Behavioral Effects of a Cash Transfer: Evidence from the World's Only Continuous Universal Income Program

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January 23, 2018

Abstract

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. We exploit the exogenous timing and amount of the PFD payment to identify the average treatment effect of the PFD on the daily number of police calls. We find a 17% increase in the average number of substance-abuse-related police calls 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 substance abuse is 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-related police calls. Additionally, we find that property crime is influenced by the PFD for a full two weeks after the PFD is issued; however, it is not statistically sensitive to the size of the PFD payment. Finally, we find that police calls requesting medical assistance during the week after the PFD payment increase by two percent with a ten percent increase in the size of the PFD payment.

Keywords: Permanent Fund Dividend; Unconditional cash transfer; Welfare effects.

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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 "no-strings-attached" guaranteed cash transfer 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 tout UBI's ability to improve economic security (Thigpen, 2016), but UBI is commonly discussed in the context of a substitute for existing welfare programs. UBI also carries less "welfare stigma," negative feelings associated with participation in income-tested programs, documented by Rainwater (1982) and modeled by Moffitt (1983).

UBI cash transfers are distinct from previously studied payment types 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 previously 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 behavioral effects induced by broader economic spillovers from a large, economy-wide cash injection. 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 as food stamp (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

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

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. In addition, the PFD is large relative to the median household income.[2] Further, the PFD program has been continuously running for over 35 years (since 1982) and based on its political popularity it has been largely viewed as a permanent institution. 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 effects of the PFD payment on outcomes that have previously been shown to be responsive to cash transfers. Specifically, we examine daily incidents of controlled-substance abuse, property crime, and requested medical assistance using daily call-log records of police officers in the Municipality of Anchorage, Alaskas 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 number of police calls. We find a 17% increase in the average number of substance-abuse-related police calls 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 substance abuse is 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-related police calls.

Property crime falls by approximately 12% the week of payment, remains depressed through the following week, but does not respond to the size of payment distribution. Finally, we find that police calls requesting medical assistance during the week after the PFD payment increase by two percent with a ten percent increase in the size of the PFD payment.

Despite the potential benefits of UBI, there is little accumulated knowledge about how

[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.

such a policy might change people's behavior along the margins we investigate. An extensive literature has, however, shown that people tend to exhibit short-run impatience, whereby consumption and economic activity increase immediately following a 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). Indeed, several studies have shown that morbidity and mortality among recipients increase 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 burglary, robbery, and theft by increasing the supply of cash available to potential offenders in the streets (Borraz and Munyo, 2014; Wright et al., 2014). 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 there is substantial evidence suggesting the existence of behavioral responses to cash 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

[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.

of school attendance. Similarly, transfers from welfare programs, such as the Supplemental Nutrition Assistance Program (SNAP, or Food Stamps) (e.g., Foley, 2011; Cotti et al., 2016), 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, therefore, 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 substance abuse and property crimes. We also compare our findings to the behavioral effects stemming from SNAP payments and find that substance abuse is much less responsive to higher distributions in the form of the PFD than in SNAP payments. Thus, our results show 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. Our results confirm that UBI payments affect people's behaviors differently and that previous work on the behavioral effects of conditional cash payments is not useful when thinking about further implementation of universal basic income programs. There are many previous studies examining the effect of transfers on substance abuse and property crime from non-universal income programs, such as welfare payments (e.g., Foley, 2011; Riddell and Riddell, 2006; Cotti et al., 2016), disability insurance (e.g., Dobkin and Puller, 2007), and conditional cash transfer programs (e.g., Mejia and Camacho, 2013; Chioda et al., 2016). The distinct nature of the PFD, however, could induce changes in substance abuse and property crimes 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.[4] Thus, unrestricted payments like the PFD could have a relatively larger absolute change in substance abuse and property crimes. 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 substance abuse and property crimes if the average PFD recipient is less likely to partake in criminal activity.[5]

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 [6] 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 Hsieh (2003) and Kueng (2015), who use self-selected samples of Alaska residents, we use the universe of police call logs in Anchorage, which allows us to draw conclusions about how receipt of the PFD at the population level affects behavioral responses. 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

[4] 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.

[5] 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.

[6] 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.

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 consumption as a result of PFD receipt; however, we are able to explicitly show that substance abuse does indeed rise in response to the income receipt, in contrast to their findings. 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. Substance use crimes 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, the results presented in this paper provide evidence that there are limits to crime reduction but there may be crime related costs in the form of substance abuse.

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 Universal Basic Income. Sections 3 and 4 describes the police call data that was used for the analysis and the empirical model used to estimate effect of the cash receipt on observed categories police calls. 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 Background

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 Alaska Permanent Fund Dividend (PFD) was paid out of general revenues rather than fund earnings. Thereafter, the payments were made from the fund's earnings. The formula 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 operated as a public trust, much like trust funds established for pension funds. This means Fund managers must balance the idea of income producing against ordinary prudence about risk. In its simplest terms, the Permanent Fund is a savings account geared to make money for Alaskans. It is managed by the Alaska Permanent Fund Corporation (APFC). 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. Later, in 1990 global public debt and equities were traded. 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 funds 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 $3269 (including a one-time supplement of $1200 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. PFDs 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, 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 is about 42% of the construction sector's GDP or 76% of the Whole-trade sector's. In addition to the sheer size, the PFD is all 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. 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. 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.

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 policing activity to 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.[7] The primary data for the analysis are call-log records for officers of the Anchorage Police Department. Such calls can be made, for example, when an officer responds to 911 call, initiates a traffic stop, or services a warrant. Each time-stamped call is associated with a particular activity, self-reported by the officer

[7]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).

and coded to one of 99 possible activities by Anchorage Police Department (APD). APD provided the complete call log for the years 2000-2016. For our analysis we aggregate these event level data up to counts at the day level for each activity code. We further categorize these specific codes to more general activity types corresponding to substance abuse, property crimes, noise violations and parties, and requests for medical assistance. Table 6 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. Figures 3 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 PFDs 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, [8] 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 historically occurred on different days of the month and different days of the week. Second, we use the inter-annual variation in the size of the PFD payment to explore whether behavior is sensitive to the amount received. As we explain in Section 2 above, the size of the PFD determined based on the returns of a diversified portfolio rather than on contemporaneous oil prices or specific factors related to the state

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

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

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 voluntary (e.g. tax exempt college savings).

To test the effect of the cash payment on the activities of interest, we estimate via OLS[9] the empirical model described in its simplest form by Eq. 1. We later extend this model to test the persistence of the effect over time and to investigate the response 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 constructed proxy for substance abuse, noise/parties, property crime, or requests for medical assistance; PFD_t is a 0-1 dummy taking a value of 1 if t equals the

[9] 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 available by request from the authors.

date of the first PFD distribution and zero for all other dates; W_t represents a vector of weather control variables (precipitation, maximum daily temperature, and snow depth), M_t is vector of month-by-year fixed effects, τ_t is a vector of special date and holiday effects, and a day-of-month 5th order polynomial trend; and ϵ_t is the model error. β_i's and γ are coefficient 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, PFD_t will capture effects from Friday at 12:00am to 11:59pm.

Controlling for weather, W_t, reduces observed variance and enables more precise estimates. Most of the seasonal (time of year) effects however will be captured in by month-by-year fixed effects represented by M. Month-by-year effects will also capture changes to average incomes, unemployment, population, police department resources, and other similar effects. τ includes day-of-week effects addressing weekend-versus-weekday effects, and a 5th order day-of-month trend to account for events that occur on a regular monthly schedule, such a rental payments and certain other income receipt (e.g. social security). Finally, 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 Years 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. In other words, 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 empirical strategy to Evans and Moore (2011) (event analysis), 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.

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

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

where y_t measures the daily calls received for a particular behavior 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 x number of first-day recipients) measured in 100 million 2016 USD. These totals are shown in the rightmost column of Table 1. The other controls 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 call 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 $week_t^{SNAP} * amount_t^{SNAP}$ 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 will therefore estimate the marginal effect for only the first week following 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 as well as several extensions and alternative specifications. The estimated results for Eq. 1 are discussed first. We next investigate the persistence of the PFD effect across several different time horizons. Then we explore the implications for variation in the size of the PFD payments on our measured outcomes. Finally, we provide context for our estimated effects by comparing changes in call-incidents around PFD distribution to changes in call incidents around food stamp distributions.

The estimated results for Eq. 1 across the six call-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 of calls received 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 the substance abuse indicators, there is an increase of ≈ 6 calls the after the first PFD direct deposit, an increase of 16.8% over the sample average. We find no significant day-after effects in calls regarding 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 daily increase in substance abuse calls 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 calls regarding noise/party complaints for the day after the PFD distribution, suggesting that PFD-related substance abuse activities are taking place away from peoples homes. Table 7 in the Appendix presents estimates of all special date effects for reference.

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 the next section (Section 5.1), we extend the treatment window to include the four weeks following PFD distribution in. Reductions in property crime are found in an extended treatment window. As we explain below, it is helpful to have a sample when payments were issued at around the same time when evaluating the length of time the PFD's effect lasts. Therefore, we examine these extended effects on a subsample from 2010 to 2016.

Table 2: Change in calls, first day after distribution, 2000-2016

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

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

Newey-West Robust Errors in parentheses.

Substance (Part) category includes calls for driving while intoxicated, drunk and disorderly calls, 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.

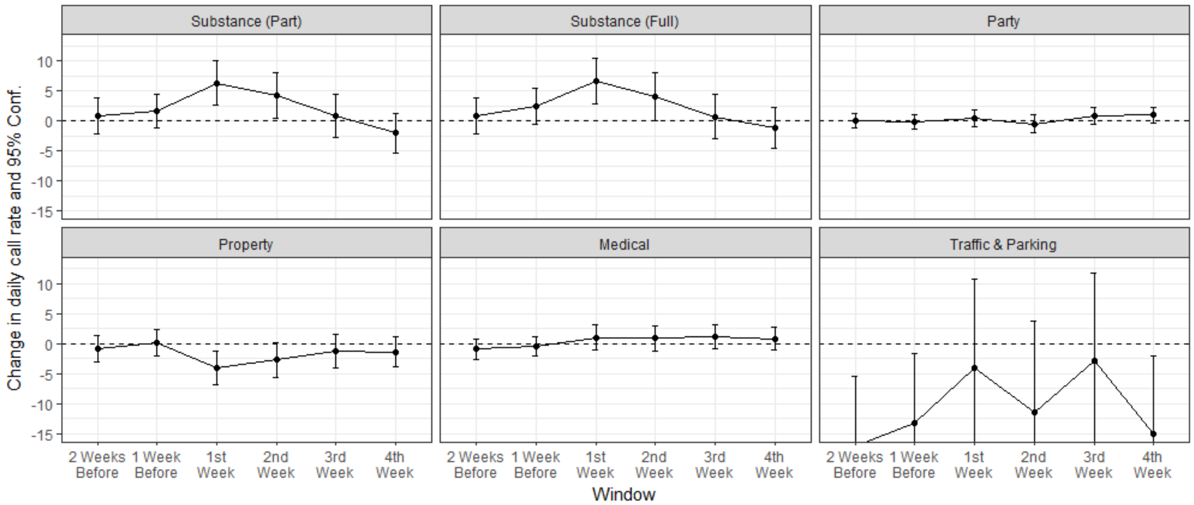
5.1 Persistence in PFD effect

While the first full day after distribution experiences particularly intense treatment, the PFD's effect is unlikely to be restricted to only this day. 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) as we explained above 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, in 2006 approximately 40% of direct deposit recipients 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, as new recipients of potentially different characteristics are receiving their checks and these new recipients 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 dates.

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 call category of interest in Figure 1. Substance calls show significant increases for the first two weeks following distribution, approximately the same 10% increase that is found

Figure 1: Persistence of the PFD effect, by week



for the first day after distribution. Not only is the effect almost 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.

We find with an extended time window (one week) property-crime calls see a decline following PFD distribution. Average daily calls related to property crime experience a significant decline for the first two weeks after the PFD is issued, with a decrease of approximately 12% and 8% in weeks one and two, respectively, relative to the sample mean. The significant week-after effect is largely driven by decreased calls during days that experience above-average property crimes (i.e., Monday to Wednesday). Like substance calls, 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 in first-day cash is evaluated at a given level of *Amount* is $\beta_2 + 2 * \beta_3 * Amount$. For our results, we will evaluate the marginal effect at the average of 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 six outcome categories, none are statistically responsive the day after PFD distribution to deviations away from the average PFD distribution size. This implies that a $600 million PFD distribution will lead to approximately the same increase in calls the first full day after distribution as a $500 million distribution. However, there are at least two reasons that such a marginal effect may not be confined to the first full day following distribution. First, given that we observe persistence of the PFD's effect in the previous section, it could be the case that larger amounts of money may induce longer lasting effects rather than a more intense effect on the first day. Second, past studies have also 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 weekends 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 six outcome categories, none are statistically responsive to the total distribution size the weekend following distribution. However, 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 calls experience approximately a 1 call-per-day increase, or a 2% increase over the average daily call rate. Additionally, in the week following distribution, a $100 million increase in PFD size is associated with an increase in medical calls of .37 calls-per-day (about a 2% increase relative to the average level of activity). 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 and of a similar magnitude for three of the six.

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

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

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

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. Many past studies that have analyzed the short-run effects of income or in-kind transfers have used programs such as food stamps as in Shapiro (2005) and Cotti et al. (2016), or social security as in Stephens (2003), Mastrobuoni and Weinberg (2009), and Evans and Moore (2011). However, these programs have important limitations when considering their implications for a universal income. Social security receipt is restricted to the elderly and food stamps 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, the food stamp program - more formally the Supplemental Nutrition Assistance Program (SNAP) program - 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 will exploit the discrete first-day-of-the-month timing for Alaskan SNAP payments.

SNAP and the PFD differ in two important respects. The universal nature of the PFD makes the average PFD recipient quite different than the average SNAP recipient. This is true both with respect to income (by definition) but also across other sociodemographic dimensions. While Grant and Dawson (1996) provide evidence that the welfare receiving population does not systematic differ from non-receivers in their alcohol abuse, Pollack and Reuter (2006) finds that drug usage was higher among welfare-receiving individuals and Hastings and Washington (2010) show alcohol and tobacco purchases for SNAP recipients (relative to non-recipients) are highest soon after SNAP receipt. No study was identified showing the universal nature of the PFD should make its effect on substance abuse smaller than that of SNAP on a per-person basis. The second factor 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. 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.

To appropriately compare the PFD to SNAP, we scale up the marginal effects estimated in Eq. 4 to the number of days treated by both payment types. As the estimated SNAP effect covers each first-week of each month, we scale it by 7. As we find in Section 5.1 The PFD effect is persistent for one to two weeks after distribution (depending on outcome).

For robustness, we will scale the PFD effect assuming both a one week and two week persistence. This will be an over-estimate for effects that are more short-lived, but the inference does not change between the two assumed durations. From the coefficient estimates of Eq. 4 and the scaling factors, we conduct a f-test that the call change associated with a $1 million increase for SNAP distribution is equal to the change in calls associated with a $1 million increase in PFD. The coefficient estimates presented in the upper region of Table 4; the difference in the scaled coefficient and formal tests for their differences are presented in the lower regions. The results indicate that a $1 million increase in monthly SNAP payments is associated with 4 more substance-related calls than a $1 million increase in PFD payments. This difference is significant at the 99.9% level. The only other response for which a significant difference exists is for medical calls, which increase by an additional 0.4 calls for a $1 million increase in SNAP payments relative to a $1 million increase in PFD payments. While this exercise allows us to add the same amount of aggregate dollars to both the 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. To address this issue, we normalize our results by scaling down the coefficient for the SNAP effect such that it represents an equivalent per-person value. The scaling factor is simply the ratio between the average number of PFD and SNAP recipients observed in the sample period, approximately 6.9. The results of the analysis with the rescaled SNAP values are presented in Table 5. Even after the normalization, we find significantly higher responsiveness to SNAP than the PFD for substance abuse categories, but medical calls no longer show significantly different responses to the two distributions. From the literature on substance consumption and food stamps, the larger response to SNAP receipt in substance calls implies that the non-universal nature of the recipient population is dominating the in-kind nature of the payments.

Table 4: Per-dollar comparison of PFD and SNAP payment effects by call category

	Substance (Part)	Substance (Full)	Party	Property	Medical	Traffic
PFD Week x PFD Amount (millions)	0.002*	0.003*	−0.001*	0.0003	0.001***	−0.006
	(0.001)	(0.002)	(0.0004)	(0.001)	(0.0003)	(0.010)
SNAP Week x SNAP Amount (millions)	0.599***	0.596***	0.018	−0.023	0.060*	0.448
	(0.094)	(0.102)	(0.028)	(0.051)	(0.033)	(0.358)
Observations	5,479	5,479	5,479	5,479	5,479	5,479
Adjusted R^2	0.609	0.600	0.690	0.487	0.438	0.661
Test: 7 x SNAP Week x SNAP Amount - 7 x PFD 1st Week x PFD Amount = 0						
SNAP - PFD Call Change	4.16	4.13	0.14	-0.16	0.40	3.22
f-Statistic	6.29	5.82	0.69	-0.45	1.77	1.28
p.value	0.00	0.00	0.49	0.65	0.08	0.20
Test: 7 x SNAP Week x SNAP Amount - 14 x PFD 1st Week x PFD Amount = 0						
SNAP - PFD Call Change	4.15	4.12	0.14	-0.16	0.40	3.23
f-Statistic	6.28	5.81	0.70	-0.46	1.75	1.29
p.value	0.00	0.00	0.48	0.65	0.08	0.20

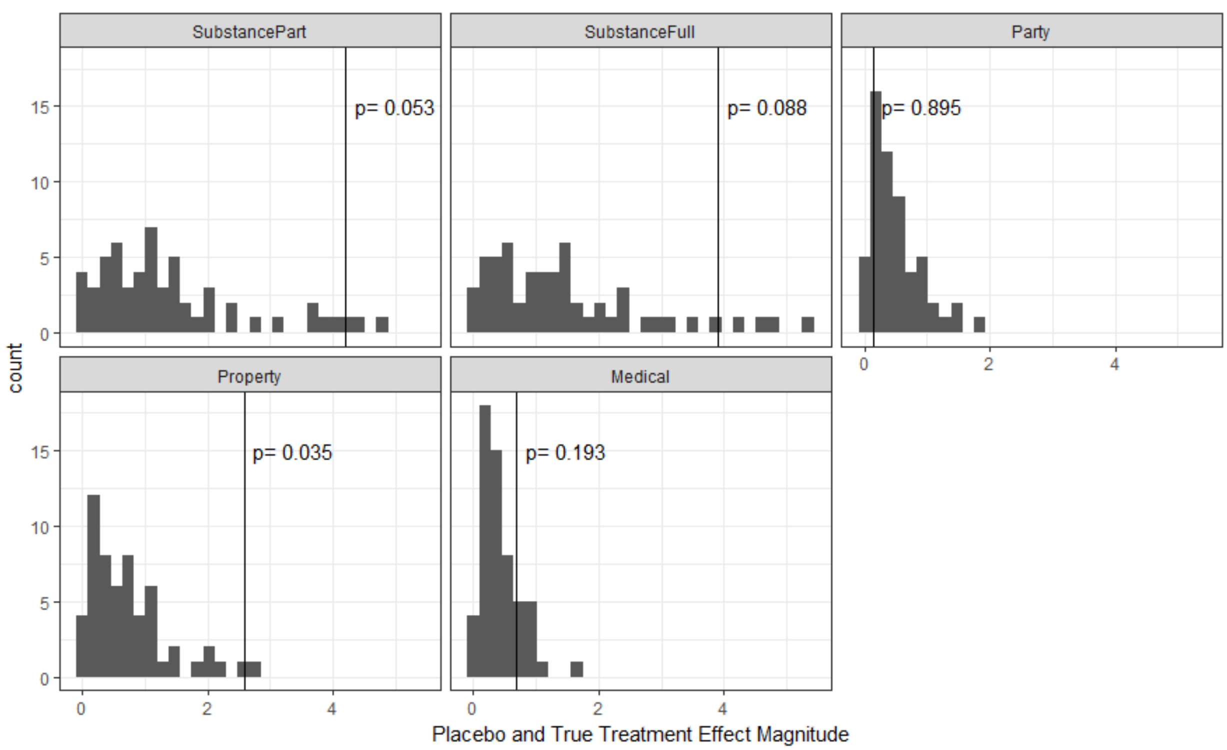
5.4 Robustness of findings

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 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 estimated effects. Placebo effects for these outcomes of similar or larger magnitude are also easily explained by spillovers from holidays or call accounting issues from daylight savings time.

Table 5: Per-dollar, per-recipient comparison of PFD and SNAP payment effects by call category

	Substance (Part)	Substance (Full)	Party	Property	Medical	Traffic
PFD Week x PFD Amount (millions)	0.002*	0.003*	−0.001*	0.0003	0.001***	−0.006
	(0.001)	(0.002)	(0.0004)	(0.001)	(0.0003)	(0.010)
SNAP Week x SNAP Amount (millions)	0.599***	0.596***	0.018	−0.023	0.060*	0.448
	(0.094)	(0.102)	(0.028)	(0.051)	(0.033)	(0.358)
Observations	5,479	5,479	5,479	5,479	5,479	5,479
Adjusted R^2	0.609	0.600	0.690	0.487	0.438	0.661
Avg. PFD to SNAP Recipient Ratio			6.87			
SNAP effect / Ratio	0.09	0.09	0.00	-0.00	0.01	0.07
Test: 7 x SNAP Week x SNAP Amount / Ratio - 7 x PFD 1st Week x PFD Amount = 0						
SNAP - PFD Call Change	0.59	0.59	0.02	-0.03	0.05	0.50
statistic	6.15	5.70	0.81	-0.48	1.58	1.35
p.value	0.00	0.00	0.42	0.63	0.11	0.18
Test: 7 x SNAP Week x SNAP Amount / Ratio - 14 x PFD 1st Week x PFD Amount = 0						
SNAP - PFD Call Change	0.58	0.57	0.03	-0.03	0.04	0.54
f-Statistic	5.89	5.47	0.96	-0.52	1.31	1.39
p.value	0.00	0.00	0.34	0.60	0.19	0.16

Figure 2: Histogram of placebo effects



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 police call-logs 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 find an increase in police calls associated with abuse of controlled substances that last approximately two weeks after the distribution. During the same period, we observe decreases in property crimes. These effects, unlike some previous findings, are 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 is 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 recipients consumption of temptation goods, or controlled substances. 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

SNAP payments to generate larger per-dollar increases in substance abuse. The results of this comparison provide quantitative evidence about 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 recipients 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 call 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.

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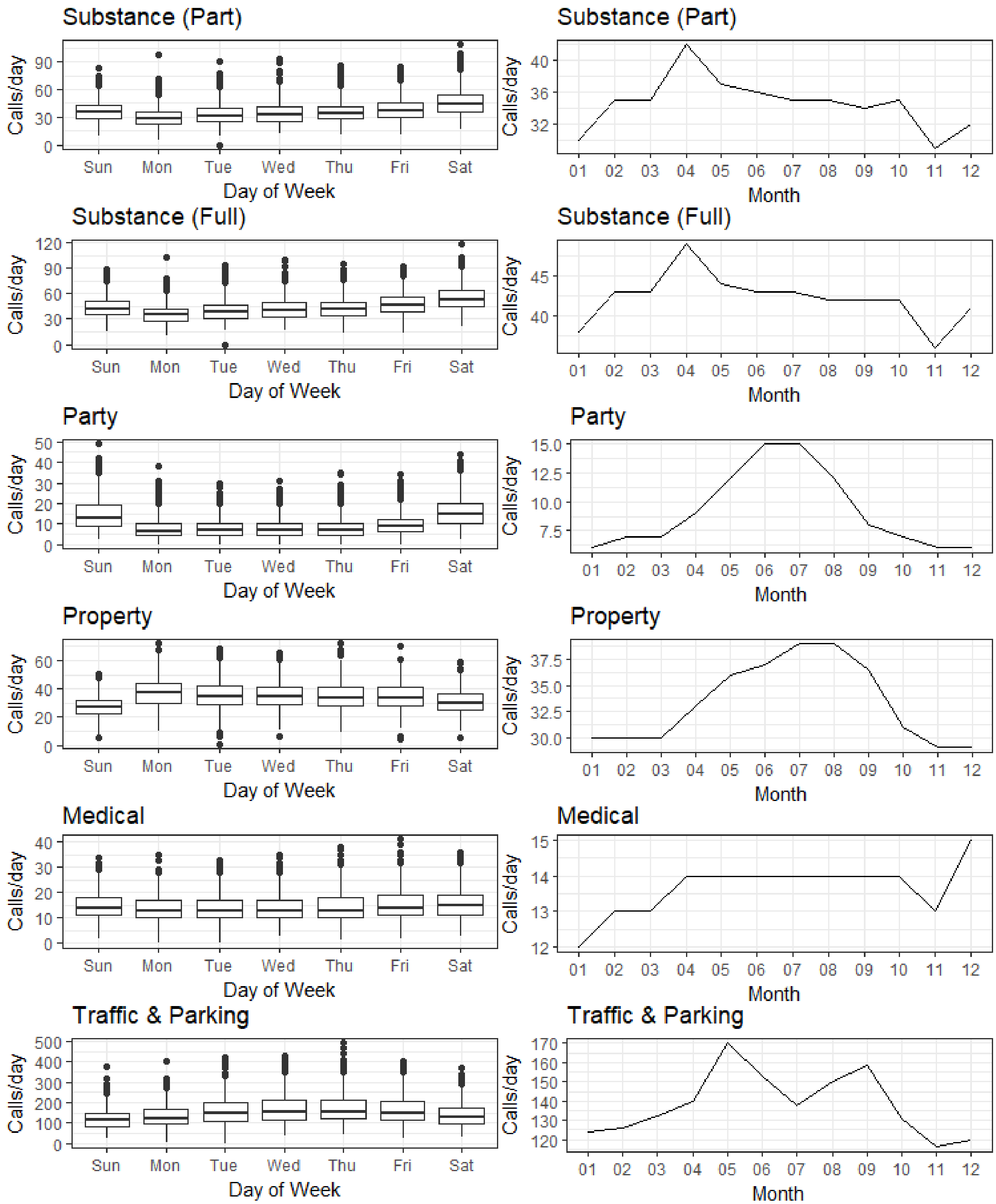
7 Additional Figures

Figure 3: Calls per day by day of week and month of year



8 Appendix Tables

Table 6: Anchorage Police Call Codes, Daily Average Count, and Assigned Category

Outcome Category	Call Code	Average Daily Calls
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 W 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
Party	Loud Disruptive Party	2.44
	Noise Violation	7.64
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
Medical	Medic Assist	14.38
Traffic & Parking	Parking Problem Over 24	21.93
	Traffic Stop	128.63

Table 7: Change in average daily calls, first full PFD day, 2000-2016

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

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