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

By Damon Jones and Ioana Marinescu*

Since 1982, all Alaskan residents have received a yearly cash dividend from the Alaska Permanent Fund. Using the Current Population Survey and a synthetic control method, this paper shows that the dividend had no effect on employment and increased part-time work by 1.8 percentage points (17 percent). A calibration of microeconomic and macroeconomic effects suggests that the empirical results are consistent with cash stimulating the local economy—a general equilibrium effect. Nontradable sectors have a more positive employment response than tradable sectors. Overall, the results suggest that a universal and permanent cash transfer does not significantly decrease aggregate employment. (*JEL* E24, H24, H75, I38, J22, R23)

The effect of cash transfers on labor market outcomes is of central interest in a number of areas, including the design of tax policy, means-tested transfers, and public pension programs. One key concern is that cash transfers could discourage work through an income effect. A number of studies based on the Negative Income Tax (NIT) experiments of the 1970s (Robins 1985; Price and Song 2016) and evidence from lottery winners (Imbens, Rubin, and Sacerdote 2001; Cesarini et al. 2017) reliably estimate an income effect of approximately −0.1 in developed countries, implying that a 10 percent increase in unearned income will reduce earned income by about 1 percent (see Marinescu 2018 for an overview). By contrast, a study of the Eastern Band of Cherokee Indians, who receive an unconditional transfer from casino profits, found no labor supply effect (Akee et al. 2010). While lottery studies leverage ideal exogeneity and the case study of the Eastern Band of Cherokee Indians involved a permanent dividend, these transfers accrue to small shares of the total population and therefore identify a microeconomic 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

* Jones: University of Chicago, Harris School of Public Policy (email: damonjones@uchicago.edu); Marinescu: University of Pennsylvania, School of Social Policy and Practice, and NBER (email: ioma@upenn.edu). Matthew Shapiro was coeditor for this article. We would like to thank Nina Kerkebane, Kohei Matsumoto, Robert Ross, Joaquin Lennon Sabatini, Shambhavi Sawhney, and Emily Sun for excellent research assistance. We are also thankful for comments from Gabriel Chodorow-Reich and Lorenz Kueng and workshop participants at the Harris School, ENSAI, Temple University, the University of Pennsylvania, NBER Labor Studies, the AEA Annual Meetings, the University of Wisconsin Institute for Research on Poverty, and the OECD Paris. Any opinions expressed here are those of the authors and not of any institution.

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and unconditional cash transfer will affect the labor market equilibrium and likely alter long-term expectations, yet little is known about the long-run, macroeconomic impact of this policy.

To analyze the long-run impact of a universal and unconditional cash transfer on the labor market, we examine the case of the Alaska Permanent Fund dividend, an annual cash payment to all Alaskan residents. In our setting, everyone within the same state receives a transfer, leaving no natural within-state control group. Furthermore, the universality of the transfer may have macro-level effects on the economy and the labor market. We therefore need to consider the entire state as the unit of observation. Estimating the effect of a policy change in one particular state, Alaska, presents us with the methodological challenge of constructing an appropriate counterfactual. We rely on the synthetic control method proposed in Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010), using data from the 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.

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

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

First, if the dividend only operated through the income effect estimated in Cesarini et al. (2017), the dividend should reduce the employment-to-population ratio in Alaska by about 1.7 percentage points. On the other hand, given estimates of the response of state-level employment to local wealth shocks (Chodorow-Reich, Nenov, and Simsek 2020) and federal spending (Chodorow-Reich 2019), the spending of the dividend should increase employment by 1.0 percentage point. The net effect of these two forces is a 0.7 percentage point decrease in employment. Our point estimates range from a 0.1 percentage point increase in employment in our main specification to about 2.8 percentage points in a few alternative specifications.

Our point estimates are therefore larger than what is predicted by the income effect alone and suggest a multiplier effect similar to or somewhat larger than has been calculated in prior studies. Overall, what is clear is that the estimated macroeconomic effects of an unconditional cash transfer on the labor market are inconsistent with large aggregate reductions in employment, though there may be intensive margin reductions.

Second, if there is a macroeconomic effect, the impact on labor demand should be especially pronounced in the nontradable sector. We show suggestive evidence consistent with this hypothesis: the estimated effects of the dividend on both employment and part-time work are sizable in the tradable sector and suggest a reduction in labor supply, but are close to zero in the nontradable sector. These estimates are only suggestive, but they are consistent with a macroeconomic feedback effect on employment.

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

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

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

In addition to the literature on income effects and labor supply mentioned above, our work is relevant to a number of other areas of research. In the public finance and optimal income tax literature, an unconditional cash transfer can essentially be thought of as a demogrant, e.g., the intercept of an NIT schedule. Although a trade-off between redistribution and labor supply disincentives is considered, the standard Mirrlees (1971) model does not take into account the potential general equilibrium effects of cash transfers. Kroft et al. (2015) show that, in a model with unemployment and endogenous wages, the optimal tax formula resembles an NIT more than an Earned Income Tax Credit when the macroeconomic effect of taxes on employment is smaller than the microeconomic effect. Our empirical results are consistent with this setting. Finally, Cunha, De Giorgi, and Jayachandran (2019) provide evidence that cash transfers result in an outward shift in demand for local goods, which is consistent with our preferred interpretation of our results.

An unconditional cash transfer may share properties with means-tested transfers, and thus our results are related to studies on the labor-supply effects of these programs. Recent studies of the labor supply effects of Medicaid have varied widely depending on the state under consideration (see Buchmueller, Ham, and Shore-Sheppard 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, Moore, and Strand 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 of them, these analyses rely on a framework that focuses on labor-supply responses, while our results suggest that general equilibrium factors may matter.

From a policy perspective, our results are relevant to understanding the potential labor market impacts of a universal basic income, an unconditional and universal cash transfer. For example, Hillary Clinton considered a universal basic income modeled after the Alaska Permanent Fund—which we study here—as part of her 2016 presidential campaign proposals.[1] The Democratic primary for the 2020 presidential election in the United States included a candidate—Andrew Yang—who made a universal basic income his key campaign proposal.

The paper is organized as follows: Section I describes the institutional context for the Alaska Permanent Fund. In Section II, we discuss the synthetic control method, and we describe our data in Section III. We present the main results in Section IV. We provide additional results and a discussion in Section V. Section VI concludes.

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

I. 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. Concerns arose after the large windfall of nearly $900 million was quickly spent 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 purpose of the fund was to diversify Alaska's revenue streams by investing a portion of oil royalties more broadly, to ensure that current revenue was preserved in part for future residents, and to constrain discretionary spending by state government officials (O'Brien and Olson 1990). The fund is managed by the Alaska Permanent Fund Corporation, and the current value of the fund as of September 2020 is $65.1 billion.[2]

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

The nominal value of the dividend was as low as $331 in 1984, but has generally exceeded $1,000 since 1996 and peaked in 2015 at $2,072 (see Figure 1 for yearly nominal and real amounts of the dividend).[3] 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, noncitizens 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 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 report a positive effect, and 16 percent report a negative effect. Thus, the majority of Alaskans report that the dividend has little to no effect on work.

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

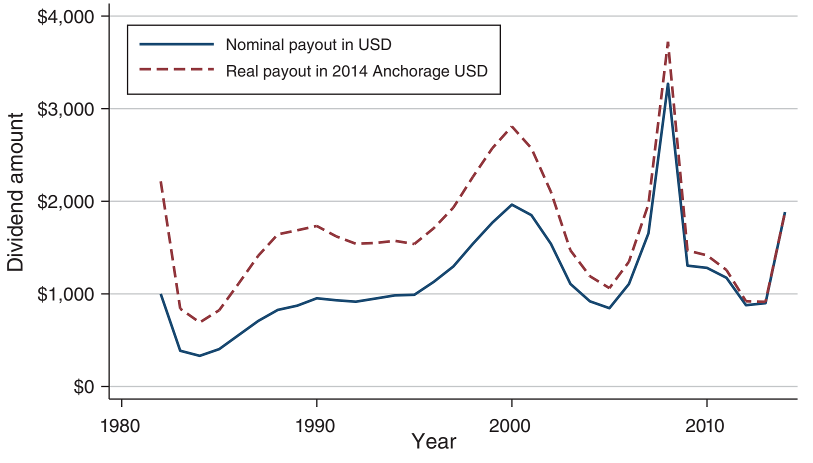

Figure 1. Alaska Permanent Fund Dividend: Nominal and Real Amounts

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

II. 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. We adopt the synthetic control method of Abadie, Diamond, and Hainmueller (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 states, which includes the $S-1$ control states and Alaska. States are indexed by s and observed for T periods. There is one treatment state with $s = 1$, while all other states are controls. The variable d_{st} indicates whether a state s is receiving treatment in period t, taking a value of 0 for all control states. For Alaska, $d_{1t} = 0$ during the pre-intervention period $t \in \{1, \dots, T_0\}$, and $d_{1t} = 1$ starting in period $T_0 + 1$.

We adopt a potential outcomes framework (Rubin 1974):

$$y_{st}(0) = \delta_t + \theta_t \mathbf{Z}_s + \lambda_t \mu_s + \varepsilon_{st} \tag{1}$$

$$y_{st}(1) = \alpha_{st} + y_{st}(0),$$

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 preperiod female share, industry shares, age category shares, and educational category 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 0, 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 $\alpha_{1t} = y_{1t}(1) - y_{1t}(0)$ for $t \in \{T_0 + 1, \ldots, T\}$, i.e., the effect of treatment for the treated state in the postintervention period.

We seek a set of $S - 1$ weights, $\mathbf{w} = (w_2, \ldots, w_S)$, in order to combine the untreated outcomes among control states and provide a reasonable approximation for the counterfactual outcome for the treated state, $y_{1t}(0)$, during periods $t \in \{T_0 + 1, \ldots, T\}$. Following Abadie, Diamond, and Hainmueller (2010), we choose the set of weights that solve the following:

$$\text{(2)} \qquad \mathbf{w}^*(V) = \arg\min_{\mathbf{w}} \left(\mathbf{X}_1 - \sum_{s=2}^{S} w_s \cdot \mathbf{X}_s\right)' \mathbf{V} \left(\mathbf{X}_1 - \sum_{s=2}^{S} w_s \cdot \mathbf{X}_s\right),$$

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, $\bar{y}_s^p = (1/T_0)\sum_{t=1}^{T_0} y_{st}$. The diagonal elements v_k of $\mathbf{V}$ are chosen using a regression-based method. For each pre-intervention period $t \in \{1, \ldots, T_0\}$, the outcome variable y_{st} is regressed on the $K \times 1$ vector of prediction variables, X_s. The resulting k coefficients, β_{kt} for each period t, are used to construct the v_k as follows:

$$\text{(3)} \qquad v_k = \frac{\sum_t \beta_{kt}^2}{\sum_k \sum_t \beta_{kt}^2}.$$

We additionally constrain the weights so that $\sum w_s = 1$ and $w_s \geq 0$ for all $s \in \{2, \ldots, S\}$. Once we have arrived at a set of weights, our estimator for α_{1t} is $\hat{\alpha}_{1t} \equiv y_{1t} - \sum_{s=2}^{S} w_s^*(\mathbf{V}^*) \cdot y_{st}$ for $t \in \{T_0 + 1, \ldots, T\}$. In practice, we report the average difference between the treatment unit and the synthetic control during the period when the dividend is in place in Alaska, $\hat{\alpha}_1 \equiv \frac{1}{T - T_0}\sum_{t=T_0+1}^{T} \hat{\alpha}_{1t}$. The synthetic control estimator can be easily implemented by using the "synth" package in MATLAB, Stata, or R.

To quantify the significance of our estimates, we implement a permutation method suggested by Abadie, Diamond, and Hainmueller (2010), comparing our synthetic control estimate to a distribution of placebo estimates. That is, we implement the above synthetic control procedure for all 50 states and the District of Columbia and repeat this exercise as if the treatment year occurred in each of our observed time periods. In our setting, we use placebo treatment years between 1978 and 2013, and for each placebo treatment year we find synthetic controls for the treated state based on five years of data prior to treatment (or the maximum number of available pretreatment years, if this is less than five years). We define $\hat{\alpha}_{st}$ as the estimate for state s with placebo treatment year t. We then conduct a two-tailed test of the null hypothesis of no effect in our treatment state by comparing the observed estimate for $s = 1$ and true treatment year $t = 1982$ to the empirical distribution of placebo estimates. Specifically, our p-value is defined as follows:

$$p = \frac{\sum_s \sum_t \mathbf{1}\{|\hat{\alpha}_{1,1982}| \leq |\hat{\alpha}_{st}|\}}{N_{st}}, \tag{4}$$

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, Diamond, and Hainmueller 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, Duflo, and Mullainathan (2002).

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:

$$y^*_{st} = \begin{cases} y_{st} & \text{for } s \neq 1 \text{ or } t \leq T_0 \\ y_{st} - \alpha^* & \text{for } s = 1 \text{ and } t > T_0 \end{cases}. \tag{5}$$

Using this transformed data, we recalculate a p-value using equation (4) and label this parameter p_{α^*}. Our 95 percent confidence interval is then defined as the set $\left\{ \alpha^* \middle| p_{\alpha^*} > 0.05 \right\}$, i.e., the set of null effects we cannot reject given the data.

The synthetic control estimator is not guaranteed to deliver a good fit for the treated unit. This depends on whether $\mathbf{X}_1$ 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 the pre-intervention fit is sufficiently close. Following Abadie, Diamond, and Hainmueller (2010) we calculate the root-mean-square error (RMSE) for pre-intervention outcomes for our main estimate and for each of our placebo estimates. We rank the RMSE across all placebos and adopt the conservative approach of focusing our discussion on outcomes where the RMSE for Alaska using the true treatment period has a low rank. The RMSE for our two primary outcomes, employment and part-time work, is at or below the thirty-second percentile in our main specification.

III. Data

We analyze data drawn from the monthly Current Population Surveys (CPS). Every household that enters the CPS is surveyed each month for four months, then ignored for eight months, then surveyed again for four 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 Alaska 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, Ruggles, and Warren 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 (National Bureau of Economic Research 2007). 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 2 of the 8 total survey months for each respondent.[4]

We define a set of synthetic control states that, collectively, best match Alaska in the preperiod based on a number of state characteristics observed during the pretreatment period (the Z variables in equation (1), 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: (i) agriculture, forestry, fisheries, mining, and construction; (ii) manufacturing; (iii) transportation, communications, utilities, wholesale, and retail trade; (iv) finance, insurance, real estate, business, repair, and personal services; and (v) 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 pretreatment years used, we merge our CPS data with deccennial census data from 1970 and 1960. In this case, we focus on the employment-to-population ratio, or 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, Hand, and Thompson 2018). Third, we merge oil production data from the State Energy Data System (US Energy Information Administration 2015) with oil prices series from the BP

[4] CPS-MORG also has data on earnings, and it would be interesting to analyze this outcome. Unfortunately, it is very hard to find a good control group for Alaska in terms of hourly earnings: the preperiod match is at the ninety-eighth percentile. For this reason, we do not have much confidence in results concerning earnings.

Statistical Review of World Energy (BP 2017) and use oil-production-to-GDP ratios as a matching variable. Finally, we combine intercensal population estimates with natality and mortality measures to further use net migration as a matching variable (CDC). For more detailed descriptions of each dataset used in this analysis, see online Appendix D.

IV. Main Results

We separately consider two margins of response to the Alaska Permanent Fund dividend. First, we examine extensive margin outcomes, the employment-to-population ratio, 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.

A. *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 pretreatment 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 II. In particular, the key outcome variable used to construct the **V** matrix from equation (3) is the employment rate in each pretreatment year for column 2, the labor-force participation in each pretreatment year for column 3, and so forth. Meanwhile, the **X** variables used in equation (2) include age, female share, industry, education, and average employment or average labor-force participation in the preperiod. 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 online Appendix Table A.9. The states include Utah, Wyoming, Washington, Nevada, Montana, and Minnesota. The online Appendix provides synthetic control states and their weights for each of the outcomes and specifications we use. It is interesting to see that many of the chosen states are mountainous, like Alaska, even though this is not something we explicitly matched.

We first focus on the employment rate (employment-to-population), where the self-employed working for pay are also counted as employed. Figure 2, panel A plots for Alaska and synthetic Alaska from 1977 to 2014. The vertical, dashed line indicates 1981, the last year before the introduction of the Alaska Permanent Fund dividend. By construction, we see that Alaska and the synthetic control track each other in the preperiod. This pattern generally continues during the postperiod—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 postperiod. The data suggest that the dividend did not have a meaningful impact on employment in Alaska.

Table 1—Pretreatment Covariate Balance

		Synthetic control outcome		
	Alaska (1)	Employment rate (2)	Labor-force participation (3)	Part-time rate (4)
Panel A. Monthly CPS				
Employment rate	0.639	0.639	—	—
Labor-force participation	0.712	—	0.706	—
Part-time rate	0.103	—	—	0.104
Age 16–19	0.108	0.102	0.098	0.096
Age 20–24	0.154	0.137	0.130	0.127
Age 25–65	0.691	0.636	0.658	0.677
Share women	0.503	0.509	0.503	0.503
Industry group 1	0.361	0.361	0.331	0.337
Industry group 2	0.097	0.126	0.122	0.106
Industry group 3	0.035	0.069	0.064	0.035
Industry group 4	0.191	0.187	0.189	0.185
Industry group 5	0.078	0.090	0.124	0.161
Education $\leq$ 11 years	0.229	0.239	0.252	0.265
Education = 12 years	0.396	0.386	0.413	0.406
		Synthetic control outcome		
	Alaska	Hours worked last week		
Panel B. CPS-MORG				
Hours worked last week	37.980	37.935		
Age 16–19	0.074	0.067		
Age 20–24	0.155	0.144		
Age 25–65	0.759	0.755		
Share women	0.435	0.432		
Industry group 1	0.148	0.185		
Industry group 2	0.051	0.090		
Industry group 3	0.292	0.255		
Industry group 4	0.123	0.150		
Education $\leq$ 11 years	0.110	0.170		
Education = 12 years	0.387	0.362		

Notes: Table reports average value of variables during the pretreatment period for Alaska and the synthetic control constructed using the method described in Section III. Columns 2–4 differ in the outcome matched in equation (3). Panel A features data from monthly CPS surveys, and panel B features data from the CPS-MORG. The omitted category for age groups is age 65 and older. The omitted categories for industry groups are entertainment and recreation, professional and related services, public administration, and active duty military in Panel A and, additionally, finance, insurance, real estate, business repair, and personal services in Panel B. The omitted group for education is more than 12 years. The pretreatment period covers 1977–1981 in panel A and 1979–1981 in panel B. See online Appendix Table A.9 for the combination of states and weights that comprise each synthetic control.

Following the details outlined in Section II, 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 2, panel B 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 gray. Since each series relies on a different placebo treatment year, we use event time on the x-axis, i.e., time relative to the placebo treatment year. As expected, the mean of the placebo differences is very close to zero (-0.002), suggesting that the method is not 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 assess the analysis in several ways. First, we calculate a measure of synthetic control quality, the root-mean-square error (RMSE) of the

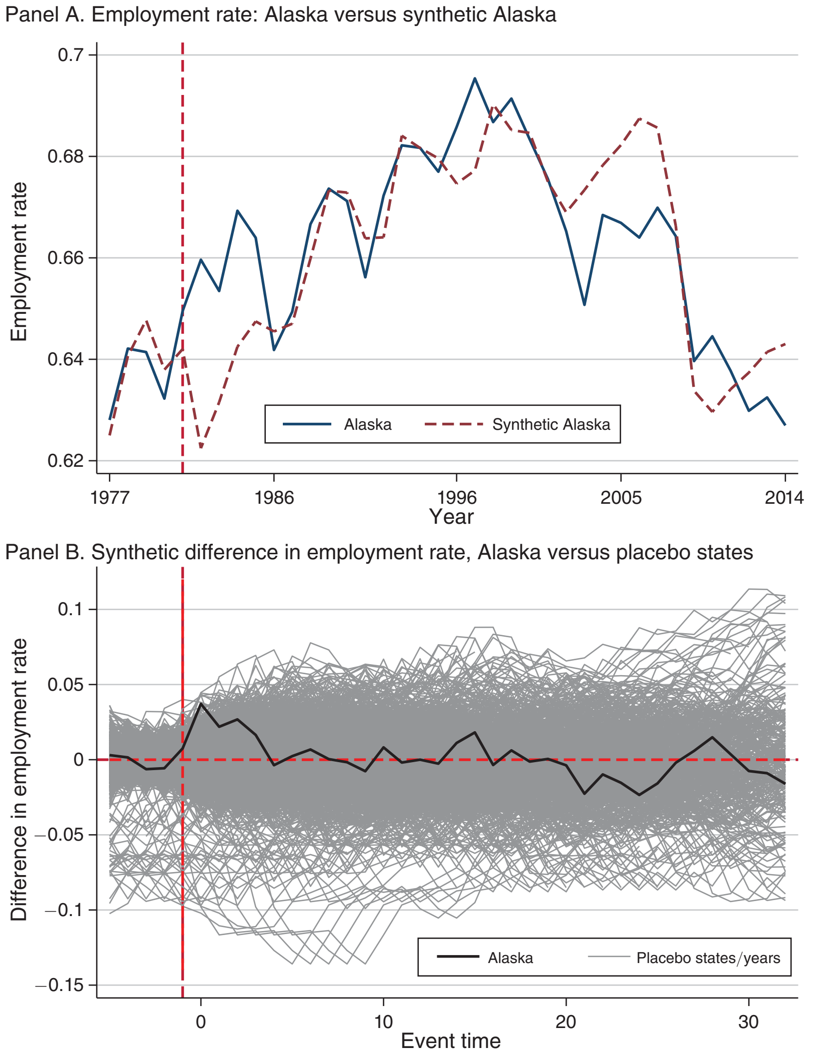

Figure 2. Employment Rate, 1977–2014

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 gray lines plot the difference using other states or other treatment years. See online Appendix Table A.9 for the combination of states and weights that comprise each synthetic control.

difference in each preperiod year between treatment and synthetic control. We then rank this measure for our actual treatment state and year relative to all placebos and find a relatively high-quality match. In Table 2, column 1, the actual treatment ranks within the top 32 percent match of quality when using employment as an outcome. Second, we use the empirical distribution of placebo treatment effects to assess the quantitative significance of our estimate, which we loosely refer to as a *p*-value.

Table 2—Synthetic Control Estimates, Average Difference 1982–2014

	Employment rate (1)	Part-time rate (2)	Labor-force participation (3)	Hours worked last week (4)
$\hat{\alpha}_1$	0.001	0.018	0.012	−0.796
p-value	0.942	0.020	0.331	0.084
95 percent CI	[−0.030, 0.033]	[0.004, 0.032]	[−0.019, 0.042]	[−1.751, 0.191]
Number of placebos	1,836	1,836	1,836	1,734
Preperiod RMSE	0.005	0.003	0.013	0.394
RMSE percentile	0.322	0.252	0.903	0.753

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes using the synthetic control method outlined in Section III. The treatment effect is averaged over the years 1982 to 2014. The p-value and confidence intervals are constructed using the permutation test described in Section III. Root-mean-square error (RMSE) is calculated using up to five years of pretreatment data, and percentile is based on a comparison among all placebo estimates. See online Appendix Table A.9 for the combination of states and weights that comprise each synthetic control.

Just over 94 percent of the placebos generate a larger estimate, underscoring our null conclusion. Finally, we construct a confidence interval using a series of placebo exercises under various null hypotheses. The resulting confidence interval in the case of employment contains zero.

We complement our analysis of extensive margin effects by also considering labor-force participation as an outcome. We summarize the results for this outcome in Table 2, column 3. In this case, we do not achieve as great a fit in the preperiod as when employment is used at the outcome: the RMSE is in the bottom ten percent of the preperiod fit rankings. Nevertheless, the treatment for labor-force participation is similarly indistinguishable from zero. Descriptive statistics during the preperiod for the synthetic Alaska constructed using labor-force participation are provided in Table 1, column 3. A graphical depiction of the estimates, as well as a list of synthetic control states and weights, is provided in online Appendix A, Table A.9, 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.

B. *Part-Time Work and Hours*

We now turn to intensive margin effects of the Alaska 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 preperiod observable characteristics. Put more rigorously, our preperiod RMSE for the part-time rate is in the top 25 percent when compared to our placebos. We therefore consider the part-time rate to be on par with the employment rate when it comes to quality of preperiod match. The synthetic Alaska in this case is composed of mostly Nevada and Wyoming (see online Appendix Table A.9).

Figure 3, panel A 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 preperiod, and there continues to be little difference between the two in the first few treatment years. The estimated treatment effect grows over time, and the rate of part-time work in Alaska exceeds that of the

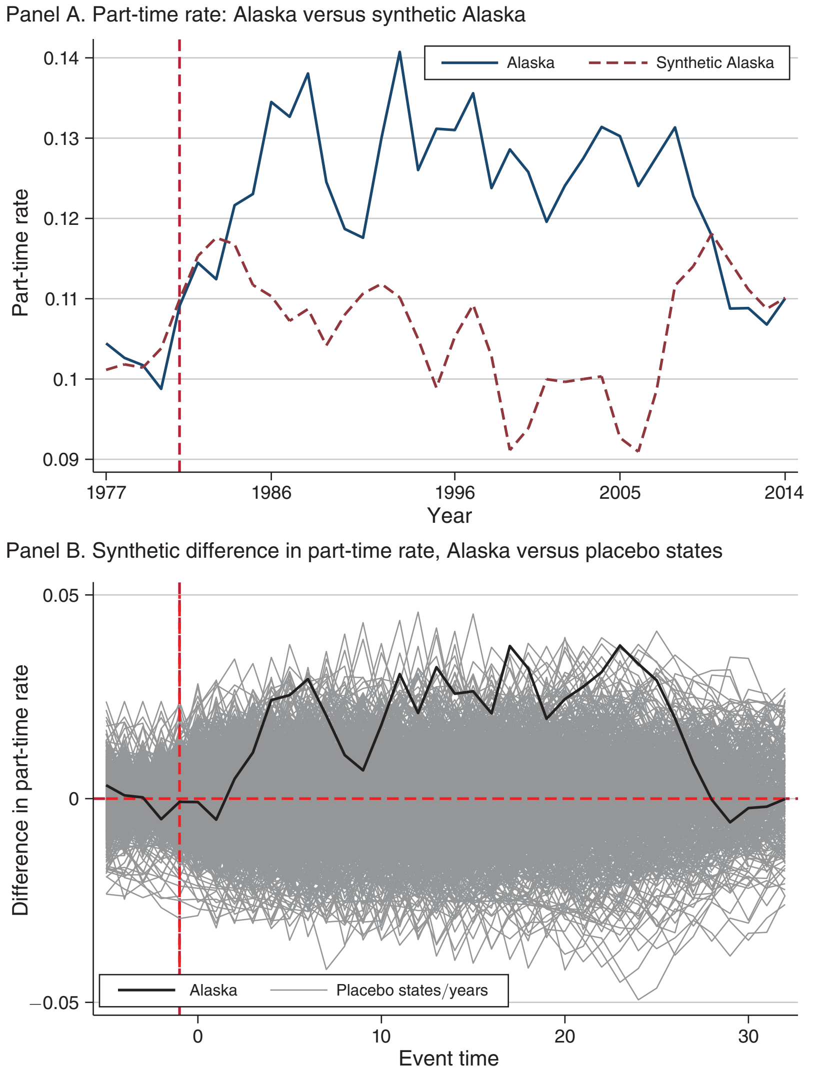

Figure 3. Part-Time Rate, 1977–2014

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 gray lines plot the difference using other states and or other treatment years. See online Appendix Table A.9 for the combination of states and weights that comprise each synthetic control.

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 preperiod. When compared to placebo estimates, this difference has a p-value of 0.020, and the confidence interval allows us to rule out a treatment effect of 0 at the 95 percent confidence level. This is visually demonstrated in

Figure 3, panel B, where the actual difference in Alaska is generally found near the upper limit of placebo differences.

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

As a secondary measure of intensive margin effects, we examine reported hours worked in the prior week for those who are employed. We can only observe this outcome in the CPS-MORG data, and thus the data are based on a smaller number of underlying observations and a shorter preperiod starting in 1979. In this case, our preperiod fit is not as well ranked—the RMSE is now just within the bottom 25 percent of the placebo rankings. We therefore place relatively less weight on this outcome. Consistent with our results for the part-time rate, we estimate a reduction on intensive margin, albeit less than one 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 preperiod match can be found in panel B of Table 1, and additional figures and synthetic control states and weights are available in online Appendix A, Table A.9 and Figure A.2.

V. Additional Results and Discussion

A. *Heterogeneity Analysis*

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

It may be the case that the dividend has a stronger effect among older workers who are closer to retirement (Price and Song 2016). In online Appendix Table A.1, we compare workers under and over age 55. Splitting the data results in poorer relative preperiod matches, but taken at face value, the results do not imply a particularly more negative labor-supply response among the older group.

B. *Robustness Tests*

In our main specification, we allow a different set of control states to be chosen, depending on the outcome variable. An alternative approach to constructing

Table 3—Synthetic Control Estimates, Average Difference 1982–2014 by Gender and Marital Status

	Employment rate—men			Part-time rate—men		
	All (1)	Married (2)	Unmarried (3)	All (4)	Married (5)	Unmarried (6)
$\hat{\alpha}_1$	0.029	0.032	−0.004	0.008	0.003	0.012
p-value	0.093	0.081	0.846	0.192	0.571	0.190
95 percent CI	[−0.008, 0.065]	[−0.008,0.071]	[−0.045, 0.037]	[−0.004, 0.019]	[−0.008, 0.014]	[−0.008, 0.031]
Number of placebos	1,836	1,836	1,836	1,836	1,836	1,836
Preperiod RMSE	0.024	0.011	0.038	0.003	0.007	0.008
RMSE percentile	0.972	0.609	0.981	0.259	0.845	0.466
	Employment rate—women			Part-time rate—women		
	All (7)	Married (8)	Unmarried (9)	All (10)	Married (11)	Unmarried (12)
$\hat{\alpha}_1$	−0.019	0.015	0.007	0.022	0.035	0.003
p-value	0.234	0.364	0.697	0.032	0.001	0.743
95 percent CI	[−0.055, 0.017]	[−0.020, 0.050]	[−0.032, 0.046]	[0.003, 0.042]	[0.016, 0.054]	[−0.019, 0.026]
Number of placebos	1,836	1,836	1,836	1,836	1,836	1,836
Preperiod RMSE	0.026	0.015	0.030	0.004	0.009	0.006
RMSE percentile	0.978	0.735	0.966	0.291	0.680	0.286

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes using the synthetic control method outlined in Section III. The treatment effect is averaged over the years 1982 to 2014. The p-value and confidence intervals are constructed using the permutation test described in Section III. Root-mean-square error (RMSE) is calculated using up to five years of pretreatment data, and percentile is based on a comparison among all placebo estimates. See online Appendix Tables A.10 and A.11 for the combination of states and weights that comprise each synthetic control.

our synthetic control involves using a common set of weights across our two main outcomes, the employment-to-population and part-time-to-population rates. This is to ensure that differences across outcomes are not simply a result of a change in the composition of control states. To that end, we amend the method outlined in Section II by jointly estimating a set of weights using both the employment rate and the part-time rate. In Table 4, we present the results of this alternative approach. The relative fit of our match during the preperiod is now at the thirty-first percentile, which lies just in between the two RMSE percentiles, when we consider employment (thirty-second percentile) and part-time work (twenty-fifth percentile) separately. In this case, we estimate a positive and significant effect of the dividend on the employment rate. On the other hand, our point estimate for the part-time rate is slightly smaller than in our main specification and becomes just marginally insignificant. Under this specification, the results imply that on net, the number of workers in full-time jobs increased, and thus the increase in part-time work did not occur at the expense of full-time work.

In our online Appendix, we consider several other robustness checks and alternative specifications. In online Appendix Table A.2, we use an "in-space" set of placebos (Abadie, Diamond, and Hainmueller 2015), holding the treatment period fixed at 1982. Our conclusions are changed significantly, although this leads to wider confidence intervals given the use of a smaller number of placebos. In online Appendix Table A.3, we follow Kaul et al. (2015) and use the outcome in the last preperiod to select synthetic control states, as opposed to the average over the entire preperiod. Our results remain very similar in this case. We also test the robustness of our results using a longer preperiod to construct our synthetic control by combining our data

Table 4—Synthetic Control Estimates, Average Difference 1982–2014, Common Weights

	Employment rate (1)	Part-time rate (2)
$\hat{\alpha}_1$	0.032	0.011
p-value	0.040	0.101
95 percent CI	[0.003, 0.065]	[−0.006, 0.028]
Number of placebos	1,836	1,836
Preperiod RMSE	0.005	0.005
RMSE percentile	0.312	0.312

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

with decennial census data from 1960 and 1970. Although this results in a weaker preperiod match than our main estimates, the results in online Appendix Table A.4 now feature a positive employment effect and thus reinforce our conclusion that the dividend is unlikely to have reduced employment rates.[5]

The long-run, average effect of the Alaska 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 online Appendix Table A.5. Using only placebos during this time period results in a poorer relative fit in the preperiod for all outcomes.[6] Furthermore, the confidence intervals include zero in all cases, consistent with a negligible impact in the very short run. Focusing on the employment and part-time rates, the effect on employment has a more positive point estimate, while the opposite is true for the part-time rate.

One potential concern is that the unique local economy of Alaska and its dependence on oil production and oil prices may confound our analysis. There is no direct link between the level of the dividend and fluctuations in yearly oil production due to the diversified nature of the fund's investments and the five-year averaging involved in the formula for dividends. Nevertheless, to check for the robustness of our results to incorporating the effects of oil prices, we add the total value of oil production as a share of state GDP to the list of variables we use to find a synthetic control for Alaska in the preperiod. We present those results in online Appendix Table A.6. We find a more positive estimate on the employment rate and a part-time effect closer to zero. Since the cumulative effect of oil discovery on real Alaskan income was rather negative after 1985 (James 2016), accounting for the effects of oil removes the potential negative bias in our employment effects. Overall, we are less likely to

[5] 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.

[6] The ranking of the preperiod fit differs for this specification, even though we use the same preperiod 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.

find any negative impact on the employment rate when using a set of control states that are chosen to better match Alaska's reliance on oil production.

Another potential concern is that other policy changes unique to Alaska may have occurred at the same time as the introduction of the Alaska Permanent Fund dividend. In particular, Alaska repealed its income tax in 1980. To the extent that this may have impacted labor markets, it might bias us against finding a negative effect of the dividend on employment. We address this in two ways. First, because this policy change precedes 1982, we are already matching Alaska to states with similar employment trajectories in the preperiod, which potentially accounts for labor market trends in Alaska that may have emerged in 1980. More directly, we can use our research design to inspect whether Alaskan employment shows any signs of a response to the tax repeal by treating 1980 as the year of treatment, and restricting attention to data prior to 1982. In online Appendix Table A.8 we find no effect for our two primary outcomes, the employment and part-time rates.

In online Appendix B, we explore how sensitive our results are to differential migration that may have coincided with the introduction of the Alaska Permanent Fund dividend. We implement three potential adjustments for differential migration. We use average net migration and annual net migration in the preperiod as matching variables. Additionally, we use the CPS Annual Social and Economic Supplement (ASEC) to assign respondents to their places of residence in the prior year and focus on outcomes in the short term, i.e., until 1985. We show in online Appendix B that while there is a relative increase in migration to Alaska during the period just prior to 1982, our results for the employment rate and part-time rate are qualitatively similar when we attempt to adjust for migration using these methods.

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

C. *From Microeconomic to Macroeconomic Effects*

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

In prior lottery studies, the micro-level income effect of a \$140K transfer has been estimated to generate a 2 percentage point reduction in employment (Cesarini et al. 2017). The per-household present value of all future Alaska Permanent Fund dividend payments is \$119,309, assuming an annual dividend payment of \$3,962 per

Table 5—Expected Microeconomic and Macroeconomic Effects on Employment Rates

Parameter	Description	Value	Source
Panel A. Parameters			
Income effect	EPOP change per \$140K of income	−0.02	Cesarini et al. (2017)
MPC	Marginal propensity to consume	0.5	Kueng (2018) (scaled up for durables)
η	Home bias	0.69	Nakamura and Steinsson (2014)
$(\alpha - 1)$	Labor share of income	0.667	Chodorow-Reich et al. (2019)
$\mathcal{M}$	Fiscal multiplier	1.8	Chodorow-Reich (2019)
κ	Wage-adjustment factor	0.9	Chodorow-Reich et al. (2019)
β	Jobs per \$100K of spending	1.9	Chodorow-Reich (2019)
EPOP	Average employment-to-population ratio	0.66	Authors' calc.
PF dividend	Average per capita dividend (2010 dollars)	\$1,495	Authors' calc.
PF dividend (PDV)	PDV of lifetime household dividends	\$119,309	Authors' calc.
PF dividends/labor income	Ratio of total dividends to total labor income	0.0725	Authors' calc.
Channel	**Formula**	**Predicted effect**	**Source**
Panel B. Labor effects			
Microeconomic effect	Income effect × PF dividend (PDV)/\$140K	−0.017	Cesarini et al. (2017)
Macroeconomic effect (version 1)	$\frac{1}{1+k}\mathcal{M}(1-\alpha)\eta \times MPC \times \frac{PFDividends}{LaborIncome} \times EPOP$	0.010	Chodorow-Reich et al. (2019)
Macroeconomic effect (version 2)	$\eta \times MPC \times \beta \times \frac{PFDividends}{\$100{,}000}$	0.010	Chodorow-Reich (2019)

Notes: The table presents estimates of the expected effect of the Alaska Permanent Fund Dividend using prior studies. The microeconomic effect corresponds to the direct income effect on labor supply of a lifetime of Alaska Permanent Fund dividend payments. The macroeconomic effect corresponds to the multiplier effect of more spending on employment, using estimates from two different methods. See Section VC for more details.

household over the course of the average Alaskan lifespan of 79 years and assuming an interest rate of 3 percent. Applying the micro-level income effect to this present value implies a 1.7 percentage point decline in the employment to population ratio (Table 5).

In order to calibrate a macroeconomic effect on employment, we must consider several factors. The Alaska Permanent Fund dividend may not be completely spent by consumers, and our setting is one of a small, open economy, where many goods are purchased from other markets. We therefore draw on two existing estimates of the effect of fiscal stimulus at the state level.

First, we consider the estimates of Chodorow-Reich, Nenov, and Simsek (2020), who model and estimate the effect of wealth shocks on consumption and, by extension, local labor markets. We draw on two key equations (Chodorow-Reich, Nenov, and

Simsek 2020, equations (11) and (12)), which imply the following relationship between dividend payments and log labor supply:

$$\Delta \log LaborSupply = \frac{1}{1+\kappa}\mathcal{M}(1-\alpha)\eta \times MPC \times \frac{PFDividends}{LaborIncome}. \tag{6}$$

Here κ is a wage-adjustment parameter capturing both sticky wages and the elasticity of labor supply, $\mathcal{M}$ the local Keynesian income multiplier, $(1-\alpha)$ the labor share of income, η the share of nontradables in spending, and *MPC* the marginal propensity to consume. The interplay between government multipliers and the stickiness of prices is similarly modeled by Woodford (2011), where the share of firms that adjust prices in each period and the amount of inflation allowed by monetary policy matter, among other factors.

One potential estimate for the *MPC* out of the Alaska Permanent Fund dividend would be 0.25, following Kueng (2018). That estimate only looks at nondurable spending, while we would like to capture all spending. We therefore scale the *MPC* up to 0.5. Using data from the BLS, we calculate an average ratio of total dividend payments to total labor income of 0.0725. The analysis of Chodorow-Reich (2019) implies a multiplier, $\mathcal{M}$, of 1.8. We choose a value of 0.69 for the home-bias parameter, η, following Nakamura and Steinsson (2014). Based on Chodorow-Reich, Nenov, and Simsek (2020), we choose values of 0.9 and 0.667 for κ and $(1-\alpha)$, respectively. Finally, we multiply the change in log labor supply by the average Alaskan employment-to-population ratio, 0.66, and obtain a macro-driven increase in employment rates of 1.0 percentage point. Our results are summarized in Table 5, macroeconomic effect (version 1).

Second, as an alternative, we can calibrate with state-level government spending multipliers for employment estimated using the American Recovery and Reinvestment Act of 2009 (ARRA) and other similar shocks, as summarized by Chodorow-Reich (2019). Here we must make two adjustments to reflect the fact that government spending impacts the economy differently than a cash transfer to consumers. We account for less than full spending of the transfer and the share of spending spent in the home state. Amending a key equation (Chodorow-Reich 2019, p. 15), we have the following relationship between the employment-to-population ratio (*EPOP*) and the dividend:

$$\Delta EPOP = \eta \times MPC \times \beta \times \frac{PFDividend}{\$100{,}000}, \tag{7}$$

where η is again a home-bias parameter, β is the number of jobs added per \$100,000 in government spending, *MPC* is the marginal propensity to consume, *PFDividend* is now the per capita dividend, and we scale by \$100,000 given the definition of β. Chodorow-Reich (2019) finds a median value of 1.9 for β. Using the values above for η and *MPC* and an average dividend amount of \$1,495 (2010 dollars), we again predict an employment rate increase of 1.0 percentage points. We summarize this calculation in Table 5, macroeconomic effect (version 2).

Because Chodorow-Reich, Nenov, and Simsek (2020) and Chodorow-Reich (2019) do not interpret their results as incorporating direct income effects on labor supply, we combine the predicted microeconomic and macroeconomic effects in order to calibrate the net effect of the Alaska Permanent Fund dividend on employment. When combined, the predicted microeconomic and macroeconomic effects imply a slight change in the employment-to-population ratio of -0.007 ($= -0.017 + 0.01$), which is not far from our main estimate in Table 2, but lower than the positive employment effects we estimate in other specifications (e.g., Table 4 and online Appendix Table A.4). Overall, our estimates imply a state-level fiscal multiplier on par with—or possibly greater than—those in the literature. Based on cross-sectional, state-level multiplier effects, Chodorow-Reich (2019) concludes in his review that a national-closed-economy, deficit-financed, no-monetary-response output multiplier would be 1.7 or above. Since our estimates of the employment effects of a stimulus are at or above the levels found in Chodorow-Reich (2019), they imply a slightly higher lower bound on the national output multiplier than 1.7.[7]

To further explore the possibility that our estimates reflect a macroeconomic effect, we inspect a related prediction: the macroeconomic employment effect should be concentrated in the nontradable sector. Di Maggio and Kermani (2016) show evidence for this channel by exploiting the increase in unemployment insurance transfers during the Great Recession, and Chodorow-Reich, Nenov, and Simsek (2020) show evidence of this using the consumption response to regional wealth shocks.

We indirectly test for the plausibility of this demand channel by re-estimating the impact of the dividend on employment and part-time status separately for industries in the tradable and the nontradable sectors. We use the same definitions of tradable and nontradable sectors as Di Maggio and Kermani (2016), which are themselves taken from Mian and Sufi (2014). We include construction in the nontradable sector. A full list of the industries can be found in online Appendix Table 1 of the online Appendix of Mian and Sufi (2014).

The results are presented in Table 6. While the preperiod 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 nontradable sector exhibits essentially no impact. This result, albeit suggestive, is consistent with an increase in consumption of nontradable goods contributing to a positive labor-demand effect, offsetting any negative labor-supply effects of the cash transfer in the nontradable sector.

An alternative interpretation of our results is that the size of the Alaska Permanent Fund dividend is too small to generate significant changes in the labor supply. Note that since the dividend is paid on a per-person basis, the average household receives about \$3,900 per year. These amounts may still be smaller than what would be expected under a universal basic income policy. Cesarini et al. (2017) found little evidence of nonlinearities in income effects, and thus our estimates may still speak to the potential impacts of a full-scale universal basic income. Moreover, the present value of these transfers at the household level are about \$119,000 and therefore are larger than a majority of the lottery winnings in Cesarini et al. (2017). When

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

Table 6—Synthetic Control Estimates, Average Difference 1982–2014 by Tradability

	Tradable		Nontradable	
	Employment rate (1)	Part-time rate (2)	Employment rate (3)	Part-time rate (4)
$\hat{\alpha}_1$	−0.048	0.015	0.002	−0.007
p-value	0.005	0.119	0.859	0.670
95 percent CI	[−0.072, −0.025]	[−0.007, 0.038]	[−0.024, 0.027]	[−0.040, 0.025]
Number of placebos	1,836	1,836	1,836	1,836
Preperiod RMSE	0.060	0.014	0.044	0.012
RMSE percentile	0.997	0.865	0.995	0.595

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

considered in that light, our null employment effects may be considered a meaningful departure from individual-level income-effect estimates, potentially driven by macroeconomic feedback factors.

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 in the 1970s. Therefore, there are potentially other types of spending that were forfeited when the fund was committed to dividends.

To get a sense of these counterfactual spending patterns, we repeat our synthetic control analysis, using as an outcome the share of government spending in four key areas: health and hospitals, education, highways, and welfare and transfer spending. We report these results in Table A.7. With these data, our preperiod 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 a smaller increase in highway spending. Importantly, we do not find a significant change in welfare and transfer spending, which is most likely to confound our analysis of the labor market. The lack of an effect of the dividends on welfare and transfer spending also alleviates the concern that the dividends crowded out other forms of redistribution.

D. *Implications for a Universal Basic Income*

Recently, the notion of a universal basic income—i.e., an unconditional cash transfer that is given to all—has generated renewed interest in the United States and around the world. Besides Hillary Clinton and Andrew Yang, whom we mentioned in the introduction, former president Barack Obama argued that the combination of advances in artificial intelligence, substitution away from labor-intensive technology, and rising wealth call for a new social compact, and he sees a universal basic income as something worth debating in this context.[8] 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,[9] the Canadian province of Ontario,[10] and the city of Stockton, California[11] have been running basic income experiments for various subset populations.

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

VI. 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 in which 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, when we calibrate the expected microeconomic and macroeconomic effects of the transfer, our empirical estimates are

[8] https://www.wired.com/2016/10/president-obama-mit-joi-ito-interview/.
[9] https://www.theguardian.com/society/2017/feb/19/basic-income-finland-low-wages-fewer-jobs.
[10] https://www.ontario.ca/page/ontario-basic-income-pilot.
[11] https://www.stocktondemonstration.org/.

generally in line with prior studies. In addition, we find suggestive evidence that the nontradable sector shows more favorable effects than the tradable sector. In the tradable sector, employment decreases and part-time work increases, while in the nontradable sector the effects on both employment and part-time work are close to zero and insignificant. Overall, we find indirect evidence of positive macroeconomic effects offsetting negative microeconomic effects and leading to an overall null effect of an unconditional cash transfer on aggregate employment, at least on the extensive margin.

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

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