The impact of social disorder on crime: The case of homelessness

Kevin Corinth Grace Finley

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Abstract

The highly influential broken windows theory predicts that homelessness and other forms of social disorder increase crime by signaling to potential criminals that the police are not monitoring a particular area. Using data on the exact times and locations of over 75,000 '311' calls about homeless individuals and 3.3 million crime reports in New York City, we estimate the contemporaneous impact of reported homelessness on reported crime under two separate approaches. First, we use a difference-in-differences approach to compare changes in crime in close proximity to a homeless report to changes in crime in nearby areas that recently (but not contemporaneously) experienced a homeless report. We find that within two hours and 100 feet of a homeless report, misdemeanor assault rises by 66 percent, consistent with the homeless individual himself being the victim or perpetrator. Meanwhile, violent crime falls within 100 feet but increases by 32 percent between 100 and 300 feet of the homeless report, consistent with the homeless individual serving as a potential witness in the more immediate area, while signaling a lack of police presence in the extended area. In our second approach, we use nighttime rainfall as an instrument for citywide unsheltered homelessness the following day. The elasticity of citywide misdemeanor assaults with respect to homelessness is 0.08 and marginally statistically significant, and the elasticity of violent crime is 0.09 but not statistically significant. We can reject elasticities for total crime of more than 0.2. Altogether, our results suggest that contemporaneous changes in visible homelessness have an important (but nuanced) local effect on crime and potentially a modest effect on citywide crime as well.

Keywords: broken windows; homelessness; crime; disorder; police

1 Introduction

According to the broken windows theory, the presence of social disorder can increase crime. Potential criminals view "broken windows," such as homelessness or minor offenses like "turnstile jumping" (fare evasion at subway stations), as signals that the police are not safeguarding a given area. In their seminal article popularizing broken windows policing, Wilson and Kelling (1982) state, "[i]f the neighborhood cannot keep a bothersome panhandler from annoying passersby, the thief may reason, it is even less likely to call the police to identify a potential mugger or to interfere if the mugging actually takes place." The idea has been highly influential in the ways cities are policed, with police officers taking a more proactive role in cracking down on social disorder. Most notably, New York City widely embraced the broken windows theory during the 1990s. The strategy has been credited by some with drastic reductions in crime in the city during the period (Kelling and Sousa 2001; Corman and Mocan 2005), although others have cast doubt on whether it can plausibly explain the large crime declines, especially since other cities experienced major decreases as well (Levitt 2004; Harcourt and Ludwig 2006).

Aside from the question of whether broken windows policing reduces crime is a more fundamental question—does social disorder increase crime in the first place? That question is difficult to answer given that social disorder is not randomly assigned to neighborhoods nor determined directly through policy, but rather, tied up with the neighborhood's inhabitants and historical characteristics. Nonetheless, a large literature has explored associations between social disorder and crime, finding some evidence for a link (e.g., Sampson and Raudenbush 1999; Sampson and Raudenbush 2004; Wheeler 2017). Keizer et al. (2008) implement a series of field experiments and find that randomly generated visual cues such as graffiti and illegal bicycle parking cause passerby to take less ethical actions (e.g., taking a small amount of highly visible cash from an envelope). However, causal evidence on the impact of social disorder on actual criminal activity is lacking.

In this paper, we estimate the causal effect of social disorder on crime using data on reports of homelessness in New York City. We observe the exact time and location of over 75,000 '311' calls made by New Yorkers about homeless encampments and requests for homeless assistance between 2010 and 2016. We combine these data with the times and locations of 3.3 million crime reports over the same period. Our empirical approach to identifying the effect of homelessness on crime is two-pronged.

First, we use a difference-in-differences design to identify local effects of homelessness on crime. We identify small discs (with a radius of 300 feet) surrounding homeless reports for a short period of time (2 hours) before and after the report was made. We compare crime that occurred within this disc during this time period to crime that occurred in nearby discs (within one mile) during the same time period. We center these other dics around homeless reports that occurred within one week of the origin homeless report. This helps ensure that the treated area is similar to the areas that serve as controls, except for the fact that the homeless report occurred within the treated area during the time period being considered. Still, there may be fixed characteristics of treated and control areas that affect the extent of crime. Thus, we focus on the difference in crime during the relevant time window compared to the average level of crime during the same time of day on the day before and after. The difference (over time) in differences (between treated and control discs) is our estimate of the treatment effect of a homeless report on reported crime. Because homeless individuals are not always reported, and because we cannot ensure that homeless individuals remain in the same location for two hours before and two hours after a report is made, our estimates will understate the effect of visible homelessness on reported crime.

We find that reported homelessness increases violent crime by 15 percent within 300 feet of the homeless report, although this is the sum of two opposing effects. Within 100 feet, violent crime actually falls by 27 percent, while between 100 and 300 feet, violent crime increases by 32 percent. Declining violent crime within 100 feet is consistent with homeless individuals deterring crime within very close proximities through a "witness effect." Carr and Doleac (forthcoming) posit that such an effect could help explain why gunshots incease when curfews are implemented—curfews reduce the number of youths and their family members who could serve as potential witnesses and thus lead to increased crime. In our case, a homeless individual may be a particularly important potential witness (even in Manhattan where sidewalks are crowded) if he has a preexisting relationship with police or is more focused on his surroundings. Meanwhile, the increase in crime between 100 and 300 feet away is consistent with the broken windows theory in which the homeless individual signals to potential criminals a lack of police presence. We also find that misdemeanor assault increases in both regions, but especially within 100 feet where it rises by 66 percent, consistent with homeless individuals themselves being victims or perpetrators. Property crime is unaffected.

Second, we consider the citywide effect of homelessness on crime using nighttime rainfall as an instrument for homeless reports the following day. If nighttime rainfall both leads fewer homeless individuals to remain outdoors and does not affect criminal activity the following day (except through its effect on homelessness), then nighttime rainfall can serve as a valid instrument for citywide unsheltered homelessness the following day. We find that rainfall between 6:00pm and 6:00am reduces citywide reports of homeless encampments the following day by 9 percent, with larger effects under heavy rain. One threat to identification is if nighttime rainfall reduces foot traffic the next day, potentially reducing the number of potential victims, non-homeless witnesses, or people who report on homelessness or crime. We partially test this assumption using non-homeless '311' complaint calls. We find that precipitation during the night is in general not significantly associated with other types of calls, with the exception of strong effects for calls about problems such as damaged trees and sewer conditions that are directly linked to rainfall. Also, using data on nightly homeless shelter populations, our point estimates imply that a significant portion of the unsheltered population enters shelter under heavy nighttime rainfall, consistent with homeless individuals moving in off the streets.

When using rainfall during the nighttime as an instrument for citywide homeless reports the following day, we find that a one percent increase in homeless encampments increases misdemeanor assault by 0.08 percent, which is marginally significant. Violent crime increases by 0.09 percent but the estimate is not statistically significant. Elasticities for other crime types are smaller and not statistically different from zero.

Ex ante, it is not clear whether local estimates or citywide estimates should be larger. On the one hand, local effects of homelessness on crime may simply displace crime from other areas, or homelessness may not be sufficiently widespread throughout the city to affect aggregate levels of crime. This would increase local estimates relative to aggregate ones. On the other hand, our local estimates are likely biased downward because homelessness is not confined to instances that are reported, and homeless individuals likely do not remain in the spot reported for the entire time window we consider. This would reduce the magnitude of local estimates relative to aggregate ones. Ultimately, we find evidence consistent with meaningful effects of homelessness on misdemeanor assault and violent crime under the two approaches. We find significant local effects of homelessness on misdemeanor assault and violent crime. Likewise, there appears to be a small citywide effect of homelessness on misdemeanor assault and possibly on violent crime, although we cannot reject no effect in the latter case.

It is also important to emphasize that both of our empirical approaches identify the contemporaneous impact of temporary changes in social disorder on crime—we do not identify the impact of sustained changes in neighborhood disorder. Of course, the fact that the timing and placement of homelessness is highly variant and also subject to temporary changes in environment (i.e., rain) is what allows us to identify the effects of social disorder in the first place. In that sense, homelessness provides a unique setting for assessing the fundamental premise of the broken windows theory—that social disorder increases crime.

There are theoretical reasons for why temporary changes in social disorder, rather than solely long-run changes, would contemporaneously increase crime. Our study is based in New York City, where broken windows policing continues to be employed. If police officers respond to and remove disorder, then the presence of disorder at a specific time signals to potential criminals that police officers are less likely to be nearby. Rational criminals would thus increase crime in areas surrounding homeless individuals relative to other areas. Meanwhile, an exogenous reduction in homelessness across the city on a given day reduces the signals available to potential criminals about where the police are less likely to be monitoring, implying that crime would fall. Our findings of positive local effects on violent crime outside of the immediate vicinity of the homeless individual, along with potential modest citywide effects are consistent with criminal rationality. Furthermore, our finding that violent crime falls in very close proximities to homeless individuals is consistent with homeless individuals deterring crime through a witness effect. That misdemeanor assault is especially escalated in very close proximities is consistent with homeless individuals being the victims or perpetrators of increased crime, not necessarily changes in criminal behavior in response to differences in the perceived probability of being caught.

From a policy perspective, it is important to emphasize that our findings do not necessarily imply that broken windows policing is an effective strategy for reducing crime. Rather, if potential criminals use homelessness as a signal of police proximity, broken windows policing may actually increase crime. After all, if the police had a policy of never removing homeless people, then the presence of a homeless individual would not provide any information about whether the police were nearby. Broken windows policing could thus be a matter of the police "showing their cards" about where they are likely to catch a criminal. As a result, these results should not be taken as evidence that broken windows policing is effective in reducing aggregate crime. It is also important to emphasize that our results pertain only to temporary changes in social disorder, and that long-term changes in disorder may have different effects.

This paper relates most generally to the literature on the economics of crime. Becker (1968) developed a model in which the decision to commit a crime depends on two basic parameters—the probability of being caught and the penalty conditional on being caught. The product of these parameters determines the expected cost of committing a crime, which a rational criminal compares to the value of the criminal act. The more relevant parameter to this paper is the probability of being caught, and a large literature has studied whether increases in this probability decrease crime. Some have focused on the impacts of a city's police force on aggregate crime within cities, finding that increases in a police force reduces crime (Levitt 1997; McCrary 2002; Levitt 2002; Evans and Owens 2007; Lin 2009; Chalfin and McCrary 2013; Mello 2017). Tella and Schargrodsky (2004) and Draca et al. (2011) also find that more police officers reduce crime using terrorist attacks as natural experiments that increase police presence. Doleac and Sanders (2015) find that daylight deters crime, potentially as a result of an increased probability of capture.

Others study the impact of localized increases in police utilization, or "hotspots" policing, finding that increasing police presence in specific areas with substantial crime and disorder significantly reduces crime without creating spillover effects for other areas (e.g., Sherman and Weisburd 1995; Braga and Bond 2008). Of particular note, Berk and MacDonald (2010) find that increased police attention to crime related to homelessness in the Skid Row area of Los Angeles reduced crime in the area without displacing it elsewhere.

A subset of this literature focuses on broken windows policing, popularized by Wilson and Kelling (1982).[1] They hypothesized that the presence of social disorder, including low-level infractions and homelessness, signaled to potential criminals that the police would not enforce the law and thus that they were less likely to be caught committing crimes. New York City adopted broken windows policing in cracking down on disorder during the 1990s and experienced a substantial decline in crime in the ensuing years. Kelling and Sousa (2001) and Corman and Mocan (2005) each find that misdemeanor arrests, their proxy for broken windows policing, are associated with overall crime reductions in the city. However, Levitt (2004) and Harcourt and Ludwig (2006) cast doubt on the ability of these studies to draw causal conclusions on the effect of broken windows policing. A number of studies

[1]The idea was introduced by Zimbardo (1969) who conducted an experiment in which one abandoned car was left in New York, NY and one in Palo Alto, CA. The car in New York was immediately vandalized, while the car in Palo Alto remained untouched until the authors began to vandalize it and others soon participated as well.

have explored the associations between disorder itself and crime, finding some evidence of a link (e.g., Sampson and Raudenbush 1999; Sampson and Raudenbush 2004; Wheeler 2017). Also, Keizer et al. (2008) implement a series of field experiments and find that randomly generated visual cues such as graffiti and illegal bicycle parking cause passerby to take less ethical actions (e.g., taking a small amount of highly visible cash from an envelope in a mailbox).

Our paper provides new evidence using naturally occurring data that social disorder has a causal impact on crime—within close distances outside of the immediate vicinity of a homeless individual, we find a significant reduction in violent crime. This validates the fundamental premise of the broken windows theory that potential criminals utilize the presence of social disorder as a signal that police are less likely to respond. More generally, this result provides new evidence of the response of potential criminals to the probability of being caught. Our results are consistent with potential criminals basing decisions about whether to commit a crime in a given time and place on signals of police proximity. This is consistent with other research finding that potential criminals are deterred by more police, although our results suggest an additional level of sophistication. We also find evidence of a more direct effect of homelessness on crime, in which homeless individuals are either the victims or perpetrators, both locally and citywide. The positive citywide effect implies that reducing unsheltered homelessness would not simply displace crime to shelters or other locations to which otherwise unsheltered individuals turn. Of course, like most crime-related research, we only have data on reported crime, and so we cannot rule out the possibility that some of these effects are influenced by differences in reporting rather than criminal acts themselves.

The paper proceeds as follows: Section 2 describes the homeless, crime and weather data. Section 3 provides the methodology and results for our difference-in-differences approach. Section 4 provides the methodology and results for our natural experiment based on nighttime rainfall. Section 5 discusses the results and policy implications. Section 6 concludes.

2 Data

Our primary sources of data are reports about homeless individuals and reports of crime in New York City. The city's homeless reporting mechanism is a component of a broader '311' system in which residents can inform authorities about various conditions, such as broken streetlights, dirty conditions and blocked driveways. New Yorkers can either dial '311' on their phones or for certain report types upload information via an app or website.[2] There are over 300 report types, two of which are to report on homeless encampments and to request assistance for a homeless individual. In either case, the caller (or uploader) is asked to provide the exact location of the homeless individual. The information is then forwarded to the police department in the case of encampment reports and to social service workers in the case of assistance reports, which are then charged with responding to the incident. The city makes the homeless and other '311' reports publicly available through its Open Data portal. Figure 1 shows monthly volumes of encampment reports, and Figure 2 shows monthly volumes of assistance reports. Between 2010 and 2016, there were 21,491 homeless encampments reported and 55,861 requests for homeless assistance.

Homeless encampment reports display a clear seasonal trend, with much higher volumes during the warmer months. Cold weather is clearly a strong deterrent to sleeping on the streets.[3] They also exhibit an upward trend. While this may reflect an increase in unsheltered homelessness during the sample period, it likely reflects greater use of the '311' reporting system as well. Meanwhile, homeless assistance reports were first tracked beginning in June 2013. They increase dramatically in March 2016, due to changes in how reports can be made and to the use of city workers to locate homeless individuals and include them in the "311"

[2]Screen shots of the app are shown in the appendix.

[3]O'Flaherty and Wu (2008) finds that populations of single adult shelters in New York City are positively associated with colder weather in a given month, suggesting that at least some otherwise unsheltered individuals turn to shelters.

data.[4]

For both types of homeless reports, we observe the reported location as well as the time at which the call was placed. Figure 3 and Figure 4 map locations for encampment and assistance reports during the sample period. In a recent study, we document that homeless reports are concentrated in more affluent neighborhoods and in close proximity to subway stations and homeless shelters (Corinth and Finley 2017). Given that a homeless report requires both a homeless person and someone to report the homeless person, these locations do not necessarily represent all of the locations in which homeless people are located. This implies that our difference-in-differences approach will identify the local effect of social disorder in relatively more affluent neighborhoods. Figures 5 and Figure 6 show the distribution of reports over the time of day—the most common times are in the morning.

To investigate the effect of homelessness on crime, we use historical crime data also publicly available in New York City's Open Data portal. The crime data include all felonies, misdemeanors, and violations reported to the New York Police Department between 2006 and 2016. The monthly time series is shown in Figure 7, and Figure 8 maps crime reports by census tract. In our analysis, we focus on seven categories of crime—total, misdemeanors, felonies, violations, property crime, violent crime, and misdemeanor assault. Note that while misdemeanors, felonies and violations are collectively exhaustive, property crimes and violent crimes are not. Property crime includes burglary, larceny, motor vehicle theft and arson. Violent crime includes robbery, felony assault, rape, murder, and non-negligent manslaughter. Table 1 shows the number of crimes for each of these types by year. As with the '311' report data, the crime data report the location and time of the crime, although the locations of crime reports are based on the closest intersection or midpoint between intersections, not necessarily the exact address at which the crime occurred. Nonetheless, the granular locations of crime reports allow us to estimate the effect of homelessness on crime in highly local areas within short windows of time.

[4]See Corinth and Finley (2017) for more detailed discussion of these issues.

Our final primary source of data is the National Oceanic and Atmospheric Administration's (NOAA) climate data. We use hourly data on precipitation and temperature recorded at the La Guardia Airport weather station, due to consistent data availability over our entire period. Weather is recorded every hour, typically at the fifty-first minute. In the case that no weather was recorded at this time, the weather conditions nearest to the fifty-first minute are used in order to create a consistent hourly time-series between 2010 and 2016.

3 Difference-in-differences

3.1 Methodology

Our first approach to estimating the effect of homelessness on crime is to compare crime that occurs in small discs centered around a given homeless report (treatment area) to crime in similar areas at the same time without a homeless report (control area). We select control areas by first identifying all homeless reports that occurred at a similar time of day, and within one mile and one week (but not within one day), of the homeless report in the treatment area. This ensures that control areas are also the types of places in which homeless reports sometimes (and in fact recently) occur. We then eliminate those areas which overlap or come close to overlapping the treatment area.

Optimally, we would define the treatment period as the exact time period during which the homeless individual was actually in the reported location. However, we observe only the location of the homeless individual at a particular point in time, and so we use short windows (two hours) before and after the report time as the treatment period. Because differences may nonetheless remain between treatment and control areas, we compare differences in crime over time across areas, thus differencing out any fixed characteristics of the areas. We examine the difference in crime reported during the treatment period compared to the average of crime reported in the same time period the day before and the day after.

More formally, each homeless report $h \in \mathscr{H}$ is defined by the time $t(h) \in \mathbb{R}$ and two locational coordinates $l(h) \in \mathbb{R}^2$ at which it occurs so that $h \equiv \{t(h), l(h)\}$. Similarly, each crime report $c \in \mathscr{C}$ is defined by $c \equiv \{t(c), l(c)\}$. The units for locational coordinates are feet and the units for time are days. A disc D centered around location l with radius r is given by $D(l,r) \equiv \{m \in \mathbb{R}^2 : d(m,l) \leq r\}$, where $d(\cdot,\cdot)$ is the Euclidean metric. We define the period centered around time t with window τ as $P(t,\tau) \equiv \{v \in \mathbb{R} : |v - t| \leq \tau\}$.

We write the (average) number of crime reports Y that occur in an area centered around homeless report h', $s \in \{0,1\}$ days from $t(h)$, as

$$Y(h', h, s) \equiv \frac{1}{1+s}|\mathscr{C} \cap D(l(h'), r_L) \times (P(t(h)+s, \tau_L) \cup P(t(h)-s, \tau_L))| \tag{1}$$

In the treatment period $s = 0$ and we simply count the number of crime reports found within r_L feet of h' and within τ_L days of h. In the control period $s = 1$ and we take the average number of calls one day before and one day after the homeless report h but during the same time window as h.

For each homeless report $h \in \mathscr{H}$, we have the set of equations

$$\begin{aligned} & Y(h', h, s) = \alpha_h + \delta \mathbb{1}(h' = h) + \lambda \mathbb{1}(s = 0) + \beta \mathbb{1}(h' = h)\mathbb{1}(s = 0) + \mu_{h',s} \qquad (2) \\ & \forall s \in \{0,1\}, \\ & \forall h' \in (D(l(h), 5280 - r_L) \setminus D(l(h), 1000 + r_L)) \times (P(t(h), 7) \setminus P(t(h), 1 + \tau_L)), \\ & s.t. |td(h') - td(h)| < 2 \end{aligned}$$

where $td(\cdot)$ is the time of day expressed in hours, so that we only include homeless reports h' that occurred within two hours of the time of day of the origin homeless report h. In our baseline specification, $r_L = 300$ and $\tau_L = \frac{2}{24}$. The parameter α_h is a fixed effect corresponding to all homeless reports oriented around origin homeless report h, including the origin homeless report itself. We also include an indicator variable based on whether the homeless report is the origin (or treatment) homeless report, whether the time period is the day when the origin homeless report was made (the treatment period), along with the interaction of these two indicator variables (whose associated coefficient is the treatment effect). Furthermore, because the control period is composed of both the day before and day after the treatment period, an average is taken to maintain consistency with the treatment period. The restrictions on control discs include that they (i) fall entirely inside a one mile radius but outside of a 1,000 foot radius of the origin homeless report, (ii) occur within one week of the origin homeless report but not during the experimental period, and (iii) occur within two hours of the time of day of the origin homeless report. Figure 10 depicts the experimental region.

A potential problem in estimating equation (1) is that control areas may be contaminated by homeless reports that overlap with them during the experimental period, i.e., within one day of the origin homeless report at a similar time of day. Formally, a homeless report h' is contaminated as a control for an origin homeless report h if there exists some $h'' \in \mathscr{H}$ such that (i) $t(h'') \in P(t(h), 1 + 2\tau_L)$, (ii) $|td(h'') - td(h)| < 2\tau_L$, and (iii) $d(l(h'), l(h'')) < 2r_L$. If contaminated homeless reports are dropped as controls for an origin homeless report, they may still be used as controls for other homeless reports assuming they are not contaminated in such cases.

Origin homeless reports may be contaminated as well. Homeless reports that occur near an origin homeless report on the same day do not pose a problem for our design. While the intensity of treatment could increase if more homeless individuals in a particular area serve as a stronger signal of police non-proximity, we seek only to estimate the impact of the existence of at least one homeless individual in a particular area. However, homeless reports that occur near an origin homeless report on either the day before or the day after would lead to downward bias of the treatment effect. Formally, an origin homeless report h is contaminated if there exists some $h'' \in \mathscr{H}$ such that (i) $t(h'') \in P(t(h), 1+2\tau_L) \setminus P(t(h), 2\tau_L)$, (ii) $|td(h'') - td(h)| < 2\tau_L$, and (iii) $d(l(h), l(h'')) < 2r_L$. If contaminated origin homeless reports are dropped, they may still serve as control homeless reports for other origin homeless reports.

A final issue is that homeless individuals can be the victims, perpetrators or witnesses of crime. While effects on these margins are relevant components of the local impact of homelessness on crime, distinguishing between these effects and police signaling effects is important. For example, a witness effect that reduces crime in immediate vicinities and cancels out a signaling effect that increases crime in less immediate vicinities would support the hypothesis that signals of police presence driven by social disorder increase crime, even if homelessness itself has no net impact on crime. It is also possible that significant local effects of homelessness on crime are driven by homeless individuals being victims or perpetrators, in which case signaling effects would be unimportant. In order to distinguish between crime effects due to signals of police presence and effects due to homeless individuals being victims, perpetrators or witnesses, we differentiate between crime that occurs within very small radii of the homeless report (100 feet) and crime that occurs outside of this immediate vicinity (between 100 and 300 feet of the homeless report). In terms of equation (1), estimating the effect of homeless on crime between 100 and 300 feet requires replacing discs with rings of the form, $R(l, r_1, r_2) \equiv \{m \in \mathbb{R}^2 : r_1 < d(m, l) < r_2\}$.

3.2 Results

Difference-in-differences estimates based on entire discs, without dropping contaminated reports, are shown in Table 2.[5] Overall, we have just over 9 million observations, based on 55,181 origin homeless discs and 4.45 million control discs. It is apparent that crimes are infrequent—the average number of total crimes in a control disc is 0.0458. This is not surprising given that discs are quite small, an area of 0.01 square miles, and cover only four hours. Total crime in control discs during the treatment period falls very slightly to 0.0456, implying that little is changing during the three day experimental period in control disks. Meanwhile, the average amount of crime in treatment discs during the control period

[5] In all specifications we drop homeless reports that occur at the same location within four hours of another homeless report. We also drop origin homeless discs without at least one corresponding control disc.

is 0.0428, relatively similar to that in the control discs during this period, and grows to 0.0439 in the treatment period. The treatment effect in levels is 0.0015. We also express the treatment effect in logs, implying a 3.3 percent increase in total crime due to homelessness.

The effect of homelessness on misdemeanors is 6.2 percent and statistically different from zero at the 90 percent level. Meanwhile, the effect on felonies is -1.1 percent, the effect on violations is 1.4 percent, and the effect on property crime is -0.8 percent. None of these estimates are statistically different from zero. The effect of homelessness on violent crime is larger, 14.8 percent, though it is not statistically significant given the relative rarity of violent crime. And finally, the effect on misdemeanor assault is 34.7 percent and statistically different from zero at the 95 percent level. Results when dropping contaminated treatment and control discs are similar, although standard errors are larger due to the decrease in sample size. Estimates are shown in Table 3. The effect of homelessness on misdemeanors grows to 10.3 percent, the effect on violent crime grows to 18.8 percent, and the effect on misdemeanor assault falls to 24.7 percent.

We next consider heterogeneous effects within discs as a result of homeless individuals themselves either deterring crime through a witness effect, or through involvement as a victim or perpetrator. Table 4 shows results based on discs with radii of 100 feet. The effects on total crime and misdemeanors are about twice as large, increasing to 7.5 percent and 13.8 percent respectively. The effect on property crime is 0.01 percent. Meanwhile, the effect on violent crime is -27 percent, compared to 14.8 percent within the disc of 300 foot radius. The effect on misdemeanor assault is 65.7 percent in the smaller disc, compared to an effect of 34.7 percent in the larger one. In general then, the treatment effect for crime is larger in the immediate vicinity of the homeless report, with the exception of violent crime for which the treatment effect turns negative. Table 5 shows results based on the remaining ring, at least 100 feet but less than 300 feet away from the homeless report. Here, the effect of homelessness on violent crime is 32 percent and marginally significant. Effects on other crime types are generally near zero, with the exception of misdemeanor assault for which we still find a 23 percent increase. Table 6 shows results for the remaining ring when dropping contaminated homeless reports. Results are similar, but again standard errors are larger. The point estimate for violent crime increases to 41 percent and remains marginally statistically significant.

It is likely that all of these estimates serve as lower bounds on the effect of visible homelessness on crime, for at least two reasons. First, we use arbitrary four-hour windows centered around homeless report times as our treatment period. Homeless individuals may not be in the reported location for the entire period. If they move to other areas, the treatment effect would be weakened. Second, the homeless reports we observe account for only a fraction of the locations of all homeless individuals at a given time. While reports are likely correlated with the intensity of visible homelessness, there are almost certainly homeless individuals located in our control regions as well. Thus, we are in reality estimating the effect of homelessness that leads to a report relative to areas that may have included a homeless individual but did not lead to a report.

4 Natural experiment

4.1 Methodology

Our second approach for estimating the effect of homelessness on crime is to use nighttime rainfall as a natural experiment applied to the entire city. We expect that rain during the night will reduce the number of homeless people visible the following day, either because they enter shelters, abandoned buildings, housing, or otherwise less visible areas. Research on the determinants of homeless population sizes is consistent with this hypothesis, with cross-sectional studies finding that unsheltered homeless counts are lower in places with less precipitation (see Byrne et al. (2013) for a review of this literature). At the same time, nighttime rainfall should not affect the activities of non-homeless people the following day, controlling for daytime precipitation. We focus on rain instead of precipitation more generally because rain generally dissipates through sewers quickly after falling. While snow may indeed cause homeless individuals to find refuge indoors the prior night and be less likely to inhabit the streets the following day, accumulated snow on the ground may deter other individuals from utilizing sidewalks and roads, and some crimes may be made more difficult. Thus, we exclude winter months from our analysis.

Our general approach is to use nighttime rainfall as an instrumental variable for homeless reports the following day, in order to estimate the effect of daily homeless reports on citywide crime. Our first and second stage equations take the form[6]

$$\begin{aligned}\log(Homeless_t) =& \alpha_0 + \alpha_1 Rain_{t-0.5} + \alpha_2 Weather_t + \alpha_3 \log(Calls_t) \\ &+ \delta_{1,d(t)} + \gamma_{1,my(t)} + \epsilon_{1,t} \qquad (3)\\ \log(Crime_t) =& \beta_0 + \beta_1 \log(Homeless_t) + \beta_2 Weather_t + \beta_3 \log(Calls_t) \\ &+ \delta_{2,d(t)} + \gamma_{2,my(t)} + \epsilon_{2,t} \qquad (4)\end{aligned}$$

Each date t corresponds to the 12-hour period between 6:00am and 6:00pm, and we use the notation $t - 0.5$ to denote the 12-hour period between 6:00pm and 6:00am that directly precedes it. Rainfall is measured in inches, and we sometimes include polynomials in rainfall as well as categorical variables based on ranges of rainfall. Weather is a vector of variables including minimum and maximum daytime temperature, daytime rainfall and its square, and rainfall at 6:00am to control for any rainfall that continues through the night and into the following day. The variable $Calls_t$ is the number of non-homeless related '311' reports placed between 6:00am and 6:00pm. This variable is intended to serve as a proxy for general city activity. And finally, $\delta_{d(t)}$ is a day-of-week fixed effect corresponding to date t, and $\gamma_{my(t)}$ is a month/year fixed effect corresponding to date t. Thus, we exclusively rely on within-month/year variation to identify the effect of homelessness on crime. This is important given that homeless reports trend upward over time, and because reports exhibit

[6]Given that some variables have at least one observation with a value of zero, we use inverse hyperbolic sine transformations as approximations of logarithms that are still defined at zero.

seasonality. Summary statistics are shown in Table 7.

While instrumenting for homeless reports with the prior night's rainfall serves the primary purpose of overcoming the inherent endogeneity problem in estimating the effect of homelessness on crime, it serves an additional purpose in our context as well. Homeless reports do not constitute a census or even a representative sample of unsheltered homeless people on a given night. Rather, they reflect the interaction of a homeless person and a non-homeless person who makes the report. Thus, even if a true measure of homelessness was not endogenous in an equation explaining crime, we would still be unable to estimate the effect of homelessness because our measure of it is based on the number of non-homeless people as well, which itself is related to crime. But because nighttime rainfall presumably affects only whether homeless people are on the streets the next day, and not non-homeless people, we are able to identify the effect of unsheltered homeless people on crime.

Our key identifying assumption is that nighttime rainfall affects the number of unsheltered homeless people the following day but is uncorrelated with unobserved factors that affect crime. While this cannot be tested directly, we test whether nighttime rainfall is associated with specific non-homeless related '311' reports, many of which relate to foot traffic or related activity potentially correlated with unobserved determinants of crime. And while the first stage results directly indicate whether nighttime rainfall is strongly associated with homeless reports the following day, we also test whether nighttime rainfall is associated with higher daily shelter censuses in New York City single adult homeless shelters.[7] To the extent that rain drives homeless people off the streets, some may be pushed into shelter. This would provide additional evidence that rainfall affects homeless reports via the number of homeless people on the street rather than the number of non-homeless people reporting on them.

Finally, it may be important to distinguish between reports on homeless encampments and reports on homeless assistance. Homeless encampments entail homeless individuals accompanied by physical items used to facilitate sleeping outdoors, and thus are closely

[7]During annual counts of its homeless population, there are generally zero families found sleeping unsheltered.

linked to where people spent the prior night. Requests for homeless assistance are more likely to be based on the perceived attributes of a given individual, rather than on where they slept the previous night. We also have data on homeless assistance requests for a shorter period, and call volumes were relatively low until March 2016. Thus, we consider these report types separately.

4.2 Results

First stage estimates for homeless encampment reports based on equation (2) are shown in Table 8, with various sets of instruments based on nighttime rainfall. Specification (1) includes only a dummy variable for whether or not it rained the prior night, specification (2) includes a set of dummy variables based on light, medium and heavy rainfall, specification (3) includes a single continuous measure of rainfall in inches, and specification (4) adds squared rainfall. Across all specifications, nighttime rainfall significantly reduces homeless encampments the following day. For example, specification (1) implies that when it rains during the night, homeless encampment reports fall by 9 percent the following day. Other specifications suggest that heavier rainfall leads to greater reductions in homeless encampments than lighter rainfall, and specification (4) implies that rainfall reduces homeless encampment reports at an increasing rate. Given the strength of continuous measures of rainfall in predicting homelessness, and the significance of the squared term, we use specification (4) for our baseline results.

Other variables generally have the expected signs. Daytime rainfall significantly reduces homeless encampment reports, while warmer maximum and minimum daytime temperatures increase reports. Rainfall between 6:00am and 7:00am is positively associated with homeless encampments, implying that holding constant total daytime rainfall, the fact that rain is concentrated early in the morning rather than later in the day is associated with more encampment reports. Finally, homeless encampment reports are strongly and positively related to non-homeless related ‘311’ calls.

First stage results for homeless assistance reports are shown in Appendix Table A1. It is clear that nighttime rainfall does not significantly affect homeless assistance reports the following day. While this precludes us from considering the effect of assistance reports on crime, it provides a source of validity for encampment results. Specifically, the finding that assistance reports are unaffected by nighttime rain suggests that non-homeless people (in addition to people who appear to be homeless and in need of help) are no less likely to be outdoors and willing to report on homeless-related conditions after it rains the preceding night.

In order to inform the validity of our assumption that nighttime rainfall does not affect crime through mechanisms aside from homelessness, Table 9 replicates the first stage specification using a number of non-homeless related '311' calls. Most are unrelated or weakly related, although calls directly linked to rain are strongly related to rain the previous night. These include damaged trees and sewer conditions. Though perhaps unlikely, if such conditions have significant effects on crime, our instrumental variables estimates would not identify the effect of homelessness on crime.

As a further test of whether rainfall induces unsheltered homeless people off the street, we test whether daily shelter populations are higher during nights when there is more rainfall. While individuals may find shelter in many locations, we would still expect city shelter populations to rise with rainfall. Table 10 shows the relationship between the single adult shelter population on a given night and rainfall on the same night. The shelter data is collected by the New York City Department of Homeless Services as part of their daily census. As expected, nighttime rainfall increases adult shelter counts, although the effect is not statistically different from zero. The point estimate nonetheless suggests an important response. On the basis of specification (4) and an average daily shelter census of 11,755, one inch of nighttime rainfall increases the shelter census that night by 1,427 people. During the 2016 homeless count, conducted during the winter, 2,794 people were found sleeping on the street (New York City Hall 2016).

Second stage estimates for the impact of homeless encampments on crime, using specification (4) for the first stage, are shown in Table 11.[8] The elasticity of total crime with respect to homeless encampments is 0.03, for felonies the elasticity is 0.07, for misdemeanors it is 0.00 and for violations it is 0.03. None of these effects is significantly different from zero. At the 95 percent confidence level, we can reject that a one percent increase in homeless encampments increases total crime by more than 0.20 percent. Table 12 shows second stage results for property crime, violent crime, and misdemeanor assault. The elasticity for property crime is 0.05, for violent crime it is 0.09 and for misdemeanor assault it is 0.08. Only the effect on misdemeanor assault is marginally significant. Thus, consistent with local estimates, the largest effects are found for violent crime and misdemeanor assault.

Discussion

The broken windows theory has played an important role in how cities are policed. However, its central premise that disorder has a casual effect on crime has been difficult to test. Homelessness provides a unique setting for testing this hypothesis given that unsheltered homeless individuals are highly mobile within short periods of time and their locations are dependent on exogenous changes in environmental conditions (i.e., the rain).

From our difference-in-differences approach, we find important local effects of homelessness on violent crime. Visible homelessness increases violent crime by 15 percent within 300 feet and two hours of the homeless report, although this masks two opposing effects. Violent crime falls by 27 percent within 100 feet of the homeless report, and increases by 32 percent between 100 feet and 300 feet away. This is consistent with homeless individuals deterring violent crime in their immediate vicinity through a witness effect, but serving as a signal of lack of police presence in nearby but less immediate vicinities. Police officers are charged with responding to homeless encampments in New York City, and so the presence

[8]Second stage results based on each of the different first stage specifications are shown in Appendix Table A2.

of an encampment may reasonably signal that officers are less likely to catch a criminal at a particular place and time.[9] An alternative explanation is that homelessness simply primes violent crime, although our finding of reductions in violent crime in the immediate vicinity suggests sophistication among criminals about their likelihood of being caught that is consistent with a signaling explanation.

We also find important local effects of visible homelessness on misdemeanor assault, although the mechanism appears to differ. In this case, misdemeanor assault is not stunted within the immediate vicinity of the homeless individual, but rather, magnified. Misdemeanor assault increases by 66 percent within 100 feet of the homeless report, compared to an effect of 23 percent in the ring between 100 and 300 feet away. These results are consistent with a more direct effect of homelessness on misdemeanor assault—homeless individuals may be the victims or perpetrators. A positive effect in the outer ring may still be due to signaling, or it could be due to imprecisions in crime report locations as well as movement of the homeless individual.

While the difference-in-differences analysis suggests that there are important local effects of homelessness on crime, a remaining question is whether there are significant citywide effects. To the extent that crime effects are driven by signaling of police presence, they could in part displace crime from other areas in the city. To the extent that crime effects are driven by homeless individuals themselves being the victims or perpetrators, the same individuals may be just as likely to be involved in crime regardless of whether they are on the streets. Moreover, the aggregation of local effects may be insufficient to meaningfully impact citywide crime. At the same time, it is likely that our local estimates understate the true effect of homelessness on crime given that we only observe homelessness that triggers a report, and because we only observe the location of each homeless individual at a single point in time.

[9] If police officers responded quickly to homeless encampments, then encampments could instead be a signal of greater police presence in the near future. However, Corinth and Finley (2017) document that median response police times are over two hours. Also, for homeless assistance reports, in the majority of cases the homeless individual is not found.

Our natural experiment based on nighttime rainfall can help resolve whether homelessness has a significant effect on citywide crime. Instrumental variables estimates imply that a one percent increase in homeless encampments increases aggregate crime by 0.03 percent, and we can reject increases of more than 0.20 percent at the 95 percent level. Reductions in homelessness likely would have at most modest effects on aggregate crime. The effect on violent crime is somewhat larger, increasing by 0.09 percent for every one percent increase in homeless encampments, although it is not statistically different from zero. Misdemeanor assault increases by 0.08 percent and is marginally significant. Given that local effects on misdemeanor assault appear to be driven by direct involvement of homeless individuals, this suggests that crime does not completely follow homeless individuals into shelters or other alternative locations when it rains, although we cannot rule out the influence of changes in reporting. From a policy perspective, this suggests that reducing unsheltered homelessness would reduce crime in which homeless individuals are the victims or perpetrators.

While our results suggest that reducing unsheltered homelessness could reduce crime, the implications for broken windows policing are less clear. Findings are certainly consistent with the underlying premise of the broken windows theory, which would predict both local and citywide effects of homelessness on crime. Homelessness should increase crime locally because the probability of being caught is revealed to be lower near the homeless individual. Moreover, exogenous shocks that reduce the daily volume of homelessness should decrease crime because there are fewer areas that are revealed to have lower probabilities of being caught.

However, while our results are consistent with the fundamental premise of the broken windows theory—that disorder increases crime by signaling a lack of police enforcement—they are not necessarily consistent with its conclusion—that removing disorder reduces crime. In fact, it is quite plausible that "broken windows policing" actually increases crime. As a simple example, consider a city, with two neighborhoods A and B, that does not engage in broken windows policing. A potential criminal must choose whether to commit a crime in neighborhood A, neighborhood B, or neither. His problem is

$$\max\{V - P_A Q, V - P_B Q, 0\} \tag{5}$$

where V is the benefit from committing the crime, Q is the punishment if caught, and P is the probability of being caught in a given neighborhood. Clearly, conditional on deciding to commit a crime, the potential criminal would choose the neighborhood with the lower probability of being caught. Aggregate crime is thus a function of the minimum of P_A and P_B. Without loss of generality, let $P_A \leq P_B$.[10]

Now suppose the city engages in broken windows policing. Assuming that potential criminals know the aggregate effort that police exert across neighborhoods, any informative signal of police effort via removing disorder would increase the probability of capture in one neighborhood and decrease it in the other. Any increase in P_B would would thus reduce P_A even further and therefore increase aggregate crime. Meanwhile, small increases in P_A could decrease aggregate crime, while large increases that decreased P_B below the initial value of P_A would increase aggregate crime. Thus, it is quite plausible that social disorder increases crime, but that the only reason this effect exists is because the city engages in broken windows policing. In other words, homeless people would not send a signal of police presence if they were never removed.

Finally, there are important limitations of our results. Most notably, we provide no evidence on whether sustained changes in social disorder affect crime. Motivation for broken windows policing is derived in part from more dynamic processes in which disorder builds on itself and affects how communities organize themselves. While our findings of significant contemporaneous effects imply that disorder plays a role in decisions by potential criminals to commit crime, the types of crimes affected and the magnitudes of effects may differ when considering sustained changes in disorder. Another limitation is that we identify the effect of

[10] An optimizing police force would allocate effort so that $P_A = P_B$ (assuming the social value from crime aversion and the marginal cost of police effort are equal across neighborhoods).

disorder that is actually reported by others and concentrated in more affluent neighborhoods. Effects of disorder on crime may differ in neighborhoods in which disorder generally goes unreported. Indeed, these are the neighborhoods often thought to have the most to gain from broken windows policing. It is also possible that homelessness has different effects on crime than do other types of disorder. Most obviously, physical disorder cannot be a witness, victim or perpetrator of crime. Finally, we rely on reports of crime, not crime itself. Effects on reports of crime rather than crime itself could influence results.

Conclusion

Using detailed data on the times and locations of reports of homeless individuals and crime in New York City, we find that visible homelessness has important effects on crime. Within two hours and 100 feet of a homeless report, violent crime falls by 27 percent, consistent with a "witness" effect. Between 100 and 300 feet away, violent crime increases by 32 percent, consistent with signaling a lack of police presence. Meanwhile, misdemeanor assault increases by 66 percent within 100 feet but has a much smaller effect further out. Property crime is unaffected. Using nighttime rainfall as an instrument for homeless encampments the following day, we find at most modest effects of homelessness on citywide crime. The elasticity of homelessness with respect to violent crime is 0.09, and the elasticity of homelessness with respect to misdemeanor assault is 0.08. Elasticities for other crime types are smaller and not statistically different from zero. Our results support the hypothesis that potential criminals respond to signals of police presence via social disorder. They also suggest that reductions in visible homelessness could modestly reduce crime, in part because it would reduce signals of lack of police proximity and curtail violent crime, and in part because homeless individuals themselves would be less likely to be victims or perpetrators of misdemeanor assault. Finally, although the results support the fundamental premise of the broken windows theory that social disorder increases crime, they do not necessarily imply that broken windows policing that cracks down on disorder decreases crime.

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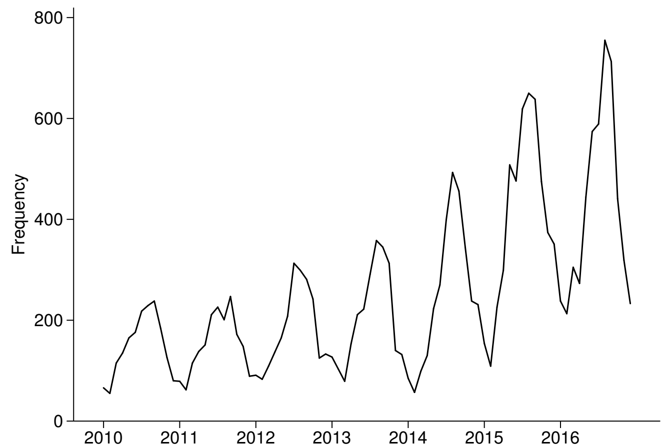


Figure 1: Monthly homeless encampment reports, 2010–2016

Source: New York City Open Data, 311 Service Requests

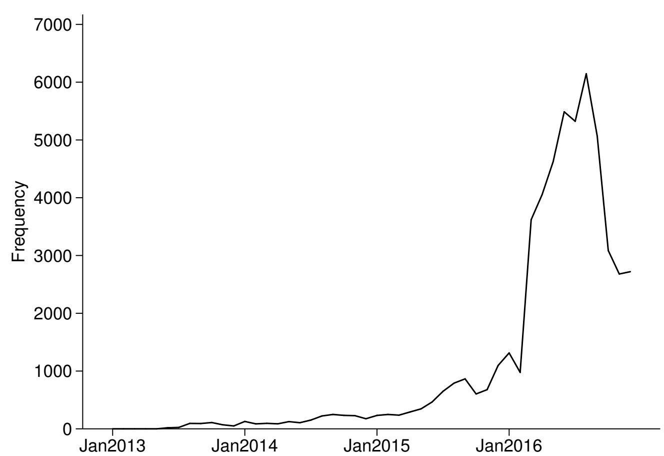


Figure 2: Monthly homeless assistance reports, June 2013–December 2016

Source: New York City Open Data, 311 Service Requests

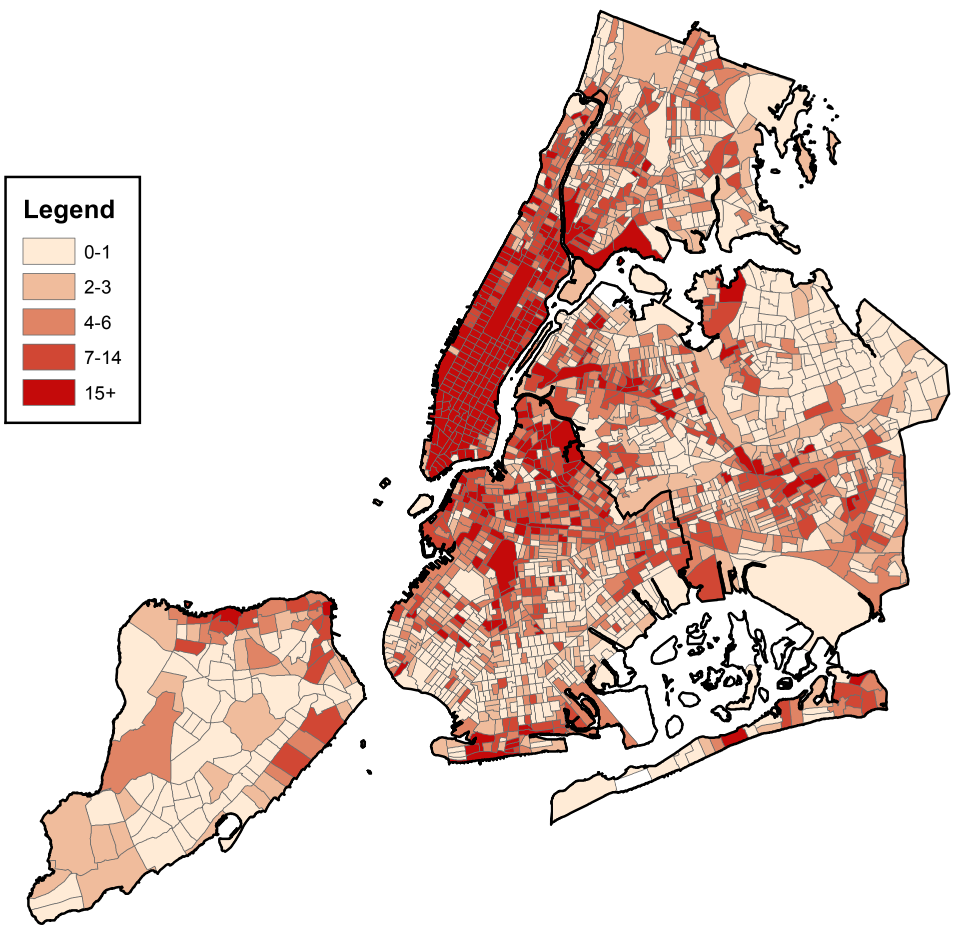


Figure 3: Homeless encampment reports by census tract, January 2010 through December 2016

Source: New York City Open Data, 311 Service Requests

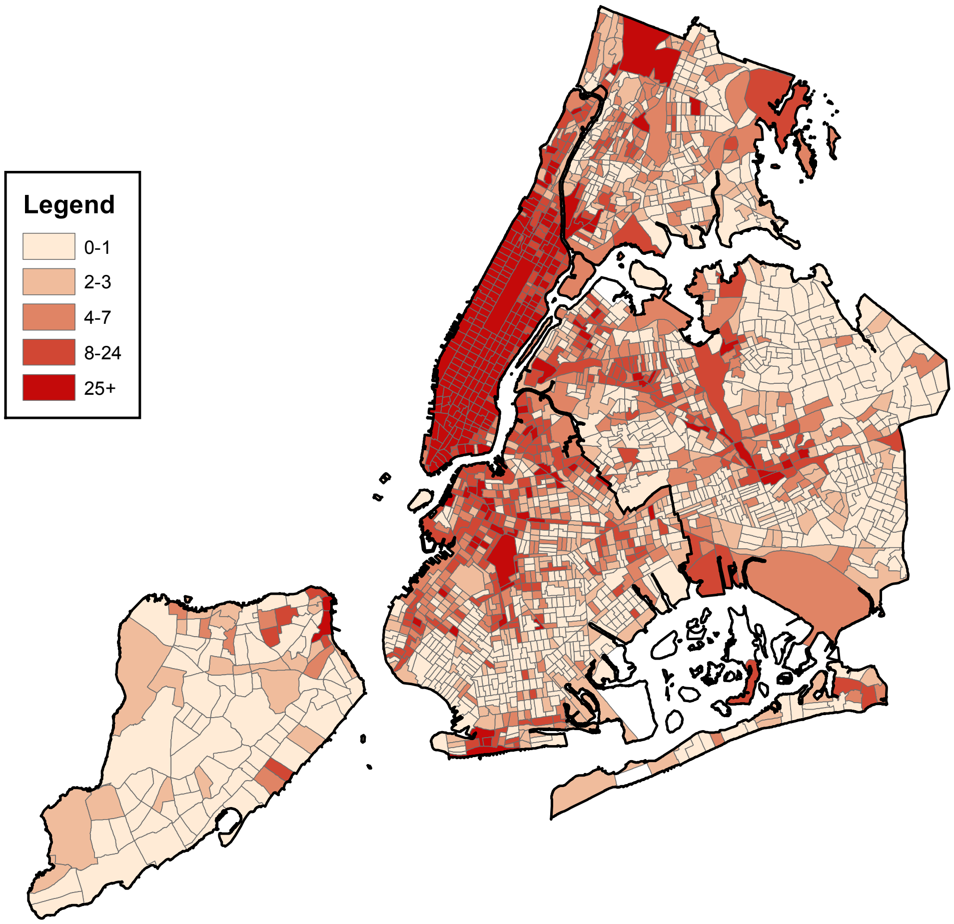


Figure 4: Homeless assistance reports by census tract, June 2013 through December 2016

Source: New York City Open Data, 311 Service Requests

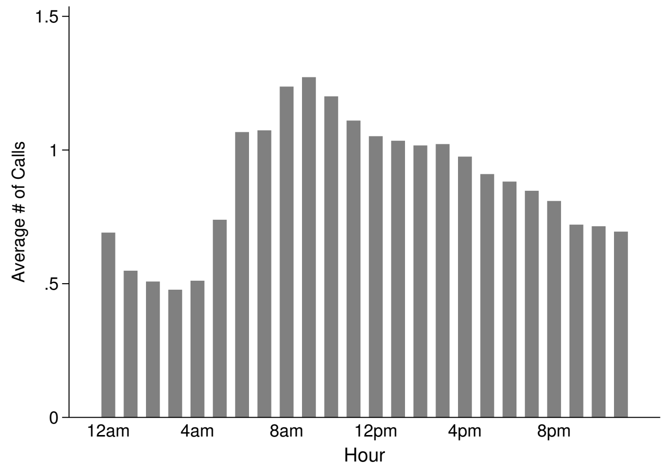


Figure 5: Average homeless encampment reports by hour of day

Source: New York City Open Data, 311 Service Requests

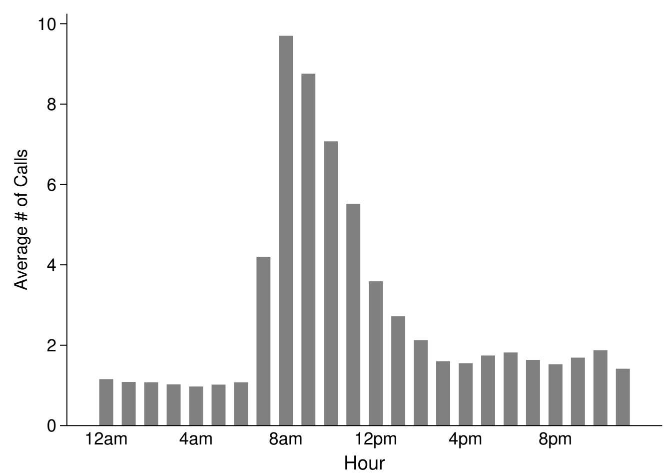


Figure 6: Average homeless assistance reports by hour of day

Source: New York City Open Data, 311 Service Requests

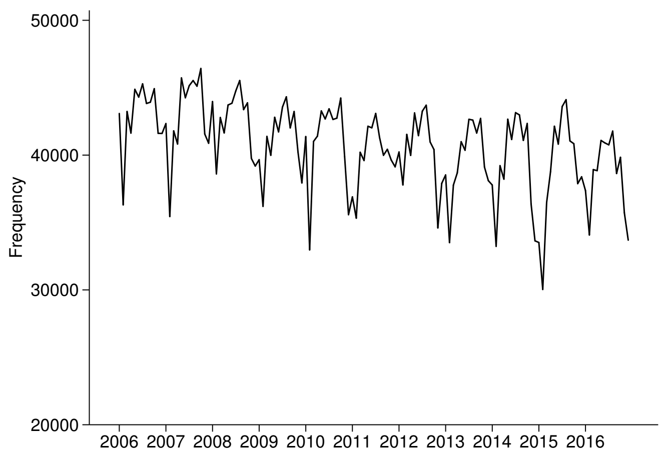


Figure 7: Monthly total crimes committed between 2006-2016

Source: New York Police Department, Historic Complaint Data

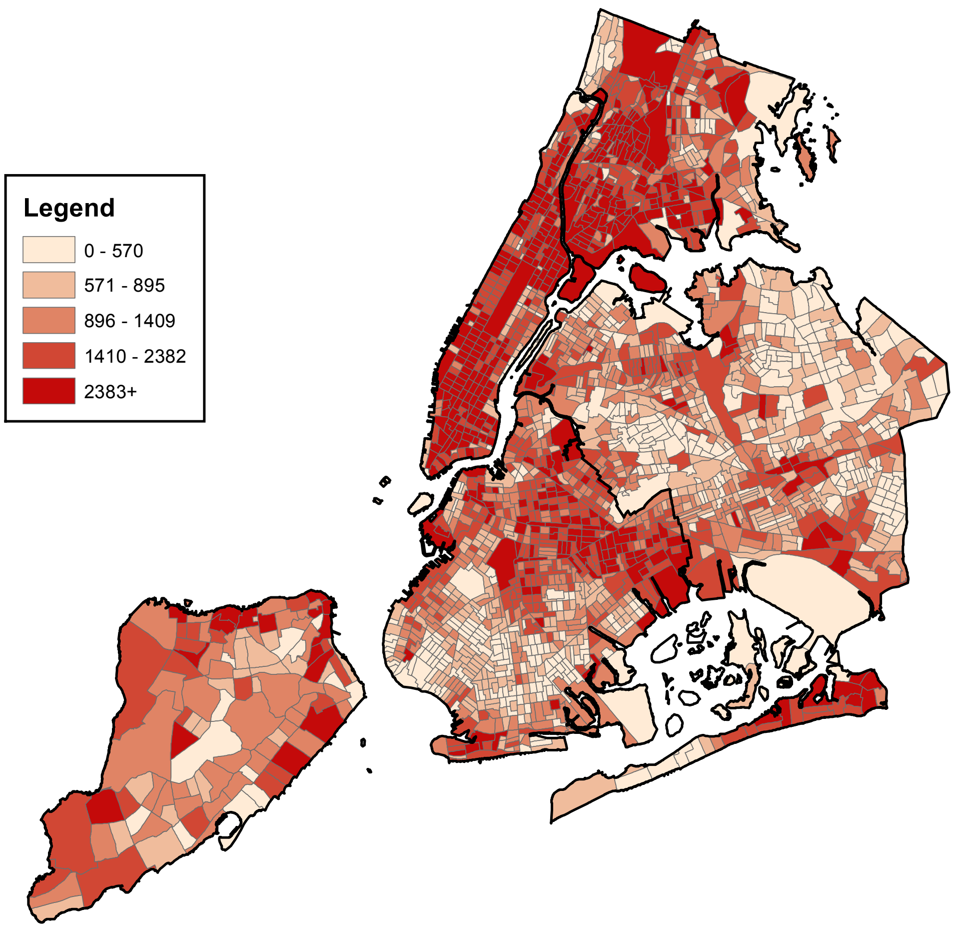


Figure 8: Crime reports by census tract, 2010 through 2016

Source: New York Police Department, Historic Complaint Data

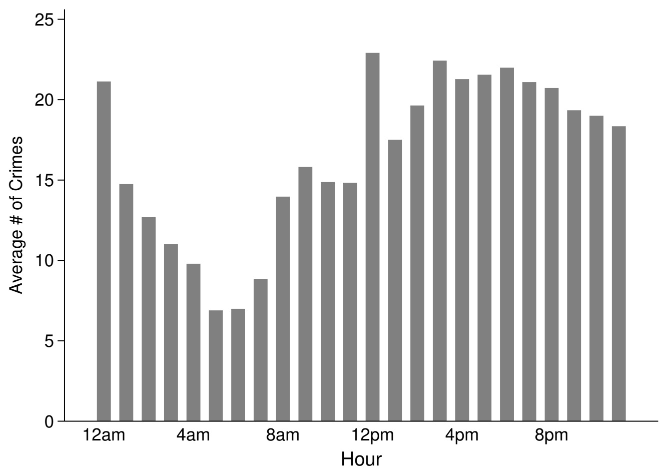


Figure 9: Average total crime by hour of day, 2010 through 2016

Source: New York Police Department, Historic Complaint Data

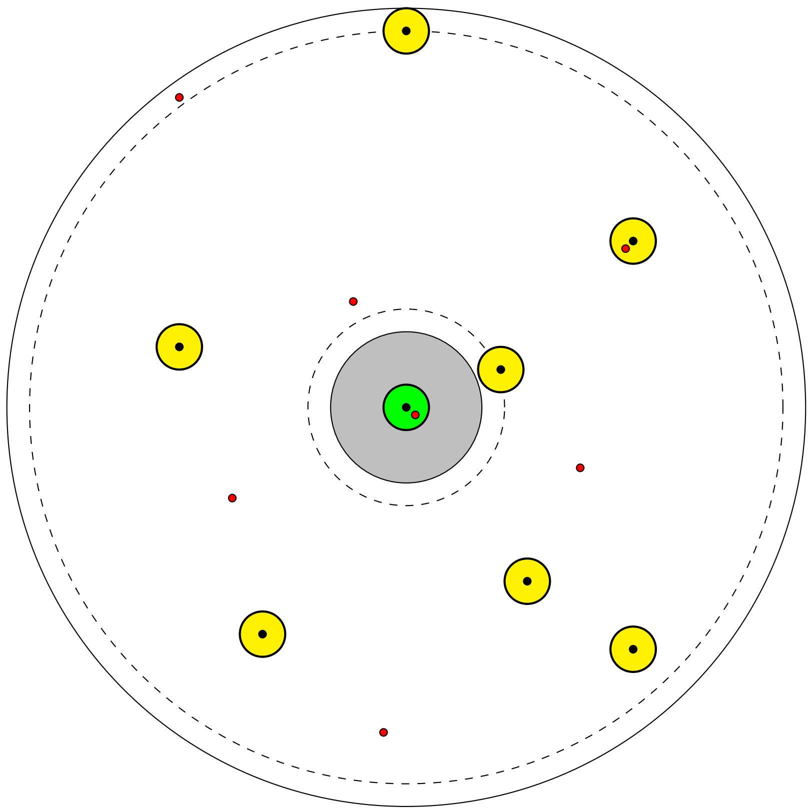


Figure 10: Diagram of experimental region for difference-in-differences analysis

Note: In the figure above, drawn to scale, the black dot is the origin homeless report. The green disc centered around the origin homeless report is the treatment region and has a radius of 300 feet. The larger gray disc has a radius of 1,000 feet. Any control disc intersecting this region is excluded. The entire disc bounds the experimental region and has a radius of 5,280 feet (one mile). Yellow discs are control regions, centered around homeless reports that occurred within one week of the origin homeless report at a similar time of day (but not during the experimental period), and have radii of 300 feet. Red dots indicate the locations of crime reports.

Table 1: Crime by type and year

Crime type	2010	2011	2012	2013	2014	2015	2016
Total	491,255	479,775	484,981	476,675	471,861	467,665	461,642
Misdemeanor	291,514	282,509	279,994	270,860	264,573	257,657	253,165
Felony	141,962	142,624	147,686	148,036	145,321	147,053	142,579
Violation	57,779	54,642	57,301	57,779	61,967	62,955	65,898
Property crime	147,278	146,514	151,404	153,243	151,662	149,728	143,448
Violent crime	36,382	38,042	39,249	38,942	36,304	37,583	36,439
Misdemeanor Assault	51,878	50,075	53,638	52,636	52,868	52,016	52,181

Note: Property crime includes arson, burglary, grand larceny, grand larceny of motor vehicle, other offenses related to theft, petit larceny, and petit larceny of a motor vehicle. Violent crime includes felony assault, murder, non-negligent manslaughter, rape and robbery.
Source: New York Police Department, Historic Complaint Data

Table 2: Difference in differences estimates by crime type (disc radius of 300 feet)

Crime Type	Disc	Control Period	Treatment Period	Difference	Diff-in-diff (levels)	Diff-in-diff (logs)
All	Control	0.04584	0.04556	-0.00028		
		(0.000152)	(0.000153)	(0.000304)		
	Treatment	0.04277	0.04394	0.00117	0.001445	0.033
		(0.000746)	(0.00101)	(0.001135)	(0.001174)	(0.0269)
Misdemeanor	Control	0.02581	0.02551	-0.0003		
		(0.000116)	(0.000116)	(0.000231)		
	Treatment	0.02396	0.0252	0.00124	0.00154*	0.0621*
		(0.000566)	(0.000779)	(0.000875)	(0.000903)	(0.0363)
Felony	Control	0.01528	0.01529	0.00001		
		(0.000084)	(0.000084)	(0.000168)		
	Treatment	0.01418	0.01404	-0.00014	-0.000159	-0.0112
		(0.000391)	(0.000534)	(0.000618)	(0.00064)	(0.0452)
Violation	Control	0.00475	0.00476	0.00001		
		(0.000045)	(0.000046)	(0.000091)		
	Treatment	0.00462	0.00469	0.00007	0.000064	0.0139
		(0.000221)	(0.0003)	(0.000358)	(0.00037)	(0.0791)
Property Crime	Control	0.02622	0.02597	-0.00025		
		(0.000115)	(0.000115)	(0.00023)		
	Treatment	0.02465	0.02424	-0.00041	-0.000157	-0.0071
		(0.000545)	(0.000726)	(0.000813)	(0.000846)	(0.0345)
Violent Crime	Control	0.00165	0.00169	0.00004		
		(0.000026)	(0.000026)	(0.000052)		
	Treatment	0.00152	0.00181	0.00029	0.000245	0.1478
		(0.000134)	(0.0002)	(0.000231)	(0.000236)	(0.1387)
Misdemeanor Assault	Control	0.00231	0.00206	-0.00025		
		(0.00003)	(0.000031)	(0.000061)		
	Treatment	0.00187	0.00236	0.00049	0.000739**	0.3466**
		(0.000171)	(0.00025)	(0.00029)	(0.000296)	(0.1363)

Note: The sample includes 55,181 origin homeless reports, and 4,449,077 control homeless reports. Altogether, there are 9,008,516 observations. Fixed effects based on the origin homeless report are included. Treatment discs are centered around origin homeless reports. Control discs are centered around homeless reports that occurred within one week and one mile of the origin homeless report, but outside 1,300 feet, within two hours of the time of day. Treatment discs without any associated control discs are dropped. Homeless reports that occurred within four hours of another homeless report in the same location are dropped. Robust standard errors clustered by the origin homeless report are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1 (shown for diff-in-diff estimates only)

Table 3: Difference in differences estimates by crime type (disc radius of 300 feet), dropping contaminated

Crime Type	Disc	Control Period	Treatment Period	Difference	Diff-in-diff (levels)	Diff-in-diff (logs)
All	Control	0.04189	0.04239	0.00051		
		(0.000363)	(0.000377)	(0.000723)		
	Treatment	0.04072	0.0427	0.00198	0.001475	0.0355
		(0.000997)	(0.001347)	(0.001528)	(0.001689)	(0.0401)
Misdemeanor	Control	0.02415	0.0238	-0.00036		
		(0.000273)	(0.000284)	(0.000544)		
	Treatment	0.02354	0.02572	0.00218	0.002536*	0.1034**
		(0.00076)	(0.001042)	(0.001183)	(0.001301)	(0.0524)
Felony	Control	0.01352	0.01434	0.00081		
		(0.000194)	(0.000201)	(0.000386)		
	Treatment	0.01294	0.01262	-0.00032	-0.001134	-0.0835
		(0.00052)	(0.000699)	(0.000814)	(0.000902)	(0.0698)
Violation	Control	0.00421	0.00426	0.00005		
		(0.000109)	(0.000114)	(0.000217)		
	Treatment	0.00424	0.00437	0.00012	0.000073	0.0168
		(0.000309)	(0.000411)	(0.000499)	(0.000545)	(0.1264)
Property Crime	Control	0.02298	0.02364	0.00066		
		(0.000265)	(0.000272)	(0.000525)		
	Treatment	0.02234	0.02289	0.00055	-0.000107	-0.0039
		(0.00071)	(0.000948)	(0.001072)	(0.001193)	(0.0523)
Violent Crime	Control	0.00155	0.00163	0.00008		
		(0.000059)	(0.000061)	(0.000117)		
	Treatment	0.00144	0.00183	0.00039	0.000309	0.1884
		(0.000185)	(0.000275)	(0.000323)	(0.000343)	(0.2048)
Misdemeanor Assault	Control	0.00283	0.00253	-0.0003		
		(0.000093)	(0.000096)	(0.000185)		
	Treatment	0.00235	0.00269	0.00034	0.00064	0.2469
		(0.000248)	(0.000348)	(0.000414)	(0.000452)	(0.1749)

Note: The sample includes 28,019 origin homeless reports, and 317,347 control homeless reports. Altogether, there are 690,732 observations. Fixed effects based on the origin homeless report are included. Treatment discs are centered around origin homeless reports. Control discs are centered around homeless reports that occurred within one week and one mile of the origin homeless report, but outside 1,300 feet, within two hours of the time of day. Treatment discs without any associated control discs are dropped, as are contaminated treatment and control discs. Homeless reports that occurred within four hours of another homeless report in the same location are dropped. Robust standard errors clustered by the origin homeless report are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1 (shown for diff-in-diff estimates only)

Table 4: Difference in differences estimates by crime type (disc radius of 100 feet)

Crime Type	Disc	Control Period	Treatment Period	Difference	Diff-in-diff (levels)	Diff-in-diff (logs)
All	Control	0.0095	0.00945	-0.00005		
		(0.000049)	(0.000049)	(0.000098)		
	Treatment	0.0094	0.01008	0.00068	0.000727	0.0748
		(0.000329)	(0.000461)	(0.000523)	(0.000532)	(0.054)
Misdemeanor	Control	0.00565	0.00552	-0.00013		
		(0.000038)	(0.000038)	(0.000076)		
	Treatment	0.0053	0.00594	0.00064	0.000774*	0.1381*
		(0.000243)	(0.000352)	(0.000396)	(0.000403)	(0.0705)
Felony	Control	0.00293	0.00297	0.00003		
		(0.000027)	(0.000027)	(0.000053)		
	Treatment	0.00324	0.00316	-0.00008	-0.000115	-0.037
		(0.000184)	(0.000257)	(0.000298)	(0.000302)	(0.0954)
Violation	Control	0.00091	0.00096	0.00005		
		(0.000016)	(0.000016)	(0.000031)		
	Treatment	0.00087	0.00099	0.00012	0.000068	0.074
		(0.000096)	(0.000136)	(0.000164)	(0.000167)	(0.177)
Property Crime	Control	0.00545	0.00544	-0.00001		
		(0.000038)	(0.000038)	(0.000076)		
	Treatment	0.00561	0.0056	-0.00001	0.000004	0.0008
		(0.000244)	(0.00033)	(0.000371)	(0.000378)	(0.0675)
Violent Crime	Control	0.00037	0.00035	-0.00001		
		(0.000008)	(0.000008)	(0.000016)		
	Treatment	0.00054	0.0004	-0.00014	-0.000129	-0.2695
		(0.000077)	(0.000098)	(0.000119)	(0.00012)	(0.2777)
Misdemeanor Assault	Control	0.0005	0.00042	-0.00008		
		(0.00001)	(0.00001)	(0.000019)		
	Treatment	0.00044	0.00071	0.00027	0.00035**	0.6566**
		(0.000083)	(0.000133)	(0.000154)	(0.000155)	(0.2649)

Note: The sample includes 55,849 origin homeless reports, and 4,857,287 control homeless reports. Altogether, there are 9,826,272 observations. Fixed effects based on the origin homeless report are included. Treatment discs are centered around origin homeless reports. Control discs are centered around homeless reports that occurred within one week and one mile of the origin homeless report, but outside 1,100 feet, within two hours of the time of day. Treatment discs without any associated control discs are dropped. Homeless reports that occurred within four hours of another homeless report in the same location are dropped. Robust standard errors clustered by the origin homeless report are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1 (shown for diff-in-diff estimates only)

Table 5: Difference in differences estimates by crime type (ring between 100 and 300 feet)

Crime Type	Disc	Control Period	Treatment Period	Difference	Diff-in-diff (levels)	Diff-in-diff (logs)
All	Control	0.03632	0.03611	-0.00021		
		(0.000126)	(0.000127)	(0.000253)		
	Treatment	0.03334	0.03377	0.00043	0.000636	0.0185
		(0.000659)	(0.000884)	(0.001008)	(0.001037)	(0.0307)
Misdemeanor	Control	0.02015	0.01998	-0.00017		
		(0.000095)	(0.000096)	(0.00019)		
	Treatment	0.01867	0.01921	0.00054	0.000718	0.0374
		(0.000503)	(0.000682)	(0.000779)	(0.000799)	(0.0419)
Felony	Control	0.01234	0.01233	0		
		(0.000071)	(0.000071)	(0.000142)		
	Treatment	0.0109	0.01086	-0.00005	-0.000044	-0.004
		(0.000342)	(0.000465)	(0.00054)	(0.000558)	(0.0509)
Violation	Control	0.00384	0.0038	-0.00003		
		(0.000038)	(0.000039)	(0.000077)		
	Treatment	0.00377	0.0037	-0.00007	-0.000038	-0.0104
		(0.000197)	(0.000267)	(0.000319)	(0.000328)	(0.088)
Property Crime	Control	0.02076	0.02053	-0.00022		
		(0.000094)	(0.000095)	(0.000189)		
	Treatment	0.01899	0.01857	-0.00043	-0.000202	-0.0118
		(0.000481)	(0.000638)	(0.000726)	(0.00075)	(0.0399)
Violent Crime	Control	0.00128	0.00134	0.00006		
		(0.000022)	(0.000022)	(0.000043)		
	Treatment	0.00097	0.00141	0.00043	0.000373*	0.3225**
		(0.000108)	(0.000173)	(0.000198)	(0.000202)	(0.1632)
Misdemeanor Assault	Control	0.0018	0.00165	-0.00016		
		(0.000026)	(0.000026)	(0.000052)		
	Treatment	0.00144	0.00166	0.00022	0.000373	0.2303
		(0.000148)	(0.000211)	(0.000246)	(0.000252)	(0.1586)

Note: The sample includes 55,181 origin homeless reports, and 4,449,077 control homeless reports. Altogether, there are 9,008,516 observations. Fixed effects based on the origin homeless report are included. Treatment rings are centered around origin homeless reports. Control rings are centered around homeless reports that occurred within one week and one mile of the origin homeless report, but outside 1,300 feet, within two hours of the time of day. Treatment rings without any associated control rings are dropped. Homeless reports that occurred within four hours of another homeless report in the same location are dropped. Robust standard errors clustered by the origin homeless report are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1 (shown for diff-in-diff estimates only)

Table 6: Difference in differences estimates by crime type (ring between 100 and 300 feet), dropping contaminated

Crime Type	Disc	Control Period	Treatment Period	Difference	Diff-in-diff (levels)	Diff-in-diff (logs)
All	Control	0.03342	0.03414	0.00072		
		(0.000308)	(0.000322)	(0.000614)		
	Treatment	0.03156	0.03313	0.00157	0.000846	0.0271
		(0.000881)	(0.001194)	(0.001357)	(0.001489)	(0.0454)
Misdemeanor	Control	0.0192	0.01906	-0.00014		
		(0.000236)	(0.000247)	(0.00047)		
	Treatment	0.0183	0.02026	0.00196	0.002103*	0.1092*
		(0.000674)	(0.000936)	(0.001066)	(0.001164)	(0.0596)
Felony	Control	0.01089	0.01179	0.0009		
		(0.000171)	(0.000178)	(0.000341)		
	Treatment	0.01001	0.00978	-0.00023	-0.001128	-0.1026
		(0.000459)	(0.000615)	(0.000717)	(0.000795)	(0.0788)
Violation	Control	0.00333	0.0033	-0.00003		
		(0.000086)	(0.00009)	(0.000172)		
	Treatment	0.00326	0.0031	-0.00016	-0.000129	-0.0411
		(0.000267)	(0.000349)	(0.000423)	(0.000457)	(0.1441)
Property Crime	Control	0.01829	0.01897	0.00069		
		(0.000228)	(0.000234)	(0.000451)		
	Treatment	0.01745	0.01766	0.00021	-0.000471	-0.0246
		(0.000635)	(0.000846)	(0.000963)	(0.001063)	(0.0598)
Violent Crime	Control	0.00129	0.00145	0.00016		
		(0.000054)	(0.000057)	(0.000107)		
	Treatment	0.00097	0.00165	0.00068	0.000522*	0.4141*
		(0.000155)	(0.000258)	(0.000295)	(0.000315)	(0.2299)
Misdemeanor Assault	Control	0.00222	0.00204	-0.00018		
		(0.000077)	(0.00008)	(0.000153)		
	Treatment	0.0017	0.00195	0.00025	0.000428	0.2209
		(0.000212)	(0.000302)	(0.000354)	(0.000385)	(0.203)

Note: The sample includes 28,019 origin homeless reports, and 317,347 control homeless reports. Altogether, there are 690,732 observations. Fixed effects based on the origin homeless report are included. Treatment rings are centered around origin homeless reports. Control rings are centered around homeless reports that occurred within one week and one mile of the origin homeless report, but outside 1,300 feet, within two hours of the time of day. Treatment rings without any associated control rings are dropped, as are contaminated treatment and control rings. Homeless reports that occurred within four hours of another homeless report in the same location are dropped. Robust standard errors clustered by the origin homeless report are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1 (shown for diff-in-diff estimates only)

Table 7: Summary Statistics

	Observations	Mean	Median	Std. dev.
"311" report variables				
Homeless assistance	1,002	38.80	7	59.221
Homeless encampment	1,925	6.78	5	5.200
Adult shelter population	892	11,758	11,922	1,418
Non-homeless "311" reports	1,925	2,879.86	2,910	1,111.73
Crime variables				
Total crime	1,926	609.77	628	88.49
Misdemeanor	1,926	350.15	355	50.98
Felony	1,926	198.79	204	37.19
Violation	1,926	60.82	60	12.07
Property crime	1,926	245.03	252	41.86
Violent crime	1,926	42.32	42	9.03
Misdemeanor assault	1,926	64.99	64	11.11
Weather variables				
Daily rain (inches)	1,925	0.0541	0	0.228
Night rain (inches)	1,925	0.0654	0	0.247
Rain 6am (inches)	1,816	0.0046	0	0.037
Max temp (degrees Fahrenheit)	1,921	69.00	70	15.00
Min temp (degrees Fahrenheit)	1,919	57.51	58	13.74

Note: Sample excludes days in December, January and February. Homeless assistance reports are first available in June 2013. Homeless shelter population is first available in August 2013. Daily rain is total rain between 6:00am and 6:00pm, while night rain is total rain between 6:00pm and 6:00am the previous night. Max and min temp are the maximum and minimum temperatures for the period from 6:00am to 6:00pm. Rain 6am is the sum of rainfall between 5:51am and 6:51am; rainfall during this hour is missing for a small subset of observations.
Sources: New York City Open Data, 311 Service Requests; New York Department of Homeless Services, Homeless Shelter Census; New York Police Department, Historic Complaint Data; National Oceanic and Atmospheric Administration Climate Data

Table 8: First stage estimates, impact of nighttime rainfall on homeless encampment reports

	(1)	(2)	(3)	(4)
Night rain dummy	-0.0917**			
	(0.0357)			
Night low rain		-0.0829**		
		(0.0372)		
Night medium rain		0.0181		
		(0.0918)		
Night high rain		-0.420**		
		(0.169)		
Night rain			-0.243***	-0.0690
			(0.0618)	(0.106)
Night rain^2				-0.0909**
				(0.0370)
Daily rain	-0.259**	-0.251**	-0.235**	-0.233**
	(0.109)	(0.108)	(0.105)	(0.105)
Daily rain^2	-0.00343	-0.00644	-0.0122	-0.00624
	(0.0259)	(0.0270)	(0.0265)	(0.0267)
Max. temp	0.00642**	0.00638**	0.00673**	0.00728**
	(0.00303)	(0.00301)	(0.00298)	(0.00298)
Min. temp	0.00864***	0.00882***	0.00837**	0.00786**
	(0.00330)	(0.00330)	(0.00332)	(0.00330)
Rain 6am	0.629	0.697*	0.784**	0.645*
	(0.448)	(0.407)	(0.389)	(0.372)
Log(calls)	0.574***	0.593***	0.597***	0.604***
	(0.108)	(0.108)	(0.106)	(0.106)
Observations	1,810	1,810	1,810	1,810
R^2	0.599	0.601	0.602	0.603
Kleibergen-Paap F-stat.	6.597	3.701	15.46	22.08

Note: The dependent variable for all specifications is the log of daily homeless encampment reports. Daily rain is total rainfall in inches between 6:00am and 6:00pm, while night rain is total rainfall in inches between 6:00pm and 6:00am the previous night. Max and min temp are the maximum and minimum temperatures for the period from 6:00am to 6:00pm. Rain 6am is the sum of rainfall between 5:51am and 6:51am. Night low rain, night medium rain, and night high rain are a series of dummy variables equal to 1 if nightime rainfall is strictly positive and less than 0.5 inches, between 0.5 inches and 1 inch, and greater than 1 inch respectively. log(calls) is the log of daily 311 reports excluding homeless encampment and homeless assistance reports. The months December through February are excluded from all specifications. Kleibergen-Paap F-statistic is based on excluded instruments (nighttime rainfall variables). All specifications include year/month and day-of-week fixed effects. Heteroskedastic and autocorrelation consistent standard errors with a bandwidth of seven days are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Impact of nighttime rainfall on selected "311" reports

Complaint	Total Reports	Rain Coeff.	Rain SE	Rain^2 Coeff.	Rain^2 SE	F-statistic
All non-homeless	5,543,729	0.0103	0.0477	0.0307	0.0281	2.956
Air Quality	32,044	-0.127	0.0949	-0.025	0.0438	4.33
Blocked Driveway	214,732	0.0885	0.0874	-0.0808	0.0701	0.678
Broken Muni Meter	104,636	-0.166	0.145	0.0612	0.0493	0.774
Broken Parking Meter	10,979	0.0824	0.109	-0.0662	0.0506	0.962
Building/Use	120,101	0.00219	0.1	-0.0618	0.0708	1.343
Consumer Complaint	75,442	-0.0275	0.0539	-0.00206	0.0184	1.177
Damaged Tree	142,041	0.723***	0.148	-0.112	0.0821	21.38
Derelict Vehicles	133,875	-0.089	0.0688	0.000864	0.0439	3.466
Dirty Conditions	146,898	-0.198***	0.0706	0.0453	0.0278	4.926
Construction/Plumbing	144,113	-0.0599	0.0829	0.0662	0.046	1.492
Graffiti	63261	-0.342***	0.129	0.154***	0.0536	4.222
Highway Condition	15,904	0.225*	0.121	-0.0153	0.0712	4.261
Illegal Tree Damage	10,669	-0.113	0.157	-0.00143	0.0925	0.889
Missed Collection	96,791	0.15	0.111	-0.0452	0.041	0.979
New Tree Request	66,914	-0.0691	0.153	0.0212	0.0655	0.102
Noise	129253	-0.0326	0.0727	-0.0341	0.0456	2.812
Noise - Commercial	26,988	-0.154	0.109	0.00712	0.0553	2.154
Noise - Residential	193,665	0.0674	0.0825	-0.0594	0.0577	0.549
Noise - Street/Sidewalk	45,063	-0.205	0.138	-0.0299	0.0848	6.741
Noise - Vehicle	29,509	0.102	0.117	-0.107	0.086	0.89
Overgrown Tree/Branches	81,806	0.303**	0.139	-0.109***	0.0382	4.366
Sidewalk Condition	49,364	-0.229	0.17	0.114*	0.0668	1.447
Sanitation Condition	131,639	-0.187***	0.046	0.028	0.0203	17.11
Sewer	152,444	0.465***	0.0787	-0.0454	0.0518	32.31
Sidewalk Condition	31,367	-0.0624	0.103	0.0273	0.0493	0.188
Street Condition	400,623	0.0138	0.0599	0.0103	0.0217	1.818
Street Light Condition	443,425	-0.136	0.137	0.0711	0.057	0.807
Street Sign - Damaged	22,179	0.0999	0.127	-0.0459	0.076	0.314
Street Sign - Missing	16,328	0.0243	0.115	0.00227	0.0482	0.105
Taxi Complaint	59,190	0.0638	0.0498	-0.0151	0.0201	0.968
Traffic Signal Condition	155,619	0.228***	0.0747	0.0264	0.0344	18.17
Vacant Lot	11,450	-0.333***	0.126	0.116**	0.0505	3.522
Water System	244,099	-0.184***	0.0632	0.0681***	0.0201	5.732

Note: Total reports indicates total reports during the sample period. Coefficients and standard errors for rain and rain^2 correspond to nighttime rainfall. Control variables are the same as those used in specification (4) in Table 8: daily rain and its square, maximum and minimum temperature, 6am rain, and log of non-homeless reports. The final column displays the Kleibergen-Paap F-statistic based on nighttime rainfall and nighttime rainfall squared. The months December through February are excluded from all estimations. All regressions include year/month and day-of-week fixed effects. Heteroskedastic and autocorrelation consistent standard errors with a bandwidth of seven days are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 10: Impact of nighttime rainfall on single adult homeless shelter population

	(1)	(2)	(3)	(4)
Rain dummy	0.0556			
	(0.0388)			
Night low rain		0.0440		
		(0.0355)		
Night medium rain		0.163		
		(0.110)		
Night high rain		0.0192		
		(0.0242)		
Night rain			0.0650	0.155
			(0.0453)	(0.103)
Night rain^2				-0.0404
				(0.0292)
Daily rain	0.133	0.149	0.170	0.166
	(0.105)	(0.109)	(0.128)	(0.126)
Daily rain^2	-0.0773	-0.0877	-0.103	-0.0977
	(0.0659)	(0.0690)	(0.0815)	(0.0792)
Max. temp	0.00595	0.00601	0.00558	0.00588
	(0.00462)	(0.00464)	(0.00436)	(0.00451)
Min. temp	-0.00736	-0.00741	-0.00714	-0.00740
	(0.00652)	(0.00653)	(0.00639)	(0.00647)
Rain 6am	-0.255	-0.400	-0.185	-0.265
	(0.167)	(0.266)	(0.120)	(0.170)
Log(calls)	-0.0532	-0.0449	-0.0503	-0.0465
	(0.0823)	(0.0838)	(0.0810)	(0.0807)
Observations	842	842	842	842
R^2	0.088	0.089	0.087	0.088
Kleibergen-Paap F-stat.	2.049	0.975	2.052	1.188

Note: Dependent variable is the log of the single adult homeless shelter population in New York City on the night of rainfall. Other weather variables correspond to weather the following day, remaining consistent with Table 8. Daily rain is total rain between 6:00am and 6:00pm, while max and min temp are the maximum and minimum temperatures for the period from 6:00am to 6:00pm. Rain 6am is the sum of rainfall between 5:51am and 6:51am. log(calls) is the log of daily 311 reports excluding homeless encampment and homeless assistance reports the following day. The months December through February are excluded from all specifications. Kleibergen-Paap F-statistic is based on excluded instruments (nighttime rainfall variables). All regressions include year/month and day-of-week fixed effects. Heteroskedastic and autocorrelation consistent standard errors with a bandwidth of seven days are shown in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 11: Second stage estimates, impact of homeless encampments on crime (total, felonies, misdemeanors and violations)

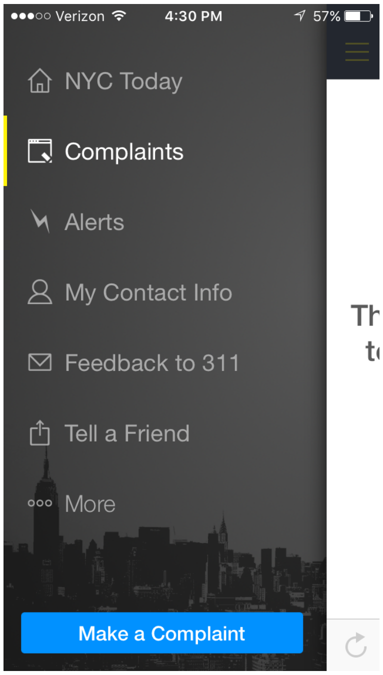
	(1) log(total)	(2) log(felony)	(3) log(misdemeanor)	(4) log(violation)
Log(encampment)	0.0280	0.0730	0.00156	0.0309
	(0.0878)	(0.102)	(0.0836)	(0.0869)
Daily rain	-0.106***	-0.0426	-0.135***	-0.129***
	(0.0273)	(0.0326)	(0.0281)	(0.0305)
Daily rain^2	0.0118***	-9.59e-06	0.0173***	0.0178***
	(0.00397)	(0.00589)	(0.00429)	(0.00570)
Max. temp	0.000799	9.17e-05	0.00136*	0.000203
	(0.000834)	(0.00103)	(0.000804)	(0.00103)
Min. temp	-0.000587	-0.00102	-0.000870	0.00130
	(0.000864)	(0.00105)	(0.000909)	(0.00106)
Rain 6am	-0.00147	0.0185	0.0667	
	(0.0703)	(0.0938)	(0.0791)	(0.0948)
Log(calls)	0.221***	0.221***	0.224***	0.218***
	(0.0582)	(0.0676)	(0.0552)	(0.0606)
Observations	1,810	1,810	1,810	1,810
R^2	0.767	0.728	0.710	0.571

Note: log(encampment) is the predicted value of the log of daily homeless encampment reports based on specification (4) in Table 8. Daily rain is total rain between 6:00am and 6:00pm, while max and min temp are the maximum and minimum temperatures for the period from 6:00am to 6:00pm. Rain 6am is the sum of rainfall between 5:51am and 6:51am. log(calls) is the log of daily 311 reports excluding homeless encampment and homeless assistance reports. The months December through February are excluded from all specifications. All regressions include year/month and day-of-week fixed effects. Heteroskedastic and autocorrelation consistent standard errors with a bandwidth of seven days are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

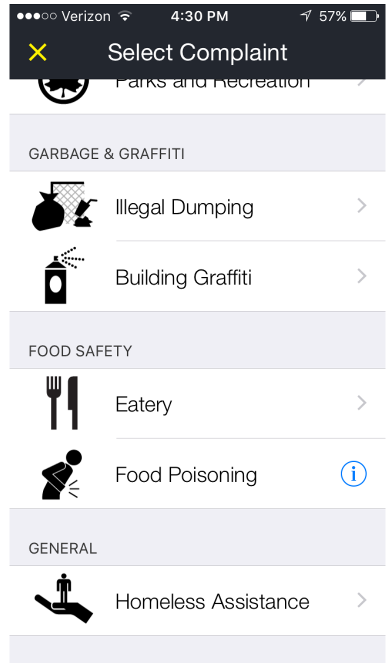
Table 12: Second stage estimates, impact of homeless encampments on crime (property crime, violent crime and misdemeanor assault)

	(1) log(property crime)	(2) log(violent crime)	(3) log(mis. assault)
Log(encampment)	0.0496	0.0919	0.0780*
	(0.107)	(0.0696)	(0.0402)
Daily rain	-0.110***	0.0345	-0.134***
	(0.0273)	(0.0392)	(0.0314)
Daily rain^2	0.0127**	-0.00897	0.0289***
	(0.00580)	(0.00899)	(0.00839)
Max. temp	0.000567	0.000504	0.00118
	(0.000957)	(0.00120)	(0.000895)
Min. temp	-0.00112	0.000784	0.000327
	(0.00106)	(0.00150)	(0.00108)
Rain 6am	0.0666	-0.173	0.0771
	(0.0848)	(0.138)	(0.134)
Log(calls)	0.200***	0.169***	0.0741**
	(0.0684)	(0.0510)	(0.0333)
Observations	1,810	1,810	1,810
R^2	0.748	0.212	0.186

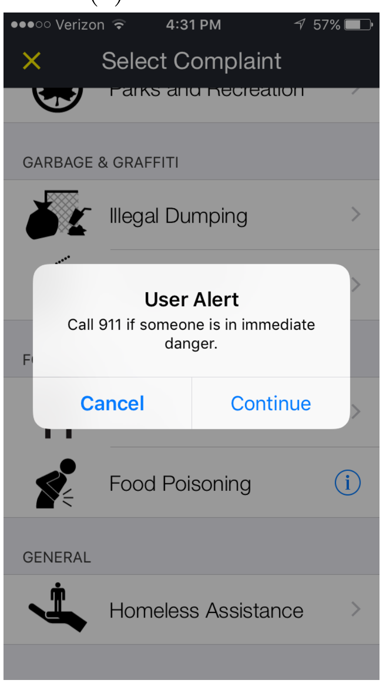
Note: log(encampment) is the predicted value of the log of daily encampment calls based on specification (4) in Table 8. Daily rain is total rain between 6:00am and 6:00pm, while max and min temp are the maximum and minimum temperatures for the period from 6:00am to 6:00pm. Rain 6am is the sum of rainfall between 5:51am and 6:51am. log(calls) is the log of daily 311 reports excluding homeless encampment and homeless assistance reports. The months December through February are excluded from all specifications. All regressions include year/month and day-of-week fixed effects. Heteroskedastic and autocorrelation consistent standard errors with a bandwidth of seven days are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1



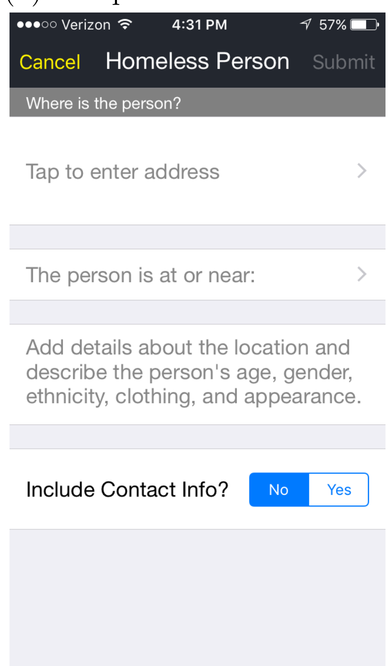
(a) Home screen



(b) Complaint selection screen



(c) Alert popup



(d) Homeless assistance form

Figure A1: Screen shots from “NYC 311” app

Source: NYC 311 app, image captured on February 10, 2017

Table A1: First stage estimates, impact of nighttime rainfall on homeless assistance reports

	(1)	(2)	(3)	(4)
Night rain dummy	0.0585			
	(0.0526)			
Night low rain		0.0645		
		(0.0558)		
Night medium rain		0.0926		
		(0.156)		
Night high rain		-0.131		
		(0.220)		
Night rain			-0.0292	-0.0228
			(0.0865)	(0.171)
Night rain^2				-0.00293
				(0.0449)
Daily rain	-0.323	-0.325	-0.284	-0.284
	(0.354)	(0.357)	(0.351)	(0.351)
Daily rain^2	0.221	0.216	0.190	0.190
	(0.395)	(0.397)	(0.392)	(0.392)
Max. temp	0.0142**	0.0142**	0.0135**	0.0135**
	(0.00610)	(0.00609)	(0.00597)	(0.00601)
Min. temp	-0.000312	-0.000292	0.000234	0.000216
	(0.00496)	(0.00493)	(0.00492)	(0.00493)
Rain 6am	-0.804	-0.718	-0.603	-0.607
	(0.565)	(0.565)	(0.594)	(0.603)
Log(calls)	0.484***	0.490***	0.489***	0.490***
	(0.181)	(0.183)	(0.180)	(0.181)
Observations	943	943	943	943
R^2	0.899	0.900	0.899	0.899
F-statistic	1.235	0.641	0.114	0.142

Note: The dependent variable for all specifications is the log of daily homeless assistance reports. Daily rain is total rainfall in inches between 6:00am and 6:00pm, while night rain is total rainfall in inches between 6:00pm and 6:00am the previous night. Max and min temp are the maximum and minimum temperatures for the period from 6:00am to 6:00pm. Rain 6am is the sum of rainfall between 5:51am and 6:51am. Night low rain, night medium rain, and night high rain are a series of dummy variables equal to 1 if nightime rainfall is strictly positive and less than 0.5 inches, between 0.5 inches and 1 inch, and greater than 1 inch respectively. Log(calls) is the log of daily 311 reports excluding homeless encampment and homeless assistance reports. The months December through February are excluded from all specifications. Kleibergen-Paap F-statistic is based on excluded instruments (nighttime rainfall variables). All specifications include year/month and day-of-week fixed effects. Heteroskedastic and autocorrelation consistent standard errors with a bandwidth of seven days are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A2: Second stage estimates, impact of homeless encampments on total crime, alternative instruments

	(1)	(2)	(3)	(4)
Log(encampment)	-0.0272	0.0167	0.0269	0.0227
	(0.0630)	(0.0502)	(0.0722)	(0.0881)
Daily rain	-0.0965***	-0.0841***	-0.0812***	-0.0824***
	(0.0279)	(0.0201)	(0.0231)	(0.0262)
Daily rain^2	0.00939**	0.00891**	0.00880**	0.00885**
	(0.00439)	(0.00393)	(0.00398)	(0.00408)
Max. temp	0.000690	0.000361	0.000284	0.000315
	(0.000679)	(0.000620)	(0.000724)	(0.000830)
Min. temp	0.000107	-0.000235	-0.000314	-0.000282
	(0.000730)	(0.000646)	(0.000777)	(0.000857)
Rain 6am	0.0181	0.00347	5.68e-05	0.00146
	(0.0898)	(0.0749)	(0.0720)	(0.0728)
Log(calls)	0.230***	0.205***	0.200***	0.202***
	(0.0432)	(0.0384)	(0.0495)	(0.0579)
Observations	1,810	1,810	1,810	1,810
R^2	0.715	0.735	0.732	0.733
F-statistic	6.597	3.701	15.46	22.08

Note: Dependent variable is the log of daily total crime. log(encampment) is the predicted value of the log of daily homeless encampment reports based on the specification with the same number in Table 8. Daily rain is total rain between 6:00am and 6:00pm, while max and min temp are the maximum and minimum temperatures for the period from 6:00am to 6:00pm. Rain 6am is the sum of rainfall between 5:51am and 6:51am. log(calls) is the log of daily 311 reports excluding homeless encampment and homeless assistance reports. The months December through February are excluded from all specifications. All regressions include year/month and day-of-week fixed effects. Heteroskedastic and autocorrelation consistent standard errors with a bandwidth of seven days are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1