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COVID and crime: An early empirical look ☆

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ABSTRACT

Data from 25 large U.S. cities is assembled to estimate the impact of the onset of the COVID-19 pandemic on crime. There is a widespread immediate drop in both criminal incidents and arrests most heavily pronounced among drug crimes, theft, residential burglaries, and most violent crimes. The decline appears to precede stay-at-home orders, and arrests follow a similar pattern as reports. There is no decline in homicides and shootings, and an increase in non-residential burglary and car theft in most cities, suggesting that criminal activity was displaced to locations with fewer people. Pittsburgh, New York City, San Francisco, Philadelphia, Washington DC and Chicago each saw overall crime drops of at least 35%. Evidence from police-initiated reports and geographic variation in crime change suggests that most of the observed changes are not due to changes in crime reporting.

1. Introduction

The Coronavirus pandemic that began in China in December, 2019 and in the U.S. in January, 2020 (Holshue et al., 2020) caused the biggest voluntary impact on the economy in U.S. history. It also changed the nature of policing, criminal opportunities, and criminal penalties. This paper explores some of the initial impact on criminal activity of the pandemic and responses to it. The pandemic impacted all aspects of society, a number of which have caused changes in observed levels of crime. As concern about the pandemic rose, individuals stopped their regular activities and stayed home (Fig. A1).

Police departments modified policies, including de-emphasizing particular types of crimes, like drugs (Sisak et al., 2020), and eliminating arrests for some crimes (Melamed and Newall, 2020). Jails and prisons have seen some of the most severe outbreaks and as such a number have released inmates early (Surprenant, 2020; Williams et al., 2020). Courts shut down and deferred cases (Melamed and Newall 2020) which may result in fewer prosecutions. There was also massive job loss as businesses across the country closed (Chetty et al., 2020). Together, this has resulted in a change in the opportunities for crime, probability of observation, capture, arrest, prosecution and penalty.

In addition to understanding an important impact of the pandemic, this work may be valuable to individuals and police departments as the pandemic and responses to it continue. Police departments making resource allocation decisions should pay attention to the pandemic-related changes to the quantity and distribution of crime. Individuals may also want to update their beliefs about crime in addition to other changes as they make decisions about what activities to engage in during a pandemic. But even beyond the current context, this work may help shed light on how substantial changes in mobility and other inputs impact crime.

Observed levels of crime were indeed sharply impacted, and I summarize the main results here. Crime reports and arrests fell significantly in almost every city examined in response to the onset of the pandemic. Like the economic impact, the crime impact

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preceded stay-at-home (SAH) orders, as individuals voluntarily changed their behavior in light of the disease. The cities with the greatest declines were Pittsburgh, New York City, San Francisco, Philadelphia, Washington DC, and Chicago, which each had declines of at least 35% for overall reported crime rates. There was no significant change in Cincinnati or Seattle.

Drops in drug crimes were by far the greatest, with many cities showing massive declines of over 65%. There were also substantial declines in residential burglaries, theft, and violent crime except for homicides and shootings. In general, changes in arrests tended to parallel changes in criminal incidents. There was an increase in non-residential burglary as individuals spent more time at home and other buildings were left less occupied. The results for car theft and theft from cars varied substantially by city.

1.1. Literature

The current research sits within the long and extensive empirical literature on the economics of crime, a full review of which is well beyond this paper. Recent reviews by Doleac (2020) on desistance from crime, and Chalfin and McCrary (2017) on deterrence point to some modern empirical work on these topics. Although several years older, Levitt and Miles (2004) is an excellent, broad survey of the empirical economics of crime literature to that point.

Not surprisingly, the literature on the impact of the COVID-19 pandemic on crime is limited given the brief time that has passed since the pandemic began. In a recent working paper, McDonald and Balkin (2020) simply report changes in crime rates in four major U.S. cities relative to the prior year without any standard errors. Other papers focus on specific jurisdictions, including Chicago (Campedelli et al., 2020b), Los Angeles (Campedelli et al., 2020a) Queensland, Australia (Payne and Morgan, 2020a, 2020b), and Lancashire, UK (Halford et al., 2020). These analyses report mixed results from little change in Queensland to substantial declines in crime in Los Angeles. In Halford et al. (2020) the authors focus on a single jurisdiction in the UK and find substantial declines across many different crime types. They compute a "mobility elasticity of crime" which covers a substantial range of values. All of their findings are directionally the same as those in this paper, except for non-residential burglary. Halford et al find a decrease in this crime during pandemic onset, while this paper finds a substantial increase as individuals spend more time at home. This difference may due to variation in SAH restrictions or to greater intermixing of residential and non-residential buildings in the UK jurisdiction under study.

The closest research to the current paper (Ashby 2020) investigates time series variation in crime in 16 US cities in the first two months of the pandemic relative to forecasts based on historical data. Ashby finds a handful of city-weeks that diverge significantly from historical averages but that most crime changes were not statistically significant. The current paper finds substantially more significant deviation in crime rates, and the difference appears to be at least partly attributable to greater data availability and a different level of aggregation.

Two recent papers focus specifically on the impact of the pandemic onset on domestic violence. Examining calls for service from 14 municipalities, Leslie and Wilson (2020) find an increase of 7.5% in the first several weeks after the onset of the pandemic, although they are not able to distinguish real crime changes from changes in reporting patterns. Another paper focuses just on domestic violence incident reports in Dallas and finds weak evidence for an increase (Piquero et al., 2020).

There is little evidence on the impact of prior pandemics on crime. But a report by the Chicago Department of Health (Robertson, 1919) indicates a 38% decline in crime rates in Chicago from 1917 to the same 3 week period in 1918 during which there was a lockdown. This is astonishingly similar to the 35% overall decline in crime in Chicago in the first 4 weeks of the 2020 pandemic, relative to the same period for the prior 5 years. The author of the 1919 report concludes that, "so far as vicious conduct and immorality are concerned it would seem that 'to keep the home fires burning' and to stay off the streets late at night lessen the number of misdemeanors and misconduct of every kind." I might state things a bit differently, but the same link between presence on the street and crime appears to hold today.

The rest of the paper proceeds as follows. Section 2 contains additional background on crime and the pandemic, and Section 3 introduces the data. The main analysis is presented in Section 4, followed by a discussion in Section 5. Section 6 concludes.

2. Background

While a formal model is beyond the scope of this paper, here I briefly present some heuristic predictions for how reported crime may be impacted by the pandemic onset, as well as additional background.[1] The crime rate is a function of available opportunities and expected penalties. Reported crime incidents depend not just on the true crime rate, but also the reporting rate (the share of crimes reported). For now I assume no change in the reporting rate, but return to this crucial assumption in Section V below.

Crimes vary in their primary source of detection – police, public, or victim. For most crime types other than drugs, public or victim reports are the main source of detection. Fig. A1 provides evidence of a substantial and widespread decline in mobility at the onset of the pandemic. Thus when mobility in a location declines, the probability of observation by the public drops, which reduces the expected penalty. However, the lack of street traffic could also increase likelihood of detection by police, as individuals on the street are more noticeable.

There is evidence that police presence and enforcement of certain laws decreased during the pandemic onset (Elinson and Chapman, 2020; Melamed and Newall, 2020), which would tend to increase crime rates due to the decreased expected penalty. Prosecutors and courts have been impacted by the pandemic; it is too early for substantial data to be available, but the most reasonable expectation is that the probability of prosecution and sentence conditional on conviction will fall. Jails and prisons have seen some of the highest infection rates of SARS-CoV-2. The likelihood of illness if imprisoned and therefore the cost of imprisonment has increased after the pandemic. But since many offenses result in no incarceration and all of the other components of the expected cost of prison likely decline, I assume that the change in the expected penalty is negative or zero.

Thus, one should expect to see an increase in property crimes like car theft, theft from vehicles, and non-residential burglaries because of the decline in expected penalty due to a drop in mobility. The one location where individuals were spending more time is at home, so one should expect a decrease in residential burglaries.[2]

In addition to changes in individual behavior and criminal justice routines, unemployment jumped dramatically (Fig. A1) during the pandemic onset (Chetty et al., 2020). The modern economic theory of crime, beginning with Becker (Becker, 1968) posits that crime rates will be a function of expected gains and penalties from crime, which will naturally be affected by the outside option for income. For many individuals, this is regular employment and thus crime rates should be impacted by the unemployment rate. While

[1] In the spirit of (Becker 1968), (Ehrlich 1981), (Balkin and McDonald 1981) and (Fu and Wolpin 2018).

[2] I assume that the cost of violating stay-at-home orders is small for most crime types relative to other considerations.

there are some good empirical studies (Raphael and Winter-Ebmer, 2001; Lin, 2008) that support this theory, there are not as many as one might hope. Theory predicts the crime-employment link to be strongest for property crimes, and this is what the empirical studies show, with no real evidence of a link between violent crime and unemployment rates.

Absent intervention, one might expect the change in economic climate to lead to an increase in crime, at least for property crime. But in fact there was tremendous governmental response, first in the form of the Families First Coronavirus Response Act (Match 18. 2020) and then the Coronavirus Aid, Relief, and Economic Security Act (March 27, 2020), the latter of which cost over $2 trillion. The act included hundreds of billions of dollars for unemployment insurance, direct payments to individuals, as well as aid to businesses (WSJ, 2020). So while the economic impact of the pandemic onset was enormous, the massive government response suggest that this is unlikely to have had a substantial impact on crime. Nevertheless, the unemployment rate is included in some specifications (Section 5).

There is considerable variation in the nature of violent crimes, from those with greater financial incentive, like robbery, to those that are often associated with alcohol or drug use, like assault. Unlike property crime, there is always a victim of violent crime who may report it (other than homicide) although reporting rates may still vary substantially. For assault, rape, and robbery, the lower level of interaction with individuals outside the home should decrease opportunities and thus decrease the crime rate. The change in violent crimes will naturally be strongest in areas with the largest change in potential victims and perpetrators, as around now-closed bars (Section 5).

A different pattern should be expected for homicide due to the fact that in large cities an appreciable share is related to drugs or gangs (National Gang Center, 2020) and unlikely to be impacted by SAH orders and overall mobility. Individuals likely to be involved in a homicide may not be deterred by SAH orders or the prospect of disease. The same pattern will likely hold for shootings – that is, no substantial impact of the pandemic. Domestic violence is in this context the opposite of assault. With much more time spent at home, one might expect a large increase in domestic violence. However, domestic violence has a notoriously low reporting rate, and one that is likely to be strongly impacted by SAH orders. These reports are probably among the least reliable of the data collected.

Drug crimes differ from property and violent crimes in that police are the main source of drug crime detection as opposed to victims or the public. Given this fact, policing priorities should have the dominant impact on reported drug crime rates and the true rate may diverge most from the reported rate.

3. Data

The goal of this paper is to rapidly collect current data from large cities to estimate the impact of the pandemic onset on crime. This would not have been possible a decade ago, prior to the widespread success of the Open Data movement, which has made up-to-date crime statistics more available than ever before (US Gov, 2014). As such not only is it possible to rapidly obtain data to inform this study of a very recent phenomenon, but I have also aggregated it on a new website that is available to researchers and the public: https://citycrimestats.com. Table A1 summarizes the data types obtained by city, which includes 25 of the largest cities in the U.S. Cities differ in the data they make available and therefore the set of cities analyzed in each figure or regression varies. Data definitions, sources and cleaning procedure are reported in Appendix B.

Table A2 presents summary statistics of crime incident rates per 100,000 residents before and after SAH orders in a city. Almost all crime categories experience substantial crime drops. A few crimes, like homicide and aggravated assault, show an increase after SAH orders but as will be seen in the next section, these mostly reflect seasonal trends. Some cities make information on shootings available; almost all of this data was obtained in response to email inquiries and calls.

Data on COVID-19 prevalence is now pervasive. The source used in this paper is The New York Times. Data on individual movement (mobility data) comes from Google COVID-19 Community Mobility Reports. This data is generated from individuals who have turned on Location History for their Google Account and use a mobile device. It is available at the county level for 6 types of location: Grocery and pharmacy, parks, transit stations, retail and recreation, residential and workplaces. Several measures of the timing of responses to the pandemic are obtained from government websites or local newspapers. These include orders of: social distancing, closure of non-essential services, stay-at-home, and end of stay-at-home. In most cities social distancing orders were issued several days before SAH orders.

4. Main results

4.1. Magnitude of crime drop

The change in crime from the pandemic onset was large and sharp enough that the impact is clear from the time series. Fig. 1 shows crime rates for all cities by broad crime category from 7 weeks prior to 7 weeks after SAH orders, for 2020 (black) and 2015–2019 (grey). The vertical line indicates when the SAH order went into place. The crime drop actually begins 10–14 days prior to the SAH orders, and is almost coincident with mobility drop. This is likely due to the fact that there was considerable media coverage prior to SAH orders as well as some other less severe orders.[3] It is clear from this figure that declines in all types of crime were substantial.

To quantify the magnitude of the crime decline and understand how it varies by crime type and location, I run a series of difference-in-difference regressions where the comparison group for each city is itself in years prior to 2020:

$$\ln(crime_{it}) = \alpha + \beta_1 Treat * After_{it} + \beta_2 After_{it} + \sum_{k=2015}^{2020} (\gamma_k Year_k) + \theta_t + \mu_i + \varepsilon_{it} \quad (1)$$

where $crime_{it}$ is the number of crime incidents in city i at week t, $Treat$ is 1 if the year is 2020 and 0 otherwise, $After_{it}$ is 1 beginning on the calendar week when the stay-at-home order went into effect in city i regardless of year, $Year_k$ is a year dummy, θ_t is a vector of dummies to control for week-of-year fixed effects. City fixed effects are included as μ_i and standard errors are computed using wild bootstrap. Data is aggregated to the week level, since in some cities more severe crimes are zero on many days.[4] The data is a balanced panel, and for each city uses the same weeks of the year, beginning 7 weeks before the SAH order and ending 4 weeks after, for the years 2015–2020.

Table 1 reports coefficients from the difference-in-difference specification in Equation (1) by crime category. Standard errors computed by wild bootstrap are in parentheses. Reported crime

[3] The SAH order date is used for clarity and consistency across cities; the results do not change substantially when using measures of mobility decline to define After.

[4] The use of daily data for more frequent crimes yields similar results without much improved standard errors, likely due to substantial noise in precise timing of reporting. For consistency, the weekly analysis is used throughout.

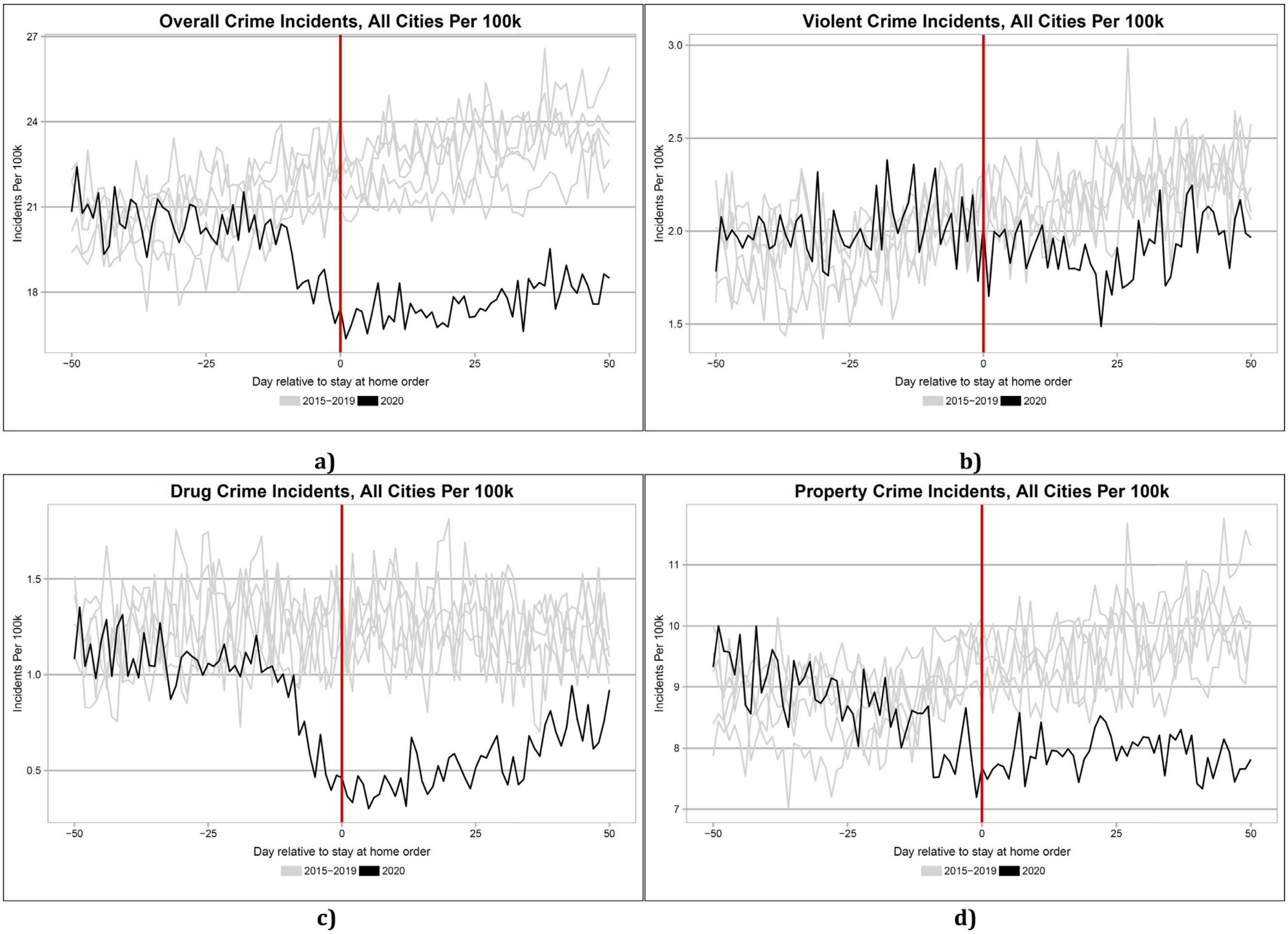

Fig. 1. Crime Rate Change around Pandemic Onset. *Note:* Each panel combines data from 23 cities (16 for drugs) to show a time series of crime incidents per 100,000 residents from 7 weeks before to 7 weeks after the stay-at-home order issued in a city. In order to account for varied timing in pandemic onset, each line combines the data such that time zero (red vertical line) is when the stay-at-home order was issued in a city. The black line is 2020 data; the grey lines show the same time period but for years 2015–2019. Panel a reports overall crime rate, panel b reports violent crime, panel c reports drug crime, and panel d reports property crime. Data source: city police departments. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

fell rapidly, broadly and substantially, 23.3% overall. Since total crime can hide substantial variation it is instructive to consider the changes in crime, first in broad categories and then more narrowly. Property crime dropped 19.3% relative to the same period over the past 5 years, as did violent crime. Drug crimes saw the biggest decline, with a drop of 65% - this regression includes only the 12 cities that provided drug data. In each broad category except drugs, the coefficient on *After* is positive, statistically significant and of large magnitude. This reflects the substantial seasonality to crime. The number of cities varies somewhat by crime category due to differences in reporting, and is reported in Table 1. In Section 6 discuss a number of robustness checks, including modifying the weeks included, changing the comparison years, and including additional controls – none change the results in a meaningful way.

In order to gain greater insight into the timing of the crime change, I estimate an event study specification where as before time zero is when SAH are enacted in each city:

$$\ln(crime_{it}) = \alpha + \sum_{j=-8}^{7}(\beta_{1j}Treat * Week_j) + \beta_2 After_{it} + \sum_{k=2015}^{2020}(\gamma_k Year_k) + \theta_t + \mu_i + \varepsilon_{it} \quad (2)$$

where $crime_{it}$ is the number of crime incidents in city i at week t, *Treat* is 1 if the year is 2020 and 0 otherwise, $Week_j$ is a dummy for the number of weeks which time t is after the calendar date when the stay-at-home order went into effect in city i regardless of year, $Year_k$ is a year dummy, θ_t is a vector of dummies to control for week-of-year fixed effects. City fixed effects are included as μ_i and standard errors are computed using wild bootstrap.

The results are presented visually in Fig. 2 for overall crime rate and in Fig. A2 by specific crime type. The event study results are consistent with the time series data in Fig. 1 and the difference-in-difference results in Table 1. A small crime decline begins two weeks prior to the SAH orders and in earnest (almost 20% below baseline) one week before SAH orders. Crime rates then remain low for the 7 weeks following the SAH orders that is the extent of the timespan studied[5]. Fig. A2 reveals a similar pattern for specific crime categories. In all cases, the timing of any changes is similar to that for overall crime, and the direction and magnitude consistent with that estimated in the difference-in-difference specification.

4.2. Property crime

Most property crimes fell substantially with the onset of the pandemic, with two major exceptions, non-residential burglary and car theft (Table 1). The drop in residential burglary by 23.5%

[5] This time period was chosen in order to restrict focus to the pandemic onset and responses to it as cities began lifting SAH orders after this period.

Table 1
Impact of Pandemic Onset on Crime.

Panel A (Property and Drug Crime)

	Dependent variable: Log of Crime Incidents							
	Overall (1)	Property (2)	Drug (3)	Burglary (Residential) (4)	Burglary (Non-Residential) (5)	Theft (6)	Theft from Car (7)	Car Theft (8)
After*Treat	−0.265***	−0.214***	−1.047***	−0.268***	0.321***	−0.331***	−0.227***	0.02
	(0.024)	(0.029)	(0.112)	(0.052)	(0.077)	(0.033)	(0.052)	(0.055)
After	0.108***	0.107***	0.083	0.02	0.120*	0.093***	0.186***	0.059
	(0.024)	(0.032)	(0.063)	(0.038)	(0.068)	(0.032)	(0.045)	(0.036)
Treat	−0.051	−0.015	−0.318***	−0.473***	0.218***	−0.052**	0.148***	0.170***
	(0.015)	(0.019)	(0.055)	(0.032)	(0.057)	(0.023)	(0.035)	(0.033)
Observations	1221	1221	759	890	887	1221	1034	1220
# of Cities	19	19	12	14	14	19	16	19
Adjusted R^2	0.976	0.952	0.871	0.882	0.81	0.959	0.897	0.893

Panel B (Violent Crime)

	Dependent variable: Log of Crime Incidents							
	Violent (1)	Robbery (2)	Aggravated Assault (3)	Simple Assault (4)	Homicide (5)	Rape (6)	Domestic Violence (7)	Shooting (8)
After*Treat	−0.215***	−0.226***	−0.173***	−0.406***	0.056	−0.487***	−0.190***	0.01
	(0.031)	(0.042)	(0.047)	(0.035)	(0.095)	(0.084)	(0.05)	(0.085)
After	0.135***	0.085**	0.179***	0.129***	0.206***	0.177**	0.052	0.095
	(0.011)	(0.017)	(0.018)	(0.013)	(0.037)	(0.032)	(0.017)	(0.087)
Treat	0.074***	−0.084***	0.149***	0.039	0.132*	−0.112*	−0.095***	0.297***
	(0.022)	(0.03)	(0.031)	(0.024)	(0.066)	(0.066)	(0.028)	(0.083)
Observations	1221	1220	1155	836	901	1083	264	844
# of Cities	19	19	18	13	19	18	4	14
Adjusted R^2	0.961	0.921	0.936	0.969	0.608	0.714	0.976	0.824

Note: *p<0.1 **p<0.05 ***p < 0.01
This table reports the change in crime incidents from the pandemic onset using the difference-in-difference specification in equation (1). Each column reports a separate regression, with overall crime, all property crimes, drug crime, and specific property crimes reported in Panel A using weekly crime data from 19 large U.S. cities for 2015 – 2020. Panel B reports the results for overall violent crime as well as by specific violent crime category. Observations range from 7 weeks before stay-at-home order to 4 weeks

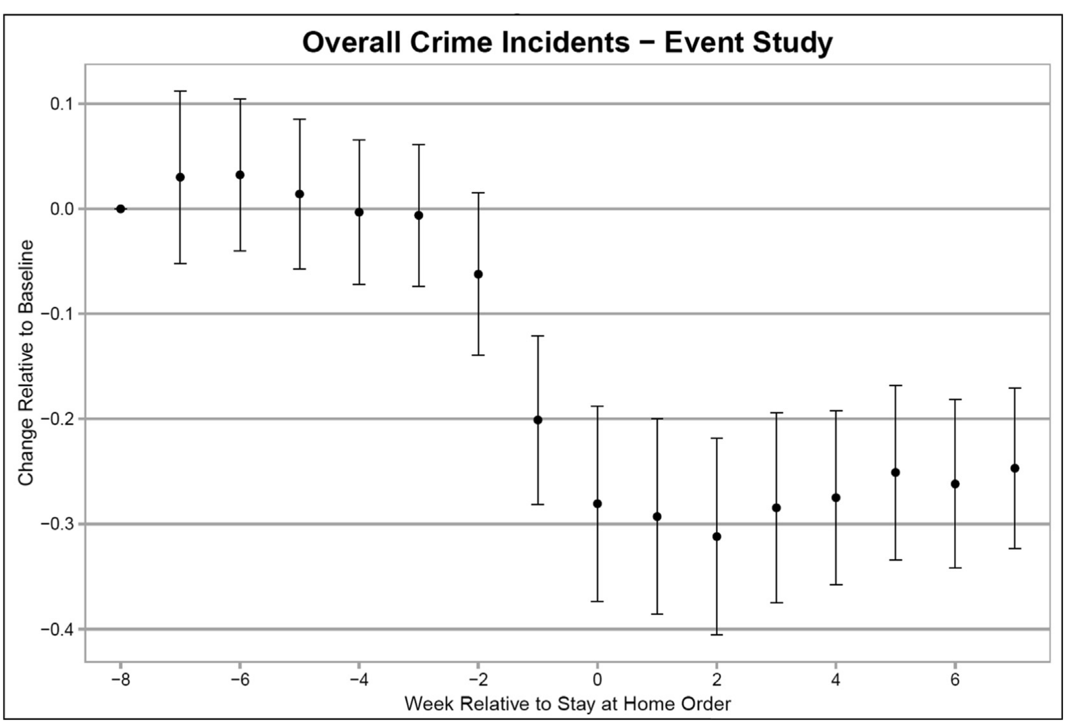

Fig. 2. Overall Crime Incidents – Event Study. *Note:* Coefficients from event study specification (equation (2)) are reported for the period from 8 weeks before to 7 weeks after stay-at-home order in a city. Baseline is 8 weeks prior to stay-at-home order. Data from 19 cities, obtained directly from police departments.

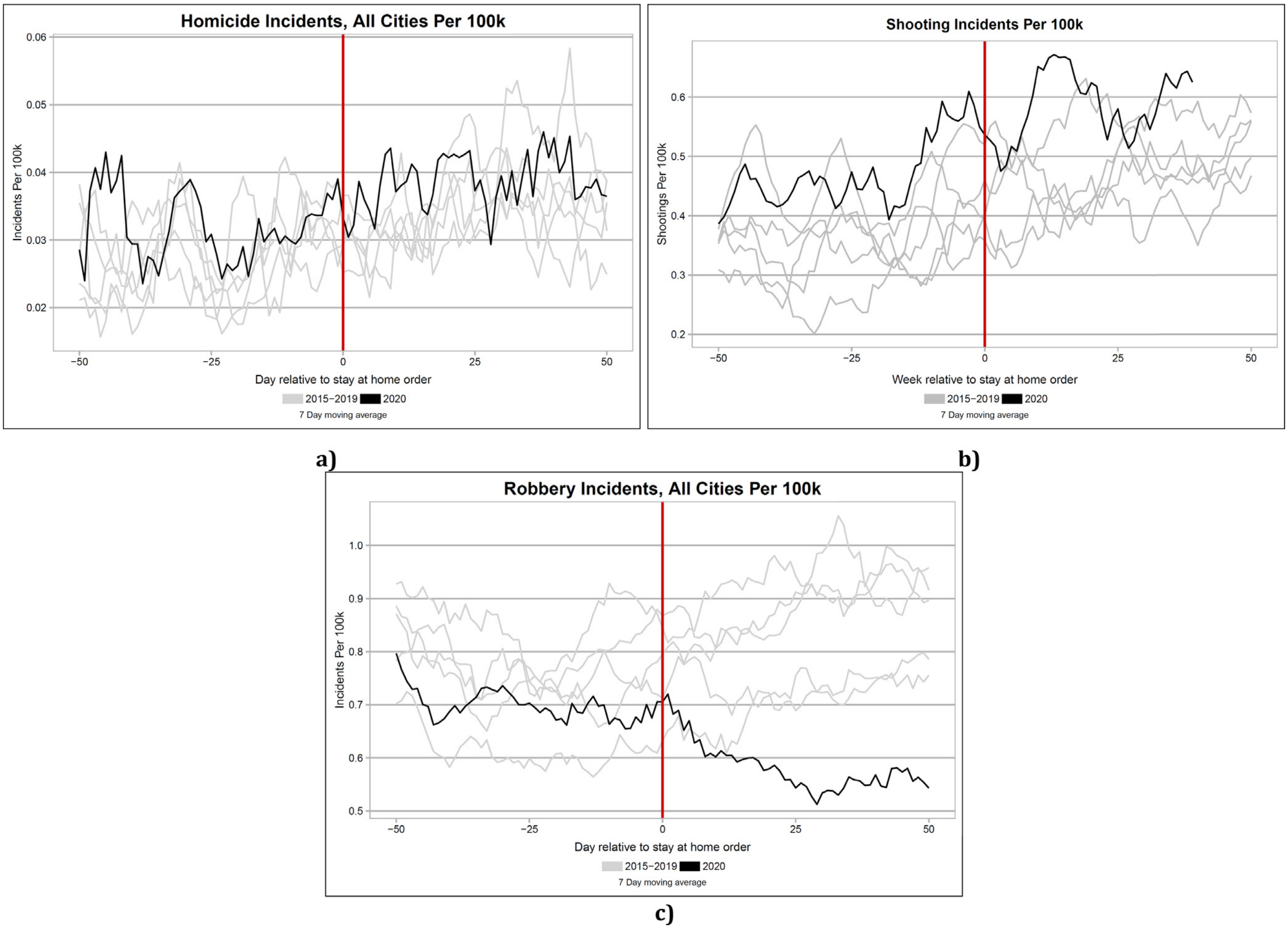

Fig. 3. Violent Crime around the Pandemic Onset. *Note:* Each panel combines data from 23 cities (15 for shootings) to show a time series of violent crime incidents per 100,000 residents from 7 weeks before to 7 weeks after the stay-at-home order issued in a city. In order to account for varied timing in pandemic onset, each line combines the data such that time zero (red vertical line) is when the stay-at-home order was issued in a city. The black line is 2020 data; the grey lines show the same time period but for years 2015 – 2019. Panel a reports homicide rate, panel b reports shooting incidents, and panel c reports robbery. Data source: city police departments. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

is more than offset by the 37.8% rise in non-residential burglary. These results are well explained by the fact that individuals were largely confined to their homes, providing a deterrent to home burglaries, but resulting in a much lower level of detection for non-residential buildings. Theft also dropped substantially by 28.2%, while theft from vehicles declined by 20.3%.

Results for car theft are mixed, with some cities showing an enormous increase, like Philadelphia. Other cities had little change, like Cincinnati and some even saw a decline, like Baltimore. The net effect in the cities examined is a positive point estimate but not statistically significant at conventional thresholds.

4.3. Violent crime

Every violent crime category saw a decline in reported crime except for homicide, as well as shootings.[6] Robbery and aggravated assault both had substantial levels of decline, at 20.2% and 15.9%. But the drop for simple assault was far greater, at 33.3%. The disparity between simple and aggravated assault is likely due to a combination of changes in reporting and availability of victims. There were almost certainly fewer low-level public altercations, especially associated with alcohol, as many public establishments were closed. This is explored further in Section 5.

The difference between other categories of violent crime and homicides and shootings (Fig. 3) is striking. A few observations are worth noting here. Shootings in 2020 were already elevated relative to prior years, even before the pandemic onset. A large share of murders (National Gang Center, 2020) are associated with gangs, which may be less likely to be deterred by SAH orders during a pandemic. Even homicides and shootings not associated with gangs are likely committed by individuals with outlier risk preferences, which makes them less responsive to the pandemic. While the same may be true of perpetrators of other violent crimes, victims of those crimes may be more responsive to the pandemic and thus less available to be victimized.

The decline in domestic violence reports (17.3%) and the substantial drop (38.6%) in reported rapes likely overstate true changes in crime. For domestic violence (DV), the data is limited to four cities (Austin, Chicago, Nashville, and San Francisco) and is almost certainly a lower bound on the true level.[7] Some victims

[6] Shootings are not a separate crime in the Uniform Crime Reports, but are reported by many cities.

[7] Some share of domestic violence is reported as simple or aggravated assault, although familial violence likely accounts for roughly 15% of assaults (Durose and Harlow, 2005). More detailed data that is not yet available could allow for a better understanding of whether the familial violence share of assaults increased as people spent more time at home.

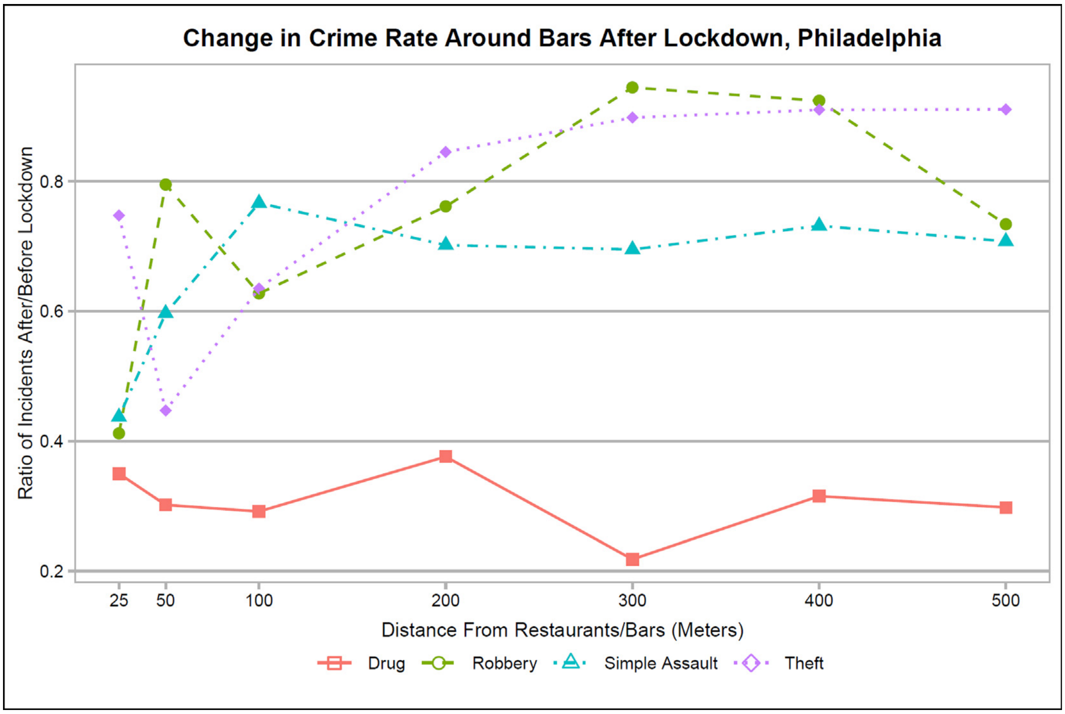

Fig. 4. Change in Crime Rate Around Bars After Lockdown, Philadelphia. *Note:* The distance to the nearest bar or restaurant is computed for each crime incident. Incidents are summed by time period and distance ranges. For each distance range indicated on the x-axis, the crime change is simply the ratio of crime incidents in that area 7 weeks after the stay-at-home order to 4 weeks before the order. The distance range is from the next largest increment to the one plotted (e.g. 200 includes incidents from 100 to 200 meters from the nearest establishment). The type of crime is indicated by the line style, color and market type. Data source: City of Philadelphia.

were staying in the same building with abusers and likely were unable to report. As schools closed, this important source of reports also disappeared. See Leslie and Wilson (2020) for evidence that DV likely increased. For rape there may be an undercount for similar reasons, but possibly not to the same extent.

5. Discussion

5.1. Crime drop vs observed crime drop

A crucial question regarding the interpretation of the findings in this paper and any using crime incident data is: to what extent do changes in observed crime reflect changes in the real level of crime, rather than changes in reporting of crime? In this section I attempt to shed some light on the question and present suggestive evidence that much of the crime change is not simply a reporting artifact.

Crimes may be reported by police or any other individual. One potential indicator of reporting rate changes is changes in the share of reports by the police. The source of crime incident reports is only available from two cities in the data set, Dallas and Nashville and reported in Table A3. The vast majority of crimes in both cities are reported by individuals other than police, with one main exception, drug crimes. For drug crimes about 2/3 of reports are due to police. In Nashville, all crime types saw a modest increase in the share reported by police. This suggests that the decline in overall crime reports was likely not predominately due to reporting changes, or that reporting dropped in very similar proportions by police and individuals. This seems unlikely given the overall mobility decline of about 50% but little evidence that policing dropped by anything near that amount. It is possible to bound the contribution of reporting changes to observed crime rate change under the assumption that all of the increase in police share of reports is due to missing reports from non-police. In Nashville those bounds are 10.1 percentage points for violent crimes, 6.3 pp for property and 7.0 for drugs. This would mean the estimated decline in crime is substantially overstated, but nevertheless still large.

The evidence from Dallas is similar for drug crimes, with a modest increase in the share due to police. However, violent crime share due to police increased substantially, while the opposite was true for property crime. One way to interpret the overall changes in Dallas is that police accounted for a greater share of reports overall as the pandemic reduced the number of other observers. The differences between violent and property crime relative shares likely point to an emphasis on violent crimes and de-emphasis on property crimes as the pandemic began.

Another approach to examining the impact of reporting changes is based on geographic variation. As lockdown orders were put into place across the country, bars and restaurants were some of the establishments forced to close or to provide takeout service only. Prior research shows that crime is localized around bars and also impacted by the closure of bars or similar establishments at the very local level (Klick and MacDonald, 2020; Chang and Jacobson, 2017). Thus it is natural to examine changes in crime around bars when SAH go into effect to gain insight into reporting rates.

If changes in crime reports reflect an overall reporting decline, then there should be a consistent drop in crime that is independent of distance from bars. If declines reflect a real change, one should expect a sharp decline in crimes most associated with large gatherings of individuals around bars, often at night and inebriated: simple assaults, theft and robbery. This decline should drop off fairly rapidly with distance from bars. Other crimes should see a negligible impact of bar closures and shouldn't exhibit a distance dependence.

To assess this possibility, I analyze data from Philadelphia that includes crime location along with that of bars and restaurants. I compute the distance to the nearest bar or restaurant for each crime incident and then count the number of incidents by time period and distance ranges (Fig. 4). For each distance range indicated on the x-axis, the crime change is simply the ratio of crime incidents in that area 7 weeks after the stay-at-home order to 4 weeks before the order. The distance range is from the next largest increment to the one plotted (e.g. 200 includes incidents from 100 to 200 meters from the nearest establishment).

Simple assaults, drug crimes and robbery all dropped to between 30 and 40 percent of pre-pandemic levels within 25 meters of the establishments. But the relative patterns in crime changes diverge when moving away from the establishment. The drop in simple assault, theft and robbery rates decreases with distance from bars and restaurants. This is to be expected – when these locations close, there are fewer people to get into fights or to be robbed nearby, but this falls off rapidly with distance from the establishment.

Drug crimes exhibit a completely different pattern, independent of distance from the bar or restaurant. If the decline with distance was simply due to changes in reporting due to decline in people, one would expect to see a similar drop off with distance for drug crimes. But in fact the pattern is exactly what would be expected with reporting rates that don't change substantially, since drug crimes aren't strongly associated with bars or restaurants.

The prior literature (Chang and Jacobson, 2017) and Fig. 4 suggest that crime varies in a nonlinear way with distance from similar establishments. Thus in order to quantify the variation in crime drop-off with distance from establishments, I run non-parametric regressions of the crime change around SAH on distance (Table A4) using the following specification:

$$\begin{aligned}\ln(crime_{it}) = \alpha + \beta_1 Treat * After_t * Region_i + \beta_2 Treat * After_t \\ + \beta_3 Treat * Region_i + \beta_4 After_i * Region_i \\ + \beta_5 Region_i + \sum_{k=2015}^{2020} (\gamma_k Year_k) + \theta_t + \varepsilon_{it} \qquad (3)\end{aligned}$$

where variables are defined as in equations (1) and (2) and $Region_i$ is a set of regions 0–100 m, 100–200 m, 200–300 m and >300 m from the nearest bar/restaurant. The regression results show a statistically significant increase in relative crime rates with distance for theft and robbery, but nothing significant for simple assault or drugs. Based on Fig. 4, it is likely that the result for simple assault is because most of the variation is within a small distance from the establishments, that was not picked up in the specification. While none of what is presented here is conclusive, along with the evidence above this suggests that much of the crime change at the pandemic onset is not due to reporting changes.

5.2. Robustness checks and heterogeneity

Criminal incidents have been used as the measure of crime so far in this paper. While the above discussion provides evidence that these are a true reflection of crime changes, it is also useful to use an alternate measure when possible. For a subset of 9 cities, an additional measure is available – arrests. Results from regressions using arrests (Table A5) are broadly similar but show an even greater decline than incidents. Arrests in these cities fell by 45.6% overall, substantially greater than incidents. Drug arrests fell a massive 77% and property arrests by 34.8%. Violent crime arrests had the smallest decline, at 16.8%, slightly lower than the drop in incidents. Together this evidence suggests that police resources were focused less on arrests for most crime types, except for violent. This is consistent with stated temporary revisions to police department policies as the pandemic began (Mallin and Barr, 2020).

The results presented in the paper are robust to several other alternative specifications presented in Table A6. The main analysis is performed using a period of 7 weeks before SAH orders and 4 weeks after. The before period was chosen to establish a baseline that balances more observations without getting far before the pandemic. The after period was chosen in order to cleanly estimate the full impact of SAH as some cities began opening up after 5 weeks. But the results are quite robust to changing both the before and after window, as shown in columns 1–7 of Table A6. In all cases the estimated crime decline is within a few percentage points of the base specification (columns 1 and 5).

In order to establish a less noisy overall crime rate, the main specifications use 5 years of prior data (2015–2019) to compare to 2020. Some readers may be interested in a single year comparison to 2019 as better capturing current crime rates – this is reported in Column 8 of Table A6. Changing the comparison time to 2019 reduces the magnitude of the estimated pandemic impact on crime by less than 1 percentage point. In addition to the direct impact on the criminal justice system, the pandemic had a massive effect on the economy, with ballooning unemployment rates (Fig. A1). Column 9 of Table A6 presents the results of the main specification with unemployment added as a control variable. The estimated impact of the pandemic is under 4 percentage points lower when including this control, no statistically significant difference from the main specification.

Another concern may be that extreme outlier cities could drive some of the results. This is not very likely given the inclusion of city fixed effects, but for additional confidence all of the main regressions were run leaving out one city at a time. None of these estimates are statistically significantly different from the main specification. Finally, a placebo test is run, where the day of SAH order is randomly chosen from a 6-month window beginning on February 19, 2019 – roughly a year before the first cases began making news in the U.S. In these simulations, the coefficients are all of magnitude several times smaller than the magnitude estimated in the real regressions, and with no statistically significant difference from zero.

One noticeable characteristic about the impact of the pandemic onset on crime is just how widespread it was. Almost all cities examined had a crime rate drop of at least 15% in at least one broad category (violent, property or drugs) (Table A7). Six cities – Pittsburgh, New York City, San Francisco, Philadelphia, Washington DC and Chicago – saw overall drops in crime of over 35%. Observable characteristics examined do not explain cross-city variation in crime drop. None of the following were statistically significant explanatory variables in regressions with city crime drop as the outcome: median household income, police officers as a proportion of the population, proportion of African Americans, location within the US, share Republican, and various measures of crime.

6. Conclusion

The onset of the global pandemic in the U.S. in the Spring of 2020 had a massive impact on almost all types of crime. It led to a decline in both violent and property crime by 19% overall. The effect on drug crimes was substantially larger – about 65% on average in the cities examined. The decline in crime began prior to SAH orders and coincided closely in time to the substantial drop in mobility.

Some of the specific categories with the largest declines were theft (28%), simple assault (33%), and rape (39%). Not all crime rates fell – in particular as people spent more time at home, commercial burglaries rose by 38% and car thefts in some cities rose dramatically. Some types of serious violent crime seemed unaffected by the pandemic onset, notably homicide and shootings.

Arrests followed similar, although even more pronounced patterns as crime reports.

While most cities experienced a significant drop in crime, there was substantial variation across cities. The following cities saw overall crime rates drop by at least 35%: Pittsburgh, New York City, San Francisco, Philadelphia, Washington DC and Chicago.

Given the magnitude, breadth and rapidity of these changes in crime, it is important to attempt to understand whether much of it is due to substantial changes in reporting rates. Two separate strands of evidence suggest that much of the crime change is real. First, there is not a large change in the share of crime reported by police versus the public, in the two cities that report this data. Second, in Philadelphia there was evidence of a drop in crime that varied as a function of distance from closed bars for simple assaults, robberies, and thefts, but not for drug crimes that were unlikely to be impacted. This contrast suggests that a large portion of the change in reports reflects a real change in crime.

At this writing, the pandemic is still raging in the US and likely to continue for months if not years. As such, political leaders, law enforcement, as well as individuals will need to account for the changed circumstances as they make decisions for some time to come. The hope is that these initial findings about the pandemic impact on crime will help inform those decisions. In addition, these unique events may help us better understand the factors that impact crime in normal times as well.

Appendix A. Additional figures and tables

See Figs. A1 and A2.
See Tables A1–A7

Appendix B. Data construction

B.1. Incidents

The focus of this paper is on crime incidents – these are crimes reported to the police by individuals or observed by the police. Most data comes from Open Data web portals for each city with a preference for using APIs where available. The data sets for Columbus and Fort Worth were provided directly in response to inquiries.

Crime categories included in the original data are used when available. Not all cities report all categories of crime. For example, 6 cities do not report drug crimes in these data sets and 3 do not report rapes. Some subcategories (e.g. non-residential burglary) cannot be assigned for all cities when the data does not allow for this level of granularity. Standardized UCR or NIBRS offense categories are frequently missing, which limits the utility of direct cross-city comparison. Where crime categories are not included, a keyword-based classification algorithm is employed to assign categories. The emphasis is on ensuring that within-city categorization is consistent across time. This occasionally requires categories to be dropped if categories are not reported for all years. Since all of the analysis contains city or city*crime fixed effects, differences in crime classification should not impact the results.

Crime category definitions:

- Violent – Includes homicide, rape (not statutory), robbery and aggravated assault
- Property – Includes arson, theft, burglary and motor vehicle theft

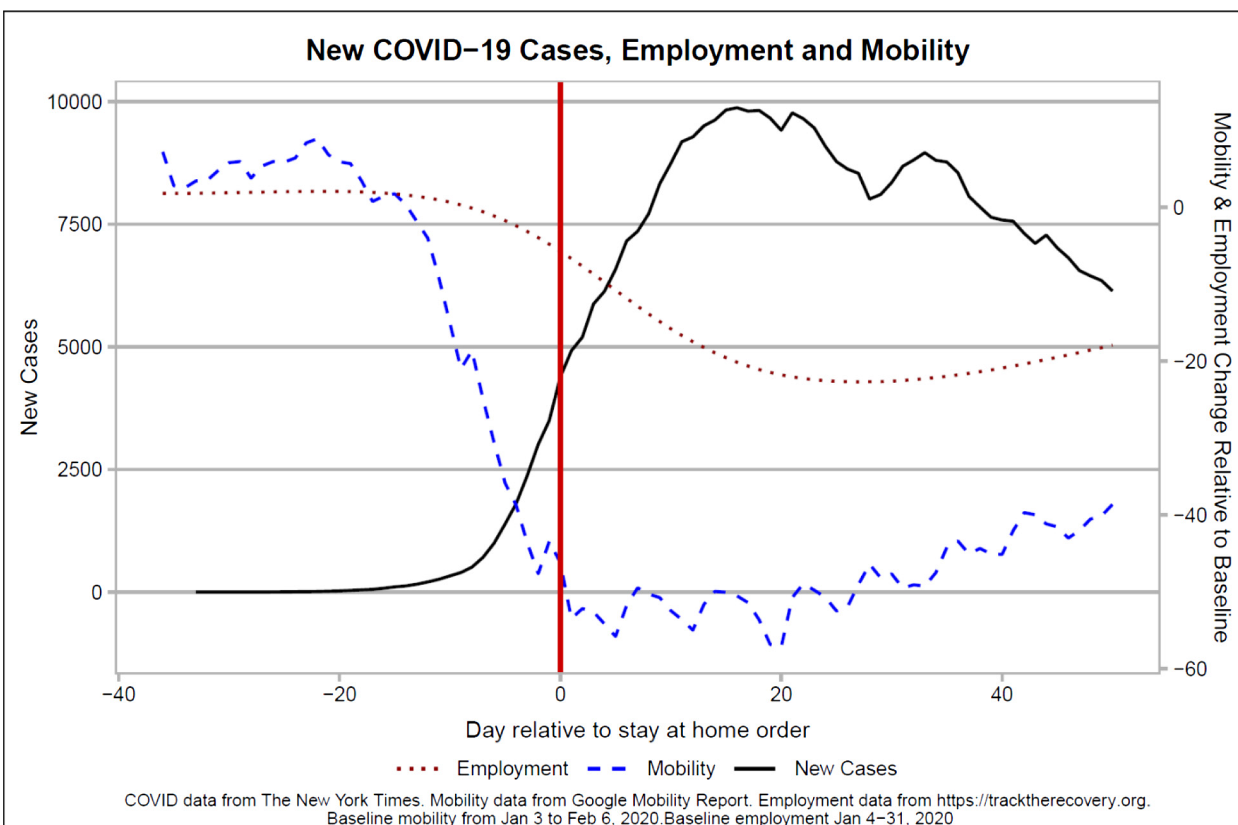

Fig. A1. New COVID-19 Cases, Employment and Mobility. Note: Three time series are reported relative to the day the stay-at-home order is issued, for 25 cities. The left axis indicates the scale for new COVID-19 diagnoses, shown in the black solid line. The blue, dashed line indicates change in mobility relative to baseline, established Jan 3 – Feb 6, 2020 (right axis). The mobility measure is an average of these Google Mobility categories: Retail/Recreation, Transit, Workplace and Residential. Change in employment relative to baseline (Jan 4–31, 2020) uses the right axis scale and reported in the red dotted line. Data sources: New York Times (COVID-19 diagnoses), Google Mobility Report (mobility), Track the Recovery (employment). (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

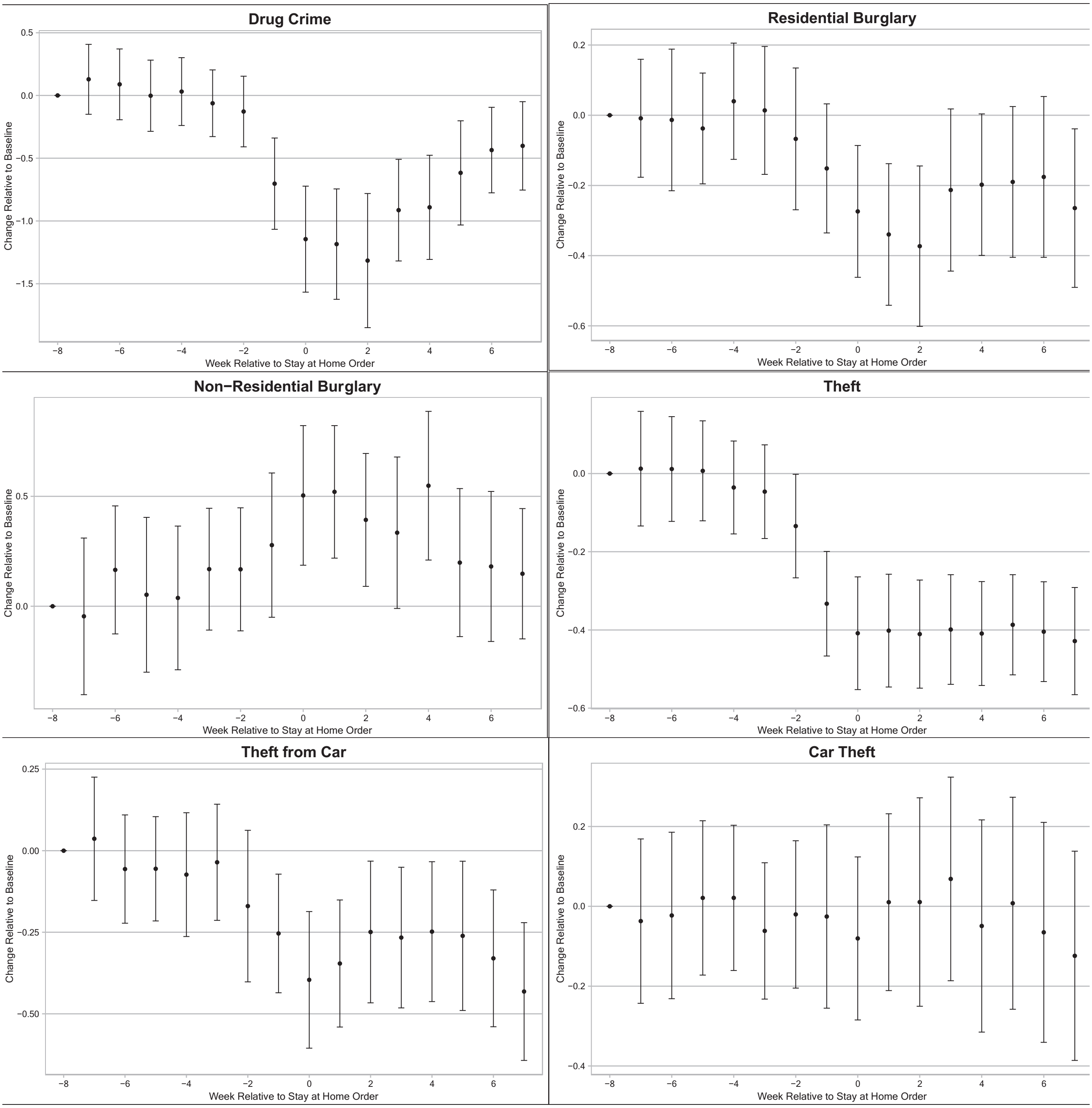

Fig. A2. Event Study by Crime Type. Note: Coefficients from event study specification (equation (2)) for crime type indicated are reported for the period from 8 weeks before to 7 weeks after stay-at-home order in a city. Baseline is 8 weeks prior to stay-at-home order. Data from 19 cities, obtained directly from police departments.

- Drug – Includes possession and distribution of illegal drugs
- Aggravated assault – An unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily harm.
- Burglary – All burglaries regardless of premises
- Burglary (Residential)- Burglaries of a residence itself, does not include burglary of a car in a driveway or of a hotel
- Burglary (Non-Residential) – Burglary of commercial premises
- Domestic violence – Non-property crimes of a domestic nature either against a partner, child or dependent adult. This includes but is not limited to assault, stalking, violation of protection orders and child abuse.
- Homicide – Murder and manslaughter not including vehicular manslaughter
- Motor vehicle theft – Theft of a vehicle includes cars, buses, trucks and motorbikes

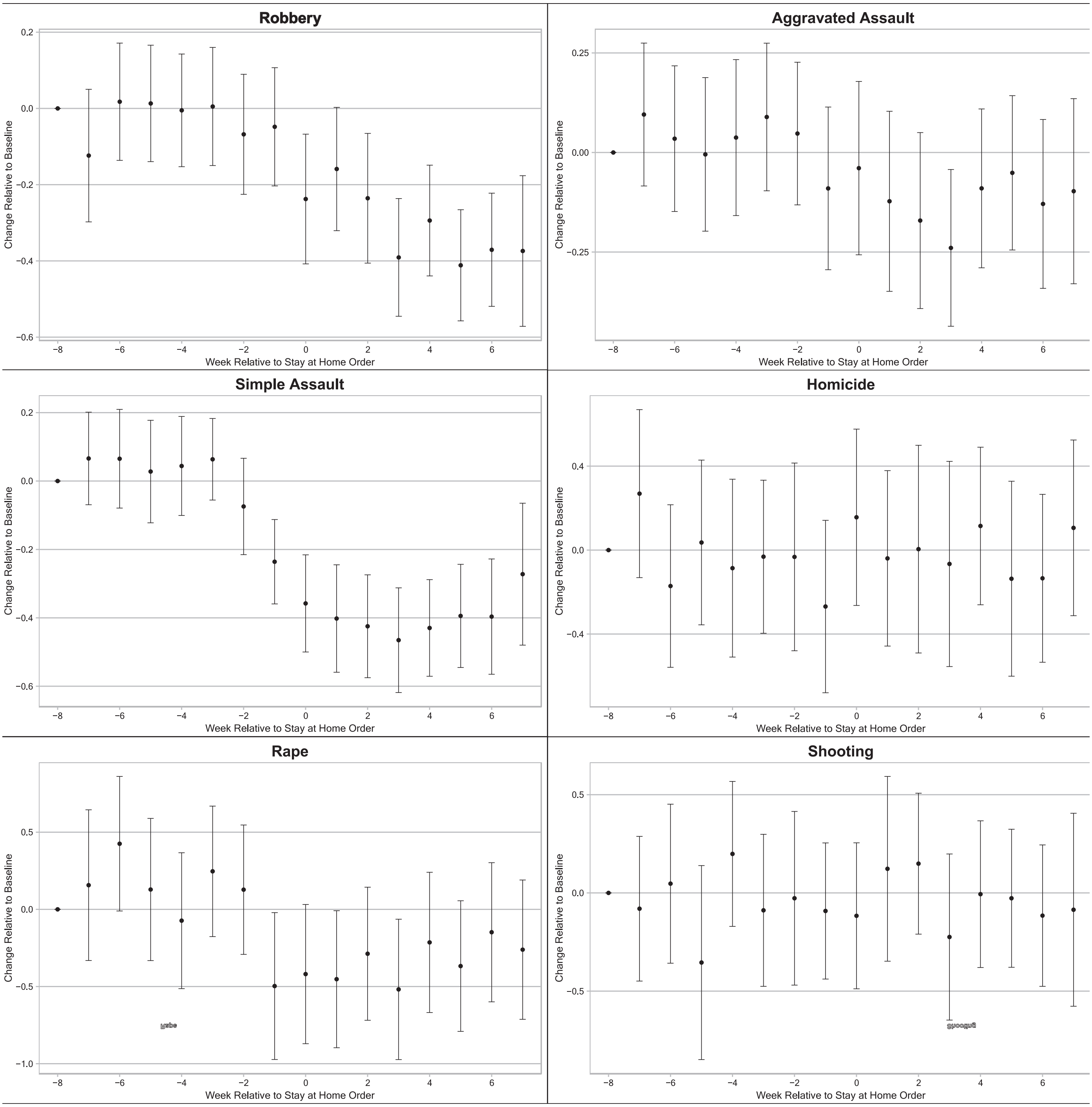

Fig. 6 (*continued*)

- Rape – Sexual penetration but not including statutory rape where it is possible to separate
- Robbery – Includes robbery with or without a weapon
- Shooting – Where an individual is shot with any type of firearm regardless of whether the injury caused the death of the victim
- Simple assault – An unlawful physical attack upon another person without the use of a weapon and where the victim did not suffer severe or aggravated bodily injury. Where possible to exclude, assaults without physical contact such as stalking and intimidation were not included.
- Theft – All forms of theft other than motor vehicle theft
- Theft from car – Theft of items from a car separate from the theft of the car itself

B.2. Arrests

Arrest data is typically obtained from Open Data Portals, often in the same datasets as the incident data, and uses the same categorization methodology. Some arrest data is also obtained directly from the cities. As there often isn't a separate arrest date, the crime occurrence date is generally used

B.3. COVID

Data on COVID incidence is taken from New York Times available at https://github.com/nytimes/covid-19-data. This data is provided at the county level and thus we proxy cases and deaths within the cities we analyze with the data for the county in which they are located. Note that New York City is comprised of multiple counties and includes the sum of those figures. As the dataset reports total cases and deaths to date we difference the data to calculate new daily cases and deaths.

B.4. Mobility

Google Mobility reports https://support.google.com/covid19-mobility?hl = en#topic = 9822927 were used to assess level of activity as the pandemic began. The data comes from use of the Google Maps and is available at the county level. It is reported as a change in mobility relative to a baseline which is the average value for that day of the week from the period Jan 3 – Feb 6, 2020. We make use of 4 of the 6 categories provided: Retail/Recreation, Transit, Workplace and Residential. Note that the residential category measures a change in duration while all other categories measure a change in the total number of visitors.

B.5. Key Dates

The key dates such as stay at home orders, reopening and protests start dates are generally sourced from news articles. The mobility drop dates are identified as the first day a city experienced a 20% drop in mobility within the transit and retail/recreation categories alongside a 5% increase in residential which roughly corresponds to a 2 standard deviation move from the mean from 15Feb2020-15Mar2020.

Links to city data sources (when available) listed below. Additional data construction detail available on request from the author.

City	Data Type	Link
Atlanta	Jail	http://www.dcor.state.ga.us/Research/Monthly_Profile_all_inmates
Austin	Incidents/ Arrests	https://data.austintexas.gov/resource/fdj4-gpfu.csv
	Jail	https://www.traviscountytx.gov/media/kunena/attachments/101/TC-JAIL-DASHBOARD-5–18-2020.pdf
Baltimore	Incidents	https://data.baltimorecity.gov/resource/wsfq-mvij.csv?
	Arrests	https://data.baltimorecity.gov/resource/3i3v-ibrt.csv?
	Shootings	https://data.baltimorecity.gov/resource/kj8k-eunk.csv?
Boston	Incidents	https://data.boston.gov/dataset/6220d948-eae2-4e4b-8723-2dc8e67722a3/resource/12cb3883-56f5-47de-afa5-3b1cf61b257b/download/tmph3gusfdy.csv
Chicago	Incidents/ Arrests	https://data.cityofchicago.org/resource/qzdf-xmn8.csv?
Cincinnati	Incidents	https://opendata.dc.gov/search?q = crime%20incidents
	Stops	https://data.cincinnati-oh.gov/resource/hibq-hbnj.csv?
Dallas	Incidents/ Shootings	https://www.dallasopendata.com/resource/qv6i-rri7.csv?
	Arrests	https://www.dallasopendata.com/resource/sdr7-6v3j.csv?
Denver	Incidents	https://www.denvergov.org/media/gis/DataCatalog/crime/csv/crime.csv
Detroit	Incidents	https://data.detroitmi.gov/datasets/rms-crime-incidents/data?geometry = -83.465%2C42.264%2C-82.733%2C42.442
Fort Worth	Incidents	https://data.fortworthtexas.gov/resource/k6ic-7kp7.csv?
Houston	Incidents	https://www.houstontx.gov/police/cs/Monthly_Crime_Data_by_Street_and_Police_Beat.htm
Los Angeles	Incidents	https://data.lacity.org/resource/2nrs-mtv8.csv?
	Arrests	https://data.lacity.org/resource/amvf-fr72.csv?
	Stops	https://data.lacity.org/resource/ci25-wgt7.csv?
Miami	Jail	https://gis-mdc.opendata.arcgis.com/datasets/db658214dccf4331bc1d82c8547d170a_0/data
Milwaukee	Incidents	https://data.milwaukee.gov/dataset/e5feaad3-ee73-418c-b65d-ef810c199390/resource/87843297-a6fa-46d4-ba5d-cb342fb2d3bb/download/wibr.csv
	Jail	https://doc.wi.gov/Pages/DataResearch/DataAndReports.aspx
Minneapolis	Incidents	http://opendata.minneapolismn.gov/search?q = police%20incidents
	Stops	http://opendata.minneapolismn.gov/datasets/police-stop-data
Nashville	Incidents/ Arrests	https://data.nashville.gov/resource/sie3-y9k4.csv?
	Stops	https://www.nashville.gov/Police-Department/Executive-Services/Strategic-Development/Crime-Analysis/Reports.aspx
	Jail	https://www.tn.gov/correction/statistics-and-information/jail-summary-reports.html
New York	Incidents	https://data.cityofnewyork.us/resource/5uac-w243.csv?

Key Dates (*continued*)

City	Data Type	Link
	Arrests	https://data.cityofnewyork.us/resource/uip8-fykc.csv?
	Shootings	https://data.cityofnewyork.us/resource/5ucz-vwe8.csv?
	Jail	https://www.criminaljustice.ny.gov/crimnet/ojsa/jail_population.pdf
Philadelphia	Incidents	https://phl.carto.com/api/v2/sql?filename = incidents_part1_part2&format = csv
	Arrests	https://raw.githubusercontent.com/phillydao/phillydao-public-data/master/docs/data/arrest_data_daily_by_district.csv
	Shootings	https://phl.carto.com/api/v2/sql?q = SELECT+*,+ST_Y(the_geom) + AS + lat,+ST_X(the_geom) + AS + lng + FROM + shootings&filename = shootings&format = csv
	Stops	https://phl.carto.com/api/v2/sql?filename = car_ped_stops&format = csv
Phoenix	Incidents	https://www.phoenixopendata.com/dataset/cc08aace-9ca9-467f-b6c1-f0879ab1a358/resource/0ce3411a-2fc6-4302-a33f-167f68608a20/download/crimestat.csv
	Jail	https://corrections.az.gov/reports-documents/reports/corrections-glance
Pittsburgh	Incidents	https://data.wprdc.org/dataset/uniform-crime-reporting-data
	Arrests	https://data.wprdc.org/dataset/arrest-data
Portland	Incidents	https://www.portlandoregon.gov/police/71978
San Francisco	Incidents/ Arrests	https://data.sfgov.org/resource/wg3w-h783.csv?
	Jail	https://data.sfgov.org/City-Management-and-Ethics/Scorecard-Measures/kc49-udxn
Seattle	Incidents	https://data.seattle.gov/resource/tazs-3rd5.csv?
	Stops	https://data.seattle.gov/resource/28ny-9ts8.csv?
	Jail	https://data.kingcounty.gov/Law-Enforcement-Safety/King-County-jail-COVID-19-statistics/qdny-y8ei
St. Louis	Incidents	http://www.slmpd.org/Crimereports.shtml
Washington DC	Incidents	https://opendata.dc.gov/search?q = crime%20incidents
	Jail	https://doc.dc.gov/sites/default/files/dc/sites/doc/publication/attachments/DCDepartmentofCorrections_FactsandFigures_April2020_0.pdf

Table A1
Data Availability by City.

City	Incidents	Arrests	Domestic Violence	Shootings
Austin	2015–May, 2020	2015–May, 2020	2015–May, 2020	–
Baltimore	2015–May, 2020	2015–May, 2020	–	2015–May, 2020
Boston	June, 2015–May, 2020	–	–	June, 2015–May, 2020
Chicago	2015–May, 2020	2015–May, 2020	2015–May, 2020	2015–May, 2020
Cincinnati	2015–May, 2020	2015–May, 2020	–	–
Cleveland	–	–	–	2015–May, 2020
Columbus	2015–May, 2020	–	–	2015–May, 2020
Dallas	2017–May, 2020	–	–	2015–May, 2020
Denver	2015–May, 2020	–	–	–
Detroit	2017–May, 2020	–	–	2015–May, 2020
Fort Worth	2019–May, 2020	–	–	2017–May, 2020
Houston	2015–May, 2020	–	–	–
Los Angeles	2015–May, 2020	2015–May, 2020	–	2015–May, 2020
Miami	–	–	–	2015–May, 2020
Milwaukee	2015–May, 2020	–	–	–
Minneapolis	2015–May, 2020	–	–	2015–May, 2020
Nashville	2015–May, 2020	2015–May, 2020	2015–May, 2020	–
New York City	2015–May, 2020	2015–May, 2020	–	2015–May, 2020
Philadelphia	2015–May, 2020	2015–May, 2020	–	2015–May, 2020
Phoenix	Nov, 2015–May, 2020	–	–	–
Pittsburgh	2016–May, 2020	–	–	2015–May, 2020
Portland	Apr, 2015–May, 2020	–	–	–
San Francisco	2015–May, 2020	2015–May, 2020	2015–May, 2020	2015–May, 2020
Seattle	2015–May, 2020	–	–	–
Washington DC	2015–May, 2020	–	–	–
City Count	**23**	**9**	**4**	**15**

Note: This table lists the 25 cities from which data was obtained and the time period covered for each data type. Not all cities report each crime type – see row labeled "# of

Table A2
Summary Statistics.

	Incidents per 100 k				
	Mean (before)	Mean (after)	SD (overall)	Difference	t-stat
Overall	23.23	18.84	9.20	−4.39	−25.1
Violent	2.36	2.29	1.56	−0.07	−2.1
Property	10.13	8.32	3.68	−1.81	−23.3
Drug	1.51	0.63	1.42	−0.87	−45.14
Homicide	0.04	0.05	0.09	0.01	5.89
Shooting	0.41	0.44	0.77	0.03	1.31
Aggravated Assault	1.09	1.19	0.76	0.10	6.02
Simple Assault	2.90	2.42	1.59	−0.48	−12.07
Rape	0.16	0.11	0.18	−0.06	−18.11
Robbery	0.90	0.65	0.64	−0.26	−23.80
Burglary	1.85	1.75	1.14	−0.10	−2.47
Burglary (Residential)	1.28	0.79	0.91	−0.49	−32.5
Burglary (Non-Residential)	0.47	0.87	0.65	0.40	9.5
Theft	5.64	3.85	2.75	−1.79	−48.2
Car Theft	1.24	1.41	0.80	0.17	9.7
Theft from Car	2.88	2.22	2.06	−0.66	−20.7

Note: Crime incident summary statistics reported for all cities that report the specified crime (see Table 1 for count). Mean incidents reported per 100,000 people are presented separately for the 7 weeks before and 4 weeks after the stay-at-home order is issued in a city. The overall standard deviation is reported by crime type, as well as

Table A3
Police-originated Share of Crime Incidents Relative to Pandemic Onset.

	Nashville		Dallas	
	Pre	Post	Pre	Post
Violent	3.6%	4.1%	10.8%	18.0%
Property	3.4%	3.7%	7.0%	4.8%
Drug	64.3%	69.1%	64.4%	70.3%

Note: Share of crime incidents reported by police for 3 broad crime categories is presented for Nashville and Dallas. Data from the 7 weeks prior to the stay-at-home order is in the Pre columns; Post includes the 4 weeks after the stay-at-home order. Data obtained from the respective cities.

Table A4
Change in Crime Rate Around Bars After Lockdown, Philadelphia.

	Dependent variable: Log of Crime Incidents			
	Theft (1)	Robbery (2)	Simple Assault (3)	Drugs (4)
After*Treat	-0.651^{***}	-0.615^{***}	-0.490^{***}	-1.375^{***}
	(0.112)	(0.170)	(0.129)	(0.325)
After*Treat*200 m	0.365^{**}	0.248	0.063	0.257
	(0.144)	(0.265)	(0.154)	(0.488)
After*Treat*300 m	0.395^{***}	0.667^{**}	−0.048	−0.269
	(0.142)	(0.274)	(0.159)	(0.384)
After*Treat*Remainder	0.324^{***}	0.462^{**}	0.044	0.139
	(0.120)	(0.204)	(0.140)	(0.385)
Constant	4.231^{***}	2.287^{***}	3.573^{***}	2.585^{***}
	(0.042)	(0.080)	(0.046)	(0.086)
Observations	264	264	264	264
Adjusted R^2	0.943	0.839	0.957	0.823

Note: *p<0.1 **p<0.05 ***p<0.01. This table reports the change in Philadelphia crime incidents from the pandemic onset conditioning on proximity to bars/restaurants. Crimes are classified into regions of 0–100 m, 100–200 m, 200–300 m and >300 m from the nearest bar/restaurant. The change in crime incidents is then reported using a similar specification to the difference-in-difference in equation (1) but interacting After*Treat with a dummy for each region (After*Treat*100 m is excluded). Each column reports a separate regression. Observations range from 7 weeks before stay-at-home order to 4 weeks after; the same weeks of the year are used for all years. After = 1 beginning the week of the stay-at-home order and 0 otherwise; Treat = 1 for 2020 and 0 otherwise. All regressions include year and week fixed effects. Standard errors calculated by wild

Table A5
Pandemic Onset Impact on Arrests.

	Dependent variable: Log of Number of Arrests			
	Overall (1)	Violent (2)	Property (3)	Drug (4)
After*Treat	-0.610^{***}	-0.184^{***}	-0.428^{***}	-1.468^{***}
	(0.065)	(0.066)	(0.083)	(0.198)
After	0.017	0.0003	0.017	0.017
	(0.01)	(0.019)	(0.016)	(0.023)
Treat	-0.411^{***}	-0.053	-0.387^{***}	-0.580^{***}
	(0.031)	(0.037)	(0.035)	(0.088)
Observations	594	594	594	528
Adjusted R^2	0.977	0.96	0.946	0.85

Note: *p<0.1 **p<0.05 ***p<0.01. Using arrests as the measure of crime, the difference-in-difference specification (equation (1)) is estimated for all crime types as well as by broad categories using weekly crime data from 9 large U.S. cities for 2015 – 2020. Observations range from 7 weeks before stay-at-home order to 4 weeks after in that city; the same weeks of the year are used for all years. After = 1 beginning the week of the stay-at-home order and 0 otherwise; Treat = 1 for 2020 and 0 otherwise. All regressions

Table A6
Robustness Checks.

	Dependent variable: Log of Overall Crime Incidents								
	Before window			After window					
	7 weeks (1)	5 weeks (2)	3 weeks (3)	2 weeks (4)	4 weeks (5)	6 weeks (6)	8 weeks (7)	2019 (8)	Unemployment (9)
After*Treat	-0.265^{***}	-0.241^{***}	-0.203^{***}	-0.259^{***}	-0.265^{***}	-0.255^{***}	-0.248^{***}	-0.255^{***}	-0.227^{***}
	(0.024)	(0.025)	(0.027)	(0.031)	(0.024)	(0.020)	(0.018)	(0.022)	(0.047)
Unemployment Rate									-0.005
Observations	1,221	999	777	999	1,221	1,443	1665	418	1,221
Adjusted R^2	0.976	0.983	0.985	0.975	0.976	0.977	0.978	0.985	0.976
									*p**p***p < 0.01

Note: *p<0.1 **p<0.05 ***p<0.01. This table present results robustness checks of the main results presented in Table 1. Overall crime rate is the dependent variable in each column, which presents a separate regression based off the difference-in-difference specification in equation (1) with the following differences. The first 3 columns vary the number of weeks in the before period; the next 4 columns vary the number of weeks in the after period. 7 weeks in the before period and 4 weeks in the after period is the baseline that is used in Table 1. Column 8 uses only data from 2019 and 2020. Column 9 adds the unemployment rate as an additional control variable to the base specification. After = 1 beginning the week of the stay-at-home order and 0 otherwise; Treat = 1 for 2020 and 0 otherwise. All regressions include city and week fixed effects.

Table A7
Crime Drop by City.

		Austin	Baltimore	Boston	Chicago	Cincinnati	Columbus	Denver	Houston	Los Angeles	Milwaukee	Minneapolis	Nashville	NYC	Philadelphia	Phoenix	Pittsburgh	San Francisco	Seattle	Washington DC
Overall	After*Treat	−0.135**	−0.344**	−0.345**	−0.423**	−0.063	−0.183**	−0.161**	−0.116**	−0.175**	−0.209**	−0.191**	−0.159**	−0.485**	−0.458**	−0.081**	−0.606**	−0.461**	0.005	−0.452**
		(0.033)	(0.077)	(0.063)	(0.042)	(0.065)	(0.037)	(0.05)	(0.035)	(0.036)	(0.054)	(0.045)	(0.047)	(0.036)	(0.049)	(0.016)	(0.156)	(0.046)	(0.042)	(0.084)
	After	0.041*	0.261**	0.079	0.072	0.131**	0.102**	0.108*	0.002	−0.003	0.142**	0.081	0.135	0.085**	0.136**	0.02	0.477	−0.038	0.095	0.198*
		(0.018)	(0.082)	(0.049)	(0.04)	(0.035)	(0.026)	(0.042)	(0.031)	(0.021)	(0.052)	(0.098)	(0.077)	(0.025)	(0.028)	(0.03)	(0.401)	(0.023)	(0.06)	(0.084)
	Treat	−0.015**	−0.003	−0.034**	−0.009	−0.019**	−0.022**	0.002	−0.049**	−0.002	−0.027**	0.042**	−0.004	−0.0004	−0.005	0.019**	−0.018	−0.011**	−0.006	−0.002
		(0.003)	(0.01)	(0.009)	(0.006)	(0.006)	(0.006)	(0.008)	(0.003)	(0.004)	(0.006)	(0.007)	(0.008)	(0.005)	(0.007)	(0.002)	(0.019)	(0.003)	(0.004)	(0.013)
Violent	After*Treat	0.058	−0.424**	−0.246**	−0.283**	0.098	−0.3**	−0.094	−0.018	−0.173**	−0.181**	−0.257	−0.152	−0.467**	−0.327**	−0.043	−0.614**	−0.514**	0.071	−0.249**
		(0.097)	(0.136)	(0.068)	(0.077)	(0.106)	(0.092)	(0.101)	(0.046)	(0.047)	(0.064)	(0.134)	(0.103)	(0.039)	(0.044)	(0.047)	(0.188)	(0.099)	(0.072)	(0.082)
	After	0.06	0.424**	0.011	0.135*	0.109	0.044	0.229**	0.037	0.122*	0.066	0.243	0.092	0.058	0.125**	0.131**	0.579	−0.039	0.113	0.29*
		(0.072)	(0.12)	(0.099)	(0.053)	(0.095)	(0.069)	(0.057)	(0.043)	(0.05)	(0.069)	(0.155)	(0.139)	(0.031)	(0.035)	(0.044)	(0.342)	(0.066)	(0.062)	(0.116)
	Treat	0.002	0.045**	0.008	0.028**	−0.015	0.02*	0.01	0.031**	0.012*	0.018*	0.029	0.022*	0.009	0.017**	0.042**	−0.031	−0.029**	0.01	−0.062**
		(0.008)	(0.011)	(0.011)	(0.007)	(0.01)	(0.008)	(0.013)	(0.003)	(0.006)	(0.007)	(0.015)	(0.01)	(0.005)	(0.006)	(0.006)	(0.021)	(0.008)	(0.011)	(0.014)
Property	After*Treat	−0.073	−0.288**	−0.117	−0.463**	−0.074	−0.204**	0.113*	−0.163**	−0.137**	−0.179*	−0.129*	−0.197**	−0.479**	−0.208**	−0.101**	−0.432*	−0.491**	0.011	−0.48**
		(0.049)	(0.075)	(0.069)	(0.05)	(0.06)	(0.054)	(0.045)	(0.037)	(0.032)	(0.074)	(0.063)	(0.062)	(0.037)	(0.062)	(0.022)	(0.182)	(0.056)	(0.05)	(0.091)
	After	0.002	0.218*	0.067	0.058	0.083*	0.094**	0.092*	−0.005	−0.03	0.164*	−0.017	0.214*	0.098**	0.105**	0.002	0.568	−0.064	0.12	0.185*
		(0.033)	(0.091)	(0.056)	(0.062)	(0.037)	(0.034)	(0.045)	(0.032)	(0.022)	(0.073)	(0.104)	(0.087)	(0.028)	(0.032)	(0.036)	(0.535)	(0.039)	(0.07)	(0.085)
	Treat	0.003	−0.032**	−0.028**	0.001	−0.034**	−0.009	0.029**	−0.073**	0.0002	−0.062**	0.033**	0.041**	0.016**	0.03**	0.01**	−0.025	0.014**	−0.004	0.008
		(0.005)	(0.012)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.004)	(0.003)	(0.006)	(0.006)	(0.01)	(0.006)	(0.006)	(0.003)	(0.023)	(0.004)	(0.006)	(0.014)
Drug	After*Treat	−1.01**		−1.204**	−1.859**		−1.05**	−1.544**					−0.275	−1.284**	−1.297**	0.022	−1.707**	−1.353**	−0.003	
		(0.149)		(0.291)	(0.159)		(0.261)	(0.254)					(0.155)	(0.103)	(0.209)	(0.11)	(0.417)	(0.167)	(0.155)	
	After	0.000		0.164	−0.029		0.068	0.395*					0.139	−0.004	0.154	−0.02	0.331	0.188	−0.204	
		(0.105)		(0.242)	(0.081)		(0.157)	(0.187)					(0.128)	(0.047)	(0.104)	(0.078)	(0.546)	(0.107)	(0.199)	
	Treat	0.049**		−0.134**	−0.113**		−0.089**	−0.05					−0.117**	−0.081**	0.041	0.067**	0.013	−0.021	−0.051**	
		(0.019)		(0.032)	(0.011)		(0.018)	(0.027)					(0.014)	(0.011)	(0.025)	(0.008)	(0.036)	(0.015)	(0.017)	

Note: *p<0.1 **p<0.05 ***p<0.01. Using incidents as the measure of crime, the difference-in-difference specification (equation (1)) is estimated for all crime types as well as by broad categories using weekly crime data separately for 19 large U.S. cities for 2015 – 2020. Observations range from 7 weeks before stay-at-home order to 4 weeks after in that city; the same weeks of the year are used for all years. After = 1 beginning the week of the stay-at-home order and 0 otherwise; Treat = 1 for 2020 and 0 otherwise. All regressions include week fixed effects. Standard errors calculated by wild bootstrap. Data source: city police departments.

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