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

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Abstract

What are the effects of universal and permanent cash transfers on the labor market? Since 1982, all Alaskan residents have been entitled to 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 percent). Although theory and prior empirical research suggests that individual cash transfers decrease household labor supply, we interpret our results as evidence that general equilibrium effects of widespread and permanent transfers tend to offset this effect, at least on the extensive margin. Consistent with this story, we show suggestive evidence that tradable sectors experience employment reductions, while non-tradable sectors do not. Overall, our results suggest that a universal and permanent cash transfer does not significantly decrease aggregate employment.

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. Hillary Clinton considered a universal basic income modeled after the Alaska Permanent Fund — which we study here — as part of her 2016 campaign proposals.[1] 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.[2] In Hawaii, House Concurrent Resolution 89 passed in 2017: it established a Basic Economic Security Working Group focused on financial security to all families, including an evaluation of basic income policies.[3] 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[4] and the Canadian province of Ontario[5] are running basic income experiments for disadvantaged populations.

Policy makers may be concerned that a universal basic income 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

[1]https://www.vox.com/policy-and-politics/2017/9/12/16296532/hillary-clinton-universal-basic-income-alaska-for-america-peter-barnes
[2]https://www.wired.com/2016/10/president-obama-mit-joi-ito-interview/
[3]https://www.weforum.org/agenda/2017/06/hawaii-has-become-the-first-state-to-support-universal-basic-income
[4]https://www.theguardian.com/society/2017/feb/19/basic-income-finland-low-wages-fewer-jobs
[5]https://www.ontario.ca/page/ontario-basic-income-pilot

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 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, yet 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.[6] 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 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.

Our primary analysis focuses on two outcomes for which very well-matched synthetic controls could be constructed: the employment to population ratio and the share working

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

part time. For these two outcomes, better controls could be found for Alaska than for 75% 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. That is, the unconditional cash transfer could result in consumption increases that stimulate labor demand and mitigate potential reductions in employment. The impact on labor demand should be especially pronounced in the non-tradable sector. We show evidence consistent with this hypothesis: indeed, the estimated effects of the dividend on both employment and part-time work are sizeable in the non-tradable sector, but are close to zero in the tradable sector. The positive impact of the cash transfer on part-time work is consistent with a negative income effect on the intensive margin. However, some specifications suggest a marginally positive employment effect, which, in combination with the increase in part-time worked, could indicate new entrants into employment who work part-time. These new entrants may be 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 inconsistent with large aggregate reductions in employment.

An alternative interpretation of our results is that the size of the average Alaska Permanent Fund dividend is too small to affect labor supply. However, it should be noted that the dividend is paid on a *per person* basis — with a dividend of $2,000, a family of four receives $8,000. Sizable employment effects have been found in the case of the EITC, and, at the level of dividend payments in the previous example, the total household dividend exceeds the maximum EITC credit. Moreover, Cesarini et al. (Forthcoming) do not find strong evidence of nonlinearities in the income effect, which suggests that our evidence might be relevant for cash transfers of larger magnitude.

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 we 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 finance and optimal income tax literature, a universal basic income 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.

A universal basic income shares 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.

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 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.[7]

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.[8] 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, 40 percent of respondents say the yearly dividends have made a great deal or quite a bit

[7] http://www.apfc.org/
[8] https://pfd.alaska.gov/Division-Info/Summary-of-Applications-and-Payments

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. 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 (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, $\bar{y}_s^p = \frac{1}{T_0}\sum_{t=1}^{T_0} y_{st}$.[9] 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 $t \in \{T_0+1,\ldots,T\}$.[10] 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:

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

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

[9] 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.

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

$w_s \geq 0$, 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 $W^{OLS} = \mathbf{X}_c' \left(\mathbf{X}_c \mathbf{X}_c'\right)^{-1} \mathbf{X}_0$. 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 10th 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. 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).[11]

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 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 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

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

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.[12]

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 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).

[12] 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 for column (2), the labor force participation in each pre-treatment year for column (3), and so forth. Meanwhile, the $\mathbf{X}$ variables used in equation (3) include age, female share, industry, education and average employment 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.1 — the states include Utah, Wyoming, Washington, Nevada, Montana, and Minnesota.[13] 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 1a plots the employment rate for Alaska and synthetic Alaska from 1977 to 2014.

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

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 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 mean of the placebo differences is very close to zero — -0.0004 — 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 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 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 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 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 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, Table A.1, 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 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 Alaska 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 (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 0.025 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 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 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, Table A.1, and Figure A.2.

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 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, Tables A.3 and A.5.

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 Table 4. Using only placebos during this time period results in a poorer relative fit in the pre-period for all outcomes[14], especially for labor force participation and hours worked 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 our two main outcomes, employment to population and part-time to population. 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. 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 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. Overall, while there is some movement in our point estimates when using common weights, we continue to find no evidence of a negative effect of an extensive margin reduction in employment and suggestive evidence of an intensive margin reduction.

In Table 6 we 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

[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.

rate, we are no longer able to achieve satisfactory pre-period fit, falling among the lowest 17 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.

Finally, we test the robustness of our results to matching Alaska to control states using a longer pre-period. The CPS does not have data on Alaska prior to 1977. However, we can augment our analysis using Census data from 1960 and 1970. Using a longer pre-period yields a worse match for Alaska: the pre-period RMSE is above the 60th percentile (Appendix Table A.6). The employment effects become more positive when using a longer pre-period: whether using data from 1960 and 1970 or from 1970 only, the dividend is found to increase the employment to population ratio by about 3 percentage points, and the effect is significant at the 10 percent level. These results reinforce the conclusion that the Alaska Permanent Fund dividends are unlikely to have reduced the employment to population ratio.[15]

5.4 Discussion

Although theory and some 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 find no evidence of a decrease on the extensive margin. One way to reconcile our results with these prior findings is to consider the general equilibrium effects of transfer income universally and on a permanent basis. In particular, a cash transfer can lead to an increase in consumption, and therefore an increase in employment in the non-tradable sector. Maggio et al. (2016) show evidence for this channel exploiting the increase in unemployment insurance transfers during the Great Recession. In the case of Alaska, the consumption response to the dividend could result in an outward shift in labor demand,

[15]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.

offsetting the partial equilibrium effects of cash transfers.

To test for the plausibility of this demand channel, we re-estimate the impact of the dividend on employment and part-time status separately for industries in the tradable and the non-tradable sector. The results are presented in Appendix Table A.7. 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 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.

We construct a back of the envelope calculation of the implied macro labor demand effect, assuming that the micro labor supply effect is the same as in prior literature. According to the estimates by Cesarini et al. (Forthcoming), a \$140K transfer leads to a 2 percentage points reduction in employment. If we assume a value of \$2,000 for the Alaska Permanent Fund Dividend, discounted over 45 years, with a 0.95% yearly discount rate, this leads to a present discounted value of the dividend of about \$36K. Applying the effect size from Cesarini et al. (Forthcoming), the Alaskan dividend should reduce employment by 0.5 percentage point in the absence of any macro effect.

Our point estimate for employment is 0.001. To make up for the negative 0.5 percentage point micro effect, we need an employment multiplier of 0.5. The empirical estimates of the employment and GDP multipliers based on prior literature are typically above 1 (Ramey, 2011; Chodorow-Reich, 2017). We therefore do not need an implausibly large multiplier to make sense of a null effect of the Alaska Permanent Fund dividend on employment. When we use common weights for both the part-time and the employment outcomes, the effect on employment is 3.2 percentage points (Table 5); when we use a longer pre-period for the match, the effect on employment is also positive and in this range (Appendix Table A.6). A 3.2 percentage point effect on employment to population corresponds to an employment multiplier of about 1.6, which is close to the consensus estimate from the literature.

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 negative intensive margin effects on labor supply 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 consumption response to the dividend (Kueng, 2015) that adjusts more quickly than the labor supply response.

There may be a concern 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 just under $5,000 per year. This is on the order of the maximum EITC credit, which has been demonstrated to significantly affect employment. Now, these amounts may still be smaller than what would be expected under a universal basis income policy. However, Cesarini et al. (Forthcoming) 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.

A final consideration involves the financing of a universal basic income. In order 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 countefactual 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 any 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 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 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, we find 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, the evidence is consistent with positive macro effects offsetting any negative micro effects, and leading to an overall null effect of an unconditional cash transfer on aggregate employment in the long-run.

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 universal basic income on the labor market. Our results show that adverse labor market effects are limited, and, importantly, a universal and unconditional cash transfer does not significantly reduce aggregate employment. Future research should investigate how the mode of financing of a universal basic income affects its impact, and how a universal basic income interacts with existing social welfare programs.

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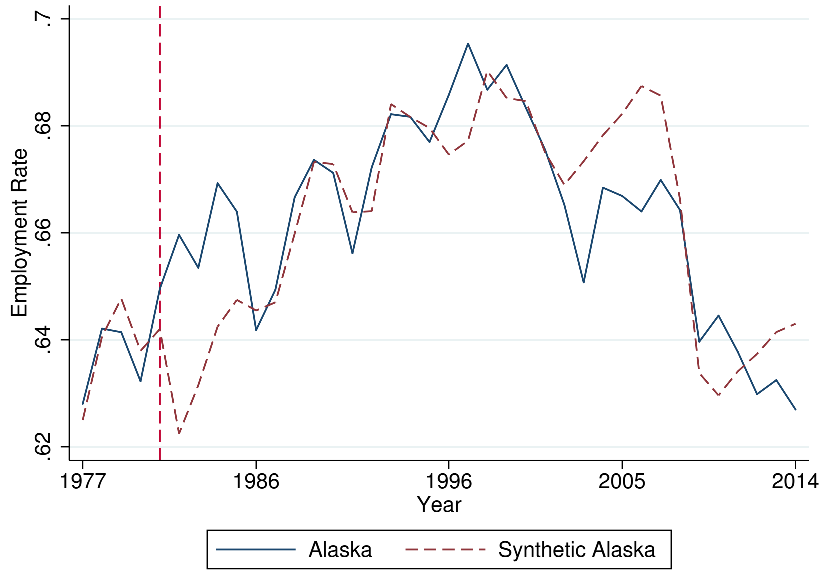
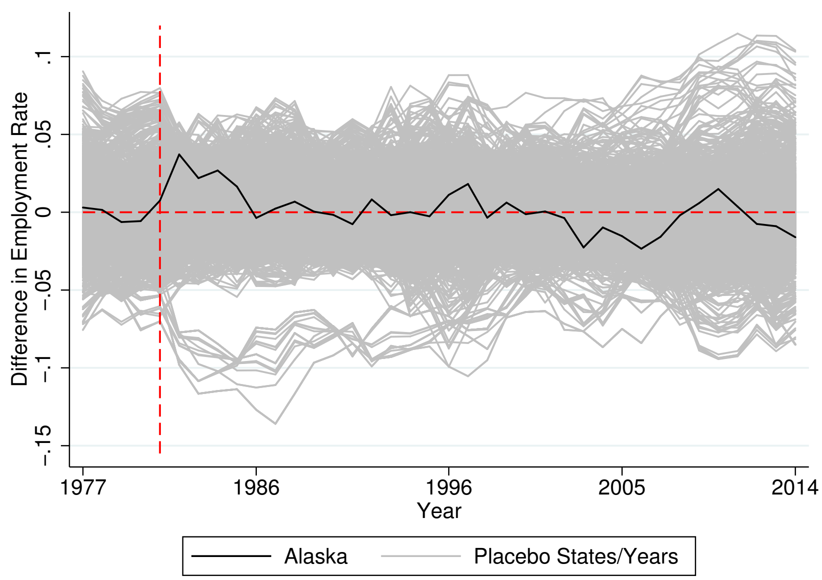
Figure 1: Employment Rate, 1977-2014



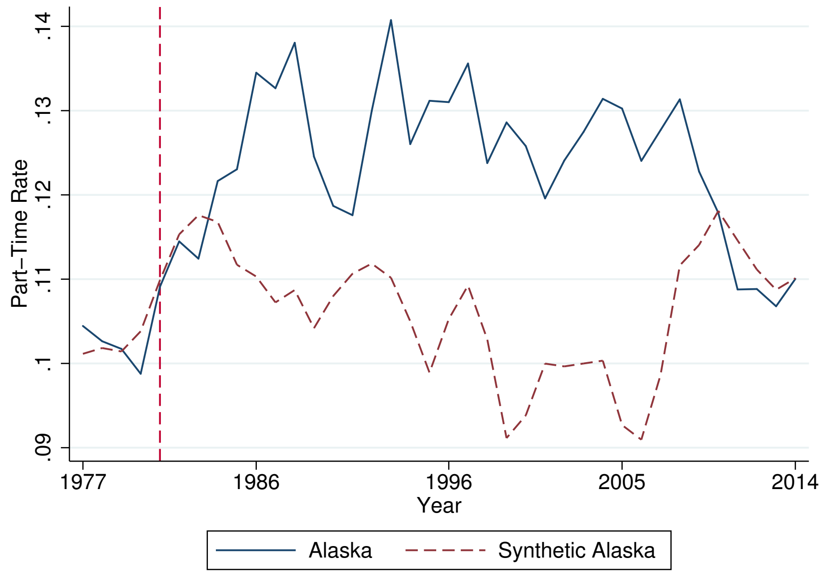
(a) Employment Rate: Alaska vs. Synthetic Alaska



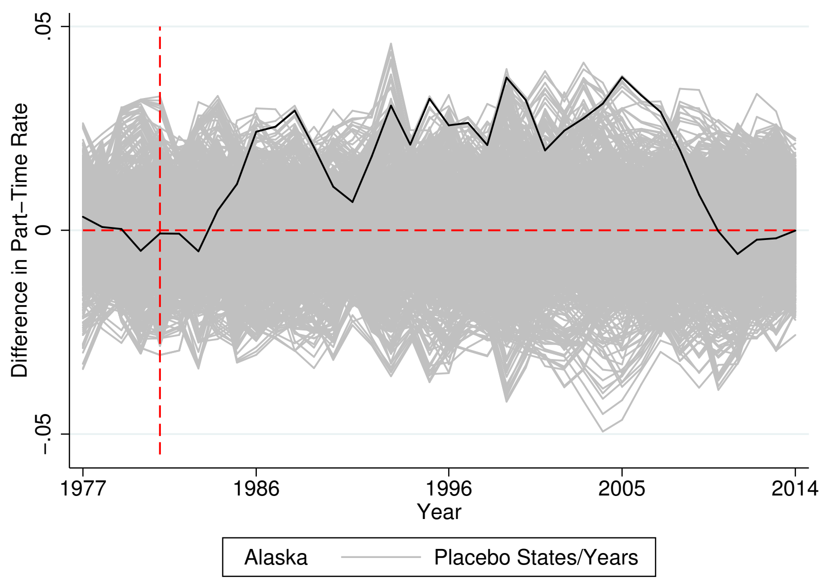
(b) Synthetic Difference in Employment Rate, Alaska vs. Placebo States

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.5 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.5 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.2 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

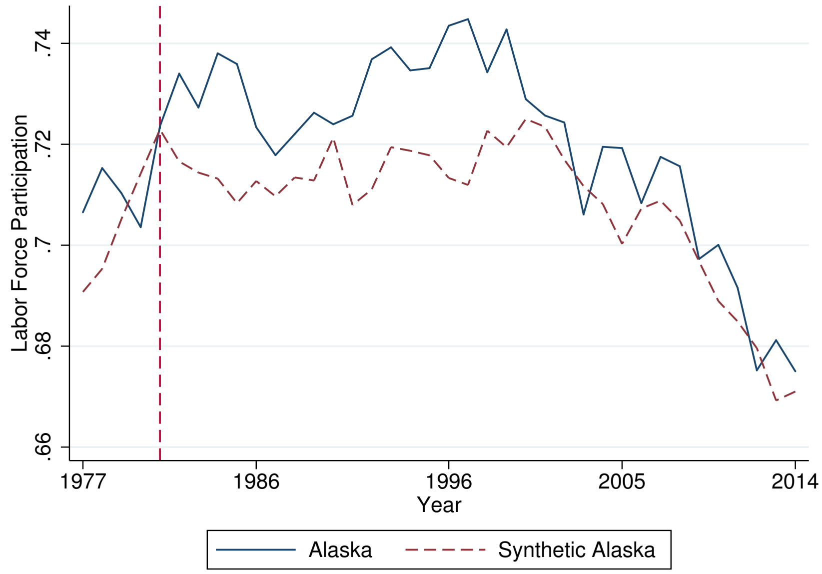
Notes: Table reports the combination of states and weights chosen using the method in Section 3 to construct a synthetic control for Alaska. Panels A through D correspond to columns (1) through (4) in Table 2.

Table A.2: State Weights for Synthetic Alaska

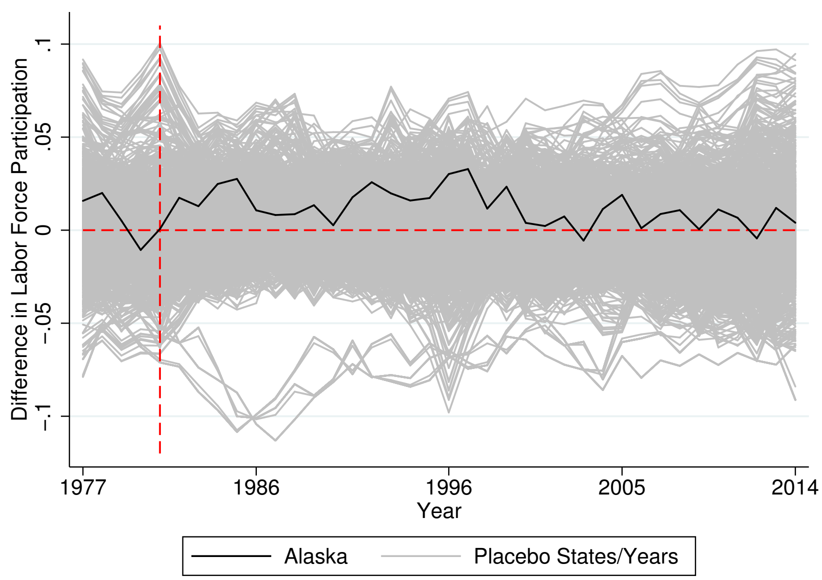
State	Weight
Panel A: Employment Rate – Male	
Montana	0.511
Washington	0.371
District of Columbia	0.081
Florida	0.037
Panel B: Part-Time Rate – Male	
Wyoming	0.340
Maryland	0.191
District of Columbia	0.185
Washington	0.133
Nevada	0.095
Pennsylvania	0.055
Panel C: Employment Rate – Female	
Minnesota	0.848
Wyoming	0.110
Nevada	0.041
Panel D: Part-Time Rate – Female	
Oklahoma	0.720
Kansas	0.129
Wyoming	0.081
District of Columbia	0.065
Texas	0.004

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 6.

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



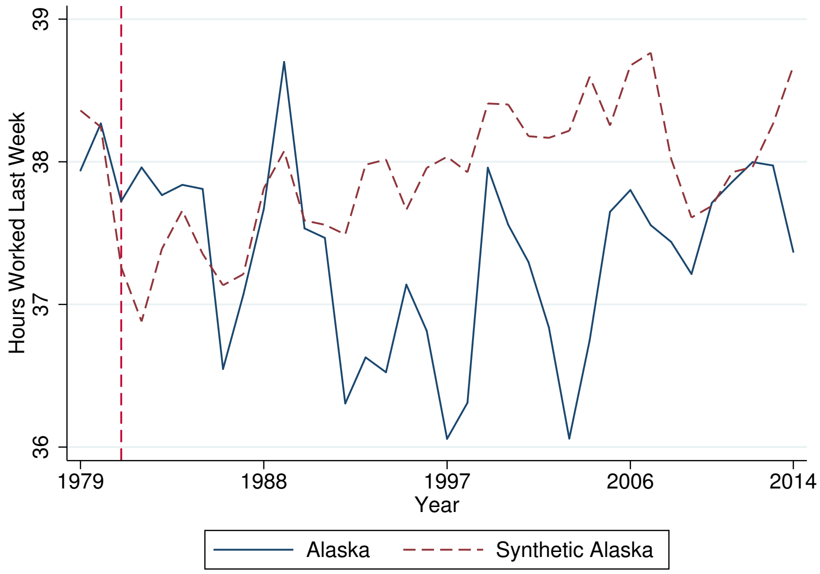
(a) Labor Force Participation: Alaska vs. Synthetic Alaska



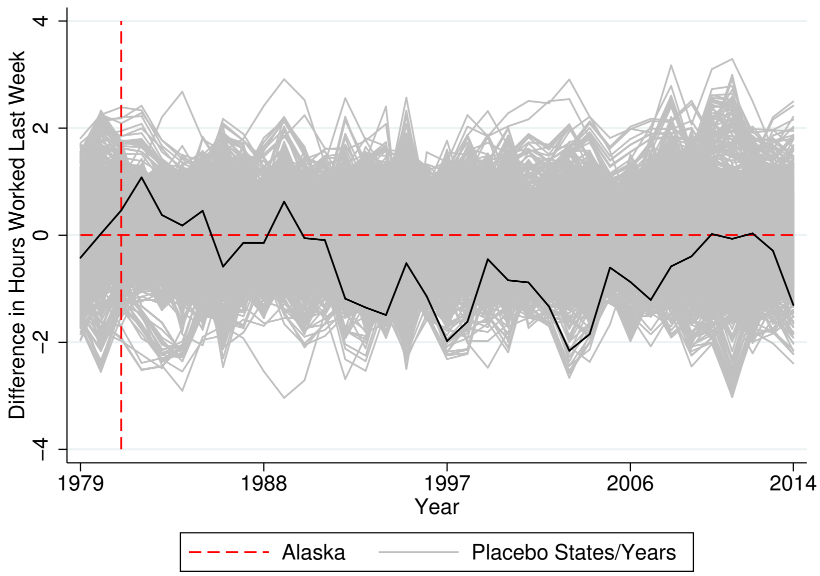
(b) Synthetic Difference in Labor Force Participation, Alaska vs. Placebo States

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.3: 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.5 for the combination of states and weights that comprise each synthetic control.

Table A.4: 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.5 for the combination of states and weights that comprise each synthetic control.

Table A.5: 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. Panels A and B correspond to Table 3, while Panel C corresponds to Table 5.

Table A.6: 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	0.027	0.030
p-value	0.942	0.048	0.046
95% CI	[-0.031,0.032]	[0.000,0.061]	[0.000,0.062]
Number of placebos	1,836	1,836	1,836
Pre-Period RMSE	0.0053	0.0110	0.0110
Percentile	0.095	0.345	0.321

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on the employment rate, using the synthetic control method outlined in Section 3. The treatment effect is averaged over the years 1982 to 2014. Estimates differ by length of pre-period — data for 1970 and 1960 come from the decennial census data. In column 3, Census data from both 1960 and 1970 is used. 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 pre-treatment data, and percentile is based on a comparison among all placebo estimates. See Appendix Table A.8 for the combination of states and weights that comprise each synthetic control.

Table A.7: 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
$\hat{\alpha}_0$	-0.047	0.019	0.005	0.000
p-value	0.013	0.070	0.651	0.997
95% CI	[-0.068,-0.024]	[-0.003,0.041]	[-0.021,0.034]	[-0.032,0.031]
Number of placebos	1,836	1,836	1,836	1,836
Pre-Period RMSE	0.0621	0.0109	0.0441	0.0121
RMSE Percentile	0.700	0.508	0.973	0.201

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

Table A.8: State Weights for Synthetic Alaska: Different pre-periods, Tradable, and Non-Tradable Sectors

State	Weight
Panel A: Employment Rate: Pre-period beginning with 1977	
Utah	0.428
Wyoming	0.342
Washington	0.092
Nevada	0.079
Montana	0.034
Minnesota	0.025
Panel B: Employment Rate: Pre-period beginning with 1970	
Hawaii	0.737
Nevada	0.256
Wyoming	0.006
Panel C: Employment Rate: Pre-period beginning with 1960	
Hawaii	0.752
Nevada	0.248
Panel D: Employment Rate: Tradable Sector	
Oregon	1.000
Panel E: Part-Time Rate: Tradable Sector	
Utah	0.614
Arizona	0.167
Michigan	0.097
Louisiana	0.065
Idaho	0.065
Washington	0.028
Panel F: Employment Rate: Non-Tradable Sector	
West Virginia	0.711
District of Columbia	0.289
Panel G: Part-Time Rate: Non-Tradable Sector	
Louisiana	0.660
District of Columbia	0.322
Nevada	0.018

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 C correspond to columns (1) through (3) of Table A.6, while Panels D through G coorespond to columns (1) through (4) of Table A.7.

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

	(1)	(2)	(3)	(4)
	Health/Hospitals	Education	Highways	Welfare/Transfers
$\hat{\alpha}_0$	-0.005	-0.080	0.033	0.007
p-value	0.706	0.012	0.025	0.791
95% CI	[-0.034,0.025]	[-0.131,-0.024]	[0.005,0.061]	[-0.050,0.061]
Number of placebos	1,836	1,836	1,836	1,734
Pre-Period RMSE	0.0128	0.0139	0.0215	0.0080
Percentile	0.778	0.268	0.803	0.180

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.10: State Weights for Synthetic Alaska: Government Spending

State	Weight
Panel A: Health/Hospitals	
Nevada	1.000
Panel B: Education	
Wyoming	0.606
Wisconsin	0.329
California	0.033
Massachusetts	0.032
Panel C: Highways	
Nevada	0.520
South Carolina	0.290
California	0.133
Wyoming	0.033
Utah	0.023
Panel D: Welfare/Transfers	
Wisconsin	0.919
South Carolina	0.081

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) of Table A.9.