visits found by us. Overall, our findings are consistent with the argument that the perception of crime and the risk of victimization scare off consumers, potentially making businesses less profitable.

Apart from the main results above, we also study the variation in crime effects by exploiting alternative outcomes and estimating heterogeneous effects by venue type and neighborhood crime profiles. Our findings suggest that crime has a negative effect on consumers in the extensive margin (number of visits and number of customers), but no sizable effects in the intensive margin (venue dwell time). We also provide evidence that night visits are more sensitive to changes in crime than day time visits. Furthermore, we find that consumers respond to salient crimes (e.g. street crime) in low crime neighborhoods, and severe crimes (e.g. violent crime) in high crime neighborhoods. Lastly, we find evidence of asymmetric effects by neighborhood crime trend. While in areas where criminal activity is uptrending consumers react to crimes committed outdoor, in neighborhoods facing crime decline consumers are only sensitive to crimes happening at venues.

The paper is organized as follows. The next section provides background information and frames the relationships between the variables we are interested in studying. Details on data sources and descriptive statistics are provided in Section 3. Section 4 explains the empirical strategy. Section 5 presents the results and discusses validity tests. Finally, the paper concludes in Section 6.

2. Background

To understand the relationship between crime and consumer choice we need to examine the roles of three key agents: consumers, offenders and businesses. These three agents act and take decisions endogenously based on observed conditions and by inferring the preferences and actions of the others. In this section we lay out only the aspects of an agent's behavior that are relevant to the relationship we are trying to measure. That is, we abstract from other nuances and complexities of these agents to the greatest possible extent.

2.1. Consumers

The criminology theory recognizes that a motivated offender, the presence of a suitable target and the absence of capable guardianship are essential elements for a criminal act to occur (Cohen and Felson, 1979). In awareness of these elements, citizens assimilate the risk of becoming a victim and change their actions which generate negative and positive externalities (Ayres and Levitt, 1998). The level of crime associated with a venue's location can affect consumers' choice on attending the business in two ways.

First, individuals may take under consideration the risk of being victims of crime while physically visiting an establishment. As crime victimization often imposes monetary and psychological costs, consumers may opt to avoid certain areas (Skogan, 1986; Levi, 2001). In fact, safety conditions have been a factor in short and long term life choices. Perception of violence has affected residential decision, reshaping American cities with the fleeing to the suburbs of families in search for safer surroundings (Cullen and Levitt, 1999). Regarding routine decisions, Janke et al. (2016) document that concerns about personal safety lead individuals to change their physical activity habits, while Mejia and Restrepo (2016) find that reduction in property crime increases households' consumption of conspicuous goods.

A secondary way by which crime can affect consumers is through the emotional experiences associated with the use of a service. Andreu et al. (2006) show that positive perceptions of a retail environment have a positive influence on emotions, repatronage intentions, and the desire to remain longer in the retail area. Thus, the perception of safety developed by the consumers while attending a business may affect their future decision in the extensive margin about returning or not to the establishment, or in the intensive margin by shorting their stay and possibly consuming less.

There are various ways individuals can learn about crime and develop their safety perceptions. Evidence suggests that personal crime victimization is directly related to the person's perceived risk (Dugan, 1999). Individuals can also assess their safety risk from observational elements (Broken Window Theory by Wilson and Kelling, 1982) or by learning from experiences of others. Given current technological tools, consumers of food and entertainment services have various means to learn about the experiences of other users, for example, through review websites such as Yelp and social media platforms such as Facebook and Twitter. Moreover, several cities offer the population access to crime maps synchronized to police reported incidents.[2] Local news broadcasting and neighborhood blogs are also common sources of information on local criminal events. Finally, consumers likely also become aware of criminal incidents by witnessing police presence due to a crime response.

Because individual safety perceptions are rarely observed, we instead exploit reported crime incidents, which can be used as proxy for safety level around a business. How reported crimes translate into individual perceptions and fears that nudge consumer choice is not what we focus on in this paper. The effect we are after - which is a crucial and relevant policy parameter - is the average effect that changes in the level of crime around a business impose on consumers' decisions, perhaps induced by the two channels discussed above and possibly amplified by individual perceptions.

2.2. Offenders

In Becker's seminal model (Becker, 1968) of illicit activity, would-be criminals face a trade off between the costs and benefits of committing an offense. Before acting, potential offenders weigh the probability of being caught and the severity of the punishment if arrested against the benefits of the crime. When benefits are greater than punishment weighted by the probability of apprehension, crime occurs.

In the context of Becker's model there are two direct ways consumer flows affect individuals' decision to commit a crime. First, consumers are potential victims. Places with more people offer more opportunities for criminals to strike. Second, the greater circulation of people in urban areas may also give a more diffused sense of social order or facilitate the disguise of illicit actions, decreasing the probability of apprehension (Glaeser and Sacerdote, 1999). On the other hand, the influx of consumers may bolster informal social control. More visitors mean more eyes to watch over the area, which raises the probability of apprehension and prevents crime from happening (Jacobs, 1961).

A tertiary way that consumer traffic can affect crime occurs when consumers become offenders. Gatherings may generate social conflicts, increasing the occurrence of incidents like assault, public disorder and vandalism.

It is evident that there is a circular relationship between consumer activity and crimes. Criminal actions in the surroundings of a venue may affect the expected utility of patronizing the location. At the same time, the flow of people generated by consumer traffic could impact criminal behavior.

2.3. Businesses

Crime can cause direct and indirect burden on business owners. They may directly suffer from offenses such as thefts and robberies, and spend on prevention and protective measures to increase private security. Crime could also affect businesses indirectly through potential decrease in revenues if crime scares consumers away, which is the focus of our study. Finally, venues may reallocate due to fear of crime.

A key aspect of businesses in this context is that they are not static economic agents. They adapt as a result of the macro and micro socioeconomic factors. Before starting operation, businesses decide where to locate based on proximity to demand and supply markets, inferring about consumer preferences, and assessing other local conditions like safety level. For instance, Abadie and Dermisi (2008) find that business activities were reduced in neighborhoods where the perceived threat of terrorist was higher. The sorting of certain business sectors into safer locations is confirmed in a detailed analysis by Rosenthal and Ross (2010).

Businesses not only react to crime, they may also contribute to it. As discussed before, venues attract crowds often targeted by criminals. On the other hand, small businesses may improve public safety by providing employment opportunities (Wilson, 1996). Moreover, at the neighborhood level, the decay or prosperity of stores may contribute to crime by changing social order impressions. Businesses can bestow positive spillovers by improving neighborhood amenities. Stacy et al. (2016) estimate the effect of neighborhood-level economic activity on crime holding residential characteristics constant. Their findings indicate that increases in economic activity are associated with reductions in property crime.

Finally, there are external factors that affect businesses and crime simultaneously. For instance, the opening of a rapid transit line nearby brings consumers, but also potential offenders (Phillips and Sandler, 2015). Public interventions in the local labor market may alter businesses' financial decisions regarding employment. At the same time changes in job opportunities affect potential offenders' trade off according to Becker's model. Overall, it is natural to observe a negative relationship between crime and local economic activity. Flourishing communities have prosperous venues and low violence. On the other hand, decaying neighborhoods often face violence surge and business deterioration.

[2] The crime map that reflects reported incidents of crime in Chicago over the past year minus the most recent seven days can be accessed here: https://data.cityofchicago.org/Public-Safety/Crimes-Map/dfnk-7re6.

In summary, it is not obvious whether the association between local crime and consumer activity in an urban environment should be positive or negative. This study thus invests a great effort addressing endogeneity problems in order to present reasonable estimates about consumers' reaction to criminal activity.

3. Data

Our analysis is based on two main data sources, the incident level crime data from the Chicago Police Department and point-of-interest visit data from SafeGraph, a company collecting foot-traffic pattern data from mobile devices. We combined the two data sources to form a longitudinal dataset of 14,893 venues in the city of Chicago for the time period from January 1st, 2017 to September 30th, 2019.[3]

Information on crime is drawn from the incidents in the Citizen Law Enforcement Analysis and Reporting provided by the Chicago Police Department and publicly available at the city of Chicago data portal.[4] The data include coordinates corresponding to the most proximate address to where a crime incident occurred. Each incident is then linked to a census block and consequently to a block group or tract. The data also report crime type description and its classification from FBI Uniform Crime Reporting program, as well as a brief description of the crime location, such as sidewalk, apartment and retail store. From the Chicago data portal we also collect information on business licenses and building permits in order to construct additional control variables.[5] The numbers of active building licenses and new building permits can be used as proxies for private investment.

Consumer visit data are provided by SafeGraph which collects foot-traffic pattern data to 3.6 million commercial points-of-interest from over 45 million mobile devices in the United States.[6] The population sample is a panel of opt-in, anonymized smartphone devices, and is well balanced across demographics and geographies of the United States (Squire, 2019). From this source we obtain daily level data on consumer visits to venues in food and entertainment industries. These venues are selected based on the North American Industry Classification System (NAICS) sector codes. They are in sector 44–45 (retail trade), sector 71 (arts, entertainment, and recreation) or sector 72 (accommodation and food services). We further restrict our sample by excluding venues that first show up later than January 1st, 2017 or last show up before September 30th, 2019 (910 venues are dropped due to this restriction). From our data source, we are not able to tell why these venues show up later or disappear earlier.

The venues covered by SafeGraph are collected using a combination of web crawling, publicly available APIs and third-party licensing.[7] SafeGraph covers not only a large number of urban venues, but also a great variety of venue categories (86% of all six-digit NAICS codes of the categories used in this study). The venues in our sample correspond to about 30% of the active retails according to business licenses issued by the city administration during the period of analysis.[8] Figure 1 compares the spatial distributions of active retail business licenses and establishments in our sample. There exists a high spatial correlation – the correlation between the % of venues by tract in our sample and % of active business licenses by tract is about 0.94. Overall, little evidence suggests that our sample of venues over- or under-represent a particular category of venues or a particular community in the city of Chicago.

Nevertheless, our sample is probably not representative of the overall United States consumer population, since smartphone ownership varies, for example, by age and income (Pew Research Center, 2019). Given that information on consumer profile is not available to us, we do not further investigate the direction of this bias.[9] Thus, estimates in this study are in the context of consumers who own smartphones and have agreed to sharing their location.

3.1. Data aggregation

Similar to other empirical work in the crime literature (Jacob et al., 2007; Freedman and Owens, 2016; Caetano and Maheshri, 2018), we face three aggregation choices before estimation: how to classify crime incidents, how to define local neighborhoods and how to choose time periods over which we construct crime rates. They are important decisions that directly determine the model we estimate and are inseparable from our empirical strategy.

Theoretically, more disaggregated classification of crimes is preferred because it better exploits crime heterogeneity and contains treatment effects that are sharply interpretable. However, as number of parameters to estimate grows, it is increasingly more difficult to control for unobserved confounders. Apart from that, more disaggregated crimes are usually not precisely measured and have little variation over time. Considering the argument above and the fact that consumers' response to crime is influenced by how salient the crime is, we classify crime incidents into two relatively heterogeneous sets: one by type and the other by location. The first set includes violent, property and light crimes categorized using crime types provided by the FBI Uniform Crime Reporting program.[10] The second set contains six categories based on where a crime incident occurs: crime in streets, crime in residences, crime in parking or public transportation areas, crime in venues, crime in vehicles and crime at gas stations. These crime categories tend to be accurately reported and have relatively high variations.

With datasets detailed in high geographical dimension, we face many neighborhood choices, however, there is no clear criterion for the most appropriate one. On the one hand, we would like neighborhoods to be coarsely defined to account for spatial spillovers. For instance, considering a census block as a neighborhood may be too fine, because the effect of a crime may spill over to nearby blocks. On the other hand, we do not want our neighborhoods to be too large. The causal influence from a crime tends to remain close to where the crime occurs. Defining neighborhoods too coarsely may underestimate the effect of crimes in a neighborhood on the consumer visits of a venue located in the same neighborhood. Considering the trade-off, we define a block group as a neighborhood.[11] A block group in the city of Chicago has about 20 cen-

[3] We choose this time period because it is the range we have SafeGraph data.

[4] The Chicago Data Portal (2001).

[5] Business licenses that were active during our period of analysis were issued by the Department of Business Affairs and Consumer Protection. This dataset provides rich information on business exact location and their sector of activity. Building permit data were obtained from the Chicago Department of Buildings and provide information on the address of the issued permit and type of permit (new building, renovation or demolition).

[6] SafeGraph (2017–2019).

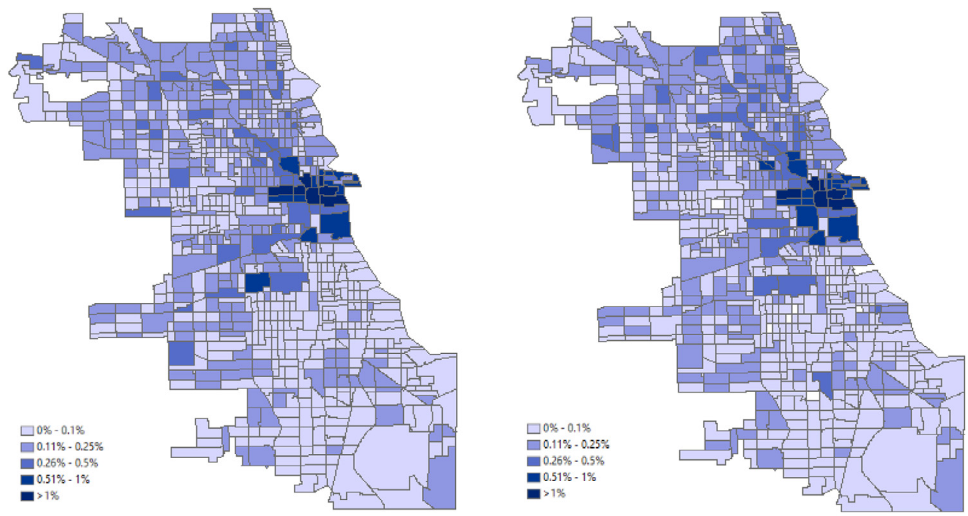
[7] Detailed information is available at https://www.safegraph.com/blog/safegraphs-data-sourcing-process. Summary statistics of the venues covered by SafeGraph are available at https://docs.safegraph.com/docs/places-summary-statistics.

[8] There is a mismatch between NAICS classification and the classification used in the business license data. To compare active retail business licenses to the type of businesses considered in our sample we count licenses for the following business activities in the administrative dataset of the city of Chicago: Retail Food Establishment, Music and Dance, Wholesale Food Establishment, Tavern, Performing Arts Venue, Public Place of Amusement, Regulated Business License, Limited Business License and Pet Shop.

[9] To better understand the potential bias caused by the opt-in sample, Safegraph performs a variety of analyses which suggest that their data do line up with Census data across multiple geographic and demographic dimensions, e.g. state, county, race and household income. More detail is available at https://colab.research.google.com/drive/1u15afRytJMsizySFqA2EPlXSh3KTmNTQ

[10] Violent crimes include aggravated assault, sexual assault, robbery and homicide. Property crimes include arson, burglary, motor vehicle theft and theft. Light crimes include criminal trespass, public peace violation, liquor law violation, stalking, gambling, intimidation, obscenity, non-criminal public indecency, non-criminal weapons violation and interference with public officer.

[11] A typical block group in Chicago is at least 1.25 miles (2km) in distance (about 20 min walk at a moderate pace).



(a) Active business licenses (b) Venues in our sample

Fig. 1. Spatial Distribution of Active Business Licenses and Venues in Our Sample. *Note:* The plots present the proportion of venues in the census tract from the total number of establishments in each dataset.

sus blocks and an average population of 1200 inhabitants in 2010. There are approximately 2000 block groups in Chicago whose average area is 0.15 square miles.[12]

There is a similar trade-off when aggregating time. Consumers' response to crime tends to be in the short run immediately after a crime's occurrence. Thus, using a large time period, say a year, would be inappropriate. However, if the time period is too short, within area crime may have little variation. In addition, local crime serves as a measure of fear and perceived risk of victimization which are difficult to develop or change within a very short time period, such as a week or a day, if there are no extreme crime events (e.g. homicide). Given the trade-off, we use a month as time period dimension. This allows us to capture the local impact of crime in a relatively short time period of its occurrence. At the same time enough variation in local crime likely remains, even after accounting for time varying confounders.[13]

In conclusion, we associate criminal activities to a venue based on monthly numbers of crime incidents of different categories that occur in the census block group in which the venue is located.

3.2. Descriptive statistics

Table 1 provides basic summary statistics for the main variables in our study. These variables are averages across venues over the study period (Jan 2017-Sep 2019). The first panel presents venue level information. In a given month a venue in our sample has about 315 consumer visits. The standard deviation (805) indicates that there is a large variance in the number of visits across businesses and months. Comparing the number of consumers to consumer visits we find that, on average, 61% (193/315) of the visits in a given month is from unique consumers. Consumers spend a median time of 46 min in a venue visit. The second panel of Table 1 provides information for variables measured at the block group level. On average a block group has about eight venues

Table 1
Summary Statistics.

	Mean	St.D
At venue level		
Consumer visits	315.49	805.47
Number of consumers	193.69	454.32
Night popularity†	334.04	1615.54
Day popularity†	613.13	2211.94
Median venue dwell time (minutes)	45.63	60.68
At block group level		
Property crime	13.78	33.66
Violent crime	4.94	8.52
Light crime	1.07	2.31
Crime in streets	6.32	9.90
Crime in residence	3.28	3.11
Crime in parking/transp. areas	2.52	8.41
Crime in venues	8.53	23.58
Crime in vehicles	0.35	0.72
Crime at gas stations	0.18	0.71
Building permits	10.41	32.71
Building violations	6.58	12.19
Business licenses	6.87	19.83
Number of block groups	1874	
Number of tracts	765	
Number of observations	475,290	

Notes: The characteristics presented in this table are averages across venues over the study period (Jan 2017- Sep 2019). †Popularity is measured using number of visits seen in each hour of the day. If a consumer stays in a venue for multiple hours, her stay will be counted multiple times, one for each hour. Night popularity is the sum of hourly popularity between 7pm-7am. Day popularity is the sum of hourly popularity between 7am-7pm.

(14,893/1874). In a given month, a venue's block group has 27.54 offenses on average, from which about a half is classified as property crime and about one fifth is classified as violent crime. Unsurprisingly a large proportion of neighborhood incidents occur in streets (23%) and residences (12%). Notably, on average 8.53 crimes, about 30% of total crimes, happen in commercial establishments. The table ends by displaying our sample size which includes 475,290 venue-month observations.

4. Empirical strategy

Our goal is to identify the average effects of different local crimes on consumer visits, which we denote by the vector β. Each element of β represents the effect of a crime of a classified category (e.g. property

[12] We perform sensitivity analysis by expanding block group borders using 500 m (0.3 mile) buffers and re-estimate our equation including crimes within the buffer. Our findings remain unchanged. Results of this exercise are available in Tables B.1 and B.2 of online Appendix B.

[13] We also analyze consumer response to local crime using week as time period. Our results by crime location are very similar. But after adding the fixed effects introduced in Section 4, the coefficients by type of crime become insignificant and close to zero, which indicates that we may not have enough variation left to estimate the parameters of interest. These results are available in Tables B.3 and B.4 of online Appendix B.

crime). In light of the discussion in the Section 2, we should suspect that a simple regression of consumer visits on local crimes would return a biased estimate of β. Unobserved determinants of consumer visits that are also correlated with crime, such as consumers' sorting, neighborhood trends and venue's location, would make us fail to identify the effects of interest. A standard solution is the use of instrumental variables. Jacob et al. (2007), for instance, employ weather shocks to instrument lagged crime rates. This type of instrument would only work for large geographic areas, not within-city neighborhoods, and it is unlikely valid in our context since weather shocks affect consumer activity trends. Crimes of multiple categories are included in our model, thus at least the same number of instrumental variables would be required, which makes the instrumental variable approach less practical.

A model that includes venue fixed effects and time fixed effects would deal with unobservables such as business heterogeneity and citywide trends. However, it would not be enough to address time variant confounders at a smaller geographical unit than city. On the one hand, at a fine geographic level, foot-traffic creates a positive simultaneity bias between certain types of crime and consumer visits. This is in particular true for theft and robbery (due to their characteristic of opportunistic crime), as well as light crimes such as vandalism, simple assault and public peace violation which are offenses often derived from social gatherings. On the other hand, neighborhood socioeconomic trends pose a negative association between criminal and business activities. Prosperous areas normally experience new businesses opening and also public safety improvements. Thus, underlying trends on local socioeconomic profile is likely to introduce negative biases in estimates from standard models with only venue fixed effects and time fixed effects.

To identify the parameter of interest, we leverage on longitudinal and geographic variations as shown in the following equation. The equation illustrates the reduced form relationship between consumer visits and local crimes.

$$Visit_{t,k(j)} = \sum_{w=1}^{W} \beta^{w} Crime^{w}_{t-1,j} + \alpha Visit_{t-1,j} + \delta_{T,j} + \delta_{t,J} + \delta_{k(j)} + \epsilon_{t,k(j)} \quad (1)$$

The outcome variable $Visit_{t,k(j)}$ is the number of consumer visits to venue k in a given month t. Venue k is located in block group j. β^w captures the effect of a crime of category w on future consumer visits. Our parameter of interest is denoted by $\beta = (\beta^1, \beta^2, \dots, \beta^W)$ if there are W crime categories. δ denotes fixed effects and ϵ represents random shocks and other unobserved factors.

Our identification strategy starts with the intuition that the impacts of crime occur at fine temporal and geographical levels, whereas most confounders only vary at fine temporal or geographical levels, but not both (Caetano and Maheshri, 2018). While the causal response to a crime will likely remain close to the scene of the crime and be strongest in the time period immediately following when the crime occurs, confounders tend to vary at more aggregated levels in at least one of the two dimensions. For example, weather varies rapidly but affects nearby neighborhoods similarly, and localized confounders such as neighborhood demographic composition change relatively slowly over time.

In light of this, we specify fixed effects varying at different temporal and geographical levels from our variables of interest, which are measured monthly and at the block group level (i.e. neighborhood). Specifically, we include two fixed effects: *i*) block group by year, $\delta_{T,j}$ where T denotes year, and *ii*) tract by month, $\delta_{t,J}$ where J denotes census tract. Block group-year fixed effects absorb neighborhood-specific confounders that change more slowly than crime. This component accounts for land use patterns and local gentrification that could affect crime and consumer visits.[14] Block group-year fixed effects also control for business composition of a neighborhood. Tract-month fixed effects capture all time-varying unobservables that vary at a larger geographic area than a block group. That is, they account for all short term time variant factors that affect consumer visits and crime at the census tract level such as weather conditions and events like parades or sport competitions. This component also absorbs all tract-level trends in law enforcement, public interventions and so on. On average a collection of three block groups forms a census tract. Analysts have customarily used data aggregated at the census tract level to characterize areas differentiated by public service provision or socioeconomic composition (Goodman, 1977).

$Visit_{t-1,j}$ controls for past number of visits at the block group level. It addresses simultaneity bias from foot traffic that generates crime and correlates with consumer visits. For instance, suppose venues located in the same block group promote an event at $t-1$ to attract customers. Foot traffic change due to the event is likely to affect crime by increasing social interactions and the pool of victims. Without controlling for lagged foot traffic, this change in local crime at $t-1$ would be treated as exogenous, which clearly is not the case in this example.

Finally, as in standard approaches, we include venue fixed effects $\delta_{k(j)}$ which account for any time invariant characteristics of a venue, such as business size and industry category. Therefore, based on our identification strategy, the only potential confounders that could bias our estimates would have to vary across months within a year and across block groups within a census tract.

Eq. (1) aims to measure how short term changes in crimes around a venue's location affect business through consumers' sensitivity to safety conditions. Moreover, the model explores crime heterogeneity and allows us to identify particular categories of crime that influence consumers the most, which provides effective policy suggestions. Nevertheless, because our identification strategy relies on consumers' response and number of criminal incidents being temporally and spatially dynamic, we are not able to identify how changes in city-level crime affect businesses.

5. Results

5.1. Main results

In this section we present and interpret the baseline findings of the paper. Assessments of the results' validity are discussed in Section 5.2.

Table 2 displays our estimations progressing from the raw relationship between crimes of different types and consumer visits to our preferred specification. The results from a naive linear regression shown in column (1) tell us that there exists a positive association between all types of crime, i.e. property, violent and light, and number of consumer visits. The coefficients of property and light crimes are statistically different from zero which sustains the argument about positive bias due to local foot-traffic. The inclusion of lagged number of visits at the block group level in column (2) flips the sign of the coefficients of property and light crimes and substantially improves the explanatory power of the model. The lagged term controls for positive bias due to foot traffic as well as heterogeneity across neighborhoods by accounting for different levels of business activities.

In column (3) we add block group-year fixed effects to further account for neighborhood differences across venue locations. The fixed effects absorb local conditions that change annually, such as local urban development or gentrification. As discussed previously, improvements in the socioeconomic profile of the area around a venue are likely to introduce negative bias in our estimates, because the area will usually experience growth in local businesses and reduction in crime. The decrease in magnitude of property crime's coefficient supports this argument. Estimates for violent and light crimes remain statistically insignificant.

Column (4) displays the results from a specification that adds census tract-month fixed effects, which absorb all factors changing monthly within a census tract. The effects of violent and light crimes remain statistically insignificant, while property crime is statistically relevant to explain consumer visits at 5% significance level. The precision loss, in terms of getting larger standard errors, is expected since census tract-

[14] Twinam (2017) found that commercial uses lead to more street crime in their immediate vicinity.

Table 2
Main Results I - Crime by Type.

	(1)	(2)	(3)	(4)	(5)	(6)
Property crime ($t-1$)	1.04***	−6.59***	−1.79***	−1.36**	−1.13**	−1.01**
	(0.34)	(1.66)	(0.55)	(0.65)	(0.56)	(0.49)
Violent crime ($t-1$)	0.30	0.26	0.22	−0.40	−0.36	−0.43
	(1.77)	(3.49)	(1.07)	(0.95)	(0.87)	(0.71)
Light crime ($t-1$)	14.11***	−3.37	4.36	2.51	1.81	1.75
	(5.39)	(7.46)	(3.11)	(3.21)	(2.90)	(2.58)
R-squared	0.01	0.02	0.12	0.16	0.46	0.46
Observations	475,290	475,290	475,290	475,290	475,290	475,290
Lagged block group visits	×	✓	✓	✓	✓	✓
Block group × year FE	×	×	✓	✓	✓	✓
Tract × month FE	×	×	×	✓	✓	✓
Venue FE	×	×	×	×	✓	✓
Controls	×	×	×	×	×	✓

Notes: This table presents the estimation results for various specifications using three types of crimes. Standard errors, shown in parentheses, are clustered at the block group by year level. Controls in column (6) include median distance from home by visitors, median venue dwell time, number of building permits issued and number of active business licenses in the venue's block group, and lagged property, violent, light crimes in a venue's nearest adjacent neighborhood. $^{*}p<0.1$, $^{**}p<0.05$, $^{***}p<0.01$.

month fixed effects are likely to absorb a large proportion of the variation in local crimes.

Column (5) reports estimates from a specification with the full set of controls as described in Eq. (1). We find that one additional property crime in the block group where the venue is located decreases consumer visits by 1.13 in a given month. In standard deviation term the interpretation is that one standard deviation (33.3) increase in property crimes results in 37.63 (-1.13×33.3) fewer consumer visits, about 12% (37.63/315) reduction in the average number of visits per venue.

Finally, in the last column of Table 2 we add several control variables at the block group-month level to control for biases due to other confounders varying at the same geographic and temporal level as our variables of interest. First, we include monthly building permits and number of active business licenses (proxies for private investment as in Lacoe et al., 2018) to account for any remaining unobserved economic factors. Median travel distance from home by visitors and median venue dwell time are added to control for venue attractiveness. We also include lagged crime of each category in a venue's nearest adjacent neighborhood to alleviate the concern that effects of crime may spill over to adjacent neighborhoods if our neighborhoods are defined too narrowly. As shown in column (6), our results are robust to these additional controls.[15]

Table B.5 in the online Appendix allows us to better interpret the significant effect of property crime by breaking it into more disaggregated crime types, i.e. burglary, motor vehicle theft, theft and arson. The coefficients of burglary, motor vehicle theft and arson are not statistically significant to explain consumer visits. Theft is driving the result behind property crime.

Table 3 follows the same sequence of specifications as Table 2 to present the estimates of crimes by place of occurrence. In particular, due to fear of victimization, we would expect consumers to be more sensitive to variation in outdoor crimes rather than residential incidents. From the naive regression reported in column (1) we see that there exists a positive and statistically significant association between consumer visits and incidents happening in streets and at venues. Again, that is likely due to foot-traffic.

Once lagged block group visits is added on the right hand side of the regression, point estimates change considerably and all the coefficients become statistically relevant. However, after we add block group-year and tract-month fixed effects to further address other confounders we see that only crime happening in streets survives. In particular, from our preferred specification in column (5), we conclude that one additional crime in the streets of the block group where the venue is located results in 3.03 fewer consumer visits in the following month, about 1% reduction in the average number of visits per venue. In standard deviation term, it is 30 (-3.03×9.90) fewer consumer visits and 10% (30/315) reduction with one standard deviation increase in street crimes. Moreover, according to column (6), this finding is robust to the additional controls at the block group-month level.

To better interpret the effect of street crime, we further disaggregate it based on crime type, i.e. theft, battery/assault, motor vehicle theft, robbery, criminal damage and other crime in streets. Table B.6 in the online Appendix presents the results. Theft and battery/assault are the types of crime in streets that have a negative and statistically significant effect on consumer visits.

5.2. Validity tests

This section presents two tests to support the validity of the identification strategy. First, we confirm that our estimates are not likely to suffer from endogeneity via an exogeneity test developed by Caetano (2015) and subsequently used by Caetano and Maheshri (2018) and Caetano et al. (2019). Then, we perform a falsification test using incidents that happened in residences and are classified as domestic-related by the Illinois Domestic Violence Act.[16]

The recently developed test of exogeneity (Caetano, 2015) yields an objective statistical criterion for whether the parameters of interest in an empirical model can be interpreted as causal. The test requires that unobservables vary discontinuously at a known threshold of the explanatory variable of interest, which often happens when observations bunch at this threshold. In the context of this paper, such discontinuities exist at the zero crime threshold.[17] For example, neighborhoods with zero violent crime tend to be so wealthy, safe or heavily patrolled by police that their violent crime would stay at zero even if they were slightly poorer, more dangerous or less policed. Additionally, because violent crime cannot be negative, these unobserved neighborhood characteristics tend to accumulate at zero. As a result, neighborhoods with zero violent crime

[15] Replacing crimes in the nearest adjacent neighborhood with crimes in the three nearest adjacent neighborhoods barely changes our estimates in column (6) of Table 2.

[16] We also perform a test for causality in the spirit of Granger (1988) in which we check whether future crime predicts number of consumer visits in the current period. As desired, the coefficients on the leading variables are jointly statistically zero. The results are in online Appendix A.

[17] For instance, neighborhoods with five violent crimes in the previous month are similar to those with four violent crimes in the previous month. Furthermore, neighborhoods with four violent crimes in the previous month are similar to those with three violent crimes in the previous month, and so on. However, the notion of similarity breaks down at zero violent crime.

Table 3
Main Results II - Crime by Location.

	(1)	(2)	(3)	(4)	(5)	(6)
Streets ($t-1$)	2.96***	−5.15***	−2.69*	−3.43***	−3.03***	−2.82**
	(1.06)	(1.15)	(1.48)	(1.24)	(1.15)	(1.10)
Residence ($t-1$)	0.83	4.86***	1.28*	0.43	0.45	0.39
	(1.64)	(1.36)	(0.67)	(0.79)	(0.69)	(0.68)
Parking/transp. areas ($t-1$)	−1.92	−22.79***	−6.11**	−6.67	−5.81	−4.90
	(1.39)	(2.88)	(2.80)	(4.24)	(3.77)	(3.29)
Venues ($t-1$)	2.24***	−3.87***	−0.32	−0.22	−0.08	0.10
	(0.81)	(1.26)	(0.78)	(1.03)	(0.90)	(0.93)
Vehicles ($t-1$)	−2.57	15.54***	3.49	2.90	3.58	2.84
	(4.11)	(5.97)	(3.12)	(3.24)	(3.04)	(2.69)
Gas stations ($t-1$)	−4.83	11.61***	1.92	−5.05	−5.27	−5.55
	(4.42)	(4.33)	(2.69)	(3.59)	(3.42)	(3.58)
R-squared	0.01	0.03	0.12	0.16	0.46	0.46
Observations	475,290	475,290	475,290	475,290	475,290	475,290
Lagged block group visits	×	✓	✓	✓	✓	✓
Block group × year FE	×	×	✓	✓	✓	✓
Tract × month FE	×	×	×	✓	✓	✓
Venue FE	×	×	×	×	✓	✓
Controls	×	×	×	×	×	✓

Notes: This table presents the estimation results for various specifications using crimes occurred at different locations. Standard errors, shown in parentheses, are clustered at the block group by year level. Controls in column (6) include median distance from home by visitors, median venue dwell time, number of building permits issued and number of active business licenses in the venue's block group, and lagged crime of each category in a venue's nearest adjacent neighborhood. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4
Exogeneity Test Results.

	(1)	(2)	(3)	(4)	(5)	(6)
Crime by type	23.33	50.34	**0.27**	**0.26**	**0.25**	**0.24**
	(0.00)	(0.00)	**(0.85)**	**(0.85)**	**(0.86)**	**(0.87)**
Crime by location	25.17	42.99	6.70	1.85	**1.55**	**1.54**
	(0.00)	(0.00)	(0.00)	(0.09)	**(0.16)**	**(0.16)**

Notes: This table presents the F-statistic and corresponding p-value (in parentheses) of the exogeneity test developed by Caetano (2015) for each specification in Tables 2 and 3. Entries in bold denote "surviving specifications" for which we cannot reject exogeneity at 10% significance level. All standard errors are clustered at the block group by year level.

are likely discontinuously different from neighborhoods with barely positive amounts of crime. To test whether such unobserved heterogeneity exists, we exploit the idea that crime varies continuously from say, five incidents down to zero, while unobservables correlated with crime vary discontinuously at zero. If any of these discontinuous unobservables are incorrectly omitted from our specification, the dependent variable (in our case consumer visits) will vary discontinuously at zero, leading us to reject the null hypothesis that our parameters of interest are causal.[18]

To implement the test, we create an indicator variable $d^{w}_{t-1,j}$ for each $Crime^{w}_{t-1,j}$ that is equal to one if $Crime^{w}_{t-1,j} = 0$. Then we add these indicator variables as regressors on the right-hand side of Eq. (1). The coefficient associated to $d^{w}_{t-1,j}$ represents the size of the discontinuity at $E[\text{Visit}_{t,k(j)}|\text{Crime}^{w}_{t-1,j} = 0, \text{Crime}^{-w}_{t-1,j}, \text{Controls}]$ where $\text{Crime}^{-w}_{t-1,j}$ denotes all other types of crimes in Eq. (1). Finally we implement an F-test on whether the coefficients of $d^{w}_{t-1,j}$ for $w = 1, \dots, W$ are jointly zero, which is equivalent to testing whether Assumption 1 in Caetano (2015) holds.

Table 4 presents the exogeneity test F-statistic and corresponding p-value (in parentheses) for each specification we consider in Tables 2 and 3. The F-statistics and p-values in bold present the surviving specifications, that is, specifications we are unable to reject exogeneity at the 10% significance level. Columns (5) and (6) pass the test regardless of how we categorize crimes, by type or place of occurrence. In light of the test results we consider the specification in column (5) of Tables 2 and 3 as our preferred one and use it to investigate variation in crime effects in Section 5.3.

Our next effort in verifying the validity of our approach involves a falsification test using domestic violence in residences. These types of crimes are correlated to neighborhood unobservable trends, but they should not directly impact consumers' decision about venue visits. For our preferred specification the estimated effect of domestic violence in residences on consumer visits is not statistically significant (results available in Table B.7 of online Appendix B). This result lends additional credibility that our empirical strategy is not capturing spurious correlation between neighborhood trends and consumer activity.

5.3. Variation in crime effects

In this section we investigate in greater detail the effects of crime on consumer behavior. First, we consider four alternative outcomes, number of unique visitors, venue's night popularity, venue's day popularity and consumer venue dwell time, all of which help us better understand how consumers respond to a variety of local crimes. Then we present heterogeneous results by types of venue. Finally, we examine whether consumers' sensitivity to crimes differ by level of criminal activity in the venue's neighborhood, and whether consumers respond asymmetrically to neighborhood crime trends.[19]

Estimation results for alternative outcomes are presented in Table 5. For each outcome, estimation with crimes by type and crimes by location are performed separately. The results also pass the validity tests implemented for the main results as described in Section 5.2.[20] Number

[18] Failing to reject the null hypothesis of exogeneity for a specification does not guarantee the specification is exogeneous. We systematically study the power of the test in this context. Our empirical evidence suggests that the specifications passing the test likely provide causal effects of local crimes on consumer visits. The empirical evidence is available upon request.

[19] We also explore heterogeneous estimates on whether or not a venue is located in a Business Improvement District (BID). In Chicago, BID consists of a program called Special Service Areas with about 50 areas where projects such as security services, advertising assistance, or any variety of small scale capital improvements are supported through a modest property tax levy. We do not find statistically significant evidence that the impacts of crimes on consumer visits differ whether a venue is located in a Special Service Area or not. Results are available upon request.

[20] Results of Caetano (2015)'s exogeneity test using the four alternative outcomes are presented in Table B.8 of online Appendix B. The estimates for crimes by location with venue's day popularity as the outcome has a p-value 0.09 in our

Table 5
Alternative Outcomes.

Outcome variable:	(1) Unique Consumers	(2) Night Popularity	(3) Day Popularity	(4) Dwell Time
Crime by Type				
Property crime $(t-1)$	−0.63**	−5.04**	−6.53**	−0.02
	(0.31)	(2.44)	(3.16)	(0.02)
Violent crime $(t-1)$	0.01	−4.30	−4.83	−0.03
	(0.47)	(3.44)	(4.58)	(0.05)
Light crime $(t-1)$	1.26	10.31	13.57	0.03
	(1.73)	(10.08)	(13.98)	(0.08)
Crime by Location				
Streets $(t-1)$	−1.57***	−10.92***	−14.33***	−0.02
	(0.60)	(4.15)	(5.52)	(0.02)
Residence $(t-1)$	0.28	−0.75	−0.86	0.06
	(0.39)	(1.60)	(2.12)	(0.04)
Parking/transp. areas $(t-1)$	−3.55	−23.73*	−31.81*	−0.10*
	(2.17)	(13.75)	(18.69)	(0.06)
Venues $(t-1)$	0.00	−2.01	−1.58	−0.09***
	(0.53)	(3.62)	(4.90)	(0.02)
Vehicles $(t-1)$	1.71	6.76	11.71	−0.05
	(1.72)	(8.80)	(11.73)	(0.15)
Gas stations $(t-1)$	−2.96	−18.12**	−22.77*	−0.04
	(1.80)	(9.14)	(11.96)	(0.15)
Observations	475,290	475,290	475,290	475,290

Notes: For each alternative outcome, two models are estimated using crimes by type and crimes by location respectively. Results presented here are based on the specification with block group year, tract month and venue fixed effects (i.e. column (5) in Tables 2 and 3). Popularity in columns (2) and (3) is measured using number of visits seen in each hour of the day. If a consumer stays in a venue for multiple hours, her stay will be counted multiple times, one for each hour. Standard errors, shown in parentheses, are clustered at the block group by year level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

of unique consumers is the outcome in column (1) of Table 5. Similar to the results for number of visits, the coefficients of property crime and crime in streets are negative and statistically significant. In particular, one additional street crime in the previous month implies 1.57 fewer consumers on average. Given that venues in our sample on average have about 194 customers monthly, one more crime in streets nearby reduces the number of consumers by 0.8% (−1.57/193.69). Because we cannot reject that this effect is statistically equal to the street crime effect on number of visits, we can infer that crime is bad for business in reducing overall number of consumers, not necessarily by reducing patronage. That is, if crime were to affect businesses mostly by reducing number of trips (but not lessening the total of customers) we should have observed an asymmetry in its effects on number of visits and on number of consumers, which is not the case.

Venue's popularity in columns (2) and (3) is measured using number of visits seen in each hour of the day. If a consumer stays in a venue for multiple hours, her stay will be counted multiple times, one for each hour. As a consequence, given the same time range (e.g. one day), popularity is likely to be greater than number of visits. Considering the mean day and night popularity levels in Table 1, the relative impact for day popularity is smaller. For instance, one additional crime in streets in the previous month reduces day popularity in the next month by 2.3% (−14.33/613.13) on average, whereas its impact on night popularity is 3.3% (−10.92/334.04). These effects are statistically different at 5% significance level. These results go in line with the narrative in the behavioral economics literature that individuals' choices are sensitive to environmental conditions. Using random allocation of street lights to public housing developments, Chalfin et al. (2019) find evidence that areas assigned more lighting experienced sizable reductions in crime. Our finding is parallel to theirs in the sense that the safety perception, and therefore reaction to it, is a monotonic function of street brightness level.

The extensive margin by which crime affects consumers' decision in going to a certain location is given by the results on number of visits and number of consumers. In order to study whether crime also impacts consumers in the intensity margin, measured by the amount of time spent in a location, in column (4) of Table 5 we present crime effects on consumers' median dwell time to a venue (in minutes). Interestingly, the significant coefficients come from crime in venues and in parking or transportation areas. Property crime and crime in streets no longer have a significant impact on the outcome. This is consistent with the narrative in the retail environment and consumer behavior literature (Andreu et al., 2006) that positive perceptions of a retail environment have a positive influence on the desire to remain in the store longer. However, the size of the impact is fairly small. One additional crime in venues decreases the length of median dwell time by 0.20% (−0.09/45.63).

Table 6 presents estimates by business category according to industry classification.[21] The main findings for property and street crimes remain unchanged for food and retail establishments. Interestingly, violent crime has a negative effect on accommodation businesses and beauty salons. Criminal activities in parking or transportation areas have a large detrimental effect on visits to retail stores.[22]

To explore effects across block groups with different initial crime levels, we create a dummy variable to interact with local crimes based on the median census tract crime rate at the beginning of the analytical time period (i.e. January 2017). Census tract crime rate is defined as the ratio of number of crimes to number of venues. The results, presented in Table 7, show that consumers respond to different local crimes in areas with different crime rates. Specifically, consumers react to property crime for venues located in low crime (below median) neighborhoods and to violent crime for venues located in high crime (above median) neighborhoods. In low crime areas, violent crimes (such as robbery and

preferred specification, however, the Granger test suggests that these estimates are likely causal. Results using the Granger test are available upon request.

[21] We add to our sample hair, beauty and nail salons (374 venues), a service sector with predominantly female visitors, to proxy for asymmetric gender reaction to criminal activities.

[22] At 5% significance level, we reject that the effect of violent crimes on beauty salon visits is the same as the effect on visits to venues in other industries. Assuming that the difference in estimates are due to the majority of beauty salon clients being female, these findings suggest that an increase in violent crime translates into a larger drop in consumer activity for women. This conclusion is consistent with previous work in the literature that finds women's attitude toward perceived crime to be more sensitive than men's (Hipp, 2010).

Table 6
Heterogeneous Results: Type of Venues.

	Accommodation	Food	Entertainment	Retail	Beauty Salon
Crime by Type					
Property crime $(t-1)$	−0.54	−1.31**	−0.59	−1.12**	−1.79**
	(0.85)	(0.61)	(0.63)	(0.48)	(0.74)
Violent crime $(t-1)$	−3.95*	−0.75	0.12	0.46	−2.08***
	(2.05)	(0.96)	(1.70)	(0.84)	(0.55)
Light crime $(t-1)$	−2.63	1.09	13.88**	0.67	−0.82
	(4.91)	(3.11)	(6.77)	(2.88)	(1.45)
Crime by Location					
Streets $(t-1)$	−0.26	−2.99***	−1.28	−3.53***	−1.83**
	(1.85)	(1.15)	(1.36)	(1.22)	(0.75)
Residence $(t-1)$	1.19	−0.39	0.35	1.42	−0.77
	(2.35)	(0.73)	(1.60)	(1.17)	(0.50)
Parking/transp. areas $(t-1)$	−2.24	−5.32	−3.91	−7.31*	0.42
	(3.93)	(3.75)	(3.78)	(3.79)	(1.67)
Venues $(t-1)$	−0.05	−0.37	0.22	0.31	−3.45***
	(1.05)	(0.93)	(1.01)	(0.92)	(1.07)
Vehicles $(t-1)$	3.41	3.96	−2.95	4.64	−4.18*
	(8.14)	(3.27)	(4.48)	(3.39)	(2.23)
Gas stations $(t-1)$	−10.90	−6.14	−4.59	−4.36	5.54**
	(6.80)	(3.97)	(5.48)	(3.17)	(2.82)
Observations			487,226		

Notes: The table shows results of two regressions: consumer visits on crimes by type, and consumer visits on crimes by location. The variables of interest are interacted with dummies of the venue type. In our sample there are 258 venues in the accommodation, 7052 venues in food, 1556 in entertainment, 6027 in retail, and 374 in service-beauty salon. Results presented here are based on the specification with block group year, tract month and venue fixed effects (i.e. column (5) in Tables 2 and 3). Standard errors, shown in parentheses, are clustered at the block group by year level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7
Heterogeneous Results by Neighborhood Crime Level and Neighborhood Crime Trend.

	Neighborhood Crime Profile		Neighborhood Trend in Crime	
	Low	High	Decrease	Increase
Crime by Type				
Property crime $(t-1)$	−1.22**	1.95	−0.05	−1.29**
	(0.58)	(1.42)	(0.64)	(0.65)
Violent crime $(t-1)$	0.01	−1.78**	−1.05	−0.02
	(1.05)	(0.81)	(0.84)	(1.21)
Light crime $(t-1)$	2.18	−0.33)	−1.58	3.07
	(3.71)	(1.14)	(1.39)	(4.09)
Crime by Location				
Streets $(t-1)$	−3.58***	−0.39	−0.61	−5.77***
	(1.37)	(1.22)	(0.56)	(1.82)
Residence $(t-1)$	0.97	−0.82*	−0.49	1.54
	(0.96)	(0.47)	(0.49)	(1.35)
Parking/transp. areas $(t-1)$	−6.56*	9.50	2.76	−8.56**
	(3.87)	(6.48)	(2.11)	(4.31)
Venues $(t-1)$	0.08	−5.69**	−2.47**	0.39
	(0.91)	(2.42)	(0.96)	(0.93)
Vehicles $(t-1)$	5.44	−1.45	−1.25	3.58
	(3.81)	(2.95)	(1.93)	(4.45)
Gas stations $(t-1)$	−11.58**	2.20	−1.48	−9.12
	(5.85)	(1.89)	(2.52)	(5.93)
Observations	475,290		475,290	

Notes: The table shows results by initial crime level (first two columns) and by tract crime trend over study period (last two columns). The top and bottom panels show results of separated regressions: consumer visits on crimes by type, and consumer visits on crimes by location. The variables of interest are interacted with a dummy variable about initial level of total crime in the venue's tract in the first two columns and in the last two columns the dummy variable is whether crime increased overall during the study period in the venue's tract. Results presented here are based on the specification with block group year, tract month and venue fixed effects (i.e. column (5) in Tables 2 and 3). Standard errors, shown in parentheses, are clustered at the block group by year level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

homicide) occur less frequently and more idiosyncratically. Therefore, they play less of a role in a consumer's decision to visit a venue in a low crime neighborhood and more of a role in a high crime neighborhood. In other words, consumers are less likely to associate themselves with victims of violent crimes in low crime neighborhoods. Outdoor crimes including those in streets, in parking or transportation areas and at gas stations, where majority of property crimes (such as theft) occur, have significant and negative effects on consumer visits in low crime areas. Crimes in venues impact consumer visits in high crime areas.

Table 7 also reports heterogeneous results by crime trend in the venue's location. We group census tracts based on whether crime increased or decreased overall during the study period and check consumers' responses to crime changes accordingly. The results show that for venues located in neighborhoods facing increase in criminal activity, consumers' response to the increase is large, negative and significant and concentrates on property crime and crimes in streets and parking/transportation areas. On the other hand, for venues in places facing overall reduction in crimes, consumers are not sensitive to changes in outdoor crimes, but they do react to crime occurring in venues. These findings are mostly consistent with the intuition that crime occurrence is more salient than lack of it. The significant negative effect of crime in venues suggests that even in neighborhoods with a decreasing crime trend reducing crime in venues is still effective in attracting consumers.

6. Conclusion

Extensive research has been done about the determinants of crime and the efficacy of different prevention and policing strategies. Much less attention, however, has been given to the economic impacts of crime, especially with regard to patterns of consumer behavior. This paper fills part of the gap by providing robust evidence of effects of short term changes in local crimes on consumer visits to retail and food service establishments in a large city in the United States. Central to our analysis is the idea that consumers' sensitivity to crime depends on crime type and place of occurrence.

We employ a conservative approach that leverages temporal and geographical variations and the richness of the data to account for unobserved heterogeneity and time variant confounders. Our identification strategy builds on the conjecture that consumers' response to crime occurs at fine levels of geography and time, whereas confounders only vary at fine levels of geography or time, but not both. Several validity tests confirm that our estimates are not likely to suffer from endogeneity.

Our main results find stronger effects for property than for violent offenses. In addition, the main results suggest that the crime effect on consumer visits is large and significant for incidents that occur in public spaces, whereas crimes that occur within residences do not have a statistically significant effect. This provides additional evidence that unobserved factors are not driving the association between crime and consumers visits found by us.

By exploring variation in crime effects we find that crime has a negative effect on consumers in the extensive margin (number of visits and number of customers), but we do not find sizable effects in the intensive margin (venue dwell time). Our results also provide evidence that night visits are more sensitive to changes in crime than day time visits. Furthermore, we find that consumers respond to salient crimes in low crime neighborhoods and severe crimes in high crime neighborhoods. While in areas where criminal activity is uptrending consumers react to crimes committed outdoor, in neighborhoods facing crime decline consumers are only sensitive to crimes happening at establishments.

Our work indicates that consumers take crime rates into consideration when deciding whether to visit a business. We conclude that our findings are consistent with the argument that the perception of violence and the risk of victimization, induced by crime incidents, scare off consumers, potentially making businesses less profitable. Our results add to the research on costs of crime by quantifying the effect of criminal events on consumer activity in urban areas. They are useful in helping policy makers and local agencies plan communities revival and economic development.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jue.2022.103448.

CRediT authorship contribution statement

Hao Fe: Conceptualization, Data curation, Formal analysis, Funding acquisition, Investigation, Methodology, Writing – original draft, Writing – review & editing. **Viviane Sanfelice:** Conceptualization, Data curation, Formal analysis, Investigation, Methodology, Visualization, Writing – original draft, Writing – review & editing.

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