

# Beyond Crime Rates: How Did Public Safety in U.S. Cities Change in 2020?\*

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This version: March 8, 2022

## Abstract

This paper argues that changes in human activity during the COVID-19 pandemic led to an unusual divergence between crime rates and victimization risk in US cities. Most violent crimes declined during the pandemic. But analysis using foot traffic data shows that the *risk* of street crime victimization was elevated throughout 2020; people in public spaces were 15-30 percent more likely to be robbed or assaulted. This increase is unlikely to be explained by changes in crime reporting or selection into outdoor activities by potential victims. Traditional crime rates may present a misleading view of recent changes in public safety.

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# 1 Introduction

The onset of the COVID-19 pandemic brought massive disruptions to economic and social life, including an unprecedented spike in homicides. Overall crime, however, was down, spurring a conventional narrative that gun violence has been an exception (Graham, 2021; Nass, 2020) and that, contrary to public opinion, the pandemic has not had a large impact on public safety (Ashby, 2020; Abrams, 2021; Koerth and Thomson-DeVeaux, 2020). This understanding, based on analyses of traditional crime data, informs an important discussion among researchers, public safety advocates, and journalists about recent changes in crime. But is it substantively correct?

In this paper, we illustrate how changes in human activity during the COVID-19 pandemic led to an unusual divergence between crime rates and victimization risk. Beginning in March 2020, public violence—assaults and robberies occurring in public or commercial spaces—declined by approximately 35% as people responded to disease risk and mandated lockdowns by spending more time at home. These offenses remained 10-15% below 2019 levels throughout the summer before returning to 2019 levels in the fall. But what happened to the *risk* of violent crime that people faced in while spending time in public spaces? Because our principal measure of public safety—the crime rate—is so limited, conventional analyses are unable to answer this question.<sup>1</sup>

Using a variety of administrative and survey sources, as well as information on the movement of people based on mobile location data, we study the risk of public victimization in 2020. Newly-available survey data from the American Time Use Survey as well as foot traffic data from Google, Apple, Facebook and Safegraph, show that time spent outdoors fell rapidly in March 2020 before slowly rebounding thereafter. While Americans eventually began to return to many of their normal activities, even at the end of 2020, time spent outdoors remained about 20% lower than in 2019.

We then compare the number of violent crimes known to law enforcement to measures of

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<sup>1</sup>From its earliest inception, the per capita crime rate has always been intended to provide a rough proxy for victimization risk (Boggs, 1965; Stipak, 1988; Vaughan et al., 2020; Ramos, 2021). Prior research has proposed various of ways of moving beyond crime rates to better capture victimization risk. We provide a brief history of prior thinking about how to compute an “activity adjusted crime rate” in Appendix A.

time spent away from home in order to calculate the risk of street crime victimization. We formulate this as the count of violent crimes occurring in public spaces divided by a measure of public foot traffic, inferred from SafeGraph data. While analyses of traditional crime data show discrete drops in offending during the post-pandemic lockdown period, we find that, in 2020, the risk of outdoor street crimes initially *rose* by more than 40% and was consistently between 10-15% higher than it had been in 2019 through the remainder of the year.<sup>2, 3</sup> In the cities we study, people were more likely to be robbed or assaulted while spending time in public.

We consider two main challenges to this conclusion. First, it is possible that the reporting of crimes to law enforcement changed in 2020 either due to fears of COVID-19 exposure or the legitimacy crisis which unfolded after the murder of George Floyd in May 2020. To address this possibility, we use data from the 2020 National Crime Victimization Survey (NCVS), a nationally representative repeat cross-sectional survey of American residents. Comparing the pooled 2015-2019 and 2020 waves of the NCVS, we see little evidence that crime reporting changed appreciably.

A second potential issue is selection. If in 2020, people selecting into outdoor activities—and who were thus at risk of a street crime—are those with a higher baseline risk of victimization, our activity-adjusted estimates of crime risk could be biased towards a finding of increased risk. We assess the importance of selection in three different ways. First, in a DFL-style reweighting exercise (DiNardo et al., 1996), we use the American Time Use Survey (ATUS) and NCVS to calculate a counterfactual 2020 crime rate using observed activity

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<sup>2</sup>See Ashby (2020); Boman and Gallepe (2020); Gerell et al. (2020); Hodgkinson and Andresen (2020); Langton et al. (2021); Payne et al. (2020); Piquero et al. (2020); Shayegh and Malpede (2020); Abrams (2021); Boman and Mowen (2021); Campedelli et al. (2021); De la Miyar et al. (2021); Nivette et al. (2021). Another strand of pandemic-related crime research concerns domestic violence which many observers theorized might rise during mandated lockdowns. See e.g., Leslie and Wilson (2020), Miller et al. (2020), Bullinger et al. (2021), Hsu and Henke (2021), Ivandic et al. (2020) and Piquero et al. (2021).

<sup>3</sup>These findings complement and greatly extend recent research which uses Google’s COVID-19 Community Mobility Reports to assess post-pandemic changes in criminal opportunities in the United States (Lopez and Rosenfeld, 2021) and the United Kingdom (Halford et al., 2020). While this research notes that criminal opportunities shifted in 2020, it does not utilize a pre-pandemic measure of mobility, does not distinguish between crimes that occurred in residential and public locations, does not address selection effects and ultimately, it does not construct a quantitative test of the change in victimization risk during the pandemic.

changes and 2015-2019 victimization rates by age, race, and gender. We find that the counterfactual rates are nearly identical to actual crime, suggesting that selection into activity among higher-risk groups is not biasing the results. Second, using microdata from NYC and Los Angeles, we directly examine changes in the demography of crime victims. If anything, crime victims in the police data had *lower*, not higher, historical rates of victimization. Third, to test whether the increase in risk is driven by people in high-risk neighborhoods spending relatively more time outside, we condition on Census block group fixed effects, finding that within-neighborhood risk also increased during the pandemic. The analyses support our main finding that risk for the average person increased.

Finally, though our primary results focuses on three large cities, two additional analyses suggest that these results apply more broadly. We replicate our primary measure of risk, which uses police reports combined with SafeGraph data, with entirely different data sources: national survey data on crime victimization and time use. Those sources suggest a strikingly similar increase in the risk of victimization. More speculatively, we show using data from [Abrams \(2021\)](#) that across the largest cities in the US, the decrease in mobility was larger than the decrease in crime, which also implies—albeit with more stringent assumptions—that crime risk increased broadly across large US cities.

## 2 Data

### 2.1 Crime data

Crime is measured using publicly available microdata from NYC ([NYC, 2022](#)), Los Angeles ([LA, 2022](#)) and Chicago ([Chicago, 2022](#)), the three largest cities in the United States. In each city, the microdata represent the universe of crimes know to law enforcement. For each crime, we observe the date, time, and type of crime. In NYC and Los Angeles, we also observe the age, gender and race of crime victims. We use these data to address the possibility that the demographics of crime victims changed in 2020.

Critically, in all three cities we observe detailed information on the location of each crime, allowing us to identify whether the crime occurred inside a residence or in a public setting. In our primary analysis, we consider a crime to occur in a public space if it happened in any location that is not within the confines of a residential building or a connected property

such as a yard or garage. Using this definition, public spaces include streets, parks and alleyways as well as commercial establishments and offices where people work. We also present alternative results where we count only streets, parks and alleyways as public spaces and confirm that the findings remain unchanged.

Because rates of crime reporting to law enforcement may have changed in 2020, we present additional analysis of reporting behavior using the 2015-2020 waves of the National Crime Victimization Survey (NCVS), a nationally representative cross-sectional survey of American residents, the most accurate source of data on the reporting of crimes to law enforcement in the United States (Gutierrez and Kirk, 2017). The NCVS directs respondents to indicate if they have been the victim of a crime during the past six months. If so, respondents are asked to identify the types of crimes they have experienced and provide key details about each incident including the type of location in which the crime occurred. We use this information to focus on the subset of violent crimes that occurred in public spaces. For details about how we classify crimes as occurring in either public or residential locations in both the administrative data and the NCVS, see Appendix B, section 1.

## 2.2 Mobility data

Our next task is to measure how the availability of potential street crime victims changed during the pandemic. In an ideal world we would be able to observe the number of person-hours spent in public spaces and use this as a denominator against which to compare street crimes in 2019 and 2020. While activity cannot be captured perfectly using available administrative data, we employ several data sources that allow us to construct proxy measures. In this section, we describe each of these data sources.

### 2.2.1 Safegraph patterns data

Our primary data source is SafeGraph’s Neighborhood Patterns data, anonymized GPS data from cell phones with location services enabled. These data provide a count of the number of unique people present in each Census block group in each month, disaggregated according to some broad features such as the Census block group that each visitor came from and whether a given day was a weekday versus a weekend day. There are two distinct advantages of the

Safegraph data. First, the data provide a direct measurement of the movement of people throughout the day that does not rely on self-reports from survey data. Second, microdata are available by neighborhood, which allows for city-level and even sub-city level analyses.

While the data allow us to identify foot traffic with considerable granularity, they are subject to limitations. In particular, the data come from a non-random sample of users using certain apps. If this sample exhibits differential trends in activity, we could mismeasure changes in risk. We test the reliability of the data by measuring its trends against four other known sources of mobility information. In addition, to account for the fact that the data is a sample, following prior literature, all visitor counts are scaled by the inverse sampling probability in the home Census block group. More details about the data construction are in the Appendix B, section 2.

The SafeGraph data are accurate proxies for our ideal denominator if overall time spent outdoors scales linearly with the number of unique census block groups visited. For instance, if Census block groups in NYC went from 15,000 unique daily visitors in 2019 to 10,000 unique daily visitors in 2020, this would register as a 33% decline in outdoor activity in our data. However, this would overstate the decrease if people were spending the same amount of time outside but in a fewer number of neighborhoods, and it would be an underestimate if people were spending even less time outside when they were in their home neighborhoods. To address this possibility, we complement our SafeGraph analysis with survey data from the American Time Use Survey as well as geolocation data gathered by Google, Apple and Facebook. Each of these data sources leads to a similar conclusion—in 2020, time spent in public spaces declined discretely in the spring and slowly rebounded thereafter.

### 2.2.2 Other mobility data

The additional mobility datasets we analyze are provided by [Google \(2022\)](#), [Apple \(2022\)](#), and [Meta \(2022\)](#). These datasets have been used in several studies on COVID-19 and social distancing (e.g. [Cot et al., 2021](#); [Venter et al., 2020](#)); examples which emphasize methodology are [Ilin et al. \(2021\)](#) and [Arnal et al. \(2020\)](#). Each of the three sources uses a different technique to define mobility. The Facebook data consists of two daily indexes, provided at the county level, measuring how often people stayed in a given Bing tile (a 600

square meter area) and how many different tiles people visited (Herdağdelen et al., 2021). The Apple data measures direction requests for walking, driving, and transit compared to a baseline measured on January 13, 2020 (Cot et al., 2021). The Google data show mobility trends across four different categories: grocery and pharmacy, parks, retail and recreation, residential, and workplaces (Google, 2021; Cot et al., 2021). The Apple data is based on all Apple Maps users, while Google and Facebook data is based on those with location services enabled.

Finally, we consider survey data from the American Time Use Survey (ATUS). The ATUS is a product of both the US Bureau of Labor Statistics and the US Census Bureau, designed to study how and where people living in the United States spend their waking hours. The survey has been administered annually to a nationally representative sample of between 20,000 and 40,000 American households since 2003 and asks respondents to carefully document how they spent their time during a recent day. Importantly, the data permit researchers to delineate between time spent at one’s home versus elsewhere. Respondents are surveyed throughout the year except for a gap corresponding to April 2020. We use each individual’s survey date to build a monthly repeated cross-section dataset of time use, limiting the data to residents of metropolitan areas. For each respondent, we aggregate time spent in all activities that occurred in one’s home or yard. To identify time spent away from one’s home we subtract this number from the respondent’s total waking hours.<sup>4</sup>

### 3 Methods

Researchers and social planners typically track changes in public safety using the crime rate: the number of crimes known to law enforcement divided by an area’s population (Nolan III, 2004). While public safety can refer to a number of different ideas, a common conception in research and policy involves the risk of victimization for a typical citizen (Boggs, 1965; Stipak, 1988; Vaughan et al., 2020; Ramos, 2021). This conception of victimization risk motivates the central statistics—crime incidence and prevalence—that are released by the US Bureau of Justice Statistics’ in their annual distillation of the US National Crime Victimization

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<sup>4</sup>While the ATUS does not ask respondents where they slept, it does record detailed data about where a respondent spent his or her waking hours.

Survey, the sole national victimization survey in the United States. Victimization risk is likewise key to how members of the public think about safety, especially when it comes to the risk of becoming the victim of a violent street crime (Ferraro, 1995; Pickett et al., 2012).

The crime rate is given as the count of crime divided by some measure of the population at risk. Our crime measure is the count of violent street crimes—assaults and robberies occurring in public—which constitute the majority of violent crime. The ideal denominator would be a direct measure of the amount of time spent outdoors, so that a crime rate could be calculated in terms of person-hours spent in public:

$$Risk_t = \frac{Crimes_t}{PersonHoursOutside_t} \quad (1)$$

The proxy that Safegraph provides for the denominator in (1) is a measure is of the total number of visitors across neighborhoods, counting both residents and non-residents. This will increase by 1 for a given Census block group whenever a new person enters the area and remains there for at least a few minutes.

Pooling crimes and visits across all cities in our sample, our measure of victimization risk in period  $t$  is given by:

$$Risk_t = \frac{Crimes_t}{Visitors_t}. \quad (2)$$

This simple construction implies that if crimes decrease by a smaller percentage than visitors, risk has increased. The change in risk across periods 1 and 2 is:

$$Risk_2 - Risk_1 = \frac{Crimes_2}{Visitors_2} - \frac{Crimes_1}{Visitors_1} \quad (3)$$

which is only positive if:

$$100 * \frac{Crimes_2 - Crimes_1}{Crimes_1} > 100 * \frac{Visitors_2 - Visitors_1}{Visitors_1}. \quad (4)$$

where (4) shows the percent change in crime on the left and the percent change in visitors on the right. This formulation is useful because if percentage changes in mobility closely track the percentage change in exposure as we define it, we can use mobility indexes from Apple, Google and Facebook in order to measure the percentage change in risk without having the precise quantities underlying the indexes, which are not available from those data sources.<sup>5</sup>

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<sup>5</sup>We focus primarily on SafeGraph data because it is available in both 2019 and 2020 and because their measures are the easiest to interpret.

Finally, since mobility data from SafeGraph could be misleading if it is a poor proxy for time at risk, we also show that we obtain similar results when we switch to survey-based measures of time use.

## 4 Results

### 4.1 Mobility

Data on time spent in public spaces in 2019 and 2020 are presented in [Figure 1](#). The figure presents trends using mobility data from Safegraph (panel A), Google (panel B), Apple (panel C), Facebook (panel D) as well as ATUS survey data (panel E). For the mobility data, we restrict the sample to our three cities—NYC, Los Angeles and Chicago—and aggregate across the cities to present information for the combined sample.<sup>6</sup> All five sources of data indicate that outdoor foot traffic declined steeply at the beginning of the pandemic. In the Safegraph data, the total count of unique neighborhood visits across our three cities fell from an average of 220 million to 110 million from 2019 to 2020, a 50% decrease. The Google, Apple and Facebook data are more limited in that they are only available for 2020 and provided in terms of indexes relative to early 2020.<sup>7</sup> Still, these data sources support a similar story.

Next, we consider survey data from the ATUS, plotting the share of waking hours spent away from one’s home by month in 2019 and 2020. In 2019, respondents reported spending approximately 46% of their waking hours away from home, a statistic which exhibits relatively little seasonal variation. While this trend continued in January and February 2020, by May 2020, the proportion had fallen to 20%.<sup>8</sup> By the end of 2020, time spent outdoors remained approximately 50% lower than in 2020. Though the precise magnitudes differ somewhat depending on the source of data used, the broad trends are strikingly similar.

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<sup>6</sup>We present the same data, disaggregated by city in [Appendix E](#) and note that patterns are extraordinarily similar across cities. While the time-path of the pandemic’s impact differed widely among the three cities — see [Appendix Figure F.2](#) — the fact that mobility patterns are broadly similar suggests that these are driven to a greater extent by lockdowns or national trends than explicitly by localized health risks.

<sup>7</sup>Apple data is since January 13, 2020. The Google baseline is Jan 3–Feb 6, 2020 ([Google, 2022](#)). Facebook baseline is from the end of February 2020 ([COVID-19 Mobility Data Network, 2022](#)).

<sup>8</sup>The ATUS was temporarily suspended between mid-March and mid-May 2020 due to pandemic-induced lockdowns.

## 4.2 Victimization Risk

We plot crimes known to law enforcement for our combined NYC-Los Angeles-Chicago sample in panel A of [Figure 2](#), presenting monthly counts of violent crimes separately for 2019 and 2020.<sup>9</sup> Public violence was approximately 10% higher in the winter of 2020 than the winter of 2019 but began to fall steeply after March 2020. In April 2020, public violence was approximately one third lower than it had been in April 2019. Consistent with the seasonality documented in prior research ([Andresen and Malleson, 2013](#); [Jacob et al., 2007](#)), public violence rose during the spring and summer months in both years. However, throughout the summer and into the fall months, these crimes remained between 5-20% lower in 2020 than they had been in 2019. Aggregating across the entire pandemic period of 2020 (March through December), public violence was 19% lower in 2020 than it had been in 2019.<sup>10</sup>

Next, Panel B plots violent street crime risk per 100,000 people using Safegraph data to measure the change in exposure. In March 2020, street crime victimization risk was roughly equal to risk in the previous year at approximately 5 violent street crimes per month per 100,000 person-visits to a community.<sup>11</sup> However, at the same time that street crimes fell by more than 30% in April 2020, the *risk* of street crime victimization rose by nearly 40%. Victimization risk remained elevated throughout the summer—approximately 12% higher in July 2020 than July 2019—and never fully returned to trend, even by the end of the year. For the March-December period, street crime victimization risk was approximately 14% higher in 2020 than it had been in 2019, despite a 19% decline in the incidence of public violence.

Taken together, these results suggest that the increase in risk was not restricted to an aberration in gun violence. While gun violence rose by 47% in the three largest cities in the United States, the risk of other violent street crimes rose as well. Though street crimes appear to be less sensitive to the effects of the pandemic than shootings, the trends are much more similar after accounting for changes in criminal opportunities.

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<sup>9</sup>Results for individual cities can be found in [Appendix E](#). An alternative set of results using a narrower definition of public locations is available in [Figure C.2](#).

<sup>10</sup>A corresponding decline in violent victimization is also observed in survey data from the NCVS which recorded a 24% decline between the 2019 and 2020 surveys.

<sup>11</sup>This statistic cannot be directly compared to per capita crime measures generated from administrative data as the denominator uses a different proxy for exposure.

### 4.3 Robustness and Heterogeneity

In Appendix C we probe the robustness of our main results. We briefly summarize these analyses here. First, in Appendix Figure C.1, we re-produce Figure 2 using ATUS survey data to measure the change in mobility between 2019 and 2020.<sup>12</sup> The estimate is even larger, suggesting a 32% increase in risk over the sample period. Next, we consider an alternative definition of public violence, limiting the analysis to crimes that occur in outdoor locations as opposed to all non-residential locations. Those results can be found in Appendix Figure C.2 and are also substantively similar to our main results.

In Appendix D, we consider an alternative setting in which a changes in crime risk has an especially transparent and well-measured denominator: crimes on public transit, where we also record a large increase in risk. In Appendix E we present results for all three cities separately.

## 5 Alternative Explanations

In the preceding section we observed that while the official crime rate in America’s three largest cities was lower in 2020 than it had been in 2019, the risk of public violence increased. In this section, we provide evidence that the increase in the risk of street crime victimization that we observe in 2020 is unlikely to be an artifact of changes in crime reporting behavior by victims or compositional changes in the pool of people outside.

### 5.1 Victim Reporting

To the extent that victims became more reluctant to report crimes to law enforcement due to public health risks, a legitimacy crisis in policing (Tyler, 2004; Tankebe, 2014; Wolfe et al., 2016) sparked by the killing of George Floyd (Nix, 2021), or the perception that police and prosecutors were otherwise occupied, declining crime might be a mechanical artifact of a change in reporting behavior (Van Dijk, 1979; Levitt, 1998; Davis and Henderson, 2003). In using the official data and transforming it to account for changes in mobility, we might then

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<sup>12</sup>The ATUS data does not contain the city of residence, so we restrict to respondents living in a metropolitan area to approximate the mobility changes in our three-city sample.

understate the increase in victimization risk. Similarly, we could overstate the risk of crime reporting increased.

To address this concern, we appeal to newly released data from the 2020 National Crime Victimization Survey in which a nationally representative sample of respondents are asked about crime victimization as well as whether they reported crimes to law enforcement. Appendix Figure C.3 presents the share of outdoor violent crimes that victims reported crimes to police among respondents interviewed in the 2019 and 2020 waves of the survey. To maintain consistency with our administrative data from large cities, we focus on survey respondents living in metropolitan areas. Contrary to concerns that crime reporting may have fallen during the pandemic, the rate at which outdoor person crimes were reported to the police did not change appreciably in 2020. To address the concern that law enforcement may have simply discovered fewer crimes in 2020, in Section 6, we use the same NCVS survey data and show that we obtain a similar estimate when we rely solely on survey data to compute victimization risk.

## 5.2 Selection

A second potential issue is selection, the possibility that the composition of individuals who spent time outdoors and were therefore at risk of street crime victimization changed in 2020. Since the pandemic has had unequal public health impacts according to gender, race, and especially age (e.g. Hutchins et al., 2020; Miller et al., 2021), each of which is among the strongest predictors of victimization risk (e.g. Perkins, 1997), this is a critical concern. In the extreme, it is possible that the aggregate risk of victimization could increase while decreasing for every demographic group, a case of Simpson’s paradox (Blyth, 1972). In this section, we motivate three tests for the importance of selection using national data on time use from the ATUS, city-specific data on realized crime victimization, and within-neighborhood changes in victimization risk derived from both municipal microdata and Safegraph patterns data. None of the three tests suggests that the increase in victimization risk that we observe after March 2020 is likely to be an artifact of selection.

### 5.2.1 Test of demographic selection

Our first test of selection uses a DFL-style reweighting exercise (DiNardo et al., 1996) to assess the contribution of selection into activity to the crime rate. This computation addresses how crime would have changed if activity had evolved as observed and victimization rates were fixed at their pre-2020 levels. Using data from the 2015-2019 NCVS we compute public victimization rates for each of eighteen age-race-gender cells defined by the intersection of race (White, Black, Other), gender (male, female) and age ( $< 25$ , 26-49 and  $> 50$ ).<sup>13</sup> These data are combined with estimates from the ATUS, which gives the share of time spent outside in 2019 and 2020 for each group. In Figure 3 we plot baseline time use (Panel A), the change in time use between 2019 and 2020 (Panel B) and baseline victimization rates (Panel C) for each of the eighteen demographic groups.

Prior to the pandemic, though older people spent less time outdoors than younger people, overall there was little variation in time spent outdoors among the eighteen groups. How did time spent outside change after the COVID-19 pandemic? The decline in the share of time spent in public spaces was fairly universal and, across all groups, there was a 16% decrease in waking hours spent outdoors in 2020. However, several dimensions of heterogeneity are notable. First, the behavioral response to the pandemic was notably muted among prime-age Black men. Second, the largest disruption to pre-pandemic behavior occurred in individuals under the age of 25, some of whose routine activities were disrupted by school closures.

In Panel C, we consider heterogeneity in baseline victimization risk, measured during the pre-pandemic period. In the figure, the blue bars represent the number of violent victimizations occurring in public locations per 1,000 people in each group. The red bars use the data from Panel A to compute activity-adjusted victimization risk. This computation shrinks or inflates each group's victimization rate according its outdoor activity relative to the population average. For example, white males under 25 spend more time outside: 10.9 hours compared to an average of 9.6 hours in the ATUS population. Their activity-adjusted victimization rate is scaled by  $9.6/10.9$  or about 0.88. Accounting for their increased activity makes their activity-adjusted victimization rate lower.

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<sup>13</sup>We use less granular age categories to guard against drawing inferences from small bins in the ATUS data.

As is evident from the raw victimization rates, there is a great deal of heterogeneity in victimization rates across the eighteen groups with an especially strong age gradient. While young men face annual outdoor violent victimization rates of nearly 30 per 1,000, individuals over the age of 50 face rates that are uniformly below 10 per 1,000 and are sometimes as low as 5 per 1,000. For individuals below age 50, men face uniformly higher victimization rates than women with the gender gradient attenuating somewhat in the highest age category. When we adjust victimization rates for differences in time spent outside, the differences between groups narrow albeit only slightly. An implication of this result is that, prior to the pandemic, between-group differences in victimization risk are not well explained by between-group differences in mobility.

Given the outsize importance of age in predicting victimization risk and the fact that time spent outside declined more among younger individuals than among older individuals, selection effects point to a reduction in baseline risk in 2020. An implication of this finding is that the increase in victimization risk we observe in 2020 is unlikely to be explained by changes in the demography of the risk set. To further scrutinize this informal observation, we predict the change in victimization risk that would arise from an observed shift in the demographics of individuals spending time outside their homes. We compute three quantities: 1) the total number of public violent crimes experienced by Americans in the pre-pandemic period, 2) the expected change in victimization given the observed change in time spent in public for each demographic subgroup and 3) the expected change in victimization assuming that every demographic subgroup reduced their time spent in public by the same amount in 2020. The difference between (2) and (3) indicates the importance of demographic selection effects. Algebraic details for this set of computations may be found in Appendix [G.1](#).

We begin by computing the total number of crimes experienced by Americans living in large metro areas in the pre-pandemic period. Summing over each of the eighteen groups, we obtain an annual estimate of 1.34 million outdoor violent crimes. Next, holding victimization risk fixed, we estimate that 923,000 outdoor violent crimes would have accrued on the basis of the observed reduction in time spent in public across the eighteen demographic groups. This number is 31% less than the number of crimes in 2019, reflecting the reduction in time spent in public. Finally, constraining each group to have changed their public time use by 15.8%,

the mean decline in time spent outside across all groups, we obtain an estimate of 970,000 outdoor violent crimes. The difference between the counterfactual condition in (2) and counterfactual condition in (3) is approximately 47,000 crimes, indicating that demographic selection effects would predict a 5% *decrease* in crime. This is consistent with our casual observation in Figure 3 that, if anything, the risk set grew relatively *older* in 2020.

Finally, we assess the presence of demographic selection effects directly using crime microdata from NYC and Los Angeles, where data on the demography of crime victims are available. If our finding of an increase in risk is driven by selection, crime victims in 2020 should, in general, be those who have higher baseline victimization risk than in the past. Evidence from this analysis is presented in Appendix G.2 and suggests that changes in the demography of potential crime victims is unlikely to account for a meaningful share of the large increase in victimization risk that we observe in the data. Consistent with the above analysis, if anything, 2020 crime victims had lower, not higher, historical rates of victimization.

### 5.2.2 Test of geographic selection

Given wide variation in crime rates across different communities, another potential source of selection is geographic. If time spent outdoors changed more in some communities than in others—for example, if individuals living in higher poverty communities were less able to shelter at home—changes in risk could be an artifact of between-community differences in victimization risk. We assess whether the estimates presented in Figure 2 attenuate when we focus on the change in victimization *within a given community*. This analysis is especially salient as Census block group fixed effects explain nearly 80% of the variation in crime risk among our three cities.

We study within-community changes in the following specification:

$$Risk_{cmy} = \sum_{k \in \{1, \dots, 12\}} \beta_k 1\{m = k, y = 2020\} + \alpha_{cm} + \epsilon_{cmy} \quad (5)$$

where  $Risk_{cmy}$  gives risk per 100,000 visitors in census block group  $c$  during month  $m$  in year  $y$ .  $\alpha_{cm}$  denote Census block group by month fixed effects, so that the coefficients  $\beta_k$  track the within-area change in risk compared to the same month in 2019. Observations are

weighted by the number of visitors, and standard errors are clustered at the Census block group level.

We plot coefficients  $\beta_k$  and the associated 95% confidence intervals in [Figure 4](#). The pattern in the coefficients is strikingly similar to those reported in [Figure 2](#), indicating that victimization risk rose most steeply in April and May 2020 but remained elevated throughout the remainder of the year. The increase in risk that we observe is therefore not driven by a shift in the geographic composition of the population at risk.

## 6 Discussion

Beginning in March 2020, the number of street crimes in the three largest cities in the US declined by 35% as people adapted to disease risk and mandated lockdowns by spending more time at home. In contrast to analyses of traditional crime data ([Abrams, 2021](#)), we find that, in 2020, the risk of outdoor street crimes initially *rose* by more than 40% and was between 10-15% *higher* than it had been in 2019 through the remainder of the year. These differences are unlikely to be a mechanical artifact of changes in crime reporting or differential selection into outdoor activity during the COVID-19 pandemic.

Do these results extend beyond our three cities? We merge data on twenty-five large U.S. cities collected by [Abrams \(2021\)](#) with Google mobility data for those cities and plot the change in violent crimes between 2019 and 2020 against the change in mobility from Google’s baseline period (Jan 3rd–Feb 6th, 2020) to the post-pandemic period. We present those data in panel A of [Figure 5](#). Consistent with our main analysis, almost all cities lie above the 45-degree line, implying that the drop in activity was larger than the drop in crime. This suggests that risk, overall, increased. Next, in panel B we present an analysis of national survey data from the NCVS and the ATUS. Summing over both samples, we compare the change in public violent victimization reported in the NCVS to the change in time spent in public spaces reported in the ATUS. This analysis suggests that public victimization risk rose by approximately 25% in 2020—from approximately 9 to approximately 12 victimizations per million outdoor hours. Both analyses suggest that the increase in crime risk is not unique to the three largest cities in the US.

More generally, the data and methods we employ represent a path forward in going

beyond crime rates to measure crime *risk*. By comparing public victimization to mobility, we can better understand how public safety varies among neighborhoods within a given city, by time of day, or by mode of transportation. Better measurement can lead to improvements in urban planning—for example, more cost-effective deployment of police officers and social services and the enhanced targeting of investments in public infrastructure such as street lighting, garbage removal and the remediation of blighted properties.

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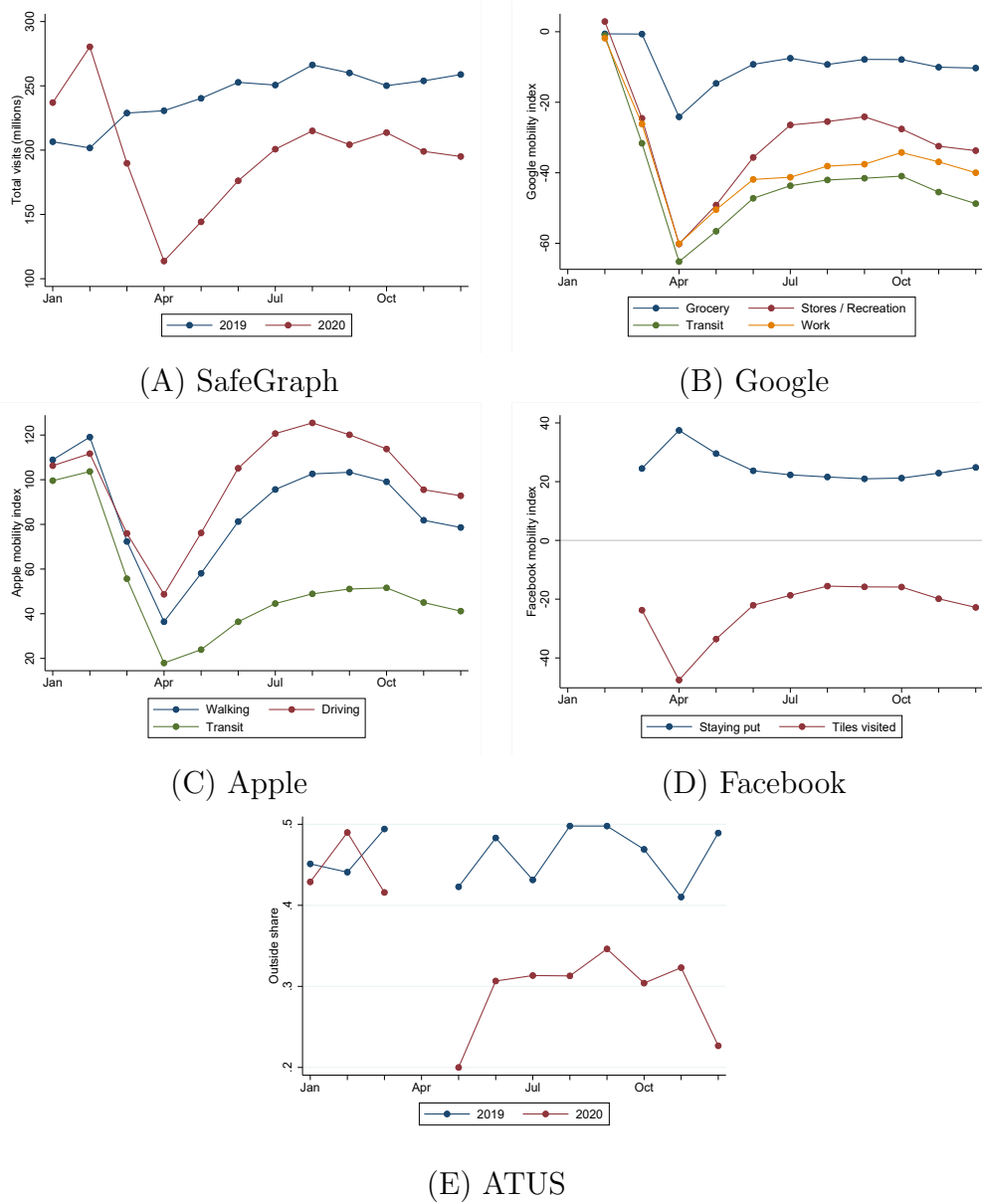
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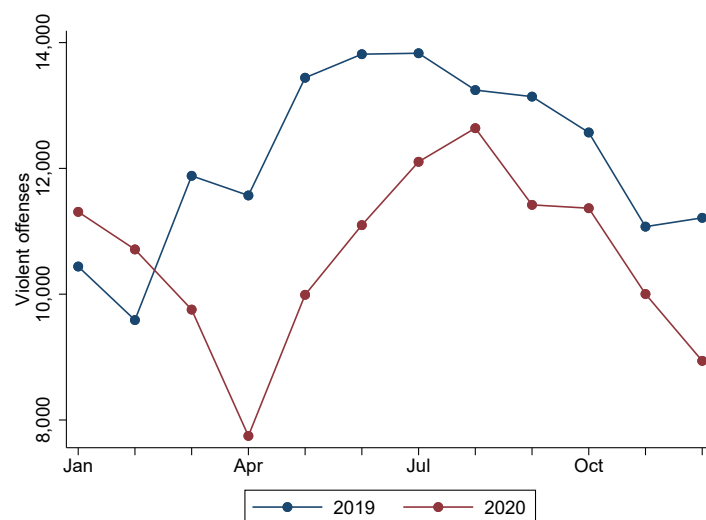
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Figure 1: Changes in mobility — NYC, Los Angeles and Chicago (2019-2020)

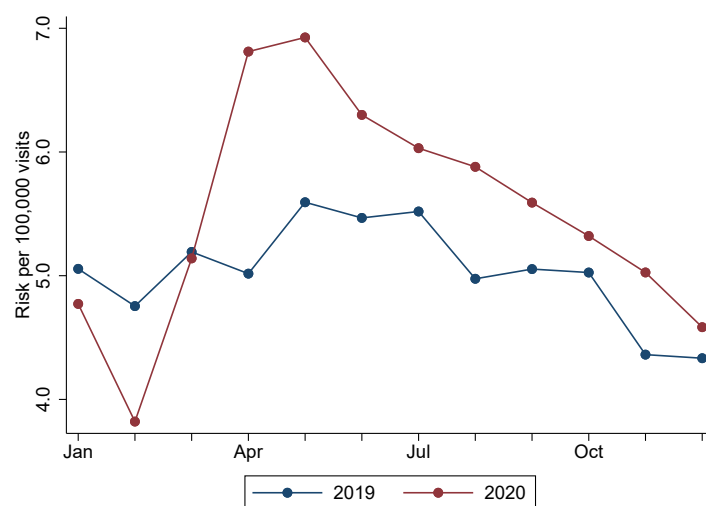


Note: Figure plots changes in mobility for the combined sample (NYC + Los Angeles + Chicago) during the COVID-19 pandemic using five sources of data. Panel (A) presents monthly counts of neighborhood visitors calculated using the SafeGraph Neighborhood Patterns data. Panel (B) presents county-level mobility indexes using Google community mobility reports for the five counties of NYC (Bronx, Kings, New York, Queens, Richmond), Cook County, Illinois (Chicago) and Los Angeles County, CA. Changes in time use are estimated separately for grocery shopping, transit, recreation and work. Panel (C) presents population-weighted average for mobility indexes from Apple at the city level. Changes in time use are estimated separately for walking, dining and transit. Panel (D) presents mobility data from Facebook. Panel (E) presents the average time spent outside using the American Time Use Survey (ATUS), restricting the sample to respondents living in metropolitan areas. Safe-graph and ATUS data are available for both 2019 and 2020; other data sources are available only for 2020.

Figure 2: Change in Public Violence and Victimization Risk — New York, Los Angeles and Chicago (2019-2020)



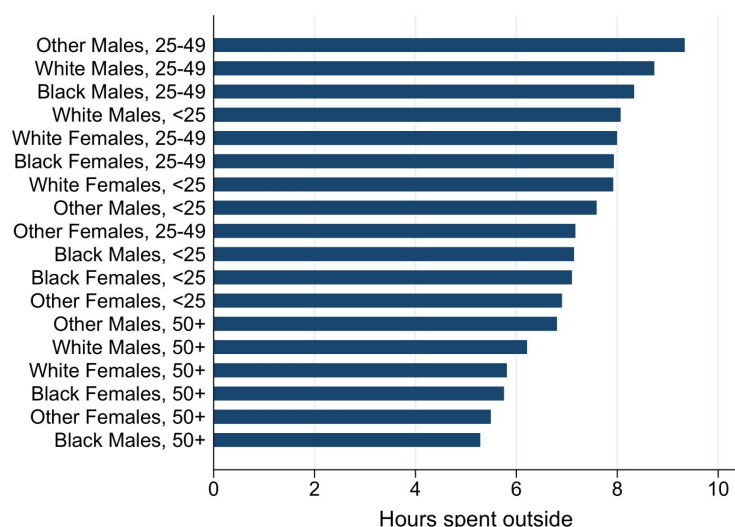
(A) Violent crimes



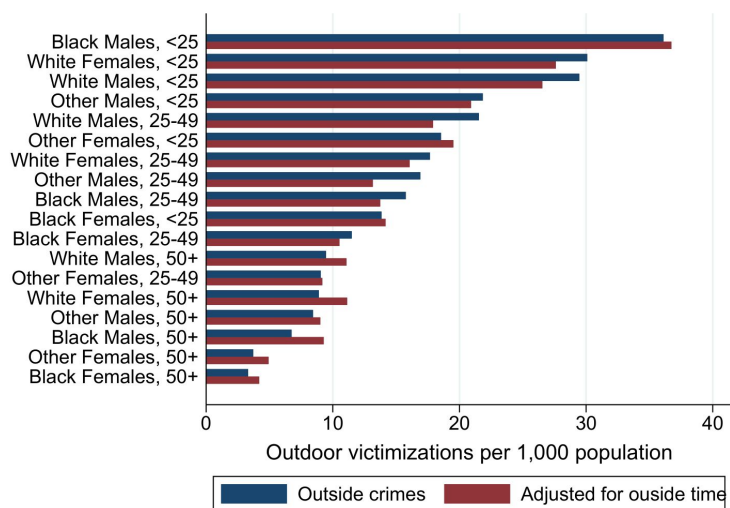
(B) Risk, SafeGraph

Note: Figure plots the monthly number of non-residential (public) violent crimes (Panel A) and the number of non-residential (public) violent crimes per 100,000 visitors based on mobility data from Safegraph (Panel B) for the combined study sample (NYC + Los Angeles + Chicago). In each plot, data are presented separately for 2019 (the blue line) and 2020 (the red line).

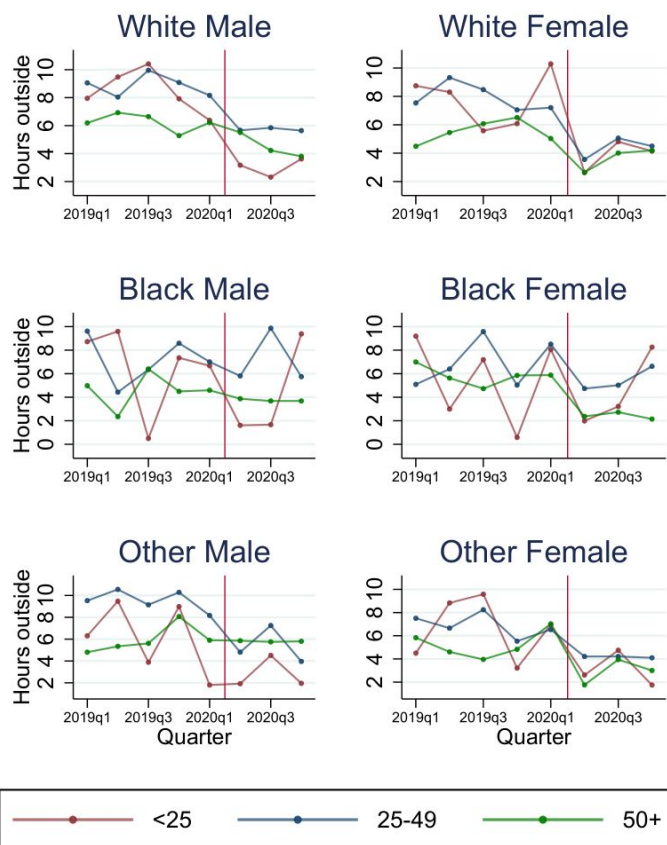
Figure 3: Victimization per 1,000 Population for Selected Demographic Groups, 2015-2019  
National Crime Victimization Survey



(A) Hours spent outside home, 2015-2019 ATUS



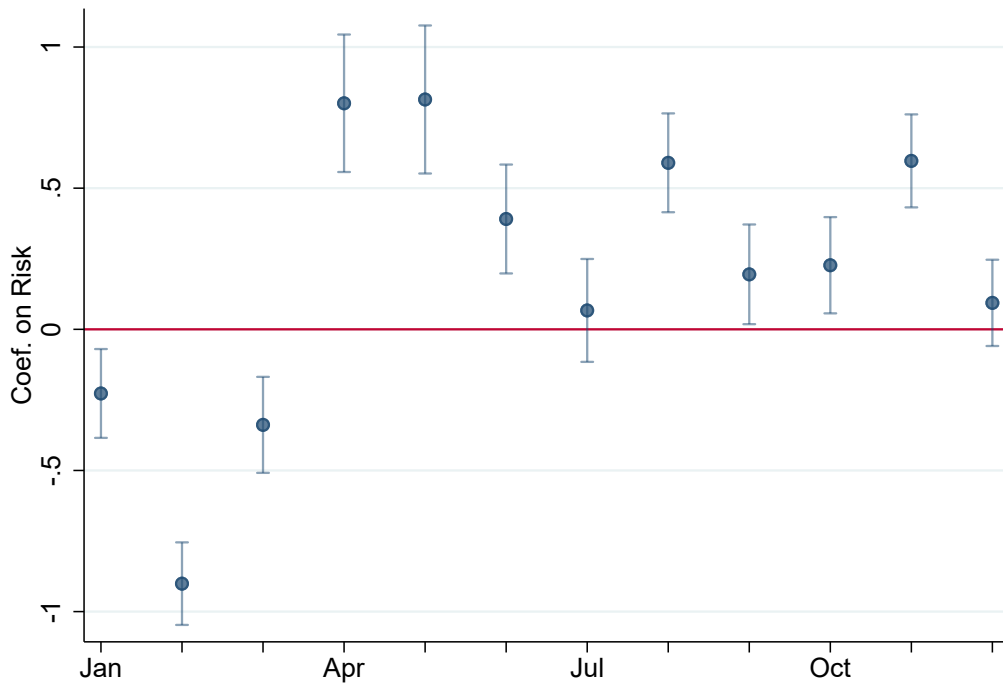
(C) Victimization rates, 2015-2019 NCVS+ATUS



(B) Change in hours outside, ATUS

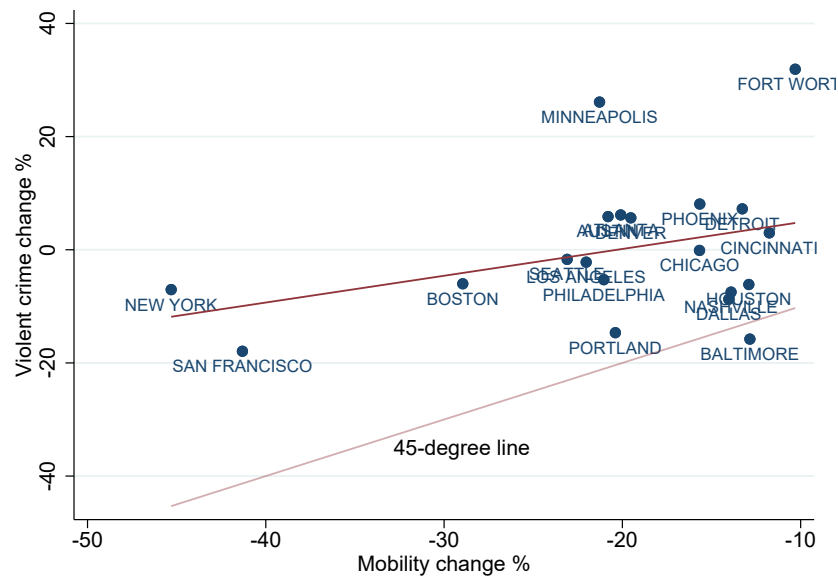
Note: Figure provides descriptive evidence on time use and victimization rates prior to the COVID-19 pandemic. Panel (A) summarizes the percentage of time spent away from one's home or yard using the 2015-2019 American Time Use Survey (ATUS) for each of eighteen age-gender-race groups. Panel (B) summarizes non-residential (public) violent victimizations per 1,000 population using the 2015-2019 waves of the National Crime Victimization Survey (NCVS). The blue bars are the unadjusted rates. The purple bars are rates of public violent victimizations per person-hour spent in public.

Figure 4: Victimization Risk, Conditional on Census Block Group Fixed Effects

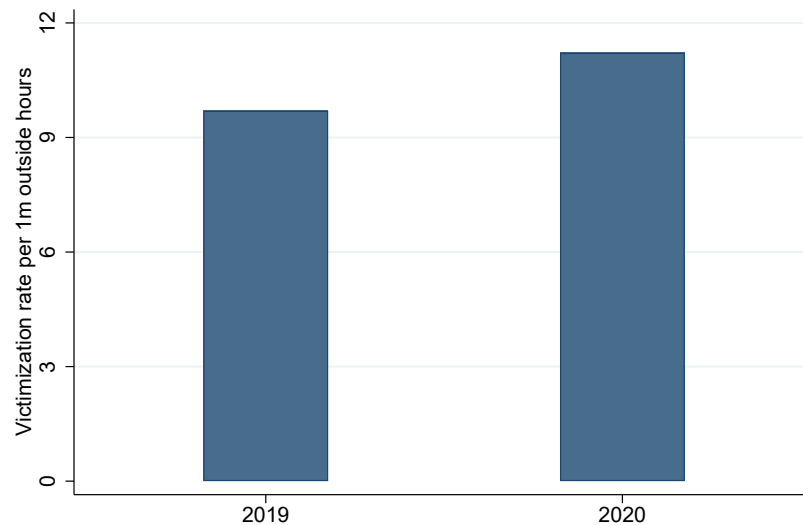


Note: Figure plots coefficients on month-of-year dummy variables from a regression of victimization risk on month, year and Census block group fixed effects. The horizontal line at  $y=0$  indicates no change in victimization risk between 2019 and 2020, conditional on Census block group fixed effects. The 95% confidence interval is plotted using standard errors clustered at the Census block group level.

Figure 5: National Estimates



(a) Percent Change in Crime vs. Percent Change in Mobility in 20 US cities



(b) Risk of violent crime outdoors, ATUS and NCVS

Note: Panel (a) is a scatter plot of the percentage change in assaults against the percentage change in mobility. Each dot is a city. Assaults data come from [Abrams \(2021\)](#), using all cities where data for 2020 was available. Change in mobility is measured using county-level Google data as the simple average of the retail and grocery indexes. Panel (b) shows the outdoor victimization risk calculated using the ATUS (for total hours outside) and NCVS (for total outdoor violent victimizations), restricting to respondents in metropolitan areas.

# Online Appendix

## A History of Measuring Activity-Adjusted Crime Risk

Researchers typically track changes in public safety using the crime rate: the number of crimes known to law enforcement divided by an area’s population (Nolan III, 2004). Crime rates are used to compare criminal activity across cities and to understand how public safety has changed over time. The advantage of the crime rate is that it is transparent and can be straightforwardly computed for individual cities as well as nationally using publicly-available data.

The crime rate is a convenient albeit imperfect heuristic for public safety, a core concept but one that can be difficult to define and even harder to measure. While public safety can refer to a number of different ideas, a common conception employed in research and the policy world involves the risk of victimization for a typical citizen (Boggs, 1965; Stipak, 1988; Vaughan et al., 2020; Ramos, 2021). Victimization risk motivates the central statistics — crime incidence and prevalence — that are released by the U.S. Bureau of Justice Statistics in their annual distillation of the U.S. National Crime Victimization Survey, the sole national victimization survey in the United States. Victimization risk is likewise a key ingredient in how members of the public think about safety, especially when it comes to the risk of becoming the victim of a street crime (Ferraro, 1995; Pickett et al., 2012).

Using the crime rate as a proxy for public safety has led to a litany of critiques in the criminology literature. The most common criticism of the crime rate is that crimes that become known to law enforcement — the only city-level measure of victimization that is consistently available in the United States — represent only a subset of criminal activity. In particular, researchers have worried about the “dark figure of crime,” the number of crimes which are unreported to and undetected by state or local police agencies (Biderman and Reiss Jr, 1967; Penney, 2014). In the presence of victim underreporting and incomplete crime detection by police, the crime rate will underestimate the true risk of becoming a crime victim, a problem which is thought to be particularly large for stigmatized crimes like domestic violence and sexual assault (Felson and Paré, 2005), crimes with lower social costs and relatively low clearance rates like theft (Skogan, 1977) and crimes against juveniles who may be particularly apprehensive about interacting with the police (Finkelhor and Ormrod, 2001).<sup>14</sup> While the difficulty of accurately recording crime represents an important challenge to using the crime rate, this challenge is far from insurmountable. Using national survey data, it is possible to estimate the magnitude of the dark figure of crime, including hard-to-measure crimes like domestic violence (Bachman, 1994) or crimes against juveniles (Hashima and Finkelhor, 1999). Likewise, by focusing on crimes which tend to be consistently recorded — for example, murder and motor vehicle theft — measurement artifacts can be minimized.<sup>15</sup>

In this paper, we focus on a deeper and more challenging issue in the measurement public safety — a problem that has been noted by researchers for many years but which, due to severe data constraints remain unresolved. While measuring the crime rate’s numerator has

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<sup>14</sup>In the United States, a related problem is that, despite national reporting standards set forth by the Federal Bureau of Investigation, inconsistent recording practices by local law enforcement agencies can distort the validity of between-city crime comparisons.

<sup>15</sup>Other challenges to using the crime rate to understand the risk of victimization include those created by population heterogeneity and differential selection into risky activities.

received the lion’s share of scholarly attention, it is actually the fraction’s denominator that poses the most salient challenge for researchers (Boggs, 1965). That is, assuming that we have an accurate accounting of the number of crimes, against what reference group should that number be compared for measuring public safety? While population is a useful starting point, for several reasons it may be a poor proxy for the number of criminal opportunities.

As has been recognized in previous research, a city’s resident population is an imperfect proxy for the number of individuals who are at risk to become a crime victim (Boggs, 1965; Boydell, 1969; Skogan, 1978; Stipak, 1988; Newton, 2018; Gerell, 2021; Vaughan et al., 2020; Ramos, 2021), particularly the victim of the types of street crimes that capture an outsize amount of fear in the public’s imagination (Skogan, 1986; Ferraro and Grange, 1987). For example, prior to the COVID-19 pandemic, Manhattan Island, home to approximately 1.7 million individuals, swelled to a population of approximately 3.4 million people during a typical workday. Clearly, deflating the number of crimes by 1.7 million leads to a crime rate that is biased upwards, an issue noted by Boggs nearly sixty years ago.<sup>16</sup> At the same time, since people who work in Manhattan likely spend less time there, on average, than Manhattan residents, using 3.4 million would yield an underestimate. Ideally, we would deflate crime by the number of person-hours spent in Manhattan during a given period. However this figure is, for obvious reasons, difficult to estimate.

A related concern is that crime risk is, in large part, a function of its opportunity, an idea that criminologists generally refer to as routine activities theory (Cohen and Felson, 1979; Felson, 1987; Branic, 2015). This theory offers a parsimonious explanation for why juvenile offending peaks after school lets out (Fox and Newman, 1997; Fischer and Argyle, 2018) and why vehicle thefts tend to be counter-cyclical, falling during recessions — there are fewer vehicles to steal from downtown areas (Cook and Zarkin, 1985; Bushway et al., 2012). If a population changes its behavior due to an exogenous shock, the crime rate might change even if the risk of crime for those who are unresponsive to the shock remains constant. This issue will be especially pronounced in times where activity fluctuates; sharp changes in the crime rate could belie less radical changes in risk or vice versa.

Prior literature has proposed several different innovations to better measure the risk of crime. Recognizing that the risk of exposure varies from crime to crime and might be poorly correlated with population, the earliest literature proposed a series of crime-specific denominators including vehicles registered for vehicle thefts (Lottier, 1938; Cohen et al., 1985), female population for rape (Boggs, 1965) and the number of occupied housing units as a denominator for residential burglary (Boggs, 1965; on Crime Prevention et al., 1977). Recognizing the multi-dimensional nature of risk, others have proposed regression adjustment as a way to empirically model crime risk across population as well as other relevant denominators such as vehicles registered (Stipak, 1988).

Subsequent research has focused on time spent at and away from home as creating a salient measure of the risk of exposure. Starting with a seminal contribution by Cohen and Felson (1979) which noted that individuals spend, on average, two-thirds of their time at home, one hour per day outdoors, and the remaining time indoors at a location that is not

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<sup>16</sup>As Boggs noted, “spuriously high crime occurrence rates are computed for central business districts, which contain small numbers of residents but large numbers of such targets as merchandise on display, untended parked cars on lots, people on the streets, money in circulation, and the like.”

their own residence. Using these denominators, the authors calculated that, per unit of time, the risk of assault by a stranger on the street was more than 20 times greater than the risk of assault at home by someone who is known to the victim. While this result may seem perfectly intuitive today, at the time, this research was instrumental in overturning a common belief by scholars that home was the place where crime risk was greatest (Vaughan et al., 2020). A recent innovation proposed by Vaughan et al. (2020) and Lemieux and Felson (2012) is to use national survey data to measure changes in the time that individuals spend outside their homes. By comparing data on victimization from the National Crime Victimization Survey to data on time use from the ATUS, these papers have computed victimization rates for various activities, finding that victimization risk for the 2003-2008 period is approximately 8 per one million hours spent in public. We update and build upon this approach, augmenting national survey data from the ATUS with new data from mobile phone-based geo-location software.

## B Data Details

In this appendix we provide additional detail on the sources of data used in the paper and how our measures of crime and mobility were constructed from those data.

### B.1 Defining Crime Locations

We begin by providing details on how crimes are classified as occurring in either a public or residential space in both the administrative data from NYC, Los Angeles and Chicago as well as the NCVS.

#### B.1.1 Administrative Crime Data

Crimes known to law enforcement were gathered using incident-level administrative crime data released by the NYPD, the Los Angeles Police Department and the Chicago Police Department. Each city provides details about the location of a criminal incident, although the categories differ across cities.

We use these data to determine whether a crime occurred inside a residence or in a public location. In NYC, an incident is classified as residential if it occurred inside a private home, an apartment building or a public housing residence. In Los Angeles, we count as residential any crime in a residence, condominium/townhouse, foster home, dormitory, group home, shelter, mobile home, nursing home, transitional home, or yard. All other crimes are considered to have occurred in a public location. In Chicago, we count as residential any crime in a residence, apartment, basement, Chicago Housing Authority structure, college residence, hotel, residential driveway, or nursing home. All other crimes are considered to have occurred in a public location.

#### B.1.2 National Crime Victimization Survey

Both the 2015-2019 and 2020 waves of the NCVS ask respondents to identify the type of location in which they were victimized. Using the 2015-2019 NCVS, we consider a crime to have occurred in a residential setting if the crime occurred at or near the victim's home or at or near a friend, neighbor or relative's house. Crimes that occurred in a commercial place, a parking lot, a school or another unspecified location are assumed to have occurred in a public location.

The 2020 NCVS elicits more granular information on crime locations than the 2015-2019 NCVS. We map the more granular 2020 location categories on to the less granular 2019 location categories as follows. Crimes that occurred in these locations are considered to be residential crimes: 1) in one's own dwelling, own attached garage or enclosed porch, 2) in a detached building on one's own property, 3) in a vacation/second home, 4) in a hotel or motel room that the respondent was staying in, 5) in one's own yard or driveway, 6) in a hallway, laundry room or storage area in one's own apartment building or 7) in a yard belonging to one's own apartment building. All other crimes are assumed to have occurred in a public location.

## B.2 Safegraph data

We use the SafeGraph Neighborhood Patterns data ([SafeGraph, 2021](#)) to calculate a count of people in each Census Block Group (CBG) every month. We build this off of the “device home areas” column, which gives a count of visitors by the visitors’ home CBG. The home CBG is needed for weighting: we scale the number of visits from each CBG by the inverse of the sampling probability in that CBG, following [Huntington-Klein \(2020\)](#). CBG populations are downloaded from SafeGraph’s Open Census data, and the counts of devices in each CBG comes from SafeGraph’s monthly Neighborhood Home Panel Summary files. The population of each CBG from Census, along with SafeGraph’s reported device sample within each origin CBG, allows us to calculate the sampling probability each month.

## C Robustness

In this section we provide alternative results using different methods to calculate victimization risk. First, we use survey data from the ATUS rather than Safegraph mobility data to activity-adjust the number of crimes. Next, we use a narrower definition of public spaces, excluding crimes committed indoors in public locations.

### C.1 Alternative Measure of Victimization Risk

Our primary estimate of the risk of public violence is computed using Safegraph data which is the only source of mobility data which is publicly available prior to 2020. In this appendix, we re-compute our measure of victimization risk drawing on survey data from the American Time Use Survey (ATUS). To protect respondent anonymity, the survey data do not contain sufficient geographic detail to identify respondents living in NYC, Los Angeles and Chicago. We therefore focus on respondents who were living in metropolitan areas in 2019 and 2020. [Figure C.1](#) is identical to Panel B of [Figure 2](#) except that ATUS data is substituted for Safegraph data in computing victimization risk. The  $y$ -axis represents the risk per 1 million hours spent outdoors.

As the ATUS was temporarily suspended from mid-March until mid-May 2020 due to public health concerns, we are not able to observe the change in victimization risk that occurred just after the beginning of the pandemic.<sup>17</sup> However, for the remainder of the year, estimates of victimization risk using the Safegraph data and the ATUS survey data are substantively the same: that the risk of street crime victimization increased markedly in Spring 2020 and remained elevated — by approximately 10% — during the remainder of the year.

### C.2 Alternative Measure of Public Locations

We begin by plotting the change in the incidence and risk of public violence using an alternative definition of what it means for the crime to have occurred in public. [Figure C.2](#) is analogous to [Figure 2](#) except it uses a more limited definition of public violence. In this figure, we count only crimes that occurred on streets, alleyways, public parks, or transit system. Estimated changes in risk are extraordinarily similar to the main estimates reported in the paper.

### C.3 Crime Reporting

To the extent that victims became more reluctant to report crimes to law enforcement due to public health risks, a legitimacy crisis in policing (Tyler, 2004; Tankebe, 2014; Wolfe et al., 2016) sparked by the killing of George Floyd (Nix, 2021) or the perception that police and prosecutors were otherwise occupied, declining crime might be a mechanical artifact of a change in reporting behavior (Van Dijk, 1979; Levitt, 1998; Davis and Henderson, 2003). To address this concern, we appeal to newly released data from the 2020 National

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<sup>17</sup>See: <https://www.bls.gov/tus/covid19.htm>.

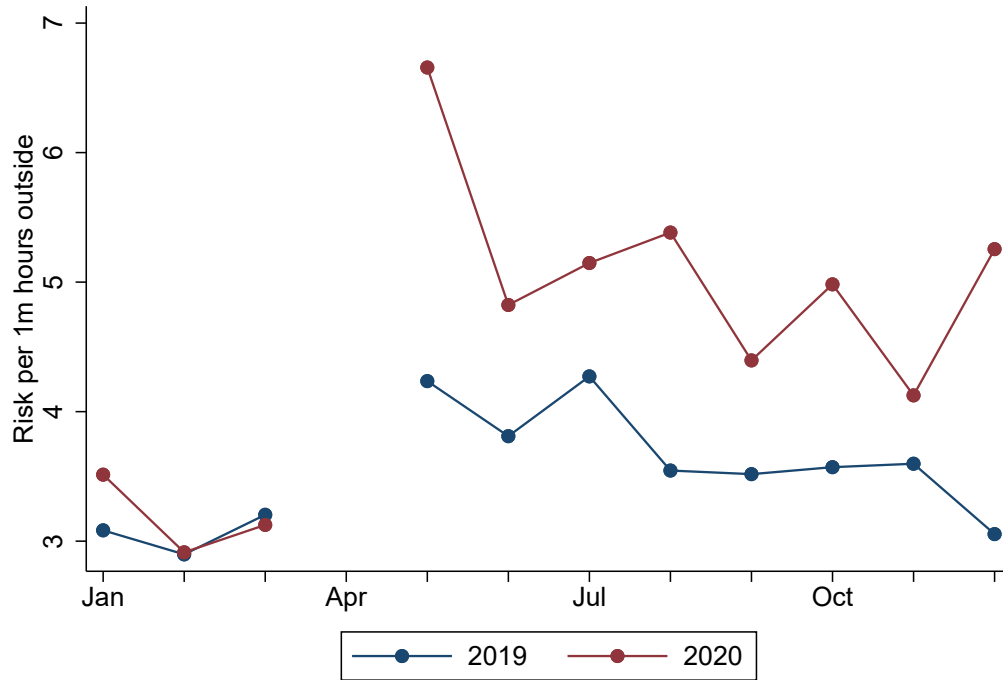
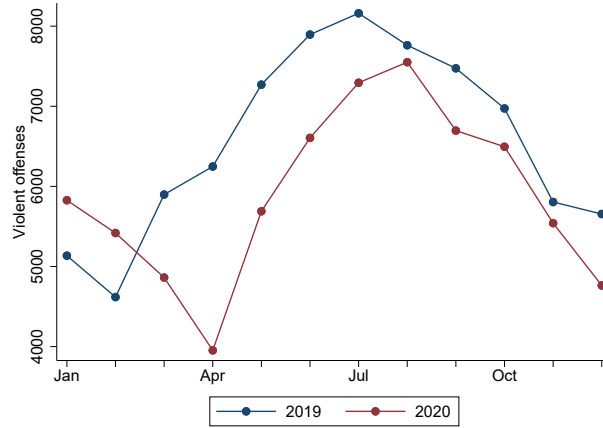


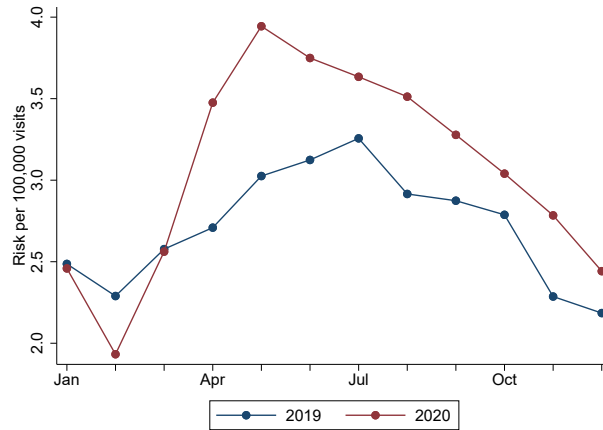
Figure C.1: Risk in New York, Los Angeles, and Chicago using ATUS

Note: Figure plots the number of non-residential (public) violent crimes per 1 million person-hours outside based on survey data from the American Time Use Survey (ATUS) and crime for the combined study sample (NYC + Los Angeles + Chicago). Data are presented separately for 2019 (the blue line) and 2020 (the red line). The ATUS was not administered in April 2020.

Figure C.2: Change in Public Violence and Victimization Risk using Alternative Definition of Public Crimes — New York, Los Angeles and Chicago (2019-2020)



(A) Violent crimes

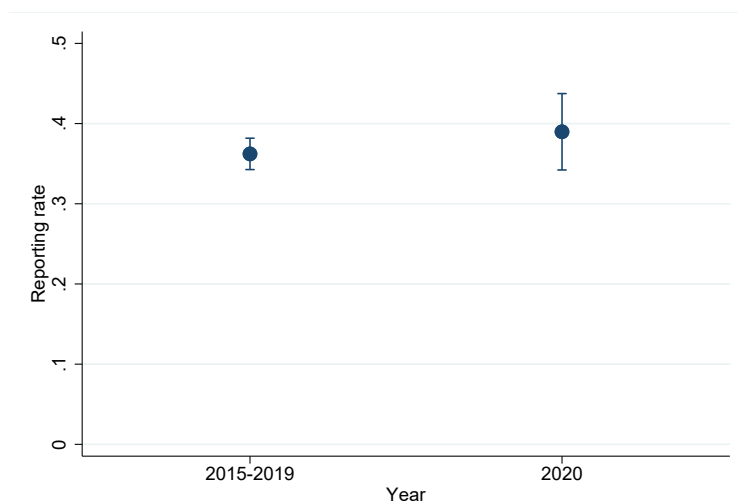


(B) Risk, SafeGraph

Note: This plot is analogous to [Figure 2](#) except it uses a more limited definition of public violence. In each city, we count only crimes that occurred on streets, alleyways, public parks, or transit system.

Crime Victimization Survey in which a nationally representative sample of respondents are asked about crime victimization as well as whether they reported crimes to law enforcement. Appendix Figure [C.3](#) presents the share of outdoor violent crimes that victims reported crimes to police among respondents interviewed in the 2015-2019 and 2020 waves of the survey. To maintain consistency with our administrative data from large cities, we focus on survey respondents living in metropolitan areas. Contrary to concerns that crime reporting may have fallen during the pandemic, the rate at which outdoor person crimes were reported to the police did not change appreciably in 2020.

Figure C.3: Share of Outdoor Violent Crimes Reported to Police — 2015-2019 and 2020 National Crime Victimization Surveys



Note: Figure plots the percentage of non-residential (public) violent crimes reported to law enforcement in the 2015-2019 and 2020 waves of the National Crime Victimization Survey (NCVS). For each survey wave, the point estimate is provided along with a 95% confidence interval.

## D Crime on public transit

In this appendix section, we consider an alternative setting in which victimization risk can be studied. We focus on a specific application for which there is a well-measured and transparent denominator by which crimes can be divided to obtain a measure of victimization risk: crimes on public transit (Pearlstein and Wachs, 1982; Newton, 2008; Newton et al., 2014; Estévez-Soto, 2021).

Crimes that occur on public transit systems—subways, trolleys and buses—are especially well-suited to risk calculations because the population at risk of victimization is easily defined and is, in general, well-recorded.<sup>18</sup> We collect data from NYC and San Francisco, two pedestrian-oriented cities in which public transit is the primary means of traversing the city.<sup>19</sup> San Francisco data on passenger boardings and crimes occurring on transit vehicles and transit stations come from the San Francisco Metropolitan Transit Authority (SFMTA) website. This includes ridership on San Francisco Municipal Transit Agency (MUNI) trolleys and buses but does not include the Bay Area Rapid Transit (BART) regional rail system.<sup>20</sup>

We study the number of passenger boardings and the number of violent crimes that occurred in each city’s transit system to measure how the risk of victimization changed in 2020 relative to 2019. Figure D.1 presents quarterly data on crime and ridership for the 2019-2020 period for public transit systems in NYC and San Francisco. For each city, panel (A) plots the quarterly number of boardings (in millions) separately for 2019 and 2020. In both cities, boardings exhibited little seasonal variation in 2019 and were constant at approximately 600 million boardings per quarter in NYC and 130 million boardings per quarter in San Francisco. In contrast, there is a sharp decline in passenger boardings in the second quarter of 2020. From 2019Q2 to 2020Q2, boardings fell by 80% in NYC and by 70% in San Francisco. While boardings began to pick up after the second quarter of 2020, they remained more than 50 percent lower throughout the remainder of the year in both cities. We plot the number of violent crimes on public transit in Panel (B). In 2019 and the first quarter of 2020, there were between 45 and 60 transit crimes in San Francisco per quarter. In 2020Q2, this fell to just 14, a 75% drop. Transit crimes increased thereafter to 40 in 2020Q3 and 48 in 2020Q4. By 2020Q4, the difference in crimes between 2019 and 2020 had fallen to just 10 percent. The story is similar in NYC where transit crimes initially fell by

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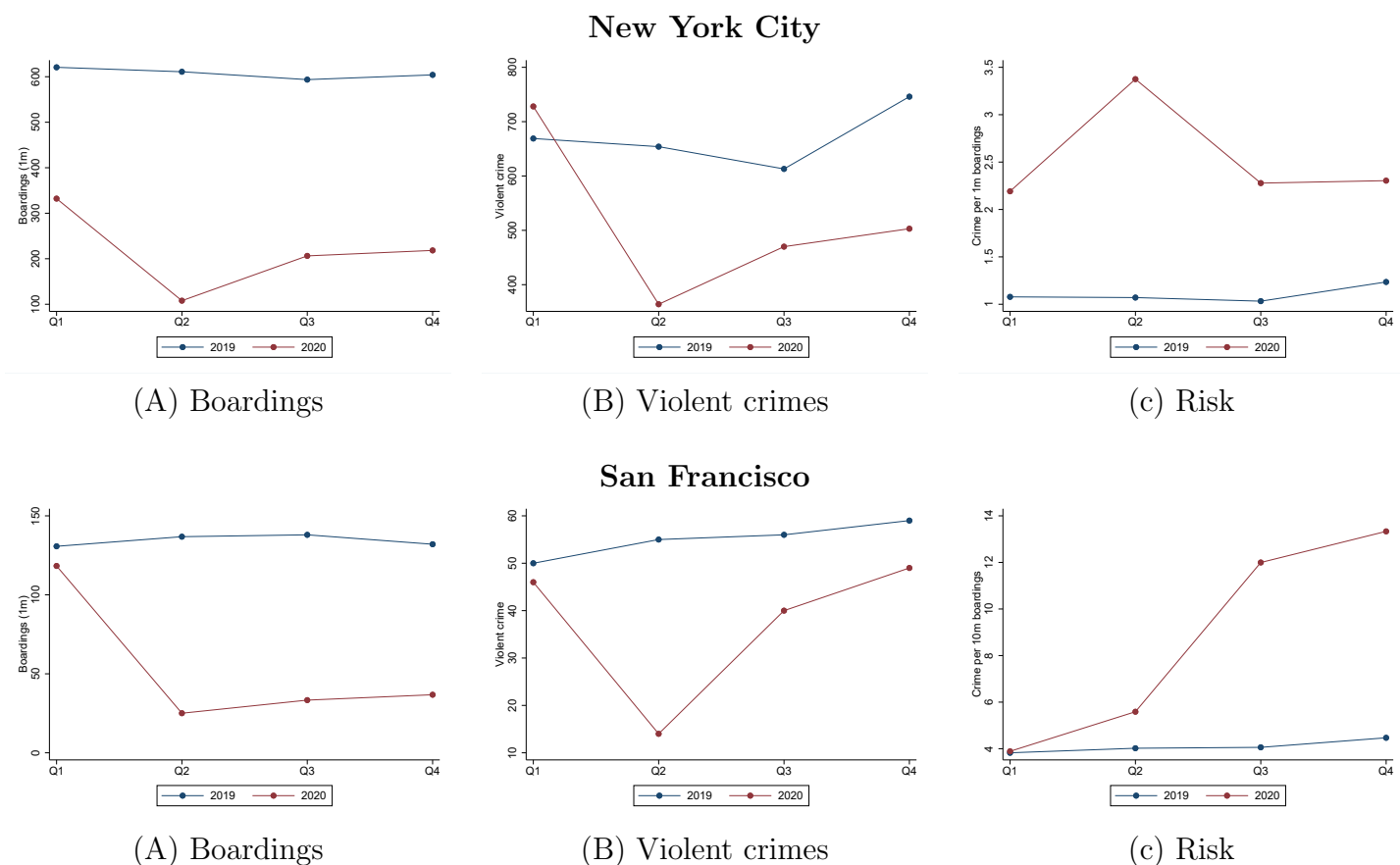
<sup>18</sup>Of course, ridership is not perfectly captured in administrative data. In particular, fare evasion is a persistent and possibly growing problem in American cities. In NYC, one estimate is that fare-beaters accounted for 6% of subway entries in January and February of 2020 and 13% by September 2020 (Meyer, 2020). An implication of these figures is that the estimated change in victimization risk during the pandemic could be biased upward. However, as we show, the change in fare-beating is very small compared to the change in risk and therefore cannot account for anything other than a very small percentage of the year-over-year change.

<sup>19</sup>According to the 2015 American Community Survey, 56% of NYC residents and 33% of San Francisco residents use public transit to commute to work compared to an average of 17% among the typical resident of a US city.

<sup>20</sup>Data on public transit crime in San Francisco were accessed from <https://www.sfmta.com/reports/sfpd-reported-muni-related-crimes-100000-miles>. Ridership comes from <https://www.sfmta.com/reports/muni-ridership>. In NYC, transit crimes are delineated separately by the NYPD’s transit bureau.<sup>21</sup>

approximately 50% in the second quarter of 2020 and remained 25% below the prior year by year's end.

Figure D.1: Violent crime and boardings on public transit — NYC and San Francisco (2019-2020)



Note: Figure presents quarterly data on the number of boardings (Panel A), violent crimes (Panel B) and violent crimes per 10 million boardings (Panel C) for two cities with extensive transit systems: NYC and San Francisco. Data are presented separately for 2019 (the blue line) and 2020 (the red line).

In panel (C) we compute the number of crimes per 10 million transit boardings for each city. In 2020Q1, the crime rate in both cities was virtually identical to the rate in 2019Q1, approximately 4 violent crimes per 10 million boardings in San Francisco and 1 violent crime per 10 million boardings in NYC. Despite the fact that the number of violent transit crimes fell by between half and two thirds in 2020Q2, crimes per passenger increased by 50% in San Francisco and 300% in NYC. By the end of 2020, the risk of victimization had more than doubled in NYC and more than tripled in San Francisco even though the number of crimes remained lower than in 2019.<sup>22</sup> While violent transit crime remained rare throughout the

<sup>22</sup>There is some indication that fare-beating increased during the pandemic—see Meyer (2020). To the extent that fare evasion increased, relying on the official boarding data would lead to an

pandemic, registering a peak rate of between 3 and 13 per 10 million boardings depending on the city, the official crime statistics appear to present a substantively misleading view of the risk that public transit passengers faced in these two cities in 2020.

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overestimate of the change in risk. However, the estimated change in fare-beating—from 6% to 13%—is far too small to explain very much of the change in victimization risk. Allowing for this change in fare-beating only changes the risk ratio in NYC from 2.1 to 2.0.

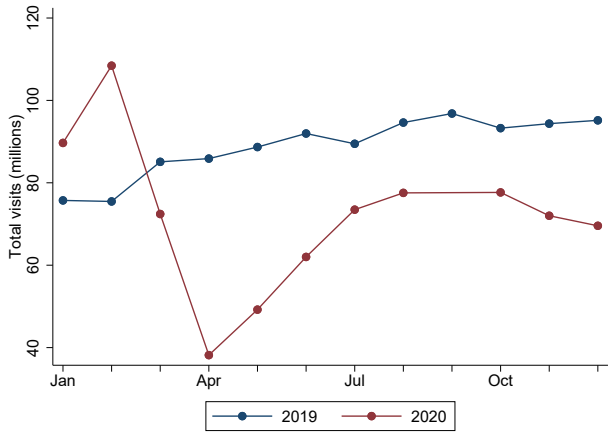
## E City-Specific Results

For brevity, in the main body of the paper, we present aggregated results for NYC, Los Angeles and Chicago as a whole. In this section, we present each set of results — changes in mobility, reported crimes and street crime victimization risk — for each of the three cities individually. While there is some variation in the evolution of crime rates and victimization risk among the three cities, the findings are substantively similar and suggest that some of the dynamics set in motion by the pandemic have been fairly universal.

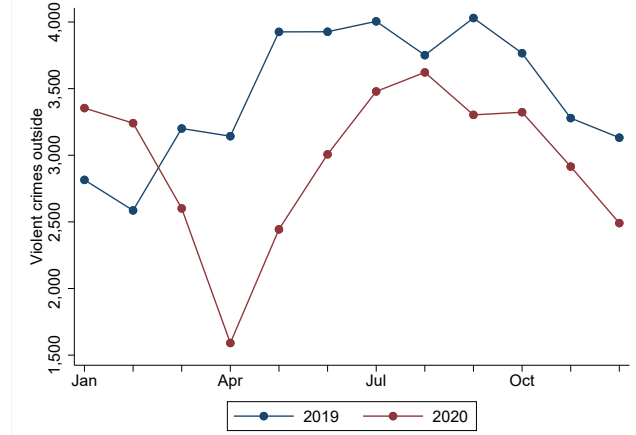
In all three cities, mobility dropped considerably in March and April 2020. While foot traffic began to recover during the summer, each of the three cities ended the year with notably lower foot traffic than during the same months in 2019. Along with mobility, violent street crimes fell dramatically in April 2020 relative to April 2019 — by 50%, 20% and 43% — in NYC, Los Angeles and Chicago, respectively. In all three cities, street crimes began to converge back to pre-pandemic levels over the summer. However, by years' end, street violence remained approximately 10-15% lower than it had been in 2019. Overall, summing across the March through December period, public violence per 1,000 residents declined by 25%, 7% and 24% in NYC, Los Angeles and Chicago, respectively.

With respect to public violence, all three cities experienced an upward shift in risk of victimization in 2020. However, the initial increase was the largest in Los Angeles where street crimes fell less sharply than in NYC or Chicago despite a sizable decline in outdoor activity. In Chicago, the change in risk was quite modest with risk increasing year-over-year by less than 5% in most months. Despite the fact that each of the three cities faced a qualitatively different shift in victimization risk, in all three cities the risk of victimization and the crime rate diverged markedly in 2020. Overall, summing across the March through December period, the risk of public violence increased by 10%, 23% and 3% in NYC, Los Angeles and Chicago, respectively.

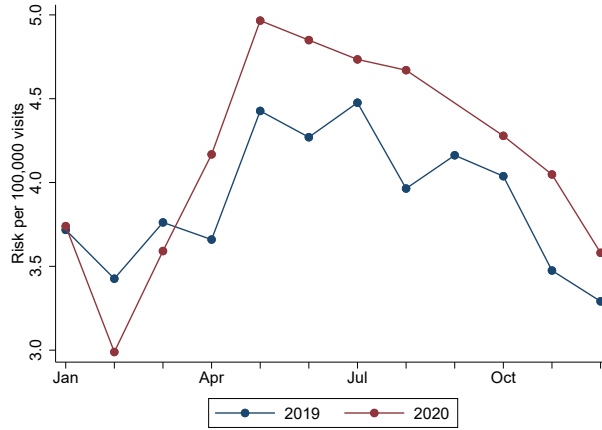
Figure E.1: Violent crime and foot traffic in New York City



(A) SafeGraph foot traffic



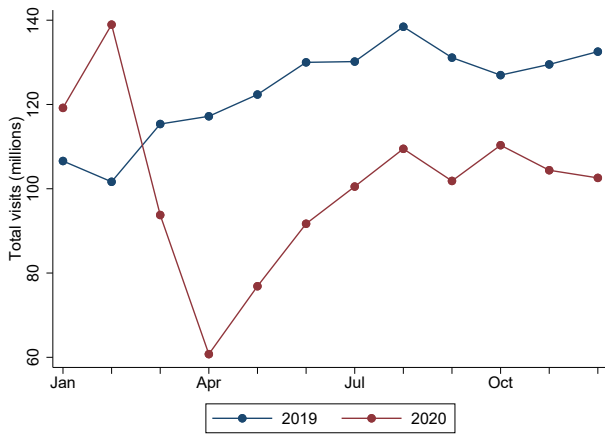
(B) Violent crime



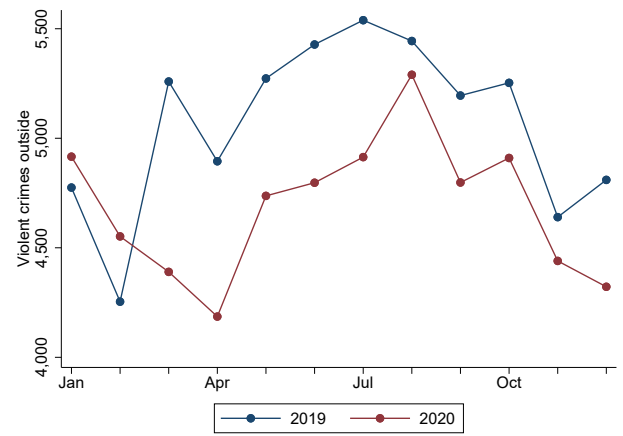
(C) Risk

Note: Figure plots the monthly change in foot traffic using Safegraph data (Panel A), the monthly number of non-residential (public) violent crimes (Panel B) and the monthly number of non-residential (public) violent crimes per 100,000 visitors based on mobility data from Safegraph (Panel C). Data are presented for NYC. In each plot, data are presented separately for 2019 (the blue line) and 2020 (the red line).

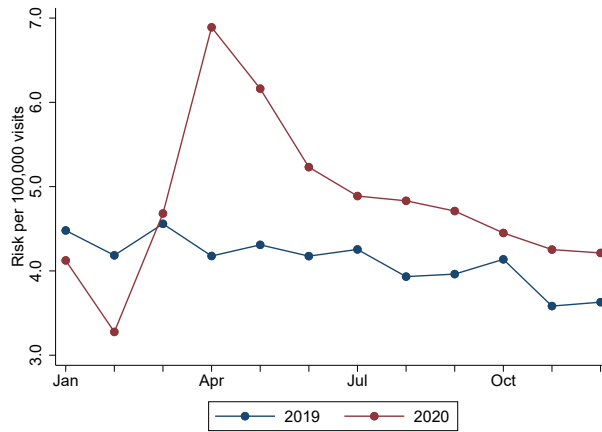
Figure E.2: Violent crime and foot traffic in Los Angeles



(A) SafeGraph foot traffic



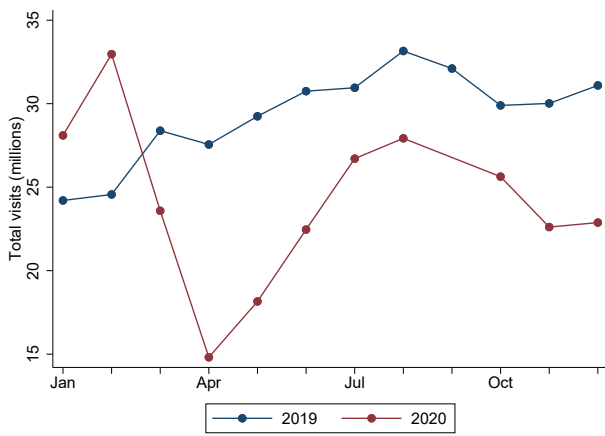
(B) Violent crime



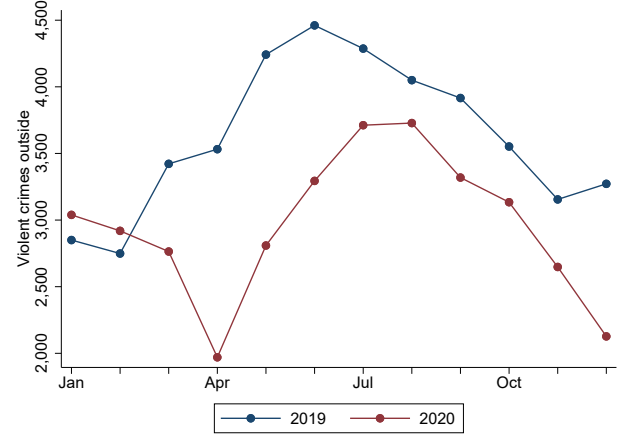
(C) Risk

Note: Figure plots the monthly change in foot traffic using Safegraph data (Panel A), the monthly number of non-residential (public) violent crimes (Panel B) and the monthly number of non-residential (public) violent crimes per 100,000 visitors based on mobility data from Safegraph (Panel C). Data are presented for Los Angeles. In each plot, data are presented separately for 2019 (the blue line) and 2020 (the red line).

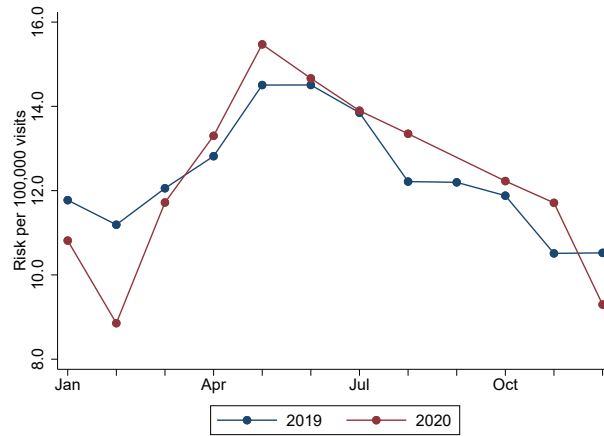
Figure E.3: Violent crime and foot traffic in Chicago



(A) SafeGraph foot traffic



(B) Violent crime



(C) Risk

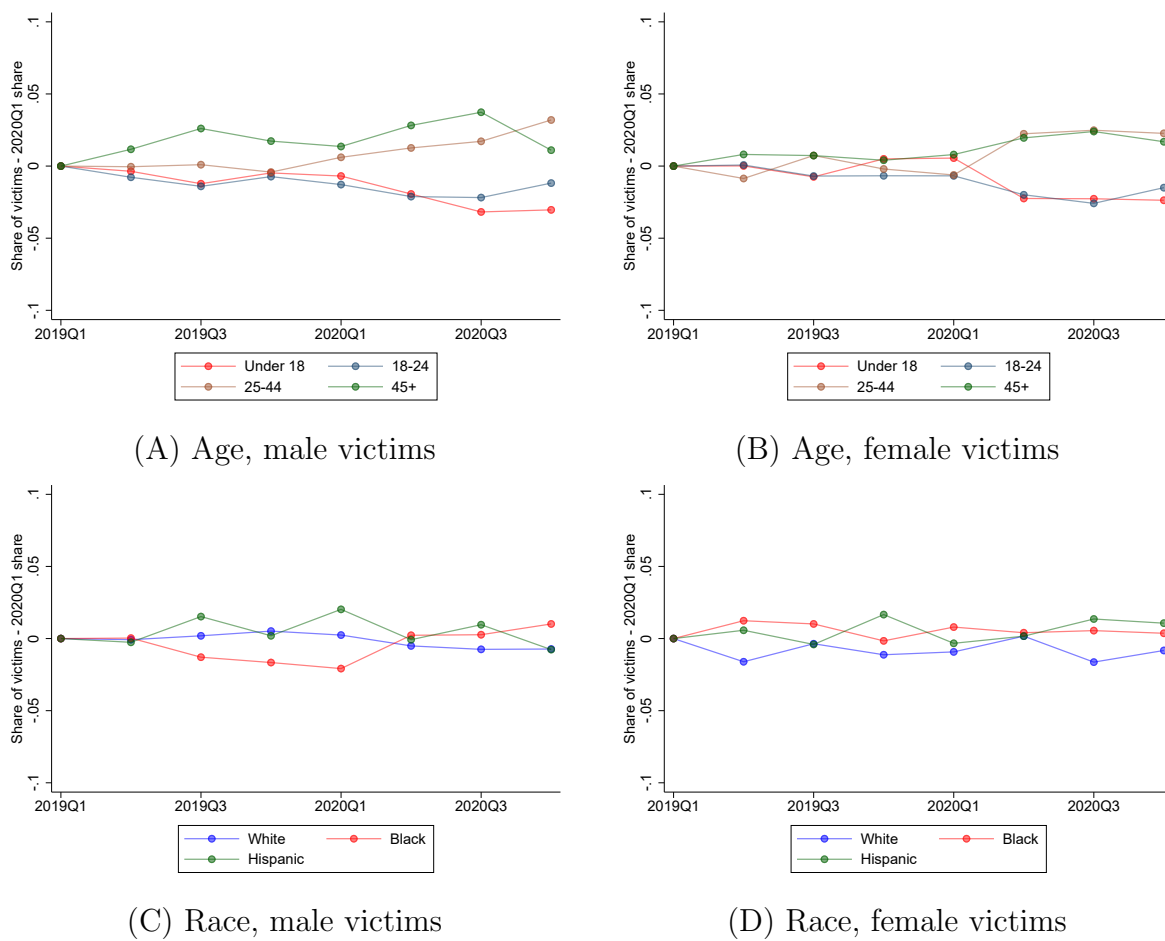
Note: Figure plots the monthly change in foot traffic using Safegraph data (Panel A), the monthly number of non-residential (public) violent crimes (Panel B) and the monthly number of non-residential (public) violent crimes per 100,000 visitors based on mobility data from Safegraph (Panel C). Data are presented for Chicago. In each plot, data are presented separately for 2019 (the blue line) and 2020 (the red line).

## F Additional Results

In this appendix we provide additional results on the demographic composition of crime victims in NYC and Los Angeles prior to and during the COVID-19 pandemic. We also provide descriptive evidence on the time-path of disturbances to public health as the timing of the impact of the COVID-19 pandemic differed among the three cities in our sample.

Figure F.1 shows additional details on victim demographics, drawing from microdata in NYC and Los Angeles which include information on the age, race and gender of crime victims. Across the two cities, changes in the age distribution of victims in 2020 were modest. There was a small decrease in the relative shares of victims under the age of 18 or between the ages of 18 and 24. As these are the age groups that are most likely to be victimized at baseline, if anything, it appears as though changes in the demography of victims point to reduced risk of victimization rather than an increase in victimization risk.

Figure F.1: Victim demographics, New York and Los Angeles

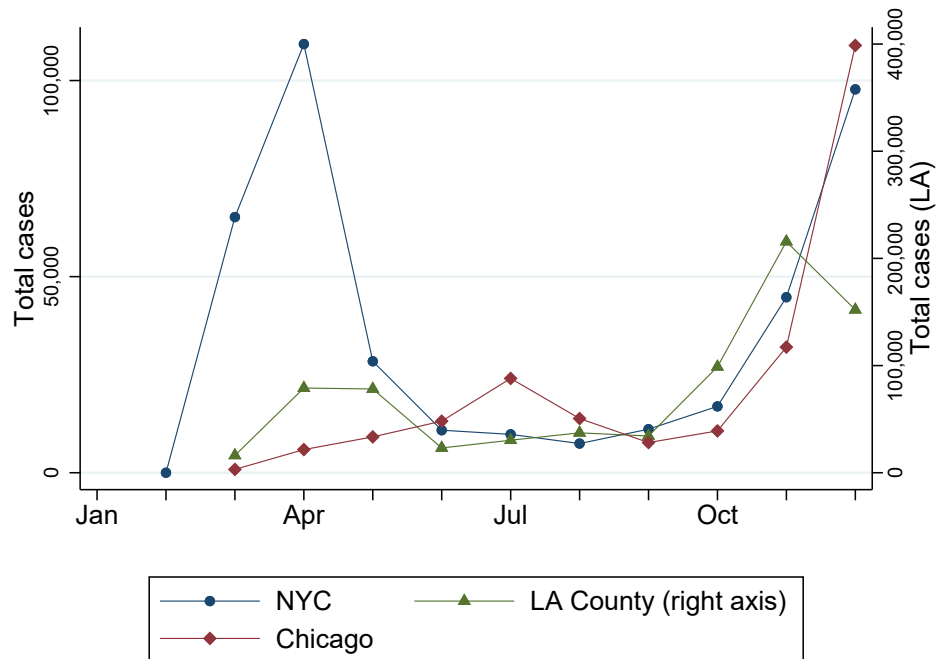


Note: These plots show how the composition of violent crime victims changed in New York City and Los Angeles, the two cities where victim demographics are available.

Next, we consider the time-path of the intensity of the COVID-19 pandemic in each

of our three cities, plotting COVID-19 total cases per month in [Figure F.2](#). While NYC's case loads peaked early in April, in Los Angeles and Chicago, the peak came at the end of 2020. Despite the fact that the impact of the pandemic on public health was very different in each of our cities, patterns in mobility and crime risk are more similar. As such, the evidence suggests that changes in crime risk do not seem to be driven to a large degree by the pandemic's health impacts.

Figure F.2: Monthly COVID-19 Cases, 2020



Note: Figure presents total monthly COVID-19 caseloads by city.

## G Testing for Selection Effects

In this section we provide computational details for the two tests of selection introduced in Section 5.2.1.

### G.1 Test based on time use data

We motivate a formal model to account for the impact of compositional changes on victimization risk. We begin by noting that the annual number of outdoor crimes experienced by Americans in 2019 can be expressed as:

$$C_{2019} = \sum_{j \in J} n_{j,2019} \times ShareOutside_{j,2019} \times v_{j,2019} \quad (6)$$

where  $n_{j,2019}$  is the number of people in group  $j$  in 2019,  $ShareOutside_{j,2019}$  is the share of time individuals in group  $j$  spent outside in 2019 and  $v_{j,2019}$  is the group's outdoor victimization rate which we define as the number of outdoor crimes divided by the number of person-hours spent at risk:

$$v_j = \frac{OutdoorCrimes_{j,2019}}{n_{j,2019} \times ShareOutside_{j,2019}}$$

Next we employ this notation to compute the two counterfactual quantities that we use to identify the importance of compositional effects. Let  $\Delta_j$  be the change in the proportion of outdoor time, where  $\Delta_j = ShareOutside_{j,2020} - ShareOutside_{j,2019}$ .<sup>23</sup> The number of crimes that would have been experienced by Americans, holding victimization rates constant but allowing behavioral responses to the pandemic (i.e., the change in the amount of time spent in public spaces) to vary by group is given by  $\hat{C}_{2020}$ :

$$\hat{C}_{2020} = \sum n_{j,2020} \times (ShareOutside_{j,2019} + \Delta_j) \times v_{j,2019} \quad (7)$$

Equations (6) and (7) are identical except that, in (7), each group changes the share of time spent in public spaces in 2020 according to  $\Delta_j$ .  $\hat{C}_{2020}$  is the predicted number of outdoor violent crimes in metropolitan areas of the United States, holding hourly victimization risk constant.

Next, we consider the counterfactual victimization rate under the constraint that all groups have an identical response to the pandemic (i.e., all groups reduce their time spent outdoors by the same quantity), substituting  $\bar{\Delta}$ , the population-weighted average across all  $J$  groups, for  $\Delta_j$ :

$$C_{2020}^* = \sum n_{j,2020} \times (ShareOutside_{j,2019} + \bar{\Delta}) \times v_{j,2019} \quad (8)$$

The number of outdoor violent crimes under this “alternative pandemic”,  $C_{2020}^*$ , is generated by constraining each group to have an identical response with respect to time use. The

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<sup>23</sup>In practice, to reduce noise, we use the 2015-2019 pre-period.

ratio of  $\hat{C}_{2020}$  to  $C_{2020}^*$  provides a measure of the importance of compositional effects—the additional number of crimes per day that we would expect, given constant victimization risk, from a heterogeneous as compared to a homogenous behavioral response to the pandemic.

We use the NCVS to obtain an estimate of  $v_i$  for each of eighteen age-gender race groups (Figure 3) and the ATUS to estimate  $\bar{\Delta}$  as well as  $\Delta_i$  for each demographic subgroup. We begin by computing the total number of crimes experienced by Americans in the pre-pandemic period,  $C_{2019}$ . Taking a weighted sum over each of the groups, we obtain an annual estimate of 1.34 million outdoor violent crimes.<sup>24</sup> Next, we compute  $\hat{C}_{2020}$  and  $C_{2020}^*$ . Using observed time use in 2020 and pre-determined victimization risk, we estimate that  $C_{2020}^{\hat{}} = 923,000$  outdoor violent crimes. This number is 31% smaller than the number of crimes measured in 2019 which is consistent with time spent outdoors having declined. Finally, constraining each group to have changed their public time use by  $\bar{\Delta} = 15.8\%$ , we obtain an estimate of  $C_{2020}^* = 970,000$  outdoor violent crimes. The difference between  $C_{2020}^*$  and  $\hat{C}_{2020}$  is approximately 47,000 crimes, indicating that selection effects, captured using changes in the demography of individuals spending time outdoors, would predict a 5% *decrease* in crime. While the pandemic led to some re-sorting of individuals in outdoor spaces in 2020, on net, compositional changes, based on key observable dimensions of victimization risk, do not predict a large change in offending.

## G.2 Test based on victim demographics

We can also assess the size of potential selection effects directly using crime microdata from NYC and Los Angeles. If our finding of an increase in risk is driven by selection, crime victims in 2020 should, in general, be at a higher risk of victimization than they were in the past. We can probe this possibility using data on victim demographics. As in the main results, we restrict to violent crimes that occurred outdoors.

We match each victim to their expected victimization rate according to their age, race, and gender based on data from the 2015-2019 National Crime Victimization Surveys (NCVS), restricting to outdoor crimes and to respondents living in cities with populations over one million. We use 24 demographic cells in total, representing the interaction of three race/ethnicity groups (White, Black, Hispanic), two genders (male, female), and four age groups (12-18, 18-24, 25-44, 45+).<sup>25</sup> More formally, the NCVS data yields  $v_g$ , a victimization rate for group  $g$ , with  $g \in \{1, \dots, 24\}$  where, e.g.,  $v_1$  could be the victimization rate White females under 18 which is 20 per 1,000 (per year). Next, we assign the victimization rate  $v_g$  to each crime in the NYC and LA data according to victim demographics and calculate the average victimization rate for each quarter  $q$ :

$$\bar{v}_q = \frac{\sum_{g=1}^{24} v_g * N_{g,q}}{N_q} \quad (9)$$

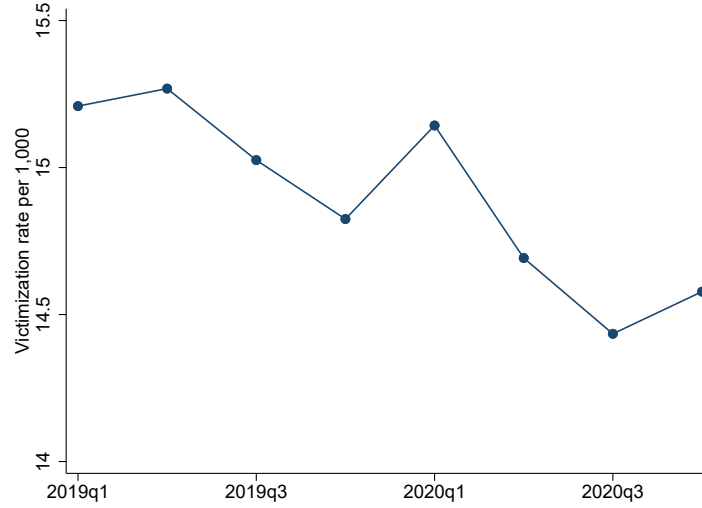
where  $N_{g,q}$  is the total number of victims who belong to group  $g$  in quarter  $q$  and  $N_q$  is the

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<sup>24</sup>Formally  $v_i$  is obtained by dividing the victimization rate for each group by the number of hours spent outside by individuals in each group during the six-month recall period in the NCVS.

<sup>25</sup>The last age group is an imperfect match with the NCVS, which bins all ages 35-49. Victims 45+ in the NYC and LA crime data are matched with NCVS respondents over 50.

Figure G.1: Historical victimization rates of victims, New York City and Los Angeles



Note: Figure presents average historical victimization rates of crime victims from the NYPD and LAPD incident data. The historical victimization rates are calculated using data from the 2015-2019 waves of the NCVS and victim demographics are reported in the incident data. Let  $g$  index demographic groups and  $v_g$  be the victimization rate for group  $g$ . The average historical victimization rate in quarter  $q$  is given by:

$$\bar{v}_q = \frac{\sum_g v_g * N_{g,q}}{N_q}$$

where  $N_{g,q}$  is crimes against group  $g$  in quarter  $q$  and  $N_q$  is total crimes.

total count of crimes in quarter  $q$  where we observe victim demographics. The time series of  $\bar{v}_q$ , the average expected victimization rate, shows us how selection into crime victimization based on risk changes over time.

Using data for the combined two-city sample, we plot the average expected victimization rate of victims over time in [Figure G.1](#). The series drops sharply in the second quarter of 2020 and remains lower, decreasing from 15.1 to 14.7 violent offenses per 1,000 people across years. This suggests that, if anything, crime victims of 2020 were *less likely* to have been victimized than in previous years. This implies that within-group increases in risk were likely larger than in our aggregated estimates. Taken together, the available evidence suggests that compositional changes in the demography of potential crime victims is unlikely to account for a meaningful share of the large increase in victimization risk that we observe in the data.