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Does emergency financial assistance reduce crime?☆

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ABSTRACT

Does emergency financial assistance reduce criminal behavior among those experiencing negative shocks? To address this question, we exploit quasi-random variation in the allocation of temporary financial assistance to eligible individuals and families that have experienced an economic shock. Chicago's Homelessness Prevention Call Center (HPCC) connects such families and individuals with assistance, but the availability of funding varies unpredictably. Consequently, we can determine the impact of temporary assistance on crime by comparing outcomes for those who call when funds are available to those who call when no funds are available. Linking this call center information to arrest records from the Chicago Police Department, we find some evidence that total arrests fall between 1 and 2 years after the call. For violent crime, police arrest those for whom funds were available 51% less often than those who were eligible but for whom no funds were available. Single individuals drive this decrease. The decline in crime appears to be related, in part, to greater housing stability—being referred to assistance significantly decreases arrests for homelessness-related, outdoor crimes such as trespassing. However, we also find that financial assistance leads to an increase in property crime arrests. This increase is evident for family heads, but not single individuals; the increase is mostly due to shoplifting; and the timing of this increase suggests that financial assistance enables some families to take on financial obligations that they are subsequently unable to meet. Overall, the change in the mix of crime induced by financial assistance generates considerable social benefits due to the greater social cost of violence.

1. Introduction

In theory, emergency financial assistance targeted towards people facing an unexpected decline in income should reduce crime. In models of rational behavior where crime is an income-generating activity and leisure is a normal good, an income transfer would decrease crime. Scarce income can also affect cognition, encouraging focus on immediate (Mullainathan and Shafir, 2013) rather than long run consequences of committing crime or diminishing executive control (Mani et al., 2013) that might otherwise dampen impulsive violent actions. In addition, negative income shocks can create housing instability, placing people in situations where conflict is more likely to erupt (Desmond, 2016). For all of these reasons, insuring income shocks may generate public benefits by reducing crime. However, little evidence exists on whether timely financial assistance reduces crime.

In this paper, we test whether temporary financial assistance affects the likelihood of being arrested for people who experience a major shock to income or housing. To determine the effect of this emergency assistance on crime, we use data on people who call Chicago's Homelessness Prevention Call Center (HPCC) to request emergency financial assistance to pay rent, security deposits, utilities, and other expenses. The HPCC screens for callers with a significant but temporary crisis, allowing us to focus attention on households experiencing adverse shocks. Two additional key features of the HPCC allow us to examine the impact of financial assistance on crime through a quasi-experimental design. First, the call center collects information on all callers to determine eligibility before informing them about whether any funds are currently available. Second, the availability of funding for financial assistance varies unpredictably over time. Consequently, those who receive assistance are effectively a random subset of eligible households, once we condition on a small set of observable characteristics that affect access to assistance from particular funding agencies. We

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verify that the availability of emergency financial assistance is functionally random by showing that observable characteristics are very similar across the two groups at the time of the call.

To measure the impact of financial assistance on crime, we link the call center information to individual-level arrest records from the Chicago Police Department (CPD). Arrest rates for violent crimes are 0.87 percentage points (51%) lower for those who call when funds are available, and this effect persists for at least three years. The effect is particularly evident for single individuals, among whom violent crime arrest rates are 2.2 percentage points lower for those who call when funds are available. Battery committed by single individuals drives this reduction in violent crime. Increased property crime—particularly shoplifting—partially offsets the reduction in violent crime, though after a one-year delay. Overall, we find some evidence that calling when funding is available reduces overall arrest rates within 1 to 2 years. The offsetting changes in violent and property crime that we observe bear similarity to the effects of other interventions found in the literature: receiving a housing voucher restricted to a low poverty neighborhood (Sciandra et al., 2013), moving out of demolished Chicago public housing (Chyn, 2018), and closing high-risk schools for the day (Jacob and Lefgren, 2003). As in these prior examples, shifting from violent to property crime generates public benefits because of the high social cost of violence.

Further analyses help identify mechanisms that drive our results. Two pieces of evidence support the idea that financial assistance leads to a reduction in violent crime by stabilizing housing. First, previous research shows that the financial assistance we study significantly reduces entry into homeless shelters (Evans et al., 2016). Second, we find a significant decline in arrests for crimes associated with a lack of stable housing, such as trespassing, particularly for single individuals. Financial assistance might also change the recipient's neighborhood environment or alleviate the cognitive load induced by a crisis, but these mechanisms prove more difficult to test empirically.

Regarding property crime, we find that shoplifting arrests spike roughly one year after the original call, particularly for people requesting security deposits for new rental contracts. This evidence is consistent with a hypothesis that temporary assistance helps some recipients enter rental contracts that they struggle to fulfill. They commit property crimes one year later, perhaps to keep current on their rent at a time when the landlord could easily remove them during a lease renewal. Finally, we eliminate some potential mechanisms. One potential explanation for our findings is that financial assistance may affect arrests without necessarily affecting actual criminal activity if, for example, the police are less likely to arrest those who commit crimes while unstably housed, perhaps because they are harder to locate. The data do not support this explanation; we find no evidence that financial assistance affects arrests on bench warrants likely issued prior to receipt of assistance. Another potential explanation is that financial assistance is leading people to change the types of crimes they commit, substituting property crime for violent crime. This explanation, however, is not consistent with our results indicating that single individuals account for the decline in violent crime while family heads account for the rise in property crime.

We add to the existing literature in several substantive ways. First, we directly test whether targeted, temporary, financial assistance to address an income shock can reduce crime. Previous work has looked at the crime effects of income support for vulnerable populations such as ex-offenders or of more permanent assistance such as housing subsidies. Our study, however, is the first to examine the crime-reducing effects of emergency financial assistance. The program we examine provides a unique opportunity to determine whether insurance against transitory shocks can reduce crime. All eligible callers have received a shock (experiencing a crisis is a condition for eligibility for funds), but only a random subset of these callers receive assistance. Second, financial assistance programs such as the one we examine are available in nearly every community in the country, yet previous research has never examined the direct relationship between this assistance and crime. Previous work has shown that programs such as these reduce homelessness (Evans et al., 2016), but understanding the impact of financial assistance on crime is particularly important given the considerable social costs associated with crime. Finally, we combine demographic information with data on the timing and location of arrests and the nature of the charges to provide new evidence on the mechanisms by which financial assistance can affect crime.

2. Income shocks, crime, and public policy

Employment and income occupy a central place in canonical economic models of crime. Typical models since Becker (1968) consider potential criminals as economic agents that balance costs and benefits when deciding whether to commit a crime. In a standard labor-leisure model, if legal and illegal activities provide substitutes for obtaining income and leisure is a normal good, then financial assistance generates an income effect resulting in less crime. A large empirical literature examines whether a healthy local labor market can reduce crime. While some earlier research discounted the role of economic conditions (e.g. Levitt, 2004), recent studies indicate an important role (Chalfin and McCrary, 2017; Schnepel, 2016; Yang, 2017). Criminal activity can increase in response to an unemployment spell (Aaltonen et al., 2013; Bennett and Ouazad, 2016) or to debt troubles (Aaltonen et al., 2016), while Heller (2014) finds that a summer jobs program for youth cuts violent crime.[1]

Three studies provide perhaps the clearest evidence that income itself affects crime. Blakeslee and Fishman (2018) find that weather shocks to agricultural income can affect crime in developing countries. Foley (2011) finds that cities that pay Supplemental Nutrition Assistance Program (SNAP), Temporary Assistance for Needy Families (TANF), and Supplemental Security Income (SSI) benefits at the first of the month experience a monthly cycle in property crimes. Crime falls at the beginning of the month when the state pays benefits but rises as recipients exhaust this resource. Similarly, Carr and Packham (2017) find that spreading in-kind benefit allotments across the month can reduce theft. These results suggest that some poor households turn to crime when they cannot fully smooth income fluctuations.

Income shocks may also subvert optimal decision-making, leading to criminal behavior. Automatic responses to volatile situations can generate violence. Heller et al. (2017) and Blattman et al. (2017) find that cognitive behavioral therapy, which attempts to help participants think beyond automatic responses and build new decision-making processes, reduces violence among high-risk young men in Chicago, IL and Monrovia, Liberia, respectively. Low income can impede executive control over these automatic responses (Mani et al., 2013). Also, surprising negative outcomes relative to expectations can lead to violence when people are loss averse (Card and Dahl, 2011). Hence, negative income shocks may cause crime through behavioral mechanisms.

Income shocks also generate housing instability, which could lead to disruptive situations, criminal activity, and arrests. People experiencing shocks such as job loss are more likely to be evicted (Desmond and Gershenson, 2017). Qualitative work suggests that the threat of eviction can lead to interaction with the justice system by generating disputes with landlords about property damage, fomenting violence between tenants, affecting drug use, and so on (Desmond, 2016). Eviction may also lead to homelessness. Homeless individuals tend to commit more crimes and be arrested more often than the general population (Snow et al., 1989; Cronley et al., 2015). Many advocacy organizations argue that the homeless receive greater attention from law enforcement

[1] Employment may affect crime independent from its effect on income by impacting the time available for criminal activity. See Bushway and Reuter (2002) for a comprehensive discussion of theories relating employment and crime.

(USICH, 2010). Finally, housing moves may also change a household's neighborhood environment, including peer groups and police presence, both of which can affect criminal behavior (Jacob and Lefgren, 2003; Billings et al., 2013; Billings and Phillips, 2017; Draca et al., 2011). Thus, housing can also matter via the neighborhood environment.

These theories suggest that public policy could reduce crime by insuring people against income shocks. A large literature examines employment-related interventions for ex-offenders with mixed results.[2] In two prominent randomized control trials, Uggen (2000) finds that older ex-offenders offered subsidized employment recidivate at lower rates, while Berk et al. (1980) do not detect an overall effect of extending unemployment insurance to ex-offenders on arrests, likely because income transfers reduce poverty but also decrease employment. Less evidence exists on how providing traditional social insurance programs to a broader population affects crime. Labor market shocks from Chinese imports generate less crime for groups of people eligible for more generous unemployment insurance (Beach and Lopresti, 2016). Fishback et al. (2010) find that crime fell most in locations receiving the most intense aid during the New Deal. The literature on housing subsidies and crime mostly focuses on long-term interventions. Demolishing public housing and dispersing residents in Chicago reduced overall crime rates but also redistributed crime across the city (Aliprantis and Hartley, 2015; Sandler, 2017; Chyn, 2018), and low-income housing development spurred by tax credits reduces neighborhood crime (Freedman and Owens, 2011). The effect on criminal behavior of obtaining a housing voucher through a lottery varies widely across time horizon, sex, and study context (Sciandra et al., 2013; Jacob et al., 2015; Carr and Koppa, 2016). However, the literature on short-term responses to shocks remains scarce. A small but growing literature (Rolston et al., 2013; Gubits et al., 2015; Evans et al., 2016; Popov, 2016; Lucas, 2017) measures the effectiveness of homelessness treatment and prevention policies but has thus far not considered the impact on crime.

3. The Homelessness Prevention Call Center (HPCC)

The lack of evidence on the impact of temporary financial assistance proves surprising given its prevalence and the important part it plays in the social safety net. Local governments and nonprofit organizations provide short-term financial assistance throughout the country. Financial support for these efforts come from federal, state, and local funding as well as from community foundations and other private organizations. For example, many providers receive support for financial assistance programs through the Emergency Solutions Grants (ESG) Program. In 2014, the ESG allocated \$250 million to state and local governments, who then allocated these funds to local agencies. Each ESG grant must be matched nearly 100% by funds at the state or local level (HUD, 2014). The most common way that those in need connect with agencies providing financial assistance is through call center referral networks. For example, the 2-1-1 Network, in collaboration with United Way and the Alliance of Information & Referral Services (AIRS), operates call centers throughout the United States that process >15 million calls annually (211.org, 2015b). As of February 2015, the 2-1-1 Network operated regional information and referral call centers that were accessible by 93% of the American population; this coverage includes parts of all 50 states, Washington, D.C., and Puerto Rico, with only 11 states having <100% coverage (211.org, 2015a).

Chicago residents who are at risk of becoming homeless can call 3-1-1 (the city's services and information hotline) to request temporary financial assistance for rent, security deposits, or utility bills. These callers are then routed to the HPCC, which processes about 75,000 calls annually. The HPCC does not provide financial assistance directly. Rather, it is a centralized processing center that screens callers for eligibility and connects eligible callers with local funding (or delegate) agencies that provide resources to help address their crisis by making payments directly to landlords or utility companies.

There are two key features of the HPCC that allow us to examine the impact of temporary financial assistance on homelessness through a quasi-experimental design. First, the HPCC collects descriptive information on all callers to determine eligibility regardless of whether funds are currently available. Thus, they collect and maintain data for a group of eligible callers who do not receive financial assistance. Second, the availability of financial assistance from delegate agencies varies unpredictably over time. Consequently, those who receive assistance are effectively a random subset of eligible callers, once we condition on a small set of observable characteristics that affect access to financial assistance from certain delegate agencies.

At the beginning of each call to the HPCC, Information & Referral (I&R) Specialists collect detailed information in order to determine whether the client is eligible for financial assistance. General eligibility is based on four criteria. First, the client must be able to demonstrate self-sufficiency; his or her monthly income must be high enough to cover monthly housing expenses (and other re-occurring obligations such as child support payments) after he or she receives the temporary financial assistance. This income can come from earnings, transfers, or other sources. Second, the client must have an eligible crisis that has led to the need for assistance. While the HPCC uses this criterion for targeting, it also proves useful empirically, allowing us to examine crime among a unique sample of households facing significant adverse economic shocks. The crisis may be job loss, decreased work hours, reduction in public benefits, medical emergency, crime victimization, forced displacement, natural disaster, etc. In our sample, 63% of households face shocks to income while another 17% experience solely changes in housing, and the remaining 20% experience other shocks (e.g. increased family size). Some delegate agencies require documentation that a crisis beyond the control of the client caused the need. Third, the client must face imminent risk of homelessness or utility shut-off. Typically, the client can satisfy this requirement by presenting a five or ten-day eviction notice from his or her landlord or a notice of utility disconnection. Fourth, the current crises must be solvable by the financial assistance. In other words, the financial assistance must cover the entire debt remaining after taking into account all other sources of assistance that have already been secured. So, for example, if the maximum amount of assistance any delegate agency will provide is \$1500, then a caller whose total outstanding need exceeds \$1500 would typically be deemed ineligible even if he or she satisfies all the other eligibility criteria.

At any given time, the HPCC will have many different delegate agencies to which it can refer eligible callers for assistance. These delegate agencies have additional fund-specific restrictions beyond those imposed by the general eligibility rules. These fund-specific restrictions mean that some observable characteristics of eligible callers can affect the likelihood of receiving assistance. For example, the maximum amount of rent assistance varies across funding agencies, ranging anywhere from \$300 to \$1500 with many agencies having a \$900 ceiling. Thus, a caller whose "need amount" (which is calculated as total need for rent assistance less the amount the caller can contribute towards this debt) is \$900 is more likely to match with available funds than an otherwise similar eligible caller whose need is \$901 because the latter need amount exceeds the cap for more funds.

The two most important fund-specific restrictions that affect an eligible caller's access to funding are the request type (rent, mortgage, security deposits, and heating, gas, electric, and water bills) and the need amount. Other fund-specific restrictions that affect access to funding include veteran status (a few agencies restrict funding to veterans), receipt of housing subsidies (some agencies will not assist those who receive Section 8 vouchers), and the number of months of

[2] See Chalfin and McCrary (2017) for a systematic summary.

rent that are unpaid (some funds will not pay for more than one month of rent).

Funding is not available for all eligible callers seeking financial assistance, and availability varies unpredictably over time. New delegate agencies are coming online and existing agencies are shutting down throughout the year. In addition, currently operating agencies may not provide assistance continuously because they may temporarily run out of funds. The availability of funding on any given day depends on many factors. For example, some delegate agencies require that callers meet with a financial counselor before funds are dispersed, and an I&R specialist will not refer a caller for assistance if an interview slot is not available at the time of the call. For some agencies, there are only a fixed number of appointments available each week or month, but new interview slots might become available throughout the month due to cancellations. Variation in funding also results from the fact that some delegate agencies are supported by local or state programs that provide an inconsistent and unpredictable funding stream.

The HPCC has a preset order of delegate agencies to which it refers callers. The I&R Specialist will proceed through this list until she comes to an agency that has funds currently available and for which the eligible caller satisfies all the fund-specific restrictions. In this case, the caller is referred to that agency for financial assistance. For some delegate agencies, the I&R Specialist will provide the caller with the contact information for the agency, but other agencies prefer to contact the client themselves. In this case, the HPCC provides the contact information for the eligible client directly to the delegate agency. If no agency currently has funds available for a particular eligible caller, the HPCC refers the caller to non-financial support services. Ineligible callers are also referred to these support services.

From the perspective of the client, the availability of funds is difficult to predict. Resource availability varies within a given day and across days and months. It is HPCC policy not to provide any information about future funding. HPCC script guidelines include instructions for I&R Specialists to say that they do not have information on when funds will be available and to not recommend the best time to call back. The I&R Specialists are provided the following instructions (HPCC, 2013):

> If anyone asks, "when will a fund be available?" please respond the following:
>
> "I do not have information on when funds will be available. Unfortunately, there are not enough funds for everyone who needs assistance and availability is sporadic."
>
> If anyone asks, "should I call back?" please reply:
>
> "That is up to you."
>
> If anyone asks, "but what is the best time to call?" please reply:
>
> "There is no 'best time' to call. The need is so high in <Chicago/the Suburbs>, there are so many people trying to get access to the limited number of grants."

All calls are recorded. The I&R Specialists typically do not have specific information on future fund availability, and even when they do, they have little incentive to deviate from the guidelines by providing this information to callers.

4. Data

The empirical analysis for this study relies on administrative data on callers seeking temporary financial assistance provided by the HPCC and arrests from the CPD.

4.1. HPCC call data

The HPCC provided us with detailed call information for all calls from January 20, 2010 to April 3, 2013. In addition, the HPCC provided limited information on calls going back to June 1, 2009, so we could identify who among the callers in the early part of our sample were repeat callers. Data for all calls that are routed to the HPCC are entered into a proprietary electronic database that is part of the broader Homeless Management Information System (HMIS) for the city of Chicago. As a result, each caller is assigned a unique ID that is also used if they receive other housing services. These HPCC records include the call date, demographic information (such as name, date of birth, address, last four digits of Social Security Number (SSN), age, and gender), request type (for rent, security deposit, or utilities), other information gathered to determine general eligibility (such as sources and dollar amounts of income, type of crises, and whether they have an eviction notice), and information to determine whether they satisfy fund-specific restrictions (such as need amount, veteran status, receipt of housing subsidies, and whether the total debt exceeds one month of rent).

Because we have the ZIP code for each caller's residence at the time of the call, we can merge in data from the American Community Survey (ACS) and CPD incident reports on the characteristics of the caller's neighborhood. For each caller we calculate the following ZIP code level characteristics: the fractions of people with at least a high school degree, below the poverty line, and participating in the labor force; the percentage of people who are white, black, Asian, or of another race; the unemployment rate; median age; monthly housing cost; household income; and the arrest rate.

4.2. CPD arrest data

We use data from the CPD to measure arrests. The CPD data covers all arrests in the city of Chicago between January 1999 and September 2015. We match arrests to HPCC data using name, address, birthdate, and final 4 SSN digits from the call center data.[3] The data include offenses ranging from serious violent crimes to minor misdemeanors and code violations but do not include offenses that only result in a ticket. For example, we observe driving without a license but not speeding. Importantly, our data include charge codes that can be mapped to FBI Uniform Crime Report categories or other crime categories. We follow Uniform Crime Report designations for our violent (homicide, manslaughter, sexual assault, robbery, aggravated assault, aggravated battery, simple assault, simple battery, and criminal sexual abuse) and property (burglary, larceny, motor vehicle theft, arson, forgery/counterfeiting, fraud, stolen property, and vandalism) crime categories. Drug crimes are already recorded in a separated category by the CPD from the remaining other categories (weapons violations, prostitution, gambling, offenses against family, driving under the influence (DUI), liquor laws, disorderly conduct, misc. non-indexed offenses, municipal code violations, traffic violations, and warrant arrests). We can both place the crime within the city (according to police beat) and categorize the immediate environment, including outdoor versus indoor locations.

HPCC callers are arrested at a fairly high rate relative to arrest rates for the overall population, but these rates are comparable to those for the neighborhoods in which they live. In our main sample, 5.6% of eligible callers to the HPCC are arrested at least once within a year of the initial call. To compare to the whole City of Chicago, consider arrests during 2009, the year prior to the earliest calls to the HPCC in our sample. In this year (a relatively high-crime year) our sample experienced 9.0 arrests per 100 callers, while the entire city experienced 6.3 arrests per 100 people (CPD, 2009). In Fig. 1, we plot the residential locations for eligible callers and arrest rates, by ZIP code. As evident in this figure, callers tend

[3] Chapin Hall linked the CPD data to the HPCC data for us.

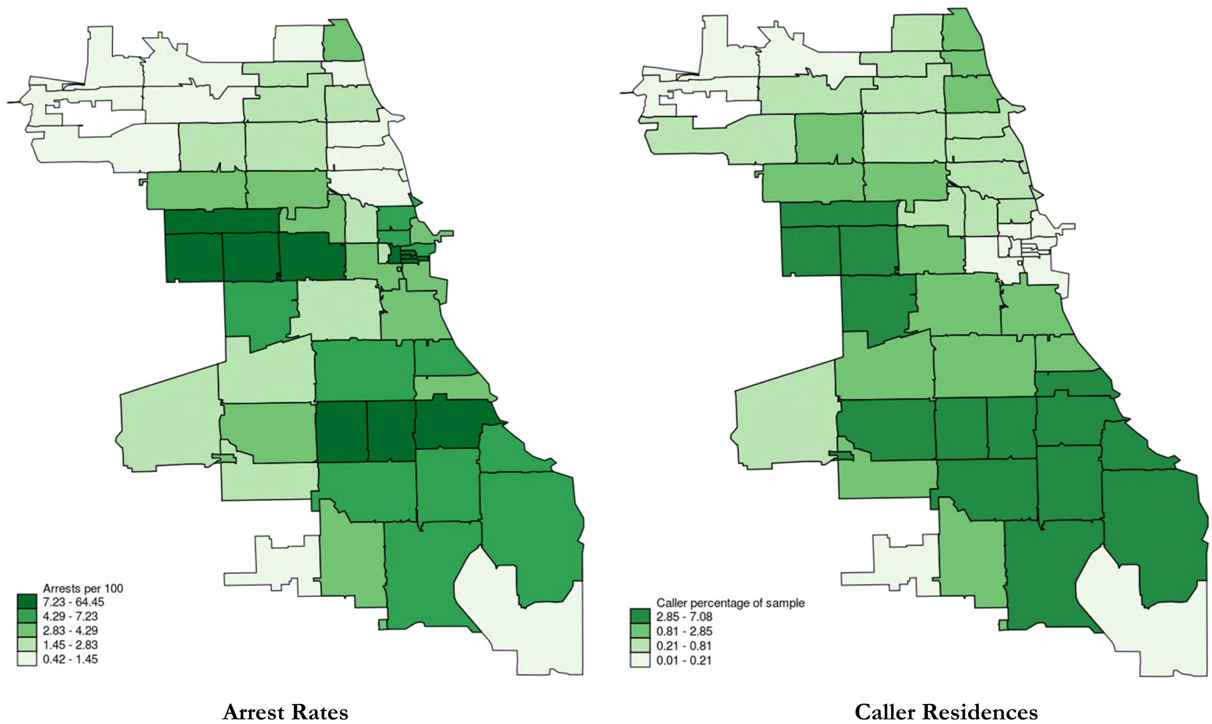

Fig. 1. Arrest rates and HPCC caller residential locations, by ZIP code. *Notes*: In the left pane, the shaded colors map neighborhood crime rates as measured by arrests per 100 residents. Darker areas indicate greater arrest rates. Arrests for all people in Chicago come from CPD records of all reported incidents in 2009 with a listed arrest. We map incidents to ZIP codes using the location of the incident. Population counts for ZIP codes come from the 2015 ACS. Shading in the right pane shows the proportion of HPCC calls within the main sample that are attributable to each ZIP code.

to be concentrated in high crime neighborhoods. Weighting district-level arrest rates by the residential locations of callers in our sample yields an arrest rate of 9.0 arrests per 100 people, which matches the actual arrests per 100 callers in our sample.[4]

Arrest data provide an admittedly imperfect measure of criminal activity. First, arrests differ from crimes committed, both because committing a crime does not always lead to being arrested and because the police may arrest the wrong person. Emergency financial assistance could affect how easily the police can find a known offender, which would affect the likelihood of arrest given a certain level of criminal behavior. For example, if the assistance helps reduce eviction, then those who do not receive assistance may be harder to locate as their housing has become less stable. In this case, higher arrest rates for those who do receive assistance would not necessarily imply more crimes committed. We address this issue below using data on bench warrants, which measure only the police's ability to locate suspects. Second, the data do not include arrests outside Chicago. We will not be able to measure if emergency financial assistance affects the tendency to commit crime outside the city. Mobility is relatively low, though. According to 2007–2011 ACS data, only 4% of households from Cook County with income below \$25,000 leave the county within one year. In any case, if our control group exits the city more frequently due to less stable housing, we will underestimate the social benefits of crime reduction.

4.3. Sample for analysis

The sample used for this study is drawn from the extract of all calls to the HPCC from January 20, 2010 to April 3, 2013. We narrow the window of calls to those occurring before September 14, 2012 so that we can observe information on arrests for at least 36 months after the call. We include not only requests for rent or security deposit but also utilities and other needs. We restrict our sample based on previous call history. It is quite common for callers to contact the HPCC multiple times. Our concern is that subsequent calls may not be exogenous—the characteristics associated with such calls may be correlated with both the availability of funds and criminality. For example, the persistence of repeat callers may generate a greater likelihood of receiving assistance, but this persistence may also indicate a different propensity to end up arrested, regardless of assistance. We restrict our attention to those who have not called recently for whom availability of funds should be exogenous. Our main analysis will use a sample of calls for which the caller has not called the HPCC in the past six months. In other words, we keep the first call for any person in our data and any subsequent call for which the gap between that call and the most recent call is >6 months. See Appendix Table 1 for the mean characteristics of these different samples. To demonstrate the robustness of our results to this sample selection criterion, we present results for both stricter and laxer sample restrictions in the appendix, restricting to no previous calls since June 2009 and no calls within the last week, respectively. See Appendix Tables 6, 7, 12, 13, 22, and 23.

Table 1 shows the impact of each additional restriction on sample size. During our sample period, the HPCC received 200,661 total calls. The HPCC data include an indicator for whether the caller is eligible for financial assistance based on the criteria described in Section 3. This indicator is calculated by the HPCC based on all intake information. Most callers are not eligible for financial assistance. Restricting the sample to eligible callers leaves us with 14,819 calls.[5] Further restricting the sample to the first call from an individual in the past six months yields our main sample of 8655 callers. At times, we will instead focus on the sample of 12,880 calls which are the first call within the past week or a sample of 7222 callers who have not called since June 1, 2009. As

[4] This comparison is only approximate. District arrests rates per 100 people use arrest location for the numerator and residential location for the denominator, but the people arrested in a given police district may not be the same as those living there.

[5] To be consistent with prior work in Evans et al. (2016) we also remove people already homeless when moving from all calls to eligible calls.

Table 1
Call volume, HPCC, January 20, 2010-September 14, 2012.

Sample composition	N	% funds available	# prior calls	Proportion with a prior call
All calls	200,661	5.4	0.7	0.31
Eligible calls	14,819	47.9	1.1	0.47
First call within last week	12,880	48.1	0.9	0.41
First call within last six months	8655	50.0	0.3	0.15
First call since June 2009	7222	49.8	0.0	0.00

Notes: The sample restrictions for each row include the restrictions imposed in all rows above it. For example, the sample in the third row that is restricted to first calls in the last week is also restricted to eligible calls.

noted above, funding availability is sporadic, so not all eligible callers are matched to available funds when they call. In total, same-day funds are available to 50% of callers in our main sample.

5. Empirical strategy

5.1. Regression specification

If the availability of funds were random, one could determine the impact of offering financial assistance on crime by comparing outcomes for eligible individuals who call the HPCC when funds are available to those for individuals who call when funds are not available. Specifically, one could estimate:

$$Y_i = \alpha_1 + Funds_i\beta_1 + \epsilon_{1i} \tag{1}$$

where Y_i is the dependent variable indicating whether person i was arrested after calling, and $Funds_i$ is an indicator that equals 1 if funds were available for that particular caller. Because $Funds_i$ is a dummy variable, the estimate for β_1 is simply the difference between mean outcomes for those who call when funds are available and those who call when they are not.

Table 2 reports the means for some of our key outcomes for both of these groups as well as the difference between these means for all eligible callers. Those who call when funds are available are 0.4 percentage points more likely to be arrested within 1 year than those who call when funds are not available, though this difference is not statistically significant. The overall difference in arrests masks heterogeneity by property and violent crime. Those calling when funds are available are 0.5 percentage points less likely to be arrested for violent crime and 0.4 percentage points more likely to be arrested for property crime. The difference in violent and property crime arrests are statistically different from zero at the 10% level.

The key assumption necessary for obtaining an unbiased estimate of β_1 is that availability of funds is not correlated with characteristics of the individual or of the call that affect the likelihood of being arrested. However, this assumption is not valid because at a given point in time not all eligible callers have the same likelihood of having funds available to them due to fund-specific restrictions. For example, delegate agencies differ in the maximum amount of assistance they will provide, and funds are not available to a caller if the fund cannot cover the entire need amount. Hence, funds are more likely to be available to eligible callers with a lower need amount. As shown in Table 2, a caller for whom funds are available (column 3) is much less likely to have a need amount of at least $900—34% of those for whom funds are available have a need amount of $900 or more. For those for whom funds are not available, 46% have a need amount of at least $900. Likewise, 41% of the full sample, but only 31% of those for whom funds are available, requests more than one month of rent. Callers requesting rent assistance compose the majority (55%) of the calls in our sample and are more likely to call when funds are available.

Another concern is that the availability of funds varies over time and this variation may be correlated with caller characteristics that directly

Table 2
Arrests and fund availability factors among HPCC callers, by availability of funds.

	(1) All Calls	(2) Calls When Funds Are Not Available	(3) Calls When Funds Are Available	(4) Difference
Outcomes				
Arrested within 1 year of call	0.057	0.055	0.059	0.0037
				(0.0050)
Arrested for violent crime within 1 year of call	0.015	0.017	0.012	−0.0048[a]
				(0.0026)
Arrested for property crime within 1 year of call	0.0090	0.0072	0.011	0.0037[a]
				(0.0020)
Fund availability factors				
Rent assistance	0.55	0.38	0.73	0.35[b]
				(0.0100)
Security deposit assistance	0.15	0.20	0.095	−0.10[b]
				(0.0075)
$900 or more in need	0.40	0.46	0.34	−0.12[b]
				(0.010)
Veteran	0.027	0.026	0.028	0.0019
				(0.0035)
Receiving housing subsidy	0.013	0.019	0.0074	−0.012[b]
				(0.0025)
Requesting >1 month of rent	0.41	0.59	0.31	−0.28[b]
				(0.014)
N	8655	4328	4327	8655

Notes: Results are for our main sample of eligible first-time calls within the last six months for rent, security deposit, utility, and other assistance, January 20, 2010 - September 14, 2012. See text for additional restrictions. Means are shown in the first three columns. The final column shows the simple difference as measured by a regression of the outcome on a fund availability dummy and no controls; heteroskedasticity-robust standard errors are in parentheses.

[a] Significant at the 10% level, for the difference between means.

[b] Significant at the 1% level, for the difference between means.

affect homelessness. For example, in our HPCC data the fraction of eligible callers for whom funds are available is the greatest on Mondays. If resourceful individuals are more likely to call on Mondays and this resourcefulness means they are less likely to become homeless regardless of whether they receive assistance, then this would bias our estimates of β_1.[6]

Fortunately, we can account for these fund-specific and call characteristics. We observe in the call center data the same characteristics that the I&R specialist uses to determine whether funds are available for eligible callers, so we can control for factors that affect access to funds. In particular, we can estimate the following model:

$$Y_i = \alpha_2 + Funds_i\beta_2 + X_i\delta_2 + Z_i\gamma_2 + \epsilon_{2i} \tag{2}$$

where X_i is a vector of observable characteristics of the caller (including age, gender, race, ethnicity, income, and receipt of benefits) that should not affect a caller's access to funds but are included in the model to reduce residual variance. The vector Z_i is a set of individual characteristics that may affect whether one is eligible for specific funds, including request type (i.e. rent assistance), need amount, veteran status, receipt of housing subsidies, and whether the total debt exceeds one month of rent. To account for patterns in call volume we also include in Z_i measures of call characteristics such as the rank of the call within the day, day of the week, month, and time of the month (first five days, last five days, and middle days). Because the maximum amount offered by various delegate agencies changes somewhat over the sample period, we also include interactions of need amount with year and

[6] To test whether callers might have information on fund availability, we also examine the relationship between call volume and past or future funding rates. We regress the log number of calls each day on leads and lags of the fraction of eligible callers that are referred to funds as well as indicators of the timing of the call within a year, month, or week. Results from this analysis indicate that call volume is not noticeably sensitive to prior or future funding rates, conditional on controls for a quarter of the year. See Appendix Table 2.

quarter indicators. The key coefficient of interest is β_2, which captures the difference in the outcome between those who call when funds are available and those who call when funds are unavailable, adjusting for these key factors.

5.2. Fraction of those for whom funds are available that receive assistance

Estimates of β_2 measure the intent-to-treat (ITT) effect of calling when funds are available and therefore being referred to an agency for financial assistance. This is different from the treatment-on-the-treated (TOT) effect of *receiving* assistance because of noncompliance—some callers who are eligible and notified that funds are available on that day never end up receiving funds. For example, the agency may not be able to contact the client, or the funding agency may determine the client to be ineligible once they meet. Furthermore, some callers seeking assistance when funds are not available may receive funds by calling back when funds are available. With data on which callers actually receive funds, we could estimate a first stage by regressing eventual receipt of funds on whether funds are available at the time of the call. Unfortunately, our data sources do not include information on actual receipt of financial assistance.

However, we do have information on receipt of funds for a small subset of HPCC callers. Loyola University of Chicago's Center for Urban Research and Learning (CURL) conducted a descriptive evaluation of the HPCC (George et al., 2011). As part of this evaluation, CURL conducted a follow-up phone survey of callers within 7 days of the HPCC call. This phone survey included 357 eligible callers seeking financial assistance—108 called when funding was available, while 249 called when it was not. Of the 105 surveyed callers in the CURL sample who called the HPCC when funds were available and provided information for the survey on the status of their request, 71% had already received funds from the designated agency, were anticipating the receipt of funds, or their request was being processed; 18% were never contacted by the agency; and 10% were deemed ineligible by the agency and denied funds. The CURL study also found that only 13% of those who called when no funding was available had already paid their outstanding bill within 7 days of the call, while 40% of those who called when funding was available had paid their bill. These numbers indicate that calling when funds are available has a noticeable impact on ability to address the presenting need that necessitated the call.

The CURL study does not report how often callers who contact the HPCC when funds are not available call back when funds are available. However, since we have call data over an extended period of time, we can calculate this directly. Among those who call when funds are not available in our sample of first-time eligible callers, only 12.6% called back when funds were available. Assuming that this group actually receives funds at the same rate as the group that is referred to available funds initially (71%), this implies that about 9% of the sample initially calls when no funds are available but eventually receives financial assistance through an HPCC referral. Accounting for both incomplete take-up by the treatment group and return visits by the control group, the first stage difference in take-up would be roughly 0.62, which implies TOT effects roughly 60% larger than our ITT estimates.

5.3. Exogeneity of fund availability

Fund availability varies considerably over time. On some days, funds are available to all eligible callers with a given set of characteristics, while on other days a subset or none of these eligible callers will be matched. The variation in the availability of funding is evident in Fig. 2, which is similar to in Evans et al. (2016). It shows the fund availability rate by week from 2010 through 2012. To ensure that the variation in this figure is not due to changes over time in caller characteristics, we focus on a subset of callers who are identical with respect to qualifying for specific funds. In particular, we restrict the

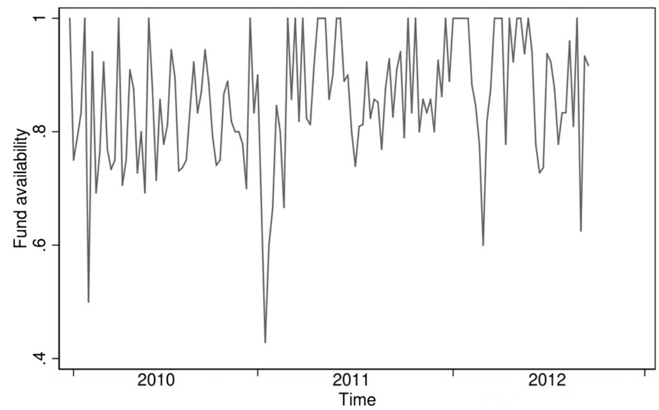

Fig. 2. Fund availability rate, by week, eligible callers to the HPCC. *Notes*: This figure is similar to Evans et al. (2016), but for a slightly different sample. Sample includes all eligible callers from 2010 to 2012 who are seeking rent assistance with need amounts between \$300 and \$900, who are non-veterans, who neither receive housing subsidies nor request more than one month of rent, who report both Social Security Numbers and family-scaled incomes below twice the poverty line, and who are not homeowners ($N = 2035$). The fund availability rate is the frequency of fund availability to those eligible callers who call within that week.

sample to callers seeking rent assistance who are requesting between \$301 and \$900, who are non-veterans, and who neither receive housing subsidies nor request more than one month of rent. As Fig. 2 shows, even after controlling for characteristics related to fund-specific restrictions, the likelihood of fund availability varies considerably. For some weeks, same-day funds are available to all eligible callers with these characteristics. But for most weeks, same-day funds are available to only a subset of these callers, and for two of these weeks funds are available to only half of eligible callers.

For our empirical strategy, the key assumption is that $Cov(Funds_i, \epsilon_{2i}|Z_i) = 0$. If this assumption is valid, then we would expect the characteristics of those who call when funding is available to look very similar to the characteristics of those who call when no funding is available once we control for Z_i. We test whether there is evidence of such balance by comparing the rich set of characteristics available in the HPCC data across these groups. In particular, we estimate regressions of the following form:

$$x_i = \alpha_3 + Funds_i\beta_3 + Z_i\gamma_3 + \epsilon_{3i} \tag{3}$$

Recall that x_i represents an observable characteristic for eligible caller i that should not be related to fund availability, such as age, gender, race, or income.

Table 3 reports the result of this analysis for eligible callers. In column 1 we present the means for observable characteristics for our comparison group—callers who are eligible but to whom same-day funding is not available. In column 2 we report β_3 from Eq. (3). For 33 of our 39 cases, we fail to reject the hypothesis that the characteristics are the same at the 5% level.[7] If these characteristics were independent of each other (which they are clearly not), we would expect about two rejections using a standard 95% critical value. So, we reject slightly more often than would be expected due to chance.[8] A joint test based on regressing the funds availability treatment on all the listed baseline characteristics (X_i) as well as characteristics related to fund-specific

[7] We calculate standard heteroscedasticity-robust standard errors. Evans, Sullivan, and Wallskog (2016) report results clustered by ZIP code, but they note that this clustering has little effect on the standard errors. We do not cluster because in practice the correlation of treatment within a ZIP code is low. In any case, clustering has little effect on our standard errors. Results available on request.

[8] In Appendix Tables 9–13 we report these results separately for those seeking help with rent, security deposits or other needs and for first-time callers within different windows of time. In general, the differences in means are similar for these subgroups.

Table 3
Mean characteristics of eligible, first-time callers for all types of assistance.

Dependent variable	Control group mean	Adjusted difference
Ever arrested before call	0.32	0.0074
Arrested 1 year before call or less	0.053	0.010[a]
Arrested 1 year before call or less — Violent	0.010	0.0020
Arrested 1 year before call or less — Property	0.0069	0.0025
Arrested 1 year before call or less — Drugs	0.0099	0.0011
Arrested 1 year before call or less — Other	0.021	0.0031
Female	0.83	−0.035[c]
White, non-Hispanic	0.063	0.011[a]
Black, non-Hispanic	0.89	−0.013[a]
Other, non-Hispanic	0.041	0.00045
Hispanic	0.072	0.00099
Age	40.8	−0.73[c]
Number of adults in caller's household	1.43	−0.021
Number of minors in caller's household	1.51	−0.072[b]
Percentage in ZIP code with HS degree (standardized)	0.00098	−0.019
Labor force participation rate in ZIP code (standardized)	−0.013	0.011
Unemployment rate in ZIP code (standardized)	0.0080	−0.018
Median age in ZIP code (standardized)	−0.0053	0.0047
Monthly housing cost in ZIP code (thousands, standardized)	0.014	−0.030
Median household income in ZIP code (thousands, standardized)	0.011	−0.015
Fraction black in ZIP code (standardized)	0.0054	−0.015
Fraction white in ZIP code (standardized)	0.00084	0.0060
Fraction other races in ZIP code (standardized)	−0.017	0.032
Applying due to benefit loss	0.12	−0.0055
Applying due to inability to pay bills	0.049	−0.010[b]
Applying due to exiting shared housing	0.058	0.0038
Applying to flee abuse	0.012	0.0014
Applying due to job loss	0.25	−0.0025
Monthly income (thousands)	1.08	−0.038[b]
Receiving SNAP benefits	0.69	−0.0083
Receiving child support	0.057	−0.0024
Receiving earned income	0.50	−0.0085
Receiving SSI	0.18	−0.0045
Receiving income from TANF	0.085	0.0054
Receiving unemployment payments	0.14	0.012
Receiving other income sources	0.082	−0.0076
Living situation: rent housing	0.84	−0.012
Living situation: shared housing	0.13	0.012
Shelter inhabitancy in past 18 months	0.047	0.014[b]
N	4328	8655

Notes: Results are for our main sample. The second column shows the coefficient on fund availability from a regression of the listed baseline characteristics on a fund availability dummy and controls for fund-specific restrictions.
[a] Significant at 10%; based on heteroskedasticity-robust standard errors.
[b] Significant at 5%; based on heteroskedasticity-robust standard errors.
[c] Significant at 1%; based on heteroskedasticity-robust standard errors.

restrictions (Z_i) rejects a null that the baseline characteristics (X_i) variables balance.

However, for the characteristics where we do reject the null, the differences in means are small and biased against detecting crime reductions. Past arrest behavior, which should be most predictive of future arrests,[9] has a positive coefficient. Those who were notified that same-day funds were available are 1.0 percentage points more likely to be arrested in the year before calling, which would bias negative effects on crime towards zero. Other baseline imbalances are likewise small and make our conclusions conservative. The treatment group is 3.5 percentage points more likely to be male, 0.73 years younger, has $38 less monthly income, and is 1.4 percentage points more likely to have entered an emergency shelter in the past 18 months. All of these differences are associated with a greater likelihood of being arrested in the future. As we show below, when we include additional observed characteristics as controls in our main specification, our estimates of how much fund availability reduces arrests become slightly larger.

5.4. External validity

Our sample closely represents those examined in other papers on housing instability. As shown in Table 3, the control group in our main sample consists of 83% female and 89% black callers with an average age of almost 41. Papers on housing subsidies in Chicago report primarily female and black samples. Voucher lottery applicants in Jacob and Ludwig (2012) are 88% female, 94% black, and average 32 years of age. Adults displaced due to public housing demolitions in Chyn (2018) are 87% female and average 29 years of age. Outside Chicago, in Collinson and Reed's (2018) study of the effect of evictions in New York City on poverty, participants in eviction court are 70% female and 59% black.

Samples of people receiving housing subsidies differ from the profile of a typical arrestee. A 2010 report released by the CPD indicates that 72% of all those arrested in the calendar year were black, but only 13% were female (CPD, 2010). Additionally, the arrested population in Chicago is much younger, with almost 85% of those arrested under the age of 45. While atypical in age and sex, the eligible callers in our sample have considerable exposure to the criminal-justice system. As Table 3 shows, one third of our sample has a previous arrest record with the CPD. Adults in Jacob and Ludwig (2012) and Chyn (2018) have 0.63 and 0.74 past arrests, respectively. Unstably housed people have some characteristics that typically predict low criminal activity but still often engage with the criminal justice system.

6. Results

We present our main results for the impact of emergency financial assistance on crime in Tables 4 and 5. We report these results for five different measures of arrests within one and three years of the call for our main sample as well for the subsamples of single individuals and family heads.[10] We only present the estimates for the effect of the main variable of interest, fund availability (β_2 in Eq. (2)); those for the other right hand side variables are reported in Appendix Table 14. For our full sample, fund availability leads to a 0.99 percentage point (18%) decrease in the probability of being arrested for a crime within one year of the call (column 1), and the effect is significant at the 10% level.[11] When we estimate a probit model the effect size is very similar and it is significant at the 5% level (Appendix Table 18). The results in the remaining rows of Table 4 show that a decline in arrests for violent crimes accounts for much of the overall decline in arrests. Calling when funds are available reduces arrests for violent crime within one year of the call by 0.87 percentage points, which represents a decline of 51% compared to the mean for those calling when funds are unavailable, and this estimate is significant at the 1% level. We do not find evidence of an effect of fund availability on arrests for property, drug, and other crimes within one year of the call.[12]

The results for single individuals and family heads reveal considerable heterogeneity in the effect of fund availability on arrests within

[9] Arrests are positively auto-correlated in our data. For example, a dummy for being arrested in the year before the call and a dummy for being arrested between 1 and 2 years before the call have a correlation coefficient of 0.24.

[10] We calculate heteroscedasticity-robust standard errors, but clustering at the ZIP code level has little effect on our standard errors. Randomization inference also yields similar results (compare Appendix Figure 1 to Table 4).

[11] This estimate differs from the raw difference in means reported in Table 2 (0.37) because of the inclusion in these specifications of controls for both factors that relate to fund-specific restrictions (Z_i) and other observable characteristics (X_i) (equation 2). Alternative specifications with no controls, controls for only need amount and need category, and only controls related to fund availability (Appendix Tables 15, 16, and 17 respectively) yield very similar results.

[12] Testing for multiple different measures of arrests could lead to multiple hypothesis testing concerns. If we apply the Benjamini and Hochberg (1995) correction to four tests for the different types of crime, the reduction in violent crime within 1 year and property crime within 3 years are statistically significant at the 5% level. The reduction in violent crime within 2 and 3 years is not statistically significant with the correction.

Table 4
OLS estimates of the effect of fund availability on arrests.

	(1) 1 year	(2) 2 years	(3) 3 years
Effect on all arrests	−0.0099[a]	−0.0080	−0.0031
	(0.0058)	(0.0071)	(0.0078)
Control group mean	0.055	0.087	0.108
Effect on violent arrests	−0.0087[c]	−0.0086[b]	−0.0086[a]
	(0.0033)	(0.0041)	(0.0046)
Control group mean	0.017	0.028	0.037
Effect on property arrests	0.0021	0.0052	0.010[c]
	(0.0024)	(0.0032)	(0.0037)
Control group mean	0.007	0.015	0.019
Effect on drug arrests	−0.00039	−0.0018	−0.0023
	(0.0026)	(0.0033)	(0.0039)
Control group mean	0.012	0.020	0.026
Effect on other arrests	0.0010	−0.0027	−0.0013
	(0.0042)	(0.0054)	(0.0061)
Control group mean	0.024	0.042	0.055
Controls for characteristics related to fund availability	Yes	Yes	Yes
Controls for other observable characteristics	Yes	Yes	Yes
N	8655	8655	8655

Notes: Results are for our main sample of eligible first-time calls within the last six months for rent, security deposit, utility, and other assistance, January 20, 2010–September 14, 2012. See text for additional restrictions. Each cell shows the coefficient on funds availability from a separate regression. The outcome is a dummy for being arrested for the listed type of crime within the listed time frame. Calendar and fund availability controls include linear controls for rank of the call within the day and ZIP code crimes rates for all crime, violent crime, and non-larceny crime as well as dummies for need amount category interacted with year and quarter, day of week, month, time of month, veteran status, housing subsidy receipt, needing >1 month rent, having income >2 times the poverty line, having an SSN, need request type, owning one's dwelling, senior status, and receiving disability payments. Other observable characteristics are the variables in Table 3, excluding lagged arrest records and shelter entry. We code missing values as zero and also include a set of dummy variables indicating when a variable is missing. Heteroskedasticity-robust standard errors are in parentheses.
[a] Significant at 10%.
[b] Significant at 5%.
[c] Significant at 1%.

one year of the call. As shown in Table 5, the crime-reducing effects of financial assistance is most evident for single individuals (column 1). For this group, fund availability leads to a 2.2 percentage point (34%) decrease in the probability of being arrested for a crime within one year of the call, and this effect is significant at the 5% level. This decline in arrests stems from a sharp decline in arrests for violent crime where the effect is significant at the 1% level. The point estimates of the effect of fund availability on arrests for property, drug, and other crimes for this group are small and not statistically significant. For all arrests and arrests for violent crime, the point estimates for families (column 3) are small and not statistically significant. However, we find a sizable and statistically significant positive effect of fund availability on property crime for this subgroup. The difference between singles and family heads (column 5) is statistically significant at the 1% level for violent crime and 10% level for property crime.

One concern with temporary financial assistance programs is that by addressing the immediate needs of an individual, the assistance is merely postponing the consequences of a negative income shock. Thus, any beneficial effects of the assistance may be short lived. Because we observe arrests for several years after each call in our data, we can examine whether our effects persist as time since the call increases. Specifically, we re-estimate Eq. (2) with the dependent variable being whether the caller has been arrested within τ months of the call, where τ ranges from −24 to 36. We report the main point estimates from these specifications along with the 95% confidence intervals for our main sample (Fig. 3), single individuals (Fig. 4) and family heads (Fig. 5). In addition, we report the estimates at 36 months in Tables 4 and 5.

These figures reveal small and statistically insignificant differences in arrests prior to the call. After the call, however, we see that fund availability has a persistent effect on arrests (as was shown in Tables 4 and 5). For the full sample (Fig. 3), the point estimate for the effect of fund availability on all arrests within two years of the call is very similar to the one-year estimate, although these estimates are not statistically significant. For violent crime, the effect of fund availability grows over the first 12 months after the call but then stabi-

Table 5
OLS estimates of the effect of fund availability on arrests by household type, single individuals vs. family heads.

	(1) Single 1 year	(2) Single 3 years	(3) Families 1 year	(4) Families 3 years	(5) Difference 1 year	(6) Difference 3 years
Effect on all arrests	−0.022[b]	−0.015	−0.0030	0.0055	−0.018	−0.019
	(0.010)	(0.013)	(0.0071)	(0.0099)	(0.013)	(0.017)
Control group mean	0.066	0.115	0.050	0.105		
Effect on violent arrests	−0.022[c]	−0.028[c]	−0.0019	0.0025	−0.020[c]	−0.030[c]
	(0.0060)	(0.0080)	(0.0040)	(0.0057)	(0.0072)	(0.0098)
Control group mean	0.021	0.042	0.015	0.034		
Effect on property arrests	−0.0042	0.0035	0.0055[b]	0.013[c]	−0.0096[a]	−0.0095
	(0.0047)	(0.0068)	(0.0027)	(0.0046)	(0.0054)	(0.0082)
Control group mean	0.010	0.021	0.006	0.018		
Effect on drug arrests	−0.0019	−0.0077	0.00088	0.00083	−0.0025	−0.0080
	(0.0052)	(0.0070)	(0.0031)	(0.0047)	(0.0060)	(0.0084)
Control group mean	0.019	0.038	0.008	0.020		
Effect on other arrests	0.0077	0.0094	−0.0017	−0.0046	0.0098	0.015
	(0.0074)	(0.010)	(0.0052)	(0.0077)	(0.0090)	(0.013)
Control group mean	0.024	0.053	0.025	0.056		
Controls for characteristics related to fund availability	Yes	Yes	Yes	Yes	Yes	Yes
Controls for other observable characteristics	Yes	Yes	Yes	Yes	Yes	Yes
N	3021	3021	5634	5634	8655	8655

Notes: Results are for our main sample of eligible first-time calls within the last six months for rent, security deposit, utility, and other assistance, January 20, 2010–September 14, 2012. See text for additional restrictions. Each cell shows the coefficient on funds availability from a separate regression. Columns 5 and 6 report the coefficients on the interactions between fund availability and a dummy for being single in a regression that also includes all controls and the interaction of single with all controls. The outcome is a dummy for being arrested for the listed type of crime within the listed time frame. See Table 4 for a list of controls. Heteroskedasticity-robust standard errors are in parentheses.
[a] Significant at 10%
[b] Significant at 5%.
[c] Significant at 1%.

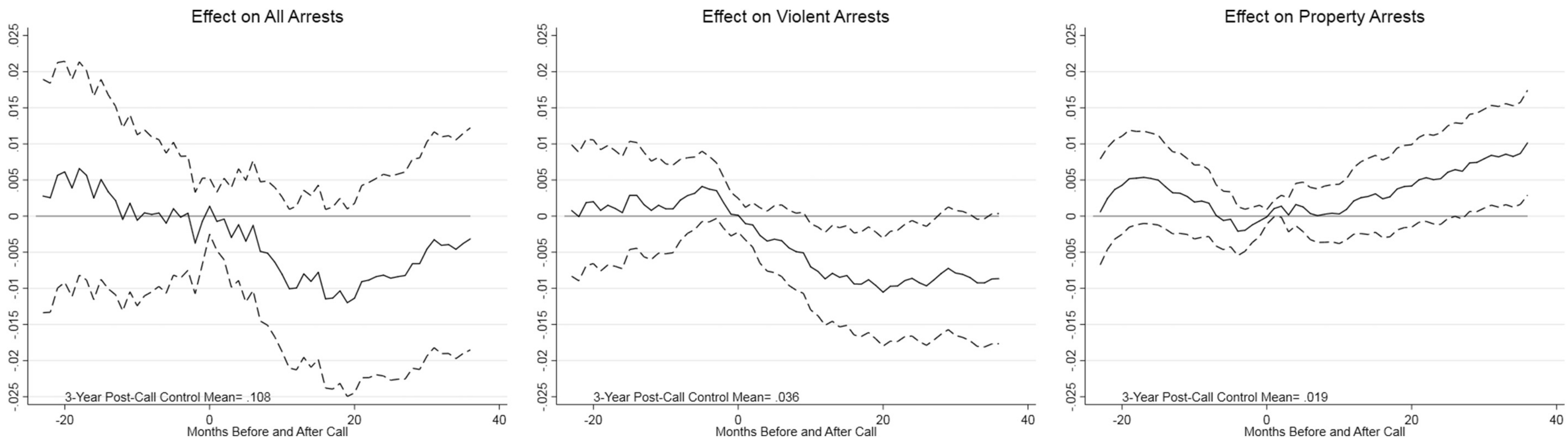

Fig. 3. Effect of financial assistance on arrests, full sample. *Notes:* the solid line plots the coefficient on fund availability in a regression where the outcome is a dummy for ever arrested in the τ months before or after calling. To the left of zero, the outcome is a dummy for ever arrested in the τ months before calling, multiplied by -1 (so that an upward slope indicates more arrests τ months before the call.) The regression includes a fund availability dummy and controls, as in Eq. (2). See the notes of Table 4 for a list of controls. The dashed lines show 95% confidence intervals with robust standard errors.

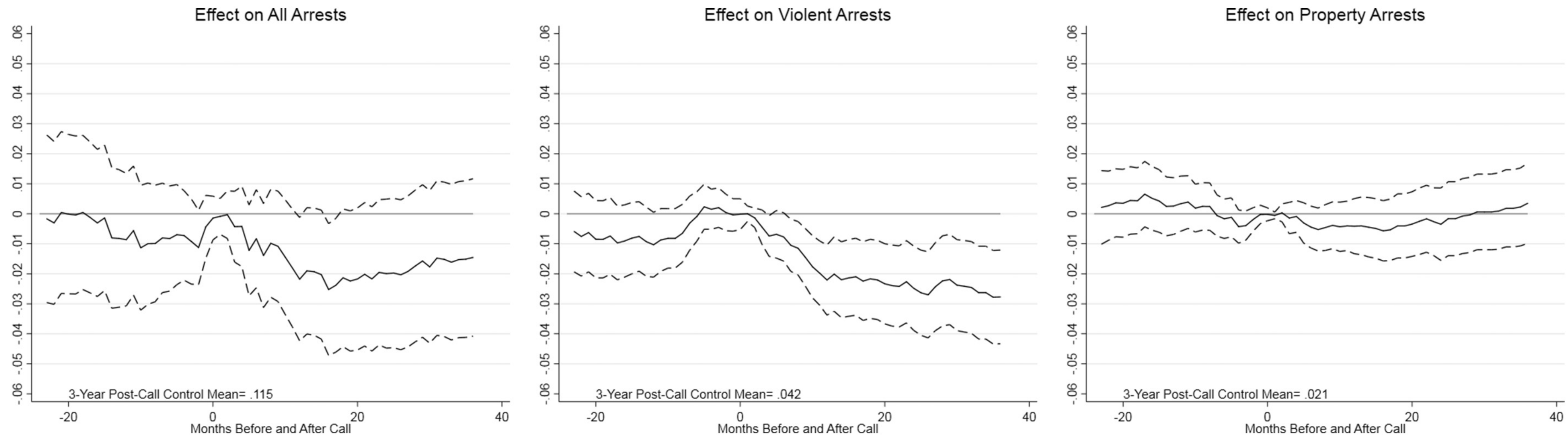

Fig. 4. Effect of financial assistance on arrests, single individuals. *Notes:* the solid line plots the coefficient on fund availability in a regression where the outcome is a dummy for ever arrested in the τ months before or after calling. To the left of zero, the outcome is a dummy for ever arrested in the τ months before calling, multiplied by -1 (so that an upward slope indicates more arrests τ months before the call.) The regression includes a fund availability dummy and controls, as in Eq. (2). See the notes of Table 4 for a list of controls. The dashed lines show 95% confidence intervals with robust standard errors.

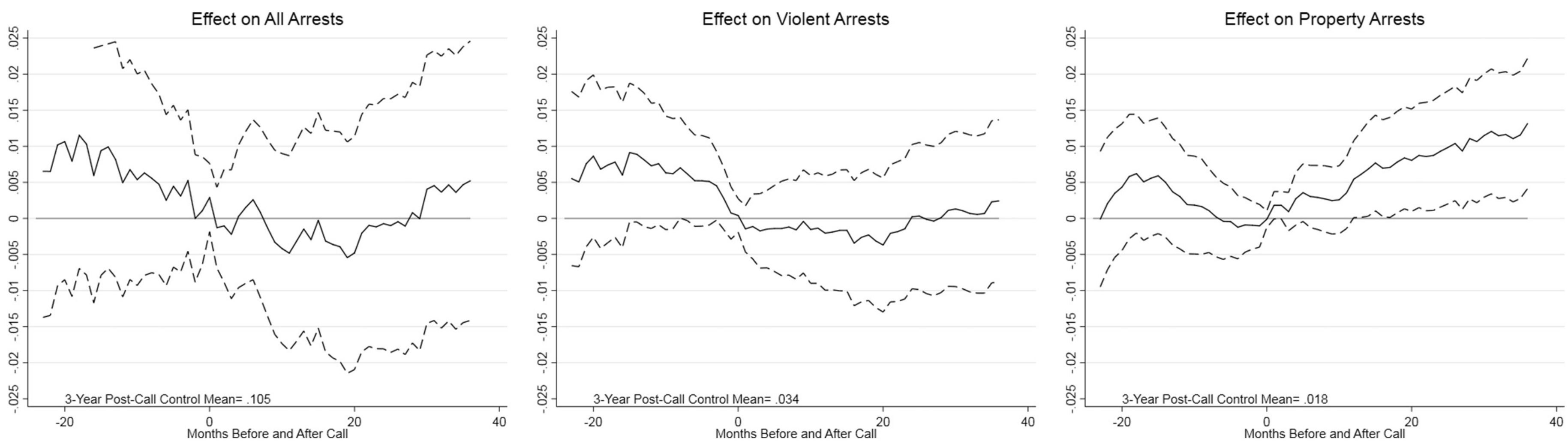

Fig. 5. Effect of financial assistance on arrests, family heads. *Notes:* the solid line plots the coefficient on fund availability in a regression where the outcome is a dummy for ever arrested in the τ months before or after calling. To the left of zero, the outcome is a dummy for ever arrested in the τ months before calling, multiplied by -1 (so that an upward slope indicates more arrests τ months before the call.) The regression includes a fund availability dummy and controls, as in Eq. (2). See the notes of Table 4 for a list of controls. The dashed lines show 95% confidence intervals with robust standard errors.

lizes and generally remains statistically significant, thereafter. The effect of fund availability on arrests for property crime, on the other hand, changes after a delay. In the first year after the call, property crime arrest rates are similar for eligible callers regardless of funding availability. However, 10–12 months after calling, those who call when funds are available begin accumulating more arrests for property crimes. By three years after the call, this difference in property arrests is statistically significant. The drop in violent crime arrests for singles (Fig. 4) mirrors the full sample results with larger magnitude, but unlike the full sample, singles experience no significant increase in property crime arrests. Among family heads (Fig. 5), fund availability has no discernable effect on arrest for all crimes or for violent crimes at any point over the 36 months following the call. For property crime, however, the positive effect of fund availability appears to increase as more time since the call passes. This effect is small and not statistically significant in the first several months after the call, but the magnitude of the effect grows noticeably from between 10 and 14 months after the call. The positive effect of fund availability on property crime arrests remains significant three years after the call.

We report the effect of financial assistance on arrests for all, violent, and property crimes within one year of the call for other subgroups in Table 6. Similar subgroup effects for other crimes and for longer time periods are reported in Appendix Tables 24–28. Each column estimates a different model with an interaction between fund availability and a baseline characteristic. The first column shows that the difference in the effects of fund availability between singles and family heads is not explained by differences in effects by gender. In fact, females account for the vast majority (81%) of our sample and drive the decrease in violent crime. The main effect on fund availability represents the effect for females and shows that our results are qualitatively similar if we exclude males from the sample. The statistically insignificant interaction terms indicate that effects for males and females do not differ for all arrests or property crime arrests. The positive and marginally significant interaction for violent crime suggests that if effects differ between men and women, they may be larger for women. We also find some evidence that changes in crime are concentrated among certain subgroups. Property crime increases are focused on those with below median income. We find no statistical relationship between age or need amount and the treatment effect. One group that might be particularly vulnerable to engaging in criminal activity in response to an income shock is those with a criminal history. Our results suggest that this group benefits considerably from emergency assistance. For those with an arrest record prior to the call, fund availability leads to a reduction in the likelihood of being arrested within one year that is 5.8 percentage points larger than the effect for those who have not been previously arrested. The difference between these sub-group effects is significant at the 1% level.

Table 6
OLS estimates of the effect of fund availability on arrests within 1 year, by subgroup.

	Male	>Median income	Age 30+	>Median need amount	Called within 5 years of arrest
All					
Funds	−0.019[b]	−0.012[b]	0.010	−0.0018	0.0015
	(0.0089)	(0.0056)	(0.015)	(0.0084)	(0.0049)
Funds × characteristic	0.018	0.014	−0.023	−0.014	−0.058[c]
	(0.012)	(0.021)	(0.016)	(0.012)	(0.022)
Characteristic	0.21[a]	0.35[a]	−0.38	−0.24	0.33
	(0.12)	(0.19)	(0.41)	(0.42)	(0.34)
Mean for control group, characteristic = 0	0.068	0.040	0.085	0.057	0.030
Violent					
Funds	−0.014[c]	−0.0078[b]	−0.0062	−0.0079[a]	−0.0057[b]
	(0.0048)	(0.0032)	(0.0085)	(0.0045)	(0.0027)
Funds × characteristic	0.012[a]	0.00059	−0.00085	−0.0013	−0.011
	(0.0066)	(0.011)	(0.0092)	(0.0066)	(0.012)
Characteristic	0.0077	−0.038	−0.13	−0.52	−0.60[c]
	(0.063)	(0.093)	(0.29)	(0.39)	(0.16)
Mean for control group, characteristic = 0	0.018	0.012	0.027	0.014	0.0080
Property					
Funds	0.0014	0.0044[a]	0.0084	0.0046	0.0024
	(0.0040)	(0.0023)	(0.0060)	(0.0043)	(0.0022)
Funds × characteristic	0.0014	−0.020[b]	−0.0076	−0.0047	−0.0014
	(0.0047)	(0.0090)	(0.0065)	(0.0049)	(0.0092)
Characteristic	−0.0064	0.067	0.087	−0.15[a]	0.017
	(0.057)	(0.079)	(0.089)	(0.086)	(0.10)
Mean for control group, characteristic = 0	0.011	0.0070	0.015	0.011	0.0055

Notes: Results are for our main sample of eligible first-time calls within the last six months for rent, security deposit, utility, and other assistance, January 20, 2010–September 14, 2012. See text for additional restrictions. The outcome is a dummy for being arrested for the listed type of crime within 1 year. Funds refers to an indicator for fund availability and characteristic refers to a dummy for the condition listed in the column titles. The funds, funds × characteristic, and characteristic coefficients come from a regression that includes the control variables from Table 4 as well as the interaction of these controls with the baseline characteristic dummy. Heteroskedasticity-robust standard errors are in parentheses.
[a] Significant at 10%.
[b] Significant at 5%.
[c] Significant at 1%.

7. Mechanisms

As discussed above, several different mechanisms could link emergency financial assistance to arrests. This intervention could affect criminal activity by changing the incentives to commit crime, decision-making processes, housing stability, or other circumstances. We now investigate these different possibilities empirically.

7.1. Incentives to commit crime

An economic model in the spirit of Becker (1968) would consider potential criminals as economic agents that balance costs and benefits when deciding whether to commit a crime. In this setting, if illegal activity is a means for obtaining income, a standard labor-leisure model would suggest that financial assistance generates an income effect, resulting in less crime as long as leisure is a normal good. This prediction, however, is not consistent with our results for property crime.

Alternatively, an income shock might alter incentives to commit different types of crimes. For example, sudden loss of income might lead an individual to substitute away from less serious crimes such as shoplifting towards more serious crimes such as armed robbery. This sort of substitution is consistent with our findings of decreases in arrests for violent crimes but increases in arrests for property crimes. However, if substitution between different types of crimes were to explain our findings, then we would expect to observe opposing effects for violent and property crime for the same groups of people. In our results, however, the decline in arrests for violent crime is evident for single individuals while the rise in arrests for property crime is evident for family heads. This result holds even for detailed crime types. Fig. 6 shows treatment effects according to crime categories from the CPD that generally align with those from FBI Uniform Crime Reports. Each point shows the coefficient on fund availability from Eq. (2) using a different outcome. The outcomes are indicators for being arrested for the listed crime category within three years of the call. A substitution story would predict that, within one group, treatment would lead to increases in some types of arrests and decreases in others. However, for singles we observe no categories with crime increases to offset decreases in simple battery and perhaps drugs. For heads of families, we observe increased larceny but no categories with decreases except perhaps for traffic offenses. Driving without a license seems an unlikely candidate for a substitution mechanism.

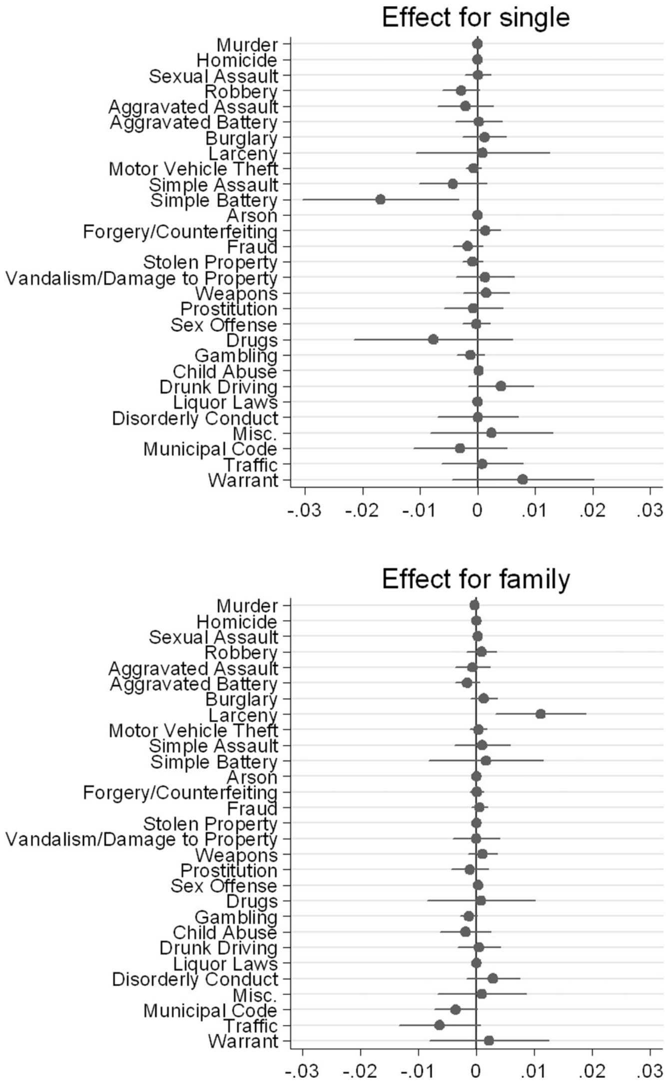

Fig. 6. 3-Year effects of financial assistance on arrests, by detailed crime category. *Notes:* each plotted point corresponds to a separate regression coefficient on fund availability. We regress a dummy for being arrested for the listed crime type in the 36 months after the call on a fund availability dummy and controls, as in Eq. (2). See the notes of Table 4 for a list of controls. The whiskers show 95% confidence intervals with robust standard errors.

7.2. Other explanations for the decline in crime

There are a number of other reasons why income shocks might lead to a rise in crime and, consequently, why insurance against these shocks might reduce it. First, a negative shock, such as job loss, may generate conflict if the attention required by the situation and the resulting stress makes it difficult for people to effectively resolve interpersonal disputes (Mullainathan and Shafir, 2013). These stressful situations can be made even worse when housing becomes less stable. While it is difficult to empirically test this explanation, it is consistent with the decline we find in arrests for crimes involving another person, particularly simple battery (Fig. 6).

Income shocks might also lead to greater crime by making housing less stable. People experiencing shocks such as job loss are more likely to be evicted Desmond and Gershenson, 2017. And recent qualitative work suggests unstable housing causes conflict to erupt when people move in with strangers (Desmond, 2016, e.g. chapters 12 and 15). Evans et al. (2016) show that emergency financial assistance leads to a significant reduction in homelessness. This reduction in homelessness may, in turn, lead to a reduction in crime. We can test the importance of housing stability by examining charges that are strongly associated with homelessness. Specifically, we compile a list from a National Coalition for the Homeless (2006) report that documents common charges issued against the homeless, such as trespassing.[13] We also observe the location of the arrest and can particularly focus on outdoor arrests for these crimes. In Panel A of Table 7 we report the effect of fund availability on these homelessness-related crimes. We find negative effects for homelessness-related crimes overall, though these estimates are not statistically significant. However, the results do suggest that availability of funds leads to a significant reduction in outdoor, homelessness related crimes. For the full sample, this effect is large and statistically significant at the 5% level in the year after the initial call for assistance. A decrease in homelessness-related arrests matches what one would predict if financial assistance stabilizes housing, which in turn prevents crime.

Income shocks might also generate crime through increased drug and alcohol use. In this case, emergency assistance could reduce crime by preventing the shocks that lead to substance abuse. On the other hand, the income transfer could provide support for drug or alcohol use and thereby increase crime. If drugs and alcohol were an important mechanism, then we would expect to see an effect of fund availability on arrests for drug and alcohol related crimes. As shown in Table 4 and Fig. 6, arrests for drug crimes are lower for those who call when funding is available, but this difference is not statistically significant. We find little evidence that funding is related to arrests for alcohol related crimes such as liquor law violations, drunk driving, drinking in the public way, and disorderly conduct.

7.3. Police behavior

Financial assistance may affect arrests by changing police behavior rather than by changing criminal activity. In theory, the police might respond in either direction to those receiving financial assistance. Police officers may target homeless individuals because they live in the open. Similarly, crimes committed by those in homeless shelters might be more likely to lead to an arrest if shelter staff are aware of and report these crimes. In scenarios such as these, arrests would be lower for those receiving financial assistance not because of a decrease in criminal behavior but because of a lower probability of arrest given any level of criminal behavior. On the other hand, the police might be unable to find unstably housed people because they move frequently, which would make us understate the reduction in criminal behavior.

We can test these hypotheses in our data using warrant arrests. Warrant arrests indicate times when the police arrest a person for a warrant issued by a judge for a past violation. Warrant arrests are quite common, making up 10% of all arrests in our data. The vast majority of warrant arrests were due to bench warrants (98.5%), which are generally issued when a defendant fails to appear in court. Bench warrants are typically given low priority and they often take extensive time to resolve. The median number of days reported to close a bench warrant for failure to appear is 29 days (Reaves and Perez, 1994).[14] Hence, warrant arrests in the months just after calling likely reflect arrests for failure to appear in court before the call and, therefore, before the realization of the income shock and treatment status. We should be able to observe whether financial assistance affects police behavior by examining warrant arrests in this period just after the call. Fig. 7 shows the effect of fund availability on warrant arrests over time. There is no clear difference between those with and without funds available, particularly in the first few months after the

[13] In our data, these charges mainly fall in three categories: trespassing (87%), prohibited forms of selling/panhandling (8%), and public urination/defecation (3%).

[14] The average number of days to close a bench warrant is in fact much higher, as there are some fugitives who never return to custody.

Table 7
OLS estimates of the effect of fund availability on arrests, by detailed type and location.

	(1)	(2)	(3)	(4)	(5)	(6)
	All 1 year	All 3 years	Single 1 year	Single 3 years	Families 1 year	Families 3 years
Panel A: effect of fund availability on homelessness-related crime						
Homelessness-related	−0.002	−0.002	−0.004	−0.008	−0.001	0.002
	(0.002)	(0.003)	(0.004)	(0.005)	(0.002)	(0.003)
Homelessness-related, outside	−0.003[b]	−0.003	−0.005[a]	−0.010[b]	−0.002	0.000
	(0.001)	(0.002)	(0.003)	(0.004)	(0.001)	(0.002)
Panel B: effect of fund availability on property crime						
Property	0.0021	0.0101[c]	−0.0042	0.0035	0.0055[b]	0.0131[c]
	(0.0024)	(0.0037)	(0.0047)	(0.0068)	(0.0027)	(0.0046)
Property, larceny	0.0002	0.0075[b]	−0.0041	0.0008	0.0028	0.0111[c]
	(0.0021)	(0.0032)	(0.0041)	(0.0059)	(0.0023)	(0.0039)
Larceny, inside commercial	0.0008	0.0079[c]	−0.0015	0.0033	0.0028	0.0108[c]
	(0.0018)	(0.0028)	(0.0033)	(0.0051)	(0.0021)	(0.0036)
Larceny, inside residential	−0.0003	−0.0006	−0.0012	−0.0022[a]	0.0000	−0.0001
	(0.0003)	(0.0005)	(0.0010)	(0.0013)	(0.0001)	(0.0005)
Larceny, outside	−0.0011	−0.0003	−0.0022	−0.0026	−0.0009	0.0006
	(0.0011)	(0.0016)	(0.0024)	(0.0030)	(0.0011)	(0.0018)
Property, not larceny	0.0020[a]	0.0022	−0.0000	0.0015	0.0029[b]	0.0022
	(0.0012)	(0.0021)	(0.0022)	(0.0037)	(0.0014)	(0.0026)
N	8655	8655	3021	3021	5634	5634

Notes: Results are for our main sample of eligible first-time calls within the last six months for rent, security deposit, utility, and other assistance, January 20, 2010–September 14, 2012. See text for additional restrictions. Each cell shows the coefficient on funds availability from a separate regression. The outcome is a dummy for being arrested for the listed type of crime within the listed timeframe. Heteroscedasticity-robust standard errors are in parentheses. For a list of control variables, see notes to Table 4.

[a] Significant at 10%.
[b] Significant at 5%.
[c] Significant at 1%.

call. Thus, we find no indication in the data that financial assistance changes police behavior or their ability to locate offenders.

7.4. Potential explanations for increased property crime arrests

The bottom panel of Table 7 reports the effect of fund availability on arrests for different types of property crime. These results indicate that larceny arrests account for nearly all of the increase in arrests of family heads for property crime. By far, the most common charge for larceny is retail theft. This delayed increase in shoplifting is not consistent with the prediction that property crime should decrease in response to insuring an income shock.

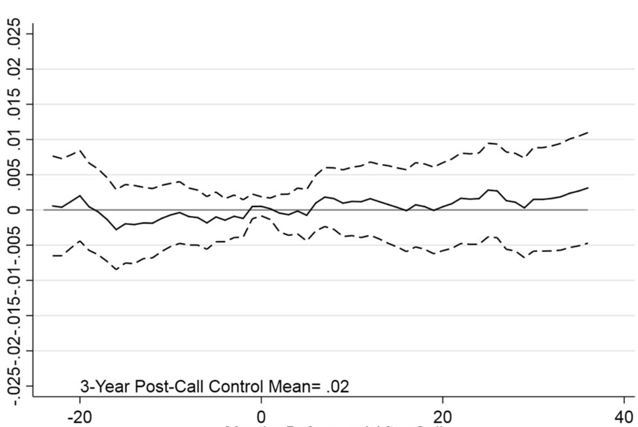

Fig. 7. Effect on warrant arrests. *Notes:* the solid line plots the coefficient on fund availability in a regression where the outcome is a dummy for ever arrested in the τ months before or after calling. To the left of zero, the outcome is a dummy for ever arrested in the τ months before calling, multiplied by -1 (so that an upward slope indicates more arrests τ months before the call.) The regression includes a fund availability dummy and controls, as in Eq. (2). See the notes of Table 4 for a list of controls. The dashed lines show 95% confidence intervals with robust standard errors.

Property crime could increase if emergency assistance allows households to take on financial burdens that some households struggle to repay later. Emergency financial assistance keeps tenants in existing rental contracts or guarantees new rental contracts with a security deposit. While the assistance insures the current shock, tenants may experience shocks in the future that again prevent their ability to pay. gtWith financial assistance no longer available, tenants could turn to property crime to supplement income and/or non-housing consumption, allowing them to pay rent. Such a mechanism seems plausible given what is known about shoplifting. Industry sources report that the most-shoplifted items include expensive food and items that can be resold: health/beauty products, meat, liquor, razor blades, baby formula, and over-the-counter painkillers (Food Marketing Institute, 2009).

If true, this mechanism should be particularly apparent for people who request a security deposit to support a new rental contact. Security deposit assistance allows tenants to incur the obligation of a full, new rental contract, and the contract will take effect shortly after the call. These tenants will be most vulnerable at contract renewal, likely 12 months later, when the landlord can more easily remove a tenant behind on rent. Fig. 8a tests this theory by showing the effect of fund availability on property crime arrests for family heads by type of assistance requested. As predicted, those requesting security deposits experience the largest increase in property arrests. Moreover, we see a pronounced increase in the effect on property crime arrests right around 12 months after the call, which is the time when lease agreements would be expected to expire. Increases in property crime arrests for those requesting rental assistance are smaller and accumulate more gradually, which matches lease renewal dates which are scattered throughout the following years. While this evidence is only suggestive, it is consistent with the idea that financial assistance enables families to take on financial obligations, and some small fraction of family heads turn to shoplifting when they cannot meet these obligations.

However, any delayed hardship experienced by families appears to be relatively small. Previous research shows that financial assistance has a persistent effect on homelessness—lower entry rates into emergency shelters persist for multiple years (Evans et al., 2016). In addition,

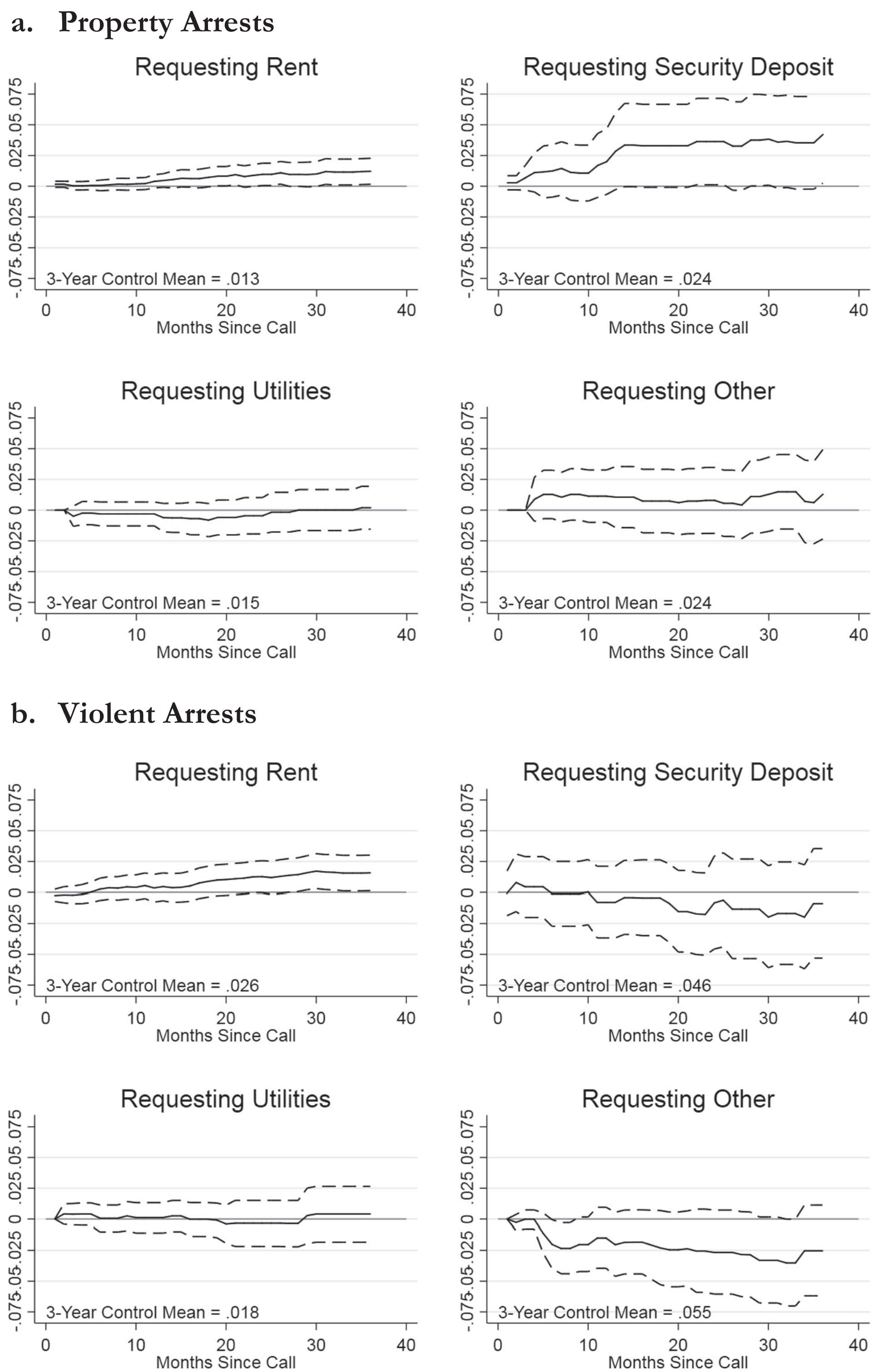

Fig. 8. Effects of financial assistance on arrests for family heads, by request type. *Notes:* the solid line plots the coefficient on fund availability in a regression of a dummy for ever arrested in τ months since calling on a fund availability dummy and controls, as in Eq. (2). See the notes of Table 4 for a list of controls. The dashed lines show 95% confidence intervals with robust standard errors. The four panes split the full sample by type of assistance requested. The sample is limited to heads of families.

we find no indication that violent crime arrest rates increase for family heads receiving security deposit assistance, in general or at 12 months (Fig. 8b). If emergency financial assistance simply "kicked the can down the road" by delaying the solution of long-term problems, we would expect to observe shelter entry effects that decay and a spike in all types of crime at 12 months. Instead, we only observe a spike in shoplifting.

8. Conclusion

Providing temporary financial assistance to people facing adverse shocks can reduce violent crime. We identify a group of Chicago residents who experience a negative shock and request financial assistance from the Homelessness Prevention Call Center. Because the availability of funding varies unpredictably from day to day, funds are available for some eligible callers but not for others. We match caller information for both groups to arrest records from the Chicago Police Department and test whether the police arrest people for whom funds are available at a different rate than those for whom funds are not available. We find some evidence that calling when funding is available reduces the overall likelihood of being arrested within 1 to 2 years of the call, and this effect is marginally significant. The effect is strongest for violent crime; arrest rates within a year of the call for these most serious crimes are 0.87 percentage points (51%) lower for those for whom same-day funds are available. Moreover, this effect persists; the effect of fund availability on violent crime after three years is similar to the effect after the first year. A reduction in arrests of single individuals for battery drives most of the decrease in violent crime. We find some evidence that financial assistance leads to less violent crime because it increases housing stability. On the other hand, arrests for property crime increase after a 1-year delay if funds are available. Shoplifting among family heads drives most of the increase in property arrests. While assistance helps families stabilize housing on average, we find suggestive evidence that some small proportion of callers eventually have difficulty paying rent and shoplift to make ends meet. Overall, we find that offering financial assistance shifts arrests away from violent crime towards property crime.

Changing the mix of crime generates significant public benefits. Consider a rough valuation of crime-reduction benefits per person offered funds. Available funds reduce arrests for violent crime by 0.86 percentage points per person over 3 years, mostly due to fewer assaults and batteries. Adjusting for the gap between incidents and eventual arrests implies a larger decrease in crime. National data show that only 48% of assaults are reported to police (Planty and Truman, 2011) and in Illinois only 37% of reported assaults can be associated with an arrest (Illinois State Police, 2011). Thus, 0.0086 fewer arrests imply roughly 0.048 fewer assaults and batteries committed per person matched with available funds. Taking into account the cost of assistance, overhead operating costs, and adjusting for imperfect take-up of assistance, the average cost of referring an HPCC caller in our sample to funding is $806. Thus, the HPCC spends $16,644 to avoid one assault. Standard values from the literature place the benefits to victims at nearly double this value. Victim costs from Miller et al., 1993 inflated by the consumer price index to 2012 indicate that avoiding one assault saves $28,018 in victim costs. We do observe a roughly 1-for-1 replacement of assault with shoplifting, but the social benefits of reducing violent crime dominate. In our data, the most common larceny charge is shoplifting of less than $150 and the vast majority of larceny charges are for stealing less than $500. Industry sources indicate that the average loss per shoplifting incident in 2015 was less than $400 (National Retail Federation, 2016). Even a generous accounting for shoplifting incidents would place their social cost far below the benefits from violence reduction. The benefits to victims of crime alone can justify the cost of temporary financial assistance.

Thus, we show that insuring households against shocks can create significant external benefits by reducing crime. Importantly, these benefits accrue to crime victims rather than the original recipients of funding. In addition to these benefits, such assistance can also benefit recipients by increasing housing stability as has been shown in previous work.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jpubeco.2018.10.012.

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