Universal Cash Transfers and Labor Market Outcomes*

Andrew Bibler
ajbibler@alaska.edu

Mouhcine Guettabi
mguettabi@alaska.edu

Matthew Reimer
mnreimer@ucdavis.edu

February 25, 2020

Abstract

One major criticism of universal basic income is that unconditional cash transfers discourage recipients from working. We estimate the causal effects of a universal cash transfer on short-run labor market activity by exploiting the timing and variation of a long-running unconditional and universal transfer: Alaska's Permanent Fund Dividend. We find evidence of both a positive labor demand and negative labor supply response to the transfers, document important heterogeneity across workers, and provide a set of placebo tests supporting our main results. Altogether, a $1,000 increase in the per-person disbursement leads to a 0.7% labor market contraction on an annual basis.

JEL Classification: J2, I38, H53

Keywords: Permanent Fund Dividend, labor supply, universal income.

*University of Alaska Anchorage, Institute of Social and Economic Research. We thank Keith Teltser and Gary Solon for helpful comments, and gratefully acknowledge support from The Economic Security Project. This paper was previously circulated with the title *Short-Term Labor Responses to Unconditional Cash Transfers*

1 Introduction

Universal Basic Income (UBI) constitutes an unconditional cash transfer that is provided universally to all residents on a long-term basis, regardless of income and with no "strings attached" (Marinescu, 2017). UBI has recently garnered considerable attention from policy makers, Silicon Valley entrepreneurs, and politicians alike.[1] Proponents favor UBI as a replacement for existing welfare programs and consider it a promising route to address inequality (Murray, 2008; Thigpen, 2016). Although interest is high, we still know little about the consequences and benefits of UBI as opportunities for empirical evaluation are rare.

One often-cited criticism of UBI is that the income effect of unconditional cash disbursements provides a strong work disincentive (Robins, 1985).[2] Because universal cash transfers are rare in practice, most prior research has focused on labor-market responses to targeted and/or conditional transfers, such as the Earned Income Tax Credit (EITC), Negative Income Tax Experiments (NIT), casino revenues, or lottery winnings, finding some evidence of a small work disincentive (e.g., Munnell, 1987; Maynard and Murnane, 1979; Cesarini et al., 2017; Price et al., 2016; Yang, 2018). Unlike means-based and conditional transfers, UBI is given to the population at large, which is critical for at least two reasons. First, if labor supply responses to transfers are heterogeneous, the average response of targeted populations covered in prior research may not accurately reflect the average response across the entire population. Second, the universal nature of UBI could lead to a positive demand shock for consumption goods and services, providing upward pressure on the demand for labor and offsetting any negative labor supply response. Because of these differences, conclusions from research regarding non-universal transfers do not necessarily apply to universal transfer

[1] For example, several countries, such as Finland and India, have recently implemented UBI experiments. Y Combinator Research, a nonprofit research lab in Silicon Valley, has recently undertaken a randomized control trial aimed at understanding the effectiveness of universal income on well-being including employment, social networks, and health. On the political front, the Democratic Party nominee for the 2016 US presidential election, Hillary Clinton, considered running on a platform whose central component was UBI. The program was intended to be named "Alaska for America" given the inspiration it drew from the Alaska Permanent Fund Dividend.

[2] https://www.dailysignal.com/2018/10/09/universal-basic-income-has-been-tried-before-it-didnt-work/

programs.

In this paper, we estimate the short-run labor-market effects of universal, unconditional, and anticipated cash transfers from a long-running cash distribution program: the Alaska Permanent Fund Dividend (PFD). The PFD is useful for making inference on potential labor-market effects of UBI because it is a universal cash-transfer program: almost all Alaska residents are eligible to receive the PFD, regardless of income. Furthermore, the annual distribution of the PFD is the single largest infusion of money into Alaska's $55 billion economy, and therefore, has the potential to generate a positive demand shock for consumption goods and services.[3] Indeed, Kueng (2018) finds that the average marginal propensity to consume nondurable goods out of the PFD following the distribution is 24 cents per dollar, and 73 cents per dollar for total expenditures.

We focus on the short-run labor-market effects of the PFD for two reasons. First, estimates of the long-term effects of the PFD also capture structural changes in the economy that resulted from the program—i.e., long-run general equilibrium effects (Jones and Marinescu, 2018). Focusing on short-term responses effectively controls for long-term structural changes in the economy, thereby capturing pure labor-market responses. Moreover, focusing on the short-run responses to universal income is relevant for testing for the presence of PFD-driven demand shocks in the months following the disbursements (Kueng, 2018) and testing the permanent income hypothesis (Yang, 2018). Second, identification of long-run labor-market effects of the PFD is complicated by several confounding factors. Specifically, the inaugural PFD disbursement in 1982 came just five years after the completion of the Trans-Alaska Pipeline, which had significant effects on the Alaskan labor market (Carrington, 1996). The new revenue source also led to the repeal of the Alaska state income tax in 1980, immediately before the first PFD disbursement.[4] These confounding factors make it difficult to draw conclusions about the causal impact of the PFD on the labor market from

[3] In 2018, for example, the 1.022 billion dollar PFD distribution was about 51% of the construction sectors GDP, or 44% of the retail sector's.

[4] Alaska instituted a state income tax in 1949. At the time of the repeal in 1980, the tax had a progressive structure with brackets ranging from 3% to 14.5% of personal income.

long-run trends.

We use data from the Current Population Survey (CPS) from 1994-2017 and exploit two exogenous sources of temporal variation to examine the PFD's effect on labor-market outcomes: the intra-annual g (Hsieh, 2003; Kueng, 2018), crime (Watson et al., 2019), and long-run employment (Jones and Marinescu, 2018). The PFD is funded through a wealth fund, not through taxes or reductions in public programs, so there is no offsetting effect on demand for goods and services through other indirect channels. Over our sample period, all PFD disbursements were made over a short period of time in the fall of the calendar year with an average per person PFD of $1,750.[5] Our identification strategy is based on the correlation between the size of the per-person PFD and corresponding changes in employment and hours worked around the PFD disbursement date.

We estimate that an additional $1,000 in the per-person PFD leads to an average *decrease* of 1.25 hours of work per week in the months following the PFD disbursement in the sample of employed women. However, we find no significant impact on the probability of employment for the population of women. Our estimates are inclusive of both labor supply and demand responses; thus, we interpret the negative intensive margin response with no corresponding extensive margin effects as evidence of a labor supply response to the cash distribution. In contrast, for the sample of men, we estimate that an additional $1,000 in the per-person PFD leads to a 1.7 percentage-point *increase* in the probability of employment in the months following the disbursement with no statistically significant change in hours worked. We consider this positive short-run effect on male employment to be the most direct evidence to date of a labor demand shock induced by universal cash disbursements.

Altogether, our results suggest that an increase in the PFD induces both a positive labor demand shock and a negative labor supply response. Combining the extensive and intensive margin estimates, we find that an additional $1,000 in the per-person PFD results in a 1.6% labor market contraction in the months following the disbursement, or a 0.7% contraction

[5] In 2016 dollars. This is the per-person transfer, so a family of four was eligible for $7,000, on average.

on an annual basis.

To verify that our estimated labor-market response is driven by the PFD, we conduct placebo tests based on a reference distribution of state-specific estimates for the rest of the U.S., which does not receive the PFD. The intuition for this comparison is that we do not expect the PFD to have a causal effect on the labor markets of other states; thus, if our estimated effects are indeed driven by the PFD, they should be extreme, relative to the reference distribution of non-Alaskan effects. The estimates for Alaska for both hours of work (women) and employment (men) are more extreme than any other state-specific estimate, which provides strong support that our estimates reflect the effect of the PFD transfer on the Alaskan labor market.

We evaluate the role of heterogeneity by comparing responses across several important dimensions. Our findings confirm the importance of heterogeneity as the negative intensive-margin response is largest among younger, lower-wage earning women with young children in the household. Our ability to test for heterogeneous responses across the entire income distribution highlights one benefit of evaluating a universal transfer such as the PFD and validates concerns about using targeted transfers, such as EITC, to inform UBI policy.

The remainder of this paper is organized as follows. Section 2 provides a brief discussion of the relevant literature, the history of the Alaska PFD, and why the PFD provides a useful setting for UBI research. Sections 3 and 4 describe our data and empirical strategy. We discuss results in Section 5. The implications of our findings and conclusions are discussed in the final two sections.

2 Background

2.1 Related Literature

Our research contributes to the broader literature on the behavioral effects of unconditional cash transfers, which is critical for designing UBI-related policy. Marinescu (2017) provides a thorough review of the labor-market effects of unconditional cash transfers, such as the NIT Experiments, the Eastern Cherokee Nations casino revenue dividend, lottery winnings, and the PFD. The NIT Experiments provided guaranteed income to primarily low-income households. In the most generous of the NIT Experiments,[6] Price et al. (2016) estimate a 3.3 percentage-point decrease in the probability of employment and a 7.4% earnings reduction. However, the NIT treatment was not permanent (recipients knew that the payments were temporary) and had a high implicit income tax rate, thereby inducing both an income and substitution effect. It is thus challenging to compare the NIT Experiments to an established unconditional cash transfer program such as the PFD, which is guaranteed and induces only an income effect.[7]

There is some evidence that individuals reduce labor supply and earnings in response to winning the lottery (Cesarini et al., 2017; Imbens et al., 2001; Sila and Sousa, 2014). However, the more modest transfers of casino revenues disbursed to the Eastern Band of Cherokee Indians (around $4,000 in two installments per year starting in 1997) were found to have no discernible impact on recipients' labor supply (Akee et al., 2010). While lottery winnings and casino revenues have the benefit of constituting a pure income effect, there are still important differences in the structure of the payments and the number of recipients that make it difficult to compare these cash transfers directly to the PFD. Importantly, in each of these settings, the distributions are to a relatively small proportion of the population, which means they are unlikely to induce the demand shock that we might expect from a universal transfer (Marinescu, 2017).

Two papers that are closely related to our study examine the long-run (Jones and Marinescu, 2018) and short-run (Feinberg and Kuehn, 2018) labor-market responses to the Alaska PFD disbursements. Jones and Marinescu (2018) estimate the long-run labor market impacts of the PFD using a synthetic-control design, which uses a weighted average of control

[6]The Seattle/Denver Income Maintenance Experiment

[7]Additionally, under-reporting of earnings partially explains the employment reduction from the NIT Experiments (Burtless, 1986).

states to estimate the counterfactual labor-market outcomes in Alaska in the absence of the PFD program. The study finds no evidence of a PFD effect on employment, but finds a small increase in the share of workers in part-time jobs, which is interpreted as evidence of a reduction in labor supply on the intensive margin. Jones and Marinescu (2018) also provide evidence of a decline in employment in tradable sectors with no corresponding effect in non-tradable sectors, which they interpret as evidence of a labor-demand shock for non-tradables. Jones and Marinescu (2018) thus provide some evidence of a long-run general equilibrium effect from the PFD; however, as discussed previously, long-run estimates of the PFD should be considered cautiously given the existence of confounding factors that had considerable effects on the labor market in Alaska around the time that the PFD was instituted (Carrington, 1996).

Feinberg and Kuehn (2018) estimate the short-run effects of the PFD on the labor market using inter-annual variation in the size of the PFD and annual data based on usual hours and weeks of work from the American Community Survey (ACS). The study finds evidence of significant negative labor-market elasticities with respect to the PFD: -0.10, -0.11, and -0.11 for men, single women, and married women, respectively. Our empirical design differs considerably from the short-run analysis by Feinberg and Kuehn (2018). We use the intra-annual timing of the PFD disbursement to focus on changes in the labor market in a short window around the time of the disbursement, whereas Feinberg and Kuehn (2018) use annual data from the ACS, for which the reported hours of work do not align with the PFD disbursement dates. Further, Feinberg and Kuehn (2018) construct family-specific PFD disbursements to use as the right-hand-side variable of interest. However, information on individual eligibility and disbursements cannot be elicited from survey data used by Feinberg and Kuehn (2018), leading to error in the right-hand-side variable of interest. Finally, the inclusion of year fixed effects in their model specification, which are collinear with the individual size of PFD payments, means that their estimated labor-market responses are driven by differences in family-size, which is likely endogenous. Because neither ACS or CPS data are well-suited to accurately construct the size of household PFD disbursements, we focus on year-to-year changes in the size of the per-person PFD. While our strategy does not separate the demand and supply side responses, neither the ACS or CPS data are suited to isolate the supply-side income effects.

Our paper also contributes to the literature on behavioral responses to other types of transfers (e.g., means tested, Moffitt, 2016). In a recent analysis of the short-term labor supply responses to EITC payments, Yang (2018) estimates that for an additional $1,000 received in EITC, married women reduce their proportion of weeks worked by 2.7% in the month of the transfer. In contrast, our estimated reduction in hours worked for the sample of women equates to a 1.2% decrease for an additional $1,000 transfer to the household. While the focus on the immediate response to the EITC makes the setting in Yang (2018) more comparable to our analysis of the PFD, direct comparison of our estimates is difficult because EITC payments are targeted toward low-income households, do not generally induce pure income effects, and are substantially smaller than PFD transfers.[8,9]

2.2 The Alaska Permanent Fund Dividend

The annual PFD is paid to Alaska residents from the investment earnings of the state's sovereign wealth fund, the Alaska Permanent Fund. The Fund was established via a constitutional amendment in 1976 to save and invest a portion of the annual mineral royalties with the purpose of diversifying Alaska's revenue stream, preserving mineral wealth for future residents, and ensuring that royalties were not spent haphazardly by politicians. The Fund's value currently stands at over 63 billion dollars. Distributing dividends from the fund was not part of the initial plan but changed with Governor Hammond's desire to ensure the

[8]The average EITC transfer for the married sample in Yang (2018) was about $2,836, whereas a family of the same size was eligible for a PFD of $7,266 on average over our sample period based on an average number of eligible children of 2.2. Both numbers are in 2016 dollars. The average predicted EITC of $2,450 in Yang (2018) is measured in 2007 dollars.

[9]Comparing our estimates to Yang (2018) is also complicated by differences in outcomes measures. Yang (2018) focuses on the proportion of weeks worked in a given month, which only captures reductions that decrease hours in a given week to zero.

sustainability of the Fund by involving the public. The first payout of $1,000 was made in 1982.

PFD payments were initially paid out of the general fund in the first year of the program; however, payments have since been 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. The fund is currently well diversified with 26 of the 63 billion dollars in stocks, and the rest in bonds, real-estate, private equity, and other asset classes. While the fund was originally capitalized by state oil revenue, investment returns are the main growth mechanism. Specifically, Watson et al. (2019) note that since 1985, only 2-3% of the annual growth comes from state oil revenues, whereas the rest of the growth is from reinvested earnings. Given this investment profile, returns are not reflective of Alaska's economic conditions, which are heavily tied to oil prices and production. 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.

The dividend established an income floor for the state's residents. This cash transfer is particularly important in rural areas where economies lack economic bases and are still a mixture of subsistence and a small formal economy. Alaskans have received the yearly dividend since 1982, with the amount varying on an annual basis 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 payments 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 nearly universal. The dividend represents a non-negligible portion of Alaskans' earnings. The 1982 dividend distribution of $450 million amounted to 6.3 percent of personal income in Alaska, the same amount as the payroll of the petroleum industry for that year. The average annual aggregate distribution is similar in size to the payroll of many sectors in the Alaska economy. In 2017, for example, the 651 million dollar distribution was almost exactly the same as the manufacturing sector's payroll, or 57% of the construction sector's. The PFD also has the unique distinction of being distributed over a short period of time, resulting in it being the most significant concentrated cash distribution.

It is important to note that the decision to distribute payments in October is a result of administrative processes, as opposed to any intention on behalf of the founders of the dividend. Most Alaskans—84.17% in 2017—receive their PFDs through direct deposit in the first week of October, while the rest received mailed checks. Over our study period (1994-2017), direct deposits have always been issued either before or on the same day that the first checks are mailed.

3 Data

We use the Current Population Survey (CPS) basic monthly survey (Flood et al., 2018) supplemented with information on the annual PFD size and disbursement date to estimate the short-run impact of disbursement on the labor market. The CPS is well suited to measure short-run fluctuations in the labor market since the survey is given each month to a large number of respondents, and finding an adequate sample size for the Alaskan labor market is challenging. We focus on two measures of the labor market: the number of hours worked in the reference week and a dummy variable indicating whether the respondent was employed in the reference week.[10] We use these two measures to estimate responses along the intensive and extensive margins, respectively. To focus on working-age individuals who are likely to receive the PFD, we restrict our sample to respondents age 20 to 55 who are either the head of the household or the spouse to the head of the household. We exclude cohabiting couples in our final sample. Finally, we drop those who are not US citizens.[11]

[10] Hours are top-coded at 80 hours per week.

[11] Citizenship is not required to receive the PFD, but a smaller proportion of non-citizens will be eligible.

4 Empirical Strategy

Our empirical strategy exploits two sources of temporal variation in the PFD. First, we use the discrete intra-annual variation created by the timing of the PFD distribution by comparing labor-market outcomes from the months immediately following the PFD disbursement to the months prior to the PFD disbursement. Previous work has demonstrated that there are significant behavioral responses in consumption (Kueng, 2018) and crime (Watson et al., 2019) immediately following the PFD disbursement. The time of year in which the PFD is issued is a useful source of variation because it is determined only by administrative processes. Unfortunately, despite this useful feature, we cannot rely solely on the timing of the PFD to identify the PFD's effect on labor-market outcomes because of seasonal trends in the Alaskan labor market.

One potential solution is to use the labor markets of other U.S. states—which do not receive the PFD—as an estimate of the counterfactual of the Alaska labor-market in the absence of the PFD. However, as we demonstrate below, other states are not adequate controls for Alaska because they exhibit considerably different seasonal trends from Alaska. Instead, we exploit a second source of temporal variation in the PFD: inter-annual variation in the size of the PFD payment. As we demonstrate below, Alaska labor-market trends are very similar in low- and high-PFD years during the pre-disbursement months, suggesting that deviations in labor-market trends in the post-disbursement months arise solely from differences in the size of the PFD.

Our empirical strategy leads to a DiD-type estimator, using differences around the timing of the disbursements and heterogeneity in the size of the disbursements, for identification. Finally, we supplement our within-state analysis with a placebo test that leverages the fact that labor markets in other states should be unaffected by the PFD, and thus, serve as a useful reference distribution under the null hypothesis that the PFD has no effect on the Alaskan labor market.

4.1 Labor market seasonality

Figure 1 displays average hours worked by month in Alaska and the rest of the United States. Seasonality in the Alaskan labor market differs from the average state: the average number of hours worked increases considerably during the summer months in Alaska, particularly for men, whereas hours worked during the summer months noticeably decreases across the rest of the U.S. In addition, the average hours worked is lower in Alaska than in the average state in both the male and female subsamples. While the contrast is less stark in the sample of women, there is a noticeable dip in the average hours worked in Alaska, relative to the national average, in the beginning and end of the calendar year. Panel (C) of Figure 1 displays the average seasonality in employment in the Alaskan labor market as percentage growth from January of the same year, relative to the average state (BLS, 2018). In an average year, the Alaskan labor market is roughly 15% larger in July and August, relative to January. The average among the rest of the states, in contrast, is less than 5%. While this figure masks some state-to-state heterogeneity, there is no other state with similarly drastic seasonal fluctuations in labor market size as Alaska.

Because of the drastic differences in seasonality between Alaska and the rest of the country, other states are not suitable controls for a within-year difference-in-differences (DiD) estimation strategy around the PFD disbursement. We would risk attributing differences in seasonality to the PFD disbursement.

4.2 Inter-annual variation in the size of the PFD

Instead of using other U.S states as a control for Alaska, we focus on year-to-year variation in the Alaska labor market and its association with the size of the PFD. To demonstrate, Figure 2 separates the seasonal patterns in Alaska and other states by high and low PFD years.[12] The Alaska labor market is slightly larger in high PFD years in every month, but the

[12] Table A1 displays annual statistics related to the PFD. High PFD years are defined as years with a per-person PFD over $1,700 and low PFD years had a per person PFD below $1,600

difference is essentially a parallel shift. The size of labor markets in other states appears unrelated, on average, by whether it is a high or low PFD year, although there exist differences that are masked by state-to-state heterogeneity. Figure 3 graphs the difference in average hours between high and low PFD years for Alaska and three potential control groups: all other states, states with the most comparable seasonal employment patterns to Alaska, and energy states.[13] The relatively flat line around zero among the three potential control groups in the sample of men (Panel A) and women (Panel B) suggests there is little difference in hours worked per week between high and low PFD years. In both cases, the patterns among the potential control groups highlight an important point: after leveraging heterogeneity in the PFD size across years in the Alaskan market, differencing out the analogous changes in the corresponding control group does not add any useful variation—i.e., using other states as control groups essentially leaves our estimates unaffected. Instead, we use other states to produce two placebo tests, which we discuss in greater detail below, that provide further evidence that our estimates reflect the effect of the transfer on the Alaskan labor market.

Focusing on the differences in hours in the sample of Alaskan men in high and low PFD years (Figure 3), the lines evolve similarly through the first four months of the year. There is a slight positive change in May through July, followed by a steep decline in August to December in the high PFD years, relative to low PFD years. In fact, the steep decline in the Alaskan labor market in high PFD years in the second half of the year can be seen in both panels, which means that there is an unconditional decline in hours worked among the sample in high PFD years, relative to low PFD years. Among the sample of women, the decline coincides almost perfectly with the usual first disbursement of the PFD in October, which suggests that we would estimate a decline in hours worked among that sample if using an unconditional DiD estimation strategy. While there is also a decline in the sample of men, the decline starts prior to the disbursement and there is only a sharp decline in November

[13] The most seasonally comparable states (MT, WY, SD, and ME) were chosen in an ad hoc manner based on average seasonal fluctuations and in which months each state experiences high and low periods. Energy states are based on Snead (2009).

- December. It is thus less obvious that there is a strong unconditional correlation between the PFD size and hours worked in this sample.

The similarity between high and low PFD years in the pre-disbursement months is further demonstrated in Table A2, which presents sample averages for men and women for labor-market, demographic, and economic variables. In general, the samples from high- and low-PFD years are comparable, with two exceptions: both the average Alaska unemployment rate and the crude oil price are slightly higher in low-PFD years. As previously discussed, the size of the PFD in any given year is reflective of national, rather than state, trends, because the fund is invested in a diverse set of assets with very little connection to the Alaska economy. We condition on the monthly unemployment rate and crude oil prices in all specifications discussed below. We discuss testing pre-trends of labor market outcomes in the next section.

4.3 Estimation

We first estimate month-specific impacts of a $1,000 increase in the per-person PFD payment on labor market outcomes based on Equation 1, which includes interaction terms between month-specific dummy variables and the per-person PFD disbursement in a given year:

$$L_{imy} = \alpha + \sum_{m} \beta_m \cdot PFD_y \cdot M_m + \Gamma \cdot X_{imy} + Y_y + M_m + \epsilon_{imy}, \tag{1}$$

where L_{imy} is the outcome of interest (i.e., the number of hours worked or a dummy variable for employment) for individual i in month m and year y. PFD_y is the size of the per-person PFD in thousands of dollars. Y_y and M_m represent year and month dummy variables, respectively. In this specification, the year subscript y refers to a twelve-month period from April to March of the following year, rather than a calendar year. Similarly, in January through March, PFD_y denotes the PFD from the fall of the prior calendar year. By shifting the window in this way, we are estimating the influence of the PFD on the labor market from the time of the disbursement - usually in October - all the way to the following March. We do this to check the persistence of the labor market responses to the PFD disbursement.

The coefficients of interest are the $\hat{\beta}_m$, which represent the month-specific impacts of a $1,000 increase in the size of the per-person PFD, after conditioning on our full set of controls, X_{imy}, Y_y, and M_m.[14] The identifying variation is based on the association between the within-year variation in labor-market outcomes around the PFD disbursement and the size of the per-person PFD in that year.

Equation 1 amounts to an event-study analysis, comparing month-specific relationships with the PFD size and hours or employment around the disbursement of the PFD. In each regression, we omit the interaction with the August dummy variable, so the differences are relative to August.[15] Estimates in the months before the PFD disbursement act as a test for pre-trends in the outcomes of interest: if the estimated effects are near zero in the pre-disbursement period, it suggests that the labor market outcomes in the months leading up to the disbursement are uncorrelated with the size of the upcoming PFD that has not yet been disbursed. Estimates in the months following the disbursement represent responses to a $1,000 increase in the size of the per-person PFD. By extending the post-disbursement window to track the potential responses through March of the following calendar year, we demonstrate both the short-term response to the disbursements and the fade-out of the effects over time.

To estimate the average effect over the months following the disbursements, we focus on estimates from the specification in Equation 2:

[14] X_{imy} includes a marriage indicator, age and age-squared, dummy variables for the number of children 5 years or younger in the household, dummy variables for the number of children in the household, income category indicators ($25-50k, $50-75k, $75-150, over $150k), dummies for being top coded at $75k and $150k, a dummy for missing values, a dummy for living in a metropolitan area, dummies for educational attainment (high school, some college, a college degree, or an advanced degree), race and ethnicity, the state unemployment rate by month, and crude oil price by month. Broad industry and occupation dummy variables are also included in the hours of work regressions.

[15] In most years, the initial PFD disbursement takes place in early October, but the earliest disbursement in our sample is in September.

$$L_{imy} = \alpha + \beta \cdot P_{imy} \cdot PFD_y + \gamma \cdot P_{imy} + \Gamma \cdot X_{imy} + Y_y + M_m + \epsilon_{imy}, \tag{2}$$

where P_{imy} is a dummy variable indicating observations in the months following the disbursement in a given twelve month period, and all other variables have the same definition from Equation 1. In this case, $\hat{\beta}$ represents the average impact of a \$1,000 increase in the size of the per-person PFD across the post-disbursement months. We use Equation 2 to summarize the average relationship of the PFD on labor-market outcomes and test the null hypothesis that the PFD distribution has no influence on labor-market outcomes. We determine the length of the post-disbursement period based on the evidence on fade-out that we estimate using Equation 1, and we allow this to differ for men and women.

In every case, we use per-person PFD, PFD_y, to estimate labor-market impacts. We do this for a couple of reasons. First, we do not have credible information on the actual size of the PFD that each respondent received. Although roughly 90% of the state population receives a PFD, some respondents may not be eligible. Second, specific PFD amounts depend on the dates that residents moved to the state and/or on birth dates for individuals less than one year old, further complicating our ability to accurately measure the household PFD. While using the per-person measurement may slightly change the interpretation of our estimates, it does not invalidate our estimates. In fact, we believe this is more credible than some other measures. For example, using mis-measured family size and/or income measures (to use a PFD-to-income ratio) introduces error in our variable of interest that we are able to avoid in our preferred specification. Income is not generally measured with accuracy in the CPS, and because we make use of the basic monthly survey, we have even less precise measures of income. Nonetheless, annual variation in per-person PFD size is still useful for identifying the impact of the PFD on the labor market.

We include two placebo tests to provide further evidence that our estimates reflect the effect of the transfer on the Alaskan labor market. First, we estimate state-specific effects for all other states based on Equation 2, and compare the density of non-Alaska treatment effects with our main estimates. If our findings are driven by the PFD disbursements, which are not made in any other state, then we should not expect the PFD to have any effect on the labor markets of other states. Placing our estimates in the density of placebo treatment effects essentially tells us how likely we would be to recover similarly sized estimates under the null hypothesis that the PFD has no effect on the Alaskan labor market. We extend this concept to compare month-specific estimates for the impact of an additional $1,000 in the per-person PFD on the labor market in Alaska with month-specific placebo-effect distributions. To do this, we estimate the analogous $\hat{\beta}_m$ for every state based on Equation 1 and compare the month-specific point estimates for Alaska with the distributions of month-specific estimates for all other states.

Additionally, we evaluate heterogeneous responses to the PFD, which highlight the subgroups that are most responsive to the PFD. We estimate heterogeneous responses across several important dimensions, including marital status, age, whether or not the respondent has any children or any children age 5 or younger in the household, and full- and part-time worker status. We do this by splitting the sample and re-estimating the impact of the disbursement in each subsample.[16] Our heterogeneity analysis helps us reconcile any differences between our estimates and those from previous literature, and can guide future implementations of universal income trials.

Finally, we estimate heterogeneous responses in hours of work by wage terciles. Because we do not have valid wages for all respondents, we use regression-based imputation to impute wages for a portion of the sample based on the sample with valid wages (similar to Blau and Kahn, 2007). Using the sample of observations that report valid wages, i.e. the non-self-employed outgoing rotation group, we estimate wage regressions for male and female respondents separately.[17] We use the estimated coefficients from the wage regressions to

[16]When estimating heterogeneous responses in the probability of employment by FT/PT, we focus on relative shifts in employment. To do this, we use dummy variables that indicate employment in FT or PT work and restrict to the sample of employed workers.

[17]Controlling for a quadratic in age by month of year, quadratic in age by year, educational attainment dummies by month and year, a part-time worker dummy by month, industry by occupation dummies, and industry and occupation dummies by month and year.

predict wages for the rest of our sample, and split the samples into terciles based on imputed or real wages.[18] It is important to note that error in the imputed wages is less problematic, because we are not relying on precise wage measures. We are much less likely to mis-classify a worker into the wrong tercile, than we are to mis-measure the wage.

5 Results

We first present the main results from estimating the month-specific and average effects of the increase in the per person PFD on the probability of employment and hours of work. We then present the placebo tests based on comparing our main estimates with the reference distribution of placebo effects from untreated states. Next, we consider potential differences in part- and full-time work and analyze heterogeneous effects across several important characteristics. Section 5.2 shows estimated impacts by marital status, age, the presence of children in the household, and the presence of children under the age of five in the household. Because of the contrast in the main estimates presented from the sample of men and women, we again discuss results for the two samples separately. In section 5.2.3, we show estimated effects of the PFD on hours worked by wage terciles. Finally, in section 5.3, we combine the intensive- and extensive-margin effects to produce an estimated effect of the PFD on the total amount of labor.

5.1 Main Results

Figure 4 is a graphic depiction of the estimates obtained from Equation 1, providing a monthly comparison of the impact of an additional $1,000 in the per-person PFD, relative to the difference in August. Each panel displays the estimated effects for a different group and outcome combination. For example, Panel (A) displays the estimated effects of an additional $1,000 in the per-person PFD on the hours worked in the sample of women. The

[18] In the cases with valid wages, we use the reported wage.

vertical bars represent 95% confidence intervals based on standard errors clustered at the household level. In all four panels, the pre-PFD estimates are mostly near zero, suggesting that the size of the PFD is unrelated to the employment outcomes in the months before the disbursement. In fact, every 95% confidence interval over the pre-disbursement period in Figure 4 includes zero.

Panel (A) of Figure 4 highlights a noticeable dip in the post-PFD period in the sample of women, suggesting that a larger PFD disbursement decreases hours of work in the months following the disbursement. The decline starts soon after the disbursement and persists until February of the following year. Panel (B) displays the results from the analogous exercise using an employment indicator as the outcome variable. As such, Panel (B) displays the average conditional monthly difference in the proportion of the population of women that are employed for a $1,000 increase in the per-person PFD. In contrast to Panel (A), there is no noticeable change in the probability of employment around the disbursement among women, suggesting that a larger PFD does not impact the probability of working in this sample.

The analogous estimates for the sample of men are displayed in Panels (C) and (D) of Figure 4. Panel (C) shows the month-specific effects of a $1,000 increase in the per-person PFD on the hours of work among men. Hours of work among men is seemingly unrelated to the size of the PFD, as every estimate—including estimates for post-disbursement months—is near zero. From Panel (D), we find that the relative probability of employment in the sample of men is unrelated to the size of the PFD in April through August; however, there is a visible incline in employment following the disbursement with statistically significant increases in November and December, which suggests that the proportion of the population that reports being employed in post-PFD months is increasing in the size of the PFD payment. Since the differences represented in the figure are inclusive of both labor supply and demand responses, a positive impact suggests that the PFD induced a labor-demand shock that is large enough to outweigh any supply response in this sample along the extensive margin.

Next, we report estimates of β from Equation 2 in Table 1. Columns (1) - (2) report the estimated average impact of an additional $1,000 in the size of the per-person PFD on the probability of employment during the post-disbursement months.[19] In column 1, we see that an increase of $1,000 in the per-person PFD increases the probability of employment in the male subsample by 1.7 percentage points, which is a two-percent increase over the baseline employment for men of 87% (Table A2). The increase in male employment is consistent with a demand shock stemming from the PFD and suggests that the positive demand shock outweighs any negative supply response to receiving the PFD. This seems plausible, given the low supply response of male workers to income and wages found in previous literature.[20] In contrast, we find no significant impact of the disbursement on the probability of employment among the sample of women (column 2).

Estimates of the intensive-margin responses to the PFD are provided in columns (3) - (4) of Table 1, which report the impact of an additional $1,000 in the per-person PFD on the number of hours worked per week (conditional on being employed). For the sample of men (column 3), an additional $1,000 leads to a reduction of 0.27 hours per week; however, this estimate is statistically insignificant at the 10% level. For the sample of women (column 4), an additional $1,000 in the per-person PFD leads to a decrease of about 1.26 hours per week, which is statistically significant at the 1% level. Given the average of 24.6 hours per week in this sample and the average per-person PFD of $1,750 (2016 dollars), the estimate amounts to a reduction of over five percent in hours worked and an elasticity of -0.09, which is reasonable when compared with labor supply responses to EITC payments (Yang, 2018).[21]

[19] From Figure 4, the response for men fades out by January; thus, the post-disbursement window includes the post-disbursement months up to (and including) December. For women, responses persist through February of the following calendar year, so the post-disbursement window includes the post-disbursement months up to (and including) February.

[20] For example, see Blundell and MaCurdy (1999) for a review of labor supply estimates, Nichols and Rothstein (2015) and Yang (2018) for evidence on differential responses to the EITC, and Robins (1985) for evidence related to the NIT experiments.

[21] The elasticity should be considered in context of our estimation strategy. By using all post-PFD months as a single treatment period, we are implicitly allowing the response to persist through February. Thus, this elasticity captures an average response that persists for about five months (the first disbursement is in October in all but one year). In addition, the estimated elasticity in (Yang, 2018) combines the intensive and extensive margin, which we explore in more depth in section 5.3.

Figure A1 displays the placebo effect densities generated from estimating the effect of an additional $1,000 in the per-person PFD on hours and employment in all untreated states. Comparing our main estimates with the placebo densities provides strong evidence that the estimated effects on the Alaskan labor market are actually driven by the PFD. For example, our main estimate that an additional $1,000 in the per-person PFD reduces hours among women by -1.26 hours per week is supported by the fact that there is no other state for which we could replicate an estimate of this size. In addition, the density of placebo effects is centered around zero, which confirms that including other states as control units would have little impact on the main estimates. In Panel (D) we show that the estimated effect of the PFD on employment among men in Alaska is also the highest of any state. In the other two cases, employment among women and hours among men, the estimates are well within the 5th and 95th percentiles of the placebo distributions. As with the previous two cases, both placebo-effect densities are centered around zero.

We present the month-specific effects of the PFD relative to the placebo densities in Figure 5, which provide further evidence that our main results are truly a reflection of the PFD disbursements. In Panel (A) of Figure 5, the only month-specific effects on hours worked among women that fall outside of the 5th - 95th percentile range of placebo estimates are for those months that occur after (or during) the first PFD distribution in every year of our sample (October through February). Similarly for male employment, the estimated effects in November and December are well outside of the 5th - 95th percentile range of the placebo distribution. On the other hand, the month-specific estimates for employment among women and hours worked among male are all between the 5th and 95th percentile of the corresponding reference distributions. These patterns provide convincing evidence that support our main results, as they confirm that the timing of the responses correspond with the timing of the treatment and show that we could not replicate our findings using labor market activity from any other state.

Table 2 presents estimates of differential responses in employment and hours worked by full- and part-time work status. When estimating heterogeneous responses in the probability of employment, we restrict the sample to employed respondents and use dummy variables for full-time or part-time employment as the outcome variable. This is helpful for interpretation, as the estimates in Panel (A) of Table 2 can be interpreted as relative movement toward or away from a type of work. For example, the estimated change in the probability of employment for full-time male workers is a decline of 0.6 percentage points, and the estimated change for part-time work is a 0.6 percentage point increase. We interpret this as a relative shift toward part-time work. However, it does not indicate that full-time employment declined in absolute terms, as overall employment increased among this group. Neither estimate is significantly different from zero. Similarly, neither of the estimated changes in hours worked are statistically different from zero in the sample of men.

In contrast, there is a stronger shift toward part-time work among the sample of women. We estimate a relative shift toward part-time work of 0.021, which means that there is a 2.1 percentage point increase in the proportion of all workers in part-time jobs (column 4 Panel A), which is statistically significant at the five percent level. We also find some intensive-margin differences in the sample of women. We estimate a strong decline in the hours worked among full-time workers of -1.084 hours per week, which is statistically significant at the one-percent level. On the other hand, we find no statistically significant change in the hours of part-time workers.

5.2 Heterogeneity by Age and Children

5.2.1 Men

Panel (A) of Table 3 shows that the magnitude of the increase in the probability of employment is larger among single men and men without children in the household. We estimate that an additional $1,000 in the size of the per-person PFD increases the probability of employment by 2.5 percentage points among single men, but the estimate is statistically insignificant. Among men with no children in the household, the probability of employment increases by 2.7 percentage points, which is a statistically significant increase at the five percent level. We find little evidence of heterogeneous effects in the probability of employment by age. The estimated effects on the intensive margin, hours worked (Panel B), are all negative, with the exception of the sample of men with children under 5 in the household. However, we find no statistically significant effect on hours worked in any subsample, suggesting that there is no sample in which the disbursement leads to a reduction in the total amount of labor. Instead, particularly in samples with a strong increase in the employment probability, there is an apparent increase in the total amount of labor.

5.2.2 Women

In contrast to men, we find no statistically significant increases in the probability of employment in any of the subsamples of women (Panel A of Table 4) However, we estimate a statistically significant decrease in the probability of employment among women with children under age 5 in the household. We also estimate a relatively large decrease of 1.9 percentage points in the employment probability of women under age 30. However, the estimate is not statistically different from zero.

On the other hand, there is an across-the-board reduction in hours worked, as the estimate for each subsample is statistically different from zero at conventional levels. We find little evidence of differences in the response of women by marital status.[22] In contrast, the sample of women under 30 are much more responsive than women age 31 - 55. We also find that women with children in the household respond more than women without a child in the household, with estimated decreases of 1.5 and 1 hours, respectively. The contrast between women with and without a child age five or younger in the household is even more stark, as women with a young children in the home decrease hours of work by more than 2.1 hours per week in the months following the disbursement. The strong response of women with

[22] The decrease in hours among single and married women, respectively, amounts to a 3 and 4 percent reduction in hours worked, relative baseline average of 37 and 33 hours per week. The approximate elasticities are therefore -0.05 and -0.09.

young children in the household highlights one potential benefit of the disbursement that may not be captured by considering labor market responses in isolation. If this time is reallocated toward children in the household, it could lead to long-run benefits on the child's development of cognitive and non-cognitive skills (Bettinger et al., 2014; Cunha et al., 2006; Coneus et al., 2012).

5.2.3 Heterogeneity in Hours by Wage Level

Figure 6 displays the estimated effects of an additional $1,000 dollars in the per-person PFD on hours of work for six different subsamples: male and female samples by low, medium, and high wage levels. Each point represents the estimated effect for a different subsample based on our main specification. The strongest negative response is from the sample of women in the lowest wage tercile, for whom we estimate a reduction of 2 hours per week, which is significant at the one-percent level. This might not be surprising since the PFD represents a larger portion of total income for this group. However, we do not observe this response among the sample of men in the lowest wage tercile. Neither women or men in the middle tercile appear responsive to the increased disbursement. However, we estimate a decrease of 1.3 hours per week (significant at the five-percent level) among women in the high wage tercile, which is consistent with the responsiveness of consumption to the PFD among high-income individuals, as found by Kueng (2018). We do not find a statistically significant response in any of the male subsamples.

5.3 Combined Intensive and Extensive Margin Effects

To help evaluate the economic significance of the estimated responses to PFD disbursements, we compare the potentially offsetting effects of the disbursement on the intensive and extensive margins. For example, our analysis finds that an increase in the per-person PFD leads to an increase in the probability of being employed for men, but a slight statistically insignificant decrease in the hours of work for those employed. In this case, there are counteracting influences on the aggregate amount of labor. To interpret the relative importance of intensive and extensive margin effects and evaluate the overall impact of the PFD on the size of the market, we write the average hours of work across the population in terms of the probability of employment and the conditional average hours of work. Taking the total derivative of hours worked with respect to the PFD, we obtain:

$$\frac{dAvg(Hrs)}{dPFD} = \frac{dPr(Empl.)}{dPFD} \times Avg(Hrs|Empl.) + \frac{dAvg(Hrs|Empl.)}{dPFD} \times Pr(Empl.)$$

$$= \beta_e \times Avg(Hrs|Empl.) + \beta_h \times Prob(Empl.).$$

This expression allows us to measure the total change in hours worked as a function of our estimated effects on employment and hours, $\hat{\beta}_e$ and $\hat{\beta}_h$, while decomposing the potentially conflicting forces on the two margins. The decrease in conditional hours worked suggests that the average worker reduces labor by 0.28 hours per week, or $\hat{\beta}_h \times Prob(Empl.) = (-0.28) \times (0.85) = -0.24$ on the average male. On the other hand, the positive employment effect can be calculated as $\hat{\beta}_e \times Avg(Hours|Employed) = (0.0165) \times (41.44) = 0.68$. This suggests that the average overall impact on labor among males in this sample is actually positive: $-0.24 + 0.68 = 0.44$ (p-value= 0.27).[23]

For the sample of women, we know the overall impact is negative since we find both a decrease in the average number of hours worked per week and a statistically insignificant decrease in the probability of employment in the months after the PFD is distributed. The analogous calculations suggests that the average total effect for the sample of women is $(-1.25) \times (0.73) + (-0.006) \times (34.67) = -0.91 + (-0.21) = -1.1$ hours per week (p-value= 0.005).

Combining these estimates allows us to comment on the overall labor-market effects of

[23]Standard errors were obtained from 1000 bootstrap replications with sampling at the household level. P-value is based on normal distribution. See Table A4.

the PFD and should serve as a baseline for future implementations of basic income. The calculations above indicate that the decline for the average women in our sample is greater in absolute value than the average increase among males in our sample, and persists for a longer period after the disbursement. The aggregate impact on labor depends on the relative number of males and females in the population, and the number of months that the effects persists. Applying our estimated average unconditional changes to a population with the proportion of men and women that we observe in our sample, about 53% female, we observe an unconditional average reduction of 0.4 hours worked per week in the three months after the disbursement, followed by a 0.6 ($= 0.53 \times -1.13$) hour reduction for another two months On average, this reduction amounts to a 1.6% contraction of the labor market based on the sample average of 30 hours per week in post-disbursement months, which persists for about five months following the disbursement.

6 Concluding Remarks

This paper contributes to our understanding of the short-term labor market responses to universal cash transfers. Using the timing of disbursements and annual fluctuations in disbursement size of an unconditional and nearly universal lump-sum payment, Alaska's Permanent Fund Dividend, we find evidence of both a positive labor demand response and a negative labor supply response to universal cash transfers in the short-run. We estimate that a $1,000 increase in the size of the per person PFD increases the probability of employment among men by 1.7 percent over the months following the disbursement, which we interpret as direct empirical evidence that universal transfers can induce demand shocks that increase the demand for labor. This is critical for designing UBI-related policy, because it suggests that the universal nature of UBI leads to positive demand shocks that partially offset any negative impact on labor supply.

On the other hand, we estimate that a $1,000 increase in the size of the per person

PFD leads to a reduction of 1.25 hours per week (a four-percent decrease) among employed women in the months following the disbursement, with no corresponding extensive-margin response. However, we find that decreases in hours of work among women are concentrated among those who are younger, lower wage earners, and those with young children in the household. This heterogeneity is consistent with the idea that average labor supply responses to universal transfers are likely to be smaller than responses to targeted transfers, and could help reconcile modest differences between our results and other cash transfers such as EITC. Because of both the heterogeneity in the intensive margin responses among women as well as the positive demand shock induced by the universal nature of the disbursement, conclusions from research on non-universal transfers do not necessarily provide insights into potential labor-market effects from universal transfer programs.

Altogether, our estimates suggest that a $1,000 increase in the size of the per person PFD induces a contraction in the amount of labor that is 1.6% of the size of the labor market in the months that follow, which is driven by transitory reductions in hours rather than labor force exits.

While we find our estimated effects on aggregate labor outcomes to be useful for evaluating the overall effects of the PFD, there are several caveats to consider. First, our estimates are specific to the aggregate hours of work, and there may be variation in the aggregate hours across industry or wage levels. Second, the overall impacts calculated here are taken from a particular sample and should not necessarily be applied to the rest of the population. Similarly, the size of the PFD is small, relative to what a full UBI program may look like, so caution should be taken before extrapolating results to larger payments. Lastly, the impact of the disbursement persists for about five months. Further, because the disbursement is in the fall, the contraction comes during the portion of the year when the Alaskan labor market is relatively small. Taking these last two points together, the size of the labor market contraction induced by a $1,000 increase in the per person PFD is actually much smaller (approximately 0.7%) on an annual basis.

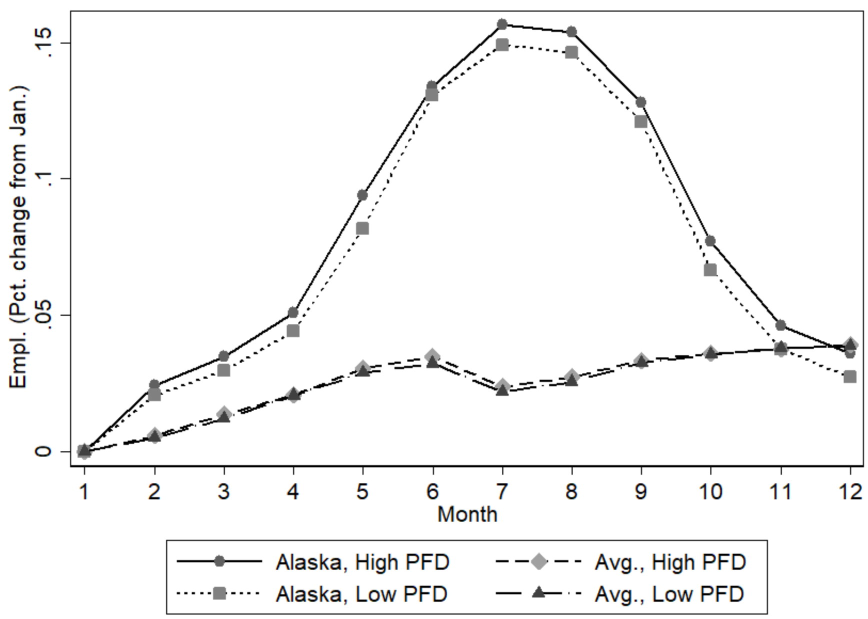
It is also important to note that calculating the average change in hours, as we do here, might overlook other potential benefits from the re-allocation of time from the labor market toward household work. In particular, the increase in employment is largely driven by single men with no children in the household. On the other hand, the decline in labor comes through a labor supply response, which is strongest among young women with young children in the household. This re-allocation could have societal benefits outside of the labor market, as the reduction concentrated in households with young children could have secondary effects on child development and human capital development (Bettinger et al., 2014; Cunha et al., 2006; Coneus et al., 2012).

Finally, one potential avenue for future research is to confirm our results using administrative data, which could better identify PFD recipients and potentially allow for an improved research design to disentangle the supply and demand side responses. In addition, the heterogeneous responses uncovered in this paper raise important questions about the long-term effects of universal cash transfers. In particular, a holistic view on the effects of unconditional transfers should also consider substitution patterns between time spent in the labor force and in other activities.

Figure 1: Hours of Work by Month

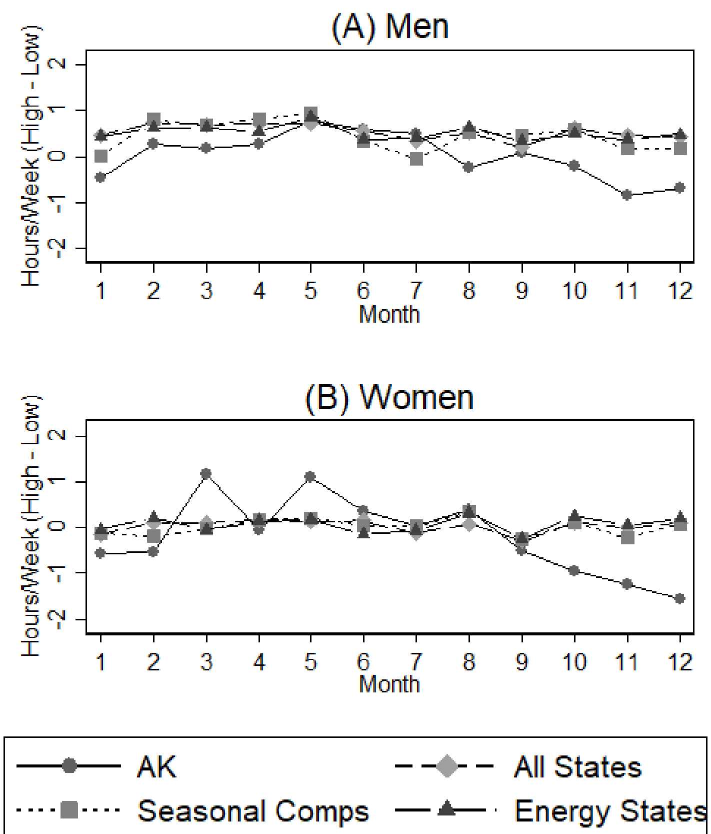
Note: Panels A, B, and C show average hours among those employed calculated from the CPS, weighted by final respondent weights. Hours are averaged to the state-year-month level, then aggregated accordingly by month. Each panel contains two lines: one graphs the average hours per week in each month for Alaska, and the other graphs the average hours of work in all other states. Panel A contains observations for men only. Panel B contains hours for unmarried and non-cohabiting women. Panel C contains observations for married or cohabiting women. Panel D graphs monthly employment in Alaska, and the average monthly employment in the average of all other states. Employment levels from BLS data. Employment is measured as percentage change from January of the corresponding year, and averaged by month.

Figure 2: Employment by Month



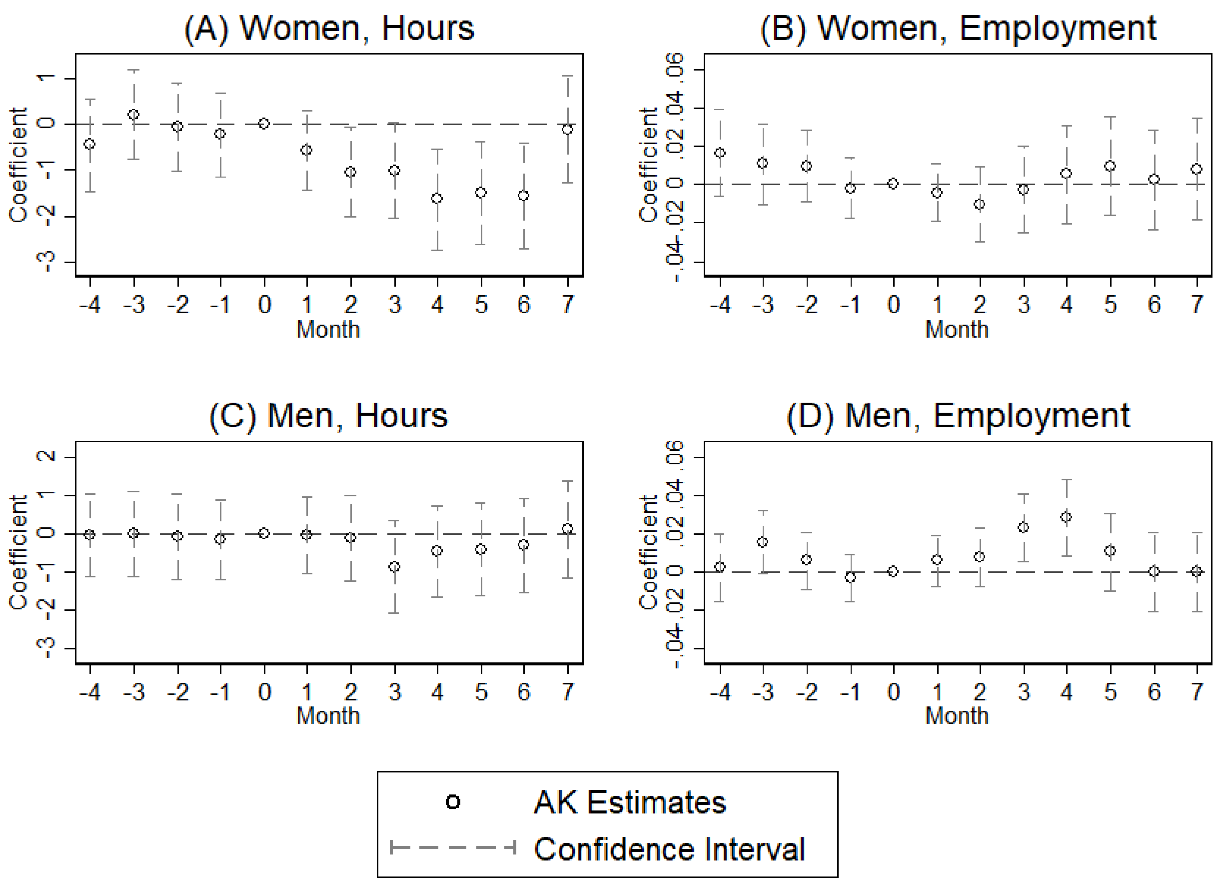
Note: From BLS employment level data. Percent change in employment, relative to January of same year. Averaged by state over 1994 - 2016. High PFD years are 1996 - 2002, 2007 - 2008, and 2014 - 2015. In High PFD years the per person disbursement was over $1,700 in 2016 dollars. In Low PFD Years the disbursement was less than $1,600 in 2016 dollars.

Figure 3: Hours Differences in High vs. Low PFD Years



Note: CPS average hours among those employed, weighted by final respondent weights. Average to the state-year-month level, then aggregated accordingly to high and low PFD years. The figures display differences between the average hours in high and low PFD years for each month. Each panel contains four lines: One for Alaska respondents, and another line for each of the three control group averages. Panel A contains observations for the sample of men. Panel B contains observations for unmarried/non-cohabiting women. Panel C contains observations from married/cohabiting women. High PFD years are 1996 - 2002, 2007 - 2008, and 2014 - 2015. In High PFD years the per person disbursement was over $1,700 in 2016 dollars. In Low PFD Years the disbursement was less than $1,600 in 2016 dollars. The states with most comparable seasonality are MT, WY, SD, and ME, and the energy states are based on Snead (2009).

Figure 4: Estimates by Month



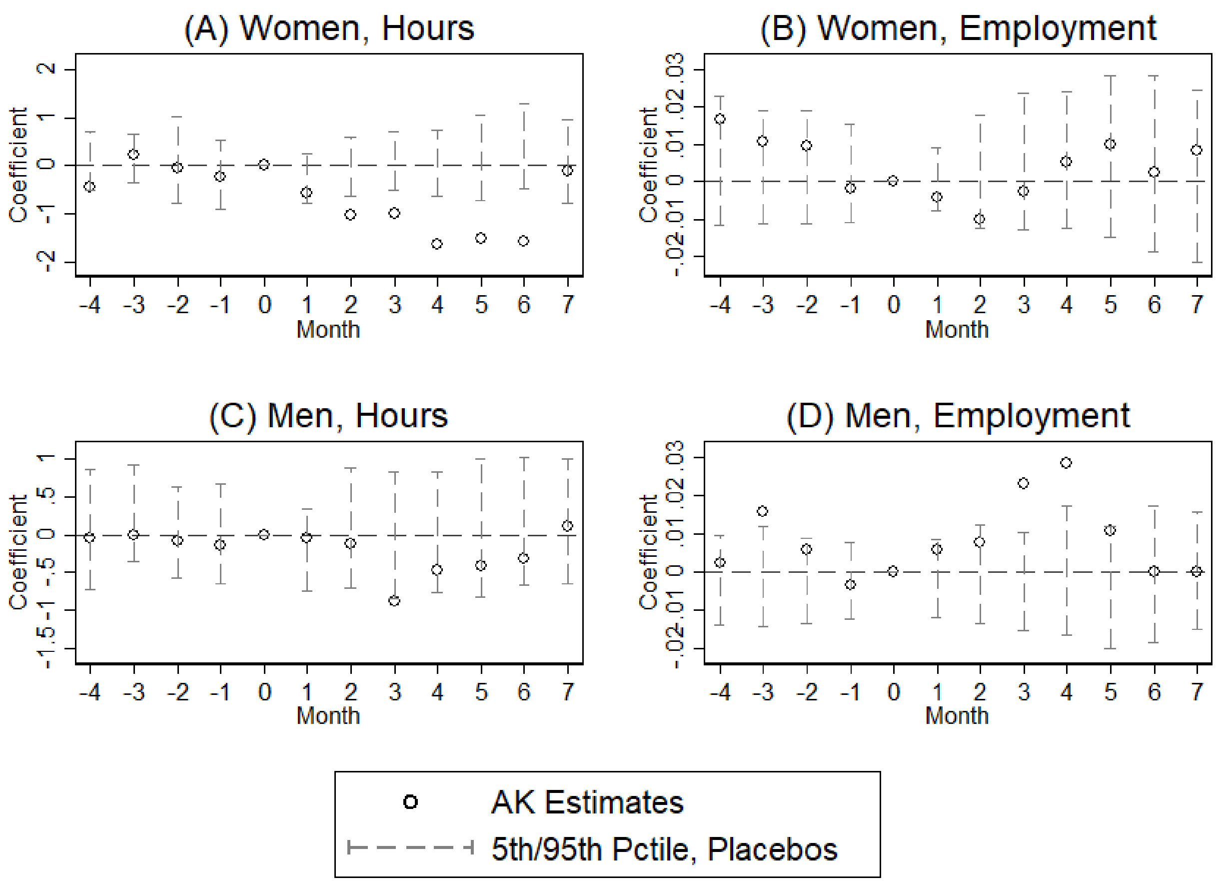
Note: This figure displays month-specific estimates for the effect of an additional $1,000 in the per-person PFD on hours and employment in Alaska, i.e. each dot represents a $\hat{\beta}_m$ from Equation 1, which are the coefficients on size of the PFD, PFD_y, interacted with month dummies. August is the omitted month in each regression. The dotted vertical lines represent 95% confidence intervals. Standard errors are clustered at the household level.

Table 1: PFD ($1,000s)

	Prob(Employed)		*(Hours | Employed)*	
	Men	Women	Men	Women
	(1)	(2)	(3)	(4)
Post X PFD(1000s)	0.017**	-0.006	-0.273	-1.265***
	(0.007)	(0.009)	(0.354)	(0.334)
Observations	79,157	88,501	68,015	64,446

Notes: Estimates of the coefficient on the *Post* interaction with PFD size as shown in Equation 2, i.e. $\hat{\beta}$. Only includes Alaska observations. In this specification, the per person size of the PFD in the given year, measured in $1,000s, is interacted with the post variable. The coefficients for that interaction term are reported in the table. All regressions weighted by the individual final weight. Standard errors are clustered at the household level. *** p<0.01, ** p<0.05, * p<0.10.

Figure 5: Placebo Tests by Month



Note: This figure displays month-specific estimates for the effect of an additional $1,000 in the per-person PFD on hours and employment in Alaska, i.e. each dot represents a $\hat{\beta}_m$ from Equation 1, which are the coefficients on size of the PFD, PFD_y, interacted with month dummies. August is the omitted month in each regression. As a placebo test, we repeat this exercise for each state and D.C., and include the 5th to 95th percentile range in this figure as the dotted vertical line for each month.

Table 2: Heterogeneity by Work Status (PFD $1,000s)

	Panel A: Employment			
	Men		*Women*	
	Full (1)	Part (2)	Full (3)	Part (4)
Post X PFD	-0.006	0.006	-0.021**	0.021**
	(0.005)	(0.005)	(0.009)	(0.009)
Observations	68,015	68,015	64,446	64,446

	Panel B: Hours per Week			
	Men		*Women*	
	Full (1)	Part (2)	Full (3)	Part (4)
Post X PFD	-0.060	-1.094	-1.084***	-0.001
	(0.349)	(0.856)	(0.322)	(0.448)
Observations	63,876	4,139	49,772	14,674

Notes: Estimated effect of the size of the PFD, measured in $1,000s, on employment and hours of work. In this specification, the size of the PFD is interacted with the post variable. The coefficients for that interaction term are reported in the table. Includes Alaska observations only. All amounts are measured in 2016 dollars. Effects on type of employment in Panel (A) are estimated by restricting to the sample of employed respondents and using a dummy variable for full- or part-time employment as the outcome. They should be interpreted as relative shifts in employment. Effects in Panel (B) for hours of work are estimated by restricting the sample to those employed full- or part-time, so the effects are directly comparable to the main estimates. All regressions weighted by the individual final weight. Standard errors are clustered by household. *** p<0.01, ** p<0.05, * p<0.10.

Table 3: Men - Heterogeneity by Age and Children (PFD $1,000s)

	Panel A: Male Employment								
	All	*Marital Status*		*Age*		*Has Children*		*Has Children LT 5*	
		Single	Married	20 - 30	31 - 55	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post X PFD	0.017**	0.025	0.012*	0.019	0.016**	0.027**	0.007	0.022***	-0.009
	(0.007)	(0.015)	(0.007)	(0.017)	(0.007)	(0.011)	(0.008)	(0.008)	(0.014)
Observations	79,157	21,095	58,062	12,221	66,936	35,703	43,454	65,005	14,152

	Panel B: Male Hours per Week								
	All	*Marital Status*		*Age*		*Has Children*		*Has Children LT 5*	
		Single	Married	20 - 30	31 - 55	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post X PFD	-0.273	-0.274	-0.276	-1.002	-0.137	-0.363	-0.193	-0.354	0.005
	(0.354)	(0.655)	(0.416)	(0.745)	(0.395)	(0.505)	(0.484)	(0.388)	(0.812)
Observations	68,015	16,365	51,650	10,450	57,565	29,160	38,855	55,283	12,732

Notes: Estimates of the coefficient on the *Post* interaction with PFD size as shown in Equation 2 for different subsamples. In this specification, the per person size of the PFD in the given year, measured in $1,000s, is interacted with the post variable. The coefficients for that interaction term are reported in the table. Includes Alaska observations only. All amounts are measured in 2016 dollars. Column (1) shows the main estimate from Table 1. All regressions weighted by the individual final weight. Standard errors are clustered by household. *** p<0.01, ** p<0.05, * p<0.10.

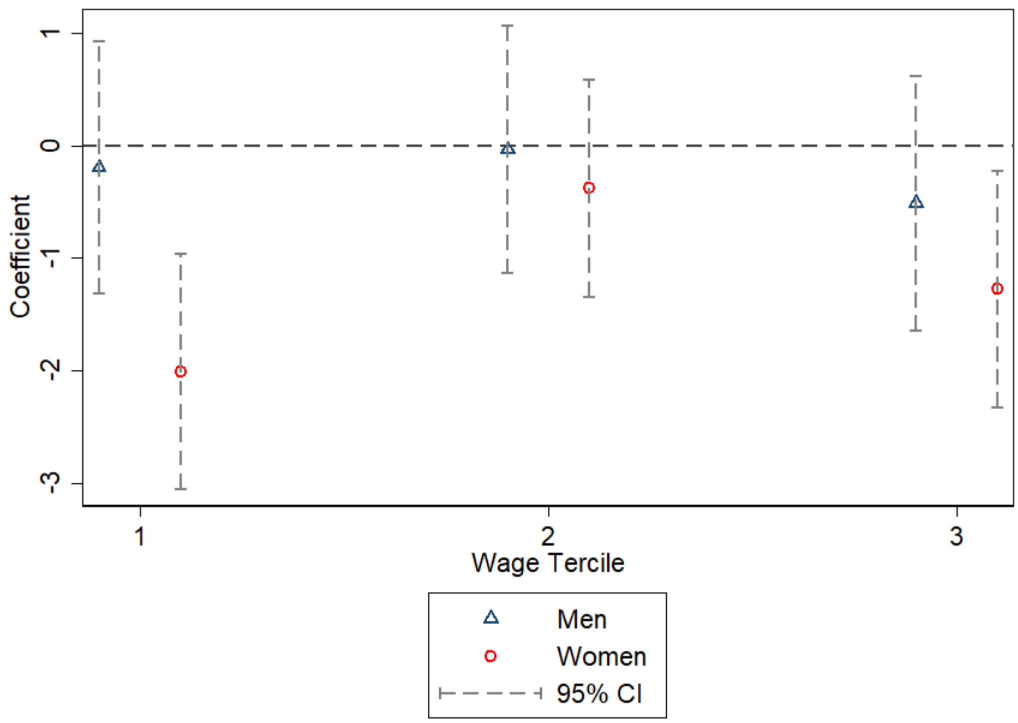
Table 4: Women - Heterogeneity by Age and Children (PFD $1,000s)

	Panel A: Female Employment								
	All	*Marital Status*		*Age*		*Has Children*		*Has Children LT 5*	
		Single	Married	20 - 30	31 - 55	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post X PFD	-0.006	-0.015	-0.002	-0.019	-0.003	0.002	-0.012	0.001	-0.038*
	(0.009)	(0.015)	(0.011)	(0.019)	(0.010)	(0.013)	(0.012)	(0.010)	(0.021)
Observations	88,501	20,624	67,877	18,037	70,464	32,158	56,343	69,515	18,986

	Panel B: Female Hours per Week								
	All	*Marital Status*		*Age*		*Has Children*		*Has Children LT 5*	
		Single	Married	20 - 30	31 - 55	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post X PFD	-1.265***	-1.126*	-1.353***	-2.142***	-1.111***	-1.064**	-1.579***	-1.149***	-2.147***
	(0.334)	(0.589)	(0.395)	(0.715)	(0.375)	(0.509)	(0.429)	(0.358)	(0.829)
Observations	64,446	15,954	48,492	11,917	52,529	25,099	39,347	53,520	10,926

Notes: Estimates of the coefficient on the *Post* interaction with PFD size as shown in Equation 2 for different subsamples. In this specification, the per person size of the PFD in the given year, measured in $1,000s, is interacted with the post variable. The coefficients for that interaction term are reported in the table. Includes Alaska observations only. All amounts are measured in 2016 dollars. Column (1) shows the main estimate from Table 1. All regressions weighted by the individual final weight. Standard errors are clustered by household. *** p<0.01, ** p<0.05, * p<0.10.

Figure 6: Heterogeneity in Hours by Wage Tercile



Note: This figure displays the estimated coefficients and confidence intervals for the effect of the PFD on hours of work in the full, male, and female samples by predicted wage tercile. In this specification, the per person size of the PFD in the given year, measured in $1,000s, is interacted with the post variable. The coefficients for that interaction term are the estimates shown in the figure. All amounts are measured in 2016 dollars. Terciles are based on reported wages for respondents with non-missing wages, and are based on predicted wages for other observations. Covariates used for predicted wages are age by month quadratic functions, age by year quadratic functions, education level by year dummy variables, education by month dummy variables, part-time work by month dummy variables, broad industry by occupation category dummies, broad industry by month dummies, broad industry by year dummies, and occupation by month and year dummy variables. Standard errors are clustered by household. All regressions weighted by the individual final weight.

References

Akee, R. K., W. E. Copeland, G. Keeler, A. Angold, and E. J. Costello (2010). Parents' incomes and children's outcomes: A quasi-experiment using transfer payments from casino profits. *American Economic Journal: Applied Economics*.

Bettinger, E., T. Hægeland, and M. Rege (2014). Home with mom: the effects of stay-at-home parents on childrens long-run educational outcomes. *Journal of Labor Economics 32*(3), 443–467.

Blau, F. D. and L. M. Kahn (2007, 7). Changes in the Labor Supply Behavior of Married Women: 19802000. *Journal of Labor Economics 25*(3), 393–438.

BLS (2018). Bureau of labor statistics, current employment statistics. retrieved from FRED, Federal Reserve Bank of St. Louis; https://fred.stlouisfed.org.

Blundell, R. and T. MaCurdy (1999). Labor supply: A review of alternative approaches. In *Handbook of labor economics*, Volume 3, pp. 1559–1695. Elsevier.

Burtless, G. (1986). The Work Response to a Guaranteed Income: A Survey of Experimental Evidence. Technical report.

Carrington, W. J. (1996). The Alaskan Labor Market during the Pipeline Era. *Journal of Political Economy*.

Cesarini, D., E. Lindqvist, M. J. Notowidigdo, and R. Östling (2017). The effect of wealth on individual and household labor supply: Evidence from Swedish lotteries. *American Economic Review*.

Coneus, K., M. Laucht, and K. Reuß (2012). The role of parental investments for cognitive and noncognitive skill formation-evidence for the first 11 years of life. *Economics & Human Biology 10*(2), 189–209.

Cunha, F., J. J. Heckman, L. Lochner, and D. V. Masterov (2006). Interpreting the evidence on life cycle skill formation. *Handbook of the Economics of Education 1*, 697–812.

Feinberg, R. M. and D. Kuehn (2018, 7). Guaranteed Nonlabor Income and Labor Supply: The Effect of the Alaska Permanent Fund Dividend. *The B.E. Journal of Economic Analysis & Policy 18*(3).

Flood, S., M. King, R. Rodgers, S. Ruggles, and J. R. Warren (2018). Integrated public use microdata series, current population survey: Version 6.0 [dataset]. Minneapolis, MN: IPUMS, 2018. https://doi.org/10.18128/D030.V6.0.

Hsieh, C. T. (2003). Do consumers react to anticipated income changes? Evidence from the Alaska permanent fund. *American Economic Review*.

Imbens, G. W., D. B. Rubin, and B. I. Sacerdote (2001). Estimating the Effect of Unearned Income on Labor Earnings, Savings, and Consumption: Evidence from a Survey of Lottery Players.

Jones, D. and I. E. Marinescu (2018). The Labor Market Impacts of Universal and Permanent Cash Transfers: Evidence from the Alaska Permanent Fund. *NBER, WP no. 24312*.

Kueng, L. (2018, 11). Excess Sensitivity of High-Income Consumers. *The Quarterly Journal of Economics 133*(4), 1693–1751.

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

Maynard, R. A. and R. J. Murnane (1979). The Effects of a Negative Income Tax on School Performance: Results of an Experiment. Technical Report 4.

Moffitt, R. A. (2016). *Economics of means-tested transfer programs in the United States.* University of Chicago Press.

Munnell, A. H. (1987). Lessons from the Income Maintenance Experiments. Federal Reserve Bank of Boston.

Murray, C. (2008). Guaranteed income as a replacement for the welfare state. *The Foundation of Law, Justice and Society*.

Nichols, A. and J. Rothstein (2015). The earned income tax credit. In *Economics of Means-Tested Transfer Programs in the United States, Volume 1*, pp. 137–218. University of Chicago Press.

Price, D. J., J. Song, M. Duggan, D. Garcia-Macia, C. Hoxby, M. Kurz, M. Lenel, Q. Li, D. Malacrino, S. Pérez, S. Saavedra, J. Rios, P. Tebaldi, and A. Villacorta (2016). The Long-Term Effects of Cash Assistance. Technical report.

Robins, P. K. (1985). A Comparison of the Labor Supply Findings from the Four Negative Income Tax. Technical Report 4.

Sila, U. and R. M. Sousa (2014). Windfall gains and labour supply: evidence from the European household panel. *IZA Journal of Labor Economics*.

Snead, M. C. (2009). Are the Energy States Still Energy States? *Federal Reserve Bank of Kansas City Economic Review Fourth Quarter*, 43–68.

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

Watson, B., M. Guettabi, and M. Reimer (2019). Universal Cash and Crime. *The Review of Economics and Statistics Forthcoming.*

Yang, T.-T. (2018). Family Labor Supply and the Timing of Cash Transfers: Evidence from the Earned Income Tax Credit. *Journal of Human Resources 53*(2), 445–473.

Appendix

(Not Intended for Publication)

Table A1: PFD Summary Statistics (1994-2016)

Year (1)	AK Pop. (2)	Pct. Applied (3)	Pct. Paid (4)	Dividend (2016 $) (5)	Date (6)
2016	739,828	91.2%	86.3%	$1,022.00	6-Oct
2015	737,625	92.0%	87.0%	$2,098.23	1-Oct
2014	735,601	92.0%	86.6%	$1,910.27	2-Oct
2013	736,399	91.3%	86.1%	$927.04	3-Oct
2012	732,298	92.8%	87.6%	$917.77	4-Oct
2011	722,190	93.9%	89.3%	$1,252.82	6-Oct
2010	710,231	94.4%	89.8%	$1,409.63	7-Oct
2009	692,314	95.4%	90.3%	$1,460.14	8-Oct
2008	679,720	95.4%	90.7%	$3,644.03	12-Sep
2007	674,510	94.1%	89.0%	$1,914.91	3-Oct
2006	670,053	93.9%	88.8%	$1,317.81	4-Oct
2005	663,253	95.4%	90.1%	$1,039.34	12-Oct
2004	656,834	96.1%	91.3%	$1,168.67	12-Oct
2003	647,747	96.6%	92.0%	$1,444.64	8-Oct
2002	640,544	97.0%	92.1%	$2,055.49	9-Oct
2001	632,241	98.1%	92.8%	$2,507.44	10-Oct
2000	627,533	98.7%	93.0%	$2,737.09	4-Oct
1999	622,000	95.3%	92.2%	$2,549.59	6-Oct
1998	617,082	94.8%	91.7%	$2,268.78	7-Oct
1997	609,655	94.4%	91.1%	$1,938.75	8-Oct
1996	605,212	93.5%	90.3%	$1,729.53	9-Oct
1995	601,581	93.9%	90.2%	$1,559.53	6-Oct
1994	600,622	93.2%	89.1%	$1,593.36	12-Oct

Notes: PFD dates and amounts come from Alaska Department of Revenue's Permanent Fund Dividend Annual reports. The date is the date of the first direct deposits for that year. Dividend amounts are measured in 2016 dollars. *Pct. Applied* refers to the percent of the state population that submitted an application and *Pct. Paid* refers to the percent of the population that received a dividend that year. Some applicant may not meet the baseline eligibility requirements. In addition, there may be involuntary (e.g. child support or uncollected government fees) or voluntary (e.g. tax exempt college savings or charitable contribution) garnishments to disbursements.

Table A2: Alaska Summary (Apr - Aug)

	Men		*Women*	
	(1) High PFD	(2) Low PFD	(4) High PFD	(5) Low PFD
Hours	37.44	36.61	24.89	24.37
	(22.67)	(22.44)	(21.32)	(20.86)
Employed	0.88	0.86	0.73	0.72
	(0.33)	(0.34)	(0.44)	(0.45)
Num. Children in HH	1.17	1.11	1.34	1.27
	(1.32)	(1.33)	(1.31)	(1.31)
Num. Children lt5 in HH	0.25	0.24	0.30	0.29
	(0.57)	(0.57)	(0.61)	(0.61)
Less than HS	0.05	0.06	0.05	0.05
	(0.23)	(0.25)	(0.22)	(0.21)
HS	0.32	0.31	0.29	0.26
	(0.47)	(0.46)	(0.45)	(0.44)
Some College	0.26	0.26	0.27	0.28
	(0.44)	(0.44)	(0.44)	(0.45)
College Degree	0.27	0.28	0.30	0.32
	(0.44)	(0.45)	(0.46)	(0.47)
Advanced Degree	0.09	0.09	0.09	0.09
	(0.29)	(0.29)	(0.28)	(0.29)
Age	40.82	40.64	39.18	39.44
	(8.89)	(9.35)	(9.43)	(9.66)
White	0.82	0.81	0.78	0.78
	(0.38)	(0.39)	(0.42)	(0.42)
Black	0.03	0.03	0.03	0.04
	(0.17)	(0.18)	(0.18)	(0.19)
Hispanic	0.02	0.03	0.03	0.03
	(0.15)	(0.18)	(0.17)	(0.18)
Am. Indian	0.09	0.07	0.12	0.10
	(0.29)	(0.26)	(0.33)	(0.30)
Asian	0.01	0.01	0.02	0.01
	(0.12)	(0.12)	(0.12)	(0.12)
Multiple Races	0.02	0.04	0.02	0.04
	(0.13)	(0.19)	(0.13)	(0.20)
Married	0.73	0.71	0.77	0.75
	(0.44)	(0.45)	(0.42)	(0.43)
Unemployment Rate	6.75	7.32	6.75	7.32
	(0.45)	(0.43)	(0.45)	(0.43)
Crude Oil Price	45.09	59.61	44.54	59.13
	(36.30)	(31.83)	(36.24)	(31.62)
Observations	15902	17162	17319	19741

Notes: Mean and standard deviations of each subsample. *Married* includes married, but not cohabiting, couples. Co-habiting couples are not included. Comparing high and low PFD years in January - August only. High PFD years are those with a PFD greater than $1700: 1996, 1997, 1998, 1999, 2000, 2001, 2002, 2007, 2008, 2014, 2015. Low PFD years had a PFD less than $1600. The average PFD among observations in high PFD years is $2,305, and the average among observations in low PFD years is $1,259. All amounts are measured in 2016 dollars. Final respondent weights are used.

Figure A1: Treatment Densities for Non-Alaska States

Note: Each panel shows the density of treatment effects from estimating the effect of an additional $1,000 in the per-person PFD on states that did not actually receive the treatment. The density in each panel includes an estimate for each other state and D.C. The dashed vertical lines are at the point of the main estimates of the effect of an additional $1,000 in the per-person PFD on labor market outcomes in Alaska (see Table 1). The solid vertical lines are at the 5th and 95th percentiles of the distribution of placebo effects.

Table A3: Heterogeneity by Wage Tercile (PFD $1,000s)

	Panel B: Men			
	All	*Wage Tercile*		
		Low	Middle	High
	(1)	(2)	(3)	(4)
Post X High	-0.27	-0.20	-0.04	-0.52
	(0.35)	(0.57)	(0.56)	(0.58)
Observations	68,015	22,616	22,788	22,611

	Panel C: Women			
	All	*Wage Tercile*		
		Low	Middle	High
	(1)	(2)	(3)	(4)
Post X High	-1.27***	-2.01***	-0.38	-1.28**
	(0.33)	(0.53)	(0.49)	(0.54)
Observations	64,446	21,621	21,519	21,306

Notes: Estimated effect of the size of the PFD, measured in $1,000s, on hours of work by wage tercile. In this specification, the size of the PFD is interacted with the post variable. The coefficients for that interaction term are reported in the table. Column (1) shows the main estimates from Table 1. Columns 2 - 4 show estimates specific to each predicted wage tercile. Terciles are based on reported wages for cases with non-missing wages, and are based on predicted wages for other observations. Covariates used for predicted wages are age by month quadratic functions, age by year quadratic functions, education level by year dummy variables, education by month dummy variables, part-time work by month dummy variables, broad industry by occupation category dummies, broad industry by month dummies, broad industry by year dummies, and occupation by month and year dummy variables. All regressions weighted by the individual final weight. Standard errors are clustered by household. *** p<0.01, ** p<0.05, * p<0.10.

Table A4: Bootstrap Estimates PFD ($1,000s)

	Men			*Women*		
	Total (1)	Employment (2)	Hours (3)	Total (4)	Employment (5)	Hours (6)
$\hat{\beta}$	0.444	0.017	-0.286	-1.133	-0.006	-1.252
(SE)	(0.406)	(0.007)	(0.347)	(0.407)	(0.009)	(0.339)
t-stat	1.094	2.581	-0.824	-2.782	-0.667	-3.698
P-value	0.274	0.010	0.410	0.005	0.504	0.000

Notes: The first row displays the main estimates of the coefficient on the *Post* interaction with PFD size as shown in Equation 2, i.e. $\hat{\beta}$, and the total effects. The *Employment* and *Hours* estimates are the bootstrapped version of the estimates in Table 1. The *Total* estimates are calculated as discussed in Section 6. The standard errors are estimated as the standard deviation of the bootstrap estimates with sampling done at the household level and based on 1000 bootstrap repetitions. We use the normal distribution to calculate p-values.