UNIVERSAL CASH AND CRIME

Brett Watson, Mouhcine Guettabi, and Matthew Reimer*

Abstract—We estimate the effects of universal cash transfers on crime from Alaska's Permanent Fund Dividend, an annual lump-sum payment to all Alaska residents. We find a 14% increase in substance-abuse incidents the day after the payment and a 10% increase over the following four weeks. This is partially offset by an 8% decrease in property crime, with no changes in violent crimes. On an annual basis, however, changes in criminal activity from the payment are small. Estimated costs comprise a very small portion of the total payment, suggesting that crime-related concerns of a universal cash transfer program may be unwarranted.

I. Introduction

UNIVERSAL basic income (UBI) has gained renewed attention in recent years in response to declining job security and for addressing distributional welfare issues more generally (Thigpen, 2016). UBI constitutes a universal and unconditional cash transfer that is provided to all residents (or citizens) on a long-term basis, regardless of income, with no strings attached (Marinescu, 2017). Proponents primarily describe UBI's ability to improve economic security (Thigpen, 2016), while others have proposed it as a substitute for existing welfare programs. A common concern regarding UBI, however, is that universal cash transfers may have unintended consequences. For example, several previous studies have found that cash transfer programs increase crime, mortality, and the consumption of "temptation goods," such as drugs and alcohol (Riddell & Riddell, 2006; Dobkin & Puller, 2007; Evans & Moore, 2011; Borraz & Munyo, 2015; Castellari et al., 2017). However, other studies have found no effect on mortality and reductions in crime (Foley, 2011; Mejia & Camacho, 2013; Cotti, Gordanier, & Ozturk, 2016; Carr & Packham, 2019), suggesting that the type of cash transfer and the setting in which cash transfers take place matters. Previously studied cash transfers stem from conditional cash or in-kind transfer programs, such as the Supplemental Nutrition Assistance Program (SNAP); universal and unconditional cash transfers, however, are distinct from these programs because the entire population receives the transfer and there are no restrictions on how recipients spend the payments. Criminal behavior may therefore respond differently to UBI than to conditional cash and in-kind transfers estimated previously.

In this paper, we provide the first estimates of the effects of a universal and unconditional income receipt on crime using the world's only continuous universal income program—Alaska's Permanent Fund Dividend (PFD)—as a case study. The PFD is an annual and unconditional lump-sum payment to Alaska residents based on the investment earnings of the Alaska Permanent Fund, the state's sovereign wealth fund, and provided to all Alaska residents (subject to eligibility rules), regardless of income. We estimate the short-run effects of the PFD on daily counts of policing incidents related to violence, controlled-substance abuse, property crime, and requested medical assistance using police reports in the Municipality of Anchorage, Alaska's largest city, between 2000 and 2016. We exploit the exogenous timing and amount of the PFD payment to identify the average treatment effect of the PFD on the daily counts and type of incidents. We find payment receipt increases the average daily number of substance-abuse incidents (10%) and incidents of police medical assistance (9%), but decreases property crime incidents (8%) in the four weeks after the PFD is issued, with no average change in violent crimes. Although these changes are statistically significant, they represent modest changes on an annual basis. The observed increase in substance-abuse crime is 1.05% of the annual level, while the declines in property crime are similarly modest at −0.61%. In terms of monetized social cost, the net effect of these changes ranges between social savings of \$328,000 to expenditures of \$3.44 million, amounting to just +0.17% to −1.78% of the 2016 PFD distribution to Anchorage residents.

The primary implication of the life cycle model with a perfect credit market is that consumption and economic behavior should not respond to the arrival of anticipated income. Recent work demonstrates, however, that people tend to exhibit short-run impatience, whereby consumption and economic activity increase immediately following cash transfers, thereby violating the permanent income hypothesis (Stephens, 2003; Shapiro, 2005; Stephens & Unayama, 2011; Kueng, 2018). Short-run impatience implies that cash transfers constitute income shocks, which may influence the decision to engage in crime. According to Becker's (1968) seminal model of crime, an individual is less likely to engage in illicit activity if his or her opportunities for legitimate income are improved from higher wages or better employment prospects. This relationship between earned income and crime has received considerable empirical support (Machin & Meghir, 2004; Lin, 2008; Blakeslee & Fishman, 2018; Bignon, Caroli, & Galbiati, 2017). In contrast, the theoretical relationship between unearned income and crime is less clear because, unlike earned income, the decision to engage in crime does not necessarily result in forgone unearned income. Thus, an unearned income shock could lead to more or less criminal activity, depending on the mechanisms at play and how they influence the expected utility of engaging in criminal activity.

In the presence of short-run impatience and credit constraints, the arrival of an unearned income shock from a cash transfer may influence the expected net utility of criminal activity through several mechanisms. For example, an income effect from the cash transfer could relieve financially stressed

Received for publication May 16, 2018. Revision accepted for publication January 23, 2019. Editor: Brigitte C. Madrian.

*Watson, Guettabi, and Reimer: University of Alaska Anchorage.

A supplemental appendix is available online at http://www.mitpressjournals.org/doi/suppl/10.1162/rest_a_00834.

The Review of Economics and Statistics, October 2020, 102(4): 678–689

https://doi.org/10.1162/rest_a_00834

individuals, thereby reducing the need to engage in financially motivated crimes, such as burglary, robbery, and theft (Foley, 2011; Mejia & Camacho, 2013; Chioda, De Mello, & Soares, 2016; Carr & Packham, 2019). The income effect can also act in the opposite direction as cash recipients increase their consumption of normal goods, including those that are complements to crime, such as leisure, drugs, or alcohol (Riddell & Riddell, 2006; Dobkin & Puller, 2007; Evans & Popova, 2014; White & Basu, 2016; Castellari et al., 2017). A cash transfer may also increase the supply of cash and purchased goods available to potential offenders in the streets, thereby increasing the expected utility of crime through a "loot effect," leading to an increase in financially motivated crimes (Borraz & Munyo, 2015; Wright et al., 2017). The expected utility of crime may also increase from a cash transfer through a peer effect if several individuals are receiving the transfer at the same time, thereby creating a social multiplier in crime (Damm & Dustmann, 2014; Billings, Deming, & Ross, 2016). Finally, if the day of a cash transfer is a highly anticipated and salient event, it may also generate a "party effect," much in the same way college football game days lead to an increase in assaults, vandalism, arrests for disorderly conduct, and arrests for alcohol-related offenses (Lindo, Siminski, & Swensen, 2018).

While there is ample support of crime-related effects from cash transfers (Foley, 2011; Mejia & Camacho, 2013; Borraz & Munyo, 2015; Cotti et al., 2016; Chioda et al., 2016; Carr & Packham, 2019), UBI payments are distinct from other payment types—such as in-kind benefits, conditional cash transfers, public pensions, or unemployment insurance—in several respects and may therefore induce different behavioral responses to cash transfers from those estimated in previous studies. UBI recipients constitute a broader and more diverse socioeconomic group relative to the segments of the population considered previously, such as the elderly (pension/social security payments) or low-income earners (SNAP payments), which likely differ in their income levels, time preferences, and consumption behaviors. We provide evidence consistent with a differential per dollar response for substance abuse crimes when the PFD is compared to SNAP payments.

Finally, our findings lend insight into universal payment designs. We show that while property crimes decrease as a result of PFD payment, there are no additional societal gains—that is, more decreases in property crime—from the distribution of larger amounts. Substance-abuse incidents, however, are responsive to both PFD amounts and increases in the distribution. Thus, increased benefits from UBI payments may be obtained by spreading out payments over multiple installments, as was found for SNAP payments (Carr & Packham, 2019). However, the overall net effect of PFD payments on crime is relatively small at the annual level, suggesting that crime-related concerns of a UBI program may be unwarranted.

The remainder of this paper is organized as follows. Section II presents a brief history of the Alaska Permanent Fund dividend and why it represents a fruitful setting for empirical research on UBI. Sections III and IV describe our data and the empirical strategy we used to estimate the effect of the cash transfers on crime. Section V presents the primary findings of the analysis: the immediate effect of the PFD on crime the day after distribution, the persistence (or longevity) of the effect, the relationship between crime and the size of the cash distribution, and a comparison of the PFD-related effect to the effect of other transfers (SNAP) within Alaska. We discuss the implications of our findings for the literature on cash transfer and the policy debate around UBI in the final two sections.

II. The Alaska Permanent Fund Dividend

In 1976, Alaskan voters passed a constitutional amendment to establish the Permanent Fund (Alaska Constitution, Article IX, Section 15). This amendment dedicated a portion of the annual oil revenues to a state investment fund, whose balance currently stands at over $61 billion. When the initial fund was created, there was no intention to share earnings with the public; however, interest in a citizen dividend eventually gained traction, and in 1982, the first PFD was paid to every resident of the state of Alaska. Since the first distribution, PFD payments have been determined by a formula that is based on a five-year rolling average of the fund's income to produce more stable dividend amounts from year to year. It is important to note that the fund is well diversified across different regions and asset classes; thus, its returns are not necessarily reflective of Alaska's economic conditions. State oil revenue, which originally capitalized the fund, currently represents only 2% to 3% of annual fund additions; since 1985, reinvestment of fund earnings is the primary way in which the fund grows. The average annual aggregate distribution is large enough to be similar in size to the gross domestic product of many sectors in the Alaska economy. In 2015, for example, the $976 million distribution was about 42% of the construction sector's GDP, or 76% of the wholetrade sector's. In addition to the sheer size, PFD payments are distributed to everyone at the same time, which means it is the single largest infusion of money at a given point in Alaska's $50 billion economy.

As a case study in universal income, the dividend established an income floor below which the cash income of residents cannot fall. This cash transfer is particularly important in rural areas where economies lack economic bases and are still a mixture of subsistence and a small formal economy. Another key feature of the PFD is that amounts are not based on a person's income or wealth. Payments are uniformly distributed to all residents—adults and children–of the state (including green card holders and refugees) who were residents of the state in the prior year. Over our study period (2000–2016), average household size was 2.83, average household income was $72,000, and average PFD size per person was $1,600. That means that the PFD represents, on average, 6.28% of overall household income. By coincidence, this is almost exactly the share of earnings generated by the first PFD in 1982. Since inception, the program has become very popular, and the public expects it to run in perpetuity.

Each year, the filing period runs from January 1 to March 31. This leaves the Permanent Fund division about six months to process the applications, determine eligibility, and handle garnishment requests. The payout month therefore is a result of administrative processes, as opposed to any intentionality on the behalf of the founders of the dividend. The vast of majority of Alaskans—82.72% as of 2014—receive their PFDs through direct deposit in the first week of October, while the rest receive checks through the mail. Over our study period, direct deposits have always been issued either before or on the same day checks are mailed. More recently (since 2010), both direct deposits and checks have been issued on the first Thursday of October. Because of the relatively small portion of the population receiving mailed checks, and because these checks are never issued before direct deposits, our primary specifications focus on the first round of direct deposit issuance.

III. Data

We employ a database on reported policing incidents in Alaska's largest city, the municipality of Anchorage. Limiting the study to Anchorage is not particularly narrow in scope, as the city accounts for a large share (40%) of the state's total population.[1] The primary data for the analysis are real-time incident reports for officers of the Anchorage Police Department (APD). An incident report is generated each time an officer calls to report his or her location and the nature of that officer's current activity to dispatch. Such reports can be made, for example, when an officer responds to a 911 call, initiates a traffic stop, services a warrant, or even reports a meal break. Each time-stamped log entry is associated with a particular activity, self-reported by the officer, and coded to one of 99 possible activities by the APD.

APD provided us with location deidentified incident reports for the years 2000 to 2016. For our analysis, we aggregate these incident report data up to counts at the day level for each activity code. Drawing from the categorization of the Federal Bureau of Investigation's Uniform Crime Report (UCR) and with consultation from APD, we further categorize and aggregate these specific codes to more general activity types corresponding to violence, substance abuse, property crimes, noise violations and parties, and police department medical assistance to other agencies. Our aggregation differs from UCR in three ways. First, UCR reports robbery as a violent crime, whereas we categorize it as a property crime (because it is financially motivated). Second, UCR includes arson as a property crime, but because it does not necessarily provide direct financial gain to the perpetrator, we omit it. Finally, UCR does not track substance abuse incidents. Therefore, in consultation with APD, we determined six incidents to be associated with substance abuse: hit-and-run events, liquor law violations, problems with a drunk individual, police transportation of a drunk individual (often for another city agency), drugs and forged prescriptions, and driving while intoxicated. Hit-and-run violations have been shown to be associated with alcohol consumption (Solnick & Hemenway, 1994). Liquor law violations tend to be associated with illegal possession or sale of alcohol rather than excessive consumption. For robustness, in appendix tables, we provide estimates for both an inclusive (Full) and restrictive (Part) categorization of substance abuse incidents that differ by including hit-and-run and liquor law violations. Because these yield qualitatively similar results, the remainder of the paper presents the results only for Substance (Full), which are inclusive of all incidents.

Appendix table A.1 shows the average daily count of calls received over our sample period for specific call codes and the more general categories to which they were assigned. Figure A.1 shows these activities by day of week and month of year. Predictable weekday-weekend and summer-winter patterns are visible in most of the series. For example, substance abuse calls peak on the weekend, while property crime is low on the weekend because people are spending time at home.

IV. Empirical Strategy

Our empirical strategy exploits two sources of temporal variation to examine the PFD's effect on crime. First, we use the discrete intra-annual variation on the day the PFD is issued by comparing daily behavioral outcomes from the days immediately following the PFD to similar days of the year that do not experience cash transfers of any kind. Given that the time of year in which the PFD is issued is determined only by administrative processes, the annual timing of the PFD is exogenous;[2] thus, similar days of the year that do not experience cash transfers are plausible estimates of the counterfactual of what behavioral outcomes would have been had the PFD not been issued. A useful feature of this source of variation is that PFD payments have occurred on different days of the month and different days of the week over the years in our sample. With variation in the day of month, the PFD can be isolated from the effect of other income payments and transfers that occur on a regular schedule every month.[3]

[1] In 2016, Anchorage's population stood at just under 300,000, with another 150,000 in the larger metropolitan area. Alaska as a whole recorded 750,000 residents in 2016. Fairbanks, Alaska's second largest city, has just one-tenth the population of Anchorage. (www.census.gov/data/datasets/2016/demo/popest/total-cities-and-towns.html).

[2] This observation is based on personal communications with current and former government officials involved in the creation of the PFD program.

[3] These transfers include food stamps and Temporary Assistance for Needy Families (TANF), which are distributed on the first day of each month in Alaska; military paychecks, which are distributed on the first and fifteenth of each month (or the nearest business day prior); and social security checks, which are distributed on the second, third, and fourth Wednesday of each month. Most salaries in the United States are paid weekly or biweekly (www.bls.gov/opub/btn/volume-3/how-frequently-do-private-businesses-pay-workers.htm), creating day-of-the-week, rather than day-of-the-month, patterns.

TABLE 1.—PFD DIRECT DEPOSIT DATES, NUMBER OF DEPOSITS, AND AMOUNTS, 2000–2016

Year	Direct Deposit Date	Day of Week	Number of Direct Deposits	% Full[a]	PFD Amount per Person	% Deposits Received First Day	Total Cash First Day (millions)
2000	4-Oct	Wed	390,312	96%	2,737	100%	1,030
2001	10-Oct	Wed	404,247	96%	2,508	100%	970
2002	9-Oct	Wed	424,490	97%	2,056	100%	844
2003	8-Oct	Wed	444,268	94%	1,445	100%	605
2004	12-Oct	Tue	448,642	94%	1,169	100%	491
2005	12-Oct	Wed	459,004	94%	1,039	100%	448
2006	4-Oct	Wed	476,775	93%	1,318	39%	227
2007	3-Oct	Wed	493,997	93%	1,915	45%	395
2008	12-Sep	Fri	497,739	92%	3,644	100%	1,670
2009	8-Oct	Thu	514,217	93%	1,460	100%	702
2010	7-Oct	Thu	527,868	92%	1,410	100%	684
2011	6-Oct	Thu	523,756	91%	1,253	100%	594
2012	4-Oct	Thu	518,334	90%	918	100%	429
2013	3-Oct	Thu	512,955	89%	927	100%	426
2014	2-Oct	Thu	518,986	88%	1,910	100%	874
2015	1-Oct	Thu	532,672	87%	2,098	100%	976
2016	6-Oct	Thu	534,156	89%	1,022	100%	484
Mean			483,672	92%	1,966	93%	697

PFD dates, number of recipients, and amounts come from Alaska Department of Revenue's Permanent Fund Dividend Annual reports. Total dispersed on first day (in 2016 USD) are our calculations based on fully untarnished payments made to recipients on the first payout date. Dollar values are real 2016 USD.
[a]The fraction of ungarnished payments issued. Garnishments may be involuntary (e.g., child support or uncollected government fees) or voluntary (e.g., tax-exempt college savings or charitable contribution).

With variation in the day of week, the PFD can be separately identified from other regular weekly patterns, such as the effects of the weekend versus the weekday. As a second source of variation, we use the interannual variation in the size of the PFD payment to provide additional identification and explore whether behavior is sensitive to the amount received. As previously discussed (section II), the size of the PFD is determined based on the returns of a diversified portfolio rather than on contemporaneous oil prices or specific factors related to the state economy. PFD payment dates, number of recipients, and amounts are listed for each year in our study period in table 1.

We estimate four empirical model specifications to investigate different aspects of the effect of the cash payment on daily criminal activity. First, we estimate an empirical model that leverages the exogenous timing of the payment only:

$$y_t = \beta_0 + \beta_1 PFD_t + \gamma W_t + M_t + \tau_t + \epsilon_t, \tag{1}$$

where y_t is the count of policing incidents on day t related to either violence, substance abuse, property crime, noise or parties, or police medical assistance to other agencies (each estimated separately); PFD_t is a dummy variable taking a value of 1 if t equals the date of the first PFD distribution and 0 for all other days, and ϵ_t is the model error. The β_i and γ coefficients are parameters to be estimated. The coefficient of interest, β_1, is the estimated change in the number of incidents y on the first full day after PFD direct deposits are issued.[4] We estimate equation (1) via OLS using heteroskedasticity- and autocorrelation-consistent Newey-West estimates of the covariance matrix (Newey & West, 1987) to address any persistence in the time series after accounting for month-by-year fixed effects.[5]

The specification in equation (1) includes a number of control variables and fixed effects. M_t is a vector of month × year (month-by-year) fixed effects, which captures changes to average incomes, unemployment, population, police department resources, and other similar effects. Following the daily crime literature (Jacob & Lefgren, 2003; Foley, 2011), W_t represents a vector of weather control variables (precipitation, maximum daily temperature, and snow depth), which reduces observed variance and enables more precise estimates. Most of the seasonal weather effects, however, are captured by the month-by-year fixed effects. τ_t is a vector of special day, holiday, day-of-week, and day-of-month effects. Specifically, τ includes day-of-week fixed effects (e.g., Monday, Tuesday), addressing weekend-versus-weekday effects or intraweek cyclicality, and a fifth-order polynomial day-of-month trend to account for events that occur on a regular monthly schedule, such as rental payments and other income receipts (e.g., social security, food stamps, or other welfare receipt). τ also includes a vector of special day dummy variables, including military paydays (to account for the predictable pay of Anchorage's large military population), New Year's Eve/Day, Super Bowl Sunday, the day of the Iditarod race start in Anchorage, St. Patrick's Day, Cinco de Mayo, July Fourth, Labor Day and Labor Day weekend, Columbus Day, Halloween/preceding weekend, Thanksgiving, Christmas Day, and federal holidays given to many public employees if a major holiday falls on a weekend. Much like

[4]For example, if the PFD is issued at sometime on Thursday, β_1 will capture the effects of PFD_t from Friday at 12:00 a.m. to 11:59 p.m.

[5]Poisson models were also fitted to address the count nature of the data but yielded similar results. These estimates are included in the appendix table A.4. We address testing multiple hypotheses for our five outcome variables by using the Bonferroni correction method. Less conservative corrections were also tested for our main results but yielded similar inference.

weather, including special date indicator variables allows for more precise effect estimates by reducing the variance in the counterfactual days.

In our second model specification, we investigate whether there is any persistence in the PFD effect beyond the first full day following the distribution by adopting an event-analysis strategy similar to Evans and Moore (2011): we broaden the time window for the indicator variable PFD_t in equation (1) to one-week intervals, from four weeks before the PFD distribution to four weeks after. Persistence of the PFD effect is estimated from equation (2),

$$y_t = \beta_0 + \sum_{i=-4}^{4} \beta_i PFD_{it} + \gamma W_t + f(t) \times Year + \tau_t + \epsilon_t, \tag{2}$$

where PFD_{it} is a dummy variable taking a value of 1 if day t is in the ith week before/after distribution and 0 otherwise. We define weeks as the seven-day periods starting from the day the PFD is distributed.[6] Weather and holiday controls, W and τ, are defined as in equation (1). The eight-week window around the PFD distribution date leaves relatively few "untreated" October days to offer independent variation for estimating the monthly fixed effect; instead, we use a fifth-order day-of-year polynomial trend, $f(t)$, to capture seasonal variation in the observed activity patterns through channels such as income, unemployment, and police department resources. We estimate such a trend for each year by interacting it with a yearly fixed effect.

Assessing the persistence of the effect of the PFD on our outcomes allows us to determine if the payment has a net cumulative impact or if the change in behavior immediately following receipt merely represents an intertemporal reallocation. In order to avoid conflating the persistence of first-day recipients (who received PFD by direct deposit) with the effect of new recipients who receive their PFDs later by check, we narrow our sample period for the persistence estimates to the years 2010 to 2016, where direct deposits and checks were issued to all recipients on the same day.[7]

An additional motivation for looking at an extended treatment period is that while the first full day after the PFD distribution experiences particularly intense treatment, the one-day treatment window may be too short to capture effects that are confined to particular days of the week. For example, Mondays have the highest daily number of property crime calls, but no Monday falls the day after a PFD payment. This limits the potential impact of the treatment effect to days that property crime might be otherwise depressed.

For our third empirical specification, we estimate the marginal response to the size of the distribution, which additionally leverages the dollar amount paid as a source of identification. As shown in table 1, PFD amounts have varied considerably year-to-year, both in the amount that each recipient is paid (between \$918 and \$3,644 in 2016 dollars) and in the number of individuals who receive their PFD as part of the first round of direct deposit payments. Consequently, the total amount of cash hitting the street on the first day provides an opportunity to investigate how changes in the total size of the distribution relate to changes in our estimated effects. These marginal effects are relevant for policy, as they potentially inform potential gains from more evenly distributing payments over time (either across or among individuals). To test for potential response to distribution size, we estimate the model in equation (3),

$$\begin{aligned} y_t = \beta_0 + \beta_1 week_t^{PFD} + \beta_2 week_t^{PFD} \times amount_t \\ + \beta_3 week_t^{PFD} \times amount_t^2 + \gamma W_t + M_t + \tau_t + \epsilon_t, \end{aligned} \tag{3}$$

where $week_t^{PFD}$ is a dummy variable taking a value of 1 if t occurs during the week after distribution and 0 otherwise, and $amount_t$ is the total amount of cash dispersed on the first day (PFD amount $\times$ number of first-day recipients) measured in \$100 million 2016 USD (table 1). The other variables are defined as in equation (1). Main effects for *amount* are not included because there is no intra-annual variation in the amount of the PFD payments or first-day recipients; both will be perfectly correlated with month-by-year fixed effects.

Finally, to provide context for our PFD estimates, our fourth specification compares the effect of the PFD to changes in police activity stemming from a transfer payment that has been the focus in previous studies: food stamp (SNAP) payments. We single out the SNAP program as it is an important social program that provides a predictable and steady stream of benefits to income- and asset-eligible households. SNAP has a large base of participation and, unlike other programs like Temporary Assistance for Needy Families, payments do not end after a given amount of time. Several previous studies have analyzed the short-run effects of income or in-kind transfers using programs such as SNAP (Shapiro, 2005; Cotti et al., 2016) or social security payments (Stephens, 2003; Mastrobuoni & Weinberg, 2009; Evans & Moore, 2011). Particularly relevant for our study, Foley (2011) and Carr and Packham (2019) examine the relationship between crime and the timing of SNAP. However, such transfer programs have important limitations when considering their implications for universal income. Social security payments are restricted to the elderly and SNAP payments to those near or below the poverty level. Differing consumption patterns may be expected from these subgroups than for the population as a whole. Further, SNAP provides in-kind benefits that can only be spent on certain grocery items. Because universal income is sometimes discussed as a substitute or complement for more traditional welfare programs, comparing the behavioral

[6]In other words, the first week after distribution, $i = 1$, includes days 1 to 7 after distribution; days 8 to 14 correspond to week $i = 2$, and so on. The first seven days *before* the PFD defines week $i = -1$; days 8 to 14 before the distribution define week $i = -2$, and so on.

[7]In most years in the 2000–2009 period, paper checks were mailed one week after direct deposits were issued. For robustness, we also estimate equation (2) on the full sample (table A.6).

responses of PFD receipt to those for such welfare programs provides insight into how these programs might differ. To our knowledge, no studies to date have explored the differential effect of cash transfers on crime between universal and nonuniversal transfer programs on a per person basis.

For this comparison, we exploit both the discrete first-day-of-the-month timing for Alaskan SNAP payments and the total size of these monthly distributions. The per household SNAP benefits in Alaska depend on household size, income, and location (urban, rural, or remote). Over our study period, statewide SNAP participation was approximately 65,000 individuals, or about 9.5% of the state's population. On average, about \$10 million is paid in total benefits across the state each month, with an average per person benefit of about \$150 per month. We compare SNAP to PFD through estimation of equation (4),

$$y_t = \beta_0 + week_t^{PFD}(\beta_1 + \beta_2 amount_t^{PFD}) + week_t^{SNAP}(\beta_3 + \beta_4 amount_t^{SNAP}) + \gamma W_t + M_t + \tau_t + \epsilon_t \quad (4)$$

where $week_t^{PFD}$ is a dummy taking a value of 1 when day t falls in the week after PFD distribution and 0 otherwise, $amount_t^{PFD}$ is the amount of PFD distributed as part of the first direct deposit (in millions of 2016 USD) measured in deviations from the average amount, $week_t^{SNAP}$ is a dummy taking a value of 1 when day t is in the week following Alaskan SNAP distribution, and $amount_t^{SNAP}$ are the SNAP payments made to Alaskans for the month that t falls measured in deviations from their average.[8] As before, W is a vector of weather control variables, M_t is a vector of month-by-year effects, and τ is a vector of special dates and day-of-week effects. For this specification, we drop the day-of-month polynomial trend that captures intramonth cyclicality since we want to instead estimate this variation as part of $week^{SNAP}$. The interactive effect of $week_t^{SNAP} \times amount_t^{SNAP}$ isolates the SNAP payment from other first-of-the-month effects (e.g., other income receipt, rent payments) which would otherwise have the potential to confound identification. Further, an important source of variation in $amount_t^{SNAP}$ is driven by a temporary benefit increase enacted as part of the American Recovery and Reinvestment Act of 2009 that was effective from April 2009 to November 2013 (Hastings & Shapiro, 2018). During this period, Alaska was only mildly affected by the economic downturn facing the rest of the country as oil prices remained at record highs. We estimate the marginal effect of the PFD for only the first week following its distribution since payments to check recipients from 2000 to 2009 period conflate estimation of persistence effect with the effect of new money being dispersed after the first week. The week-long duration for estimating the SNAP effect is chosen based on a combination of the existing literature (Cotti et al., 2016, and Foley, 2011, which use relatively short durations of ten days or less), empirical evidence from our data that suggests SNAP effects dissipate after one week, simplicity, and symmetry to the PFD estimate.

V. Results

A. Day-After Effects of the PFD

The estimated results for equation (1) across the five incident categories of interest are presented in table 2. The coefficient estimate for "First Full Day after PFD Deposit" represents the change in the average number of daily incidents one day after the PFD distribution. For reference, the daily mean and standard deviation for each outcome variable are also presented in the table. For incidents related to substance abuse, there is an increase of approximately six reported incidents after the first PFD direct deposit, an increase of 14.1% over the daily sample average. This result is statistically significant even after correcting for multiple hypotheses testing across our outcomes. We find no statistically significant day-after effects in incidents of violence, property crime, or calls for medical assistance. Incidents of loud parties and noise violations show a decrease of about one incident. However, this result is not robust to model specification or multiple hypothesis test corrections. Compared to the holidays that we estimate as controls, the increase in same-day substance abuse incidents from the PFD distribution is slightly larger than the increase on the Fourth of July and roughly half the increase on New Year's Day/Eve, two holidays that are notorious for excessive consumption of controlled substances.[9]

B. Persistence in the PFD Effect

Estimates of the PFD effect at each week interval, as well as their 95% confidence intervals, using the 2010–2016 sample are presented in figure 1.[10] Daily substance-abuse incidents show significant increases for the first three weeks following distribution. These incidents are approximately 20.2% more frequent than the daily sample average in the first week, 14.1% in the second, and 7.5% in the third. Over the entire

[8]Data for the monthly SNAP distributions come from the the US Department of Agriculture's State SNAP Tables.

[9]Table A.2 in the appendix presents estimates of all holidays and other special days of the year for reference. Appendix table A.3 presents results with a more parsimonious set of month-by-year and day-of-week controls (omitting the day-of-month polynomial, weather controls, and special days); these results yield the same inferences with the exception of party and noise noted above. Appendix table A.7 presents results for two subsample periods, 2000 to 2009 and 2010 to 2016; the latter subsample is used in the persistence analysis. The effect magnitudes across outcomes are roughly equivalent over these two periods.

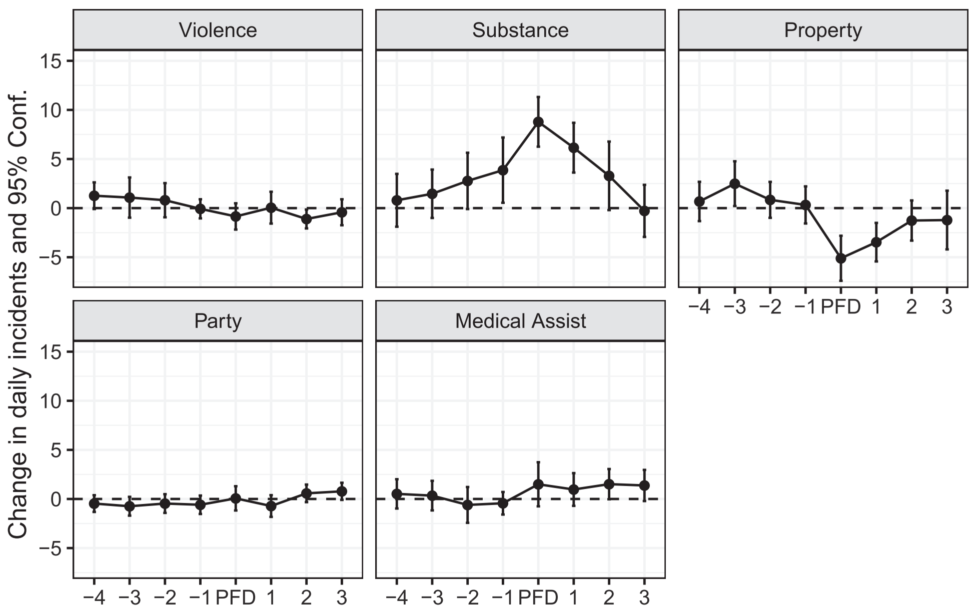
[10]Appendix table A.6 presents the tabular results. Daily-level persistence estimates are presented in figure A.2. Table A.6 also presents results estimated on the full 2000–2016 sample period as a check for sensitivity to the sample period. In the full sample, property crimes see statistically significant reductions for four weeks after the first direct deposit. In contrast, these reductions are statistically significant for only two weeks in the 2010–2016 sample. This result is consistent with our reasoning for narrowing the sample: the longer-lived reductions in the full sample could be due to new individuals receiving their payments for the first time by check and not the lasting effects of the first distribution.

TABLE 2.—CHANGE IN REPORTED INCIDENTS, FIRST DAY AFTER PFD DISTRIBUTION, 2000–2016

	Change in Daily Incident Count by Category				
	Violence (1)	Substance (2)	Property (3)	Party (4)	Medical Assistance (5)
First full day	−0.087	6.163***	−0.656	−1.022*	0.914
After PFD deposit	(0.948)	(1.964)	(1.504)	(0.603)	(0.885)
P-value	[0.927]	[0.002]	[0.663]	[0.090]	[0.302]
Bonferroni *p*-value	[1.0000]	[0.0086]	[1.0000]	[0.4498]	[1.0000]
Mean daily call count	13.63	43.59	33.70	10.08	14.38
SD call count	4.46	13.45	9.46	6.68	5.70
Weather	Yes	Yes	Yes	Yes	Yes
Holiday effects	Yes	Yes	Yes	Yes	Yes
Day of Week fixed effects	Yes	Yes	Yes	Yes	Yes
Day-of-month polynomial trend	Yes	Yes	Yes	Yes	Yes
Month × Year effects	Yes	Yes	Yes	Yes	Yes
Observations	6,193	6,193	6,193	6,193	6,193
Adjusted R^2	0.092	0.602	0.482	0.693	0.516

Newey-West robust errors in parentheses. Unadjusted p-values: $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$. Violence includes homicide, assault, and sexual assault. Substance category includes incidents of driving while intoxicated, drunk and disorderly, drug possession, hit-and-runs, and liquor law violations. Property crime includes burglary, robbery, theft, and shoplifting. Party includes noise violation and loud or disruptive party calls. Medical assistance calls only include police assistance of medical aid for another department. Complete list of holidays accompanies discussion of equation (1). Weather includes third-order effects for temperature, precipitation, and snow depth. Bonferroni p-values correct for multiple hypothesis testing.

FIGURE 1.—PERSISTENCE OF THE PFD EFFECT, BY WEEK



Persistence effects estimated over the 2010–2016 sample by equation (2). Estimates show the daily change in number of calls of a particular type averaged over a given week. "PFD" week includes days 1 to 7 after the PFD direct deposit date; weeks −1 and 1 are the 1 to 7 days before and the 8 to 14 days after, respectively. Violence includes homicide, assault, and sexual assault. Substance category includes incidents of driving while intoxicated, drunk and disorderly, drug possession, hit-and-runs, and liquor law violations. Property crime includes burglary, robbery, theft, and shoplifting. Party includes noise violation and loud or disruptive party calls. Medical assistance calls only include police assistance of medical aid for another department.

28-day post-PFD window we study, substance abuse calls are approximately 10% higher (on average) on these days than other days during the year. Not only is the effect persistent for these weeks following the distribution, there is no evidence to suggest the effect is offset by reductions in substance calls in weeks 3 and 4. Thus, the estimated positive and persistent effect for substance abuse indicates a net increase, as opposed to a intertemporal substitution, in substance abuse incidents from the cash payment. This finding is consistent with previous findings of increased drug-related mortalities (Riddell & Riddell, 2006; Dobkin & Puller, 2007) and alcohol purchases (Castellari et al., 2017) associated with welfare and social security payments.[11]

With an extended time window, we find that average daily police activity related to property crime experiences a

[11] A potential concern here is evidence of a pretrend in substance-abuse incidents before the PFD is distributed (figure 1). However, the week-of-distribution effect still positively deviates from the extrapolated pretrend. Further, we find that alternative specifications using seasonal controls mitigate this pretrend, suggesting that the pretend is picking up seasonal trends in substance-abuse crimes. Regardless, the findings are consistent with the conclusion that the payment has a small (but statistically measurable) effect on substance abuse crimes.

TABLE 3:—MARGINAL EFFECT OF ADDITIONAL $100 MILLION PFD IN FIRST WEEK, 2000–2016

	Violence	Substance	Property	Party	Medical Assistance
Marginal effect	−0.017	1.074**	−0.347	−0.008	0.35***
	(0.121)	(0.422)	(0.214)	(0.103)	(0.096)
p-value	[0.888]	[0.011]	[0.104]	[0.939]	[0.000]
Bonferroni *p*-value	[1.000]	[0.044]	[0.313]	[1.000]	[0.001]

Newey-West robust errors in parentheses. Unadjusted p-values: $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$. Deviation from average first-week effect given a $100 million change in distribution size. Estimated by equation (3). Violence includes homicide, assault, and sexual assault. Substance category includes incidents of driving while intoxicated, drunk and disorderly, drug possession, hit-and-runs, and liquor law violations. Property crime includes burglary, robbery, theft, and shoplifting. Party includes noise violation and loud and disruptive party calls. Medical assistance calls only include police assistance of medical aid for another department.

significant decline for the two weeks after the PFD is issued, with an average daily decrease of 15.2% and 10.3% in the first and second week after payment, respectively. The significant week-after effect is largely driven by decreased activities during days that experience above-average property crimes (Monday to Wednesday). Like incidents of substance abuse, this effect is not offset in the later periods, indicating a net decline in property crime, which is consistent with past findings of declines in property crime associated with the timing of benefit payments (Carr & Packham, 2019; Foley, 2011) and improvements in economic conditions more generally (Lin, 2008; Gould et al., 2002; Bignon, Caroli, & Galbiati, 2017). Further, these reductions imply that the income effect dominates the potential "loot" effect of a cash transfer, at least over the examined time horizon. Over the full 28-day post-PFD window, the average daily reduction in property crime is 8%.

Like property crime, medical assistance from the Anchorage Police Department also shows response in the extended time period not seen on the first full day after payment (table 2). Specifically, we observe a 9% increase in the 28-day post-PFD window. Note, however, that these requests do not represent the universe of 911 calls regarding medical emergencies. They only represent requests for medical assistance from police from other city agencies.

C. Variation in the Size of the PFD

From equation (3), the marginal effect of an additional $100 million in first-day cash from the PFD is $\beta_2 + 2 \times \beta_3 \times Amount$. We evaluate the marginal effect at the sample average PFD dispersement, which is approximately $697 million in 2016 dollars (see table 3). For a $100 million increase in the PFD (14% above the average distribution), the number of calls requesting police to assist with medical issues increases by 0.35 calls per day (about a 2% increase relative to the daily sample average). In addition, substance-abuse incidents experience approximately an increase of one incident per day or a 2% increase over the daily sample average, indicating an elasticity of 0.14, which is consistent with the finding that alcohol is a normal good.[12] While a variety of factors could explain the higher substance-abuse incidents with higher PFD payments, our findings are consistent with the finding that cash aid creates "full wallets" that can exacerbate substance abuse problems (Dobkin & Puller, 2007).

Despite the existence of a negative average effect on property crimes, the marginal effect of the size of the PFD is not statistically significant. A few different explanations are possible for why higher distribution amounts do not result in further reductions in property crime. First, based on the figures we cite in table 1, it is possible that the annual fluctuations in the PFD are not large enough to cause additional decreases in property crime activity. Second, it's possible that the PFD amounts observed in our sample identify a region of the income/property-crime relationship where there are diminishing returns to income. For instance, Loughran et al. (2013) find that the illegal wage rate among individuals who engage in criminal activity is $929 per week, which is approximately equal to the lowest per person PFD received over the years in our sample (table 1).[13] Thus, fluctuations in the amount distributed may not reduce the number of property crimes given that the smallest PFD amount is almost as high as the earnings a skilled criminal can earn in a week. This potential explanation is consistent with Mocan and Bali (2010), who evaluate the effect of unemployment on crime and find that most of the impact of unemployment is observed in pulling people into crime rather than increasing the number of crimes of those who are already committing crimes. Finally, it is possible that the loot effect at higher PFD amounts offsets any additional income effect.

Altogether, the estimated marginal effects have two important implications. First, they provide support for our earlier results as they use additional exogenous variation to further isolate the PFD from any early October effects. Second, since the socially undesirable outcomes of substance abuse and medical-assist instances are increasing in distribution size, but the socially desirable outcome of reduced property crime is not, there are implied gains from spreading the payments over the year.

[12] Although Evans and Popova (2017) present evidence from across the world that cash transfers are not consistently used for alcohol or tobacco, evidence from the United States suggests that alcohol is a normal good (Decker & Schwartz, 2000). Dasso and Fernandez (2013) also refer to alcohol as a "temptation good," a term that Banerjee and Mullainathan (2010) use to refer to "goods that generate positive utility for the self that consumes them, but not for any previous self that anticipates that they will be consumed in the future."

[13] Loughran et al. (2013) also note that there is considerable skew in the data (SD = $1,491). A recent paper by Nguyen and Loughran (2017) reaches a similar conclusion that the distributions of self-report criminal activity are asymmetric and right-skewed, with long right tails. In one sample, they find an average weekly criminal wage of $1,470 (median $669), whereas in the other sample, the reported average weekly criminal wage was $914 (median $316).

TABLE 4.—COMPARISON OF PFD AND SNAP PAYMENT EFFECTS BY CATEGORY, 2000–2016

	Violence (1)	Substance (2)	Property (3)	Party (4)	Medical Assistance (5)
SNAP Week	0.121	5.298***	−0.834***	−0.132	0.702***
	(0.130)	(0.376)	(0.230)	(0.118)	(0.136)
PFD Week	−0.283	3.133**	−2.890***	−0.150	0.660**
	(0.415)	(1.243)	(0.652)	(0.299)	(0.332)
SNAP Week × SNAP Amount	0.033	0.462***	−0.016	0.019	0.064**
	(0.032)	(0.099)	(0.053)	(0.027)	(0.031)
PFD Week × PFD Amount	0.001	0.002*	0.001	−0.001**	0.001***
	(0.001)	(0.001)	(0.001)	(0.0004)	(0.0003)
SNAP elasticity	0.026	0.117	−0.005	0.021	0.049
PFD elasticity	0.027	0.038	0.012	−0.050	0.051
SNAP-PFD elasticity	−0.000	0.079	−0.017	0.071	−0.002
F-test p-value	[0.990]	[0.013]	[0.440]	[0.062]	[0.942]
Bonferroni P-value	[1.000]	[0.066]	[1.000]	[0.249]	[1.000]
Weather	Yes	Yes	Yes	Yes	Yes
Holiday	Yes	Yes	Yes	Yes	Yes
Day-of-week fixed effects	Yes	Yes	Yes	Yes	Yes
Day-of-month polynomial trend	No	No	No	No	No
Month × Year Effects	Yes	Yes	Yes	Yes	Yes
Observations	6,193	6,193	6,193	6,193	6,193
Adjusted R^2	0.092	0.598	0.482	0.693	0.515

Newey-West robust errors in parentheses. Unadjusted p-values: $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$. Coefficients in upper panel estimated by equation (4). Lower panel presents calculated elasticity values and hypothesis test that the difference between elasticities is 0. Bonferroni p-values correct for multiple hypothesis testing. Violence includes homicide, assault, and sexual assault. Substance category includes incidents of driving while intoxicated, drunk and disorderly, drug possession, hit-and-runs, and liquor law violations. Property crime includes burglary, robbery, theft, and shoplifting. Party includes noise violation and loud or disruptive party calls. Medical assistance calls only include police assistance of medical aid for another department. Complete list of holidays accompanies discussion of equation (1). Weather includes third-order effects for temperature, precipitation, and snow depth.

D. *Comparison to Nonuniversal/In-Kind Payments*

In this section, we compare our estimated effects to those for a particular nonuniversal, in-kind transfer receipt, food stamps (SNAP). SNAP and the PFD differ in two important respects. First, the universal nature of the PFD makes the average PFD recipient quite different from the average SNAP recipient. This is true with respect to income (by definition) and across other sociodemographic dimensions. While Grant and Dawson (1996) provide evidence that the welfare-receiving population does not systematically differ from nonreceivers in their alcohol abuse, Pollack and Reuter (2006) find that drug usage was higher among welfare-receiving individuals. Further, Hastings and Washington (2010) show that alcohol and tobacco purchases for SNAP recipients (relative to nonrecipients) are highest soon after SNAP receipt. For these reasons, we would expect substance-abuse crimes to be more elastic with respect to SNAP payments than PFD payments. The second difference between SNAP and the PFD is the in-kind (or conditional) nature of the SNAP payments. SNAP recipients may spend food stamps disproportionately on eligible food items relative to other sources of income; thus, substance-abuse incidents may be more elastic with respect to unrestricted payments like the PFD.[14]

Table 4 displays the estimated coefficients of interest from equation (4). The main effects suggest that substance-abuse incidents are more frequent during the week in which SNAP payments are distributed, while property crime incidents are lower the week after the PFD is distributed. We note, however, that this only suggestive: the main effect estimates for SNAP (*SNAPWeek*) are not separately identified from other first-of-the-month occurrences, although our estimates for reductions in property crime are not dramatically different from Foley (2011).[15] Similarly, Dobkin and Puller (2007) find an increase in hospital admissions related to drug abuse during the first few days of the month attributable to receipt of Supplemental Security Income programs (SSI). While their results are consistent with the results we find for medical assistance and substance abuse incidents, we note that SSI is issued as a cash transfer while SNAP benefits are in-kind benefits.

Turning to the marginal effect estimates, an increase of \$1 million in the size of the monthly SNAP distribution is associated with a statistically significant increase of 0.46 in the average number of daily substance abuse incidents over the first week, compared to an increase of only 0.002 daily substance abuse incidents in the first week from a \$1 million increase in the size of the PFD distribution. Medical assistance incidents are also responsive to the distribution sizes of both payments, with an additional \$1 million resulting in 0.001 and 0.064 increase in calls for the PFD and SNAP, respectively. Given the different scales of the PFD and SNAP programs, the lower panel of table 4 facilitates the comparison of these two programs by presenting the marginal effects in elasticity terms and tests the hypothesis that the difference

[14]Cuffey, Beatty, and Harnack (2016) in a survey/meta-analysis of 59 papers and Hastings and Shapiro (2018) both find a notable gap between the marginal propensity to purchase eligible food from food stamps than from other income.

[15]Foley (2011) finds property crime to be 3.8% lower in the first ten days after SNAP distribution; we find a 2.5% decrease. While our results are somewhat smaller, this is intuitive considering that Anchorage has a lower participation rate in SNAP than the average city in Foley's study (sampled based on high participation rates in the program).

in crime-payment elasticities for SNAP versus the PFD is statistically different from 0. We find such a difference in response of substance abuse to payment receipt but not for the other categories of activities.

The goal of comparing SNAP- to PFD-related effects is to highlight the differential effects of universal cash payments relative to those previously studied for nonuniversal and in-kind payments. The more responsive nature of substance-abuse crimes with respect to SNAP payments suggests that the universal nature of the PFD more than offsets any relative difference arising from the unconditional nature of the PFD, as per the discussion above. However, we note several other potential explanations for why substance-abuse incidents are relatively more responsive to SNAP payments. PFD and SNAP payments are quite different in size, and the effect that we are trying to estimate may be highly nonlinear—for example, a \$150 SNAP payment leads to increases in crime, while a \$1,000 PFD payment does not. In addition, the scale of the PFD program has the potential to induce general equilibrium effects and create demand for labor (as Jones & Marinescu, 2018, found), which could attenuate the crime response from the PFD payments.

E. *Robustness of Findings*

To further scrutinize our findings, we test whether similarly sized effects for the PFD can be found during other one-week periods of the year. For this test, we estimate equation (1) iteratively, redefining treatment for a new week-of-year at each step. Placebo treatment weeks are defined starting at the true treatment week and working outward within each year. Because of this definition, the weeks at the beginning and end of each year will not necessarily contain seven days. Also, as the number of weeks before and after the true dispersement week vary from year to year, we will estimate more than 52 placebo effects. The results of the placebo test are consistent with the those presented in the main results tables. Appendix figure A.3 plots a histogram of the magnitude of these placebo treatment effects for each outcome, along with the fraction of observed effects that are greater in magnitude than the actual treatment. Substance abuse and property crime are both found to have true treatments in the 10th percentile of all estimated effects. Placebo effects for these outcomes of similar or larger magnitude are also easily explained by spillovers from holidays or log accounting issues from daylight savings time.

A potential threat to identification is a change in the level of police enforcement activity around the time of PFD distribution: if APD anticipates a swell in activity around the time of the distribution, it may increase its presence and observe more crime taking place (whether the actual underlying level changes or not). Our conversations with the APD suggest that staffing effort does not change around the time of PFD distribution. Further, most of the activities of interest in our analysis are so-called calls for service. Calls for service are initiated by members of the community (e.g., calling 911, hailing an officer in the field, or a request by other agency), in contrast to "self-initiated" activities, which are more responsive to changing enforcement. In appendix C, we find no evidence that the total number of calls for service, self-initiated activities, or their their ratio change on PFD distribution day, relative to other days. This is in contrast to periods and holidays with known enforcement changes.

VI. Discussion

Our analysis finds an increase in the number of substance-abuse incidents and a decrease in the number of property crimes in the weeks immediately following the PFD distribution; a remaining question is whether such changes can be considered economically significant. Estimates of the persistence of the PFD effect (figure 1 and table A.6) indicate that 126 more substance-abuse incidents and 77 fewer property crimes are realized on average over the four weeks after the PFD is distributed. On an annual basis, this constitutes only a 1.05% increase and 0.61% decreases in substance-abuse incidents and property crimes, respectively (appendix table A.8). Further, many of the substance-abuse incidents that are attributable to the PFD constitute crimes that may not be overly costly to society. For example, decomposing the substance-abuse category into its individual components indicates that our substance-abuse results are being driven by increases in crimes such as "drunk problem" and "drunk transport" (see appendix table A.5). To understand the potential cost of such crimes, we compute estimates of the costs associated with the increase in alcohol-related crimes and compare them to the cost savings associated with the reduction in property crimes (see appendix B for more detail). We provide a low-cost case, which assumes the cost of crime includes only tangible costs, and a high-cost case, which assumes that the cost of crime includes tangible and intangible costs.[16] The high-cost case also incorporates the probability that substance consumption induces additional crime.[17] Our back-of-the-envelope calculation suggests that the cost of increased alcohol-related incidents ranges between \$4,500 and \$3.9 million over the four weeks after the distribution, depending on whether both direct and indirect costs of alcohol-related crimes are included in the calculation and whether the costs include intangibles. In contrast, we estimate the cost savings from the decrease in property crimes to be between \$333,000 and \$419,000. Together, these estimates suggest that the net effect of crime attributable to the PFD lies between +\$329,000 and −\$3.4 million (appendix table A.8). While the sign of the estimated

[16]From McCollister, French, and Fang (2010), tangible costs include the victim's economic loss (medical costs, lost earnings, and property loss), criminal justice cost, and the opportunity cost for the criminal from forgone legitimate pursuits. In contrast, intangible costs incorporate pain and suffering.

[17]Alcohol-related crimes have direct costs (e.g., police resources to manage disorderly individuals, as in Rajkumar & French, 1997), but also indirect costs via an increased likelihood of committing other crimes while under the influence (e.g., the alcohol-violence link that Lindo et al., 2018, showed).

net welfare effect depends on our assumptions about which costs to include, we interpret the economic significance of the welfare effect as unambiguously small. In comparison to the size of the total PFD payment received by Anchorage residents, for instance, the welfare changes associated with crime range from a 0.17% savings to a 1.78% loss across the low- and high-cost cases. In per capita terms, these changes are between a $1.54 per person savings to a $16.12 loss, relative to 2016 per person PFD amount of $1,022.

Despite the relatively small calculated welfare impact, there may be potential to reduce the impact of PFD-related crime by restructuring the timing of the PFD. For instance, our results suggest that substance-abuse incidents increase with the size of the PFD, but no further reductions in property crimes occur. This implies that there may be benefits to staggering the PFD payments over the year: smaller amounts may reduce the negative "full wallet" effect without increasing financially motivated crimes. However, as the size of our results are small, any gains from reducing substance-abuse incidences should be weighted against any administrative costs associated with issuing multiple payments a year. In addition, the relationship between payment size and property crimes is not well identified for smaller payments that lie outside our observed sample of historical PFD payments; thus, several small payments may not have the same effect on property crime as we estimate in our analysis. Nevertheless, our findings are consistent with a number of recent papers on conditional cash transfers showing gains from staggering payments (Dobkin & Puller, 2007; Carr & Packham, 2019). The gains in our case, however, are smaller due to the universal nature of the payment and the difference in the treated population. While the deviations we find are small on an annualized basis, they do suggest that future implementations of universal income should test alternative disbursement policies that maximize the reductions of financially motivated crimes while reducing substance-abuse activity, which can result in crime.

VII. Conclusion

We present the first comprehensive analysis of the crime consequences of an unconditional and universal transfer using the world's only continuous universal income program, Alaska's Permanent Fund Dividend. Our findings provide several new insights into the impact of cash transfers on the behavior of recipients. We show that the recipient population is responsive to an unconditional and anticipated income receipt across several dimensions of interest. Over the four-week period after the PFD distribution, we find an average daily reduction in property crime of 8%, an average daily increase in substance abuse crime of 10%, and an average daily increase in medical assistance calls of 9%. Our substance abuse results confirm the mechanisms underlying previous work that finds increases in substance-abuse-related morbidity and mortality following cash transfers from SSI and welfare programs (Dobkin & Puller, 2007; Riddell & Riddell, 2006). However, our results stand in contrast to other work that finds more limited, or even negative, substance-abuse-related responses to cash transfers (Cuffey et al., 2016). Additionally, we find that substance abuse and medical calls for assistance are responsive to the total size of the payment program (in terms of dollars) but property crimes are not. Our property crime results in general support previous work that finds a decrease in property crime following SNAP payments in twelve US cities (Foley, 2011). The observed changes we describe above are, however, modest, as the increase in substance-abuse crime is 1.05% of the annual level, while the declines in property crime are -0.61%.

Our results also contribute a new dimension to the growing literature on universal basic income. We show the potential for such programs to produce both positive and negative social consequences. On the negative side, we show that unconditional cash transfers do in fact increase recipients' consumption of "temptation goods," or controlled substances (as measured by policing activities). On the positive side, we show that a universal cash transfer also decreases property crime. These positive and negative effects are quite different in magnitude (on a per dollar/per person basis) than the estimated effects of other transfers that have been the subject of past work. In our analysis, when the PFD and food stamp (SNAP) program are compared, we find that the SNAP-distribution elasticity of substance abuse calls is over four times larger than that of the PFD. The results of this comparison provide quantitative evidence regarding the fundamentally different nature of universal payments from payments such as food stamps or social security that have been the subject of many past studies. As such, generalizing the findings of conditional, nonuniversal, and in-kind transfer literature to more universal payments may be problematic due to the differences in the average recipient's response. Indeed, the small estimated crime-related costs of the PFD suggest that crime-related concerns of a universal cash transfer program may be unwarranted.

Finally, we show that lessons from the PFD can potentially be very useful in understanding the consequences associated with UBI implementations. Our focus in this paper has been on a subset of behavioral responses (i.e., criminal activity) as measured by daily police activity records. Clearly, the goals of UBI have far reaches, and some are beyond the scope of this paper. The length of time the PFD has been in existence provides a unique opportunity for researchers to investigate the health, education, labor, and other social effects on the Alaska population.

REFERENCES

Banerjee, A., and S. Mullainathan, "The Shape of Temptation: Implications for the Economic Lives of the Poor," NBER working paper 15973 (2010).

Becker, G. S., "Crime and Punishment: An Economic Approach," *Journal of Political Economy* 76 (1968), 169–217.

Bignon, V., E. Caroli, and R. Galbiati, "Stealing to Survive? Crime and Income Shocks in Nineteenth Century France," *Economic Journal* 127: 599 (2017), 19–49.

Billings, S. B., D. J. Deming, and S. L. Ross, "Partners in Crime: Schools, Neighborhoods and the Formation of Criminal Networks," NBER working paper 21962 (2016).

Blakeslee, D. S., and R. Fishman, "Weather Shocks, Agriculture, and Crime," *Journal of Human Resources* 53 (2018), 750–782.

Borraz, F., and I. Munyo, "Conditional Cash Transfers and Crime: Higher Income but Also Better Loot," Centro de Economia, Sociedad y Empresa del IEEM, Universidad de Montevideo (2015).

Carr, J. B., and A. Packham, "SNAP Benefits and Crime: Evidence from Changing Disbursement Schedules," this REVIEW 101 (2019), 310–325.

Castellari, E., C. Cotti, J. Gordanier, and O. Ozturk, "Does the Timing of Food Stamp Distribution Matter? A Panel-Data Analysis of Monthly Purchasing Patterns of US Households," *Health Economics* 26 (2017), 1380–1393.

Chioda, L., J. M. P. De Mello, and R. R. Soares, "Spillovers from Conditional Cash Transfer Programs: Bolsa Familia and Crime in Urban Brazil," *Economics of Education Review* 54 (2016), 306–320.

Cotti, C., J. Gordanier, and O. Ozturk, "Eat (and Drink) Better Tonight: Food, Stamp Benefit Timing and Drunk Driving Fatalities," *American Journal of Health Economics* 2 (2016), 511–534.

Cuffey, J., T. K. Beatty, and L. Harnack, "The Potential Impact of Supplemental Nutrition Assistance Program (SNAP) Restrictions on Expenditures: A Systematic Review," *Public Health Nutrition* 19 (2016), 3216–3231.

Damm, A. P., and C. Dustman, "Does Growing Up in a High Crime Neighborhood Affect Youth Criminal Behavior?" *American Economic Review* 104 (2014), 1806–1832.

Dasso, R., and Fernando Fernandez, "Temptation Goods and Conditional Cash Transfers in Peru, Northeast Universities Development Consortium conference paper (2013).

Decker, S. L., and A. E. Schwartz, "Cigarettes and Alcohol: Substitutes or Complements?" (2000).

Dobkin, C., and S. L. Puller, "The Effects of Government Transfers on Monthly Cycles in Drug Abuse, Hospitalization and Mortality," *Journal of Public Economics* 91 (2007), 2137–2157.

Evans, D. K., and A. Popova, "Cash Transfers and Temptation Goods: A Review of Global Evidence," World Bank policy research working paper 6886 (2014).

——— "Cash Transfers and Temptation Goods," *Economic Development and Cultural Change* 65 (2017), 189–221.

Evans, W. N., and T. J. Moore, "The Short-Term Mortality Consequences of Income Receipt," *Journal of Public Economics* 95 (2011), 1410–1424.

Foley, C. F., "Welfare Payments and Crime," this REVIEW 93 (2011), 97–112.

Gould, E. D., B. A. Weinberg, and D. B. Mustard, Crime Rates and Local Labor Market Opportunities in the United States: 1979–1997," this REVIEW 84 (2002), 45–61.

Grant, B. F., and D. A. Dawson, "Alcohol and Drug Use, Abuse, and Dependence among Welfare Recipients," *American Journal of Public Health* 86 (1996), 1450–1454.

Hastings, J., and J. M. Shapiro, " How Are SNAP Benefits Spent? Evidence from a Retail Panel," *American Economic Review* 108 (2018), 3493–3540.

Hastings, J., and E. Washington, "The First of the Month Effect: Consumer Behavior and Store Responses," *American Economic Journal: Economic Policy* 2 (2010), 142–162.

Jacob, B. A., and L. Lefgren, "Are Idle Hands the Devil's Workshop? Incapacitation, Concentration, and Juvenile Crime," *American Economic Review* 93 (2003), 1560–1577.

Jones, D., and I. E. Marinescu, "The Labor Market Impacts of Universal and Permanent Cash Transfers: Evidence from the Alaska Permanent Fund," NBER working paper 24312 (2018).

Kueng, L., "Excess Sensitivity of High-Income Consumers," *Quarterly Journal of Economics* 133 (2018), 1693–1751.

Lin, M.-J., "Does Unemployment Increase Crime?" *Journal of Human Resources* 43 (2008), 413–436.

Lindo, J. M., P. Siminski, and I. D. Swensen, "College Party Culture and Sexual Assault," *American Economic Journal: Applied Economics* 10 (2018), 236–265.

Loughran, T. A., H. Nguyen, A. R. Piquero, and J. Fagan, "The Returns to Criminal Capital," *American Sociological Review* 78 (2013), 925–948

Machin, S., and C. Meghir, "Crime and Economic Incentives," *Journal of Human Resources* 39 (2004), 958–979.

Marinescu, I., "No Strings Attached: The Behavioral Effects of U.S. Unconditional Cash Transfer Programs," Roosevelt Institute technical report (2017).

Mastrobuoni, G., and M. Weinberg, "Heterogeneity in Intra-Monthly Consumption Patterns, Self-Control, and Savings at Retirement," *American Economic Journal: Economic Policy* 1 (2009), 163–189.

McCollister, K. E., M. T. French, and H. Fang, "The Cost of Crime to Society: New Crime-Specific Estimates for Policy and Program Evaluation," *Drug and Alcohol Dependence* 108 (2010), 98–109.

Mejia, D., and A. Camacho, "The Externalities of Conditional Cash Transfer Programs on Crime: The Case of Bogotá's Familias en Accion Program," paper presented at the LACEA 2013 Annual Meeting (2013).

Mocan, H. N., and T. G. Bali, "Asymmetric Crime Cycles," this REVIEW 92 (2010), 899–911.

Newey, W. K., and K. D. West, "A Simple, Positive Semi-Definite, Heteroskedasticity and Autocorrelation Consistent Covariance Matrix," *Econometrica* 55 (1987), 703.

Nguyen, H., and T. A. Loughran, "On the Reliability and Validity of Self-Reported Illegal Earnings: Implications for the Study of Criminal Achievement," *Criminology* 55 (2017), 575–602.

Pollack, H. A., and P. Reuter, "Welfare Receipt and Substance-Abuse Treatment among Low-Income Mothers: The Impact of Welfare Reform," *American Journal of Public Health* 96:11 (2006), 2024–2031.

Rajkumar, A. S., and M. T. French, "Drug Abuse, Crime Costs, and the Economic Benefits of Treatment," *Journal of Quantitative Criminology* 13 (1997), 291–323.

Riddell, C., and R. Riddell, "Welfare Checks, Drug Consumption, and Health: Evidence from Vancouver Injection Drug Users," *Journal of Human Resources* 41 (2006), 138–161.

Shapiro, J. M., "Is There a Daily Discount Rate? Evidence from the Food Stamp Nutrition Cycle," *Journal of Public Economics* 89 (2005), 303–325.

Solnick, S. J., and D. Hemenway, "Hit the Bottle and Run: The Role of Alcohol in Hit-and-Run Pedestrian Fatalities," *Journal of Studies on Alcohol* 55 (1994), 679–684.

Stephens, M., "'3rd of tha Month': Do Social Security Recipients Smooth Consumption between Checks?" *American Economic Review* 93 (2003), 406–422.

Stephens, M., and T. Unayama, "The Consumption Response to Seasonal Income: Evidence from Japanese Public Pension Benefits," *American Economic Journal: Applied Economics* 3 (2011), 86–118.

Thigpen, D. E., "Universal Income: What Is It, and Is It Right for the U.S.?" Roosevelt Institute technical report (2016).

White, J. S., and S. Basu, "Does the Benefits Schedule of Cash Assistance Programs Affect the Purchase of Temptation Goods? Evidence from Peru," *Journal of Health Economics* 46 (2016), 70–89.

Wright, R., E. Tekin, V. Topalli, C. McClellan, T. Dickinson, and R. Rosenfeld, "Less Cash, Less Crime: Evidence from the Electronic Benefit Transfer Program," *Journal of Law and Economics* 60:2 (2017), 361–383.