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

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Abstract

We study the impact of universal and permanent cash transfers on the labor market, using the introduction of the Alaska Permanent Fund Dividend. Since 1982, all Alaskan residents are entitled to a yearly cash dividend from the Alaska Permanent Fund. We use the Current Population Survey and a synthetic control method to show that the dividend had no effect on employment, and increased part-time work by 1.8 percentage points. Theory and prior empirical research suggests that the affect of cash transfers result in a decrease in household labor supply. We interpret our results as evidence that important general equilibrium effects of widespread and permanent transfers tend to offset this labor supply effect, at least on the extensive margin.

Keywords: unconditional cash transfer, universal basic income, labor supply, employment.

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. 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. For example, 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.[1] In Hawaii, House Concurrent Resolution 89 passed in 2017, establishing a Basic Economic Security Working Group focused on financial security to all families, including an evaluation of basic income policies.[2] In France, mainstream left presidential candidate Benoît Hamon included a universal basic income as a key proposal of his electoral program in 2017. Finally, Finland[3] and the Canadian province of Ontario[4] are running basic income experiments for disadvantaged populations, with the view to extend it later to a broader population.

A key concern with a universal basic income is that it 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., Forthcoming) 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, 2017, for an overview). While lottery studies leverage ideal exogeneity and the case study of the Eastern Band of Cherokee Indians involved a permanent dividend (Akee et al., 2010), these transfers accrue to small shares of the total population, i.e. they identify a micro effect. Although the NIT experiments included

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

[2] https://www.weforum.org/agenda/2017/06/hawaii-has-become-the-first-state-to-support-universal-basic-income

[3] https://www.theguardian.com/society/2017/feb/19/basic-income-finland-low-wages-fewer-jobs

[4] https://www.ontario.ca/page/ontario-basic-income-pilot

a treatment group comprised of an entire municipality, the experiments generally lasted only three to five years. A *universal* basic income will affect the labor market equilibrium and likely alter long-term expectations, and little is known about the long-run, macro impact of this policy.

To analyze the overall impact of a universal basic income on the labor market, we examine the case of the Alaska Permanent Fund Dividend. The fund, worth nearly $61 Billion as of August, 2017, is a diversified portfolio of invested oil reserve royalties.[5] 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 have resided in Alaska for at least a year. Relative to prior studies, ours features a cash transfer that is universal, unconditional, and permanent, and thus provides evidence relevant to the evaluation of a universal basic income.

Estimating the effect of a policy change in one particular state, Alaska, presents us with the methodological challenge of constructing an appropriate counterfactual. We therefore 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 before and 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.

Our primary analysis focuses on two outcomes for which relatively well-matched synthetic controls could be constructed: the employment to population ratio and the share working part time. 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, es-

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

timate a positive increase of two-percentage points, or 20 percent, in the share of 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.

The empirical patterns we observe are consistent with a number of different combinations of underlying labor market forces. First, the null employment effect could be explained a by positive general equilibrium response offsetting a negative income effect. That is, the unconditional cash transfer could result in consumption increases that stimulate labor demand and mitigate potential reductions in employment. Alternatively, our results are also consistent with an income effect close to zero and no general equilibrium response. The positive impact of the cash transfer on part-time work is consistent with a negative income effect on the intensive margin. However, additional analysis suggests that the part-time response could also be driven by new entrants into employment who work part-time, potentially overcoming fixed costs to entry as a result of the dividend. Overall, the estimated macro effects of an unconditional cash transfer on the labor market are generally inconsistent with large negative employment effects.

Our work makes two key contributions to the literature. First, and most importantly, 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 do not detect significant employment effects suggests that the policy could have general equilibrium effects on the labor market. 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, 2015), ours is the first, to our knowledge, to examine the labor market impacts of this policy.

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 fiance and optimal income tax literature, a universal basic income can essentially be though of as a demogrant,

e.g. the intercept of an NIT schedule. Although a tradeoff between redistribution and labor supply disincentives are 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 more resembles an NIT than an EITC when the macro effect of taxes on employment is smaller that the micro effect. Our empirical results are consistent with this setting. It should be noted, however, that the mapping between a universal basic income and the demogrant may also be confounded by political economy or behavioral factors associated with the framing of the transfer.

When a universal basic income is financed by income taxes, it 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). The effects of welfare reform are 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 are rely on a framework that focuses on supply-side responses, though our results suggests that general equilibrium factors may matter.

The paper is organized as follows. Section 2 describes the institutional context for the

Alaska Permanent Fund. In section 3, we discuss our empirical method, the syntheic control method, and then describe our data in section 4. We present the main results in section 5 and provide additional results and robustness tests in section 5.3. Section 6 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 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 is $61.4 billion.[6]

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

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 during the last 5 years, spread out evenly among the current year's applicants (Kueng, 2015). The nominal value of the dividend was a low as $331 in 1984, but has generally exceeded $1,000 since 1996, and peaked in 2015 at $2,072.[7] In order to qualify for a payment, a resident must have lived in Alaska for at least 12 months. Otherwise, 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, 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.

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.

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

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:

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

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, 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, as opposed to time-invariant state fixed effects normally assumed in DD approaches.

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, i.e. the untreated outcome in the treated state during the treatment period. 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, $\bar{y}_s^p = \frac{1}{T_0}\sum_{t=1}^{T_0} y_{st}$.[8] 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 $t \in \{T_0+1,\ldots,T\}$.[9] In practice, we report the average difference between the treatment

[8] 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.

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

unit and the synthetic control:

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

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:

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

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 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 is ranked lower than the 85th percentile.

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. 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. Note, if treatment status is randomly assigned, this procedure comprises randomization inference (Abadie et al., 2015). Although randomization is unlikely to be exactly true in our setting, we nonetheless implement the permutation method, in the spirit of Bertrand et al. (2002).[10]

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: 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.

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

The synthetic control method has a number of attractive features in our empirical setting. First, the selection of the control states is carried using a data-driven process. 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 bias confounds 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 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 measure data on active labor force, employment status, and part-time employment status using the monthly CPS surveys. Specifically, we use the Integrated Public Use Micro-

data Series (IPUMS) CPS (Flood et al., 2015) provided by the Minnesota Population Center for 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 analysis of hours worked, we use the CPS Merged Outgoing Rotation Groups (MORG) provided by the National Bureau of Economic Research (NBER) for analysis of hours worked. 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.[11]

When constructing our synthetic control, we use a number of state characteristics observed during the pre-treatment period. We calculate share of population in one of three educational categories: less than a high school degree, high school degree, and at least some college. We additionally measure share female and share of population in one of 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, repare, and personal services; and (5) entertainment and recreation, professional and related services, public administration, and active duty military.

[11] 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.

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. Meanwhile, the $\mathbf{X}$ variables used in equation (3) include age, female share, industry, education and average employment in the per-period. We generally are 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.1 — the states include Utah, Wyoming, Washington, Nevada, Montana, and Minnesota.

Figure 1a plots the employment rate 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 generally track each other in the pre-period. This pattern generally continues during the post-period — even though we only use four 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 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 1b 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. As expected, the placebo differences are on average very close to zero — -0.001. Moreover, the actual treatment difference for employment lies squarely inside the range of placebo differences.

Using our placebos, we can extend 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 relative to all placebos, and find a relatively high quality match. In Table 2, column (1), we rank within the top 10 percent match 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 compliment 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 impressive a fit in the pre-period as when employment is used at the outcome — the RMSE is in the bottom half 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 Alasksa 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. In both instance, 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 just outside of the top 10 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 Alasak in this case is comprised of Oklahoma, Wyoming, Kansas, the District of Columbia, and Nevada (see Appendix Table A.1).

Figure 2a plots the part-time rate 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 two few treatment years. However, the estimate 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 0.025 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 2b, where the actual difference in Alaska is generally found near the upper limit of placebo differences.

As a secondary measure of intensive margin effects, we examine reported hours worked

in the prior week. 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 only among the best 30 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.

5.3 Additional results and robustness tests

In our main specification, we include the average value of the outcome of interest during the pre-period, $\bar{y}_s^p$, in our vector of predictor variables, $\mathbf{X}$. An alternative suggested by Kaul et al. (2015) is to instead use only the last observed outcome in the pre-period, y_{s,T_0}. In Table 3 we report estimates of the treatment effect for our two primary outcomes, the employment and part-time rates. Our point estimates are quantitatively very similar, although our effect on the part-time rate is no longer distinguishable from zero. Further details regarding the pre-period match and synthetic states and weights are available in Appendix A.

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 three years of the dividend in Table 4. Using only placebos during this time period results a poorer relative fit in the pre-period for all outcomes, especially for labor force participation and hours work last week. 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. We will return to these patterns below where we offer an interpretation of our results.

Another approach to constructing our synthetic control involves using a common set of weights across outcomes, in order 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. First, the pre-period average of both employment and part-time are included in the vector $\mathbf{X}$ in equation (3). Second, sum of squares minimized in equation (4) is now summed over both sets of outcome variables. In Table 5 we present the results of this alternative approach. The relative fit of our match during the pre-period is somewhat weaker than when we consdier each outcome separately — the RMSE percentile is now only 31 percent. In this case, we now estimate a positive increase in the employment rate. On the other hand, our point estimate for the part-time rate is slightly smaller than in our main specification, and has now become just marginally insignificant. While there is some movement in our point estimates, we continue to find no evidence of an extensive margin reduction in employment and suggestive evidence of an intensive margin reduction.

Finally, in Table 6 we attempt to conduct separate analysis among the male and female populations, again focusing on the employment rate and the part-time rate. In the case of the employment rate, we are no longer able to achieve satisfactory pre-period fit, failing close to or below the lowest 15 percent of matches. Our fit for the part-time rate, however, remains stable after splitting the sample by sex. The estimates suggest that the increase in part-time work may be driven by adjustments among female workers — the treatment effect on part-time for women is relatively large (2.2 percent) and significant at the five percent level, while the estimate for men is trivial (0.8 percent) and insignificant at the 95 percent confidence level.

5.4 Discussion

Although theory and prior estimates suggest that the individual level labor supply response to positive income shocks lead to reductions in both the probability of being employed and

hours worked, we find no evidence of a decrease on the extensive margin. One way to reconcile our results is to consider the general equilibrium effects of transfer income universally and on a permanent basis. In particular, a consumption response could result in an outward shift in labor demand, offsetting the partial equilibrium effects of cash transfers.

How significant are the general equilibrium impacts on labor supply demand? We can construct a back-of-the-envelope this impact by using existing estimates of the individual-level income elasticity of labor supply, the average household dividend amount, and prior estimates on the consumption response to the dividend ... **Calculations forthcoming**

Other features of the patterns we observe are potentially consistent with prior evidence on labor supply responses to income shocks. First, while we argue that general equilibrium factors may offset the extensive margin effects of the dividend, we continue to find intensive effects in our primary specification. This is consistent with the fact that the intensive margin responses to income shocks have been found to be larger than extensive margin responses (Cesarini et al., Forthcoming). Furthermore, the intensive margin responses we find appear to take a few years to take effect, as has been shown in other studies as well (e.g. Gelber et al., 2013; Cesarini et al., Forthcoming). Finally, the point estimates during the first years of the dividend suggest marginally more positive employment effects and smaller increases in part-time, which could be consistent with a more quickly adjusted consumption response to the dividend (Kueng, 2015) than the labor supply response.

6 Conclusion

Forthcoming

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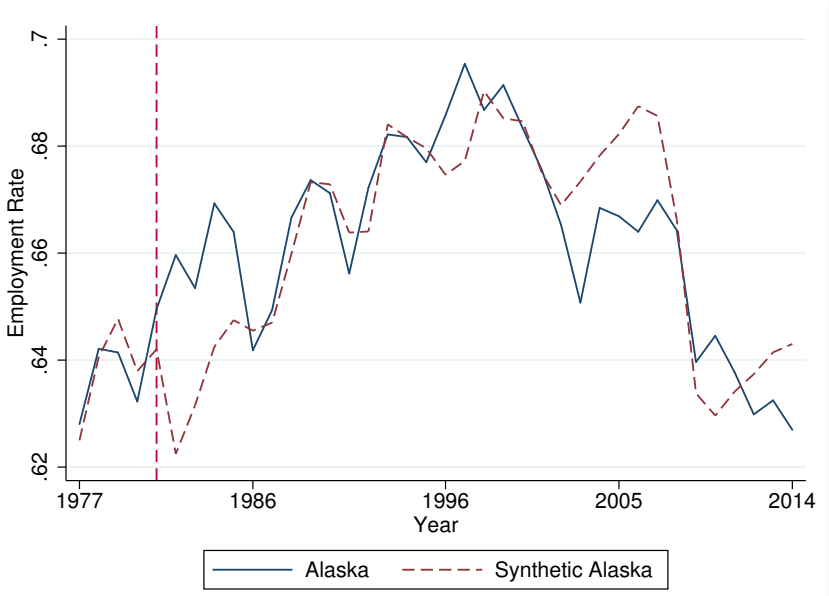
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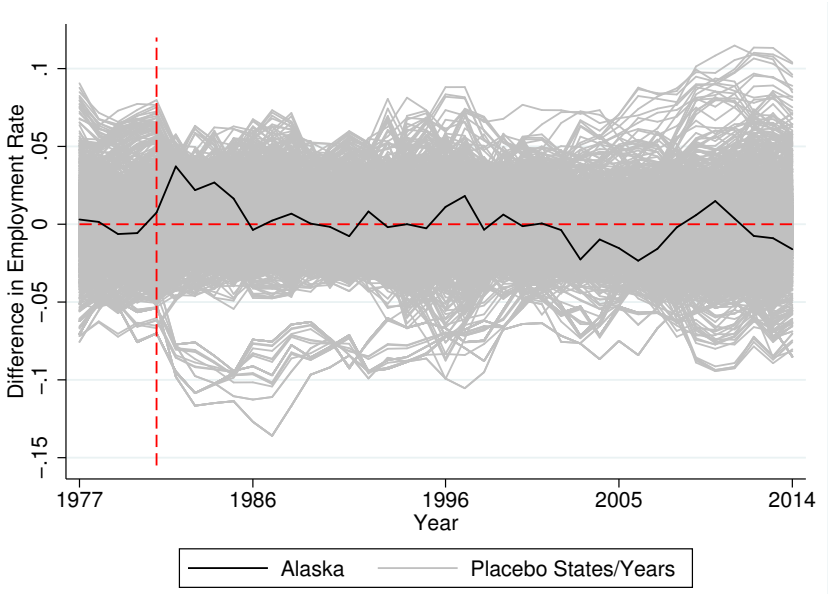
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Figure 1: Employment Rate, 1977-2014



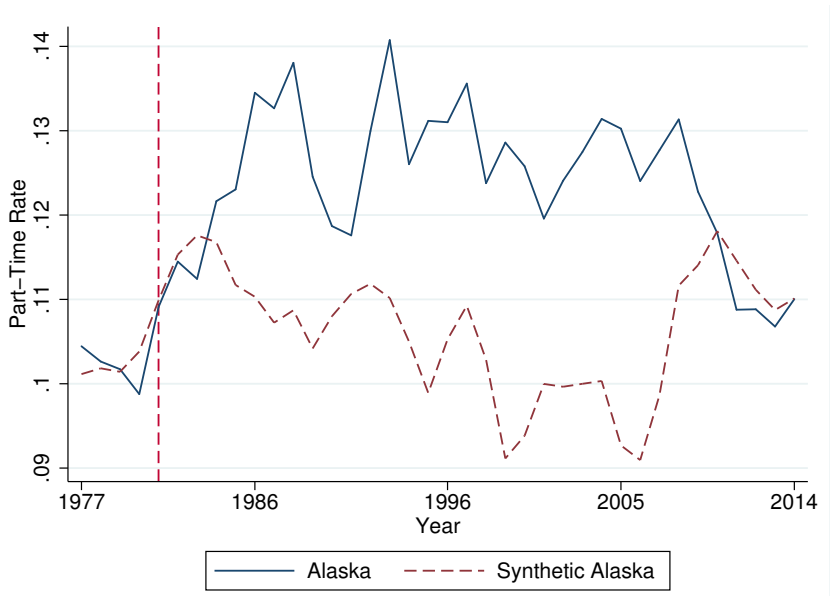
(a) Employment Rate: Alaska vs. Synthetic Alaska



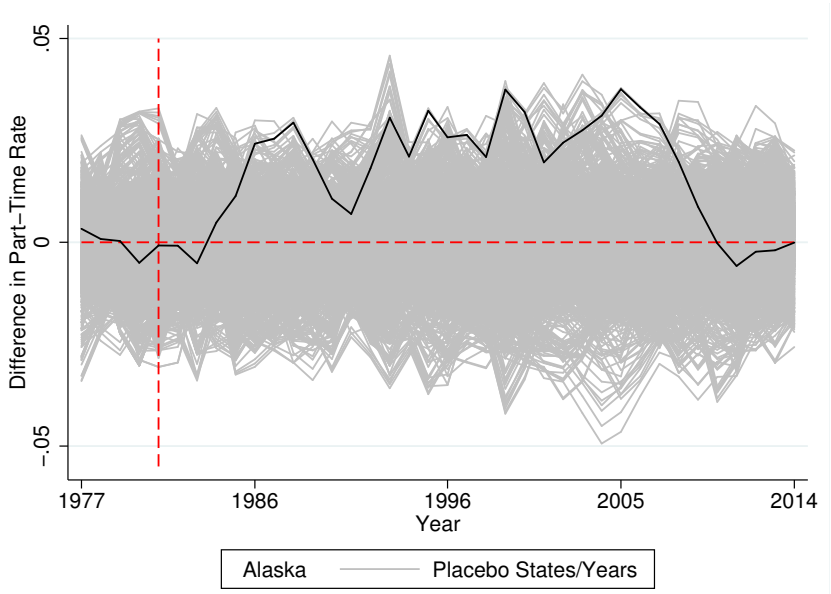
(b) Synthetic Difference in Employment Rate, Alaska vs. Placebo States

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.1 for the combination of states and weights that comprise each synthetic control.

Figure 2: 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.1 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 Female	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	37.977	37.955		
Age 16 - 19	0.074	0.072		
Age 20 - 24	0.155	0.145		
Age 25 - 65	0.759	0.744		
Share Female	0.435	0.429		
Industry Group 1	0.148	0.168		
Industry Group 2	0.051	0.124		
Industry Group 3	0.292	0.261		
Industry Group 4	0.123	0.151		
Education $\leq$ 11 years	0.110	0.173		
Education = 12 years	0.387	0.378		

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.1 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	-0.617
p-value	0.942	0.025	0.335	0.156
95% CI	[-0.031,0.032]	[0.004,0.032]	[-0.021,0.042]	[-1.577,0.324]
Number of placebos	1,836	1,836	1,836	1,734
Pre-Period RMSE	0.0053	0.0027	0.0125	0.3613
RMSE Percentile	0.095	0.105	0.568	0.279

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.1 for the combination of states and weights that comprise each synthetic control.

Table 3: Synthetic Control Estimates, Average Difference 1982-2014, Last Year Approach

	(1)	(2)
	Employment Rate	Part-Time Rate
$\hat{\alpha}_0$	-0.002	0.017
p-value	0.261	0.163
95% CI	[-0.020,0.072]	[-0.041,0.054]
Number of placebos	1,836	1,836
Pre-Period RMSE	0.0063	0.0029
RMSE Percentile	0.157	0.091

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on the employment rate and the part-time rate, 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.4 for the combination of states and weights that comprise each synthetic control.

Table 4: 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	0.523
p-value	0.106	0.669	0.092	0.196
95% CI	[-0.009,0.060]	[-0.011,0.017]	[-0.007,0.048]	[-0.408,1.436]
Number of placebos	357	357	357	255
Pre-Period RMSE	0.0053	0.0027	0.0125	0.3613
RMSE Percentile	0.443	0.431	0.922	0.729

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 1987. 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.1 for the combination of states and weights that comprise each synthetic control.

Table 5: 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	[0.003,0.062]	[-0.005,0.028]
Number of placebos	1,836	1,836
Pre-Period RMSE	0.0050	0.0050
RMSE Percentile	0.312	0.312

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on the employment rate and part-time rate, using the synthetic control method outlined in Section 3, where the weights are selected using pre-treatment employment and part-time jointly. 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.4 for the combination of states and weights that comprise each synthetic control.

Table 6: Synthetic Control Estimates, Average Difference 1982-2014, by Sex

	(1)	(2)	(3)	(4)
	Male		Female	
	Employment Rate	Part-Time Rate	Employment Rate	Part-Time Rate
$\hat{\alpha}_0$	0.029	0.008	-0.019	0.022
p-value	0.091	0.200	0.231	0.034
95% CI	[-0.008,0.066]	[-0.004,0.019]	[-0.055,0.016]	[0.003,0.042]
Number of placebos	1,836	1,836	1,836	1,836
Pre-Period RMSE	0.0237	0.0033	0.0263	0.0041
RMSE Percentile	0.839	0.090	0.876	0.126

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on the employment rate and part-time rate, 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.1 for combination of states and weights that comprise each synthetic control.

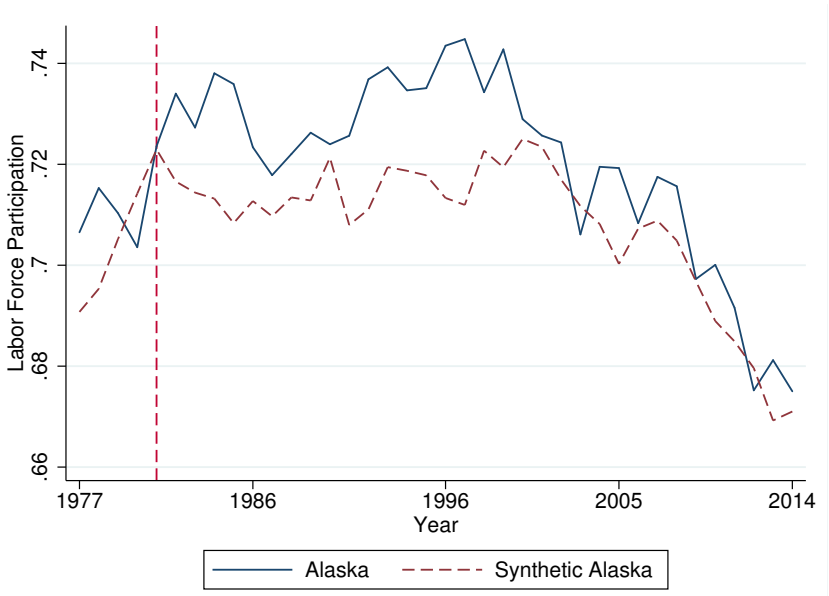
Appendix A: Appendix Tables and Figures

Table A.1: State Weights for Synthetic Alaska

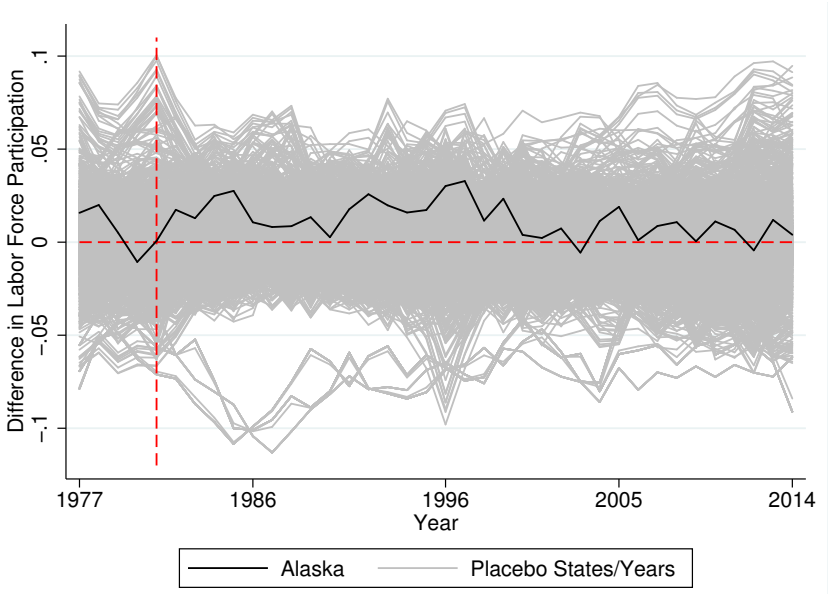
State	Weight
Panel A: Employment Rate	
Utah	0.428
Wyoming	0.342
Washington	0.092
Nevada	0.079
Montana	0.034
Minnesota	0.025
Panel B: Part-Time Rate	
Oklahoma	0.449
Wyoming	0.165
Kansas	0.164
District of Columbia	0.153
Nevada	0.069
Panel C: Labor Force Participation	
Nevada	0.373
Minnesota	0.306
Wyoming	0.301
Wisconsin	0.020
Panel D: Hours Worked Last Week	
Oklahoma	0.449
Wyoming	0.165
Kansas	0.164
District of Columbia	0.153
Nevada	0.069

Notes: Table reports the combination of states and weights chosen using the method in Section 3 to construct a synthetic control for Alaska.

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



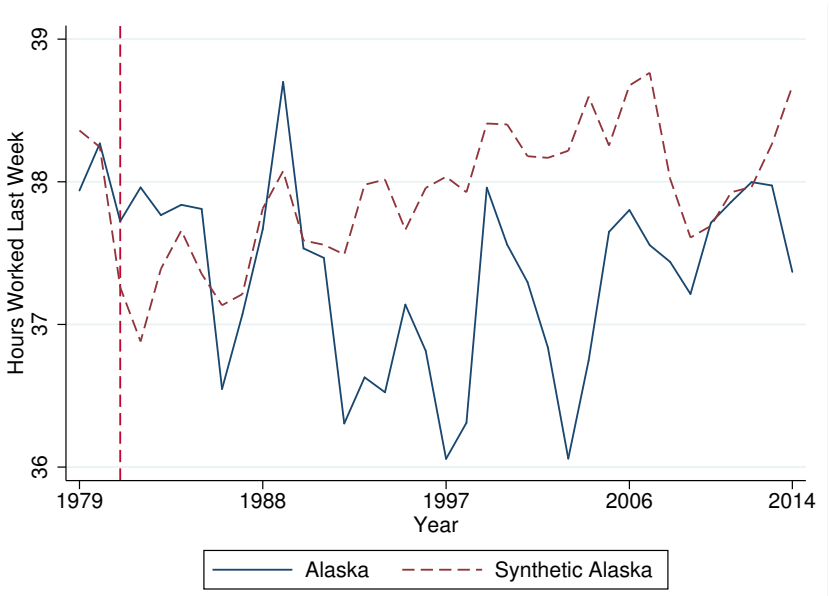
(a) Labor Force Participation: Alaska vs. Synthetic Alaska



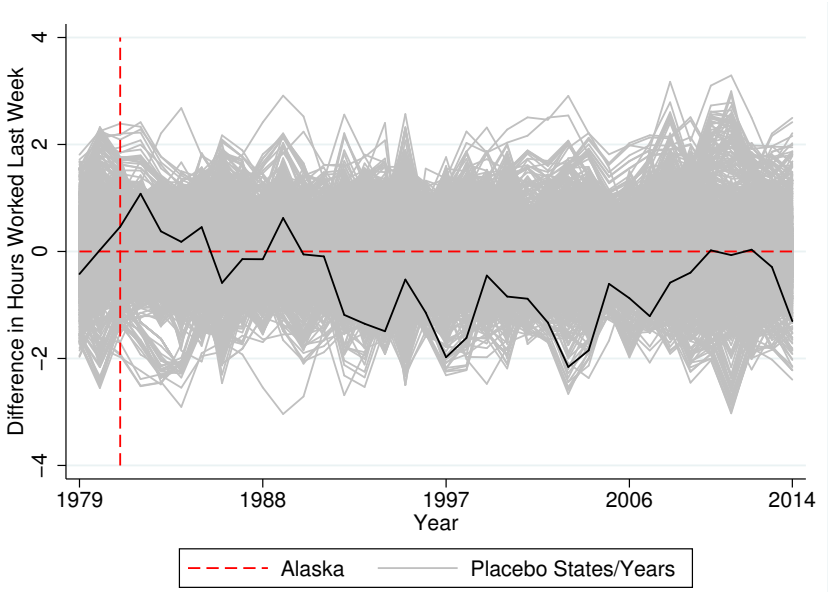
(b) Synthetic Difference in Labor Force Participation, Alaska vs. Placebo States

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.1 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.1 for the combination of states and weights that comprise each synthetic control.

Table A.2: Pre-Treatment Covariate Balance, Last Year Method

	(1)	(2)	(3)
		Synthetic Control Outcome	
	Alaska	Employment Rate	Part-Time Rate
Employment Rate	0.650	0.648	-
Part-Time Rate	0.109	-	0.110
Age 16 - 19	0.108	0.103	0.096
Age 20 - 24	0.154	0.136	0.127
Age 25 - 65	0.691	0.635	0.673
Share Female	0.503	0.513	0.504
Industry Group 1	0.361	0.361	0.344
Industry Group 2	0.097	0.123	0.103
Industry Group 3	0.035	0.075	0.041
Industry Group 4	0.191	0.188	0.185
Industry Group 5	0.078	0.087	0.157
Education $\leq$ 11 years	0.229	0.240	0.266
Education = 12 years	0.396	0.375	0.404

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) and (3) differ in the outcome matched on in equation (4). 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.4 for the combination of states and weights that comprise each synthetic control.

Table A.3: 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 Female	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.4 for the combination of states and weights that comprise each synthetic control.

Table A.4: State Weights for Synthetic Alaska, Last Year and Common Weights Methods

State	Weight
Panel A: Employment Rate (Last Year Method)	
Utah	0.420
Wyoming	0.288
Colorado	0.204
Oklahoma	0.070
Nevada	0.019
Panel B: Part-Time Rate (Last Year Method)	
Nevada	0.697
Wyoming	0.119
Louisiana	0.099
Washington	0.063
Colorado	0.022
Panel C: Employment Rate 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.