The Impact of the 2020 CARES Act Stimulus Payments on Crime

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Abstract

The fallout from the COVID-19 pandemic led to a massive economic downturn in 2020. In response, the U.S. federal government passed the Coronavirus Aid, Relief, and Economic Security (CARES) Act that, in part, provided financial resources to millions of households across the country. To study how access to additional financial resources impacts criminal behavior, I exploit the CARES Act stimulus checks’ sudden arrival on April 15. Specifically, I implement a regression discontinuity design using daily crime incident data from 47 large police departments to measure the impact on crime. I find little evidence of an effect on the overall crime rate, which disproportionately consists of minor crimes and crimes that may suffer from reporting biases. In contrast, however, the stimulus payments appear to have reduced homicides which is particularly notable because homicides have higher social costs and are better measured in official crime statistics than other types of crime. Furthermore, my estimates suggest stimulus payments reduced aggravated assaults and increased rape and sexual-related offenses.

Keywords: Pandemic, stimulus checks, CARES Act, homicide, assault, rape, violent crime, property crime, COVID-19, regression discontinuity design, public safety, coronavirus

JEL Classification: H24, K42, H59

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1 Introduction

The fallout from the COVID-19 pandemic has led to unprecedented levels of job loss, reductions in spending, and revenue losses in 2020. Recent work has found that the pandemic, social distancing, and shelter-in-place policies increased domestic violence, burglary, and motor vehicle theft (Leslie and Wilson, 2020; Abrams, 2020). In response to the economic downturn, the U.S. federal government passed the Coronavirus Aid, Relief, and Economic Security (CARES) Act, which provided stimulus checks to tens of millions of households.\footnote{This type of policy is described as a cash transfer program in the literature. It falls under the umbrella of benefit transfer programs which are broader.} Given prior research that shows that economic benefit transfer programs could increase crime and substance use (Carr and Packham, 2020; Palmer, Phillips and Sullivan, 2019; Dobkin and Puller, 2007; Watson, Guettabi and Reimer, 2020; Borraz and Munyo, 2015), in addition to the current climate of great distress, policymakers should know if and how the 2020 stimulus checks impacted crime. In this paper, I provide the first empirical evidence on how the 2020 CARES Act stimulus checks impact crime and contribute more broadly to the unresolved literature on cash transfer programs and crime.

Theoretically, the relationship between changes in economic resources and crime is ambiguous and depends on how the expression of violence is modeled (Carr and Packham, 2020). Supporting this notion is recent work that finds seemingly conflicting findings. For example, a positive income shock can decrease violence, substance abuse, and property crimes by providing better housing stability and decreasing the motivation for financial crimes (Palmer, Phillips and Sullivan, 2019; Foley, 2011; Watson, Guettabi and Reimer, 2020; Carr and Packham, 2019). In contrast, a positive income shock may also increase crime by increasing substance abuse, changes in behavior after income is exhausted, and increased household tensions (Carr and Packham, 2020; Palmer, Phillips and Sullivan, 2019; Watson, Guettabi and Reimer, 2020; Foley, 2011; Dobkin and Puller, 2007).
My study contributes to understanding how financial resources impact crime and differs from the current literature in several ways. First, the 2020 CARES Act represents the most expensive federal stimulus program ever implemented, whereas previous studies consider programs that are a fraction of the size comparatively. Second, the CARES Act distributed checks to nearly 160 million households, representing a much larger and more representative population relative to previous studies that predominantly focused on poor households receiving welfare. Third, stimulus checks were distributed recently during times of extreme economic distress, and policymakers need to know whether these stimulus checks are exacerbating the social effects of economic downturns. As a result of these differences, stimulus checks may induce a different response to crime.

To test how the disbursement of stimulus checks affects crime, I implement a regression discontinuity design that exploits the abrupt April 15 arrival date of the 2020 CARES Act stimulus checks. This design is motivated by the fact that most checks (72%) were directly deposited electronically into recipients’ bank accounts on April 15. Moreover, these checks represented a substantial increase in households’ liquidity, about 3.5 percent of annual income or roughly two weeks of wages for an average earner.

I measure the impact of the 2020 stimulus checks on overall crime and various offense types using 2020 crime incident-level data from 47 large U.S. police departments. I find no statistically significant impact on overall crimes. Using a 95% confidence interval, I can rule out an effect greater than 1.4 percent and less than -3.5 percent. These results mask substantial heterogeneity effects by offense type, most notably, homicides. My estimates suggest that the stimulus checks decreased homicides by 45 percent, corresponding to nearly ten fewer homicides on April 15 across the jurisdictions of data I use. This result is notable given that homicides have the highest social costs to society and, as a result, are typically better measured in crime statistics. Generalizing this finding to the entire U.S. imply the checks caused a reduction of 60-75 homicides on April 15.

I provide additional information on the CARES Act in Section 3.
My results also suggest that stimulus checks affected other types of violent crime. I find the stimulus checks decreased aggravated assaults by 18 percent.\(^3\) However, my estimates also suggest the stimulus checks increased rape and sexual assault incidents by 29 percent.\(^4\) To my knowledge, this is the first paper to show that an income shock can significantly reduce violent crime incidents (excluding sexual-related incidents).\(^5\) When considering the effects on other types of violent and non-violent categories, I do not find evidence of effects. My results suggest that stimulus checks can have a considerable social benefit, at least in the short-run, by reducing some of the costliest crimes to society; homicides and aggravated assaults. However, they can potentially impose a social cost by increasing sexual-related offenses. I posit that my findings are consistent with the stimulus checks decreasing economic tensions and increasing individuals being out of the household. I further discuss these mechanisms and policy implications in the discussion and conclusion section.

I organized the remainder of this paper as follows. The following section reviews the literature on the relationship between economic shocks and crime. Section 3 provides additional background information on the 2020 CARES Act. In Section 4, I describe the data used for my empirical strategy in Section 5. Section 6 reports the estimates of the impact of the 2020 stimulus checks on crime. Section 7 provides several robustness checks, and Section 8 concludes and offers policy suggestions.

2 Literature on Economic Shocks and Crime

Early economic models describing criminal behavior express potential offenders as rational individuals who weigh the costs and benefits of engaging in criminal activity (Becker, 1968).

\(^3\)Generalizing this finding to the entire U.S. imply the checks caused a reduction of 570-713 aggravated assaults on April 15.

\(^4\)Generalizing this finding to the entire U.S. imply the checks caused a reduction of 111-138 sexual incidents on April 15.

Such models predict that individuals receiving additional income through wages or employment in a legal environment will be less likely to engage in crime. In contrast, the relationship between unearned income, such as cash transfer programs, and crime is less clear because the choice to engage in crime does not necessarily result in forgone unearned income (Watson, Guettabi and Reimer, 2020). As a result, cash transfers, like stimulus checks, may lead to more or less crime depending on the mechanisms involved. In the following, I cover the different behavioral, and psychological mechanisms studied in the context of income shocks and crime and demonstrate how unresolved the literature is. The literature investigating behavioral mechanisms does so in the context of economic benefit transfer programs.

The literature finding negative effects on violent crime focuses on three primary mechanisms: 1) reduced financial obligations, 2) reduced financial stress, and 3) changes in interpersonal relationship forces. For example, stimulus checks can reduce financial stress by providing short-term security for financial obligations like rent. As a result, this could reduce the likelihood of a financially strained individual becoming homeless. Moreover, stimulus checks may decrease violent crime by alleviating financial stress. Financial stress adds a cognitive burden making it difficult for individuals to solve disputes (Mullainathan and Shafir, 2013; Mani et al., 2013). Helping individuals think about the long-term consequences of decisions through cognitive behavioral therapy reduces violence (Blattman and Sheridan, 2017). Finally, stimulus checks could provide greater bargaining power to potential victims in violence-prone relationships (Manser and Brown, 1980; McElroy and Horney, 1981; Farmer and Tiefenthaler, 1997; Pollak, 2005).

Recent empirical work highlights these mechanisms. For example, better housing stability achieved through emergency financial assistance for those experiencing adverse economic shocks decreases violent arrests (Palmer, Phillips and Sullivan, 2019). Calnitsky and Gonalons-Pons (2020) supports this and finds that government-provided guaranteed income reduced violent arrests in the 1970s. Calnitsky and Gonalons-Pons (2020) suggest this is driven by 1) reductions in financial stress and 2) improved women’s bargaining power in a relationship.
In contrast, Carr and Packham (2020) found that shifting Supplemental Nutrition Assistance Program (SNAP) benefit dates increased domestic abuse and child maltreatment, likely through increased drug abuse and household tensions. Watson, Guettabi and Reimer (2020), however, found that Universal Basic Income (UBI) has no effect on aggregated violent crime (the sum of assault, homicide, and sexual offenses) but did increase sexual assault. Furthermore, they find UBI increased substance-abuse crime incidents over the four weeks following disbursement. Several other studies also demonstrate that benefit transfer programs can increase the consumption of drugs and alcohol and increase substance-abuse crimes (Dobkin and Puller, 2007; Gross and Tobacman, 2014; Riddell and Riddell, 2006; Castellari et al., 2017). Drugs and alcohol are well known to be heavily involved in violent and sexual-related crimes, which implies stimulus checks may increase violent crime and sexual-related crime through increased consumption of drugs and alcohol (Lindo, Siminski and Swensen, 2018; Abbey, 2002; Carpenter and Dobkin, 2011; Kilpatrick et al., 2007; CBHSQ, 2015).

While violent crime has received considerable attention in the literature, other studies have focused on non-violent crimes, such as property offenses. Theoretically, the income effect generated from stimulus checks should reduce the motivation for financial crimes as individuals feel less financial stress (Foley, 2011; Carr and Packham, 2019; Palmer, Phillips and Sullivan, 2019). While this motivation decreases initially, property crime could rise towards the end of the month as households take on financial obligations they cannot meet (Carr and Packham, 2019; Palmer, Phillips and Sullivan, 2019). In contrast, more income could increase the expected utility of crime through a “better loot” effect and increase property crime (Borraz and Munyo, 2015; Wright et al., 2017).

Stimulus checks are distinct from the previously described transfer programs in several respects and may induce a different effect than those documented in the current literature. One main difference is that the 2020 stimulus check recipients constitute a broad and diverse socioeconomic group relative to the population examined in previous studies. Individuals of these previous studies likely differ in their income levels, time preferences, and consumption
patterns from the population I study (Watson, Guettabi and Reimer, 2020). Moreover, the 2020 stimulus checks also represent the most extensive federal stimulus program ever implemented, while previous studies consider programs a fraction of the size (in terms of dollars disbursed). Finally, it is becoming common for the U.S. to distribute stimulus checks during economic downturns, and policymakers may want to incorporate the social benefits and costs when designing these policies in the future.  

3 The 2020 CARES Act - Stimulus Checks

The U.S. federal government passed the CARES Act on March 27, 2020, to battle the economic collapse resulting from the COVID-19 pandemic. The main goals of the CARES Act were to increase consumer spending and reduce unemployment. In total, the CARES Act appropriated $2.2 trillion in stimulus to eligible businesses, states, municipalities, and individuals, making it the most extensive economic stimulus package in U.S. history. Of this amount, $267 billion was sent directly to nearly 160 million households (as of May 31, 2020), predominantly as electronically deposited checks. The majority of these checks (72%) were deposited electronically for households precisely on April 15, 2020, (Chetty et al., 2020). An individual’s most recent 2018 or 2019 tax return determined eligibility for a stimulus check. These checks provided a one-time payment of $1,200 to individuals who made less than $75,000, $2,400 to married couples making less than $150,000, and an additional $500 for each dependent regardless of filing status. Individuals making $75,001-$99,000 and married couples making $150,001-$198,000 received reduced amounts. Amounts were phased out for individuals making above $99,000 and married couples making above $198,000.

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6 Two other comprehensive stimulus payment programs occurred in 2001 and 2008.
7 I provide additional information on other appropriations provided by the CARES Act in Appendix. It does not appear these programs were implemented suddenly on or around the April 15 distribution date of the stimulus checks.
8 To put it in historical perspective, the New Deal in 2020’s dollars is just shy of $1 trillion.
9 I provide more in-depth information regarding the distribution of the stimulus checks in the Appendix.
A large majority of households receiving a stimulus payment, 76%, received it electronically via direct deposit (Internal Revenue Service, 2021). Individuals who did not provide banking information in their tax returns or to the IRS via the online portal received a paper check by mail or Direct Express cards (debit cards) by mail. The IRS began sending stimulus checks as paper checks and debit cards in the first week of May and were rolled out over the upcoming months. In total, 22% of stimulus payment recipients received their payment via paper check, and 2% received it via debit card (House Committee on Ways & Means, 2020). Altogether, over 86% of all payments were delivered by May 18 in the form of direct deposit, debit cards, or by check (U.S. Department of Treasury, 2020; Internal Revenue Service, 2021).

Understanding how households used their stimulus payments is important when considering how they impacted crime. Surveys and recent papers provide an insight into how households planned and spent their stimulus payment. Census Bureau surveys show that a majority of households receiving a stimulus check reported they would use it on household-related expenses (Cooney and Shaefer, 2021). 80% of households reported they would spend some of their check on food, and 78% reported they would spend it on rent and other household-related expenses. Households with incomes between $75,000 and $100,000 were more likely to report plans to pay off debt or save their check relative to households overall, while 88% of households with an income of $25,000 or less planned to use their payments to meet household expenses.

These survey results support recent work by Chetty et al. (2020); Baker et al. (2020); Karger and Rajan (2021) who found that spending jumped dramatically on April 15. Chetty et al. (2020) finds low-income households increased consumption significantly by 25 percentage points and high-income households increased consumption by 8 percentage points. Chetty et al. (2020) report 44% of the spending recovery can be attributed to an increase in durable good spending, and 18% can be attributed to an increase in in-person services. Baker et al. (2020) find that households with more significant income drops and lower levels of liquidity, in addition to low-income households, respond by increasing consumption immediately.

7
Furthermore, the consumption response is faster, and much more concentrated on food and household expenses than the consumption response to stimulus programs in 2001 and 2008. Karger and Rajan (2021) find households that live paycheck-to-paycheck spent 60% of their stimulus payment within two weeks while more financially secure households spend significantly less 24% of their check within two weeks.

I expect the stimulus checks to primarily affect violence or crimes of “passion” given the stressful environment surrounding the 2020 pandemic and what we know about how households responded. Crimes of passion are crimes that occur in the “heat of the moment” and are not premeditated. As a result, I expect the stimulus checks to reduce crimes related to aggression, rage, and anger by providing a significant, instant increase in financial stress relief on April 15. I also expect households to be more likely out of their household due to increased consumption. This mechanism can also reduce crimes associated with household violence as families are more likely to be in public than at home. Furthermore, I expect crimes of passion associated with being out of the household and crimes related to alcohol and drugs to have increased.

4 Crime Incident Data

I collected crime incident data from 47 police departments from the 100 most populous U.S. cities for incidents between January 1, 2019, and June 30, 2020. These data represent all police departments from the 100 most populous cities that host daily crime data publicly on their department’s web page or through their city’s open data portal. The incident

10The ranking of the 100 most populous cities comes from 2020 population estimates as enumerated by the U.S. Census Bureau.

11Cities include: Atlanta, Aurora, Austin, Baltimore, Boston, Chicago, Cincinnati, Colorado Springs, Dallas, Denver, Detroit, Durham, Fort Worth, Greensboro, Houston, Kansas City, Las Vegas, Lincoln, Los Angeles, Louisville, Memphis, Mesa, Milwaukee, Minneapolis, Nashville, New Orleans, New York City, Norfolk, Omaha, Orlando, Philadelphia, Phoenix, Pittsburgh, Portland, Raleigh, Riverside, Sacramento, San Antonio, San Francisco, Seattle, St. Louis, St. Paul, St. Petersburg, Tacoma, Tucson, Virginia Beach, and Washington D.C. Data from San Antonio was requested from the city of San Antonio.
data typically include incidents reported by the public and incidents observed or reported by police officers. The data are assembled by police departments similarly across all cities with a few exceptions.\textsuperscript{12,13}

I aggregate the total number of crimes by offense type across all police departments for each day. Not every department reports all offense types in their data. Table A1 shows the offense type coverage by location. For example, the Atlanta Police Department only reports a subset of crimes (homicide, robbery, burglary, larceny-theft, and a few others), while the Boston Police Department includes information on all offense types. As a result, my estimates for some offense types will contain a subset of the 47 locations. I focus my analysis on the costliest crimes involving multiple parties, violent crimes, property crimes, and substance-related crimes due to previous studies showing these being affected by income shocks. I first present results for all crimes and homicides, followed by violent crimes (aggravated assault, simple assault, rape and sexual-related offenses, and robbery) and non-violent crimes (burglary, drug offenses, larceny-theft, vandalism, and weapons offenses).\textsuperscript{14} Table A2 displays the average daily incident reports for each offense type and shows that non-violent incidents make up most overall incidents.

\textsuperscript{12} Dallas and Cincinnati use a victim-incident-based reporting system. I calculate daily incidents for these cities using the number of victims per incident. I show in Section 7 that my estimates are robust to dropping data from these departments.

\textsuperscript{13} Police departments serving Cincinnati, Colorado Springs, Dallas, Denver, Fort Worth, Kansas City, Lincoln, Minneapolis, New York City, Seattle, and Washington D.C. report crimes not by an incident date but by a potential range of dates that the crime may have occurred. For these departments, I use the first possible date of occurrence as the incident date for my main results presented in Tables 1 and 2. Police departments serving Aurora, Durham, Las Vegas, Milwaukee, New Orleans, Omaha, Raleigh, and St. Paul use a single date that could represent the incident or occur date. These cities are outside the top 25 of the 100 most populous cities. In Appendix Tables A3 and A4, I report estimates that omit incidents that have ambiguous timing. These results are generally very similar to my main results.

\textsuperscript{14} I define all crimes as the sum of all offense types. Homicides are defined as murder and manslaughter. Rape and sexual offenses are defined as rape, sexual assault, and all other sexual abuse-related crimes (when provided by a department).
5 Empirical Strategy

I implement a regression discontinuity design to exploit the sudden disbursement of stimulus checks on April 15, 2020. I define the running variable as the day of the year relative to the date of stimulus disbursement, such that the running variable is equal to -1 for April 14, 0 for April 15, 1 for April 16, and so on. My estimation strategy controls for the running variable using a linear model with a varied slope on either side of the disbursement cutoff date. To account for systematic variation in crime patterns across different days of the week, I include day-of-week fixed effects. To control for the crime that may occur due to first of the month paychecks or welfare payments, I include a dummy variable that equals one on the first of the month and zero on other days. To control for the crime that may change due to Easter, I include a dummy that equals one for April 11 and 12 (Easter weekend) and zero otherwise. My main specification is:

\[
\text{Incidents}_d = \alpha + \beta \text{Check}_d + \gamma \text{DoY}_d + \theta (\text{Check} \times \text{DoY})_d + \pi \mathbf{X}_d + u_d
\]  

(1)

where \( \text{DoY}_d \) is the running variable. Treatment, \( \text{Check}_d \), is defined as a zero for days before April 15 and a one for days on and after. I use April 15 as the cutoff since a vast majority of payments were deposited on this date. \( \mathbf{X}_d \) is a vector of controls that includes day-of-week fixed effects, a first of the month dummy variable, and an Easter weekend dummy variable. The outcome variable, \( \text{Incidents}_d \), represents the number of crime incidents for each offense type on a given day of the year \( d \).

The identifying assumption of my estimation strategy is that expected outcomes are

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15Note this empirical strategy is very similar to Doleac and Sanders (2015) who exploit Daylight Saving Time as an exogenous shock to daylight to study the impact on crime.

16Chetty et al. (2020) define the cutoff in the same way using a regression discontinuity design to study the impact of the 2020 stimulus checks on consumer spending.

17Results are virtually the same when including state fixed effects to control for differences in crime across states.

18Like Chetty et al. (2020), I omit incidents on April 14 from analysis because a small fraction of stimulus payments arrived on that day.
smooth through April 15, that is, that crime on April 15 should be similar to crime right before April 15. So long as no other major changes occur on, or right around, April 15, my identifying assumption will hold. In the Appendix, I discuss why other CARES Act provisions likely do not confound my estimates. I also closely examined many news articles and media for significant events that occurred on and right around April and found nothing that I believe could be driving my estimates. The disbursement of paper checks and debit cards will not affect my estimates as they began disbursement in early May. Finally, a common concern in studying the causal effect of income shocks is life cycle models that predict consumption will not respond to anticipated income. Chetty et al. (2020) shows consumption is smooth in the weeks leading up to April 15, implying no consumption anticipation (when controlling for day-of-week and first of the month).

My preferred estimates use a bandwidth of crime incident data three weeks pre- and three weeks post-check disbursement. I chose this bandwidth to avoid including crimes that may have changed when other policies and events occurred. For example, several states implemented stay-at-home orders mid to late March and began reopening more than three weeks after stimulus disbursement. The average number of days between when states passed a stay-at-home order and April 15 is 18. The average number of days between when states partially began to reopen and April 15 is 23 days. All estimates shown include robust standard errors.

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19 My main estimates use three weeks of data before and after April 15. A few shelter-in-place policies are taking place towards the end of March; however, bandwidth analysis shows estimates only using two weeks of data before and after yield very similar results to my main estimates. Additionally, triangular weights that apply additional weight to the most recent crime around April 15 that decreases as time is further from April 15 yield virtually identical results.

20 Smooth consumption before April 15 is supported by models that characterize individuals as very sensitive to an anticipated income shock (Campbell and Mankiw, 1989; Kaplan and Violante, 2014).

21 Additionally, because families were strapped for cash during the 2020 pandemic, it is unlikely changes in other economic behavior changed if consumer spending was smooth leading up to April 15.

22 In Section 7, I perform robustness analysis that shows my estimates are not sensitive to these different bandwidths.

23 Many cities experienced spikes in crime incidents after the murder of George Floyd; however, this event occurred on May 25, 2020, 45 days after check disbursement. As a result, changes in crime around the murder do not affect my estimates.

24 If serial dependence is present in the residuals or autoregression is present in the dependent variable, my estimates will be biased. Hausman and Rapson (2018) suggests clustering standard errors and including the
6 Results

In Figures 1 and 2, I display means plots (and estimates of the conditional expectation function) for all crimes and homicides. Figures A1 and A2 show the same for aggravated assaults, rape and sexual, robbery, simple assault, burglary, drugs, larceny-theft, vandalism, and weapons offenses. Crime incidents are plotted in bins on the y-axis for the days they occurred on the x-axis. I bin crimes in a way that creates five bins before and five bins after stimulus disbursement. Each bin can be interpreted as the average number of crimes over a four or five-day period that the bin covers. The conditional expectation function is estimated using a separate linear regression model for daily crimes before and after disbursement (within a three-week bandwidth). Visually, fitted lines for aggravated assaults, homicides, and sexual offenses across the stimulus disbursement date. All crimes and other crime types provide less or no visual evidence of a change in crime incidents.

Tables 1 and 2 show my estimated treatment effects from the linear regression discontinuity model presented in Equation 1. Recall I use a three-week bandwidth that includes crime incidents that occurred 21 days pre-and 21 days post-stimulus check disbursement and control for day-of-week fixed effects, first of the month fixed effects, and an Easter weekend fixed effect. Each column shows the estimate from a separate regression model where the outcome variable is the daily incidents by offense type. For each estimate, I show robust standard errors in parenthesis, the estimated crimes per day that would have occurred on April 15 (the mean), the estimated percent effect, and confidence interval of this effect. Percent effects are calculated by dividing the estimated treatment effect ($\beta$) by the estimated level of crime on April 15 ($\alpha$) and multiplying by 100.26

The estimates in Table 1, Column 1 show no statistically significant impact on the total

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25 The (α) is estimated from Equation 1 without including controls. This provides the estimated number of crimes that would have occurred on April 15 had stimulus checks not been disbursed.

26 There is no correct number of bins to pick. I selected five bins because they visually appeared to show the most evident crime patterns.
number of overall crimes. Constructing a 95% confidence interval around the percent effect for total crime allows me to rule out an effect greater than 1.4 percent and an effect below -3.5 percent. Thus overall, it does not appear that stimulus checks affected crime overall.

I then examine the impact on the costliest crime, homicide, and find substantial effects. My estimates suggest that stimulus checks caused homicides to decrease by 45 percent, and this effect is significant at the 5 percent level (Table 1, Column 2). This equates to a decrease of nearly ten homicides across my 47 jurisdictions of police department data on April 15. This is a notable finding in light of the fact that homicides are the costliest of crimes and are typically better measured in crime statistics.

Next, I examine the impact of stimulus checks on specific violent and non-violent offense types and find heterogeneous effects. My estimates for aggravated assaults suggest that the 2020 stimulus checks decreased these offenses by 18 percent, statistically significant at the 1 percent level (Table 1, Column 3). While I cannot directly test mechanisms as to why stimulus checks decreased homicides and aggravated assaults, my results may indicate that stimulus reduced household economic hardship. In contrast to these negative effects, the estimated impact on rape and sexual-related incidents suggest a positive effect of 29 percent (Table 1, Column 5). This result is very similar in magnitude to Watson, Guettabi and Reimer (2020). These findings could be evidence that some crimes increased due to individuals being more out of their households. Finally, I do not find significant effects on simple assaults or robbery (Table 1, columns 4 and 6). I also do not find significant effects on burglary, drug offenses, larceny-theft, vandalism, or weapons offenses (Table 2, columns 1, 2, 3, 4, and 5). In the next section, I conduct several robustness tests that support the validity of these results. In the final section, I provide a deeper discussion of mechanisms.
7 Robustness

This section examines potential confounders to my estimation strategy and provides evidence that suggests my results are not anomalous. A common concern in regression discontinuity models is that results are driven by bandwidth selection. To address this, I provide estimated effects for bandwidths between 14 - 28 days in Figures A3-A6. I selected this range because it represents two, three, and four weeks of data before and after stimulus checks. Using a bandwidth with less than two weeks of crime data before and after April 15 is likely not enough data. Using a bandwidth larger than four weeks will begin to incorporate crime on more days when states were passing stay-at-home and reopening policies, which have been shown to affect crime (Bullinger, Carr and Packham, 2020).

In Figures A3-A6, I plot the percent effect obtained from Equation (1) (on the y-axis) with the corresponding bandwidth (on the x-axis). For each estimate, I also plot the 95% confidence interval using robust standard errors. My estimates show very similar effects across these different bandwidths, which implies my estimates are not driven by bandwidth selection within this range.

An additional robustness check I perform is to test whether results are driven by one or by some subset of police departments. I do this to test the story that there was some event in one or some subset of cities in or around April 15 that are driving my results. If such a story were true, my estimated effects would comprise both stimulus checks and this potentially confounding event. To rule out this situation, I create a unique set of data sets that drop data from all combinations of one, two, and three police departments.\textsuperscript{27} I then estimate Equation 1 for each data set separately and report the density of these estimated percent effects in Figures A7-A10.\textsuperscript{28} These figures show the density of estimated effects (on the y-axis) with

\textsuperscript{27}For an offense type in which all 47 police departments report, such as Aggravated Assault, there will be a total number of 17,342 data sets and same number of estimates. A total of 46 unique data sets result from all combinations of dropping one department, 1,081 unique data sets result from all combinations of dropping two departments, and 16,215 unique data sets result from all combinations of dropping three departments.

\textsuperscript{28}Recall Equation 1 includes day of week fixed effects, a first of the month effect, an Easter weekend dummy, and uses a three-week bandwidth.
the corresponding percent effects (on the x-axis). I show my preferred estimate from Tables 1 and 2 using a red vertical line.

Figure A7 displays a density plot of the effects on total crime that results from dropping all combinations of one, two, and three departments of data. This figure shows that my preferred estimate is nestled within the distribution of effects I estimate from dropping different police department data combinations. This finding suggests that my estimates for crime overall are not driven by one, two, or three police departments. If the effect on total crime were being driven by some subset of police departments, my preferred estimate from Table 1 would be an outlier of this distribution. When examining the distribution of effects for homicides in Figure A8, many of the estimated effects resulting from dropping police department data are between -50 and -40 percent. Since my main estimate of the effect on homicides is -45 percent, this distribution implies that my estimates for homicides are also not driven by subsets of police departments. Figures A9 and A10 provide additional support that my main estimates for violent and non-violent offense types are not driven by one, two, or three police departments as these effects are contained within the distribution of effects.

A final robustness check I perform is to compare my main estimates (from Tables 1 and 2) to a distribution of effects on dates when stimulus checks were not disbursed. If my estimates that were statistically significant in 2020 (Tables 1 and 2) are not anomalous, then they should be an outlier in the distribution of effects when stimulus checks were not disbursed (on placebo dates in 2019). Conversely, my estimates that did not show significant effects in 2020 should be contained within the distribution of effects when stimulus checks were not disbursed (on placebo dates in 2019). To test this, I first aggregate 2019 crime data from the same police departments in the same way I do for my main analysis. I then estimate Equation 1 using each day in 2019 as a placebo cutoff. I also control for federal holidays in

\footnote{Specifically, I use dates January 21, 2019 - December 16, 2019, to use a full three-week bandwidth to generate each estimate. Recall Equation 1 includes day of week fixed effects, a first of the month effect, an Easter weekend dummy, and a three-week bandwidth. In total, there are 323 estimates for each offense type.}
addition to the controls used in Equation 1.\textsuperscript{30} I display the density of these estimates from these placebo dates in Figure 3 and Figures A11-A13. I denote my main estimates from Tables 1 and 2 using a red vertical line to compare to the density of placebo effects to my main findings. For each offense type, I also report the fraction of times the 2019 placebo effect is greater than my preferred estimate from 2020 in absolute value beneath each figure.

Figure 3 displays the distribution of effects for homicides from using days in 2019 as placebo cutoffs. A large majority of the estimates lie between -25 and 25 percent. The 2019 estimates are only larger (in absolute value) than my preferred estimate from 2020 (-45.2%) 9 percent of the time. This indicates that my 2020 estimated effect is an outlier in relation to the distribution of effects in 2019 and is likely not anomalous. In Figure A11, I report the distribution of effects for all crimes and find my main effect is well within the distribution of effects using 2019 placebo dates. In Figures A12 and A13, I show the distribution of effects for the other violent and non-violent offense types studied in my main analysis. These figures show that offense types that displayed a significant effect in 2020 are outliers in the distribution of effects from 2019 placebo dates. Offense types that showed no significant effect in 2020 are contained within the distribution of effects from 2019 placebo dates. Collectively, my placebo analysis indicates that all my significant estimates of the effect of the 2020 stimulus checks are outliers in the distribution of effects on placebo dates in 2019 and my insignificant estimates of the effect of the 2020 stimulus checks are well contained within the distribution of effects on placebo dates in 2019. This provides further evidence that my results are likely not anomalous.

\textsuperscript{30}Federal holidays include New Year’s Day, Martin Luther King Day, President’s Day, Memorial Day, Independence Day, Labor Day, Columbus Day, Veteran’s Day, Thanksgiving, and Christmas. These are included in my regression model when they are within the three-week bandwidth. I allow these controls to equal one for the day for the holiday and zero otherwise.
8 Discussion and Conclusion

In this paper, I present the first estimates of the effect of stimulus checks on crime and build on the contrasting literature evaluating the causal effects of income shocks on crime. My most notable findings are that 1) there is no evidence of an impact of the 2020 stimulus checks on crime overall, and 2) there is strong evidence that the 2020 stimulus checks caused a substantial reduction in homicides. I can rule out an effect greater than 1.4 percent and less than -3.5 percent with a 95% confidence interval for crime overall. My estimates on homicides suggest that the stimulus checks decreased homicides by 45 percent. Generalizing this result to the entire U.S. implies that the 2020 stimulus checks reduced the total number of homicides by 60-75 homicides.\(^{31}\)

When examining the effect of the stimulus on other types of violent and non-violent crimes, my results suggest the stimulus decreased aggravated assaults by 18 percent. While not testable directly, I hypothesize these crimes were reduced because of reduced financial hardship and increased households being out of the house, spending their payments (Cooney and Shaefer, 2021; Perez-Lopez and Monte, 2021; SafeGraph, 2020). Supporting these potential mechanisms are Census surveys that show stimulus payments helped households pay bills and lower feelings of depression and anxiety (Cooney and Shaefer, 2021; Perez-Lopez and Monte, 2021). Furthermore, Chetty et al. (2020) finds that individuals immediately spent part of their stimulus check on April 15. Safegraph Foot Traffic Data shows that trips to businesses began to increase right around April 15 (SafeGraph, 2020). Finally, my results suggest that stimulus checks increased rape and sexual-related offenses by 29 percent. I hypothesize these crimes increased as a result of individuals being more out of their house and by an increase

\(^{31}\)This range is calculated as follows. The fraction of the total U.S. population (2010) in the cities of police departments I use for data analysis is 13.1%. If my estimated effect generalizes to the entire U.S. population, then multiplying the reciprocal of 13.1% by my point estimate (-9.86) creates an upper bound of the impact nationally. The result from this calculation is 75.4. On the other hand, because my data comes from urban areas, my estimates may only generalize to these areas. I create a lower bound of the impact nationally by using the estimated U.S. population that lives in an urban area, roughly 80%. Taking 80% of the upper bound gives 60.3 as a lower bound.
(while not statistically significant for all bandwidth selections) in substance-abuse offenses.

Given the multifaceted effects of stimulus on crime, policymakers may want to account for stimulus programs’ social benefits and costs, especially when people are exceptionally financially stressed. My results suggest that stimulus checks can have a considerable social benefit, at least in the short-run, by reducing some of the costliest crimes to society; homicides and aggravated assaults. However, they can potentially impose a social cost by increasing sexual-related offenses. If there are no long-term increased effects, the government should distribute checks immediately to capture the social benefits of reducing homicides and aggravated assaults. In conjunction, the government should support services that aim at reducing sexual-related crimes, such as announcing the effects so the public and law enforcement can be prepared and support sexual crime prevention initiatives.

A limitation of my study is that the 2020 stimulus checks were disbursed during a unique period. April 2020 was a time of remarkable short-term economic decline and uncertainty. Therefore, my results may not generalize to understanding the impact of income shocks on crime during other periods or environments. Furthermore, the data I use comes from urban areas, and my results may not reflect the effects in rural areas. Future research should explore mechanisms on how stimulus checks can impact crime and study longer-run effects. Another avenue for future work would be to study how changes in time-use (and any associated changes in the environment, mood, etc.) could influence individuals’ propensities to commit a crime.\textsuperscript{32} Combined with recent work by Chetty et al. (2020) that individuals spent their checks immediately (with more significant effects in lower-income neighborhoods), time spent shopping from check receipt is a very plausible explanation for how stimulus checks impacted crime.

\textsuperscript{32}Lindo, Schaller and Hansen (2018) find that parental time use changes dramatically affect child maltreatment.
References


9 Tables and Figures

Figure 1
Aggregated plots - All Crimes

Note: This figure shows means plots and estimates of the conditional expectation function using a three-week bandwidth of data for all crimes. Crime incidents are plotted in bins on the y-axis for days in which they occurred on the x-axis. Day equal to zero represents April 15, 2020, the date in which most checks were disbursed. Crimes are binned in a way that creates five bins before and five bins after stimulus disbursement. Each bin can be interpreted as the average number of all crimes over a four or five day period. The conditional expectation function is estimated using a separate linear regression model for (unbinned) daily crimes before and (unbinned) daily crimes after disbursement (within the three-week bandwidth).
Note: This figure shows means plots and estimates of the conditional expectation function using a three-week bandwidth of data for homicides. Crime incidents are plotted in bins on the y-axis for days in which they occurred on the x-axis. Day equal to zero represents April 15, 2020, the date in which most checks were disbursed. Crimes are binned in a way that creates five bins before and five bins after stimulus disbursement. Each bin can be interpreted as the average number of all crimes over a four or five day period. The conditional expectation function is estimated using a separate linear regression model for (unbinned) daily crimes before and (unbinned) daily crimes after disbursement (within the three-week bandwidth).
Figure 3
Density of Estimated 2019 Placebo Effects - Homicide

Note: This figure displays the density of estimated treatment effects (%) from Equation 1. I implement crime data from 2019 to estimate a separate effect using each day between January 21, 2019 - December 10, 2019 as a separate placebo cutoff. Each regression model controls for day of week fixed effects, first of the month fixed effects, and holidays, and uses a three-week bandwidth. Holidays include all federal holidays and Easter (consistent with 2020 analysis). Federal holidays include New Year’s Day, Martin Luther King Day, President’s Day, Memorial Day, Independence Day, Labor Day, Columbus Day, Veteran’s Day, Thanksgiving, and Christmas. This creates a total of 323 different estimates, plotted in the density graph above. The red line represents my estimated treatment effect (%) from the 2020 stimulus as reported in Table 1. Finally, I report the fraction of times the 2019 placebo effect is greater than my preferred estimate from 2020 in absolute value.
<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1) All Crimes</th>
<th>(2) Homicide</th>
<th>(3) Aggravated Assault</th>
<th>(4) Simple Assault</th>
<th>(5) Rape and Sexual</th>
<th>(6) Robbery</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(125.40)</td>
<td>(3.68)</td>
<td>(23.59)</td>
<td>(30.89)</td>
<td>(7.27)</td>
<td>(14.30)</td>
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<td>Mean</td>
<td>10416.5</td>
<td>21.8</td>
<td>509.3</td>
<td>1008.6</td>
<td>63.0</td>
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<td>Percent Effect</td>
<td>-1.0</td>
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<td>-18.3</td>
<td>-4.8</td>
<td>28.7</td>
<td>-7.0</td>
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<td>95% C.I.</td>
<td>[-3.5 , 1.4]</td>
<td>[-79.7 , -10.7]</td>
<td>[-27.8 , -8.9]</td>
<td>[-11.0 , 1.5]</td>
<td>[5.2 , 52.3]</td>
<td>[-20.6 , 6.5]</td>
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</tbody>
</table>

**Notes:** *p <0.10, **p <0.05, ***p <0.01. Each column displays the estimated treatment effect from the regression discontinuity model described in Equation 1 where the outcome is crime incidents per day for each column offense type. Each regression uses a three-week bandwidth around the April 15 disbursement date. Each regression controls for day of week fixed effects, first of the month fixed effects, and an Easter weekend fixed effect. Robust standard errors are shown in parenthesis below the estimates. The estimated mean number of crime incidents that would have occurred on April 15 had the stimulus payments not been disbursed (the constant coefficient reported from the regression model without controls from Equation 1) is shown below standard errors. The estimated treatment effect (%) is displayed below the mean. The percent effect is calculated by dividing the estimated effect by the estimated mean and multiplying by 100. Finally, I show a 95% confidence interval for the percent effect which I calculate using robust standard errors.
Table 2
 Regression Discontinuity Effects of Stimulus Checks on Non-Violent Crime

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1) Burglary</th>
<th>(2) Drugs</th>
<th>(3) Larceny-Theft</th>
<th>(4) Vandalism</th>
<th>(5) Weapons</th>
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<td>Check</td>
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<td>2149.0</td>
<td>790.0</td>
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<tr>
<td>Percent Effect</td>
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<td>5.1</td>
<td>3.2</td>
<td>-1.1</td>
<td>-6.4</td>
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<tr>
<td>95% C.I.</td>
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<td>[-7.5, 17.8]</td>
<td>[-1.9, 8.2]</td>
<td>[-5.6, 3.3]</td>
<td>[-27.3, 14.4]</td>
</tr>
</tbody>
</table>

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Each column displays the estimated treatment effect from the regression discontinuity model described in Equation 1 where the outcome is crime incidents per day for each column offense type. Each regression uses a three-week bandwidth around the April 15 disbursement date. Each regression controls for day of week fixed effects, first of the month fixed effects, and an Easter weekend fixed effect. Robust standard errors are shown in parenthesis below the estimates. The estimated mean number of crime incidents that would have occurred on April 15 had the stimulus payments not been disbursed (the constant coefficient reported from the regression model without controls from Equation 1) is shown below standard errors. The estimated treatment effect (%) is displayed below the mean. The percent effect is calculated by dividing the estimated effect by the estimated mean and multiplying by 100. Finally, I show a 95% confidence interval for the percent effect which I calculate using robust standard errors.
10 Appendix

10.1 Additional CARES Act Information

The CARES Act included other appropriations in addition to stimulus checks, including; unemployment benefit changes ($260 billion), the Paycheck Protection Program (PPP) ($350 billion), loans to corporations ($500 billion), and aid to state and local government ($340 billion) (Snell, 2020). It does not appear these programs were suddenly implemented on or around the April 15 distribution date of the stimulus checks. As a result, the stimulus payments, rather than other CARES Act appropriations, are the likely driver of my estimates.

As individuals lost their jobs starting from the onset of the pandemic in March, the federal government implemented changes to unemployment benefits. There is no evidence that employers decided to issue mass layoffs on or around April 15. Unemployment insurance (UI) claims falling under the CARES Act provision did not show strong take up until late April (Kovalski and Sheiner, 2020). Combined with the fact that it takes 1-3 weeks for claims to be processed, it is unlikely unemployment benefit changes from the CARES Act are driving my results (California Employment Development Department, 2020; National Employment Law Project, 2020; Texas Workforce Commission, 2022). Furthermore, UI benefits are not paid on any specific day of the week and, as a result, should have been rising smoothly since the onset of the pandemic.

The PPP aimed to provide financial support through loans to small businesses. Eligible small businesses were able to apply for loans beginning on April 3, 2020, through April 16, 2020 and then again beginning on April 27, 2020 (Chetty et al., 2020). It typically took 1-2 weeks for loans to be processed and distributed to approved businesses (U.S. Small Business Administration, 2020). The government sent these loans weeks before and after the stimulus checks. However, they were not timed in any way with the stimulus payments. Furthermore, Chetty et al. (2020) found the PPP loans had no significant effect on employment and no apparent effect on consumer spending. Therefore, it is unlikely that the PPP is driving my results.

Finally, the CARES Act also provided relief to corporations and state and local governments. The "Main Street Lending Program" provided relief to mid and large corporations. Relief to state and local governments was to be predominately used for COVID-19 response efforts (Boston Federal Reserve, 2020). There is very little available information on how the government implemented these programs, and to my knowledge, there is nothing to suggest these programs were related to the April 15 stimulus checks. As a result, it is unlikely that these programs are driving my results.
10.2 Additional Stimulus Check Disbursement Information

The data in this section is provided by Internal Revenue Service (2021). Overall, 76% of stimulus checks were sent electronically, 22% of paper checks were sent via paper checks, and 2% of checks were sent via debit cards. 76% of check recipients had an Adjusted Gross Income (AGI) of less than $100,000. Distribution method varied by AGI levels. Generally, households with higher AGI were more likely to receive their stimulus check electronically relative to lower AGI households. Check amount increased for households with higher AGI levels. Below I provide more detail on variation in disbursement method by Adjusted Gross Income (AGI) levels and by states.

10.2.1 AGI < $30,000

Households in this group comprised 40% of all stimulus payments recipients. 66% of households received their payment electronically while and 34% received theirs by paper. None of these households received their payment via debit card. Furthermore, this group had an average payment amount of $1,522.

10.2.2 AGI between $30,000 - $60,000

Households in this group comprised 23% of all stimulus payments recipients. 81% of households received their payment electronically while and 19% received theirs by paper check for this AGI group. None of these households received their payment via debit card. Furthermore, this group had an average payment amount of $1,690.

10.2.3 AGI between $60,000 - $100,000

Households in this group comprised 16% of all stimulus payments recipients. 78% of households received their payment electronically, 15% received theirs by paper check, and 7% households received their payment via debit card. Furthermore, this group had an average payment amount of $1,952.

10.2.4 AGI > $100,000

Households in this group comprised 11% of all stimulus payments recipients. 74% of households received their payment electronically while and 17% received theirs by paper check for this AGI group. None of these households received their payment via debit card. Furthermore, this group had an average payment amount of $2,324.
10.2.5 No tax return information

Households in this group comprised 14% of all stimulus payments recipients. 92% of households received their payment electronically while and 8% received theirs by paper check for this AGI group. None of these households received their payment via debit card. Furthermore, this group had an average payment amount of $1,240.

10.2.6 State variation

There was very little variation in the amount of the average check across states. 92% of states had an average payment amount between $1,600 and $1,800. There is also very little variation of the percent of households receiving there payment electronically; all states had households receive their payment electronically between 69% to 79%. There some variation in the fraction of households that received their payment via paper check and debit card. There is a fairly uniform distribution of how households received payments via paper check that ranges from 17% to 31%. 50% of states had households who received their payment via debit card at less than 1%, while the distribution of states with debit card distribution above 1% is quite uniform up to the highest state of 7%.
10.3 Tables and Figures

Table A1
Offense Type Availability by Police Department

<table>
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<tr>
<th>Police Department Area</th>
<th>Homicide</th>
<th>Rape/Agg. Assault</th>
<th>Rape/Sexual Assault</th>
<th>Robbery</th>
<th>Simple Assault</th>
<th>Burglary</th>
<th>Drugs</th>
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Note: This table shows which city police departments includes which offense type in their data. An X denotes that location reports that offense type while a blank denotes it does not.
<table>
<thead>
<tr>
<th>Incident Code</th>
<th>Average Daily Incidents</th>
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<tr>
<td>Aggravated Assault</td>
<td>538</td>
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<tr>
<td>Homicide</td>
<td>17</td>
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<tr>
<td>Larceny-Theft</td>
<td>2,457</td>
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<td>Burglary</td>
<td>771</td>
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<tr>
<td>Drug</td>
<td>423</td>
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<td>Rape &amp; Sexual</td>
<td>27</td>
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<tr>
<td>Robbery</td>
<td>254</td>
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<td>Simple Assault</td>
<td>1,213</td>
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<tr>
<td>Vandalism</td>
<td>829</td>
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<td>Weapon</td>
<td>164</td>
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<tr>
<td>All Offenses</td>
<td>11,726</td>
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</table>

**Notes:** This table shows the average daily incidents taken across all jurisdictions from January 1, 2020 - June 30, 2020. All offenses includes some incident types not shown here. I focus my analysis on the costliest crimes and those that involve multiple parties, violent crimes, property crimes, and substance-related crimes due to previous studies showing these being affected by income shocks.
Figure A1
Aggregated plots - Violent Crimes

Note: This figure shows means plots and estimates of the conditional expectation function using a three-week bandwidth of data for non-violent crimes. Crime incidents are plotted in bins on the y-axis for days in which they occurred on the x-axis. Day equal to zero represents April 15, 2020, the date in which most checks were disbursed. Crimes are binned in a way that creates five bins before and five bins after stimulus disbursement. Each bin can be interpreted as the average number of all crimes over a four or five day period. The conditional expectation function is estimated using a separate linear regression model for (unbinned) daily crimes before and (unbinned) daily crimes after disbursement (within the three-week bandwidth).
Figure A2
Aggregated plots - Non-Violent Crimes

Note: This figure shows means plots and estimates of the conditional expectation function using a three-week bandwidth of data for non-violent crimes. Crime incidents are plotted in bins on the y-axis for days in which they occurred on the x-axis. Day equal to zero represents April 15, 2020, the date in which most checks were disbursed. Crimes are binned in a way that creates five bins before and five bins after stimulus disbursement. Each bin can be interpreted as the average number of all crimes over a four or five day period. The conditional expectation function is estimated using a separate linear regression model for (unbinned) daily crimes before and (unbinned) daily crimes after disbursement (within the three-week bandwidth).
Figure A3
Regression Discontinuity Impact by Bandwidth - All Crimes

Note: This figure displays the estimated treatment effect (%) on the y-axis for different bandwidths on the x-axis. Each percent effect is calculated by dividing the estimated treatment effect (from Equation 1) by the estimated mean number of crime incidents that would have occurred on April 15 had the stimulus payments not been disbursed (which is also the constant coefficient obtained from Equation 1 without controls) and multiplying by 100. Recall Equation 1 includes day of week fixed effects, first of the month fixed effects, and an Easter weekend effect. 95% confidence intervals are also shown for each estimate and are calculated using robust standard errors.
This figure displays the estimated treatment effect (%) on the y-axis for different bandwidths on the x-axis. Each percent effect is calculated by dividing the estimated treatment effect (from Equation 1) by the estimated mean number of crime incidents that would have occurred on April 15 had the stimulus payments not been disbursed (which is also the constant coefficient obtained from Equation 1 without controls) and multiplying by 100. Recall Equation 1 includes day of week fixed effects, first of the month fixed effects, and an Easter weekend effect. 95% confidence intervals are also shown for each estimate and are calculated using robust standard errors.
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Figure A7
Density of Effects from Omitting Subsets of Police Department Data - All Crimes

Note: The purpose of this figure is to show that my main results are not driven by a single police department nor a group of police departments. The final data set for my main results aggregates daily crime data from 47 police departments. If dropping some subset of police departments impacts my estimates, then this would imply my main estimates were biased. To rule this story out, this figure shows a density graph for treatment percent effects that are estimated from different versions of my main data set. I generate 17,342 unique variations of my main data set that are created by dropping one, two, and three police departments of data. A total of 46 data sets result from all combinations of dropping one department, 1,081 data sets result from all combinations of dropping two departments, and 16,215 data sets result from all combinations of dropping three departments. I implement Equation 1 using April 15 to estimate treatment effect for each of these data sets separately. I plot each of the estimated treatment effects (%) in the density graph above. Recall each regression controls for day of week fixed effects, first of the month fixed effects, an Easter weekend fixed effect and uses a three-week bandwidth. The red line represents my preferred estimated effect as reported in Table 1. Because my main estimates are centered in the distribution of effects obtained by dropping police departments, I am confident my main findings are not driven by a single police department nor a group of police departments.
Figure A8
Density of Effects from Omitting Subsets of Police Department Data - Homicide

Note: The purpose of this figure is to show that my main results are not driven by a single police department nor a group of police departments. The final data set for my main results aggregates daily crime data from 45 police departments. If dropping some subset of police departments impacts my estimates, then this would imply my main estimates were biased. To rule this story out, this figure shows a density graph for treatment percent effects that are estimated from different versions of my main data set. I generate 15,224 unique variations of my main data set that are created by dropping one, two, and three police departments of data. A total of 44 data sets result from all combinations of dropping one department, 990 data sets result from all combinations of dropping two departments, and 14,190 data sets result from all combinations of dropping three departments. I implement Equation 1 using April 15 to estimate treatment effect for each of these data sets separately. I plot each of the estimated treatment effects (%) in the density graph above. Recall each regression controls for day of week fixed effects, first of the month fixed effects, an Easter weekend fixed effect and uses a three-week bandwidth. The red line represents my preferred estimated effect as reported in Table 1. Because my main estimates are centered in the distribution of effects obtained by dropping police departments, I am confident my main findings are not driven by a single police department nor a group of police departments.
Figure A9
Density of Effects from Omitting Subsets of Police Department Data - Violent Crimes

Note: The purpose of this figure is to show that my main results are not driven by a single police department nor a group of police departments. The final data set for my main results aggregates daily crime data from 47 police departments. If dropping some subset of police departments impacts my estimates, then this would imply my main estimates were biased. To rule this story out, this figure shows a density graph for treatment percent effects that are estimated from different versions of my main data set. I generate these unique versions of my main data set by dropping all combinations of one, two, and three police departments of data (the total number of data sets for each offense type will vary because not each police department reports all offense types. See Appendix Table A1 for each crime’s availability). I implement Equation 1 using April 15 to estimate treatment effect for each of these data sets separately. I plot each of the estimated treatment effects (%) in the density graph above. Recall each regression controls for day of week fixed effects, first of the month fixed effects, an Easter weekend fixed effect and uses a three-week bandwidth. The red line represents my preferred estimated effect as reported in Table 1. Because my main estimates are centered in the distribution of effects obtained by dropping police departments, I am confident my main findings are not driven by a single police department nor a group of police departments.
Figure A10
Density of Effects from Omitting Subsets of Police Department Data - Non-Violent Crimes

Note: The purpose of this figure is to show that my main results are not driven by a single police department nor a group of police departments. The final data set for my main results aggregates daily crime data from 47 police departments. If dropping some subset of police departments impacts my estimates, then this would imply my main estimates were biased. To rule this story out, this figure shows a density graph for treatment percent effects that are estimated from different versions of my main data set. I generate these unique versions of my main data set by dropping all combinations of one, two, and three police departments of data (the total number of data sets for each offense type will vary because not each police department reports all offense types. See Appendix Table A1 for each crime’s availability). I implement Equation 1 using April 15 to estimate treatment effect for each of these data sets separately. I plot each of the estimated treatment effects (%) in the density graph above. Recall each regression controls for day of week fixed effects, first of the month fixed effects, an Easter weekend fixed effect and uses a three-week bandwidth. The red line represents my preferred estimated effect as reported in Table 2. Because my main estimates are centered in the distribution of effects obtained by dropping police departments, I am confident my main findings are not driven by a single police department nor a group of police departments.
Figure A11
Density of Estimated 2019 Placebo Effects - All Crimes

All Crimes

Fraction of times 2019 effect greater than 2020 effect in absolute: 0.81

Note: This figure displays the density of estimated treatment effects (%) from Equation 1. I implement crime data from 2019 to estimate a separate effect using each day between January 21, 2019 - December 10, 2019 as a separate placebo cutoff. Each regression model controls for day of week fixed effects, first of the month fixed effects, and holidays, and uses a three-week bandwidth. Holidays include all federal holidays and Easter (consistent with 2020 analysis). Federal holidays include New Year’s Day, Martin Luther King Day, President’s Day, Memorial Day, Independence Day, Labor Day, Columbus Day, Veteran’s Day, Thanksgiving, and Christmas. This creates a total of 323 different estimates, plotted in the density graph above. The red line represents my estimated treatment effect (%) from the 2020 stimulus as reported in Table 1. Finally, I report the fraction of times the 2019 placebo effect is greater than my preferred estimate from 2020 in absolute value.
Figure A12
Densities of Estimated 2019 Placebo Effects - Violent Crimes

**Aggravated Assault**

**Simple Assault**

**Rape and Sexual**

**Robbery**

Note: This figure displays the density of estimated treatment effect (%) from Equation 1. I implement crime data from 2019 to estimate a separate effect using each day between January 21, 2019 - December 10, 2019 as a separate placebo cutoff. Each regression model controls for day of week fixed effects, first of the month fixed effects, and holidays, and uses a three-week bandwidth. Holidays include all federal holidays and Easter (consistent with 2020 analysis). Federal holidays include New Year’s Day, Martin Luther King Day, President’s Day, Memorial Day, Independence Day, Labor Day, Columbus Day, Veteran’s Day, Thanksgiving, and Christmas. This creates a total of 323 different estimates, plotted in the density graph above. The red line represents my estimated treatment effect (%) from the 2020 stimulus as reported in Table 1. Finally, I report the fraction of times the 2019 placebo effect is greater than my preferred estimate from 2020 in absolute value.
Figure A13
Densities of Estimated 2019 Placebo Effects - Non-Violent Crimes

Note: This figure displays the density of estimated treatment effects (%) from Equation 1. I implement crime data from 2019 to estimate a separate effect using each day between January 21, 2019 - December 10, 2019 as a separate placebo cutoff. Each regression model controls for day of week fixed effects, first of the month fixed effects, and holidays, and uses a three-week bandwidth. Holidays include all federal holidays and Easter (consistent with 2020 analysis). Federal holidays include New Year’s Day, Martin Luther King Day, President’s Day, Memorial Day, Independence Day, Labor Day, Columbus Day, Veteran’s Day, Thanksgiving, and Christmas. This creates a total of 323 different estimates, plotted in the density graph above. The red line represents my estimated treatment effect (%) from the 2020 stimulus as reported in Table 2. Finally, I report the fraction of times the 2019 placebo effect is greater than my preferred estimate from 2020 in absolute value.
Table A3
Regression Discontinuity Effects of Stimulus Checks on All and Violent Crime

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1) All Crimes</th>
<th>(2) Homicide</th>
<th>(3) Aggravated Assault</th>
<th>(4) Simple Assault</th>
<th>(5) Rape and Sexual</th>
<th>(6) Robbery</th>
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</thead>
<tbody>
<tr>
<td></td>
<td>(112.50)</td>
<td>(3.33)</td>
<td>(20.60)</td>
<td>(29.12)</td>
<td>(2.74)</td>
<td>(13.50)</td>
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<tr>
<td>Mean</td>
<td>7479.1</td>
<td>17.4</td>
<td>420.6</td>
<td>912.0</td>
<td>14.6</td>
<td>193.2</td>
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<tr>
<td>Percent Effect</td>
<td>-0.6</td>
<td>-53.4</td>
<td>-11.7</td>
<td>-3.2</td>
<td>26.9</td>
<td>-6.4</td>
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</table>

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Each column displays the estimated treatment effect from the regression discontinuity model described in Equation 1 where the outcome is crime incidents per day for each column offense type. The estimates in this table omit incidents that have ambiguous dates of occurrence. Each regression uses a three-week bandwidth around the April 15 disbursement date. Each regression controls for day of week fixed effects, first of the month fixed effects, and an Easter weekend fixed effect. Robust standard errors are shown in parenthesis below the estimates. The estimated number of crime incidents that would have occurred on April 15 had the stimulus payments not been disbursed is shown below standard errors. The estimated treatment effect (%) is displayed below this baseline level and is calculated by dividing the estimated effect by the estimated mean and multiplying by 100. Finally, I show a 95% confidence interval for the percent effect which I calculate using robust standard errors.
Table A4
Regression Discontinuity Effects of Stimulus Checks on Non-Violent Crime

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<th>(3)</th>
<th>(4)</th>
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<td>Check</td>
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<td></td>
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<td>(10.21)</td>
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<td>Mean</td>
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<td>241.7</td>
<td>1666.7</td>
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<td>Percent Effect</td>
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<td>0.8</td>
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<td>95% C.I.</td>
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<td>[-2.7 , 7.7]</td>
<td>[-5.9 , 7.4]</td>
<td>[-20.4 , 16.1]</td>
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</tbody>
</table>

Notes: *p <0.10, **p <0.05, ***p <0.01. Each column displays the estimated treatment effect from the regression discontinuity model described in Equation 1 where the outcome is crime incidents per day for each column offense type. The estimates in this table omit incidents that have ambiguous dates of occurrence. Each regression uses a three-week bandwidth around the April 15 disbursement date. Each regression controls for day of week fixed effects, first of the month fixed effects, and an Easter weekend fixed effect. Robust standard errors are shown in parenthesis below the estimates. The estimated number of crime incidents that would have occurred on April 15 had the stimulus payments not been disbursed is shown below standard errors. The estimated treatment effect (%) is displayed below this baseline level and is calculated by dividing the estimated effect by the estimated mean and multiplying by 100. Finally, I show a 95% confidence interval for the percent effect which I calculate using robust standard errors.