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Evidence from Consumer Behavior

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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 the choices individuals make about where to live, work and shop and has important implications for economic development of communities. This paper investigates the local impact of crime on subsequent consumer visits to food and entertainment retails. We study this relationship using a novel longitudinal dataset with point-specific crime and consumer visit data. To estimate plausible causal effects, we leverage the rich data to eliminate time invariant factors and to absorb time variant confounders. Our results show that the estimated effects of property crime and outdoor crimes on consumer visits in the following month are negative, meaningful and strongly significant. Interestingly, the effect is larger and significant for incidents that occurs on the streets, but residential crimes are not statistical relevant to explain visits. 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.

Key words: Foot-traffic, Neighborhood, SafeGraph, Venue, Violence, Visit
JEL Codes: K40, M21, R11, R12, R22

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1 Introduction

Theoretical and empirical arguments suggest that the fear of victimization causes consumers, workers and entrepreneurs to alter their routine activities (Wilcox et al., 2018; Hamermesh, 1999; Mejia and Restrepo, 2016). 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 local business activities through its effect on consumer behavior. This paper aims to fill the gap by directly measuring consumers' sensitivity to criminal activities.

In this study, we leverage point-specific crime and consumer visit data to investigate the local 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 in the extensive margin measured by number of visits and number of consumers, not in the intensive margin (venue visit time).

Understanding consumers' sensitivity to 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. However, crime can deter potential customers leading to significant revenue loss or even business closure. It is crucial for policy makers and city planners to know how crime might affect economic development efforts and if crime control can be a viable economic development tool, especially in struggling neighborhoods where crime rates are often high. 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). Measuring consumer response to local crime would allow more effective design of policing strategies, which can maximize the effects of private and public investments in neighborhood revitalization.

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 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.

The paper also contributes to the literature of behavioral economics. Research in behavioral economics and psychology suggests that small changes to the environment can lead to large effects on human behavior and well-being. For instance, Cornaglia et al. (2014) and

Dustmann and Fasani (2016) have demonstrated a causal link between crime and mental health. Our paper is most related to two recent studies in the literature: Mejia and Restrepo (2016) which look at how crime affects individuals' conspicuous consumption, and Janke et al. (2016) which measure how perceived level of safety affects routine physical activity. Both studies find detrimental impact of criminal episodes on individuals' behavior. This paper adds to this collection by focusing on the impact of exposure to changes in local crimes on individuals' everyday choices of visiting a commercial establishment.

Our analysis focuses on Chicago, the third most populous city in the United States. 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 for 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 for various categories of crimes 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 too many plausible ways to aggregate the data, i.e. how should we classify crimes and define "local" and "subsequent"? Second, identifying the causal 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. It is hard to find instrumental variables that affect consumer visits only through local crime. More importantly, the two concerns are inseparable since identification of the causal effects depends on how the data are aggregated.

We utilize a conservative approach to account for time invariant and variant confounders.

[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 (Allcott et al., 2020; Dave et al., 2020a; Dave et al., 2020b).

The approach starts with the intuition that the local impacts of crime occur at fine levels of geography and time, whereas most confounders only vary at fine levels of geography or time, but not both, such as weather and neighborhood socioeconomic status. 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, 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.

Our research design is bolstered by multiple robustness checks. First, the results vary little adding more control variables measuring local economic development (number of active business licenses and number of building permits), venue popularity (median venue visit time and median distance from home travelled by visitors) and crime spillover effects (crimes in the nearest adjacent neighborhood). Second, we confirm that our estimates do not suffer from endogeneity via an exogeneity test developed by Caetano (2015). Finally we perform Granger tests for causality, which checks if future crime predicts the current number of consumer visits. As desired, the null hypothesis that the coefficients on the leading variables are jointly zero cannot be rejected. These validity checks support a causal interpretation of our results.

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. 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. This provides additional evidence that unobserved factors are not driving the association between crime and consumers visits. Considering other studies that also use SafeGraph data to measure consumer visits, our estimates are non-negligible, for instance, compared to consumers' sensitivity to distance (Athey et al., 2018) and stay-at-home orders from the recent pandemic (Allcott et al., 2020). 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 exploring the heterogeneous effects by venue type and by initial crime levels respectively. 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 visit 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.

To proxy for gender asymmetric reaction to criminal activities, we look at beauty salon visits, a service with predominantly female users. One additional violent crime near a venue results in two fewer visits or 2.2% reduction in the average number of visits to beauty salons. Moreover, 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 other venues in our sample. Assuming that the difference in estimates are due to the majority of hair salon clients being female, our findings suggest that an increase in violent crime translates into a larger drop in female consumer activity.

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 with a discussion 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 agent 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.

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.

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.

Focusing specifically on crime as a component of environmental conditions, the utility function of individual i's consuming in time t at business k located at area j is given by

$$U_{ik(j)t} = \beta_i crime_{ik(j)t} + \boldsymbol{Z}_{ik(j)t}\boldsymbol{\theta_i} + \delta_{k(j)t} + \xi_{ik(j)t} \tag{1}$$

β_i is the parameter of interest and measures how crime level around the venue affects consumer i's utility from attending business k. If $\beta_i < 0$, crime decreases utility and individual i is less likely to choose business k when the area around it is perceived as unsafe. Crime here is a broad concept representing individual i's perception of safety associated with business k at time t. 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]

Unfortunately, individual crime perceptions are rarely observed. In this paper, we only observe reported crime incidents. How reported crimes translate into individual perceptions and fears that nudge consumer choice is not what we focus on in this paper. It is the causal effect - namely, the impact a reduction in crime has on consumer visits, perhaps induced by the two channels discussed at the beginning of this section and possibly amplified by individual perceptions - that is a crucial and relevant policy parameter.

$\boldsymbol{Z}_{ik(j)t}$ contains all other variables that affect the utility individual i would get by attending business k in period t, such as specific aspects of the matching between them. While $\delta_{k(j)t}$ captures business heterogeneity and $\xi_{ik(j)t}$ represents random shocks (for instance, the utility of attending a beer garden will likely be higher in a non-rainy day). For a given point in time, we assume that the consumer chooses one venue from a finite set of options in order to maximize utility.

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 criminal decisions. First, consumers are potential victims. Places with more people offer more opportunities for criminals to strike. 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,

[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`.

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.

In summary, it is evident that there is a circular relationship between consumers and offenders. Criminal actions in the surroundings of a venue affect the expected utility of patronizing the location. At the same time, the flow of people generated by consumer traffic also impacts criminal behavior.

Businesses

Crime causes 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 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 by inferring about consumer preferences and by 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. 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 end, decaying neighborhoods often face violence surge and business deterioration.

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. 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

[3]We choose this time period because our SafeGraph data are only available from January 1st, 2017 to September 30th, 2019.

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.[4] 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. The population sample is a panel of opt-in, anonymized smartphone devices, and is well balanced across U.S. demographics and geographies (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 open later than January 1st, 2017 or closed before September 30th, 2019. 6% of venues are dropped due to this restriction.

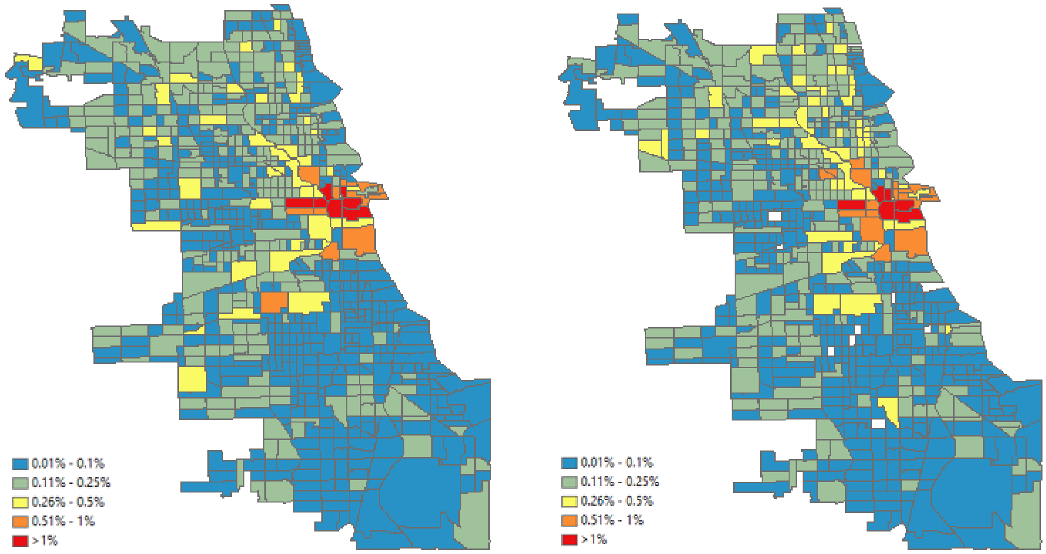
The venues in our sample correspond to about 30% of the active retails according to business licenses issued by the city administration during our period of analysis.[5] Figure 1 compares the spatial distributions of active retail business licenses and establishments in our sample. We can see that there exists a high spatial correlation between the two data, revealing that our sample of venues does not over- or under-represent communities in the city of Chicago.[6]

[4]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).

[5]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.

[6]SafeGraph's official website provides more information regarding the quality of their venue data: `https://docs.safegraph.com/v4.0/docs/places-data-evaluation`.

Figure 1: Spatial Distribution of Active Business Licenses and Venues in Our Sample



(a) Active business licenses (b) 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.

3.1 Data Aggregation

Three aggregation choices we must make before estimation. That is, how to classify crime incidents, how to define neighborhoods and how to choose time periods. They are important decisions that directly determines 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 heterogenous 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.[7] The second set contains six crimes based on where a crime occurs: crime in streets, crime in residence, 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. As a consequence, we decide to define a block group as a neighborhood. A block group in the city of Chicago has about 20 census blocks and an average population of 1,200 in 2010. There are approximately 2000 block groups in Chicago. Their average size is 0.1 square miles.

There is a similar trade-off when aggregating time. Short time periods allow us to measure short-term effects. Consumers' response to crime also tends to be in the short run immediately after a crime's occurrence. However, if the time period is too short, we are unable to estimate longer-term effects of crime. Additionally, crimes in a very short time period may have little variation and are not measured precisely. Given the trade-off, we define our time to be a month, which is short enough to capture the local impact of crime in its aftermath but not too short to lose variation in neighborhood crime rates over time.

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

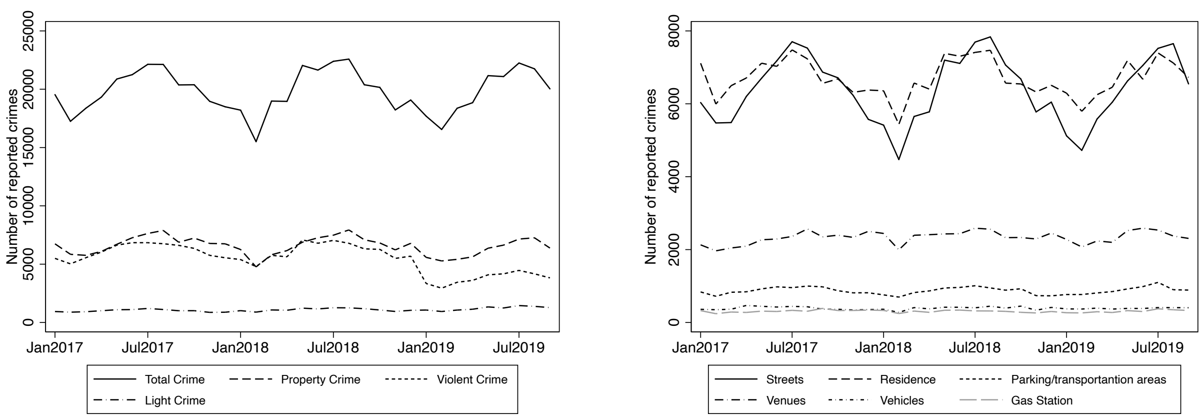
[7] Violent crimes include aggravated assault, sexual assault, robbery and homicide. Property crimes include arson, burglary, motor vehicle theft and theft. Light crimes include criminal trespass, public peace violation, liquor law violation, stalking, gambling, intimidation, obscenity, non-criminal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

venue is located.

3.2 Descriptive statistics

Figure 2 presents the time evolution of numbers of reported crimes by type in panel (a) and by location in panel (b) for block groups with at least one venue in food or entertainment industry in the time period of interest. The time trend for number of total reported crimes is also presented in panel (a). Total crime, property crime, violent crime, crime in streets, crime in residence and crime in venues show a clear seasonal trend from January 2017 to September 2019. However, over the same time period of interest, we do not observe any overall change in number of reported crimes for crimes of different types or crimes occurred at different locations with one exception - violent crime fell by 30%.

Figure 2: Chicago Crime Trends



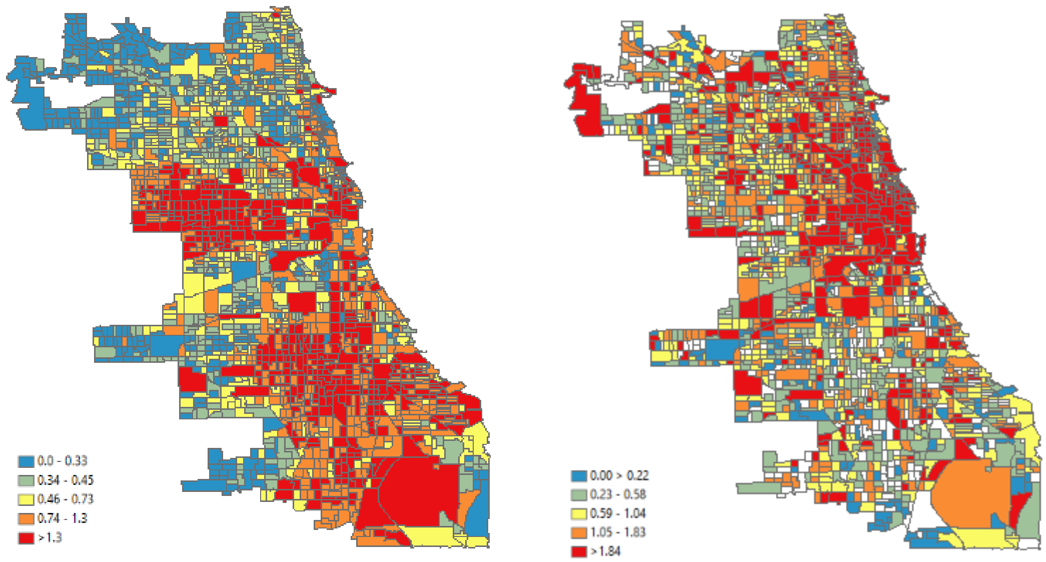
(a) Crime by type

(b) Crime by location

Notes: The figure presents crime trends for Chicago from January 2017 to September 2019. Panel (a) includes crimes by type and total crime. See footnote 7 for the definitions. Panel (b) include crimes classified by where it occurs. When calculating the numbers of reported crimes, we only consider block groups with at least one venue in food or entertainment industry in the period of interest.

Figure 3 display how crimes and consumer visits are dispersed by block groups of the city. The maps show that crime and consumer visits are both skewed geographically. In general, crime rates are higher in the central, west and south regions, while consumer visits are more concentrated in the downtown area (central region).

Figure 3: Spatial Distribution of Crimes and Consumer Visits



(a) Crime by 100 inhabitants

(b) Consumer visits by inhabitant

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. These consumers spend a median time of 45 minutes 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 (14,893/1,874). 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.

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 visit 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	1,874	
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.

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 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 solution would be the use of instrumental variables. However, it is difficult to conceive of transitory and highly localized variables that affect consumer visits only through local crimes even after conditioning on a list of relevant controls. What makes it more difficult is that crimes of multiple categories are included in our model, thus at least the same number of instrumental variables would be required.

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.

$$Visit_{t,k(j)} = \sum_{w=1}^{W} \beta^w Crime^w_{t-1,j} + \alpha Visit_{t-1,j} + \delta_{T,j} + \delta_{t,J} + \delta_{k(j)} + \epsilon_{t,k(j)} \tag{2}$$

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 $\beta = (\beta^1, \beta^2, ..., \beta^W)$ 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 local impacts of crime occur at fine temporal and geographical levels, whereas most confounders only vary at fine temporal or geographical levels, but not both. 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 at 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*) tract by month, $\delta_{T,j}$ where T denotes census tract, and *ii*) block group by year, $\delta_{t,J}$ where J denotes year. The former captures all time-varying unobservables that vary at a larger geographic area than a block group. Tract-month fixed effects account for all short term time variant factors that affect consumer visits and crime at the census tract level such as weather conditions and neighborhood events like parades or sport competitions. This component also absorbs all tract-level trends due to law enforcement, public interventions and so on.[8] The latter absorbs neighborhood-specific confounders that changes more slowly than crime. This component accounts for land use patterns and local gentrification that could affect crime and consumer visits.[9] Block group-year fixed effects also alleviate the concern of venues sorting into locations.

$Visit_{t-1,j}$ 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

[8]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).

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

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 using consumer visits, 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. In summary, after controlling for venue's heterogeneity, the idea is to compare two venues in the same census tract that had the same number of consumer visits associated to their locations during the previous month, but, for some reason unrelated to variation at the block group by year level, one of them had more crime of category w nearby than the other. If the difference in the level of $Crime^w$ between the two locations is due to exogenous factors, then we would be able to identify β^w, the effect of $Crime^w$ on consumer visits in the next period.

Equation (2) aims to measure how short term changes in crimes around a venue's location affect business through consumers sensitivity to safety conditions. Our identification strategy relies on consumers' response and number of criminal incidents being temporally and spatially dynamic, i.e. varying throughout months of a year and across areas within the same census tract. We are not able to identify how more aggregated crime prevents consumer visits to a certain neighborhood. Our model explores crime heterogeneity which allows us to identify particular categories of crime influencing consumers most so that we can provide effective policy suggestions.

5 Results

5.1 Main Results

In this section we present and interpret the baseline results of the paper. We evaluate the robustness of the results in section 5.2.

Table 2 displays our estimations progressing from the raw relationship between crimes of different types and consumer visits to our most 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. It suggests that lagged neighborhood visits play an important role in controlling for positive bias due to foot-traffic. This term also controls for heterogeneity across neighborhoods by accounting for different levels of business activities proxied by lagged number of visits.

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 lost in terms of getting larger standard errors is expected since census tract-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 Equation (2). 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 visit time are added to control for venue popularity. 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.[10]

The pie charts in Figure B.1 of the Appendix allow us to better interpret the significant effect of property crime by exploring its composition from the perspectives of more disaggregated crime types, places of occurrence and the salience of crimes. It is theft (91%) that dominates the effect of property crime on consumer visits. Most property crime (42.7%) takes place in venues and only 19.1% in streets. Weekend, daytime and indoor are when and where majority of property crime occurs.

[10] 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.

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 visit 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

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 most 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.

Our pie charts in Figure B.2 of the Appendix provides more information regarding the street crime composition, which allows us to better interpret the effect of street crime. For example, street crime consists of a variety of disaggregated crime types, among which theft and battery account for over 50%. Similar to property crime, street crime also occurs more often in weekends. However, unlike property crime, street crime is slightly more likely during night time.

To put our findings in perspective we compare them to two other studies that also use SafeGraph data to form their dependent variable, consumer activity. Athey et al. (2018) compute consumer's willingness to travel to a restaurant in the San Francisco Bay area. They find that the average elasticity across consumers and restaurants is -1.41. That means that a 1% increase in the average median distance (about 0.0306 miles or 0.612 blocks) reduces the probability of visiting a business by 1.41%. Although their result is in terms of probability of visiting a business, we can still speculate that consumers' sensibility to short term changes in street crimes is not negligible when compared to sensitivity to distance. In the context of an extreme shock, Allcott et al. (2020) document how the recent events in the U.S. coronavirus pandemic affect consumer behavior. Their estimates suggest that consumer visits drop by 18% on the day after a stay-at-home order is implemented. One example of an extreme shock in our context is to reduce the average number of street crimes to zero. In that case our findings indicate that consumer visits for the average venue would increase by 6% $(-3.03 \times -6.32)/315.49)$, which is one third the magnitude of the stay-at-home order effect.

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 visit 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

5.2 Validity Tests and Robustness Checks

In this section, we present two tests to support a causal interpretation of our results. First, we confirm that our estimates do not 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 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 zero.

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. 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 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. 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 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.[11]

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 Equation (2). The coefficient associated to $d^w_{t-1,j}$ represents the size of the discontinuity at $E[\text{Visit}_{t,k(j)}|\text{Crime}^w_{t-1,j} = 0, \text{Crime}^{-w}_{t-1,j}, \text{Controls}]$. Finally we implement an F-test on whether the coefficients of $d^w_{t-1,j}$ are jointly zero, which is equivalent to testing whether Assumption 1 in Caetano (2015) holds.

[11]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.

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.

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. For specifications with different types of crime, we reject exogeneity in specifications (1) and (2) but fail to reject in specifications (3)-(6), which implies that block group by year fixed effects and lagged block group visits are enough to absorb confounders in the model. For crimes occurred at different locations, the surviving specifications are different. Specifications (3) and (4) are not good enough to absorb all confounders. Venue fixed effects in specification (5) lead to a jump in p-value (from 0.09 to 0.16) which is suggestive of the importance of venue's heterogeneity in absorbing unobservables when crimes by location are used as variables of interest.

To test the extent to which the estimated relationship between crime and consumer visits flows in both directions – changes in crime cause consumers to not visit venues and changes in consumer visits predict changes in future crime, we estimate a causality test in the spirit of Granger (1988). The test is quite intuitive and well adopted in empirical crime studies using longitudinal data (see for instance Ellen et al., 2013; Autor et al., 2019). Specifically, we add future crimes ($t+1$) to the main model in Equation (2). Tables 5 and 6 present the results for crimes by type and crimes by location respectively. Specifications (1)-(6) have the same controls as in Tables 2 and 3.[12] In each specification, we test whether the coefficients

[12]Here is a summary of the controls: (1): no controls; (2) lagged block group visits; (3) lagged block

of future crimes are jointly zero. The F-statistics and corresponding p-values are shown at the bottom of the tables.

In both Table 5 and Table 6, we reject the null hypothesis that future crimes' coefficients are jointly zero in specifications (1)-(3), which suggests that we cannot rule out the possibility of reverse causality only controlling for lagged block group visits and the block group by year fixed effects. Specification (4) results in a jump in p-value (from 0.03 to 0.60 in Table 5 and from 0 to 0.23 in Table 6) which implies the importance of the tract by month fixed effects in controlling for unobserved trends. The coefficients of future crimes in the surviving specifications (4)-(6) are insignificant in both tables. Additionally, given a surviving specification, the main effects of crimes in $t-1$ in Table 5 (6) are not statistically different from the corresponding ones in Table 2 (3). These results provide support for the estimated effects flowing from changes in local crimes to consumer visits and not in the reverse direction.

In light of these validity tests we proceed to investigate variation in crime effects using the specification in column (5) of Tables 2 and 3. We adopt this specification to be conservative. Although the specification in column (4) also passed most validity checks, its p-value is slightly less than 10% in Table 4 when crimes by location are the variables of interest.

group visits and fixed effects at the block group by year level; (4) lagged block group visits and fixed effects at the block group by year and at the tract by month levels; (5) lagged block group visits and fixed effects at the block group by year, at the tract by month and at the venue levels; (6) lagged block group visits, fixed effects at the block group by year, at the tract by month and at the venue levels, and four control variables (median distance from home by visitors, median venue visit 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.

Table 5: Granger Causality Tests - Crime by type

	(1)	(2)	(3)	(4)	(5)	(6)
Property crime (t-1)	-0.29	-5.56***	-1.95***	-1.46**	-1.20*	-1.01*
	(0.45)	(1.45)	(0.60)	(0.74)	(0.63)	(0.57)
Violent crime (t-1)	-0.12	-0.04	0.32	-0.07	-0.08	-0.15
	(1.13)	(2.16)	(0.95)	(1.00)	(0.92)	(0.74)
Light crime (t-1)	9.55**	-1.82	4.11	2.41	1.79	1.93
	(4.42)	(7.90)	(3.17)	(3.02)	(2.75)	(2.52)
Property crime (t+1)	1.19**	-2.18**	0.18	-0.44	-0.35	-0.32
	(0.48)	(0.95)	(0.63)	(0.37)	(0.33)	(0.34)
Violent crime (t+1)	-0.15	2.19	3.07**	1.33	1.14	0.78
	(1.14)	(1.94)	(1.33)	(1.20)	(1.07)	(0.83)
Light crime (t+1)	9.63**	-1.04	2.46	1.30	1.57	2.00
	(4.28)	(2.89)	(1.63)	(2.04)	(1.88)	(2.28)
F-statistic	4.41	2.92	3.06	**0.62**	**0.56**	**0.42**
p-value	0.00	0.03	0.03	**0.60**	**0.64**	**0.74**
R-squared	0.01	0.03	0.12	0.16	0.46	0.46
Observations	460,413	460,413	460,413	460,413	460,413	460,413

Notes: This table presents the results of a causality test in the spirit of Granger (1988) for crime by type. Specifications (1)-(6) have the same controls as in Table 2 (see footnote 12 for a detailed description of the controls). In each specification, we test whether the coefficients of future crimes are jointly zero. The F-statistics and corresponding p-values are shown at the bottom of the table. Standard errors, shown in parentheses, are clustered at the block group by year level. * p<0.1, ** p<0.05, *** p<0.01

Table 6: Granger Causality Tests - Crime by location

	(1)	(2)	(3)	(4)	(5)	(6)
Streets (t-1)	0.03	-4.70***	-2.31*	-2.75**	-2.43**	-2.50**
	(0.83)	(1.11)	(1.40)	(1.17)	(1.10)	(1.12)
Residence (t-1)	0.28	2.47**	1.20*	0.48	0.54	0.55
	(1.22)	(1.07)	(0.66)	(0.72)	(0.65)	(0.67)
Parking/transp. areas (t-1)	-0.90	-20.77***	-7.87***	-8.72**	-7.67**	-6.77**
	(1.07)	(2.78)	(2.71)	(4.07)	(3.68)	(3.35)
Venues (t-1)	1.25	-2.74**	-0.57	-0.56	-0.38	-0.39
	(0.82)	(1.11)	(0.77)	(0.99)	(0.88)	(0.93)
Vehicles (t-1)	-4.85	11.65***	2.21	3.55	4.15	3.86
	(4.06)	(4.28)	(2.89)	(3.34)	(3.14)	(2.83)
Gas stations (t-1)	-3.42	8.87**	1.21	-6.02*	-6.24*	-6.15*
	(2.99)	(4.39)	(2.90)	(3.57)	(3.46)	(3.38)
Streets (t+1)	4.38***	0.20	2.14**	0.33	0.36	0.28
	(0.88)	(1.25)	(0.94)	(0.60)	(0.57)	(0.62)
Residence (t+1)	0.48	3.91***	1.47**	-0.02	0.00	0.04
	(1.27)	(0.99)	(0.70)	(0.85)	(0.73)	(0.71)
Parking/transp. areas (t+1)	-2.57***	-4.51*	1.68	2.67	2.43	2.31
	(0.91)	(2.33)	(1.69)	(1.69)	(1.50)	(1.43)
Venues (t+1)	1.11	-1.22	0.11	-0.24	-0.09	0.00
	(1.24)	(0.77)	(0.31)	(0.80)	(0.70)	(0.69)
Vehicles (t+1)	-6.81	2.59	-1.48	3.03	2.08	1.92
	(4.58)	(6.87)	(5.10)	(5.51)	(5.00)	(4.81)
Gas stations (t+1)	-4.32	0.77	-1.53	-7.57	-7.45	-7.24
	(3.31)	(3.25)	(1.52)	(7.54)	(6.85)	(6.83)
F-statistic	5.68	4.06	4.79	**1.35**	**1.33**	**1.18**
p-value	0.00	0.00	0.00	**0.23**	**0.24**	**0.31**
R-squared	0.01	0.03	0.12	0.16	0.46	0.46
Observations	460,413	460,413	460,413	460,413	460,413	460,413

Notes: This table presents the results of a causality test in the spirit of Granger (1988) for crime by location. Specifications (1)-(6) have the same controls as in Table 3 (see footnote 12 for a detailed description of the controls). In each specification, we test whether the coefficients of future crimes are jointly zero. The F-statistics and corresponding p-values are shown at the bottom of the table. Standard errors, shown in parentheses, are clustered at the block group by year level. * p<0.1, ** p<0.05, *** p<0.01

5.3 Variation in Crime Effects

Apart from number of consumer visits, we consider four alternative outcomes, number of unique visitors, venue's night popularity, venue's day popularity and consumer venue visit time, all of which help us better understand how consumers respond to a variety of local crimes. Estimation results are presented in Table 7. 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 6, so we can reasonably interpret the coefficients as causal.[13]

Number of unique consumers is the outcome in column (1) of Table 7. 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 the average venue in our sample receives 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 visit, 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.

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 popularity levels in Table 1, the relative impacts are smaller for day popularity. 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

[13]Test results of Caetano (2015)'s exogeneity test using the four alternative outcomes are presented in Table A.1 to Table A.4 of Appendix A. The estimates for crimes by location with venue's day popularity as the outcome has a p-value 0.09 in our most preferred specification, however, the Granger test suggests that these estimates are likely causal. Results using the Granger tests are available upon request.

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 safety perception also impacts consumers in the intensity margin, measured by the amount of time spent in a location, we present crime effects on consumers' median visit time to a venue (in minutes) in column (4) of Table 7. 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 literature on retail environment and consumer behavior (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 visit time by 0.20% (-0.09/45.63).

Table 8 presents estimates by business category according to industry classification. 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 criminal activities in parking or transportation areas have a large detrimental effect on visits to retail stores.

It is likely that consumers with different demographics have different elasticities to crime. For instance, Braakmann (2012) finds that females and males respond distinctly to victimization fears and have different tolerance for crime, which could result in different reactions as consumers. Unfortunately, we do not observe consumer characteristics. In an attempt to further investigate this theory, we look at hair salon visits, a service with predominantly female users, to proxy for gender asymmetric reaction to criminal activities. For this analysis we deliberately add to our sample hair salons, which belongs to the service sector and were not in our original sample[14].

[14] We add 374 venues that provide services on hair, beauty or nail salons. These are establishments with NAICS code 812112 or 812113.

The last column in Table 8 displays the estimates for hair salons. Property and violent crimes in the block group where a hair salon is located have statistically significant and negative effects on customer visits. In particular, one additional violent crime results in two fewer visits or 2.2% (-2.08/92) reduction in the average number of visits to hair salons. Moreover, at 5% significance level, we reject that the effect of violent crimes on hair 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 hair 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 7: Alternative Outcomes

	(1) Unique Consumers	(2) Night Popularity	(3) Day Popularity	(4) Visit 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.

Table 8: 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 about the past number of crimes are interacted with dummies about the venue type. In our sample there are 258 venues in the accommodation, 7,052 venues in food, 1,556 in entertainment, 6,027 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.

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 9, 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 homicide) occur less frequently and more idiosyncratically. Therefore, they play less of a role in the decision of a consumer 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. Indoor crimes including those in residences and venues, where majority of violent crimes occur, impact consumer visits in high crime areas.

Table 9: Heterogeneous Results: Crime Level

	Low crime	High crime
Crime by Type		
Property crime (t-1)	-1.22**	1.95
	(0.58)	(1.42)
Violent crime (t-1)	0.01	-1.78**
	(1.05)	(0.81)
Light crime (t-1)	2.18	-0.33
	(3.71)	(1.14)
Crime by Location		
Streets (t-1)	-3.58***	-0.39
	(1.37)	(1.22)
Residence (t-1)	0.97	-0.82*
	(0.96)	(0.47)
Parking/transp. areas (t-1)	-6.56*	9.50
	(3.87)	(6.48)
Venues (t-1)	0.08	-5.69**
	(0.91)	(2.42)
Vehicles (t-1)	5.44	-1.45
	(3.81)	(2.95)
Gas stations (t-1)	-11.58**	2.20
	(5.85)	(1.89)
Observations	475,290	

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 about the past number of crimes are interacted with a dummy variable about level of total crime 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. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

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 our 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. By specifying the appropriate fixed effects and exploiting lagged neighborhood consumer visits, we believe our estimated local impacts of crimes can be reasonably interpreted as causal. Several validity tests also 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. Exploration of the variation in crime effects finds 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 visit time). Our results also provide evidence that night visits are more sensitive to changes in crime than day time visits. Finally, beauty salon visits, a service with predominantly female users, are used to proxy for gender asymmetric reaction to criminal activities. We find that an increase in violent crime translates into a large drop in beauty salon visits.

Our work indicates that consumers take crime rates into consideration when deciding whether to visit a business within a city neighborhood. We conclude that our findings are consistent with the argument that the perception of crime 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, especially for urban areas. They are useful in helping policy makers and local agencies plan communities revival and economic development.

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Appendix A Robustness Checks for Alternative Outcomes

Table A.1: Exogeneity Test Results - Unique Consumers

	(1)	(2)	(3)	(4)	(5)	(6)
Crime by type	30.78	66.16	**0.36**	**0.45**	**0.40**	**0.49**
	0.00	0.00	**0.78**	**0.72**	**0.75**	**0.69**
Crime by location	34.38	53.12	5.92	2.02	**1.70**	**1.64**
	0.00	0.00	0.00	0.06	**0.12**	**0.13**

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 when the outcome is number of unique consumers per month. 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.

Table A.2: Exogeneity Test Results - Night Popularity

	(1)	(2)	(3)	(4)	(5)	(6)
Crime by type	10.83	12.94	2.20	**0.71**	**0.76**	**0.85**
	0.00	0.00	0.09	**0.55**	**0.52**	**0.47**
Crime by location	6.00	13.27	2.81	1.90	**1.77**	**1.80**
	0.00	0.00	0.01	0.08	**0.10**	**0.10**

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 when the outcome is venue night popularity. 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.

Table A.3: Exogeneity Test Results: Day Popularity

	(1)	(2)	(3)	(4)	(5)	(6)
Crime by type	13.81	26.34	2.97	**0.76**	**0.79**	**0.86**
	0.00	0.00	0.03	**0.52**	**0.50**	**0.46**
Crime by location	13.43	32.17	3.67	1.93	1.81	1.84
	0.00	0.00	0.00	0.07	0.09	0.09

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 when the outcome is venue day popularity. 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.

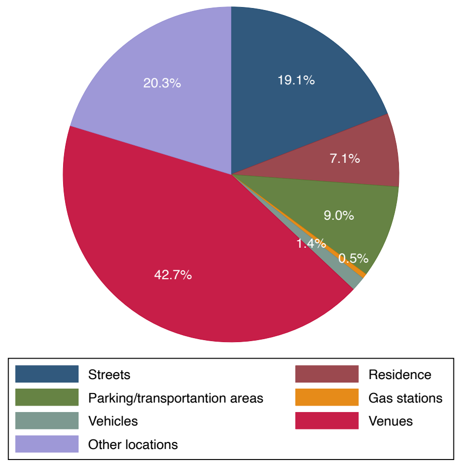
Table A.4: Exogeneity Test Results: Visit Time

	(1)	(2)	(3)	(4)	(5)	(6)
Crime by type	19.53	15.61	3.29	**0.41**	**0.30**	**0.31**
	0.00	0.00	0.02	**0.74**	**0.82**	**0.81**
Crime by location	30.53	33.10	2.65	**1.43**	**1.27**	**1.29**
	0.00	0.00	0.01	**0.20**	**0.27**	**0.26**

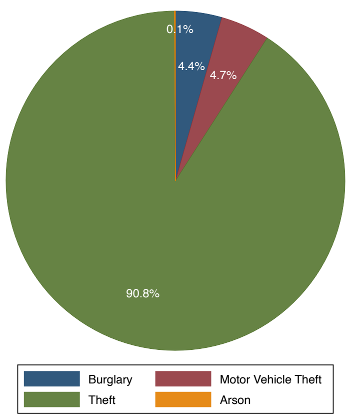
Notes: This table presents the F-statistic and corresponding p-value (in parentheses) of the exogeneity test developed by Caetano (2015) for each specification in Tables 2 and 3 when the outcome is venue visit time in minutes. Entries in bold denote "surviving specifications" for which we cannot reject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

Appendix B Crime Composition

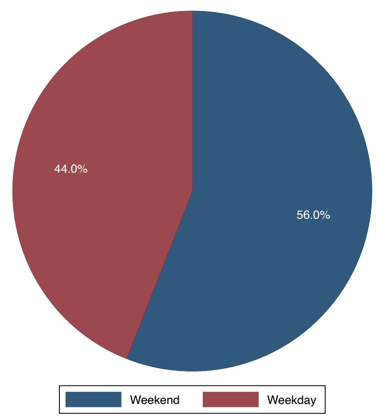
Figure B.1: Property Crime



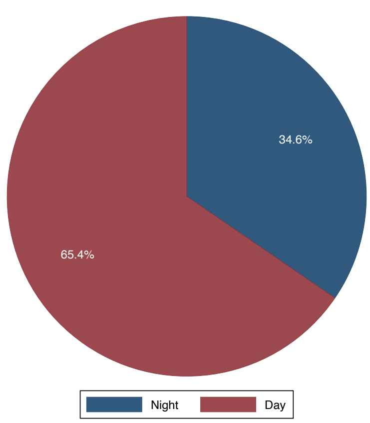
(a) By crime location



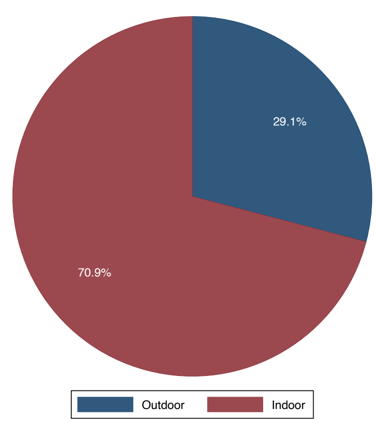
(b) By crime type



(c) Weekend and weekday

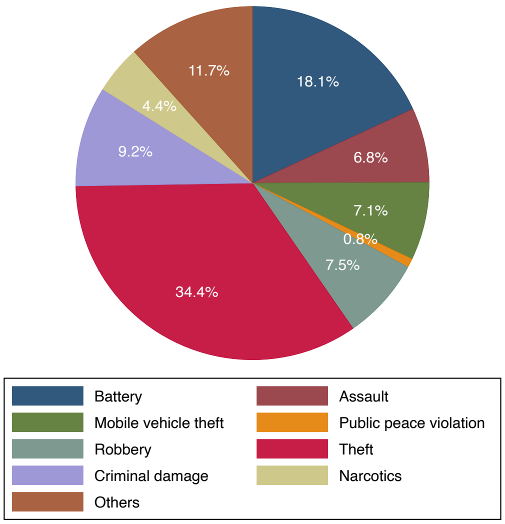


(d) Night and day time

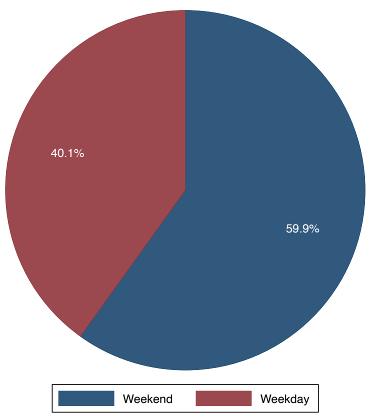


(e) Outdoor and indoor

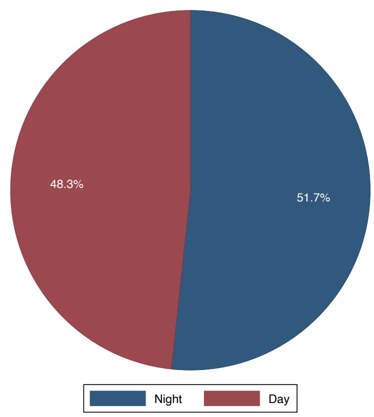
Figure B.2: Street Crime



(a) By crime type



(b) Weekend and weekday



(c) Night and day time