Universal Cash and Crime

Brett Watson [1,*]

Mouhcine Guettabi [1,†]

Matthew Reimer [1,‡]

[1] Institute of Social and Economic Research, University of Alaska Anchorage

June 26, 2018

Abstract

We estimate the causal effects of universal cash transfers on criminal activity by exploiting the exogenous timing and size of Alaska's Permanent Fund Dividend (PFD), an annual lump-sum payment to all Alaska residents. Using a database of daily policing incidents, we find a 17% increase in substance-abuse incidents and 12% decrease in property-crime for up to two weeks after the cash transfer, with no average change in violence. We further show that violence and substance abuse are increasing in payment size. The universal PFD transfer is also compared to non-universal food stamp transfers to show their differential impact on crime.

JEL Classification: H24, I38, J18, K42
Keywords: Permanent Fund Dividend; Unconditional cash transfer; Welfare effects; Crime; Universal Income.

*Post-Doctoral Researcher. Email: bwjordan2@alaska.edu
†Corresponding Author. Associate Professor of Economics. Email: mguettabi@alaska.edu
‡Associate Professor of Economics. Email: mreimer2@alaska.edu

1 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, and sufficient for covering basic living expenses (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. UBI carries less "welfare stigma," or negative feelings associated with participation in income-tested programs, as documented by Rainwater (1982) and modeled by Moffitt (1983).

Despite the potential benefits of UBI, there is little accumulated knowledge about how such a policy might change people's behavior. While previous research has linked other forms of cash transfers to short-run changes in behavior, our study is the first to test such responses from a universal and unconditional cash transfer. UBI cash transfers are distinct from other payment types—such as in-kind benefits, conditional cash transfers, public pensions, or unemployment insurance—in at least three respects. First, a broader and more diverse socioeconomic group is receiving the transfer. This distinction is important for considering behavioral effects beyond segments of the population more commonly considered, such as the elderly (pension/social security payments) or low-income earners (welfare payments), which likely differ in their consumption behaviors from those of different ages or income levels. Second, the entire population is receiving an income payment, and thus, a greater amount of money has the potential to "hit the street" at once.[1] This distinction is important for considering effects induced by broader economic spillovers from a large, economy-wide cash injection, such as the general equilibrium labor market effects shown to be generated by the PFD (Jones and Marinescu, 2018). Third, there are no restrictions on how the payments are spent by the recipients, which is important to consider if in-kind cash transfers, such

[1] In the case of Alaska, the distribution to the entire state occurs on the same day.

as food stamp (or Supplemental Nutrition Assistance Program, SNAP) payments, limit the ways in which recipients can respond to the transfer. Altogether, the distinct nature of UBI may induce different behavioral responses to cash transfers than those estimated in previous studies.

In this paper, we explore behavioral responses to a cash transfer using the world's only continuous universal income program—Alaska's Permanent Fund Dividend (PFD)—as a case study. The PFD is an annual lump-sum payment to Alaska residents based on the investment earnings of the Alaska Permanent Fund, the state's sovereign wealth fund. While the PFD was never intended as a UBI payment, it manifests several features of UBI. The PFD program is unconditional and provided to all Alaska residents (subject to eligibility rules), regardless of income. This is in contrast to recent pilot implementations of basic income programs, such as Finland's experiment which is targeted at a small, demographic subset of the population: 2,000 unemployed individuals (Donnelly, 2018). In addition, the PFD is large relative to the median household income,[2] has been continuously running for over 35 years (since 1982), and is largely viewed as a permanent institution based on its political popularity. Thus, the Alaska PFD is the closest example to a UBI program worldwide, and provides a unique opportunity to gather insights into the factors for consideration in implementing a UBI program in other places.

We estimate the short-run effects of a universal and unconditional cash transfer on outcomes that have previously been shown to be responsive to non-universal cash or in-kind transfers. Specifically, we examine 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 that for an average-size PFD distribution, the PFD does not induce changes in violent incidents the

[2] Median annual household income in the state is approximately $70,000. For a family of four, an average distribution would account for 8.5% of their income—or a full month of other earnings.

day it is received, but that violence is responsive to the size of the payment: a ten percent increase in distribution size induces a four percent increase in incidents. For substance-abuse incidents, we find a 17% increase for the first full day after the PFD payment is issued, and elevated levels for a full two weeks after the PFD. Further, we find that these incidents are sensitive to the size of the PFD payment during the week after the PFD payment is issued: a ten percent increase in the size of the PFD payment results in a two percent increase in the average daily number of substance-abuse incidents. Reported property crime falls by approximately 12% the week of payment, but does not respond to the size of payment distribution. Finally, we compare our findings to those stemming from SNAP payments and find that substance-abuse incidents are four times more responsive to the size of SNAP payments than to the size of PFD payments. Our results suggest that the behavioral effects of a cash transfer on substance abuse and property crimes could be considerably different than those previously estimated under non-universal income programs.

The existing literature shows that people tend to exhibit short-run impatience, whereby consumption and economic activity increase immediately following other forms of cash transfer (e.g., Stephens, 2003; Shapiro, 2005; Stephens and Unayama, 2011; Kueng, 2015).[3] Short-run impatience suggests people may behave differently upon receiving a cash transfer in ways that create positive and/or negative welfare effects beyond those intended by a UBI program. For instance, a common critique of cash transfers is that recipients will misuse the cash by increasing their consumption of "temptation goods" (Banerjee and Mullainathan, 2010), such as drugs and alcohol (Evans and Popova, 2014; White and Basu, 2016). Morbidity and mortality have been shown to increase among recipients following cash transfers due to increased levels of substance abuse (e.g., Dobkin and Puller, 2007; Riddell and Riddell, 2006). Similarly, increased economic activity from cash transfers has been shown to cause adverse health and mortality outcomes (e.g., Andersson et al., 2015; Evans and Moore, 2011). Cash transfers also have the potential to increase financially motivated crimes such as bur-

[3]Short-run impatience stands in contrast to the permanent income hypothesis, which posits that consumption follows a smooth pattern even in the presence of predictable changes to income.

glary, robbery, and theft by increasing the supply of cash available to potential offenders in the streets (Borraz and Munyo, 2014; Wright et al., 2017). In contrast, "income effects" from cash transfers have been shown to reduce the need to engage in criminal activities, thereby decreasing financially motivated crimes in both the short (e.g., Foley, 2011; Mejia and Camacho, 2013) and long run (e.g., Chioda et al., 2016).

While the existing evidence establishes a relationship between certain behavioral responses an certain forms of transfers, nearly all evidence to date is derived from cash transfer programs that do not manifest the key features of UBI. In particular, most studies have estimated behavioral effects for specific sub-populations and/or for cash transfers that have conditions on how the transfer can be spent. For example, studies of conditional cash transfer programs, such as *Familias en Acción* (Mejia and Camacho, 2013) and *Bolsa Família* (Chioda et al., 2016), use samples comprised of low-income households whose children satisfy a minimum level of school attendance. Similarly, transfers from welfare programs, such as SNAP (e.g., Foley, 2011; Cotti et al., 2016; Carr and Packham, 2017), are in-kind payments and provided to low-income households only. Thus, these programs do not provide basic income universally and often place restrictions on how such transfers can be spent. There is therefore limited evidence from these programs that can be used to infer the potential for behavioral responses to a cash transfer from a UBI program.

Our results contribute to the literature in several ways. First, to our knowledge, we are the first to estimate the effects of a universal income receipt on reported violence, property-crime, and substance-abuse incidents. Previous studies examine the effects of cash transfers on violence, substance abuse, or property crime from non-universal income programs, such as welfare payments (e.g., Foley, 2011; Riddell and Riddell, 2006; Cotti et al., 2016; Carr and Packham, 2017), disability insurance (e.g., Dobkin and Puller, 2007), and conditional cash transfer programs (e.g., Mejia and Camacho, 2013; Chioda et al., 2016).[4] The distinct

[4]See Appendix Table A.1 for a summary of findings from the recent literature and the outcomes investigated. While the list is not comprehensive, there are no papers to our knowledge that investigate the implications of a universal and unconditional cash transfers on criminal activity.

nature of the PFD could induce changes in behavior that are either larger or smaller than those generated by other types of cash transfer programs. For instance, the in-kind nature of payments under some welfare programs, such as SNAP, can only be used to purchase eligible items (e.g., food). The nature of such payments could restrict a recipient's ability to increase consumption of ineligible items (e.g., controlled substances) and have less influence on property crimes that finance the purchase of ineligible items.[5] Thus, unrestricted payments like the PFD could have a relatively larger absolute change in the measured behaviors. On the other hand, the universal nature of the PFD means all segments of the population receive the payment, which could result in a relatively smaller change in these outcomes if the average PFD recipient is less likely to partake in criminal activity.[6] Our results confirm that universal and unconditional payments affect people's behaviors differently from other forms of cash transfers.

Second, relatively few studies have used the PFD to investigate behavioral effects of discrete universal income payments. For instance, using the Consumer Expenditure Survey, Hsieh (2003) finds no evidence that the short-run consumption of non-durables by Alaskan households react to the large and anticipated PFD payments, lending support to the permanent income hypothesis. In contrast, using data for a subset of Alaskans who use an online financial management service, Kueng (2015) finds that the short-run consumption of non-durables and services by Alaskan households is excessively sensitive to the PFD payment, and that this response is largely driven by higher income households [7] In more closely related work, Evans and Moore (2011) show that direct deposits of the PFD increases mortality among urban Alaskans by 13%, a finding which they argue is inconsistent with the permanent income hypothesis. Our paper differs from these studies in several important ways. Unlike

[5]In a meta-analysis of fifty-nine papers, Cuffey et al. (2016) finds a notable gap between the marginal propensity to purchase eligible food from food stamps than from other income.

[6]While Grant and Dawson (1996) provide evidence that the welfare-receiving population does not systematically differ from non-receivers, Pollack and Reuter (2006) finds that drug usage was higher among welfare-receiving individuals and Hastings and Washington (2010) show that alcohol- and tobacco-purchasing behavior differs between recipients and non-recipients.

[7]Kueng (2015) provides evidence that the insignificant PFD effects on consumption in Hsieh (2003) are driven by substantial measurement error in reported family income in the Consumer Expenditure Survey.

Hsieh (2003) and Kueng (2015), who use self-selected samples of Alaska residents, we use the universe of policing incident reports in Anchorage, which allows us to draw conclusions about how receipt of the PFD affects behavioral responses at the population level. Our work is further differentiated by the behavioral margins we investigate. Evans and Moore (2011) also investigate substance abuse as a potential mechanism for the observed increase in mortality from the PFD; however, they are unable to separately identify substance abuse deaths caused by the PFD due to sample size issues. Using evidence from other forms of income receipts, such as social security, military pay, and tax rebates, Evans and Moore (2011) argue that changing levels of consumption more generally is the most plausible mechanism through which income receipt affects mortality. Our results confirm that people do change behavior as a result of PFD receipt; however, in contrast to their findings, we are able to explicitly show that substance abuse incidents do indeed rise in response to the income receipt. In addition, our study period is considerably larger than the one used by Evans and Moore (2011) (2000-2006), which allows us to exploit variation in the amount of the PFD over time to explore whether behavior is sensitive to the size of the PFD payment. Further, as we discuss in more detail below, our study period covers years in which the PFD is issued on the same day, which allows us to better identify the persistence of any effect from the PFD.

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 gains from the distribution of higher amounts. Violence and substance-abuse incidents, on the other hand, are responsive to both PFD amounts and increases in the distribution. While the goals of UBI implementation are many and well beyond the scope of this paper, our results suggest that there may be crime-related costs to UBI, in the form of increased substance-abuse incidents, and a limited ability to reduce property crime.

The remainder of this paper is organized as follows. Section 2 presents a brief history of the Alaska Permanent Fund dividend and why it represents a fruitful setting for empirical research on UBI. Sections 3 and 4 describes the daily policing incident data that we used for the analysis and the empirical model used to estimate the effect of the cash transfer on observed categories of policing incidents. Section 5 presents the primary findings of the analysis and describes three extensions: measuring the persistence (total duration) of the effect, determining how the effect size varies with amount of the cash distribution, and comparing the measured effect to the effect of other transfers within Alaska. These results and their implications for the literature on cash transfer and the policy debate around UBI are discussed in the concluding Section 6.

2 The 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 yearly oil revenues to a state investment fund. The fund balance grew slowly in its first two years, reaching $137 million by the end of fiscal year 1979. Shortly thereafter, the price of oil took a dramatic leap upward and by 1988 the fund balance, including sub accounts, passed the $10 billion mark. It currently stands at over 61 billion dollars. When the initial fund was created, there was no intention to share earnings with the public. That, however, changed with Governor Hammond's desire to ensure that the money not be spent by politicians, his wish that the fund benefit all residents equally, and that it would still exist for future generations. Interest in a payout gained traction over a few years and ended up resulting in the first payout of 1,000 dollars in 1982. In that first year, the PFD was paid out of general revenues rather than fund earnings; thereafter, the payments were made from the fund's earnings. PFD payments are determined by a formula that is based on an average of the fund's income over five years in order to produce more stable dividend amounts from year to year. It is important to note that the fund is well-diversified across different asset classes and its returns are therefore not necessarily reflective of Alaska's economic conditions. The fund is managed by the Alaska Permanent Fund Corporation (APFC) and operated as a public trust, much like trust funds established for pension funds. This means fund managers must balance the idea of income production against ordinary prudence about risk. In its simplest terms, the Permanent Fund is a savings account geared to make money for Alaskans. Over time, the permanent fund has become increasingly large and the APFCs investing increasingly sophisticated; with bonds only until 1983 when real estate and U.S. stocks became investments. In 1990 global public debt and equities were traded, and in 1999 and 2007, respectively, alternative investments and infrastructure investments were added to the portfolio. Importantly, state oil revenue, which originally capitalized the fund, has represented only 2-3% of the total amount added to the fund each year since 1985 as reinvestment of fund earnings is the primary way in which the fund grows.

There are two portions of the permanent fund which are distinguished by accounting only: principal and the earnings reserve. The principal is the amount required to remain in the fund in perpetuity, while the earnings reserve is what may be used for spending by government—either in the form of dividends, or otherwise. The permanent fund invests in assets. The assets of the fund are owned collectively by both accounts, making the funds in the portfolio indistinguishable from each other. Thus, each fund bears the same investment risks. For example, if the permanent fund were to invest in an individual house, both accounting portions of the permanent fund would own it, and allocate a portion of the ownership to both portions of the fund, based on fund balances. By 2010, close to 38% percent of the fund's overall balance had come from royalty deposits, another 22% from occasional special contributions, and the rest from inflation proofing. The dividend from this fund was intended to both create a constituency for sound management of the fund, and directly distribute the States resource wealth to its citizens. It has been argued that the dividend created a constituency for the permanent fund itself. In other words, the public kept a watchful eye on legislators decisions who could have potentially fallen prey to special interests in the absence of the linkage between the permanent fund and the dividend.

The dividend established an income floor below which the cash income of residents cannot fall. This cash transfer is particular important in rural areas where economies lack economic bases and are still a mixture of subsistence and a small formal economy. Since 1982, Alaskans have received that yearly dividend. The amount varies year-to-year depending on the funds returns. In 2008, the dividend reached a high of $3,269 (including a one-time supplement of $1,200 "energy rebate" financed by that years state budget surplus), which comes to $13,076 for a family of four. The program has become very popular and the public expects it to run in perpetuity. PFD amounts are not based on a person's income or wealth and are distributed to all residents—adults and children–of the state (including green-card holders and refugees), making it universal. The dividend represents a non-negligible portion of Alaskans earnings. The 1982 dividend distribution of $450 million increased personal income in Alaska by 6.3 percent, the same amount as the payroll of the petroleum industry for that year. 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 dollar distribution was about 42% of the construction sector's GDP, or 76% of the whole-trade sector's. In addition to the sheer size, PFD payments distributed at the same time, which means it is the single largest infusion of money at a given point in Alaska's 50 billion dollar economy.

Each year the filing period runs from January 1st to March 31st. 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 (2000-2016), 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 relative small portion of the population receiving mailed-checks, and because these checks are never issued before direct deposits, our study focuses explicitly on the first date of direct deposit issuance.

To date, there has been virtually no research linking the permanent fund distribution to aspects of health, education, crime, or any other aspects of social well being. One potential reason is that Alaskans view the dividend as a right and not a tool to address these issues. Another is its universality makes isolating its effects challenging.

3 Data

We employ a large 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 of the state's total population.[8] 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 their location and the nature of their 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. While a subset of the incident reports are in response to citizen requests for service (i.e., 911 calls), incident reports are primarily officer-initiated activities, such as observing and responding to a crime in progress.

APD provided us with the complete incident reports for the years 2000-2016. For our analysis, we aggregate these incident-report level data up to counts at the day level for each activity code. 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 requests for medical assistance. Table A.2 shows the average daily count of calls received over our sample period for specific call

[8]With an estimated population of approximately 300,000, Anchorage accounted for roughly half of Alaska's 750,000 residents in 2016. Fairbanks, Alaska's second largest city, has just 1/10 the population of Anchorage. (https://www.census.gov/data/datasets/2016/demo/popest/total-cities-and-towns.html).

codes and the more general categories to which they were assigned. Figures 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 as people are spending time at home.

4 Empirical Strategy

Our empirical strategy exploits two sources of temporal variation to examine the PFD's effect on behavior. First, we use the discrete intra-annual variation in 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,[9] the annual timing of the PFD is exogenous; 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 over the years occurred on different days of the month and different days of the week. With variation in day-of-the-month, the PFD can be isolated from the effect of transfers with fixed timing such as food stamps and Temporary Assistance for Needy Families (TANF) which in Alaska are distributed on the first day of each month, military paychecks which are distributed on the 1st and 15th each month or the nearest business day prior, or social security checks which are distributed on the 2nd, 3rd and 4th Wednesday of each month. Whereas these payments create a regular income cycle each and every month,[10] the PFD's once-a-year nature allows it to be separately identified. With variation in the day-of-the-week, the PFD can be separately identified from the effects of the weekend-vs-

[9]Based on personal conversations with current and former government officials involved in the creation of the PFD program.

[10]Most salaries in the United States are paid weekly or every other week (www.bls.gov/opub/btn/volume-3/how-frequently-do-private-businesses-pay-workers.htm), creating day-of-the-week patterns in incomes as opposed to day-of-month-patterns.

weekday. Second, we use the inter-annual variation in the size of the PFD payment to both provide additional identification of the effect of interest and to explore whether behavior is sensitive to the amount received. As we explain in Section 2 above, 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.

Table 1: PFD direct deposit dates, number of deposits, and amounts (2000-2016)

Year	Direct Deposit Date	Day of Week	Number of Direct Deposit Recipients	% Ungarnished [a]	PFD Amount per person ('16 USD)	% Deposits Received First Day	Total cash dispersed first day (million '16 USD)
2000	4-Oct	Wednesday	390,312	96%	2,737	100%	1,030
2001	10-Oct	Wednesday	404,247	96%	2,508	100%	970
2002	9-Oct	Wednesday	424,490	97%	2,056	100%	844
2003	8-Oct	Wednesday	444,268	94%	1,445	100%	605
2004	12-Oct	Tuesday	448,642	94%	1,169	100%	491
2005	12-Oct	Wednesday	459,004	94%	1,039	100%	448
2006	4-Oct	Wednesday	476,775	93%	1,318	39%	227
2007	3-Oct	Wednesday	493,997	93%	1,915	45%	395
2008	12-Sep	Friday	497,739	92%	3,644	100%	1,670
2009	8-Oct	Thursday	514,217	93%	1,460	100%	702
2010	7-Oct	Thursday	527,868	92%	1,410	100%	684
2011	6-Oct	Thursday	523,756	91%	1,253	100%	594
2012	4-Oct	Thursday	518,334	90%	918	100%	429
2013	3-Oct	Thursday	512,955	89%	927	100%	426
2014	2-Oct	Thursday	518,986	88%	1,910	100%	874
2015	1-Oct	Thursday	532,672	87%	2,098	100%	976
2016	6-Oct	Thursday	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 author's calculations based on fully untarnished payments made to recipients on the first payout date.

[a]Garnishments may be involuntary (e.g. child support or uncollected government fees) or voluntary (e.g. tax exempt college savings or charitable contribution).

To test the effect of the cash payment on the activities of interest, we estimate the empirical model described in its simplest form in Eq. 1 via OLS.[11] We later extend this model to test the persistence of the effect over time and to investigate the marginal response

[11]As the call data represent counts, 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.5

to the total size of distribution. Eq. 1 takes the form

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

where y_t is the count of policing incidents on day t related to either violence, substance abuse, noise/parties, property crime, or requests for medical assistance (each modeled separately); PFD_t is a 0-1 dummy taking a value of 1 if t equals the date of the first PFD distribution and zero for all other dates; 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. For example, if the PFD issued at sometime on Thursday, β_1 will capture the effects of PFD_t from Friday at 12:00am to 11:59pm.

The specification in Eq. 1 includes a host of control variables and fixed effects. M_t is a vector of month $\times$ year (month-by-year) fixed effects, which capture changes to average incomes, unemployment, population, police department resources, and other similar effects. Following the daily crime literature of Jacob and Lefgren (2003) and Foley (2011), W_t represents a vector of weather control variables (precipitation, maximum daily temperature, and snow depth). Controlling for weather reduces observed variance and enables more precise estimates. Most of the seasonal (time of year) effects, however, is captured by the month-by-year fixed effects. τ_t is a vector of special date, holiday, day-of-week, and day-of-month effects. τ includes day-of-week fixed effects (Monday, Tuesday, etc.) addressing weekend-versus-weekday effects or intra-week cyclicality, and a 5th-order polynomial day-of-month trend to account for events that occur on a regular monthly schedule, such as rental payments and certain other income receipts (e.g. social security, food stamps, or other welfare receipt). τ also includes a vector of special date dummy variables. These dates include military pay dates (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 4th, Labor Day and Labor Day weekend, Columbus Day, Halloween/proceeding weekend, Thanksgiving, Christmas Day, and federal holidays which are given to many public employees if a major holiday falls on a weekend. Much like weather, including special date indicator variables allows for more precise effect estimates by reducing the variance in the counter-factual days.

While month-by-year fixed effects eliminate a large portion of the persistence in the time series, we address any residual structure in ϵ through heteroskedasticity- and autocorrelation-consistent Newey-West estimators (Newey and West, 1987). A remaining 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 distribution, it may increase its presence and 'observe' more crime taking place (whether the actual underlying level changes or not). While our conversations with the APD suggest that staffing effort does not change around the time of PFD distribution, to test for such a change, we estimate Eq. 1 on a subset of activities that should capture enforcement activities: traffic stops and issuance of parking tickets.

To investigate whether there is any persistence in the PFD-effect beyond the first full day following distribution, we adopt a similar event-analysis strategy as Evans and Moore (2011), broadening the time window for the indicator variable PFD_t in Eq. 1 to one-week intervals, from two weeks before the PFD distribution to four weeks after. Persistence of the PFD effect is estimated from Eq. 2

$$y_t = \beta_0 + \sum_{i=-2}^{4} \beta_i PFD_{it} + \gamma W_t + M_t + \tau_t + \epsilon_t \tag{2}$$

where PFD_{it} is a dummy variable taking a value of 1 if day t is in the ith week before/after distribution and zero otherwise. All other variables are defined as in Eq. 1. We define weeks as the 7-day periods starting from the day the PFD is distributed.[12]

Assessing the persistence of the effect of the PFD on our outcomes allows us to determine

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

its cumulative impact (if any) or if most/all of the change in behavior immediately following receipt merely represents an inter-temporal reallocation (as found by Evans and Moore, 2011). In order to avoid conflating the persistence of the effect from first-day recipients (who received PFD by direct deposit) from the effect of new recipients who receive their PFDs later by check, we narrow our sample period to the years 2010-2016. Over this period, direct deposits and checks were issued to all recipients on the same day. This restriction is in contrast to the sample period used in Evans and Moore (2011), 2000-2006, a window that is problematic for the purpose at looking at the persistence of the PFD's effect: between 2001 and 2005, physical checks—accounting for 25% of the PFD distributions—were mailed one week following the first direct deposit payment. The results of the persistence extension are presented in Section 5.1.

As shown in Table 1, PFD amounts have varied considerably year-to-year. This variation is seen in both the amount that each recipient is paid (between 918 and 3,644 2016 USD) but also 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. To test for potential response to distribution size, we estimate the model in Eq. 3.

$$y_t = \beta_0+\beta_1 PFD_t+\beta_2 PFD_t\times Amount_t+\beta_3 PFD_t\times Amount_t^2+\beta_4 Mil_t+\gamma W_t+M_t+\tau_t+\epsilon_t \quad (3)$$

where y_t measures the daily police activity for a particular category, PFD_t is a dummy taking a value of 1 if t occurs during the first full day 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. These totals are shown in the rightmost column of Table 1. The other control variables are defined as in Eq. 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. The resulting estimates are presented in Section 5.2.

To provide context for our PFD estimates, we compare them to changes in police activity stemming from a transfer payment that has been investigated in past studies, food stamp (SNAP) payments. SNAP and PFD differ both by their universal and non-universal and unconditional and in-kind nature. We single out the SNAP program as it is an important social program which provides a predictable and steady stream of benefits to income and asset-eligible households. SNAP has a large base of participation and, unlike Temporary Assistance for Needy Families, payments do not end after a given amount of time. The per-household SNAP benefits in Alaska depend on the 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 using through estimation of Eq. 4

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

where $week_t^{PFD}$ is a dummy taking a value of 1 when day t falls in the week after PFD distribution and zero otherwise, $amount_t^{PFD}$ is total PFD distributed as part of the first direct deposit (in millions of dollars), $week_t^{SNAP}$ 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 total SNAP payments made to Alaskans for the month that t falls. 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, etc) which would otherwise have the potential to confound identification. Further, most observed variation in $amount_t^{SNAP}$ is driven by a temporary benefit increase enacted as part of the American Recovery and Reinvestment Act of 2009 (effective April 2009 to November 2013). The choice of a one-week window to estimate the PFD effect follows from the discussion in Section 5.1, as follow-up payments to check recipients in week two of the 2000-2009 period confound estimation of the persistence effect with the effect of new money being dispersed. We therefore estimate the marginal effect for only the first week following the PFD distribution and use the persistence estimated in Section 5.1 to extrapolate to the appropriate number of treated days. The week duration for SNAP is chosen based on a combination of the existing literature (Cotti et al. (2016) and Foley (2011)) which uses relatively short durations of ten days or less, empirical evidence from our data suggesting SNAP effects dissipate after one week, simplicity, and symmetry to the PFD estimate. A final consideration is that data on SNAP participation are only complete through 2015, so we restrict estimation to the years 2000-2015.

5 Results

This section discusses the primary results of the analysis, in addition to several extensions and alternative specifications. The estimated results for Eq. 1 are discussed first. We next investigate the persistence of the PFD effect estimated from Eq. 2. Then we explore the implications for variation in the size of the PFD payments on our measured outcomes from Eq. 3. Finally, we provide context for our estimated effects by comparing changes of incidents counts around PFD distribution to changes of incidents counts around food stamp distributions.

The estimated results for Eq. 1 across the six incident-categories of interest are presented in 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 the indicated category of activity. For reference, the mean and standard deviation for each outcome 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 16.8% over the sample average. We find no significant day-after effects in incidents of violence, noise/party complaints, property crime, or calls for medical assistance. Finally, our results appear to be unrelated to police enforcement activity, as traffic stop and parking ticket calls (both activities reflective of police presence) do not change the full day after distribution. As a reference, the increase in same-day substance abuse incidents from the PFD distribution is slightly larger than the increase on the 4th of July and roughly half the increase on New Years Day/Eve, two holidays that are notorious for excessive consumption of controlled substances. However, unlike these two holidays, there is no significant effect in incidents of noise/party complaints for the day after the PFD distribution, suggesting that the PFD drives activity away from people's homes. Table A.3 in the Appendix presents estimates of all special date effects for reference. Appendix Table A.4 presents results with a more parsimonious set of month-by-year and day-of-week controls (omiting the day-of-month polynomial, weather controls, and special dates); these results yield the same inferences.

Table 2: Change in reported incidents, first day after PFD distribution, 2000-2016

	Change in incident count by category:						
	Violence	Substance (Part)	Substance (Full)	Property	Party	Medical	Traffic & Parking
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
First Full Day After PFD Deposit	-0.004	6.105***	6.288***	-0.515	-0.972	0.945	-12.539
	(0.940)	(1.941)	(1.953)	(1.491)	(0.597)	(0.884)	(9.472)
Mean Daily Incident Count	13.63	36.35	43.59	33.70	10.08	14.38	150.56
St. dev Incident Count	4.46	12.68	13.45	9.46	6.68	5.70	64.38
Weather	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day of Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-Month 5th Order Spline	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,210	6,210	6,210	6,210	6,210	6,210	6,210
Adjusted R^2	0.092	0.603	0.601	0.482	0.693	0.516	0.670

*p<0.1; **p<0.05; ***p<0.01

Newey-West Robust Errors in parentheses.

Violence includes: Homicide, assault, and sexual assault. Substance (Part) category includes: incidents of of driving while intoxicated, drunk and disorderly, and drug possession. Substance (Full) broadens this scope by adding hit-and-run calls and liquor law violations. Complete list of special dates accompanies discussion of Eq. 1. Weather includes third order effects for temperature, precipitation, and snow depth.

5.1 Persistence in PFD effect

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 rate 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. In this section, we extend the treatment window to include the four weeks following PFD distribution. As we explain below, evaluating the persistence of the PFD effect benefits from limiting the sample to the years in which PFD payments were issued at around the same time. Therefore, we examine these extended effects on a subsample from 2010 to 2016.

In their analysis of Alaskan mortality in each of the four weeks following the first PFD direct deposit, Evans and Moore (2011) find a large positive effect of the PFD on mortality rates in the first week following the distribution, no effect in the second week, a significant decrease in the third week, and no change in the fourth. Evans and Moore (2011) argue that the net effect of the PFD on mortality is negligible because the mortality increase in the first week following distribution is mostly offset by the decrease in the third (the sum of the effects across the four weeks statistically zero). However, when urban Alaska counties are compared to urban counties in other states of similar temperature or income the net mortality effect is large, positive, and statistically different than zero.

The 2000-2006 sample period used by Evans and Moore (2011) is somewhat problematic for the purpose at looking at the persistence of the PFD's effect: between 2001-2005, physical checks—accounting for 25% of distributions—were always mailed one week following the first direct deposit payment. Additionally, approximately 40% of direct deposit recipients in 2006 received their payments on the first direct deposit day, while the remaining 60% received the PFD two weeks later. These subsequent payments make it difficult to isolate the persistence of the effect over time: since new recipients with potentially different characteristics are receiving their checks later, their activities are being conflated with the decaying effects of the first-round receivers. Therefore, our persistence results will focus on the period between 2010-2016, which has a single issuance date.

To measure persistence, we adopt a similar empirical strategy to Evans and Moore (2011), using discrete one week intervals from two weeks before PFD distribution to four weeks after. The estimates at each week interval, as well as their 95% confidence intervals, are presented for each activity category of interest in Figure 1.[13] Daily substance-abuse incidents show significant increases for the first two weeks following distribution, approximately 17% in the first week, and 10% in the second. Not only is the effect nearly fully persistent for these two weeks following distribution, there is no evidence to suggest the effect is offset by reductions in substance calls in weeks three and four.

With an extended time window (one week), we find that average daily police activity related to property crime experiences a significant decline for the first week after the PFD is issued, with a decrease of approximately 12% relative to the sample mean. The significant week-after effect is largely driven by decreased activities during days that experience above-average property crimes (i.e., Monday to Wednesday). Like substance-related activities, this effect is not offset in the later periods. This decline is consistent with past findings of declines in property crime associated with the timing of benefit payments. For example, Foley (2011) finds a significant decrease in such calls in the ten-day period following distribution of food stamp payments.

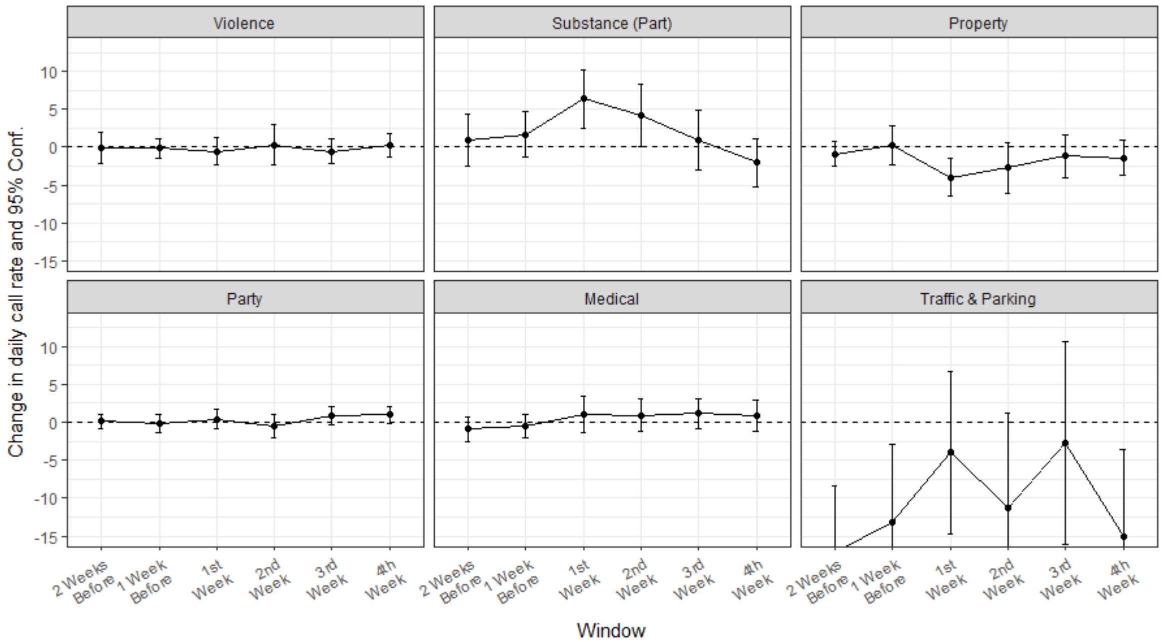
5.2 Variation in Cash Transfer Amount

Given the variation in the size of the payments and the number of individuals receiving the PFD, we investigate below how our outcomes of interest react to changes in the distribution amount. To best utilize the variation in PFD amounts, we revert back to the full sample from 2000-2016.

From the coefficients estimated in Eq. 3, the marginal effect of an additional $100 million

[13] Appendix Table A.6 presents the results as a table and Appendix Figure A.2 shows the omitted Substance (Full) result.

Figure 1: Persistence of the PFD effect, by week



in first-day cash is evaluated at a given level of $Amount$ is $\beta_2 + 2 \times \beta_3 \times Amount$. We evaluate the marginal effect at the average PFD dispersement, which between 2000 and 2016 is approximately \$697 million in 2016 dollars. The estimated marginal effects for the day-after dispersement are presented in the first row in Table 3. For the seven outcome categories, only violence is statistically responsive the day after PFD distribution to deviations away from the average PFD distribution size. We find an additional \$100 million in total PFD distribution results in a 0.7 incident increase in violence the first day following distribution. In elasticity terms, this translates to a 4% increase in violence for a 10% increase in distribution.

There are at least two reasons that marginal effects may not be confined to the first full day following distribution. First, given that we observe persistence of the PFD's effect, it could be that larger amounts of money may induce longer lasting effects rather than a more intense effect on the first day. Second, past studies have shown that responses to income receipt are sometimes delayed to particular days of the week. Cotti et al. (2016) shows such an effect for driving under the influence on the weekend following food stamp receipt. To incorporate such an effect, we estimate the marginal effect of a larger PFD on the first weekend following distribution. For completeness, such that every day-of-week is being included in the treatment window, we also estimate the marginal effect for the first full week. The estimated marginal effects for these two additional time windows are presented in Table 3. For the seven outcome categories, none are statistically responsive to the total distribution size the weekend following distribution. Looking at the full week following distribution, larger distribution amounts have a statistically significant effect for several outcome categories. For a $100 million dollar increase in the distribution (14% above the average distribution), substance-related activities experience approximately a 1 incident-per-day increase, or a 2% increase over the average daily rate. Additionally, in the week following distribution, a $100 million increase in PFD size is associated with an increase in calls for medical assistance of .37 calls-per-day (about a 2% increase relative to the average rate). One potential reason for finding an effect in the full week after distribution but not in the one day window is that the one day window reflects a changing day-of-week (i.e. high payout years happen to be issued on Friday/Saturday). In contrast, looking at the full week after distribution includes each day-of-week exactly once for each level of PFD payment amount. Comparing the week-after to the weekend-after results provides some support of this idea as both periods have fixed days-of-week included and the coefficients are of the same sign for all of the outcomes.

Table 3: Marginal Effect of Additional $100 million PFD in specified time window, 2000-2016

	Outcome Categories						
	Violence	Substance (Part)	Substance (Full)	Property	Party	Medical	Traffic & Parking
First Full Day	0.69***	-0.69	-0.789	0.021	-0.989	0.183	-0.718
	(0.217)	(0.836)	(0.732)	(0.2)	(0.624)	(0.319)	(3.51)
First Weekend	0.071	0.67	0.782	-0.131	-0.293	0.132	-0.2
	(0.244)	(0.622)	(0.688)	(0.198)	(0.378)	(0.163)	(1.366)
First Full Week	0.004	0.978**	1.104***	-0.001	-0.309	0.37***	-1.132
	(0.122)	(0.418)	(0.417)	(0.107)	(0.213)	(0.1)	(1.08)

5.3 Comparison to Non-universal/In-kind Payments

In this section we compare our estimated effects to those for non-universal and/or in-kind transfer receipt. Several previous studies have analyzed the short-run effects of income or in-kind transfers using programs such as SNAP (food stamps) (e.g., Shapiro, 2005; Cotti et al., 2016), or social security payments (e.g, Stephens, 2003; Mastrobuoni and Weinberg, 2009; Evans and Moore, 2011). However, these 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 sub-groups than for the population as a whole. Further, SNAP provides in-kind benefits which can only be spent on certain grocery items. Because universal income is sometimes discussed as substitute or complement for more traditional welfare programs, comparing the behavioral responses of PFD receipt to those for such welfare programs provides insight into how these programs might differ. For this comparison, we exploit the discrete first-day-of-the-month timing for Alaskan SNAP payments.

SNAP and the PFD differ in two important respects. First, the universal nature of the PFD makes the average PFD recipient quite different than the average SNAP recipient. This is true with respect to both 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 non-receivers 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 non-recipients) are highest soon after SNAP receipt. To our knowledge, no studies to date explore whether the universality of a cash transfer has a smaller effect on substance abuse than that of SNAP on a per-person basis. 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, unrestricted payments like the PFD could result in a relatively larger increase in substance abuse on a per-person basis. In a survey/meta-analysis of fifty-nine papers, Cuffey et al. (2016) finds a notable gap between the marginal propensity to purchase eligible food from food stamps than from other income.

Table 4 displays the estimated coefficients of interest from Eq. 4. An increase of $1 million in the size of the monthly SNAP distribution has a statistically significant increase of 0.6 in the average number of daily substance abuse incidents in 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. Assuming that the effects of both the PFD and SNAP persist for a week after the distribution, this implies that a $1 million increase in monthly SNAP payments is associated with 4.16 more substance-related incidents than a $1 million increase in PFD payments, which is statistically significant at the 99.9% level.[14] The only other response for which a statistically significant difference exists between the PFD and SNAP distributions is for parties/noise calls, but the difference scale is quite small.

While the above exercise allows us to compare the effects of increasing the same amount of aggregate dollars to both PFD and SNAP recipients, the different size of the two programs means that the average SNAP recipient is receiving more money from the $1 million dollar change in program size. For instance, a $1 million increase represents a 10% increase over the average monthly SNAP distribution, and would have the effect of increasing a SNAP recipient's monthly payment by $15.43. Conversely, a $1 million increase represents a 0.14% increase over the average PFD distribution, and would have the effect of increasing a PFD recipient's payment by $2.25. To address this issue, we use the estimated coefficients in Table 4 to calculate the SNAP- and PFD-distribution elasticities of police activity. The elasticities reported in Table 4 indicate that a one percent increase in the monthly dollars distributed by SNAP induces a 0.165 percent increase in daily substance abuse calls during

[14]As noted in Section 5.1, the PFD-effect is persistent for one to two weeks after distribution, depending on the outcome. Due to the small daily effect of the PFD on substance abuse incidents, however, scaling the estimated daily effect by two weeks (as opposed to one week) does not change our inference regarding the total effect of SNAP on substance-related calls relative to the PFD.

the first week after distribution, which is nearly four times larger than the responsiveness of substance calls from a one percent increase in the size of the PFD (elasticity of 0.047). Based on the literature on substance consumption and food stamps, the larger response to SNAP receipt in substance-related policing activities implies that the non-universal nature of the recipient population dominates the in-kind nature of the payments.

Table 4: Comparison of PFD and SNAP payment effects by category, 2000-2015

	Dependent variable:						
	Violence	SubstancePart	SubstanceFull	Property	Party	Medical	TrafficParking
SNAP Week x SNAP Amount (millions)	0.036	0.599***	0.596***	−0.023	0.018	0.060*	0.448
	(0.034)	(0.094)	(0.102)	(0.051)	(0.028)	(0.033)	(0.358)
PFD Week x PFD Amount (millions)	0.0004	0.002*	0.003*	0.0003	−0.001*	0.001***	−0.006
	(0.001)	(0.001)	(0.002)	(0.001)	(0.0004)	(0.0003)	(0.010)
Observations	5,479	5,479	5,479	5,479	5,479	5,479	5,479
Adjusted R^2	0.098	0.609	0.600	0.487	0.690	0.438	0.661
	Elasticity Hypothesis Test						
SNAP Elasticity	0.026	0.165	0.137	-0.007	0.018	0.042	0.030
PFD Elasticity	0.023	0.047	0.046	0.007	-0.047	0.060	-0.027
SNAP ϵ - PFD ϵ (F-test p-values)	0.004	0.118***	0.091***	-0.014	0.065*	-0.018	0.057

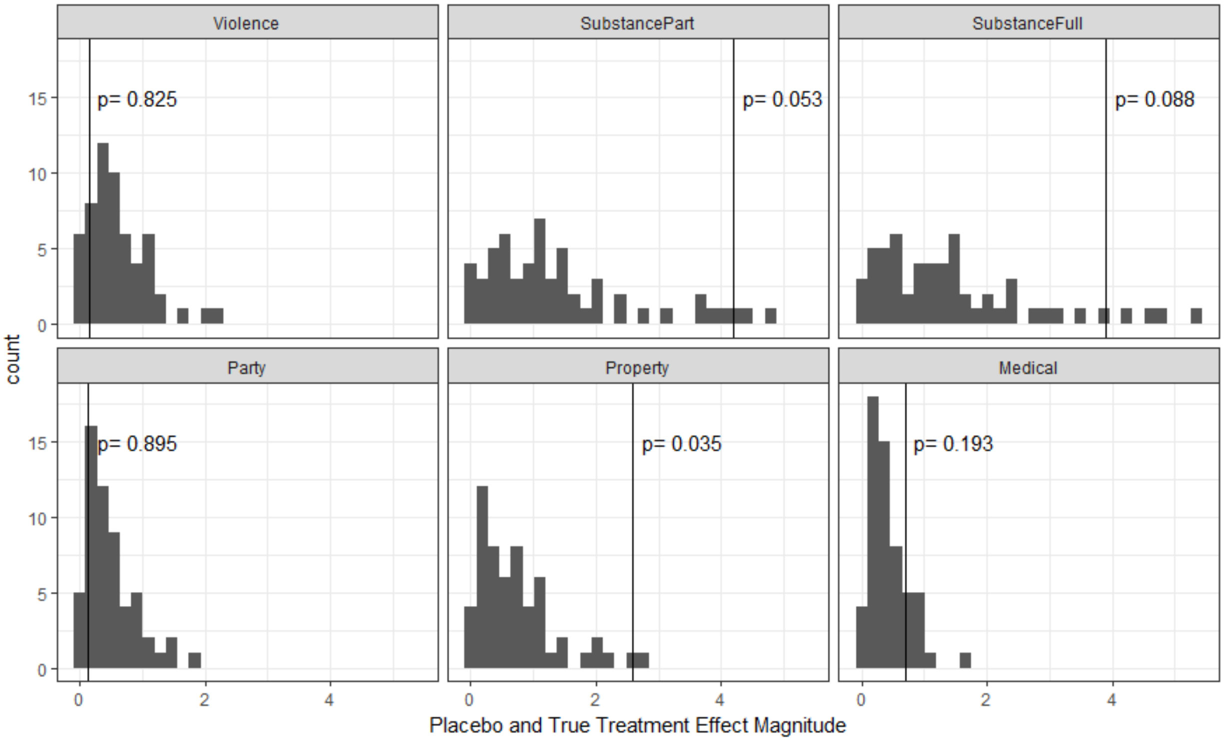
*p<0.1; **p<0.05; ***p<0.01

Note: Newey-West Robust Errors in parentheses.

5.4 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 Eq. 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 varies from year to year, we will estimate more than 52 placebo effects. Figure 2 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. The results of the placebo test are consistent with the those presented in the main results tables. Substance abuse and property crime are both found to have true treatments in the 10th percentile of all

Figure 2: Histogram of placebo effects



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.

6 Conclusion

We examine the behavioral responses to a cash transfer using the world's only continuous universal income program, Alaska's Permanent Fund Dividend, using the universe of daily policing incidents for the state's largest city, Anchorage. Our findings provide several new insights for 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. We observe decreases in property crimes of approximately 12% for a full week after the PFD payments. We also find an increase in substance abuse crime that last approximately two weeks after the distribution. These effects, unlike some previous findings, are not followed by subsequent declines, indicating an overall net positive (negative) effect on substance abuse (property crime), as opposed to a displacement effect. 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 and Puller, 2007; Riddell and 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 substance abuse and violent crimes 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).

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 recipient's 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 which 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, non-universal, and in-kind transfer literature to more universal payments may be problematic due to the differences in the average recipient's response.

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

Andersson, E., P. Lundborg, and J. Vikström (2015). Income receipt and mortality - Evidence from Swedish public sector employees. *Journal of Public Economics 131*, 21–32.

Banerjee, A. and S. Mullainathan (2010, 5). The Shape of Temptation: Implications for the Economic Lives of the Poor.

Borraz, F. and I. Munyo (2014). Conditional Cash Transfers and Crime: Higher Income but also Better Loot.

Carr, J. B. and A. Packham (2017). SNAP Benefits and Crime: Evidence from Changing Disbursement Schedules. *Working Paper*.

Chioda, L., J. M. P. De Mello, and R. R. Soares (2016). Spillovers from conditional cash transfer programs: Bolsa Fam??lia and crime in urban Brazil. *Economics of Education Review 54*, 306–320.

Cotti, C., J. Gordanier, and O. Ozturk (2016, 11). Eat (and Drink) Better Tonight: Food Stamp Benefit Timing and Drunk Driving Fatalities. *American Journal of Health Economics 2*(4), 511–534.

Cuffey, J., T. K. Beatty, and L. Harnack (2016, 12). The potential impact of Supplemental Nutrition Assistance Program (SNAP) restrictions on expenditures: a systematic review. *Public Health Nutrition 19*(17), 3216–3231.

Dobkin, C. and S. L. Puller (2007, 12). The effects of government transfers on monthly cycles in drug abuse, hospitalization and mortality. *Journal of Public Economics 91*(11-12), 2137–2157.

Donnelly, G. (2018). Finland's Basic Income Experiment Will End in 2019.

Evans, D. K. and A. Popova (2014). Cash Transfers and Temptation Goods: a Review of Global Evidence. *The World Bank Policy Research Working Paper May*(6886), 1–3.

Evans, W. N. and T. J. Moore (2011, 12). The short-term mortality consequences of income receipt. *Journal of Public Economics 95*(11-12), 1410–1424.

Foley, C. F. (2011, 2). Welfare Payments and Crime. *Review of Economics and Statistics 93*(1), 97–112.

Grant, B. F. and D. A. Dawson (1996, 10). Alcohol and drug use, abuse, and dependence among welfare recipients. *American Journal of Public Health 86*(10), 1450–4.

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

Jacob, B. A. and L. Lefgren (2003, 11). Are Idle Hands the Devils Workshop? Incapacitation, Concentration, and Juvenile Crime. *American Economic Review 93*(5), 1560–1577.

Jones, D. and I. E. Marinescu (2018, 2). The Labor Market Impacts of Universal and Permanent Cash Transfers: Evidence from the Alaska Permanent Fund. *Working Paper 24312. National Bureau of Economic Research*.

Kueng, L. (2015). Explaining Consumption Excess Sensitivity with Near-Rationality: Evidence from Large Predetermined Payments. *National Bureau of Economic Research*.

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

Mastrobuoni, G. and M. Weinberg (2009, 7). Heterogeneity in Intra-Monthly Consumption Patterns, Self-Control, and Savings at Retirement. *American Economic Journal: Economic Policy 1*(2), 163–189.

Mejia, D. and A. Camacho (2013). The externalities of conditional cash transfer programs on crime: the case of Bogotá's familias en accion program. *Lacea 2013 Annual Meeting*.

Moffitt, R. (1983). An Economic Model of Welfare Stigma. *The American Economic Review 73*(No. 5), 1023–1035.

Newey, W. K. and K. D. West (1987, 5). A Simple, Positive Semi-Definite, Heteroskedasticity and Autocorrelation Consistent Covariance Matrix. *Econometrica 55*(3), 703.

Pollack, H. A. and P. Reuter (2006, 11). Welfare receipt and substance-abuse treatment among low-income mothers: the impact of welfare reform. *American Journal of Public Health 96*(11), 2024–31.

Rainwater, L. (1982). Stigma in Income-Tested Programs. In *Income-Tested Transfer Programs*, Chapter 2, pp. 19–65. Elsevier.

Riddell, C. and R. Riddell (2006). Welfare Checks , Drug Consumption , and Health: Evidence from Vancouver Injection Drug Users. *The Journal of Human Resources 41*(1), 138–161.

Shapiro, J. M. (2005, 2). Is there a daily discount rate? Evidence from the food stamp nutrition cycle. *Journal of Public Economics 89*(2-3), 303–325.

Stephens, M. (2003). "3rd of tha Month": Do Social Security Recipients Smooth Consumption between Checks? *The American Economic Review 93*(1), 406–422.

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

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

White, J. S. and S. Basu (2016). Does the benefits schedule of cash assistance programs affect the purchase of temptation goods? Evidence from Peru. *Journal of Health Economics*.

Wright, R., E. Tekin, V. Topalli, C. McClellan, T. Dickinson, and R. Rosenfeld (2017, 5). Less Cash, Less Crime: Evidence from the Electronic Benefit Transfer Program. *The Journal of Law and Economics 60*(2), 361–383.

Appendix A Appendix Tables and Figures

Figure A.1: Calls per day by day of week and month of year

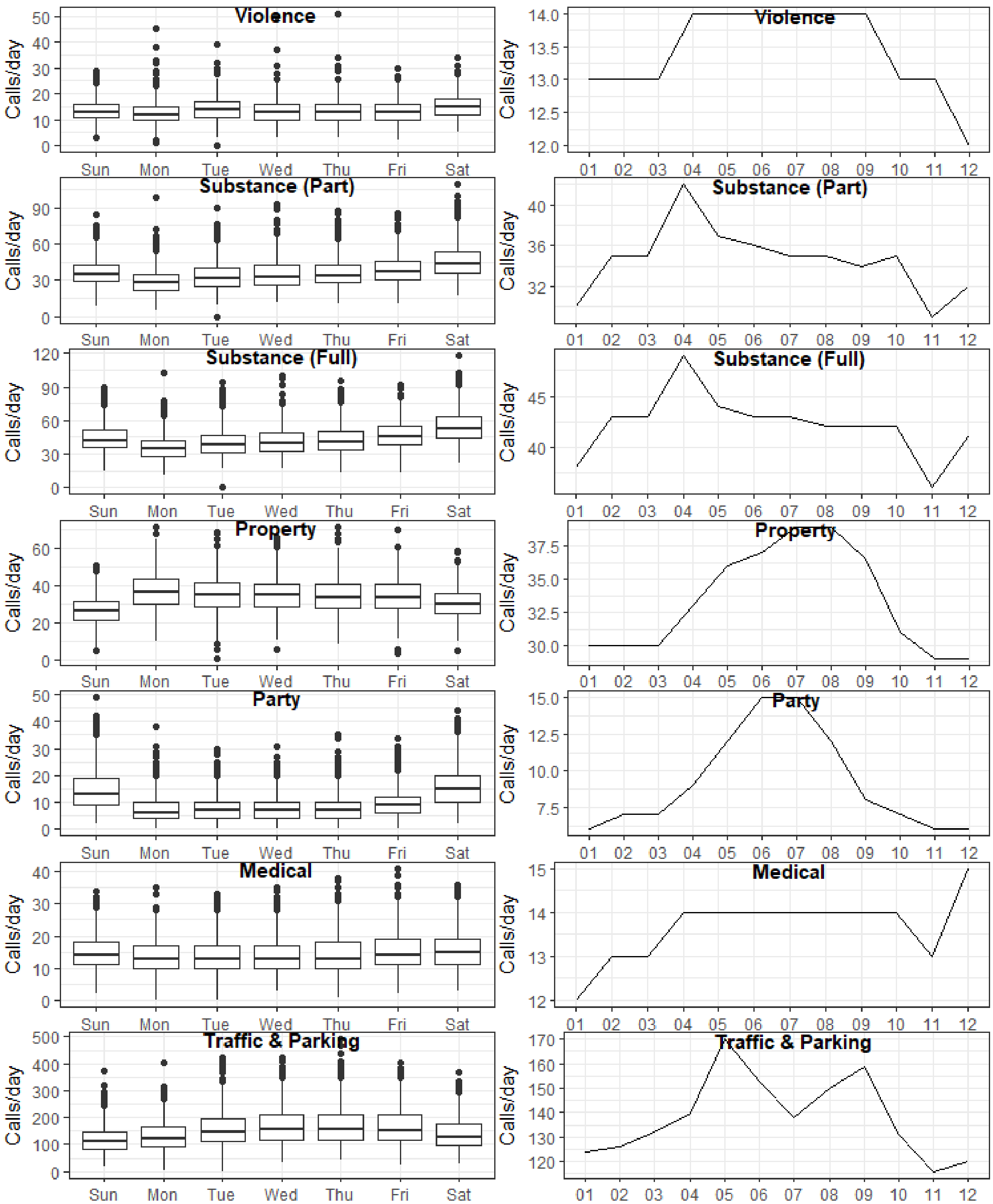


Table A.1: Selected Recent Literature on Transfers, Crime and Mortality

Year	Name	Location	Outcomes	Data	Payments	Payment Target	CCT/ In-kind?	Drugs	Alcohol	Property Crime	Violent Crime	Mortality
2006	Riddel & Riddel	Vancouver	Drug	Hospital records	Welfare payment	Poor	No	+				
2007	Dobkin & Puller	California	Drug	Hospital records	SSI	Poor/Disabled	No	+				
2011	Foley	US Cities[1]	Crime	Police incident reports	Food Stamps	Poor	Yes			-	0	
2011	Chioda, De Mello, & Soares	Sao Paulo	Crime/ Drugs	Neighborhood level policing	CCT: Bolsa Famlia	Poor with children	Yes	-		-	-	
2011	Evans & Moore	USA	Mortality	Death certificate	Various[3]	Various[5]	No					+
2012	Camacho & Meja	Bogata, Col.	Property Crime	Police incident reports	CCT: Familias en Accin	Poor with children	Yes			-		
2014	Evans & Popava	Review study	Drugs/ Alcohol	Various	Various	Poor	Both	-/0	-/0			
2015	Borraz & Munyo	Uruguay	Crime	Police incident reports	CCT: Asignaciones Familiares	Poor with children	Yes			+	0	
2015	Andersson, Lundborg, Vikstrm	Sweden	Mortality	Death certificate	Earned income/Salary	Public employees	No					+[2]
2016	Castellari et al	USA	Alcohol	Alcohol vehicle deaths	Food Stamps	Poor	Yes		-			
2016	White & Basu	Peru	Alcohol	Expense Survey	CCT: Juntos	Poor with children	Yes		+			
2017	Carr & Packham	Chicago and Indiana	Crime	Police incidents & convictions	Food Stamps	Poor	Yes	0	-	0/+	0/+	
2018	This study	Anchorage, AK	Crime	Police incident reports	PFD	Universal/ Population	No	+	+	-	0	

Notes: (+) indicates study found positive effect on outcome from the transfer, (-) a negative effect, and (0) a null/statistically insignificant effect. [1]With high food stamp participation. [2]Low income only. Middle, high income: no effect. [3]Military Salary, SS, tax rebates, PFD. [5]Dependent on payment - includes universal.

Figure A.2: Persistence of the PFD effect, by week

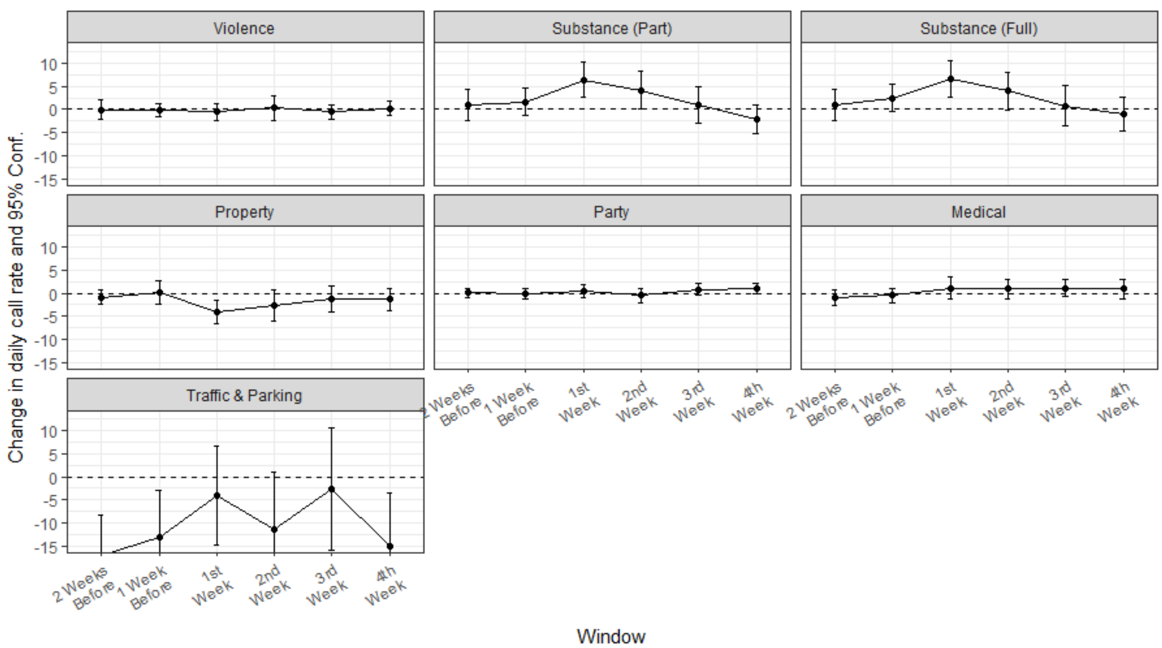


Table A.2: Anchorage Police Call Codes, Daily Average Count, and Assigned Category

Outcome Category	Call Code	Average Daily Calls
Violence	Sexual Assault (in progress)	0.05
	Homicide	0.05
	Assault with Weapon	0.76
	Sexual Assault	1.32
	Sexual Assault of Minor	1.63
	Assault	9.82
Substance (Part)	Driving While Intoxicated	4.69
	Drugs or Forged Perscription	5.45
	Drunk Transport	7.75
	Drunk Problem	18.45
Substance (Full)	Hit And Run with Injury	0.26
	Liquor Law Violation	1.48
	Driving While Intoxicated	4.69
	Drugs or Forged Perscription	5.45
	Hit And Run	5.50
	Drunk Transport	7.75
	Drunk Problem	18.45
Property	Stolen Property	0.03
	Strongarm Robbery	0.50
	Robbery	0.68
	Burglary in Progress	1.09
	Stolen Vehicle	3.25
	Shoplifter	4.13
	Burglary	4.47
	Theft	19.55
Party	Loud Disruptive Party	2.44
	Noise Violation	7.64
Medical	Medic Assist	14.38
Traffic & Parking	Parking Problem	21.93
	Traffic Stop	128.63

Table A.3: Change in average daily calls, first full PFD day, 2000-2016

	Violence	Substance (Part)	Substance (Full)	Property	Party	Medical	Traffic/Parking
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
First Full Day	-0.004	6.105^{***}	6.288^{***}	-0.515	-0.972	0.945	-12.539
	(0.940)	(1.941)	(1.953)	(1.491)	(0.597)	(0.884)	(9.472)
Mil. Pay Day/Day After	-0.118	1.094^{***}	1.165^{***}	0.172	0.206	-0.190	2.269
	(0.210)	(0.418)	(0.446)	(0.333)	(0.189)	(0.206)	(1.861)
New Years Day/Eve	5.560^{***}	11.962^{***}	13.837^{***}	-2.823^{**}	2.789^{***}	1.062	-13.490
	(0.818)	(1.627)	(1.763)	(1.408)	(0.958)	(0.839)	(10.214)
Super Bowl	0.575	-0.147	0.685	-3.634^{***}	-1.206	-0.834	-0.634
	(1.036)	(2.228)	(2.227)	(0.984)	(0.880)	(1.128)	(6.460)
Iditarod	-0.952	4.492^{*}	4.297	-0.991	-2.829^{***}	-0.194	-18.425^{**}
	(1.008)	(2.708)	(2.796)	(0.846)	(1.058)	(0.793)	(7.405)
St Patricks Day	0.689	1.210	-0.304	2.355	-0.304	-0.498	12.716
	(0.796)	(1.382)	(1.560)	(2.289)	(0.692)	(1.001)	(9.676)
Cinco de Mayo	0.250	2.744	3.870^{*}	0.512	0.568	-0.717	-12.288
	(0.992)	(2.060)	(2.309)	(1.657)	(0.983)	(1.364)	(9.778)
July 4th	1.067	5.230^{***}	3.723^{*}	-8.228^{***}	4.958^{***}	-1.009	5.706
	(0.766)	(1.807)	(1.957)	(1.703)	(1.249)	(1.112)	(10.087)
Labor Day Weekend	-0.285	2.009	1.722	-3.162^{***}	1.043	0.522	12.604
	(0.846)	(1.658)	(1.905)	(1.089)	(0.917)	(0.553)	(10.876)
Columbus Day Weekend	1.341^{*}	0.006	0.102	1.760^{*}	-1.622^{***}	0.302	11.266^{*}
	(0.742)	(1.298)	(1.333)	(1.052)	(0.605)	(0.744)	(6.168)
Halloween and Weekend	0.562	-1.233	-0.677	-1.455	2.579^{***}	0.606	-13.887^{***}
	(0.661)	(1.128)	(1.335)	(1.383)	(0.597)	(0.458)	(5.355)
Thanksgiving	-1.801^{**}	-1.415	-3.250^{*}	-10.089^{***}	1.368^{*}	0.360	-56.368^{***}
	(0.812)	(1.570)	(1.768)	(1.498)	(0.779)	(0.724)	(8.690)
Christmas	-2.496^{***}	-6.074^{***}	-9.906^{***}	-12.979^{***}	-0.229	-1.820^{**}	-47.389^{***}
	(0.893)	(1.795)	(1.846)	(1.810)	(0.699)	(0.816)	(8.359)
Federal Holiday	-0.992^{**}	0.395	-0.316	-5.484^{***}	3.663^{***}	0.039	-17.263^{***}
	(0.421)	(0.757)	(0.840)	(0.688)	(0.425)	(0.384)	(4.018)
Constant	13.975^{***}	35.792^{***}	44.939^{***}	31.320^{***}	17.375^{***}	8.087^{***}	93.300^{***}
	(0.473)	(1.696)	(1.173)	(0.707)	(0.460)	(0.337)	(4.396)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weather	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day of Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-Month 5th Order Spline	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,210	6,210	6,210	6,210	6,210	6,210	6,210
Adjusted R^2	0.092	0.603	0.601	0.482	0.693	0.516	0.670

Note: *p<0.1; **p<0.05; ***p<0.01
Newey-West Robust Errors.

Table A.4: Change in average daily calls, first full PFD day, Parsimonious Controls

	Violence	Substance (Part)	Substance (Full)	Property	Party	Medical	Traffic/Parking
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
First Full Day	−0.069	7.788***	7.910***	−0.516	−0.882	1.218	−14.407
	(0.950)	(1.982)	(2.025)	(1.457)	(0.619)	(0.912)	(9.681)
DayOfWeek: Monday	−1.091***	−7.128***	−7.448***	9.789***	−6.983***	−0.605***	13.128***
	(0.213)	(0.331)	(0.360)	(0.376)	(0.197)	(0.181)	(1.803)
DayOfWeek: Tuesday	0.188	−3.418***	−3.925***	8.179***	−7.193***	−0.783***	39.862***
	(0.224)	(0.383)	(0.410)	(0.347)	(0.191)	(0.187)	(2.657)
DayOfWeek: Wednesday	−0.419*	−1.989***	−2.203***	7.939***	−7.091***	−0.861***	47.722***
	(0.218)	(0.400)	(0.431)	(0.346)	(0.198)	(0.190)	(2.779)
DayOfWeek: Thursday	−0.352*	−1.126***	−1.186***	6.923***	−6.822***	−0.142	50.855***
	(0.212)	(0.389)	(0.413)	(0.346)	(0.198)	(0.187)	(2.852)
DayOfWeek: Friday	−0.389**	1.794***	3.090***	7.218***	−5.016***	0.784***	46.173***
	(0.193)	(0.378)	(0.407)	(0.322)	(0.194)	(0.194)	(2.332)
DayOfWeek: Saturday	1.319***	9.004***	10.722***	3.302***	1.143***	0.942***	19.091***
	(0.175)	(0.410)	(0.439)	(0.273)	(0.195)	(0.191)	(1.573)
Constant	13.250***	34.728***	44.791***	27.049***	13.990***	7.881***	84.208***
	(0.474)	(2.177)	(1.563)	(0.671)	(0.580)	(0.213)	(3.289)
Weather	No	No	No	No	No	No	No
Day-of-Month 5th Order Spline	No	No	No	No	No	No	No
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,210	6,210	6,210	6,210	6,210	6,210	6,210
Adjusted R^2	0.077	0.530	0.533	0.439	0.650	0.510	0.654

Note: *p<0.1; **p<0.05; ***p<0.01
Newey-West Robust Errors.

Table A.5: Change in average daily calls, first full PFD day, Poisson count model

	Violence	Substance (Part)	Substance (Full)	Property	Party	Medical	Traffic/Parking
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
First Full Day	−0.0004	0.132***	0.117***	−0.016	−0.147	0.044	−0.066
	(0.069)	(0.042)	(0.036)	(0.046)	(0.102)	(0.053)	(0.062)
Mil. Pay Day/Day After	−0.008	0.027**	0.024**	0.007	0.014	−0.013	0.017
	(0.015)	(0.011)	(0.010)	(0.010)	(0.019)	(0.014)	(0.012)
New Years Day/Eve	0.362***	0.296***	0.280***	−0.119**	0.338***	0.073	−0.108
	(0.047)	(0.035)	(0.032)	(0.050)	(0.079)	(0.052)	(0.078)
Super Bowl	0.045	−0.005	0.015	−0.207***	0.031	−0.063	−0.021
	(0.077)	(0.055)	(0.046)	(0.042)	(0.090)	(0.084)	(0.055)
Iditarod	−0.064	0.057	0.045	−0.060*	−0.114	−0.007	−0.152**
	(0.075)	(0.047)	(0.043)	(0.034)	(0.108)	(0.053)	(0.059)
St Patricks Day	0.052	0.033	−0.009	0.069	−0.045	−0.037	0.068
	(0.058)	(0.041)	(0.039)	(0.072)	(0.086)	(0.077)	(0.063)
Cinco de Mayo	0.018	0.054	0.068*	0.014	0.063	−0.045	−0.071
	(0.071)	(0.041)	(0.041)	(0.047)	(0.083)	(0.094)	(0.051)
July 4th	0.080	0.116***	0.073*	−0.251***	0.181**	−0.073	0.054
	(0.055)	(0.045)	(0.042)	(0.058)	(0.077)	(0.076)	(0.068)
Labor Day Weekend	−0.020	0.047	0.034	−0.083**	0.103	0.033	0.102
	(0.059)	(0.037)	(0.037)	(0.033)	(0.070)	(0.035)	(0.070)
Columbus Day Weekend	0.094*	−0.005	−0.003	0.054	−0.056	0.019	0.077*
	(0.050)	(0.034)	(0.029)	(0.035)	(0.055)	(0.047)	(0.044)
Halloween and Weekend	0.041	−0.026	−0.008	−0.052	0.326***	0.040	−0.097***
	(0.047)	(0.031)	(0.030)	(0.046)	(0.055)	(0.032)	(0.037)
Thanksgiving	−0.175**	−0.070	−0.115**	−0.523***	0.201**	0.028	−0.508***
	(0.078)	(0.054)	(0.052)	(0.079)	(0.084)	(0.051)	(0.073)
Christmas	−0.241***	−0.212***	−0.281***	−0.706***	0.089	−0.135**	−0.520***
	(0.090)	(0.059)	(0.051)	(0.113)	(0.069)	(0.060)	(0.075)
Federal Holiday	−0.078**	0.014	−0.006	−0.161***	0.398***	0.001	−0.129***
	(0.033)	(0.023)	(0.021)	(0.022)	(0.038)	(0.027)	(0.032)
Constant	2.635***	3.591***	3.815***	3.439***	2.931***	2.071***	4.565***
	(0.036)	(0.048)	(0.026)	(0.022)	(0.040)	(0.028)	(0.032)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weather	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day of Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-Month 5th Order Spline	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,210	6,210	6,210	6,210	6,210	6,210	6,210

Note: *p<0.1; **p<0.05; ***p<0.01
Newey-West Robust Errors.

Table A.6: Change in average daily calls at weekly intervals before and after PFD; 2010-2016

	Violence	Substance (Part)	Substance (Full)	Property	Party	Medical	Traffic/Parking
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fourteen to eight before	−0.043	0.861	0.879	−0.876	0.089	−0.931	−17.163***
	(1.045)	(1.741)	(1.723)	(0.795)	(0.496)	(0.826)	(4.445)
Seven to one days before	−0.166	1.645	2.494*	0.198	−0.160	−0.496	−13.197**
	(0.684)	(1.516)	(1.486)	(1.299)	(0.591)	(0.759)	(5.218)
One to seven days after	−0.540	6.360***	6.603***	−4.018***	0.423	1.048	−3.971
	(0.953)	(1.955)	(2.054)	(1.271)	(0.686)	(1.250)	(5.471)
Eight to fourteen days after	0.311	4.207**	4.018*	−2.692	−0.493	0.930	−11.373*
	(1.340)	(2.077)	(2.109)	(1.712)	(0.803)	(1.081)	(6.373)
Fifteen to twenty-one days after	−0.566	0.936	0.745	−1.212	0.809	1.129	−2.749
	(0.828)	(1.987)	(2.223)	(1.409)	(0.606)	(0.973)	(6.781)
Twenty-two to twenty-eight days after	0.225	−2.047	−1.074	−1.398	1.011*	0.858	−15.047**
	(0.801)	(1.609)	(1.864)	(1.199)	(0.571)	(1.029)	(5.865)
Military Pay Day/Day After	−0.416	0.905	0.730	0.428	0.502*	−0.415	1.878
	(0.341)	(0.671)	(0.694)	(0.463)	(0.257)	(0.359)	(2.446)
New Years Day/Eve	5.956***	8.200***	11.542***	−0.547	1.865	2.252*	−26.031***
	(1.213)	(2.558)	(2.183)	(1.761)	(1.565)	(1.322)	(9.439)
Super Bowl	−0.346	−1.488	0.120	−4.181***	−0.210	−2.404*	16.580**
	(0.471)	(4.209)	(4.130)	(1.581)	(1.227)	(1.239)	(7.519)
Iditarod	−2.402	5.112	6.600	−3.249***	−2.072	−1.784*	−13.501
	(1.925)	(4.552)	(5.033)	(1.059)	(1.362)	(1.057)	(8.847)
St Patricks Day	−0.307	3.189*	3.052**	5.663**	0.415	−1.479	26.779**
	(0.917)	(1.910)	(1.408)	(2.402)	(0.964)	(1.443)	(11.967)
Cinco De Mayo	−0.582	9.341***	11.465***	−0.024	−0.658	−3.404*	−1.634
	(0.540)	(2.825)	(3.092)	(2.482)	(0.844)	(1.948)	(9.101)
July 4th	1.620	1.281	1.499	−3.835*	6.151***	−0.233	−6.057
	(1.608)	(2.063)	(2.033)	(2.018)	(1.401)	(2.081)	(14.110)
Labor Day Weekend	−1.691	3.975	4.745	−4.249***	1.212	0.012	23.308**
	(1.463)	(2.978)	(3.362)	(1.236)	(1.391)	(0.582)	(11.033)
Columbus Day Weekend	2.169	−2.012	−1.932	1.556	−1.275	1.409	9.892
	(1.450)	(2.245)	(2.095)	(1.177)	(0.943)	(1.488)	(7.263)
Halloween and Weekend	−0.118	−0.747	−1.631	−2.266*	2.531***	−0.139	−6.962
	(1.059)	(1.444)	(1.351)	(1.170)	(0.891)	(1.046)	(7.046)
Thanksgiving	−1.795	0.156	−0.537	−8.967***	0.466	0.786	−65.353***
	(1.287)	(2.390)	(2.376)	(2.297)	(1.006)	(1.173)	(12.133)
Christmas	−2.505**	−7.002*	−9.037**	−10.392***	−0.186	−2.350*	−40.697***
	(1.219)	(3.768)	(3.613)	(1.540)	(0.832)	(1.218)	(10.603)
Federal Holiday	−2.375***	−0.302	−1.366	−3.796***	3.365***	−0.180	−16.099***
	(0.579)	(1.214)	(1.287)	(0.787)	(0.573)	(0.537)	(5.960)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weather	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day of Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-Month 5th Order Spline	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month x Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,557	2,557	2,557	2,557	2,557	2,557	2,557
Adjusted R^2	0.096	0.621	0.620	0.455	0.635	0.280	0.740

Note: *p<0.1; **p<0.05; ***p<0.01
Newey-West Robust Errors.