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Authors
Bibler, Andrew
Guettabi, Mouhcine
Reimer, Matthew N

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Universal Cash Transfers and Labor Market Outcomes

Andrew Bibler* Mouhcine Guettabi†‡ Matthew Reimer §

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Abstract

One major criticism of Universal Basic Income is that unconditional cash transfers discourage recipients from working. Evidence to date has largely relied on targeted and/or conditional transfer programs. However, it is difficult to draw conclusions from such programs because universal transfers may induce a positive demand shock by distributing cash to a large portion of the population, which may in turn offset any negative labor-supply responses. We estimate the causal effects of universal cash transfers on short-run labor market activity by exploiting the timing and variation in size 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. Small negative effects on the number of hours worked are found for women, especially those with young children. In contrast, we find an increase in the probability of employment for males in the months following the distribution. Altogether, a $1,000 increase in the per-person disbursement leads to a 0.8% labor market contraction on an annual basis.

JEL Classification: J2, I38, H53

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*Department of Economics, University of Nevada, Las Vegas, NV 89154.

†Department of Economics and Finance, University of North Carolina Wilmington, Wilmington, NC 28403

‡Corresponding author. Email: guettabim@uncw.edu

§Department of Agricultural and Resource Economics, University of California Davis, Anchorage, CA 95616

1 Introduction

Universal Basic Income (UBI) constitutes an unconditional cash transfer that is provided 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). In contrast, an often-cited criticism of UBI is that unconditional cash disbursements create a strong work disincentive if leisure is a normal good (Robins, 1985). Evaluations of means-based and conditional transfers, such as the Earned Income Tax Credit (EITC) and Negative Income Tax Experiments (NIT), and unconditional transfers, such as casino revenues and lottery winnings, find some evidence of such a work disincentive (e.g., Munnell, 1987; Maynard and Murnane, 1979; Cesarini et al., 2017; Price et al., 2016; Yang, 2018; Picchio et al., 2018). However, UBI differs from such transfer programs because it is distributed to the population at large, which may lead to a positive demand shock for consumption goods and services, and in turn, upward pressure on the demand for labor. Negative labor-supply responses may therefore be offset by such labor-demand shocks. Unfortunately, evaluating the labor-market consequences of UBI is challenging because universal cash transfers are rare in practice.

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: nearly all Alaska residents are eligible to receive the PFD, regardless of income. Furthermore, the annual

[1]For example, several countries, such as Finland and India, have recently implemented basic income experiments. Y Combinator Research, a nonprofit research lab in Silicon Valley, has recently undertaken a randomized control trial aimed at understanding the effectiveness of basic 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.

distribution of the PFD is the single largest infusion of money into Alaska's 55 billion-dollar economy, and therefore, has the potential to generate a positive demand shock for consumption goods and services (Kueng, 2018).[2] Using employment data from the Current Population Survey (CPS) from 1994-2017, we exploit two exogenous sources of temporal variation to examine the PFD's effect on labor-market outcomes: the intra-annual variation in time relative to the PFD disbursements and the inter-annual variation in the size of the PFD payment.

Our results suggest that the PFD induces both a positive labor demand shock and a negative labor supply response. 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 for employed women in the months following the PFD disbursement. 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 as evidence of a labor supply response to the cash distribution. In contrast, 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 for men 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. 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 0.8% contraction on an annual basis.

Although UBI is largely inspired by long-run objectives, we focus on estimating the short-run effects of the PFD because the implementation of any cash transfer program can induce short-run behavioral responses, which could have long-run implications. Contrary to the permanent income hypothesis, a rich literature demonstrates that people tend to exhibit

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

short-run impatience, whereby consumption and economic activity increase immediately following cash transfers.[3] Indeed, recent work has documented such short-run impatience with respect to the PFD, in terms of consumption (Kueng, 2018), criminal activity (Watson et al., 2020), and substance abuse (Evans and Moore, 2011). If recipients change their behavior in response to the payment, then the frequency and timing of transfer payments become important policy instruments. For instance, staggered payments throughout the year may aid in reducing payment-induced criminal activity (e.g., Carr and Packham, 2017; Watson et al., 2020) and/or help recipients smooth out their consumption, while larger less-frequent payments may be useful for achieving expansionary fiscal-policy objectives. The evidence in this paper implies that large annual universal payments, such as the PFD, have short-run implications for both labor supply and demand; thus, the timing and frequency of payments for any UBI program are likely to have important welfare implications (Kueng, 2018).

The size and universal nature of the PFD also makes it useful for learning about short-run behavioral responses to government stimulus programs, such as the 2001 federal income tax rebate (Johnson et al., 2006), the 2008 Economic Stimulus Act (Parker et al., 2013; Powell, 2020), and the 2009 Cash for Clunkers Program (Mian and Sufi, 2012). Previous studies in this area have largely focused on the short-run changes in consumer behavior, in terms of expenditures (e.g. Mian and Sufi, 2012; Parker et al., 2013) and bankruptcies (Gross et al., 2014), and find that consumers do indeed exhibit short-run responses to these programs. Only recently has attention been given to short-run labor-supply responses to such programs. Using exogenous variation in the timing of the 2008 tax rebates, Powell (2020) finds evidence of a negative labor-supply response: each rebate dollar reduced monthly earnings by nine cents, demonstrating that stimulus payments can crowd out short-term labor earnings and offset the gains associated with increases in consumer spending. Our results are consistent

[3]Examples include welfare payments, such as the Food Stamp Program of the Temporary Assistance for Needy Families (TANF) Program (Stephens, 2003; Shapiro, 2005; Stephens and Unayama, 2011; Foley, 2011), and stimulus programs (Feyrer and Sacerdote, 2011; Mian and Sufi, 2012; Parker et al., 2013)

with these findings, and supplement the literature by demonstrating the potential for both negative labor-supply responses and consumer-induced labor-demand shocks from large cash injections into the economy.

While the universal and unconditional nature of the PFD makes it useful for inferring potential labor-market effects of universal income, we note two differences between the PFD and popular perceptions of what a UBI program would look like. First, the PFD is funded through a wealth fund, rather than taxes or reductions in public programs. While this may differ from expected UBI funding sources, we consider this a strength of our setting, since the labor-market responses we estimate are free of any tax distortions. Given that a variety of tax policies could be used for funding UBI transfers, estimates of pure labor-market responses are preferred because they are more widely applicable to different settings. Second, while the average size of the PFD is smaller than many would expect from a UBI, it is in fact the largest continuous universal and unconditional transfer worldwide.[4] Moreover, analyzing transfers of different sizes, as we do here, can identify potential non-linearities in labor-market responses.[5] Thus, overall, despite these potential differences between the PFD and UBI perceptions, the PFD still provides the best opportunity for evaluating the labor-market implications of UBI (Marinescu, 2017).

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 present our results in Section 5, including a discussion of the role of heterogeneity across several important dimensions. The implications of our findings and conclusions are discussed in the final two sections.

[4]There have been larger unconditional distributions such as Mincome in Canada and the Finland experiment but they were both short lived. The PFD usually equated to 2% - 6% of personal income in a given year over our sample period, but the maximum was 10% in 2008 (Goldsmith, 2010; Berman and Reamey, 2016).

[5]Recent evidence suggests that there are not strong non-linearities in the size of the transfer, at least in terms of labor-supply responses to income shocks (Cesarini et al., 2017).

2 Background

2.1 Related Literature

The neoclassical model of labor-leisure choice predicts that if leisure is a normal good, utility maximizing agents will consume more leisure (i.e., work less) in response to a positive shock to unearned income. This so-called income effect on labor supply serves as the basis for concerns that UBI will create a strong disincentive to work. While useful, it is customary to further distinguish between permanent and transitory shocks to unearned income when considering the effects of cash-transfer programs. The life-cycle model, which considers an agent maximizing utility over their lifetime, predicts that income effects are only relevant to shocks that affect permanent (or lifetime) income. When such shocks become known to the agent, they can adjust and smooth their labor supply patterns over time through borrowing and savings. As a consequence, cash-transfer programs should only influence labor supply decisions through their effect on permanent income. Anticipated and transitory cash transfers, in contrast, should have no effect on labor supply decisions at the time the transfer is received. This permanent income hypothesis therefore seemingly obviates the need to consider the short-run impacts of such cash transfer programs.

Empirical evidence of short-run behavioral responses to anticipated and transitory income shocks, however, overwhelmingly contradicts the permanent income hypothesis. In particular, Kueng (2018) finds that short-run consumption responses to PFD payments are consistent with near-rational deviations from the permanent income hypothesis, whereby consumers exhibit non-optimizing behavior that results in relatively small losses of utility (Akerlof and Yellen, 1985). While the majority of such evidence comes from investigations of consumption behavior (e.g., Stephens, 2003; Johnson et al., 2006; Parker et al., 2013; Kueng, 2018), recent studies have also documented similar short-run labor-supply responses with respect to cash transfers, such as those from tax rebates (Powell, 2020), EITC payments (Yang, 2018), the NIT Experiments (Price et al., 2016), and lottery winnings (Cesarini et al., 2017; Imbens et al., 2001; Sila and Sousa, 2014; Picchio et al., 2018). For example, Price et al. (2016) estimate a 3.3 percentage-point decrease in the probability of employment and a 7.4% earnings reduction for the most generous of the NIT Experiments,[6] which provided guaranteed income to primarily low-income households. Similarly, 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. While the literature is not unanimous in finding short-run labor supply responses to cash transfers—Akee et al. (2010) find no discernible evidence of labor-supply responses to casino revenue dividends disbursed to the Eastern Band of Cherokee Indians—the evidence to date overwhelmingly suggests that short-run behavioral responses to predictable payments should be considered, especially when the timing and frequency of such payments is a policy decision.[7]

While the foregoing literature usefully documents short-term labor supply responses to anticipated cash transfers, the Alaska PFD offers several advantages over other programs for studying the labor-market effects of a UBI program. For example, unlike the PFD, the NIT Experiment 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.[8] Similarly, EITC payments are targeted toward low-income households, do not generally induce pure income effects, and are substantially smaller than PFD transfers.[9] 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 use these cash transfers to make inference on a UBI program. Importantly, in each of these settings, the distributions are to a relatively small proportion

[6]The Seattle/Denver Income Maintenance Experiment

[7]See Bastagli et al. (2019) for another example. In a review of conditional and unconditional cash transfers across a number of low- and middle-income countries, Bastagli et al. (2019) do not find consistent evidence of a systematic labor supply response to cash transfers.

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

[9]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).

of the population, which means they are unlikely to induce the demand shock that we might expect from a universal transfer (Marinescu, 2017).[10] In contrast, as we discuss in more detail below, the PFD is useful for inferring the labor-market impacts of UBI because it is universal, constitutes a pure income effect, and is large enough to induce a demand shock.

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 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, 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. 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.[11] These confounding factors make it difficult to draw conclusions about the causal impact of the PFD on the labor market from long-run trends.

Feinberg and Kuehn (2018) estimate the short-run effects of the PFD on the labor market

[10] One exception is the Spanish Christmas lottery, which distributes a large lottery prize (approximately 3.5% of a province's gross domestic product) to several thousand people sharing the same lottery ticket number. Bermejo et al. (2020) demonstrate that winning provinces experience a significant increase in entrepreneurial activity, part of which is driven by increases in local demand for goods and services.

[11] 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.

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 intra-annual variation around the 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 nor 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 nor CPS data are suited to isolate the supply-side income effects.

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 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.[12] 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. (2020) 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 Fund's returns. In 2008, the dividend reached a high of $3,269 (including a one-time supplement of $1,200 "energy rebate" financed by that year's 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

[12]The size of the disbursement deviated from the formula in 2016 when the Governor vetoed the initial disbursement proposal, reducing the PFD to one-half the size suggested by the formula. This political intervention took place in the last year of our sample. Our results are robust to dropping the last year from our sample.

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.[13] 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

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

of the household or the spouse to the head of the household. We exclude cohabiting couples in our sample. Finally, since they are less likely to receive the PFD, we drop those who are not US citizens.[14]

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., 2020) 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 seasonality 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, as a second source of temporal variation in the PFD, we exploit the inter-annual variation in the size of the PFD payment. Similar to a differences-in-differences (DiD) estimation strategy, which assumes that, in the absence of the treatment, treated states would experience the same labor-market trends as control states in post-treatment years, we assume that Alaska labor-market trends in post-disbursement months would be the same in low- and high-PFD years. As we demonstrate below, Alaska labor-market trends are

[14]We drop almost 10% of the observations due to this restriction. Citizenship is not required to receive the PFD, but a smaller proportion of non-citizens will be eligible. Citizenship status is available in the CPS starting in 1994. See Table A3 for results that include non-citizens.

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 thus exploits 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. Note that while other states do not provide good controls for a within-year DiD estimation strategy because of differences in seasonality, they make good candidates for placebo tests, which do not make within year cross state comparisons.

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 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 graphs the difference in average hours between high and low PFD years[15] for Alaska and three potential control groups: all other states, states with the most comparable seasonal employment patterns to Alaska, and energy states.[16] The relatively flat line around zero among the three potential control groups in the sample of men (Panel A) and women (Panel B) demonstrate there is little difference in hours worked per week between high and low PFD years. These patterns 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.[17]

Focusing on the differences in hours in the sample of Alaskan men in high and low PFD years (Figure 2), 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

[15]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

[16]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), and include CO, KS, LA, MS, MT, ND, NM, OK, TX, UT, WV, WY.

[17]For completeness, we also present the results of an estimation strategy that includes control states in our estimation strategy akin to the "triple difference" estimator.

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 - 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.[18] 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.[19] 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

[18] 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.

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

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:

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

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. We do this by splitting the sample and re-estimating the impact of the disbursement in each subsample. We also estimate potential shifts in full- and part-time employment[20] Our heterogeneity analysis helps us determine which subgroups are driving the main estimates, and can help guide future implementations of universal income trials.

[20]When estimating heterogeneous responses in the probability of employment by FT/PT, we use dummy variables that indicate employment in FT or PT work and estimate the probability using the full sample of employed and non-working.

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 5.3 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.

5.1 Main Results

Figure 3 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 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 3 includes zero.[21] Panel (A) of Figure 3 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

[21] We also interpret this as evidence that post-disbursement changes cannot be fully explained by reallocations of labor around the time of disbursement.

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 3. 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.[22] 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

[22] From Figure 3, 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.

demand shock outweighs any negative supply response to receiving the PFD.[23] This seems plausible, given the low supply response of male workers to income and wages found in previous literature.[24] In contrast, we find no significant impact of the disbursement on the probability of employment among the sample of women (column 2).[25]

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.[26]

To help evaluate the economic significance of the estimated responses to PFD disbursements, we combine the potentially offsetting effects of the disbursement on the intensive and extensive margins. For example, we find 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 estimate the effect on hours of working using the entire sample of employed

[23]The increased employment may also be consistent with frictions, e.g., commuting costs, that are temporarily alleviated through PFD payments.

[24]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.

[25]The positive effect on overall employment is robust to using aggregated data on employment levels from the Current Employment Statistics database.

[26]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).

and unemployed individuals. The corresponding estimates are presented in columns 5 and 6. This allows us to measure the total change in hours worked, while decomposing the potentially conflicting forces on the two margins.

We find no statistical evidence of a change in average overall labor among males, given a statistically-insignificant estimate of 0.4 hours per week. For the sample of women, analogous estimate suggests that the average total effect for the sample of women is -1.1 hours per week, which is statistically significant at the 1% level. Combining these estimates allows us to comment on the overall labor-market effects of the PFD and can serve as a baseline for future implementations of basic income. Estimating the overall effect on hours in the combined samples suggests a statistically significant decline of 0.57 hours per week. The decline amounts to a 1.9% 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.

In Table A3 we include several estimates based on alternate specifications and samples. In the first four rows, we either omit a control or include a broader sample. In all of these cases, the estimated effects are very similar to the main estimates. For example, we estimate that an increase of $1,000 in the per person PFD increases male employment by 1.4 and 1.8 percentage points in the samples that include non-citizens and cohabiting couples, respectively. Similarly, we find that the effect on hours of work among employed women is -1.2 in the samples that include non-citizens and cohabiting couples. In the last two rows of results, we estimate the effects on the under 20 and over 55 samples. The sample sizes are much smaller for these subgroups, and the estimates are generally statistically insignificant. The estimated change in hours among employed women is -2.3 in the sample under 20 years old and statistically significant at the 10% level. While the estimated effects of a higher PFD on male employment is negative in both of these samples, neither is statistically different from 0.

5.1.1 Placebo Tests

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 4, which provide further evidence that our main results are truly a reflection of the PFD disbursements. In Panel (A) of Figure 4, 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.

Comparing our estimates with the distribution of placebo effects from other states is similar to estimating our main effects while including observations from other states, similar to a triple differences estimation strategy. In Table A4 we provide estimates of regressions based on Equation 2 that are revised to accommodate two sets of untreated states: the most seasonally comparable states and energy states. We report coefficients on the interaction term *PFD x P x AK*, which describes the within-year changes in the labor market with a $1,000 increase in the per-person PFD, relative to within-year changes in non-AK states. The general patterns are similar when using this strategy, and overall conclusions remain the same. When using either set of control states, we find that the estimated change in probability of employment among men is 1.1 to 1.2 percentage points. Both are statistically significant at the 5% level, and are within about one standard error of the main estimate. Similarly, the estimated change in hours of work among employed women, a reduction of 0.8 hours per week, is smaller in magnitude than the main estimate. Both estimates are statistically significant at the 1% level, and both are in the 95% confidence interval of the main estimate in Table 1.

5.2 Heterogeneity by Work Status

Table 2 presents estimates of differential responses in employment and hours worked by full- and part-time work status. To estimate heterogeneous responses in the probability of employment, we use dummy variables for full-time or part-time employment as the outcome variable and estimate the effect of a $1,000 increase in the per-person PFD on the probability of employment in each, using the full sample. The estimates in Panel (A) of Table 2 can be interpreted as changes in the proportion of all respondents in full- or part-time work. For example, the estimated change in the probability of employment for full-time male workers is an increase of 1.1 percentage points, and the estimated change for part-time work is a 0.6 percentage point increase. These estimates describe the contribution of each type of employment to the overall increase in employment. 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 an increase in part-time work of 0.012, and a corresponding decrease in full-time work of 0.019. These indicate a 1.2 percentage point increase in the proportion of the sample in part-time jobs and a 1.9 percentage point decrease in the proportion of the sample in full-time work (columns 3 and 4 of Panel A). Both are statistically significant at the ten percent level. We also find some intensive-margin differences in the sample of women. We estimate a 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.

5.3 Heterogeneity by Age and Children

Tables 3 and 4 include differential estimates for the sample of men and women by marital status, age, and whether there are children in the household. While these are useful for determining the types of households that may be most responsive to the disbursements, the analysis is mostly suggestive and the confidence intervals for the subsample estimates generally contain the full-sample estimate.

5.3.1 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.

5.3.2 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.[27] 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 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 re-

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

allocated 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).

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 1.9% 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 0.8%) on an annual basis.

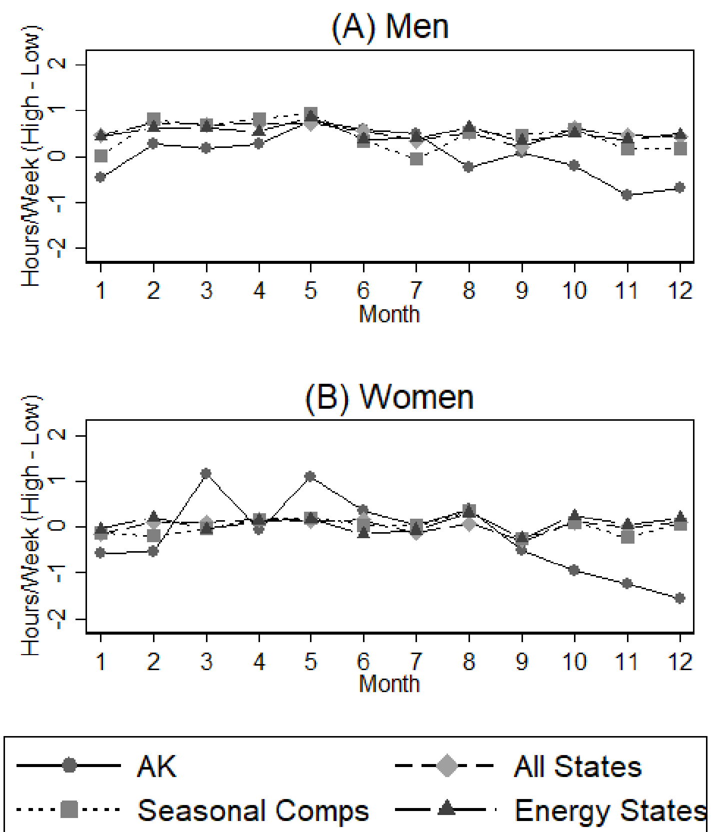
It is also important to note that calculating the average change in hours, as we do here, might overlook other potential benefits from the re-allocation of time from the labor market toward household work. In particular, the increase in employment is largely driven by single men with no children in the 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: Average Hours and Employment by Month

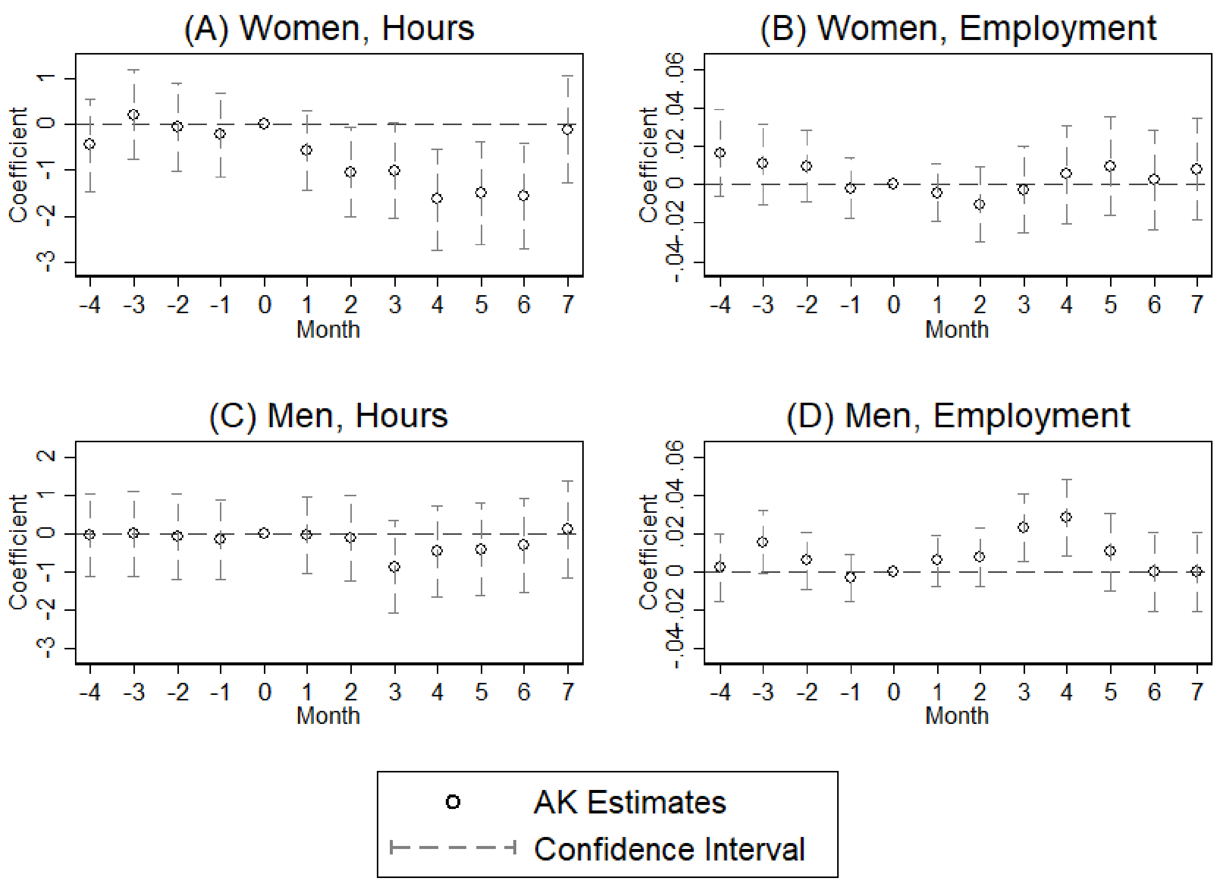
Note: Panels A and B show average hours among employed men and women, respectively. Averages were calculated from the CPS using final respondent weights. Hours are averaged to the state-year-month level, then aggregated accordingly by month. Each panel contains two lines: one graphs the average hours per week in each month for Alaska, and the other graphs the average hours of work in all other states. Panel C 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: Difference in Hours in High vs. Low PFD Years



Note: CPS average hours among employed respondents, weighted by final respondent weights. Averaged 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. 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), and energy states include CO, KS, LA, MS, MT, ND, NM, OK, TX, UT, WV, and WY (Snead, 2009).

Figure 3: Effect of PFD on Hours and Employment



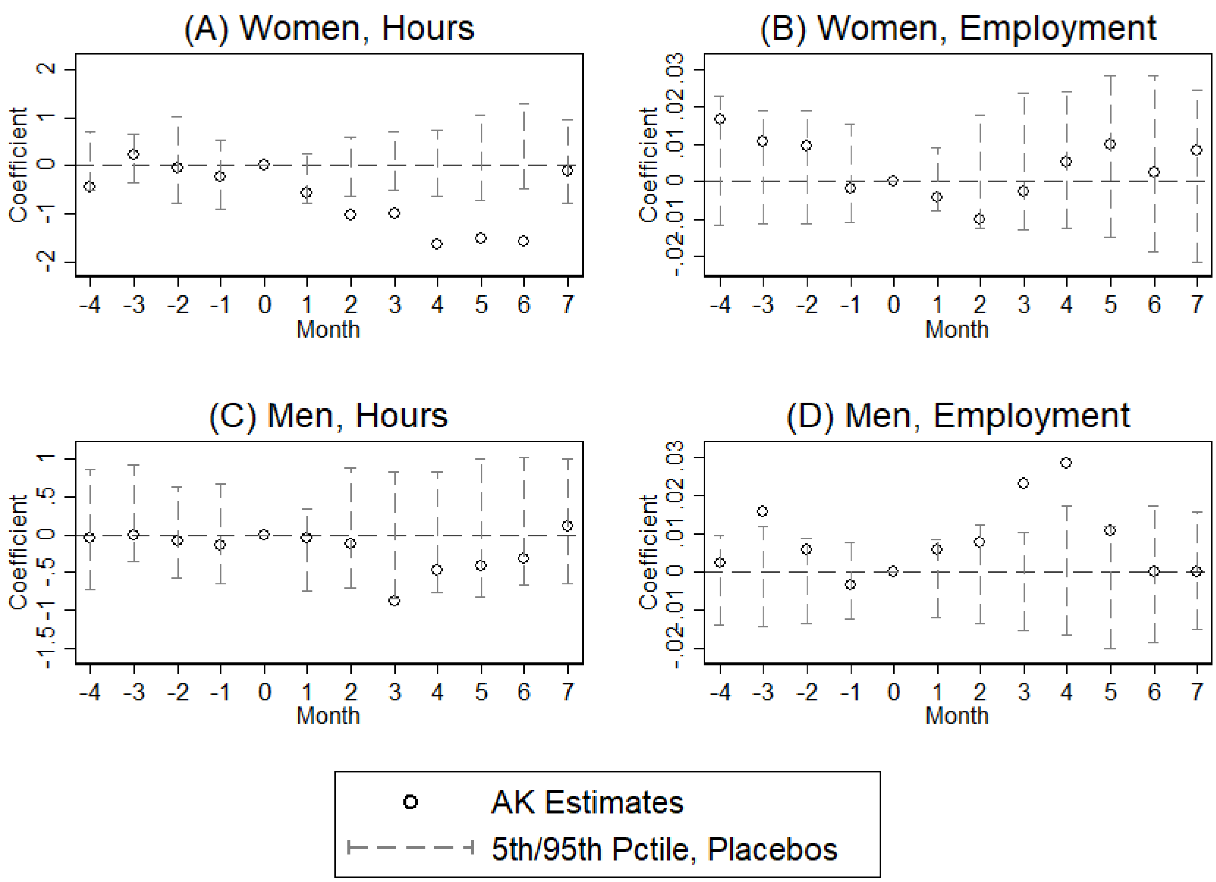
Note: This figure displays month-specific estimates for the effect of an additional $1,000 in the per-person PFD on hours and employment in Alaska, i.e. each dot represents a $\hat{\beta}_m$ from Equation 1, which are the coefficients on size of the PFD, PFD_y, interacted with month dummies. August is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

Table 1: Effect of PFD on Hours and Employment

	Prob(Employed)		*(Hours | Employed)*		*Hours*	
	Men	Women	Men	Women	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Post X PFD(1000s)	0.017**	-0.006	-0.273	-1.265***	0.397	-1.072***
	(0.007)	(0.009)	(0.354)	(0.334)	(0.421)	(0.398)
Observations	79,157	88,501	68,015	64,446	79,157	88,501

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 4: Placebo Tests for Effect of PFD on Hours and Employment



Note: Month-specific estimates for the effect of an additional \$1,000 in the per-person PFD on hours and employment in Alaska. 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

	Panel A: Employment			
	Men		*Women*	
	Full (1)	Part (2)	Full (3)	Part (4)
Post X PFD	0.011 (0.008)	0.006 (0.005)	-0.019* (0.010)	0.012* (0.007)
Observations	79,157	79,157	88,501	88,501

	Panel B: Hours per Week			
	Men		*Women*	
	Full (1)	Part (2)	Full (3)	Part (4)
Post X PFD	-0.060 (0.349)	-1.094 (0.856)	-1.084*** (0.322)	-0.001 (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: Heterogeneity by Age and Children: Men

	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: Heterogeneity by Age and Children: Women

	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.

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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: Sample means and standard deviations. *Married* includes married couples only. Cohabiting couples are excluded from the sample. Comparing high and low PFD years in April - 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: Placebo Tests for the Effect of the PFD on Hours and Employment: Treatment Densities for Untreated 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 every untreated state and D.C. (50 estimates total). 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: Robustness Checks and Alternate Samples

	Prob(Employed)		*(Hours | Employed)*	
	Men	Women	Men	Women
No Income Control	0.017**	-0.005	-0.230	-1.228***
SE	(0.007)	(0.009)	(0.356)	(0.336)
N	79,157	88,501	68,015	64,446
No Unemployment Control	0.016**	-0.005	-0.301	-1.133***
	(0.007)	(0.009)	(0.339)	(0.322)
	79,157	88,501	68,015	64,446
With Cohabiting Couples	0.018***	-0.008	-0.344	-1.184***
	(0.007)	(0.008)	(0.334)	(0.312)
	88,056	97,578	74,988	71,138
With non-citizens	0.014**	-0.010	-0.311	-1.165***
	(0.007)	(0.008)	(0.333)	(0.310)
	86,253	99,336	74,275	72,190
Age Under 20	-0.016	0.004	-2.350	-2.309*
	(0.034)	(0.036)	(1.808)	(1.273)
	4,224	4,139	1,543	1,730
Age Over 55	-0.022	0.005	1.338	0.944
	(0.020)	(0.022)	(0.910)	(0.890)
	20,305	18,366	12,427	10,069

Notes: Robustness checks for the main estimates and estimates for the sample under 20 years old and over 55 years old. Each estimate is from a different regression. The corresponding standard error and number of observations are included below each estimate. *** p<0.01, ** p<0.05, * p<0.10.

Table A4: Estimates Using Control States

Panel A: Most Seasonally Comparable States (ME, MT, SD, WY)						
	Prob(Employed)		*(Hours | Employed)*		*(Hours)*	
	Men	Women	Men	Women	Men	Women
PFD X Post X AK	0.012**	-0.002	-0.245	-0.759***	0.188	-0.642**
	(0.006)	(0.006)	(0.288)	(0.233)	(0.347)	(0.284)
Observations	424,386	463,681	376,849	359,302	424,386	463,681

Panel B: Energy States (CO, KS, LA, MS, MT, ND, NM, OK, TX, UT, WV, WY)						
	Prob(Employed)		*(Hours | Employed)*		*(Hours)*	
	Men	Women	Men	Women	Men	Women
PFD X Post X AK	0.011**	-0.005	-0.249	-0.800***	0.158	-0.759***
	(0.005)	(0.006)	(0.273)	(0.220)	(0.328)	(0.267)
Observations	1,211,198	1,378,153	1,073,264	1,010,981	1,378,153	1,211,198

Notes: Main estimates using control states. Each estimates is from a regression of the outcome on the same set of controls used in the analysis plus a *Post X AK* indicator, a *PFD X Post X AK* indicator, and state fixed effects. Panel A includes the set of most seasonally comparable states, and the Panel B includes the set of energy states. *** p<0.01, ** p<0.05, * p<0.10.