The Labor Market Impacts of Universal and Permanent Cash Transfers: Evidence from the Alaska Permanent Fund[†]

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Abstract

How would universal and permanent cash transfers affect the labor market? Since 1982, all Alaskan residents have received a yearly cash dividend from the Alaska Permanent Fund. Using data from the Current Population Survey and a synthetic control method, we show that the dividend had no effect on employment, and increased part-time work by 1.8 percentage points (17%). We calibrate expected micro and macro effects of the cash transfer using prior literature, and find our results to be consistent with cash stimulating the local economy — a general equilibrium effect. We further show that non-tradable sectors have a more positive employment response than tradable sectors. Overall, our results suggest that a universal and permanent cash transfer does not significantly decrease aggregate employment.

Keywords: unconditional cash transfers, labor supply, employment, fiscal multipliers.

JEL: H24, I38, J21, J22.

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1 Introduction

The effect of cash transfers on labor market outcomes is of central interest in a number of areas, including the design of tax policy, means-tested transfers, and public pension programs. One key concern is that cash transfers could discourage work through an income effect. A number of studies based on the Negative Income Tax experiments of the 1970s (Robins, 1985; Price and Song, 2016) and evidence from lottery winners (Imbens et al., 2001; Cesarini et al., 2017) reliably estimate an income effect of approximately -0.1 in developed countries, implying that a 10 percent increase in unearned income will reduce earned income by about 1 percent (see Marinescu, 2018, for an overview). In contrast, a study of the Eastern Band of Cherokee Indians, who receive an unconditional transfer from casino profits, found no labor supply effect (Akee et al., 2010). While lottery studies leverage ideal exogeneity and the case study of the Eastern Band of Cherokee Indians involved a permanent dividend, these transfers accrue to small shares of the total population and therefore identify a micro effect. Although the NIT experiments included a treatment group comprised of an entire municipality, the experiments generally lasted only three to five years. A universal and unconditional cash transfer will affect the labor market equilibrium and likely alter long-term expectations, yet little is known about the long-run, macro impact of this policy.

To analyze the long-run impact of a universal and unconditional cash transfer on the labor market, we examine the case of the Alaska Permanent Fund Dividend. The fund, worth \$65 billion as of June, 2018, is a diversified portfolio of invested oil reserve royalties.[1] Since 1982, all Alaskan residents of any age are entitled to a yearly dividend payment from the Alaska Permanent Fund; in recent years, the payment is about \$2,000 per person. The dividend only requires that a recipient reside in Alaska for at least a year. Relative to prior studies, ours features a cash transfer that is universal, unconditional, and permanent.

In our setting, everyone within the same state receives a transfer, leaving no natural within-state control group. Furthermore, the universality of the transfer may have macro-

[1] http://www.apfc.org/

level effects on the economy and labor market. We therefore need to consider the entire state as the unit of observation. Estimating the effect of a policy change in one particular state, Alaska, presents us with the methodological challenge of constructing an appropriate counterfactual. We rely on the synthetic control method proposed in Abadie and Gardeazabal (2003) and Abadie et al. (2010), using data from Current Population Survey. The synthetic control method chooses a weighted average of control states to best match Alaska for the outcome of interest and other observable characteristics before the dividend payments begin. This method therefore combines elements of matching and difference-in-differences (DD) estimators, and allows us to measure labor market outcomes in Alaska relative to matched controls after the beginning of the Alaska Permanent Fund dividend payments. We employ permutation methods to assess the statistical likelihood of our results given our sample.

As with all methods, our synthetic control has strengths and weaknesses, and, in particular, relies on our ability to construct a credible counterfactual for Alaska. Our primary analysis, therefore, focuses on two outcomes for which well-matched synthetic controls could be constructed: the employment to population ratio and the population share working part time. For these two outcomes, better controls could be found for Alaska than for at least 68% of other states. In our preferred specification, we do not detect any effect of the Alaska Permanent Fund dividend on employment, i.e. the extensive margin. We do, however, estimate a positive increase of 1.8 percentage points, or 17 percent, in the share of all Alaskans who work in part-time jobs. Analysis of secondary outcomes, i.e. labor force participation and hours worked, are qualitatively consistent with and confirm our primary results.

Our preferred interpretation of the empirical patterns we observe is that the null employment effect could be explained a by positive general equilibrium response offsetting a negative income effect. The unconditional cash transfer results in consumption increases that stimulate labor demand and could mitigate potential reductions in employment. While we do not directly test this channel, we do show indirect evidence for this general equilibrium effect in two ways: first, we compare our empirical employment effect to the expected micro and macro effects of the Alaska Permanent Fund dividend based on estimates from prior literature, and second we compare the impact of the cash transfer on the tradable and non-tradable sectors.

First, if the dividend only operated through the income effect estimated in Cesarini et al. (2017), the dividend should reduce the employment to population ratio in Alaska by about 0.6 percentage points. On the other hand, given estimates of the response of state-level employment to local wealth shocks (Chodorow-Reich et al., 2019) and federal spending (Chodorow-Reich, 2019), the spending of the dividend should increase employment by 0.5 percentage points. Our point estimates range between a 0.1 percentage point increase in employment for our main specification to about 2.8 percentage point in a few alternative specifications. Our point estimates are therefore larger than what is predicted by the income effect alone and suggest a multiplier effect similar to what was measured in prior studies of federal stimulus spending. Overall, what is clear is that the estimated macro effects of an unconditional cash transfer on the labor market are inconsistent with large aggregate reductions in employment, though there may be intensive margin reductions.

Second, the impact on labor demand should be especially pronounced in the non-tradable sector. We show suggestive evidence consistent with this hypothesis — the estimated effects of the dividend on both employment and part-time work are sizeable in the tradable sector and suggest a reduction in labor, but are close to zero in the non-tradable sector. These estimates are only suggestive, but are consistent with a macro feedback effect on employment.

An alternative interpretation of our extensive margin results is that the size of the average Alaska Permanent Fund dividend is too small to affect labor supply on the extensive margin. However, it should be noted that the dividend is paid on a *per person* basis — the average family receives about $3,900, or, in present value terms, about $119,000 over one's lifetime. By comparison, in the lottery study by Cesarini et al. (2017), 90% of winners received a one-time payment of $1,400 or less. The transfer is thus larger than most of the transfers received in Cesarini et al. (2017). In addition, Cesarini et al. (2017) do not find strong evidence of nonlinearities in the income effect, which suggests that our evidence might be relevant for cash transfers of a larger magnitude.

With respect to our findings on the rate of part-time employment, the results suggest that there is a reduction in labor supply on the intensive margin. However, our confidence intervals for the extensive margin of labor supply do not rule out positive employment effects, and a number of our alternative specifications find significantly positive extensive margin responses. We therefore cannot rule out the possibility that the increase in part-time work represents workers moving into the labor force on a part-time basis.

Our work makes three key contributions to the literature. First, we analyze the impact of a universal, unconditional cash transfer, which allows us to estimate the macro effect of the policy on the labor market. The fact that we do not detect significant employment reductions suggests that the policy could have general equilibrium effects that offset the income effect of a cash transfer. Second, the Alaskan policy is permanent and we are therefore in a position to estimate the long-run labor market response to such a policy. Finally, while previously studies have focused on the intertemporal consumption response to the Alaska Permanent Fund (Hsieh, 2003; Kueng, 2018), ours is the first, to our knowledge, to examine the macro labor market impacts of this policy. In a recent study, Feinberg and Kuehn (2018) estimate hours responses to Permanent Fund Dividend, using year-to-year fluctuation and variation by family size. In contrast to our results, they find negative income effects. However, their research design, which either compares Alaskans to other Alaskans or controls for state-level fixed effects, does not capture macro-effects of the policy, and is more akin to prior studies that estimate micro elasticities.

In addition to the literature on income effects and labor supply mentioned above, our work is relevant to a number of other areas of research. In the public finance and optimal income tax literature, an unconditional cash transfer can essentially be thought of as a demogrant, e.g. the intercept of an NIT schedule. Although a trade-off between redistribution and labor supply disincentives is considered, the standard Mirrlees (1971) model does not take into account the potential general equilibrium effects of cash transfers. Kroft et al. (2015) show that, in a model with unemployment and endogenous wages, the optimal tax formula resembles an NIT more than an Earned Income Tax Credit when the macro effect of taxes on employment is smaller that the micro effect. Our empirical results are consistent with this setting. Finally, Cunha et al. (Forthcoming) provide evidence that cash transfers result in a outward shift in demand for local goods, which is consistent with our preferred interpretation of our results.

An unconditional cash transfer may share properties with means-tested transfers, and thus our results are related to studies on the labor supply effects of these programs. Recent studies of the labor supply effects of Medicaid have varied widely depending on the state under consideration (see Buchmueller et al., 2016, for a review). The Earned Income Tax Credit (EITC) has generally been found to produce large, positive extensive margin labor supply responses, and a likely small or negligible intensive margin response (see Nichols and Rothstein, 2016, for further discussion). Welfare reform is typically shown to reduce take-up of Temporary Assistance for Needy Families (TANF) and increase employment and earnings, while reducing total income, taking into account lower benefits (Ziliak, 2016). Recent studies have found large income effects in the specific setting of the Supplemental Security Income Program (SSI) and Social Security Disability Insurance (SSDI) (Deshpande, 2016; Gelber et al., 2017). Finally, our work is related to the literature on unconditional cash transfers in developing countries. A review by Banerjee et al. (2015) concludes that these cash transfers do not affect labor supply in developing countries. In many cases, though not all, these analyses rely on a framework that focuses on labor supply responses, while our results suggest that general equilibrium factors may matter.

From a policy perspective, our results are relevant to understanding the potential labor market impacts of a universal basic income, an unconditional and universal cash transfer.

For example, Hillary Clinton considered a universal basic income modeled after the Alaska Permanent Fund — which we study here — as part of her 2016 presidential campaign proposals.[2]. The Democratic primary for the 2020 presidential election in the United States includes a candidate – Andrew Yang – who made a universal basic income his key campaign proposal.

The paper is organized as follows. Section 2 describes the institutional context for the Alaska Permanent Fund. In section 3, we discuss the synthetic control method, and then describe our data in section 4. We present the main results in section 5. We provide additional results and a discussion in section 6. Section 7 concludes.

2 Policy background: The Alaska Permanent Fund Dividend

During the 1970s, when the production and sale of oil from Alaska's North Slope region began in earnest, the state experienced a massive influx of revenue. However, concerns arose after a large windfall of nearly $900 million was quickly spent down by state legislators (see O'Brien and Olson, 1990, for a history of the fund). Furthermore, residents worried that a heavy reliance on oil revenue during a boom would lead to undesirable shortfalls during slowdowns in production. In response, voters established the Alaska Permanent Fund. The general design of the fund is laid out in an amendment to the state constitution:

> At least twenty-five percent of all mineral lease rentals, royalties, royalty sale proceeds, federal mineral revenue sharing payments and bonuses received by the State shall be placed in a permanent fund, the principal of which shall be used only for those income-producing investments specifically designated by law as eligible for permanent fund investments. All income from the permanent fund shall

[2] https://www.vox.com/policy-and-politics/2017/9/12/16296532/hillary-clinton-universal-basic-income-alaska-for-america-peter-barnes

be deposited in the general fund unless otherwise provided by law. (Amendment to Alaska Constitution, Article IX, Section 15)

The purpose of the fund was to diversify Alaska's revenue streams by investing a portion of royalties more broadly; to ensure that current revenue was in part preserved for future residents; and to constrain discretionary spending by state government officials (O'Brien and Olson, 1990). The fund is managed by the Alaska Permanent Fund Corporation, and the current value of the fund as of June 2018 is $64.9 billion.[3]

Since 1982, a portion of the returns to the fund have been distributed to residents of Alaska in the form of the Alaska Permanent Fund Dividend. The dividend is approximately 10 percent of the average returns to the fund during the last 5 years, spread out evenly among the current year's applicants. The fund is invested in a diversified manner across public and private assets, and is designed to generate long-term risk-adjusted returns. Moreover, oil revenues as a share of the total value of the fund have decreased from 12.2 percent in 1982 to 0.6 percent in 2016 (Kueng, 2018). For these reasons, the level of dividend payments in a given year are generally independent of the local Alaskan economy and contemporary oil production and revenue.

The nominal value of the dividend was as low as $331 in 1984, but has generally exceeded $1,000 since 1996, and peaked in 2015 at $2,072 (see Figure 1 for yearly nominal and real amounts of the dividend).[4] In order to qualify for a payment, a resident must have lived in Alaska for at least 12 months. There are some exceptions to eligibility. For example, people who were incarcerated during the prior year as a result of a felony conviction are not eligible. On the other hand, non-citizens who are permanent residents or refugees are eligible. Therefore, the payment is essentially universal, with each adult and child receiving a separate payment, generally around October of the year via direct deposit.

A representative survey of Alaskans conducted in March and April of 2017 (Harstad, 2017) shows that the dividends are popular and significant to Alaskan residents. For example,

[3] http://www.apfc.org/

[4] https://pfd.alaska.gov/Division-Info/Summary-of-Applications-and-Payments

40 percent of respondents say the yearly dividends have made a great deal or quite a bit of difference in their lives over the past five years, while only 20 percent say it has made no difference. Interestingly, Alaskans were also asked about how the dividend affects work incentives and willingness to work: 55 percent report no effect, 21 percent a positive effect, and 16 percent a negative effect. Thus, the majority of Alaskans report that the dividend has little to no effect on work.

A key feature of our policy setting is that nearly all residents of Alaska receive the dividend. We therefore do not have a natural control group within the state itself. In the next section, we outline an empirical method that allows to treat the entire state as a treated unit, by constructing a counterfactual for Alaska using a weighted average of other states.

3 Empirical method

We aim to compare the evolution of labor market outcomes in Alaska after the introduction of the dividend payments to a set of control states that proxy for the counterfactual outcomes in the absence of the Alaska Permanent Fund dividend payments. Relative to typical Difference-in-Differences (DD) approaches, which feature multiple treatment units, we are faced with the challenge of constructing a counterfactual for exactly one state, which complicates the selection of a suitable set of control states as well as statistical inference. We therefore adopt the synthetic control method of Abadie et al. (2010), which features a data-driven method for choosing a weighted average of potential control states as a comparison for a treated unit. We direct readers to that text for a detailed explanation of the method and briefly outline the method here.

Suppose we have a panel of $S + 1$ states, indexed by s and observed for T periods. There is one treatment state with $s = 0$, while all other states are controls. The variable d_{st} indicates whether a state s is receiving treatment in period t and it takes the following values:

$$d_{st} = \begin{cases} 0 & \text{if } s \geq 1 \text{ or } t \leq T_0 \\ 1 & \text{if } s = 0 \text{ and } t > T_0 \end{cases} \tag{1}$$

In other words, all states are untreated during the pre-intervention period, i.e. $t \in \{1, \ldots, T_0\}$, and the treatment state becomes treated starting in period $T_0 + 1$.

We adopt a potential outcomes framework (Rubin, 1974):

$$\begin{aligned} y_{st}(0) &= \delta_t + \theta_t \mathbf{Z}_s + \lambda_t \mu_s + \varepsilon_{st} \\ y_{st}(1) &= \alpha_{st} + y_{st}(0) \end{aligned} \tag{2}$$

where $y_{st}(0)$ is the outcome of interest in the untreated condition and $y_{st}(1)$ is the outcome of interest in the treated condition. The parameter δ_t is a time-varying factor common across states, $\mathbf{Z}_s$ is an observable $(r \times 1)$ vector of covariates (in our case: average pre-period female share, industry shares, age category shares, and educational categories shares), θ_t is a $(1 \times r)$ vector of time-varying coefficients, μ_s is an unobservable $(m \times 1)$ vector of factor loadings, and λ_t is a $(1 \times m)$ vector of common time-varying factors. The error terms ε_{st} are unobservable, mean zero, state-by-time shocks. Note that the presence of the $\lambda_t \mu_s$ term allows for time-varying and state-specific unobservable factors.

Our parameter of interest is $\alpha_{0t} = y_{0t}(1) - y_{0t}(0)$ for $t \in \{T_0 + 1, \ldots, T\}$, i.e. the effect of treatment for the treated state in the post-intervention period. However, for each state and time period, we only observe $y_{st} = d_{st} y_{st}(1) + (1 - d_{st}) y_{st}(0)$. In particular, we do not observe the counterfactual outcome for the treated state, $y_{0t}(0)$, during periods $t \in \{T_0 + 1, \ldots, T\}$.

We therefore seek a set of S weights, $\mathbf{w} = (w_1, \ldots, w_S)$, in order to combine the untreated outcomes among control states and provide a reasonable approximation for the counterfactual. Following Abadie et al. (2010) we choose the set of weights that solve the following:

$$\mathbf{w}^*(V) = \arg\min_{\mathbf{w}} \left(\mathbf{X}_0 - \sum_{s=1}^{S} w_s \cdot \mathbf{X}_s\right)' \mathbf{V} \left(\mathbf{X}_0 - \sum_{s=1}^{S} w_s \cdot \mathbf{X}_s\right) \tag{3}$$

where $\mathbf{X}_s$ $(K \times 1)$ is a vector consisting of some or all of the elements of $(\mathbf{Z}_s', y_{s1}, \ldots, y_{sT_0})'$, and $\mathbf{V}$ is a positive definite and diagonal $K \times K$ matrix. In our application, the matching vector $\mathbf{X}_s$ is comprised of a set of variables $\mathbf{Z}_s$ realized in the pre-intervention period and the average outcome over the pre-intervention period, $\bar{y}_s^p = \frac{1}{T_0}\sum_{t=1}^{T_0} y_{st}$.[5] Through an iterative process, the matrix $\mathbf{V}$ is chosen as follows:

$$\mathbf{V}^* = \arg\min_{V} \frac{1}{T_0} \sum_{t=1}^{T_0} \left(y_{0t} - \sum_{s=1}^{S} w_s^*(V) \cdot y_{st}\right)^2 \tag{4}$$

We additionally constrain the weights so that $\sum w_s = 1$ and $w_s \geq 0$ for al $s \in \{1, \ldots, S\}$. Once we have arrived at a set of weights, our estimator for α_{0t} is:

$$\hat{\alpha}_{0t} = y_{0t} - \sum_{s=1}^{S} w_s^*(\mathbf{V}^*) \cdot y_{st} \tag{5}$$

for $t \in \{T_0 + 1, \ldots, T\}$.[6] In practice, we report the average difference between the treatment unit and the synthetic control during the period where the dividend is in place in Alaska (the treatment period):

$$\hat{\alpha}_0 = \frac{1}{T - T_0} \sum_{t=T_0+1}^{T} \hat{\alpha}_{0t} \tag{6}$$

For comparison to other methods, we can frame the synthetic control method as a member of a family of more widely used estimators. Doudchenko and Imbens (2016) present the following general model for the counterfactual outcome for the treated unit in period t:

[5]Our results are largely unchanged if we instead use the last realized outcome in the pre-intervention period y_{s,T_0} instead. Note, as demonstrated by Kaul et al. (2015), if the outcomes for each pre-intervention period are used to estimate the weights, the iterative process mechanically sets the elements of V that correspond to $\mathbf{Z}_s$ to zero, and thus, these additional covariates cease to inform the procedure.

[6]The synthetic control estimator can be easily implemented by using the "synth" package in either MATLAB, Stata, or R.

$$\hat{y}_{0t}(0) = \mu + \sum_{s=1}^{S} w_s \cdot y_{st} \tag{7}$$

They note that the synthetic control method can be thought of as imposing a set of constraints on (7): namely, $\mu = 0$, $\sum_s w_s = 1$, and $w_s \geq 0$. Relative to the synthetic control method, a DD estimator relaxes the constraint that $\mu = 0$, while imposing a constraint that $w_s = \bar{w} = 1/S$. On the other hand, many matching estimators relax the constraint that $w_s \geq 0$, while imposing "perfect balance." That is, $\mathbf{X}_0 = \mathbf{X}_c W^{Match}$, where W^{Match} is an $(S \times 1)$ vector of the weights w_s and $\mathbf{X}_c = (\mathbf{X}_1, \ldots, \mathbf{X}_S)'$ is the $(K \times S)$ matrix of matching vectors for control states. Finally, Abadie et al. (2015) show that OLS regression similarly relaxes $w_s \geq 0$, while imposing perfect balance, with $W^{OLS} = \mathbf{X}_c' (\mathbf{X}_c \mathbf{X}_c')^{-1} \mathbf{X}_0$. Thus, the various methods can all be framed as using weighted averages of control states, with constant weights in the case of the DD, and possibly negative weights, i.e. extrapolation, in the case of matching or OLS.

Although the synthetic control method avoids extrapolation, the constraint that $w_s \geq 0$ means that the estimator is not guaranteed to deliver a great fit for the treated unit. This depends on whether or not $\mathbf{X}_0$ lies within the convex hull of the $\mathbf{X}_s$ vectors of the control states. In that respect, we do have to subjectively evaluate whether or not the pre-intervention fit is sufficiently close. Following Abadie et al. (2010) we estimate the root-mean-square error (RMSE) for pre-intervention outcomes, i.e. the square root of (4), for our main estimate and for each of our placebo estimates. We then rank the fit across all placebos and adopt the conservative approach of focusing on outcomes where the fit for Alaska using the true treatment period as a low rank. For example, the fit for our two primary outcomes, employment and part-time work, is at or below the 32nd percentile in our main specification.

To quantify the significance of our estimates, we implement a permutation method suggested by Abadie et al. (2010), comparing our synthetic control estimate to a distribution of placebo estimates. That is, we implement the above synthetic control procedure for all 50 states and the District of Columbia, and repeat this exercise as if the treatment year occurred in each of our observed time periods. In our setting, we use "placebo" treatment years between 1978 and 2013, and for each placebo treatment year, we find synthetic controls for the treated state based on 5 years of data prior to treatment (or the maximum number of available pre treatment years, if this is less than 5 years). We define $\hat{\alpha}_{st}$ as the estimate for state s with placebo treatment year t. We then conduct a two-tailed test of the null hypothesis of no effect in our treatment state by comparing the observed estimate for $s = 0$ and true treatment year, $t = 1982$, to the empirical distribution of placebo estimates. Specifically, our "p-value" is defined as follows:

$$p_0 = \frac{\sum_s \sum_t \mathbf{1}\left\{|\hat{\alpha}_{0,1982}| \leq |\hat{\alpha}_{st}|\right\}}{N_{st}} \tag{8}$$

where N_{st} is the total number of placebo estimates. The statistic p_0 therefore measures the share of the placebo effects that are larger in absolute value than that of Alaska. If treatment status is randomly assigned, this procedure comprises randomization inference (Abadie et al., 2015). Although randomization is unlikely to describe the data generating process in our setting, we nonetheless implement the permutation method, in the spirit of Bertrand et al. (2002).[7]

We additionally calculate confidence intervals by inverting our permutation test (e.g. Imbens and Rubin, 2015). For a given null hypothesis effect of α^* we transform the data as follows:

$$y^*_{st} = \begin{cases} y_{st} & \text{for } s \neq 0 \text{ or } t \leq T_0 \\ y_{st} - \alpha^* & \text{for } s = 0 \text{ and } t > T_0 \end{cases} \tag{9}$$

Using this transformed data, we recalculate a p-value using equations (5), (6) and (8): p_{0,α^*}. Our 95% confidence interval is then defined as the set $\{\alpha^* \,|\, p_{0,\alpha^*} > 0.05\}$, i.e. the set of null effects we cannot reject given the data.

The synthetic control method has a number of attractive features in our empirical setting. First, the selection of the control states is carried out using a data-driven process.

[7] We do not cite the published version here, since randomization inference is only featured in the working paper.

In a setting such as ours, where the treatment unit does not have a natural set of comparison states, it is useful to have a process that minimizes the extent to which researcher degrees of freedom confound the analysis. Second, the restrictions on the optimal set of weights renders our "synthetic Alaska" time series immediately interpretable as a weighted average of other states. The reader can easily determine which states are contributing most to the estimates. Moreover, the method provides for transparent visual inspection of the goodness of matching in the pre-period. Third, the synthetic control method uses a framework similar to the DD approach, but is potentially robust to relaxing the parallel trends assumptions. Abadie et al. (2010) note that this is most likely the case when a relatively long pre-period is used for matching. Finally, the method naturally implies a set of placebo exercises to determine whether any significant effects are simply artifacts of the methodology.

4 Data

We analyze data drawn from the monthly Current Population Surveys (CPS). Every household that enters the CPS is surveyed each month for 4 months, then ignored for 8 months, then surveyed again for 4 more months. Labor force and demographic questions, known as the "basic monthly survey," are asked every month. Usual weekly hours questions are asked only of households in their fourth and eighth month of the survey. Because the Permanent Fund Dividend was initiated in June 1982, we aggregate the data into years defined as twelve month intervals beginning in July and ending in June. We restrict our analysis to data for those who are 16 years old or above and collapse the data using survey weights, to create annual averages for the 50 states and the District of Columbia.

We use data on active labor force, employment status, and part-time employment status from the monthly CPS surveys. Specifically, we use the Integrated Public Use Microdata Series (IPUMS) CPS (Flood et al., 2015) provided by the Minnesota Population Center for the analysis of employment outcomes. We do not have data for the state of Alaska for the months of February, March, April, July, September, and November of 1977. Therefore, we eliminate these months from all states in 1977. Although IPUMS-CPS is available from 1962 onward, separate data for Alaska is only available from 1977 onward. Using data between July 1977 and June 2015 results in a total of 48,686,169 observations.

For the analysis of hours worked, we use the CPS Merged Outgoing Rotation Groups (MORG) provided by the National Bureau of Economic Research (NBER). Specifically, we use reported hours worked last week at all jobs. These data are only available beginning in 1979. Focusing only on employed respondents, we obtain a total of 7,206,411 observations between July 1979 and June 2015. This sample size is considerably smaller because it only uses two of the 8 total survey months for each respondent.[8]

We define a set of synthetic control states that collectively best match Alaska in the pre-period based on a number of state characteristics observed during the pre-treatment period (the Z variables in equation (2) above). We calculate the share of population in three educational categories: less than a high school degree, high school degree, and at least some college. We additionally measure the share female and the share of the population in four age groups: age 16 to age 19, age 20 to age 24, age 25 to age 64, and age 65 or older. Finally, we take into account the industrial composition of the workforce using five broad categories of industry codes: (1) agriculture, forestry, fisheries, mining, and construction; (2) manufacturing; (3) transportation, communications, utilities, wholesale, and retail trade; (4) finance, insurance, real estate, business, repair, and personal services; and (5) entertainment and recreation, professional and related services, public administration, and active duty military.

For a subset of specifications, we augment our primary data in order to conduct robustness checks. To assess the sensitivity of our analysis to the number of pre-treatment years used, we merge our CPS data with deccennial Census data from 1970 and 1960. In

[8] CPS MORG also has data on earnings, and it would be interesting to analyze this outcome. However, it is very hard to find a good control group for Alaska in terms of hourly earnings: the pre-period match is at the 98th percentile. For this reason, we cannot have much confidence in results concerning earnings.

this case, we focus on the employment rate, which is most consistently defined across the surveys. Second, we conduct limited analysis of state spending, using data from a harmonized collection of US Census of Government survey data (Pierson et al., 2015). Third, we merge oil production data from State Energy Data System (SEDS)[9], in combination with oil prices series from BP Statistical Review of World Energy,[10] and use oil production to GDP ratios as a matching variable. Finally, we combine intercensal population estimates with natality and mortality measures to further use net migration as a matching variable.

5 Main results

We separately consider two margins of response to the Alaska Permanent Fund Dividend. First, we examine extensive margin outcomes, the employment rate and labor force participation. We then turn to the intensive margin by examining the effect of the PFD on the part-time working rate and hours per week. In each case, we pay special attention to those outcomes for which we are able to achieve a particularly good synthetic match: the employment and part-time rates. Finally, we consider a number of robustness checks and alternative specifications.

5.1 Employment and labor force participation

We begin our analysis with a focus on extensive margin outcomes. In Table 1 we compare Alaska to its synthetic control using variables averaged over the pre-treatment period. We use monthly CPS data from 1977 to 1981 in Panel A and column (1) features actual data for Alaska. In column (2) we present a weighted average of these characteristics using the set of control states selected by our method from Section 3. In particular, the key outcome variable used to construct the $\mathbf{V}$ matrix from equation (4) is the employment rate in each pre-treatment year for column (2), the labor force participation in each pre-treatment year for

[9]https://www.eia.gov/state/seds/seds-data-complete.php
[10]https://www.bp.com/en/global/corporate/energy-economics/statistical-review-of-world-energy.html

column (3), and so forth. Meanwhile, the **X** variables used in equation (3) include age, female share, industry, education and average employment or average labor force participation in the pre-period. We are generally able to match Alaska across these key observables. The combination of states and weights underlying the synthetic Alaska in column (2) are detailed in Panel A of Appendix Table A.10 — the states include Utah, Wyoming, Washington, Nevada, Montana, and Minnesota.[11] It is interesting to see that many of the chosen states are mountainous like Alaska, even though this is not something we explicitly matched on.

Figure 2a plots the employment rate (employment[12] to population) for Alaska and synthetic Alaska from 1977 to 2014. The vertical, dashed line indicates 1981, the last year before the introduction of the Alaska Permanent Fund Dividend. By construction, we see that Alaska and the synthetic control track each other in the pre-period. This pattern generally continues during the post-period — even though we only use five years of data for matching, the two time series continue to line up closely for several decades. In Table 2, column (1) we calculate virtually no difference — 0.001 percentage points — in the average employment rate between Alaska and synthetic Alaska during the post-period. The data suggest that the dividend did not have a meaningful impact on employment in Alaska.

Following the details outlined in Section 3, we conduct a a total of 1,836 placebo synthetic control comparisons, using time periods other than the true onset of treatment and states other than Alaska. Figure 2b plots the difference between each "treatment" state and its synthetic control. The actual treatment state, Alaska, is highlighted in black, while the remaining placebos are plotted in grey. Since each series relies on a different placebo treatment year, we use event time on the x-axis, i.e. time relative to the placebo treatment year. As expected, the mean of the placebo differences is very close to zero — -0.002 — suggesting that the method is not in systematically prone to finding differences. Moreover, the actual treatment difference for employment in Alaska lies squarely inside the range of

[11] For interested readers, the appendix provides synthetic control states and their weights for each of the outcomes and specifications we use.

[12] Employment includes the self-employed, as long as they work for pay.

placebo differences.

Using our placebos, we can assess the analysis in several ways. First, we calculate a measure of synthetic control quality, the root-mean-square error (RMSE) of the difference in each pre-period year between treatment and synthetic control. We then rank this measure for our actual treatment state and year relative to all placebos, and find a relatively high quality match. In Table 2, column (1), the actual treatment ranks within the top 32 percent match of quality when using employment as an outcome. Second, we use the empirical distribution of placebo treatment effects to assess the quantitative significance of our estimate, which we loosely refer to as a p-value. Just over 94 percent of the placebos generate a larger estimate, underscoring our null conclusion. Finally, we construct a confidence interval using a series of placebo exercises under various null hypotheses — the resulting confidence interval in the case of employment contains zero.

We complement our analysis of extensive margin effects by also considering labor force participation as an outcome. We summarize the results for this outcome in Table 2, column (3). In this case, we do not achieve as great a fit in the pre-period as when employment is used at the outcome — the RMSE is in the bottom ten percent of the pre-period fit rankings. Nonetheless, the treatment for labor force participation is similarly indistinguishable from zero. Descriptive statistics during the pre-period for the synthetic Alaska constructed using labor force participation are provided in Table 1, column (3). A graphical depiction of the estimates, as well as a list of synthetic control states and weights are provided in Appendix A, Table A.10, and Figure A.1. In both instances, our analysis suggests a negligible impact of the Alaska Permanent Fund Dividend on extensive margin labor market outcomes.

5.2 Part-time work and hours

We now turn to intensive margin effects of the Permanent Fund Dividend. Table 1, column (4) indicates that in the case of part-time employment, we continue to achieve balance with respect to our set of pre-period observable characteristics. Put more rigorously, our pre-period RMSE for the part-time rate is in the top 25 percent when compared to our placebos. We therefore consider the part-time rate to be on par with the employment rate when it comes to quality of pre-period match. The synthetic Alaska in this case is comprised of mostly Nevada and Wyoming (see Appendix Table A.10).

Figure 3a plots the part-time rate (part-time employment as a share of the population) from 1977 to 2014 for both Alaska and the synthetic Alaska. The two time series track each other well in the pre-period, and there continues to be little difference between the two in the first few treatment years. However, the estimated treatment effect grows over time, and the rate of part-time work in Alaska exceeds that of the synthetic control for the overwhelming majority of the post-period. In Table 2, column (2) we estimate an average increase in the part-time rate of 1.8 percentage points. This represents an increase of 17 percent relative to the average part-time rate in the pre-period. When compared to placebo estimates, this difference has a p-value of 0.020 and the confidence interval allows us to rule out a treatment effect of zero at the 95 percent confidence level. This is visually demonstrated in Figure 3b, where the actual difference in Alaska is generally found near the upper limit of placebo differences.

The increase in part-time employment, in combination with our null result on employment, suggests that some workers moved from full-time to part-time work. However, we cannot rule out that increase in part-time may also be driven by workers moving into the labor force on a part-time basis. First, the confidence intervals on our extensive margin estimates cannot rule out a positive employment response. Second, in a number of our alternative specifications below, our point estimate on employment becomes positive and significant.

As a secondary measure of intensive margin effects, we examine reported hours worked in the prior week for those who are employed. We can only observe this outcome in the CPS MORG data, and thus the data are based on a smaller number of underlying observations and a shorter per-period starting in 1979. In this case, our pre-period fit is not as well ranked — the RMSE is now just within the bottom 25 percent of the placebo rankings. We therefore place relatively less weight on this outcome. Consistent with our results for the part-time rate, we estimate a reduction on intensive margin, albeit less than 1 hour per week. Furthermore, we are not able to rule out a null effect on hours given our confidence intervals. Once again, details on the pre-period match can be found in Panel B of Table 1, and additional figures and synthetic control states and weights are available in Appendix A, Table A.10, and Figure A.2.

6 Additional results and discussion

6.1 Heterogeneity Analysis

In Table 3 we conduct heterogeneity analysis among the men and women, separately by marital status. We remind the reader that each estimate uses a different group of states with different weights for the synthetic control. We again focus on the employment rate and the part-time rate. The estimates suggest that the increase in part-time work among the full population may be driven by adjustments among married women — the treatment effect on part-time for married women is relatively large (3.5 percentage points) and significant ($p = 0.001$), while the estimate for all men is trivial (0.8 percentage points) and insignificant ($p = 0.192$). Among all groups, the extensive margin responses are at best marginally significant. Our results are reminiscent of Kimball and Shapiro (2008), who likewise find relatively larger income effects among married women.

It may be the case that the dividend has a stronger effect among older workers, who are closer to retirement (Price and Song, 2016). In Appendix Table A.2, we compare workers under and over age 55. Splitting the data results in poorer relative pre-period matches, but taken at face value, the results do not imply a particularly more negative labor supply response among the older group.

6.2 Robustness tests

In our main specification, we allow a different set of control states to be chosen, depending on the outcome variable. An alternative approach to constructing our synthetic control involves using a common set of weights across our two main outcomes, the employment to population and part-time to population rates. This is to ensure that differences across outcomes are not simply a result of heterogeneous control states. To that end, we amend the method outlined in Section 3 to jointly estimate a set of weights using both the employment rate and the part-time rate. In Table 4 we present the results of this alternative approach. The relative fit of our match during the pre-period is now at the 31st percentile, which lies just in between the two the RMSE percentile when we consider employment (32nd percentile) and part-time (25th percentile) separately. In this case, we estimate a positive and significant effect of the dividend on the employment rate. On the other hand, our point estimate for the part-time rate is slightly smaller than in our main specification, and becomes just marginally insignificant. Under this specification, the results imply that on net the number of workers in full-time jobs increased, and thus, the increase in part-time work did not occur at the expense of full-time work.

In our Online Appendix, we consider several other robustness checks and alternative specifications. In Appendix Table A.3, we use an "in-space" set of placebos (Abadie et al., 2015), holding the treatment period fixed at 1982. Our conclusions are changed significantly, although this leads to wider confidence intervals given the use of a smaller number of placebos. In Appendix Table A.4, we follow Kaul et al. (2015) and consider only using the outcome in the last pre-period to select synthetic control states. Our results remain very similar in this case. We also test the robustness of our results using a longer pre-period to construct our synthetic control by combining our data with decennial Census data from 1960 and 1970. Though this results in a weaker pre-period match than our main estimates, the results in Appendix Table A.5 now feature a positive employment effect, and thus reinforce our conclusion that the dividend is unlikely to have reduced employment rates.[13]

The long run, average effect of the Permanent Fund Dividend could potentially differ from the immediate effect, for a number of reasons. We therefore report the average difference between Alaska and synthetic Alaska during the first four years of the dividend in Appendix Table A.6. Using only placebos during this time period results in a poorer relative fit in the pre-period for all outcomes[14]. Furthermore, the confidence intervals include zero in all cases, consistent with a negligible impact in the very short run. Focusing on the employment and part-time rates, the effect on employment has a more positive point estimate, while the opposite is true for the part-time rate.

One potential concern is that the unique local economy of Alaska and its dependence on oil production and oil prices may confound our analysis. As stated earlier, any worry in this regard should be limited by the fact that the level of the dividend is mechanically decoupled form fluctuations in yearly oil production, due to the diversified nature of the fund's investments and the 5-year averaging involved in the formula for dividends. Nonetheless, we can add the total value of oil production as a share of state GDP to the list of variables we use to find a synthetic control for Alaska in the pre period.[15] We present those results in Appendix Table A.7. We find a more positive estimate on the employment rate and a part-time effect closer to zero. If anything, we are less likely to conclude any negative impact on the employment rate when using a set of control states that are chosen to better match Alaska's reliance on oil production.

In Appendix B, we explore how sensitive our results are to differential migration that may have coincided with the introduction of the Alaska Permanent Fund Dividend. We implement three potential adjustments for differential migration. We use average net migration

[13]We only conduct this analysis for the employment rate, as the measure for part-time status is inconsistently measured between the Census and the CPS.

[14]The ranking of the pre-period fit differs for this specification, even though we use the same pre-period in our main estimate for Alaska. The reason is that the restriction to a shorter post period results in a different set of placebo estimates to which our main estimate is compared.

[15]James (2016) shows that the cumulative effect of oil discovery on real Alaskan income was positive until about 1985, then negative or null, which would bias us toward finding a negative effect on employment in the later period when not taking account the effects of oil.

and annual net migration in the pre-period as matching variables. Additionally, we use the CPS Annual Social and Economic Supplement (ASEC) to assign respondents to their place of residence in the prior year and focus on outcomes in the short-term, i.e. until 1985. We show in Appendix B that while there is a relative increase in migration to Alaska during the period just prior to 1982, our results for the employment rate and part-time rate are qualitatively similar when we attempt to adjust for migration using these methods.

Finally, we consider a simpler, difference-in-differences (DD) estimate by comparing Alaska to only Washington State. Kueng (2018), for example, finds Washington to provide a suitable control for consumption patterns. We present the results in Appendix Table C.1, and continue to find negligible effects on employment. Under this specification, the effects on part-time work is now much closer to zero as well.

6.3 From micro to macro effects

How do our quantitative results compare to prior empirical evidence on micro and macro effects of transfers? Although theory and prior estimates suggest that the individual level labor supply response to positive income shocks leads to reductions in both the probability of being employed and hours worked, we do not find strong evidence of a decrease on the extensive margin. We reconcile our results with these prior findings by considering the general equilibrium effects of transferring income universally. In the case of Alaska, the consumption response to the dividend could result in an outward shift in labor demand, offsetting the partial equilibrium effects of cash transfers.

In prior lottery studies, the micro-level income effect of a $140K transfer has been estimated to generate a 2 percentage point reduction in employment (Cesarini et al., 2017). Applying this estimate using the average, per-person present value of all future Alaska Permanent Fund Dividend payments ($45,000) implies a 0.6 percentage point decline in the employment to population ratio (Table 5).[16]

[16]We estimate a present value of $45,000 by assuming an annual dividend payment of $1,495, over the

In order to calibrate a macro effect on employment, we must consider several factors: the Alaska Permanent Fund Dividend may not be completely spent by consumers, and our setting is one of a small, open economy, where many goods are purchased from other markets. We draw on two existing estimates of the effect of fiscal stimulus at the state level. First, we can consult the estimates of Chodorow-Reich et al. (2019), who model and estimate the effect of wealth shocks on consumption, and by extension, local labor markets.[17] We draw on two key equations (Chodorow-Reich et al., 2019, Eqs. (9) and (10)), which imply the following relationship between dividend payments and log labor supply:

$$\Delta \log LaborSupply = \frac{1}{1+\kappa}\mathcal{M}\left(1-\alpha\right)\eta \times MPC \times \frac{PF Dividends}{Labor Income}. \tag{10}$$

Here κ is a wage adjustment parameter capturing both sticky wages and the elasticity of labor supply, $\mathcal{M}$ the local Keynesian income multiplier, $(1-\alpha)$ the labor share of income, η the share of non-tradables in spending, and MPC the marginal propensity to consume.

We choose a value of 0.25 for the MPC out of the PFD, following Kueng (2018). Using data from the BLS, we calculate an average ratio of total dividend payments to total labor income of 0.0725. The analysis of Chodorow-Reich (2019) implies a multiplier, $\mathcal{M}$ of 1.8. We choose a value of 0.69 for the home-bias parameter, η, following Nakamura and Steinsson (2014). Based on Chodorow-Reich et al. (2019), we choose values of 1.2 and 0.667 for κ and $(1-\alpha)$, respectively. Finally, we multiply the change in log labor supply from (10) by the average Alaskan employment to population ratio, 0.66, and obtain a macro-driven increase in employment rates of 0.5 percentage points. Our results are summarized in Table 5.

Alternatively, we can calibrate with state-level, government spending multipliers for employment estimated using the American Recovery and Reinvestment Act of 2009 (ARRA) and other similar shocks, as summarized by Chodorow-Reich (2019). Here we must make two adjustments, to reflect the fact that government spending impacts the economy differently

course of the average Alaska lifespan of 79 years, and assuming an interest rate of 3%.

[17] We refer the reader to the original manuscript and appendix for more details on their model.

than a cash transfer to consumers. We account for less than full spending of the transfer and the share of spending spent in the home state. Amending a key equation of (Chodorow-Reich, 2019, p. 15), we have the following relationship between the employment to population ratio ($EPOP$) and the dividend:

$$\Delta EPOP = \eta \times MPC \times \beta \times \frac{PFDividend}{\$100,000}, \tag{11}$$

where η is again a home-bias parameter, β is the number of jobs added per \$100,000 in government spending, MPC is the marginal propensity to consume, $PFDividend$ is now the per-capita dividend, and we scale by \$100,000 given the definition of β. Chodorow-Reich (2019) finds a median value of 1.9 for β. Using the values above for η and MPC, and an average dividend amount of \$1,495 (2010 dollars), we again predict an employment rate increase of 0.5 percentage points. We summarize this in Table 5 as well.

When combined, the predicted micro and macro effects imply a slight decrease in the employment to population ratio -0.001 (= -0.006 + 0.005), which is in line with our main estimate Table 2, but lower than the positive employment effects we estimate in other specifications (e.g. Table 4 and Appendix Table A.5). Overall, our estimates imply a state-level fiscal multiplier on par with or possibly greater than those in the literature. Based on cross-sectional state-level multiplier effects, Chodorow-Reich (2019) concludes in his review that a national closed economy, deficit financed, no monetary response output multiplier would be 1.7 or above. Since our estimates of the employment effects of a stimulus are at or above the levels found in Chodorow-Reich (2019), they imply a slightly higher lower bound on the national output multiplier than 1.7.[18]

To further explore the possibility that our estimates are driven by macro effect, we inspect a related prediction: the macro employment effect should be concentrated in the

[18] Although our primary estimate are in line with multipliers from the literature, we might expect, all things equal, to see smaller macro effects in Alaska because the policy is not counter-cyclical (Aghion and Marinescu, 2007). The effects in Di Maggio and Kermani (2016) and Chodorow-Reich (2019) may be expected to be larger because they were estimated during a period of economic slack when stimulus is more effective.

non-tradable sector. Di Maggio and Kermani (2016) show evidence for this channel by exploiting the increase in unemployment insurance transfers during the Great Recession, and Chodorow-Reich et al. (2019) show evidence of this using the consumption response to regional wealth shocks.

We indirectly test for the plausibility of this demand channel by re-estimating the impact of the dividend on employment and part-time status separately for industries in the tradable and the non-tradable sector.[19] The results are presented in Appendix Table A.8. While the pre-period match is relatively poor, we find reductions in the employment rate and increases in the part-time rate only among the tradable sectors. Meanwhile, the non-tradable sector exhibits essentially no impact. This result, albeit suggestive, is consistent with an increase in consumption of non-tradable goods contributing to a positive labor demand effect, offsetting any negative labor supply effects of the cash transfer in the non-tradable sector.

An alternative interpretation of our results is that the size of the Alaska Permanent Fund dividend is too small to generate significant changes in labor supply. However, since the dividend is paid on a per person basis, the average household receives about \$3,900 per year. These amounts may still be smaller than what would be expected under a universal basis income policy. However, Cesarini et al. (2017) found little evidence of nonlinearities in income effects, and, thus, our estimates may still speak to the potential impacts of a full-scale universal basic income. Moreover, the present value of these transfers at the household level are about \$120,000, and therefore are larger than a majority of the lottery winnings in Cesarini et al. (2017). When considered in that light, our null employment effects may be considered a meaningful departure from individual-level income effect estimates, potentially driven by marco feedback factors.

A final consideration involves the financing of a universal basic income. In order

[19] We use the same definitions of tradable and non-tradable sectors as Di Maggio and Kermani (2016), which are themselves taken from Mian and Sufi (2014). We include construction in the non-tradable sector. A full list of the industries can be found in Appendix Table 1 of the Online Appendix of Mian and Sufi (2014)

to provide these transfers, governments must ultimately raise taxes or reduce other types of spending. The impact of a universal basic income will thus depend on the method of financing. While the Alaska Permanent Fund Dividend is not explicitly financed by taxes, it is also not entirely a "helicopter drop" of money: the dividend was introduced in 1982, but the discovery of the underlying reserves had already been established earlier in the 1970s. Therefore, there are potentially other types of spending that were forfeited when the fund was committed to dividends.

To get a sense of these counterfactual spending patterns, we repeat our synthetic control analysis, using as an outcome the share of government spending in four key areas: health and hospitals, education, highways, and welfare and transfer spending. We report these results in Table A.9. With these data, our pre-period fit is less than ideal, and thus the evidence is at best suggestive. We find no significant difference in health and hospital spending, a potential decrease in educational spending, and smaller increase in highway spending. Importantly, we do not find a significant change in welfare and transfer spending, which is most likely to confound our analysis of the labor market. The lack of an effect of the dividends on welfare and transfer spending also alleviates the concern that the dividends crowded out other forms of redistribution.

6.4 Implications for a universal basic income

Recently, the notion of a universal basic income, i.e. an unconditional cash transfer that is given to all, has generated renewed interest both in the US and around the world. Besides Hillary Clinton and Andrew Yang, whom we mentioned in the introduction, former president Barack Obama argued that the combination of advances in artificial intelligence, substitution away from labor-intensive technology, and rising wealth call for a new social compact; and he sees a universal basic income as something worth debating in this context.[20]. In France, mainstream left presidential candidate Benoît Hamon included a universal basic income as a

[20] https://www.wired.com/2016/10/president-obama-mit-joi-ito-interview/

key proposal of his electoral program in 2017. Finally, Finland[21], the Canadian province of Ontario[22], and the city of Stockton, California[23] have been running basic income experiments for various subset populations.

Our study speaks most closely to the likely labor market impacts of a small universal basic income. Most universal basic income proposals, however, involve amounts significantly higher than the Permanent Fund dividend. For example, 2020 Democratic primary candidate Andrew Yang proposes $1,000 a month. The effect of a larger sum of money on the labor market is therefore uncertain. On the one hand, with larger transfers, the income effect may lead to larger decreases in labor supply. According to the results for lottery winners in Cesarini et al. (2017), the income effect is linear in the amount of the prize. On the other hand, to the extent that cash transfers create jobs through an aggregate demand effect, a larger transfer would also produce a countervailing positive effect on employment. Egger et al. (2019) use a randomized controlled trial to show that an unconditional cash transfer equal to 15% of local GDP leads to a local fiscal multiplier of 2.6 in Kenyan villages, with no decrease in employment. Where exactly this effect would fall in the US is still an open question for future research.

7 Conclusion

In this paper, we have investigated the impact of an unconditional and universal cash transfer on the labor market. We analyze the case of the Alaska Permanent Fund dividend, introduced in 1982 and still ongoing — this is a unique setting to learn about potential effects of a universal basic income. The employment to population ratio in Alaska after the introduction of the dividend is similar to that of synthetic control states. On the other hand, the share of people employed part-time in the overall population increases by 1.8 percentage points after

[21] https://www.theguardian.com/society/2017/feb/19/basic-income-finland-low-wages-fewer-jobs
[22] https://www.ontario.ca/page/ontario-basic-income-pilot
[23] https://www.stocktondemonstration.org/

the introduction of the dividend and relative to the synthetic controls. The unconditional cash transfer thus has no significant effect on employment, yet increases part-time work.

Given prior findings on the magnitude of the income effect, it is somewhat surprising for an unconditional cash transfer not to decrease employment. General equilibrium effects could explain why we do not find a negative effect on employment. Indeed, in our unique setting, the whole population in the state receives the dividend. Therefore, it is plausible that the dividend increases labor demand through its effects on consumption. And indeed, when we calibrate the expected micro and macro effects of the transfer, our empirical estimates are generally in line with prior studies. In addition, we find suggestive evidence that the non-tradable sector shows more favorable effects than the tradable sector. In the tradable sector, employment decreases and part-time work increases, while in the non-tradable sector the effects on both employment and part-time work are close to zero and insignificant. Overall, we find indirect evidence of positive macro effects offsetting negative micro effects, and leading to an overall null effect of an unconditional cash transfer on aggregate employment, at least on the extensive margin.

In a world where trade, technology, and secular stagnation threaten people's incomes, there is growing interest in a universal basic income to promote income security. Our study of Alaska contributes to our understanding of the likely impacts of a small universal basic income on the labor market. Our results show that adverse labor market effects are limited, and, importantly, a small universal and unconditional cash transfer does not significantly reduce aggregate employment. Future research might investigate how the mode of financing of a universal basic income affects its impact, how the transfer may affect prices of consumer goods, how a universal basic income interacts with existing social welfare programs, how these effects might scale with a significantly larger transfer.

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Figure 1: Alaska Permanent Fund Dividend: nominal and real amounts

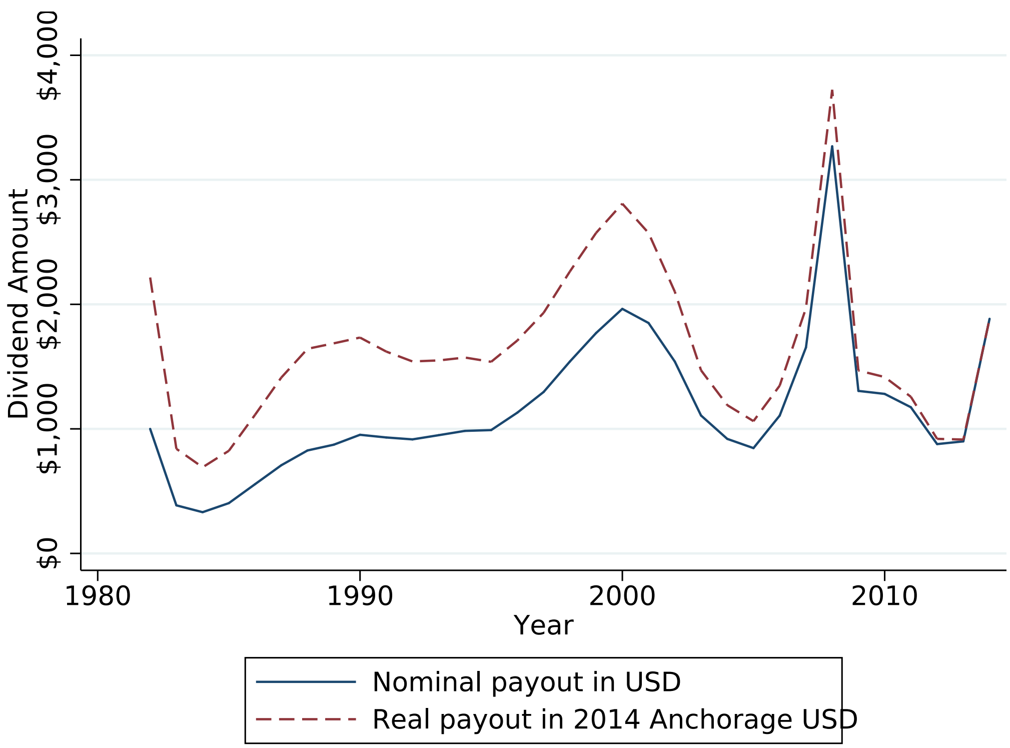
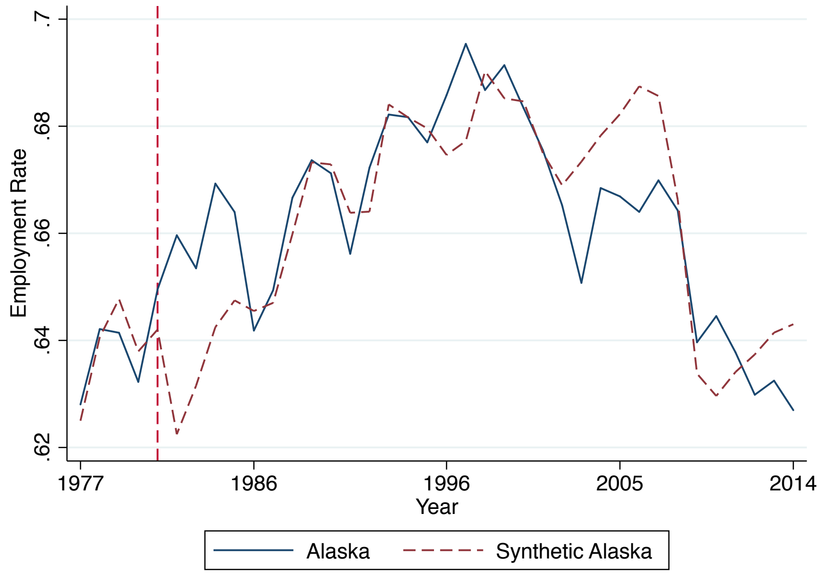
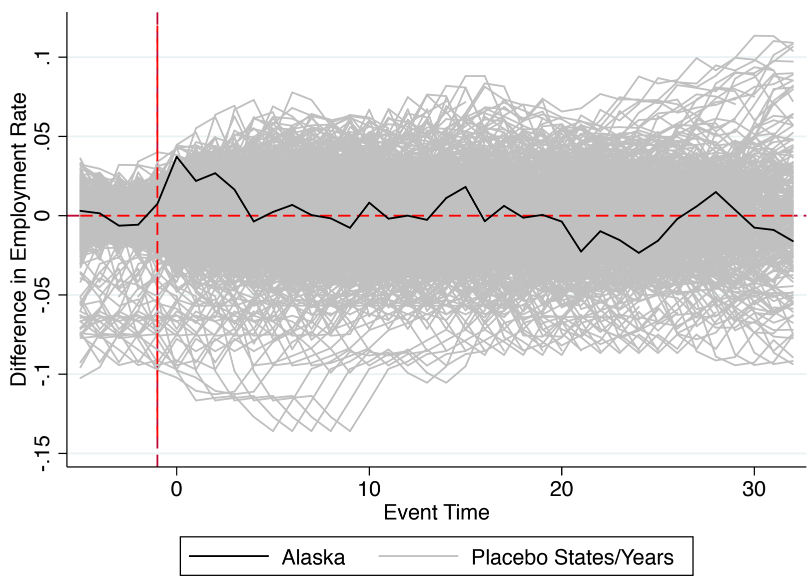


Figure 2: Employment Rate, 1977-2014



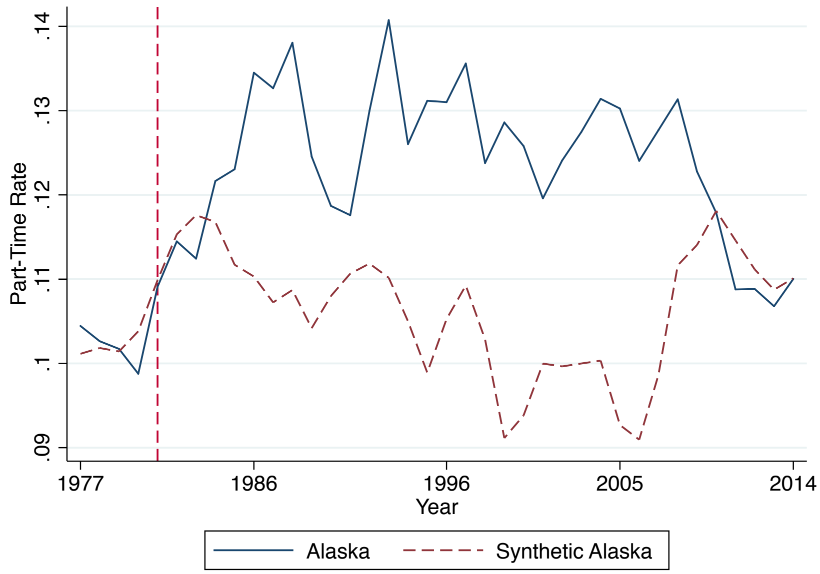
(a) Employment Rate: Alaska vs. Synthetic Alaska



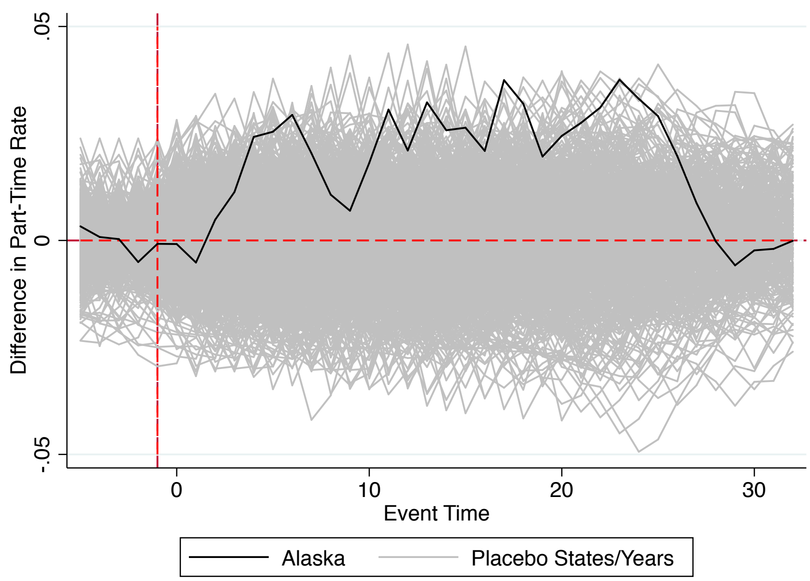
(b) Synthetic Difference in Employment Rate, Alaska vs. Placebo States

Notes: Panel (a) plots the synthetic control estimates of the employment rate for Alaska from 1977 to 2014. The solid line plots the actual employment rate in Alaska, while the dotted line plots the synthetic control estimate. The vertical dashed line indicates 1981, the year before the onset of the Alaska Permanent Fund Dividend. Panel (b) plots the results of a permutation test of the significance of the difference between Alaska and synthetic Alaska. The solid dark line plots the difference for Alaska using the true introduction of the treatment in 1982. The light grey lines plot the difference using other states and or other treatment years. See Appendix Table A.10 for the combination of states and weights that comprise each synthetic control.

Figure 3: Part-Time Rate, 1977-2014



(a) Part-Time Rate: Alaska vs. Synthetic Alaska



(b) Synthetic Difference in Part-Time Rate, Alaska vs. Placebo States

Notes: Panel (a) plots the synthetic control estimates of the part-time rate for Alaska from 1977 to 2014. The solid line plots the actual employment rate in Alaska, while the dotted line plots the synthetic control estimate. The vertical dashed line indicates 1981, the year before the onset of the Alaska Permanent Fund Dividend. Panel (b) plots the results of a permutation test of the significance of the difference between Alaska and synthetic Alaska. The solid dark line plots the difference for Alaska using the true introduction of the treatment in 1982. The light grey lines plot the difference using other states and or other treatment years. See Appendix Table A.10 for the combination of states and weights that comprise each synthetic control.

Table 1: Pre-Treatment Covariate Balance

	(1)	(2)	(3)	(4)
Panel A: Monthly CPS		Synthetic Control Outcome		
	Alaska	Employment Rate	Labor Force Participation	Part-Time Rate
Employment Rate	0.639	0.639	-	-
Labor Force Participation	0.712	-	0.706	-
Part-Time Rate	0.103	-	-	0.104
Age 16 - 19	0.108	0.102	0.098	0.096
Age 20 - 24	0.154	0.137	0.130	0.127
Age 25 - 65	0.691	0.636	0.658	0.677
Share Women	0.503	0.509	0.503	0.503
Industry Group 1	0.361	0.361	0.331	0.337
Industry Group 2	0.097	0.126	0.122	0.106
Industry Group 3	0.035	0.069	0.064	0.035
Industry Group 4	0.191	0.187	0.189	0.185
Industry Group 5	0.078	0.090	0.124	0.161
Education $\leq$ 11 years	0.229	0.239	0.252	0.265
Education = 12 years	0.396	0.386	0.413	0.406
Panel B: CPS MORG		Synthetic Control Outcome		
	Alaska	Hours Worked Last Week		
Hours Worked Last Week	37.980	37.935		
Age 16 - 19	0.074	0.067		
Age 20 - 24	0.155	0.144		
Age 25 - 65	0.759	0.755		
Share Women	0.435	0.432		
Industry Group 1	0.148	0.185		
Industry Group 2	0.051	0.090		
Industry Group 3	0.292	0.255		
Industry Group 4	0.123	0.150		
Education $\leq$ 11 years	0.110	0.170		
Education = 12 years	0.387	0.362		

Notes: Table reports average value of variables during the pre-treatment period for Alaska and the synthetic control constructed using the method in Section 3. Columns (2) - (4) differ in the outcome matched on in equation (4). Panel A features data from Monthly CPS surveys and Panel B features data from the CPS MORG. The omitted category for age groups is 65 and older. The omitted category for industry groups are not working in Panel A and industry group 5 in Panel B (since everyone is working by construction in Panel B). The omitted group for education is more than 12 years. The pre-treatment period covers 1977-1981 in Panel A and 1979-1981 in Panel B. See Appendix Table A.10 for the combination of states and weights that comprise each synthetic control.

Table 2: Synthetic Control Estimates, Average Difference 1982-2014

	(1)	(2)	(3)	(4)
	Employment Rate	Part-Time Rate	Labor Force Participation	Hours Worked Last Week
$\hat{\alpha}_0$	0.001	0.018	0.012	-0.796
p-value	0.942	0.020	0.331	0.084
95% CI	[-0.030,0.033]	[0.004,0.032]	[-0.019,0.042]	[-1.751,0.191]
Number of placebos	1,836	1,836	1,836	1,734
Pre-Period RMSE	0.005	0.003	0.013	0.394
RMSE Percentile	0.322	0.252	0.903	0.753

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes, using the synthetic control method outlined in Section 3. The treatment effect is averaged over the years 1982 to 2014. The p-value and confidence intervals are constructed using the permutation test also described in Section 3. Root mean squared error (RMSE) is calculated using up to 5 years of pre-treatment data, and percentile is based on a comparison among all placebo estimates. See Appendix Table A.10 for the combination of states and weights that comprise each synthetic control.

Table 3: Synthetic Control Estimates, Average Difference 1982-2014, by Gender and Marital Status

	Employment Rate - Men			Part-Time Rate - Men		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Married	Unmarried	All	Married	Unmarried
$\hat{\alpha}_0$	0.029	0.032	-0.004	0.008	0.003	0.012
p-value	0.093	0.081	0.846	0.192	0.571	0.190
95% CI	[-0.008,0.065]	[-0.008,0.071]	[-0.045,0.037]	[-0.004,0.019]	[-0.008,0.014]	[-0.008,0.031]
Number of placebos	1,836	1,836	1,836	1,836	1,836	1,836
Pre-Period RMSE	0.024	0.011	0.038	0.003	0.007	0.008
RMSE Percentile	0.972	0.609	0.981	0.259	0.845	0.466
	Employment Rate - Women			Part-Time Rate - Women		
	(7)	(8)	(9)	(10)	(11)	(12)
	All	Married	Unmarried	All	Married	Unmarried
$\hat{\alpha}_0$	-0.019	0.015	0.007	0.022	0.035	0.003
p-value	0.234	0.364	0.697	0.032	0.001	0.743
95% CI	[-0.055,0.017]	[-0.020,0.050]	[-0.032,0.046]	[0.003,0.042]	[0.016,0.054]	[-0.019,0.026]
Number of placebos	1,836	1,836	1,836	1,836	1,836	1,836
Pre-Period RMSE	0.026	0.015	0.030	0.004	0.009	0.006
RMSE Percentile	0.978	0.735	0.966	0.291	0.680	0.286

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes, using the synthetic control method outlined in Section 3. The treatment effect is averaged over the years 1982 to 2014. The p-value and confidence intervals are constructed using the permutation test also described in Section 3. Root mean squared error (RMSE) is calculated using up to 5 years of pre-treatment data, and percentile is based on a comparison among all placebo estimates. See Appendix Tables A.12 and A.13 for the combination of states and weights that comprise each synthetic control.

Table 4: Synthetic Control Estimates, Average Difference 1982-2014, Common Weights

	(1)	(2)
	Employment Rate	Part-Time Rate
$\hat{\alpha}_0$	0.032	0.011
p-value	0.040	0.101
95% CI	[0.003,0.065]	[-0.006,0.028]
Number of placebos	1,836	1,836
Pre-Period RMSE	0.005	0.005
RMSE Percentile	0.312	0.312

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes, using the synthetic control method outlined in Section 3. The treatment effect is averaged over the years 1982 to 2014. The p-value and confidence intervals are constructed using the permutation test also described in Section 3. Root mean squared error (RMSE) is calculated using up to 5 years of pre-treatment data, and percentile is based on a comparison among all placebo estimates. See Appendix Table A.11 for the combination of states and weights that comprise each synthetic control.

Table 5: Expected Micro and Macro Effects on Employment Rates

A: Parameters

Parameter	Description	Value	Source
Income Effect	$EPOP$ Change per \$140K of Income	0.02	Cesarini et al. (2017)
MPC	Marginal Propensity to Consume	0.25	Kueng (2018)
η	Home-Bias	0.69	Nakamura and Steinsson (2014)
$(1-\alpha)$	Labor Share of Income	0.667	Chodorow-Reich et al. (2019)
$\mathcal{M}$	Fiscal Multiplier	1.8	Chodorow-Reich (2019)
κ	Wage Adjustment	1.2	Chodorow-Reich et al. (2019)
β	Jobs per \$100K of Spending	1.9	Chodorow-Reich (2019)
$EPOP$	Average Employment to Population Ratio	0.66	Authors' calc.
PF Dividend	Average Per Capita Dividend (2010 Dollars)	\$1,495	Authors' calc.
PF Dividend (PDV)	PDV of Lifetime Dividends	\$45,000	Authors' calc.
PF Dividends/Labor Income	Ratio of Total Dividends to Total Labor Income	0.0725	Authors' calc.

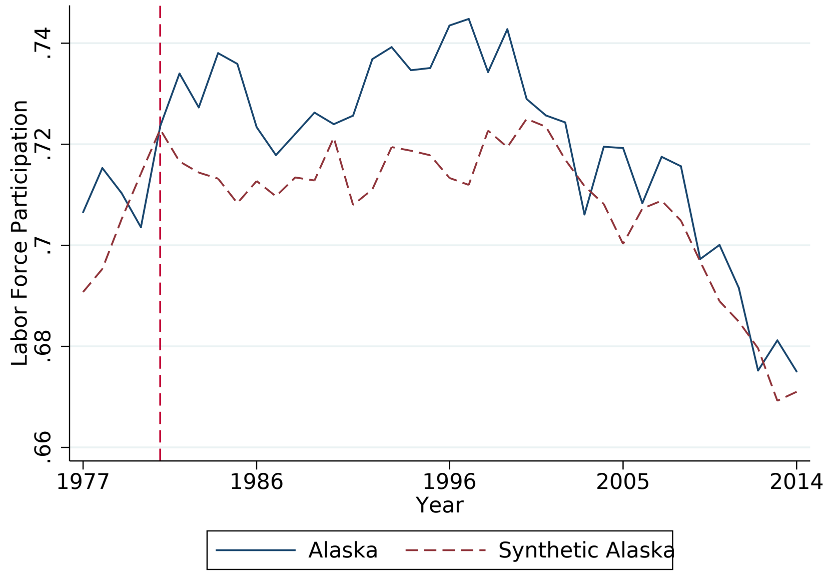
B: Labor Effects

Channel	Formula	Predicted Effect	Source
Micro Effect	Income Effect $\times$ PF Dividend (PDV) / \$140K	-0.006	Cesarini et al. (2017)
Macro Effect (version 1)	$\frac{1}{1+\kappa}\mathcal{M}\,(1-\alpha)\,\eta \times MPC \times \frac{PF Dividends}{Labor Income} \times EPOP$	0.005	Chodorow-Reich et al. (2019)
Macro Effect (version 2)	$\eta \times MPC \times \beta \times \frac{PF Dividend}{\$100{,}000}$	0.005	Chodorow-Reich (2019)

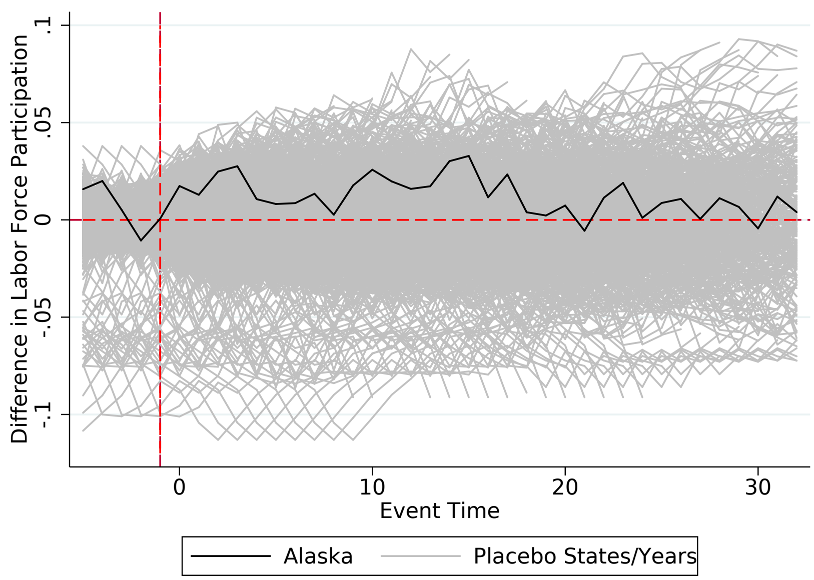
Notes: The table presents estimates of the expected effect of the Alaska Permanent Fund Dividend using prior studies. The "Micro Effect" corresponds to the direct income effect on labory supply of a lifetime of PF dividend payments. The "Macro Effect" corresponds to the multiplier effect of more spending on employment, using estimates from two different methods. See Section 6.3 for more details.

Appendix A: Appendix Tables and Figures

Figure A.1: Labor Force Participation, 1977-2014



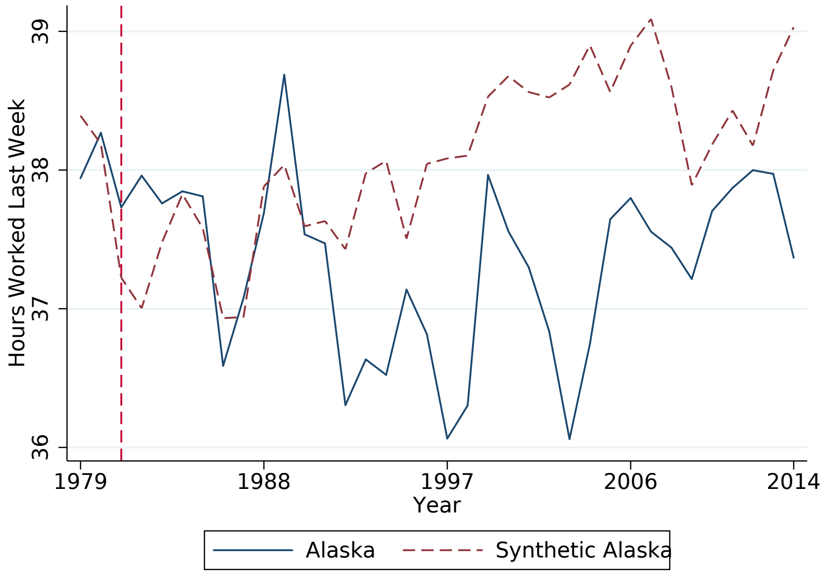
(a) Labor Force Participation: Alaska vs. Synthetic Alaska



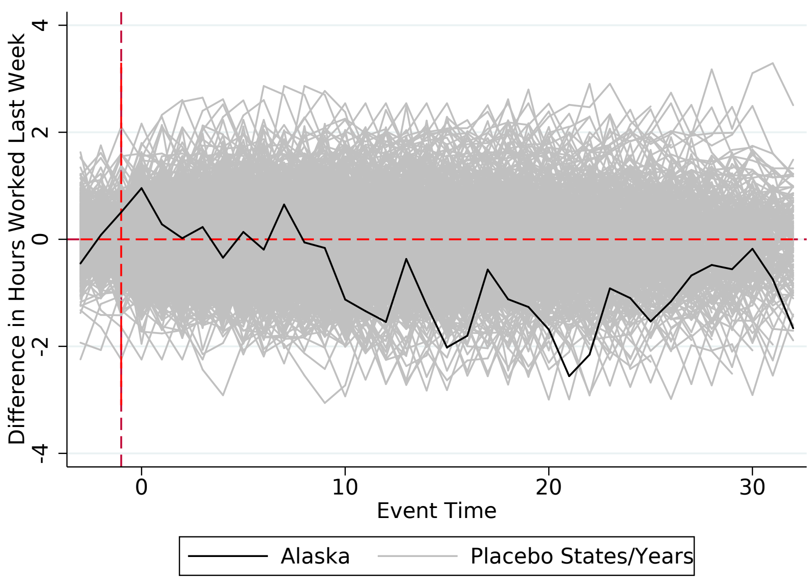
(b) Synthetic Difference in Labor Force Participation, Alaska vs. Placebo States

Notes: Panel (a) plots the synthetic control estimates of labor force participation for Alaska from 1977 to 2014. The solid line plots the actual employment rate in Alaska, while the dotted line plots the synthetic control estimate. The vertical dashed line indicates 1981, the year before the onset of the Alaska Permanent Fund Dividend. Panel (b) plots the results of a permutation test of the significance of the difference between Alaska and synthetic Alaska. The solid dark line plots the difference for Alaska using the true introduction of the treatment in 1982. The light grey lines plot the difference using other states and or other treatment years. See Appendix Table A.10 for the combination of states and weights that comprise each synthetic control.

Figure A.2: Hours Worked Last Week, 1977-2014



(a) Hours Worked Last Week: Alaska vs. Synthetic Alaska



(b) Synthetic Difference in Hours Worked Last Week, Alaska vs. Placebo States

Notes: Panel (a) plots the synthetic control estimates of hours worked last week for Alaska from 1977 to 2014. The solid line plots the actual employment rate in Alaska, while the dotted line plots the synthetic control estimate. The vertical dashed line indicates 1981, the year before the onset of the Alaska Permanent Fund Dividend. Panel (b) plots the results of a permutation test of the significance of the difference between Alaska and synthetic Alaska. The solid dark line plots the difference for Alaska using the true introduction of the treatment in 1982. The light grey lines plot the difference using other states and or other treatment years. See Appendix Table A.10 for the combination of states and weights that comprise each synthetic control.

Table A.1: Pre-Treatment Covariate Balance, Common Weights

	(1)	(2)
	Alaska	Synthetic Alaska
Employment Rate	0.639	0.639
Part-Time Rate	0.103	0.105
Age 16 - 19	0.108	0.095
Age 20 - 24	0.154	0.127
Age 25 - 65	0.691	0.664
Share Women	0.503	0.506
Industry Group 1	0.361	0.361
Industry Group 2	0.097	0.121
Industry Group 3	0.035	0.048
Industry Group 4	0.191	0.177
Industry Group 5	0.078	0.122
Education $\leq$ 11 years	0.229	0.278
Education = 12 years	0.396	0.393

Notes: Table reports average value of variables during the pre-treatment period for Alaska and the synthetic control constructed using the method in Section 3. The omitted category for age groups is 65 and older. The omitted category for industry groups is not working. The omitted group for education is more than 12 years. The pre-treatment period covers 1977-1981. See Appendix Table A.11 for the combination of states and weights that comprise each synthetic control.

Table A.2: Synthetic Control Estimates, Average Difference 1982-2014, by Age

	55 and Over		Under 55	
	(1)	(2)	(3)	(4)
	Employment Rate	Part-Time Rate	Employment Rate	Part-Time Rate
$\hat{\alpha}_0$	0.046	0.015	0.009	0.013
p-value	0.020	0.053	0.494	0.105
95% CI	[0.005,0.086]	[-0.000,0.030]	[-0.020,0.039]	[-0.004,0.031]
Number of placebos	1,836	1,836	1,836	1,836
Pre-Period RMSE	0.021	0.005	0.007	0.004
RMSE Percentile	0.895	0.389	0.511	0.427

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes, using the synthetic control method outlined in Section 3. The treatment effect is averaged over the years 1982 to 2014. The p-value and confidence intervals are constructed using the permutation test also described in Section 3. Root mean squared error (RMSE) is calculated using up to 5 years of pre-treatment data, and percentile is based on a comparison among all placebo estimates. See Appendix Table A.14 for the combination of states and weights that comprise each synthetic control.

Table A.3: Synthetic Control Estimates, Average Difference 1982-2014, in-space placebos

	(1)	(2)	(3)	(4)
	Employment Rate	Part-Time Rate	Labor Force Participation	Hours Worked Last Week
$\hat{\alpha}_0$	0.001	0.018	0.012	-0.796
p-value	0.980	0.059	0.431	0.118
95% CI	[-0.062,0.064]	[-0.001,0.038]	[-0.041,0.065]	[-1.681,0.165]
Number of placebos	51	51	51	51
Pre-Period RMSE	0.005	0.003	0.013	0.394
RMSE Percentile	0.275	0.294	0.882	0.706

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes, using the synthetic control method outlined in Section 3. The treatment effect is averaged over the years 1982 to 2014. The p-value and confidence intervals are constructed using the permutation test also described in Section 3. Root mean squared error (RMSE) is calculated using up to 5 years of pre-treatment data, and percentile is based on a comparison among all placebo estimates. See Appendix Table A.10 for the combination of states and weights that comprise each synthetic control.

Table A.4: Synthetic Control Estimates, Average Difference 1982-2014, Last Year Method

	(1)	(2)
	Employment Rate	Part-Time Rate
$\hat{\alpha}_0$	-0.002	0.017
p-value	0.880	0.034
95% CI	[-0.034,0.031]	[0.001,0.032]
Number of placebos	1,836	1,836
Pre-Period RMSE	0.006	0.003
RMSE Percentile	0.610	0.199

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes, using the synthetic control method outlined in Section 3. The treatment effect is averaged over the years 1982 to 2014. The p-value and confidence intervals are constructed using the permutation test also described in Section 3. Root mean squared error (RMSE) is calculated using up to 5 years of pre-treatment data, and percentile is based on a comparison among all placebo estimates. See Appendix Table A.15 for the combination of states and weights that comprise each synthetic control.

Table A.5: Synthetic Control Estimates, Average Difference 1982-2014, longer pre-period

	(1)	(2)	(3)
	Employment Rate		
Earliest Year	1977	1970	1960
$\hat{\alpha}_0$	0.001	0.030	0.030
p-value	0.942	0.047	0.052
95% CI	[-0.030,0.033]	[0.000,0.061]	[-0.001,0.061]
Number of placebos	1,836	1,836	1,836
Pre-Period RMSE	0.005	0.011	0.011
RMSE Percentile	0.322	0.662	0.564

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes, using the synthetic control method outlined in Section 3. The treatment effect is averaged over the years 1982 to 2014. The p-value and confidence intervals are constructed using the permutation test also described in Section 3. Root mean squared error (RMSE) is calculated using up to 5 years of pre-treatment data, and percentile is based on a comparison among all placebo estimates. See Appendix Table A.10 for the combination of states and weights that comprise the synthetic control for column (1) and Appendix Table A.16 for columns (2) and (3).

Table A.6: Synthetic Control Estimates, Average Difference 1982-1985

	(1)	(2)	(3)	(4)
	Employment Rate	Part-Time Rate	Labor Force Participation	Hours Worked Last Week
$\hat{\alpha}_0$	0.026	0.003	0.021	0.372
p-value	0.104	0.669	0.092	0.306
95% CI	[-0.009,0.061]	[-0.012,0.016]	[-0.007,0.048]	[-0.618,1.298]
Number of placebos	357	357	357	255
Pre-Period RMSE	0.005	0.003	0.013	0.394
RMSE Percentile	0.471	0.468	0.936	0.800

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes, using the synthetic control method outlined in Section 3. The treatment effect is averaged over the years 1982 to 1985. The p-value and confidence intervals are constructed using the permutation test also described in Section 3. Root mean squared error (RMSE) is calculated using up to 5 years of pre-treatment data, and percentile is based on a comparison among all placebo estimates. See Appendix Table A.10 for the combination of states and weights that comprise each synthetic control.

Table A.7: Synthetic Control Estimates, Average Difference 1982-2014, controlling for oil production

	(1)	(2)	(3)	(4)
	Employment Rate	Part-Time Rate	Labor Force Participation	Hours Worked Last Week
$\hat{\alpha}_0$	0.025	0.009	0.018	-0.824
p-value	0.097	0.141	0.169	0.082
95% CI	[-0.006,0.058]	[-0.004,0.023]	[-0.014,0.048]	[-1.776,0.177]
Number of placebos	1,836	1,836	1,836	1,734
Pre-Period RMSE	0.006	0.003	0.014	0.538
RMSE Percentile	0.335	0.298	0.932	0.881

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes, using the synthetic control method outlined in Section 3. The treatment effect is averaged over the years 1982 to 2014. The p-value and confidence intervals are constructed using the permutation test also described in Section 3. Root mean squared error (RMSE) is calculated using up to 5 years of pre-treatment data, and percentile is based on a comparison among all placebo estimates. See Appendix Table A.17 for the combination of states and weights that comprise each synthetic control.

Table A.8: Synthetic Control Estimates, Average Difference 1982-2014, by tradability

	(1)	(2)	(3)	(4)
	Tradable		Non-Tradable	
	Employment Rate	Part-Time Rate	Employment Rate	Part-Time Rate
$\hat{\alpha}_0$	-0.048	0.015	0.002	-0.007
p-value	0.005	0.119	0.859	0.670
95% CI	[-0.072,-0.025]	[-0.007,0.038]	[-0.024,0.027]	[-0.040,0.025]
Number of placebos	1,836	1,836	1,836	1,836
Pre-Period RMSE	0.060	0.014	0.044	0.012
RMSE Percentile	0.997	0.865	0.995	0.595

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes, using the synthetic control method outlined in Section 3. The treatment effect is averaged over the years 1982 to 2014. The p-value and confidence intervals are constructed using the permutation test also described in Section 3. Root mean squared error (RMSE) is calculated using up to 5 years of pre-treatment data, and percentile is based on a comparison among all placebo estimates. See Appendix Table A.18 for the combination of states and weights that comprise each synthetic control.

Table A.9: Synthetic Control Estimates, Average Difference 1982-2014 Government Spending Shares

	(1)	(2)	(3)	(4)
	Health/Hospitals	Education	Highways	Welfare/Transfers
$\hat{\alpha}_0$	-0.006	-0.074	0.030	-0.018
p-value	0.679	0.011	0.032	0.416
95% CI	[-0.034,0.024]	[-0.127,-0.020]	[0.002,0.056]	[-0.072,0.034]
Number of placebos	1,800	1,800	1,800	1,800
Pre-Period RMSE	0.012	0.019	0.022	0.007
RMSE Percentile	0.966	0.919	0.979	0.381

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes, using the synthetic control method outlined in Section 3. The treatment effect is averaged over the years 1982 to 2014. The p-value and confidence intervals are constructed using the permutation test also described in Section 3. Root mean squared error (RMSE) is calculated using up to 5 years of pre-treatment data, and percentile is based on a comparison among all placebo estimates. See Appendix Table A.19 for the combination of states and weights that comprise each synthetic control.

Table A.10: State Weights for Synthetic Alaska

State	Weight
Panel A: Employment Rate	
Utah	0.428
Wyoming	0.342
Washington	0.092
Nevada	0.079
Montana	0.034
Minnesota	0.025
Panel B: Part-Time Rate	
Nevada	0.729
Wyoming	0.160
Louisiana	0.060
Maryland	0.033
District of Columbia	0.019
Panel C: Labor Force Participation	
Nevada	0.373
Minnesota	0.306
Wyoming	0.301
Wisconsin	0.020
Panel D: Hours Worked Last Week	
Wyoming	0.384
Oklahoma	0.358
District of Columbia	0.248
Nevada	0.011

Notes: Table reports the combination of states and weights chosen using the method in Section 3 to construct a synthetic control for Alaska. Panels A through D correspond to columns (1) through (4) in Table 2.

Table A.11: State Weights for Synthetic Alaska, Common Weights

State	Weight
Panel A: Employment and Part-Time Rate, Common Weights	
Nevada	0.392
Wyoming	0.324
West Virginia	0.125
Washington	0.099
District of Columbia	0.060

Notes: Table reports the combination of states and weights chosen using the method in Section 3 to construct a synthetic control for Alaska. Panel A corresponds to columns (1) and (2) in Table 4.

Table A.12: State Weights for Synthetic Alaska, for Men, by Marital Status

State	Weight
Panel A: Employment Rate - All Men	
Montana	0.511
Washington	0.371
District of Columbia	0.081
Florida	0.037
Panel B: Employment Rate - Married Men	
Montana	0.496
Maryland	0.150
Colorado	0.149
Utah	0.086
Washington	0.079
Nevada	0.041
Panel C: Employment Rate - Unmarried Men	
Hawaii	0.479
Montana	0.289
Pennsylvania	0.232
Panel D: Part-Time Rate - All Men	
Wyoming	0.340
Maryland	0.191
District of Columbia	0.185
Washington	0.133
Nevada	0.095
Pennsylvania	0.055
Panel E: Part-Time Rate - Married Men	
Colorado	0.725
Nevada	0.167
New Mexico	0.080
Wyoming	0.026
Maryland	0.002
Panel F: Part-Time Rate - Unmarried Men	
District of Columbia	0.396
Pennsylvania	0.264
Wyoming	0.243
Nevada	0.096

Notes: Table reports the combination of states and weights chosen using the method in Section 3 to construct a synthetic control for Alaska. Panels A through F correspond to columns (1) through (6) in Table 3.

Table A.13: State Weights for Synthetic Alaska, for Women, by Marital Status

State	Weight
Panel A: Employment Rate - All Women	
Minnesota	0.848
Wyoming	0.110
Nevada	0.041
Panel B: Employment Rate - Married Women	
Wyoming	0.362
Maryland	0.195
Hawaii	0.153
Kansas	0.137
North Carolina	0.103
District of Columbia	0.049
Panel C: Employment Rate - Unmarried Women	
Wyoming	0.511
Minnesota	0.417
Nevada	0.073
Panel D: Part-Time Rate - All Women	
Nevada	0.352
Wyoming	0.262
Texas	0.222
District of Columbia	0.075
Louisiana	0.037
Hawaii	0.029
New Mexico	0.023
Panel E: Part-Time Rate - Married Women	
Nevada	0.609
Kansas	0.272
Louisiana	0.119
Panel F: Part-Time Rate - Unmarried Women	
Wyoming	0.500
Nevada	0.240
District of Columbia	0.205
Maryland	0.033
New Jersey	0.022

Notes: Table reports the combination of states and weights chosen using the method in Section 3 to construct a synthetic control for Alaska. Panels A through F correspond to columns (7) through (12) in Table 3.

Table A.14: State Weights for Synthetic Alaska, by Age

State	Weight
Panel A: Employment Rate - 55 and Over	
Wyoming	0.614
Nevada	0.386
Panel B: Part-Time Rate - 55 and Over	
Nevada	0.576
Hawaii	0.359
West Virginia	0.066
Panel C: Employment Rate - Under 55	
New Mexico	0.385
Montana	0.342
New York	0.187
West Virginia	0.058
Hawaii	0.028
Panel D: Part-Time Rate - Under 55	
Wyoming	0.238
Nevada	0.188
District of Columbia	0.187
West Virginia	0.166
Hawaii	0.100
Oklahoma	0.073
Pennsylvania	0.049

Notes: Table reports the combination of states and weights chosen using the method in Section 3 to construct a synthetic control for Alaska. Panels A through D correspond to columns (1) through (4) in Table **??**.

Table A.15: State Weights for Synthetic Alaska, Last Year Method

State	Weight
Panel A: Employment Rate, Last Year Method	
Utah	0.428
Wyoming	0.342
Washington	0.092
Nevada	0.079
Montana	0.034
Minnesota	0.025
Panel B: Part-Time Rate, Last Year Method	
Nevada	0.729
Wyoming	0.160
Louisiana	0.060
Maryland	0.033
District of Columbia	0.019

Notes: Table reports the combination of states and weights chosen using the method in Section 3 to construct a synthetic control for Alaska. Panels A through B correspond to columns (1) through (2) in Table **??**.

Table A.16: State Weights for Synthetic Alaska, longer pre-period

State	Weight
Panel A: Employment Rate - Additional pre-period from 1970	
Hawaii	0.737
Nevada	0.256
Wyoming	0.006
Panel B: Employment Rate - Additional pre-period from 1960	
Hawaii	0.752
Nevada	0.248

Notes: Table reports the combination of states and weights chosen using the method in Section 3 to construct a synthetic control for Alaska. Panels A and B correspond to columns (2) and (3) in Table A.5.

Table A.17: State Weights for Synthetic Alaska, controlling for oil production

State	Weight
Panel A: Employment Rate, controlling for oil production	
Wyoming	0.653
New Mexico	0.234
Louisiana	0.114
Panel B: Part-Time Rate, controlling for oil production	
Wyoming	0.620
District of Columbia	0.294
West Virginia	0.086
Panel C: Labor Force Participation, controlling for oil production	
Wyoming	0.830
Nevada	0.137
Michigan	0.033
Panel D: Hours Worked Last Week, controlling for oil production	
Wyoming	0.543
District of Columbia	0.308
Oklahoma	0.149

Notes: Table reports the combination of states and weights chosen using the method in Section 3 to construct a synthetic control for Alaska. Panels A through D correspond to columns (1) through (4) in Table A.7.

Table A.18: State Weights for Synthetic Alaska, by tradability

State	Weight
Panel A: Employment Rate - Tradable Sectors	
Oregon	0.987
Montana	0.013
Panel B: Part-Time Rate - Tradable Sectors	
North Dakota	0.454
Hawaii	0.448
Arkansas	0.097
Panel C: Employment Rate - Non-tradable Sectors	
West Virginia	0.899
District of Columbia	0.101
Panel D: Part-Time Rate - Non-tradable Sectors	
Louisiana	0.759
Nevada	0.123
District of Columbia	0.118

Notes: Table reports the combination of states and weights chosen using the method in Section 3 to construct a synthetic control for Alaska. Panels A through D correspond to columns (1) through (4) in Table A.8.

Table A.19: State Weights for Synthetic Alaska Government Spending Shares

State	Weight
Panel A: Health/Hospitals	
Nevada	1.000
Panel B: Education	
Wyoming	0.456
Maryland	0.230
Nevada	0.185
New Jersey	0.130
Panel C: Highways	
Utah	0.518
California	0.329
Wyoming	0.136
Hawaii	0.017
Panel D: Welfare/Transfers	
Wyoming	0.386
Nevada	0.347
Arizona	0.267

Notes: Table reports the combination of states and weights chosen using the method in Section 3 to construct a synthetic control for Alaska. Panels A through D correspond to columns (1) through (4) in Table A.9.

B Migration

In this section, we address the potential for differential migration to confound our results. In Figure B.1 we plot annual net migration, using intercensal estimates on state population growth form the Census Bureau, combined with natality and mortality records from the CDC. As can be seen Alaska has greater variation net migration, especially in the early period, and in particular, near the timing of the Alaska PFD in 1981. This is most likely a result of growth in the oil industry during the mid- to late 1970s. To assess how our sensitive our results are to this in-migration, we present three sets of results that account for migration: (1) we control for average net imigration in the pre-period, (2) we control for annual net migration in each pre-period year, and (3) we also use CPS data to reassign recent in-migrants to their home states.

First, In Table B.1 we replicate our main analysis, while introducing average net migration in between 1977 to 1981 as an additional matching variable. Compared to Table 2, we find that our conclusions are largely the same.

Second, in Table B.2, we take a further step and control for net migration in each year between 1977 and 1981, to not only match overall net migration, but also year to year changes in the pre-period. Again, we find very similar results to our main analysis in Table 2.

Third, we propose an adjustment using the Annual Social and Economic Supplement (ASEC) conducted in March that asks one's residence in the previous year. Figure B.2 shows in-migration as share of population over time for Alaska and the rest of the US. Similar to Figure B.1, Alaska experienced a relative influx of new residents during the time just before the introduction of the Alaska Permanent Fund dividend. Because the CPS is not a long panel, we cannot completely drop new migrants from the sample. To partially net out new migrants, we assign each respondent to their state of residence in the prior year. Our data from the ASEC begin in March of 1980.

Column (1) of Table B.3 is reproduced from column (2) of Table **??**. To benchmark our adjustment, we first do not adjust for migration but simply restrict analysis to just the months of March (column 2), and we see a more positive effect on the employment rate, owing to seasonal heterogeneity in our estimates. In column (3), the estimates are very similar to column 2 with a positive employment effect when we adjust for migration by reassigning respondents to their state of residence in the previous year. In columns (4) through (6), we implement the same adjustment for the part-time rate. In that case, we see even less movement in the point estimates, and again draw similar qualitative conclusions after the adjustment.

Figure B.1: Annual Net Migration: 1970 - 2014

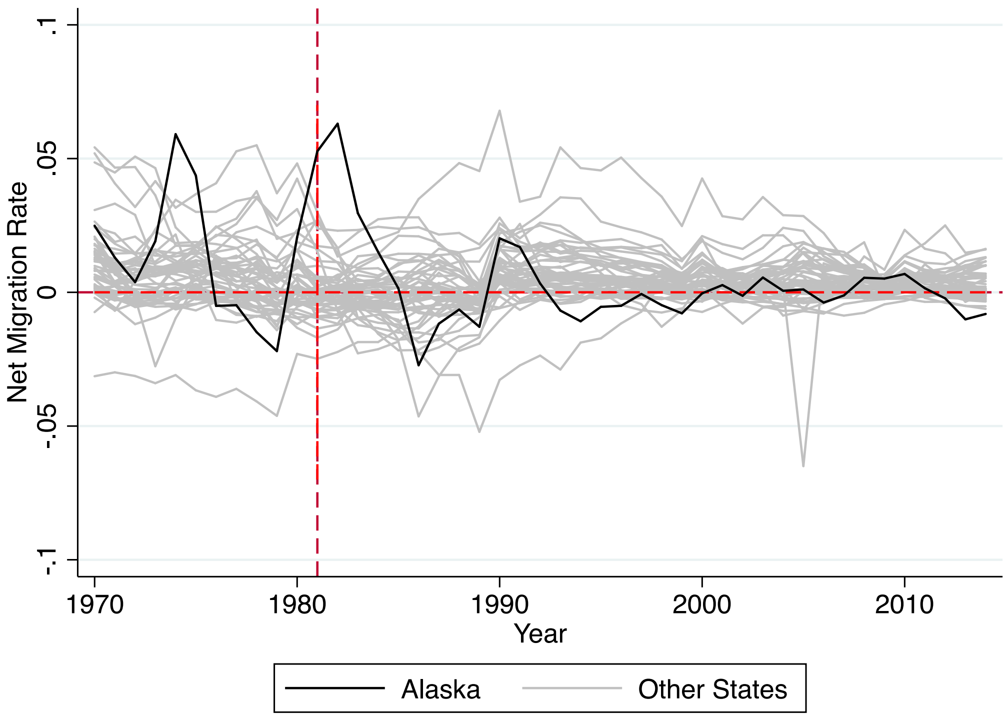


Table B.1: Synthetic Control Estimates, Average Difference 1982-2014, controlling for average net migration

	(1)	(2)	(3)	(4)
	Employment Rate	Part-Time Rate	Labor Force Participation	Hours Worked Last Week
$\hat{\alpha}_0$	0.008	0.015	0.014	-0.772
p-value	0.548	0.038	0.278	0.092
95% CI	[-0.022,0.039]	[0.002,0.029]	[-0.018,0.043]	[-1.723,0.227]
Number of placebos	1,836	1,836	1,836	1,734
Pre-Period RMSE	0.004	0.003	0.012	0.421
RMSE Percentile	0.216	0.224	0.867	0.783

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes, using the synthetic control method outlined in Section 3. The treatment effect is averaged over the years 1982 to 2014. The p-value and confidence intervals are constructed using the permutation test also described in Section 3. Root mean squared error (RMSE) is calculated using up to 5 years of pre-treatment data, and percentile is based on a comparison among all placebo estimates. See Appendix Table B.4 for the combination of states and weights that comprise each synthetic control.

Table B.2: Synthetic Control Estimates, Average Difference 1982-2014, controlling for annual net migration

	(1)	(2)	(3)	(4)
	Employment Rate	Part-Time Rate	Labor Force Participation	Hours Worked Last Week
$\hat{\alpha}_0$	-0.006	0.011	-0.007	-0.792
p-value	0.658	0.068	0.581	0.085
95% CI	[-0.040,0.027]	[-0.001,0.024]	[-0.040,0.025]	[-1.733,0.164]
Number of placebos	1,836	1,836	1,836	1,734
Pre-Period RMSE	0.009	0.002	0.021	0.477
RMSE Percentile	0.695	0.161	0.975	0.842

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes, using the synthetic control method outlined in Section 3. The treatment effect is averaged over the years 1982 to 2014. The p-value and confidence intervals are constructed using the permutation test also described in Section 3. Root mean squared error (RMSE) is calculated using up to 5 years of pre-treatment data, and percentile is based on a comparison among all placebo estimates. See Appendix Table B.5 for the combination of states and weights that comprise each synthetic control.

Figure B.2: Share of Residents Living in a Different State Last Year

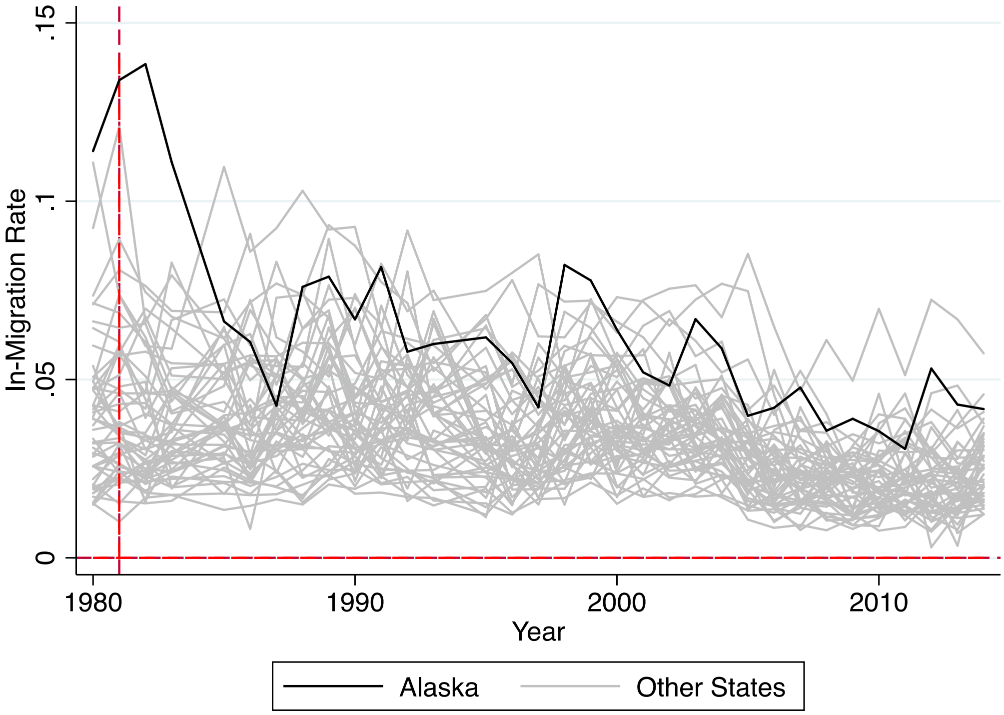


Table B.3: Synthetic Control Estimates, Average Difference 1982-2014, adjusting for in-migration

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment Rate			Part-Time Rate		
	12 Months	March	March Adjusted	12 Months	March	March Adjusted
$\hat{\alpha}_0$	0.026	0.067	0.050	0.003	-0.008	0.004
p-value	0.104	0.029	0.029	0.669	0.436	0.662
95% CI	[-0.009,0.061]	[0.027,0.110]	[0.005,0.095]	[-0.012,0.016]	[-0.032,0.013]	[-0.019,0.025]
Number of placebos	357	204	204	357	204	204
Pre-Period RMSE	0.005	0.003	0.019	0.003	0.003	0.011
RMSE Percentile	0.471	0.471	0.931	0.468	0.475	0.887

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes, using the synthetic control method outlined in Section 3. The treatment effect is averaged over the years 1982 to 2014. The p-value and confidence intervals are constructed using the permutation test also described in Section 3. Root mean squared error (RMSE) is calculated using up to 5 years of pre-treatment data, and percentile is based on a comparison among all placebo estimates. See Appendix Table B.6 for the combination of states and weights that comprise each synthetic control.

Table B.4: State Weights for Synthetic Alaska, controlling for average net migration

State	Weight
Panel A: Employment Rate, controlling for average net migration	
Colorado	0.496
Montana	0.393
Nevada	0.064
Minnesota	0.033
Wyoming	0.014
Panel B: Part-Time Rate, controlling for average net migration	
Wyoming	0.354
Nevada	0.309
District of Columbia	0.214
Maryland	0.107
West Virginia	0.017
Panel C: Labor Force Participation, controlling for average net migration	
Nevada	0.591
Minnesota	0.395
Michigan	0.015
Panel D: Hours Worked Last Week, controlling for average net migration	
Wyoming	0.413
Oklahoma	0.325
District of Columbia	0.262

Notes: Table reports the combination of states and weights chosen using the method in Section 3 to construct a synthetic control for Alaska. Panels A through D correspond to columns (1) through (4) in Table B.1.

Table B.5: State Weights for Synthetic Alaska, controlling for annual net migration

State	Weight
Panel A: Employment Rate, controlling for annual net migration	
Colorado	0.337
North Dakota	0.235
Minnesota	0.223
District of Columbia	0.205
Panel B: Part-Time Rate, controlling for annual net migration	
District of Columbia	0.449
Montana	0.307
Wyoming	0.235
Nevada	0.009
Panel C: Labor Force Participation, controlling for annual net migration	
Minnesota	0.952
Nevada	0.048
Panel D: Hours Worked Last Week, controlling for annual net migration	
Wyoming	0.425
District of Columbia	0.311
Oklahoma	0.261
New Mexico	0.003

Notes: Table reports the combination of states and weights chosen using the method in Section 3 to construct a synthetic control for Alaska. Panels A through D correspond to columns (1) through (4) in Table B.2.

Table B.6: State Weights for Synthetic Alaska, adjusting for in-migration

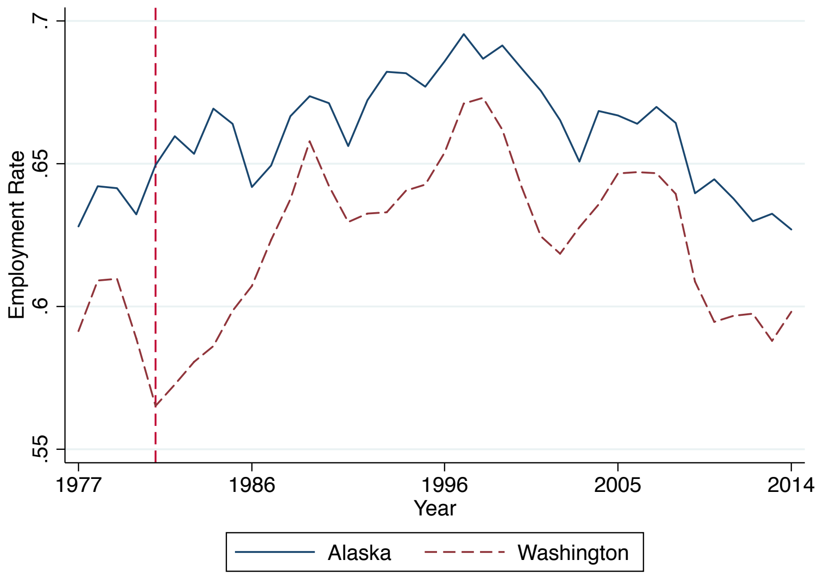
State	Weight
Panel A: Employment Rate, adjusting for in-migration	
Utah	0.428
Wyoming	0.342
Washington	0.092
Nevada	0.079
Montana	0.034
Minnesota	0.025
Panel B: Part-Time Rate, adjusting for in-migration	
Alabama	0.494
Wyoming	0.471
North Dakota	0.035
Panel C: Labor Force Participation, adjusting for in-migration	
New Mexico	0.618
Washington	0.259
Nevada	0.057
Montana	0.039
Hawaii	0.027
Panel D: Hours Worked Last Week, adjusting for in-migration	
Nevada	0.729
Wyoming	0.160
Louisiana	0.060
Maryland	0.033
District of Columbia	0.019

Notes: Table reports the combination of states and weights chosen using the method in Section 3 to construct a synthetic control for Alaska. Panels A through D correspond to columns (2), (3), (5), and (6) in Table B.3.

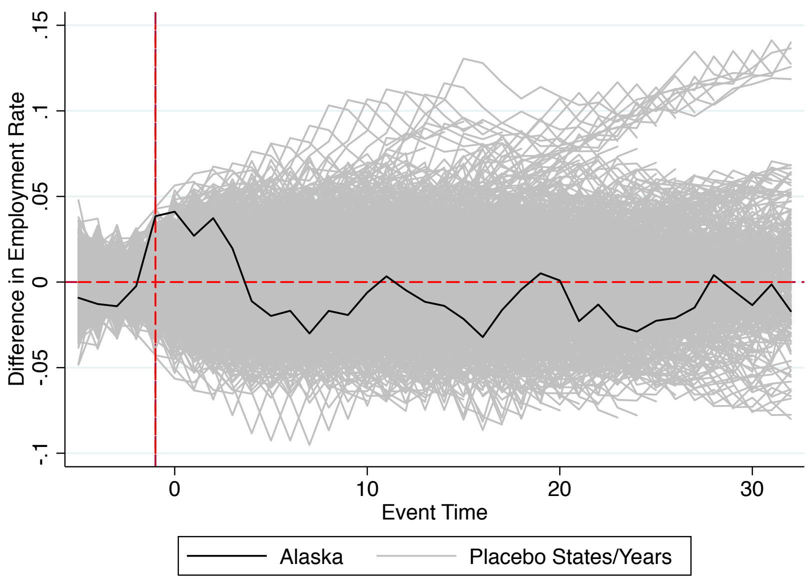
C Simple Difference-in-Differences Estimates

In this section, we present results using only Washington State as a control for Alaska. We use a difference-in-differences (DD) estimator. Inference is performed using a permutation method, as discussed in Bertrand et al. (2002), where we estimate placebo DD regressions in each of the other 50 states, using only the neighboring state that shares longest boundary with the primary state. Figures C.1 and C.2 present visual analogs to the DD estimators. In the figures with placebo plots, each DD series is shifted by the average level in the pre-period. In that case, the patterns in the pre-period can be used to asses parallel pre-trends, and the patterns in the post period preview the DD estimate.

Figure C.1: Employment Rate, 1977-2014



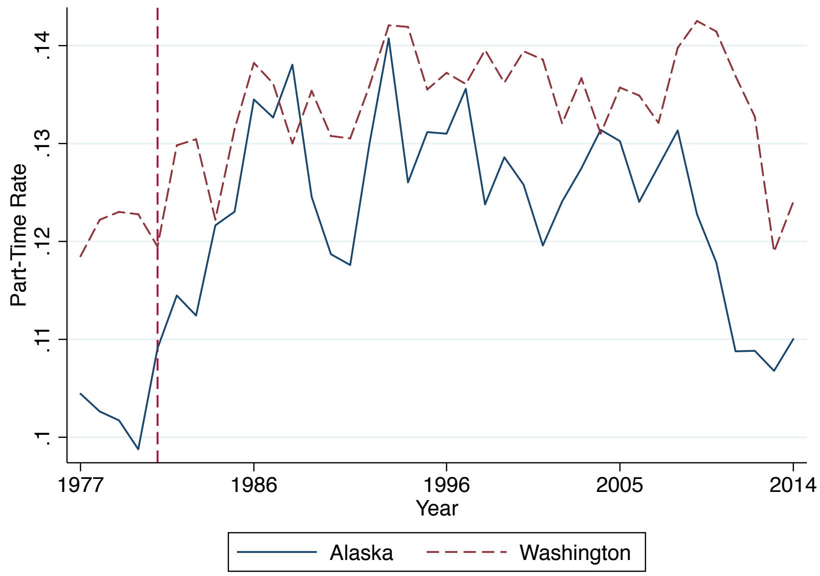
(a) Employment Rate: Alaska vs. Washington State



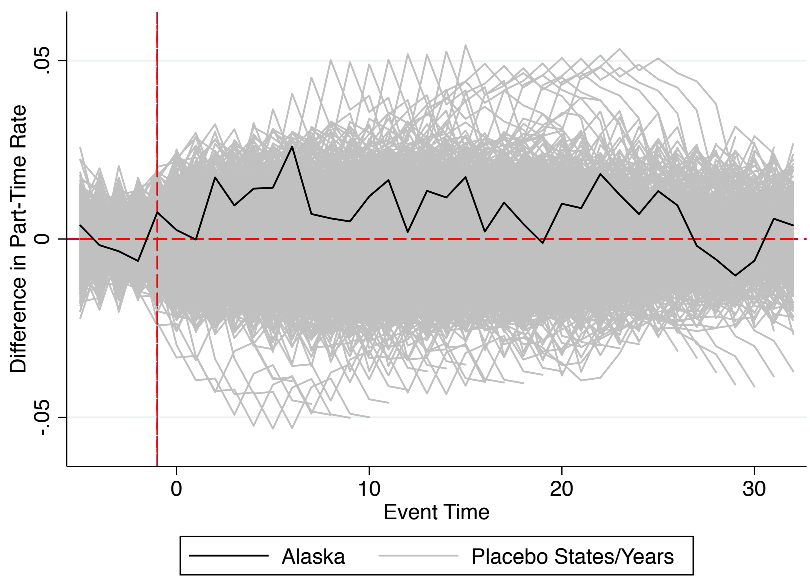
(b) Difference in Employment Rate, Alaska vs. Washington State

Notes: Panel (a) plots the employment rate for Alaska and Washington State, from 1977 to 2014. The vertical dashed line indicates 1981, the year before the onset of the Alaska Permanent Fund Dividend. Panel (b) plots the results of a permutation test of the significance of the difference between Alaska and Washington State. The solid dark line plots the difference for Alaska using the true introduction of the treatment in 1982. The light grey lines plot the difference using other states and or other treatment years. Each series is shifted by the average level in the pre-period, as described in Section C.

Figure C.2: Part-Time Rate, 1977-2014



(a) Part-Time Rate: Alaska vs. Washington State



(b) Difference in Part-Time Rate, Alaska vs. Washington State

Notes: Panel (a) plots the part-time rate for Alaska and Washington State, from 1977 to 2014. The vertical dashed line indicates 1981, the year before the onset of the Alaska Permanent Fund Dividend. Panel (b) plots the results of a permutation test of the significance of the difference between Alaska and Washington State. The solid dark line plots the difference for Alaska using the true introduction of the treatment in 1982. The light grey lines plot the difference using other states and or other treatment years. Each series is shifted by the average level in the pre-period, as described in Section C.

Table C.1: Difference-in-Differences Estimates, 1982-2014, Washington Control

	(1)	(2)
	Employment Rate	Part-Time Rate
$\hat{\alpha}_0$	-0.008	0.008
p-value	0.617	0.276
95% CI	[-0.042,0.026]	[-0.007,0.022]
Number of placebos	1,836	1,836

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes, using a difference-in-differences estimator outlined in Section 3. The pre-perid is defined from 1977 to 1981, and the post period is defined from 1982 to 2014. The p-value and confidence intervals are constructed using a permutation test similar to the one described in Section 6.2.