Summer 8-3-2022

Universal Basic Income and the Alaska Permanent Fund: Impact on Labor Outcomes and Skill Composition

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by

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Submitted in partial fulfillment
of the requirements for the degree of
Master of Arts in Economics, Hunter College
The City University of New York

2022

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Dedications

I want to thank my parents for their unconditional support throughout my academic career and for the love they demonstrate every single day. I also thank zia Anna for hosting me and supporting me in my first years in the United States.

I dedicate my thesis to nonna Marisa and nonna Madò. I was lucky to have them on my side for all these years.

Acknowledgments

I want to spend a few words acknowledging the Economics Department's faculty for their assistance throughout my studies at Hunter College. I thank Professor Kenneth McLaughlin for the fundamental notions and intuitions he gave me regarding the field of labor economics and for the generous help he provided for completing this thesis. I also thank Professor Partha Deb for helping me choose the topic for the term paper in his Nonlinear Econometrics class — a project that built the foundations for this thesis — and for his continuous support and availability. Lastly, I want to mention Kevin Wong: a classmate, a colleague, and a friend. He was an indispensable part of my journey at Hunter College and a constant source of motivation.

Abstract

I use the natural experiment of the Alaska Permanent Fund to study the impact of Universal Basic Income on labor supply and human capital. I use synthetic control to find a group of states that matches Alaska in terms of employment, part-time rate, labor force participation, and hours worked from 1995 to 2020. With this control group, I estimate difference-in-differences regressions between 1979 and 1985 using individual-level data from the Current Population Survey on the same labor outcomes and on skill data from O*NET. There is some evidence of the policy increasing part-time employment, decreasing work hours, and increasing the shares of social, fundamental, and analytical skills at the expense of mechanical skills. However, rejection of the parallel trends assumption leads me to conclude that no group of states matches Alaska's labor outcomes in the pre-event period. Thus, there is reason to be skeptical of all other estimated treatment effects of the Alaska Permanent Fund.

Contents

1. Introduction

Discussions of welfare are highly ideological. On one side, people defend welfare because they believe a healthy society should help and support the disadvantaged. On the other side, those who oppose it claim that the social safety net promotes passive dependence on government's assistance and reduces incentives to work. Universal Basic Income (UBI) could very well unite the two sides. UBI in its purest form is (1) universal since it targets all residents of a state or country, (2) basic because it provides an income large enough to meet a person's basic needs, and (3) unconditional because it releases payments regardless of one's employment status, wealth, or labor income. UBI is a program that allows people to live decent lives while creating perhaps only weak disincentives to work.

On an individual basis, because unconditional the policy creates disincentives to work that are smaller than other conditional or means-tested programs. In the context of a simple consumption leisure framework, an unconditional cash transfer reduces hours worked only through its income effect. Essentially, because people have higher endowments, their reservation wages increase, and they work less. The income effect is the only mechanism through which UBI affects labor supply. In the case of other welfare policies, however, the optimal labor choice also depends on the conditions of the program. If working and earning more, or working at all, means receiving fewer benefits, then the price of leisure decreases, and people work less. In the case of UBI, the trade-off between working and receiving the transfer does not exist. Hence, the policy's bite on labor supply is theoretically lower.

Nevertheless, because universal, UBI likely affects labor supply more than the currently existing programs, especially in the United States. Hoynes and Rothstein (2019) explain that welfare policies in the United States already attempt to minimize labor supply disincentives by mainly targeting exogenously defined groups that have low potential to work and inelastic labor supplies — a feature called "tagging". If not through tagging, other types of programs still move in the same direction thanks to high phaseouts and by avoiding punitive taxes. On the other hand, because of the amount paid and the number of people targeted, UBI's income effect alone is likely to reduce overall labor supply both in terms of extensive and intensive margins — lower employment rate and fewer hours worked, respectively. As Hoynes and Rothstein (2019) explain, however, those in favor of UBI emphasize its potential effects on human capital accumulation.

Universal Basic Income might increase entrepreneurship and human capital accumulation because it loosens credit constraints. Hoynes and Rothstein (2019) suggest that, with their basic needs covered, individuals could temporarily leave the labor force or be employed only part-time to focus on developing their skills by increasing their educational attainment or by retraining. Hence, UBI should affect the average skill portfolio of the labor market by pushing people towards jobs that require more extensive preparation. Higher-skill workers earn more and tend to work more, leading to higher wages and employment in the long-run.

My thesis hence focuses on the effects of a UBI on both the labor market and human capital. The goal is to identify and quantify the impact that unconditional income has on employment, part-time employment, labor force participation and hours of work. However, I also study its effects on human capital by analyzing how it affects the composition of skills, and whether it pushes people to jobs that require more advanced and technical skills.

Since an unconditional, basic, and universal transfer program has never been implemented, I study the natural experiment of the Alaska Permanent Fund (APF) — a smaller scale unconditional cash transfer in Alaska since 1982. As O'Brien and Olson (1990) explain, in 1977 Alaska began receiving royalty income from oil produced on state-owned land at Prudhoe Bay thanks to the construction of the Trans-Alaskan Pipeline (TAPS). The boom-and-bust nature of an economy heavily reliant on oil led the state legislators to save some of this income and invest it in domestic and international financial and real assets. Other states and provinces in Canada also established trust funds to preserve wealth coming from natural resources. Only Alaska, however, chose to redistribute a significant portion of the interest income on its fund to its residents annually.

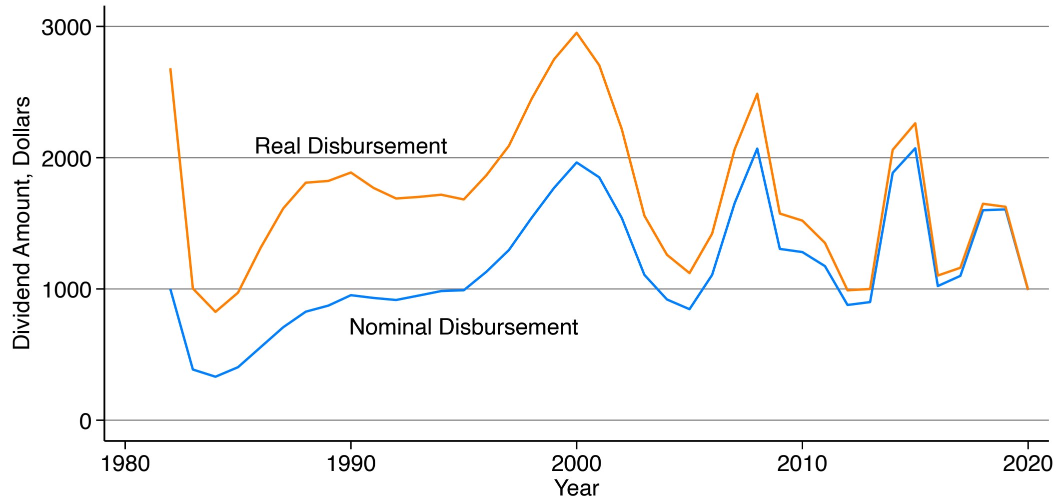
The Alaska Permanent Fund resembles a pure UBI because it is universal and unconditional. Kueng (2018) explains that essentially all Alaska residents are eligible to receive the payment regardless of their income or employment status. The only requirement is that the individual was a resident of Alaska in the previous year and intends to remain a resident. Even green card holders and refugees are eligible (Jones and Marinescu 2022, p. 319). Only those who were incarcerated during the previous year as a result of a felony conviction cannot receive the payment.

The major difference between the Alaska Permanent Fund and UBI is the size of the transfer. As Figure 1 shows, the annual amount that each resident of Alaska receives fluctuates over time and is generally small relative to median income. In nominal terms, the first payment in 1982 was $1,000; the value of the disbursements then fell until the early 2000s. It peaked in 2002 at $2,000. The highest payment in real terms was in 2000 when the dividend reached almost $3,000 (in 2020 dollars). At no point in time were the yearly payments large enough to allow Alaskans to cover their basic needs.

The dividend payments are not linked to the Alaskan economy and oil revenues. If that were the case, any analysis could suffer from serious issues related to endogeneity. Fortunately, since the fund is broadly diversified in domestic and international financial and real assets, local economic conditions do not affect the cash flows the fund generates (Kueng 2018). As further proof, the fund's oil revenues have substantially declined as a fraction of the fund's total market value from 12.2% in 1982 to 0.5% in 2016.

The smaller magnitude of the payments is not an obstacle to the study of UBI

Figure 1. Alaska Permanent Fund's Disbursements Over Time



Notes: Real disbursements are in terms of 2020 dollars.
Sources: State of Alaska, Department of Revenue, 1982-2022, and Bureau of Labor Statistics, 1982-2020.

and labor supply. First, at the household level, the payment is actually not that small. Jones and Marinescu (2022) explain that the average household receives about $3,900 per year, and the present value of these household transfers is about $120,000. Second, Cesarini, Lindqvist, Notowidigdo, and Ostling (2017) find little evidence of nonlinearities in income effects, suggesting that any result in the context of the Alaska Permanent Fund might be relevant for cash transfers of a larger magnitude (Jones and Marinescu 2022, p. 317). In addition, the most relevant feature of UBI in the context of labor supply is not the size of the payments but that, because unconditional, it does not generate a substitution effect. For this reason, the Alaska permanent fund is a useful natural experiment.

Since all residents of Alaska receive the permanent fund, it is not possible to identify a counterfactual within Alaska itself. The remaining option, therefore, is to look for other U.S. states that fit Alaska's trends in labor outcomes in the years preceding the implementation of the policy. As I explain in section 4, this task is particularly challenging because the end of the construction of the Trans-Alaskan Pipeline System (TAPS) marks the pre-event period and because employment data for the pre-event period is only available from 1977.

Jones and Marinescu (2022) attempt to resolve this issue using synthetic control on employment rate, part-time rate, participation rate and hours worked. They find evidence of an increase in part-time rates and a decrease in hours worked and of a null effect of the Alaska Permanent Fund on the employment rate. They argue that this last result is due to the increased demand for labor offsetting the income effect's reduction in labor supply. Their synthetic Alaska, however, fails to achieve a satisfactory fit in the pre-period for the analysis of hours worked and labor force participation, and the states it includes appear to be highly sensitive to the chosen dependent variable.

My first contribution to the study of the Alaska Permanent Fund is to find a control group for Alaska using data from the post-event period. Therefore, I apply synthetic control in the long-run post-event period. More specifically, I look for a group of states that systematically match Alaska's labor market in the years 1995 to 2020. This technique allows me to take advantage of more data and to study Alaska over a longer period.

My second contribution is to run difference-in-differences (DiD) regressions in the years around the implementation of the policy in 1982 to analyze the impact of the policy on four labor outcomes. Using Jones and Marinescu (2022) as my reference, I study the impact of the policy on employment, part-time employment, labor force participation, as well as on hours worked. To do so, I implement a DiD design with the control group found using synthetic control in the post period. In a first set of regressions, I estimate single treatment effects. In other specifications, I also allow for multiple treatment effects to check for delayed effects and to conduct a test of parallel trends before the event.

My last contribution is to study the impact of unconditional income on skill composition. The Occupational Information Network (O*NET) data base contains information regarding the importance of analytical, fundamental, managerial, mechanical, and social skills for each occupation in the United States. My goal is to analyze whether the permanent fund affects the average skill portfolio of the economy and whether it pushes people towards jobs that require more education, training, and experience. To do so, I apply the same DiD methods using variables on skills as my dependent variables.

The signs and magnitudes of my difference-in-differences estimates suggest that Alaska Permanent Fund has a significant impact on part-time employment and work hours and changes the composition of skills in the Alaskan labor market. I find that the policy increases part-time employment and reduces work hours, especially for women and married people. In addition, it increases the share of social, analytical, and fundamental skills at the expense of mechanical skills, while the effect on managerial skills is ambiguous.

Despite that the results are partially in line with the theory of credit constraints and human capital, I question their validity since I am unable to find a control group that satisfies parallel trends. Wyoming, Nevada, Utah, and Minnesota are, in fact, a good match for Alaska's labor outcomes in the period from 1995 to 2020, but they do not fit the state's trends in the years leading to the Alaska Permanent Fund.

2. Literature

My thesis focuses on the effects of UBI on employment and human capital and builds from three literatures. The first is the one on natural experiments in advanced countries that resemble an ideal UBI. Some of the papers focus on labor outcomes, while others study other outcomes such as health, crime, and education. The second is non-UBI literature that is relevant in the context of labor supply. In this category, the most relevant area of research is the one on lottery winners, but I also include a summary of the effects of the negative income tax. The last is the literature on specific human capital and occupational mobility.

UBI-Like Experiments

Jones and Marinescu (2022) use data from the Current Population Survey to analyze the effect of the Alaska Permanent Fund on employment, the same natural experiment I use in this thesis. Applying a synthetic control method to data from 1977 to 2014, they analyze the impact of dividends on various labor market outcomes. They find no significant effect of the Alaska Permanent Fund on employment rate and labor force participation. When they focus on intensive margins, they estimate that the permanent fund increases part-time employment by 1.8 percent and has no significant effect on hours worked. Even when controlling for differential migration or when implementing a simple DiD method, they estimate no significant change in labor outcomes.[1]

Jones and Marinescu (2022) explain that macro effects might cause the null estimates. Even if a disincentive to work exists, the increase in labor demand offsets the income effect's reduction in labor supply. They study tradable and non-tradable sectors separately in order to evaluate this hypothesis. They find that reductions in the employment rate and increases in the part-time rate are only present among the tradable sectors. They explain that the result is consistent with an increase in consumption of nontradable goods contributing to a positive labor-demand effect and offsetting any negative labor-supply effects of the cash transfer in the nontradable sector. The takeaway from their analysis is that if a UBI is implemented, an increase in labor demand stimulated by higher consumption should partially offset the reduction in labor supply.

In an experiment to study the effect of an unconditional basic income on the unemployed, Kangas, Jauhiainen, and Simanainen (2020) find evidence of a small effect of UBI on the incentive to work. In 2017 and 2018, 2,000 unemployed people between 25 and 58 years of age were randomly selected to receive monthly unconditional payments of €560. The control group was 173,000 people. They estimate a positive effect on labor supply. They argue, however, that this statistically significant increase might be due to exogenous changes in the Finnish national unemploy-

[1] They mention that a wage regression could also be insightful, but they struggle to find a good control group for Alaska in terms of hourly earnings.

ment benefits program. In 2018, in fact, the Finnish government restricted eligibility for unemployment benefits. Therefore, “the positive employment effect in the second year of the experiment was a joint effect of the basic income experiment and the amendments to the unemployment benefits legislation” (Kangas, Jauhiainen, and Simanainen 2020, p. 188). Because of the contemporaneous change in unemployment benefits, they are not able to isolate the impact of UBI.

Papers that track the effect of basic income on educational attainment, crime, and health outcomes suggest the presence of a positive impact of UBI. Akee, Copeland, Keeler, Angold, and Costello (2010) study a quasi-experiment in the Eastern Cherokee Native American tribe in which a casino began redistributing some of its profits to the adult members of the community. They find that children in households that receive dividends have a lower chance of committing minor offenses and stay in school longer. A null effect of the payments on parents’ labor force participation seems to disprove their initial hypothesis that the additional income pushes parents to substitute work for time to spend nurturing their children. They argue that the additional income reduces parental arrests, indicating that the increased income causes parents to engage in less destructive behavior. Even if not through labor supply, the increase in income seems to improve the quality of parenting.

Silver and Zhang (2022) use the Veteran Affairs (VA) disability program to analyze its effects on health and economic well-being. This compensation program pays approximately five million U.S. military veterans with disabilities. The basic income averages $1,500 per month, making the VA’s disability program the largest basic-income-like program in the United States. Unlike the other programs I review in this section, this disability program does provide basic income and is essentially unconditional. Since it only targets people with disabilities, the VA program is not useful for the study of labor supply. Focusing on health outcomes, Silver and Zhang find that basic income decreases food insecurity and homelessness and increases the utilization of health care.

Other Estimates of Labor Supply Responses

Studies of lottery winners are useful to extrapolate UBI’s impact on labor supply. Cesarini, Lindqvist, Notowidigdo, and Ostling (2017) estimate that pre-tax labor income of lottery winners decreases by about 1% of the wealth shock in each of the first ten years following the win, mostly through a reduction in hours worked. In contrast with previous literature on wealth shocks, they find no evidence of heterogeneity based on age or sex or of nonlinear effects. In addition, they find that winning the lottery does not encourage people to change employers, industries, or occupations. This result contradicts the idea that the loosening of credit constraints pushes people to switch jobs. They also find that winning a lottery prize reduces self-employment income.

Analyzing the substitution and income effects of negative income tax (NIT) experiments is another way to study how UBI would impact labor supply. In an NIT, people below a certain income cutoff receive money from the government equal to a proportion of the difference between their income and the cutoff. Those above the cutoff, instead, pay taxes. While not unconditional, NIT is equivalent to a UBI with a flat tax (Hoynes and Rothstein 2019, p. 938).

Robins (1985) compares the main findings of four NIT experiments in the United States to summarize the effects of NIT on labor supply. He reports that there is a general consensus that NIT reduces labor supply. There is even more agreement on the signs of the structural estimates of the substitution and income effects. In 80% of the cases, the substitution effect is positive and the income effect negative, and the magnitudes do not differ widely across studies. These results are in line with the theory.

Human Capital Accumulation

My thesis links UBI to the literature on specific human capital. Hoynes and Rothstein (2019) suggest that a basic income guarantee could loosen credit constraints by increasing educational investment and also by providing an incentive for on-the-job training and retraining. With their basic needs covered, workers can more easily forgo wages to change their occupations and invest in human capital. I connect to the literature on job mobility and specific human capital with the aim of finding evidence of workers moving to jobs with qualities that do not match their current skills. Before delving into the analysis of specific human capital, however, it is important to clarify the difference between job tasks and people's skills.

Tasks are characteristics of the job or occupation; skills belong to the worker. Yamaguchi (2012) suggests that tasks provide a useful interpretation of the observed occupational choice: it is a noisy signal of underlying skills. He claims that this distinction is an important departure from previous papers that implicitly assume that workers in the same occupation have identical occupational skills and use the observed occupation as a proxy for skills. While acknowledging this difference, Robinson (2018) treats occupations as similar if the skill vectors of the typical worker in the occupations are similar. For simplicity, I follow Robinson by treating unmeasurable skills and measurable occupation tasks as essentially the same.

Poletaev and Robinson (2008) and Robinson (2018) use data from the Dictionary of Occupational Titles (DOT) to provide each occupation in the Displaced Worker Survey with a vector of four skills. Based on these occupational characteristics, they define a distance measure to evaluate how different jobs are in terms of the tasks required to perform them. They use this information to provide evidence for the existence of occupation-specific human capital. Among workers who lost their jobs due to plant closings, those whose new job is more distant from their pre-displacement occupation face a larger decrease in wages.

Remarks

Given the lack of full-scale experiments, the literature on UBI is limited. Papers that analyze UBI's effects on labor, health, education, crime, and well-being outcomes take advantage of programs that are either not unconditional or do not provide a basic income. The general takeaway from the first two sections is that the impact of unconditional income and wealth shocks on labor supply is in line with what is predicted by the consumption-leisure model. When faced with an increase in non-labor income, people experience a disincentive to work due to a negative income effect. Evidence from the Alaska Permanent Fund, the only state-wide experiment concerning unconditional income, however, suggests that a large-scale UBI would have no overall effect on employment and hours worked. The possible explanation is that the resulting increase in consumption likely stimulates labor demand, fully offsetting the reduction in the supply of labor. There is also strong evidence of a significant positive impact on health and well-being.

My goal is to evaluate the effect of the Alaska Permanent Fund on the skills of workers as well as labor supply. I expand the scope of analysis to include the effects of UBI on skills. Because of the structure of Current Population Survey data in the years around the implementation of the Alaska Permanent Fund, I cannot employ the distance measures of Poletaev and Robinson (2008) and Robinson (2018). Therefore, I use skill data to evaluate the effect of the Alaska Permanent Fund on the average relative importance of skills.

3. Data

The data come mainly from the Current Population Survey (CPS). I use information on employment status and labor force participation from the basic monthly survey. For the analysis of hours worked, I use data from CPS Merged Outgoing Rotation Groups (MORG). The years included in my sample are 1995 to 2021 for my synthetic control analysis and 1979 to 1985 for the DiD regressions. Following Jones and Marinescu (2022), policy years in my DiD regressions are defined as twelve-month intervals beginning in July and ending in June. This is due to the Alaska Permanent Fund starting in June 1982. When I analyze data from 1995 to 2021, however, calendar year remains my unit of time.

I augment the CPS with data on (1) skills from the O*NET data base and (2) monthly employment in the construction industry by state. I associate each observation with a level of importance for five skills (social, managerial, mechanical, analytical, and fundamental) based on the individual's occupation.

Current Population Survey

The main data are from the basic monthly Current Population Survey (CPS) obtained from Flood, King, Rodgers, Ruggles, and Warren (2020). The CPS is a monthly survey of households that provides comprehensive data on the labor force, employment, unemployment, persons not in the labor force, hours of work, earnings, and other demographic and labor force characteristics.

I use two CPS data sets for my analysis. The basic monthly CPS gives me information on the employment status. The number of hours worked, instead, is only available in the MORG CPS for the years of my analysis. This second data set is smaller because households are only interviewed in the fourth and eighth months of the survey. For all regressions, the time span of my analysis is from policy year 1979 to 1985, and I include only individuals who are between 25 and 65 years of age.

The CPS allows me to generate four variables: one dummy indicating if the person is employed, one that reports whether the individual works part-time, and one reporting whether the individual is in the labor force, as well as a variable that measures the hours a person worked in the previous week (only for those who are employed). I choose these variables to replicate Jones and Marinescu's (2022) main results. I also obtain variables on education, race, and the age of those interviewed. Lastly, I use the occupation of each worker to merge in the skills from the O*NET data.

O*NET

Because the CPS does not report any measure or classification of skills, I merge skill data from the Occupational Information Network (O*NET) into the CPS data by four-digit occupation. I use the CPS occupation variable (in the 2010 classification) to match each job with a level of importance of five skills (analytical, fundamental, managerial, mechanical, and social). The O*NET program contains information on hundreds of standardized and occupation-specific descriptors (National Center for O*NET Development 2022). In particular, it allows me to obtain information regarding the level of importance of five main skill categories (analytical, fundamental, managerial, mechanical, and social) for each CPS occupation code.

O*NET uses a two-step design. First, it identifies a statistically random sample of businesses expected to employ workers in the targeted occupations. It then selects a random sample of workers in those occupations. To collect new data, it surveys job incumbents in the selected samples using standardized questionnaires. After collecting the information, O*NET employs occupational analysts to develop data on skills and abilities.

For each occupation, O*NET reports a level of importance for thirty-five skills. The score ranges from one to five, where one means that a skill is not important, and five means that it is extremely important. These data are useful because they provide a qualitative description of each occupation. Analyzing thirty-five different variables, however, would be unnecessarily complicated. For this reason, I aggregate all of the skills in five major categories.

I loosely follow the steps in the methodology appendix of Kochhar (2020) to group the thirty-five skills into five main categories: analytical, fundamental, managerial, mechanical, and social. The mapping from specific skills to general skills in Table 1 is the same as Kochhar's (2020). The importance level of each major category is the simple average of the importance level of all of the single skills it contains. Overall, this process of simplification makes my analysis and my results more intuitive even if at the expense of some detail.

Because O*NET skill data are not directly available for the years of my analysis, I use information from the early 2000s to study skills in the late 1970s and early 1980s. The first O*NET skill data set was released in April 2003. Since then, O*NET updated the skill rating twice per year. I average data from the first six versions of O*NET for years 2003 to 2005.

The mismatch between the years of the data and the time-span of my analysis could pose some limitations. First, jobs in the 2000s are different in terms of skill requirements from jobs in the 1980s. If I assume that the skill composition of each job relative to all other jobs remained fairly constant over time, however, my analysis should not be particularly hindered. If not, my estimates could be biased downwards or upwards depending on the way jobs evolved.

The second problem is that since data regarding the level of importance of skills in my data set do not update over time, I fail to catch any possible change of skill intensity over time within the time span of my analysis. Figure 2 tracks the average level of importance of each skill of the U.S. economy in the six versions of O*NET – released from 2003 to 2005. On average, the importance of all skills increased moderately over time, signaling that each occupation has steadily become more skill-intensive on average. Again, the validity of my analysis relies on the assumption that the importance levels of skills followed the same trend in the time span from 1979 to 1985.

I modify the data set to make it compatible with the CPS. Occupations in O*NET follow a taxonomy that does not directly match the occupation codes of the Current Population Survey. Conversion tables and crosswalks provided by O*NET, the Bureau of Labor Statistics, and Kochhar (2020), as well as manual adjustments, allow me to convert the 941 O*NET occupations into 396 occupations codes (2010 basis) in the basic monthly CPS. I merge the O*NET and CPS data set, assigning to each job the importance level of each of the five skills. I merge by occupation, and not by year. Hence, the importance levels of the five skills variables for the same job do not vary over time.

When multiple jobs in the O*NET are recoded to a single CPS code, I calculate the importance levels of each of my 5 skill categories as simple averages across occupations. In some cases, I also recode different CPS occupations as one due to single O*NET jobs corresponding to multiple CPS jobs. Because of missing O*NET data on certain occupations, I also exclude some observations from my

Table 1. Mapping from O*NET Skills to Five Major Skill Categories

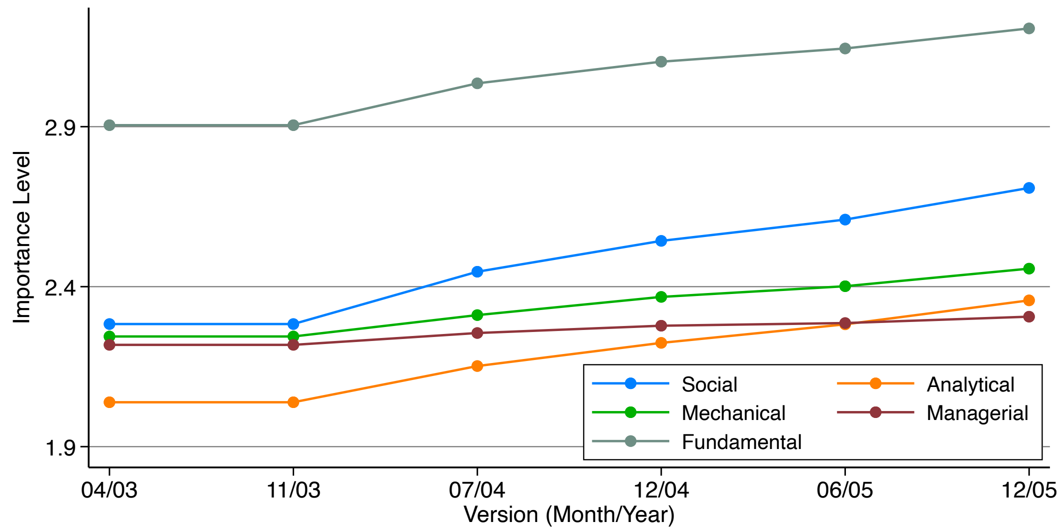
Social	**Analytical**
Monitoring	Mathematics
Social perceptiveness	Science
Coordination	Complex problem solving
Persuasion	Operation analysis
Negotiation	Technology design
Instructing	Systems analysis
Service orientation	System evaluation
	Programming
Fundamental	**Mechanical**
Reading comprehension	Equipment selection
Active listening	Installation
Writing	Operation monitoring
Speaking	Operation and control
Critical thinking	Equipment maintenance
Active learning	Troubleshooting
Learning strategies	Repairing
Judgement and decision making	Quality control analysis
Managerial	
Time management	
Management of financial resources	
Management of material resources	
Management of personnel resources	

Source: Kochhar (2020).

sample.

Because I am interested in the effect of the permanent fund on the share of each skill in Alaska's portfolio, I create relative skill variables by dividing each skill's importance level by the sum of the level of importance of all skills. The interpretation is that each person, based on their job, has a certain skill portfolio, in which each of the five skill categories has a specific relative weight. Alaska's average skill portfolio is the average of that of all of its workers. I use these data to evaluate whether the Alaska Permanent Fund had a significant effect on changing the weights of each skill in Alaska's average portfolio. Since in my data set the level of importance of skills for each occupation does not vary over time, any significant increase or decrease in the relative importance of a skill must be due to workers moving to jobs that are qualitatively different.

Figure 2. Average Importance Level of Skills Over Time



Notes: Importance level measures the importance of each skill on a scale from 1 to 5.
Source: O*NET, 2003-2006.

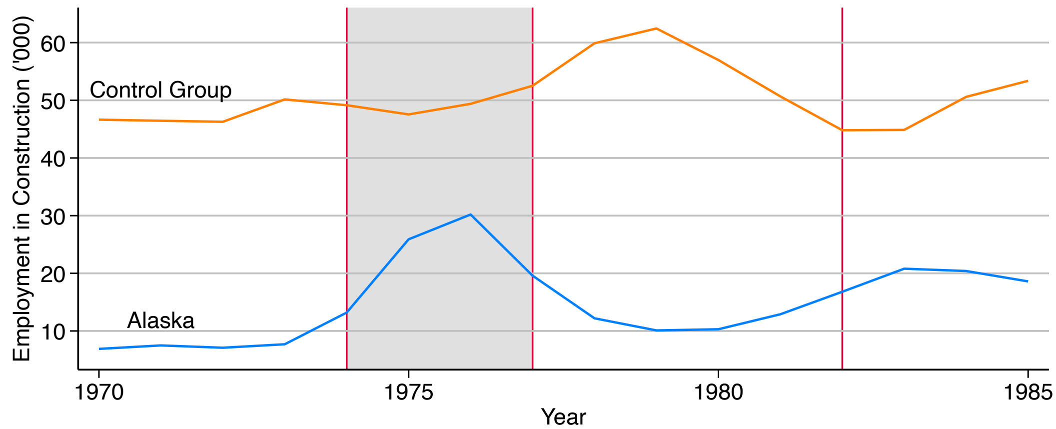
Employment in Construction

I use monthly employment in construction by state to control for Alaska's pre-event unique employment patterns due to the TAPS project finishing in 1977. State and Metro (SAE) data from the Current Employment Statistics (CES) on employment by sector before 2003 are available using the discontinued SIC series on the Bureau of Labor Statistics website. Figure 3 provides the series of construction employment in both Alaska and the treatment group from 1970 to 1985. The data show a sudden increase in construction employment in the years of the TAPS and then a 50% decrease in the first two years following the end of the TAPS construction in 1977. The control group follows the opposite trend in that same time span.

Final Data Sets

My two final data sets are repeated cross sections from the basic monthly survey and the Merged Outgoing Rotation Groups in the CPS. The unit of analysis is individuals from 1979 to 1985. Besides Alaska, the states I include in my sample as a control group are Minnesota, Nevada, Utah, and Wyoming. I explain the choice of the control group in detail in my results section. Each observation is associated with some general characteristics such as age, gender, race, and highest grade completed. In addition, for the basic monthly data set, I generate dummy variables that report whether a person is employed, works part-time, and is in the labor force. The outcome variable in the MORG data set is instead the number of hours worked last week. In both data sets, I merge data on employment in construction for each state each month. Lastly, I augment the basic monthly CPS data set with data on

Figure 3. Employment in Construction: Alaska and the Control Group



Notes: The shaded area shows the years that the TAPS project was in construction. The vertical line in 1982 marks the year that Alaska implemented the permanent fund. The control group series is a population-weighted average of construction employment in Minnesota, Nevada, Utah, and Wyoming.
Sources: Current Employment Statistics State and Metro Area, 1970-1985, and FRED Economic Data, Federal Reserve Bank of St. Louis, 1970-1985.

skills. For the subset of individuals who are employed, five skill variables describe the relative level of importance of analytical, fundamental, managerial, mechanical, and social skills for the occupation of the individual sampled. I summarize these variables in Table 2.

For the synthetic control estimation, I use data from 1995 to 2021 aggregated by calendar year at the state level. I follow Jones and Marinescu (2022) for the choice of independent variables. These are: the employment rate, part-time rate, percent in the labor force, and the number of hours worked last week. For my covariates, I calculate the share of population in three educational categories: less than a high school degree, high school degree, and at least some college. Then I measure the share of females 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. Unlike Jones and Marinescu (2022), instead of using the information on industries, I include the relative level of importance of skill variables. Summary statistics for Alaska are available in the Results section in Table 4.

4. Methods

My thesis uses two different methods of policy analysis. I use synthetic control in the years 1995 to 2021 to find a control group that matches Alaska in four major labor outcomes: employment rate, part-time rate, percent in the labor force, and hours worked last week[2]. Once I identify Minnesota, Nevada, Utah, and Wyoming to be

[2] I thank Professor McLaughlin for this suggestion.

Table 2. Summary Statistics

	1979-1981		1982-1985	
	Alaska	Control Group	Alaska	Control Group
Labor Outcomes				
Employed (=1)	0.702	0.743	0.705	0.747
	(0.458)	(0.437)	(0.456)	(0.435)
Part-Time (=1)	0.0877	0.122	0.104	0.127
	(0.283)	(0.327)	(0.305)	(0.333)
Labor Force (=1)	0.764	0.776	0.771	0.793
	(0.425)	(0.417)	(0.420)	(0.405)
Hours Worked†	38.77	38.02	38.51	37.90
	(19.10)	(17.52)	(18.15)	(16.87)
Share of Skills				
Analytical	18.02	18.27	18.29	18.44
	(2.415)	(2.685)	(2.531)	(2.727)
Fundamental	24.61	24.56	24.66	24.62
	(2.985)	(2.858)	(2.987)	(2.832)
Managerial	17.86	17.93	17.71	17.79
	(1.959)	(1.976)	(1.945)	(1.963)
Mechanical	18.99	18.77	18.69	18.55
	(6.386)	(6.218)	(6.329)	(6.088)
Social	20.52	20.48	20.65	20.61
	(3.112)	(3.200)	(3.078)	(3.124)
Demographics				
Age	38.61	41.44	38.46	40.96
	(10.75)	(12.02)	(10.42)	(11.82)
Highest Grade	12.95	12.86	13.03	12.99
	(2.983)	(2.608)	(2.860)	(2.561)
Statewide				
Construction	11.95	55.20	18.73	49.70
	(2.964)	(24.90)	(4.584)	(22.10)
N	36,549	160,142	59,067	177,328

Notes: Means with standard deviation in parentheses. Years are defined as twelve-month intervals beginning in July and ending in June. Construction employment is measured in thousands. The skill variables are only available for employed individuals. † The sample sizes for hours worked in the CPS MORG are 5,946, 32,891, 9,327, and 39,016 in columns 1-4 respectively.

Sources: Current Population Survey, 1979-1986, and Current Employment Statistics State and Metro Area, 1979-1986.

the proper control group, I estimate differences-in-differences regressions for years 1979 to 1985 on employment, part-time employment, labor force participation, and hours worked. My last set of DiD regressions analyzes the impact of the permanent fund on the skill portfolio of Alaska's labor market.

Finding a Control Group

The end of the construction of the Trans-Alaska Pipeline System (TAPS) heavily affected trends in employment, earnings, and hours worked in Alaska in the period around the implementation of the permanent fund. The TAPS project started in the Spring of 1974 and ended in June of 1977, and represented the largest privately financed construction project in world history at the time (Carrington 1996). Employment and earnings increased substantially during the construction of the pipeline and fell sharply after the project ended. Employment grew 56.8 percent between 1973 and 1976 and then shrank by more than 8.5 percent between 1976 and 1977. Only by 1981 was it close to what one could have predicted from the pre-1974 trend. Monthly earnings also increased by 56 percent due to higher hourly wage rates and more time spent working and reached pre-pipeline levels in 1979 (Carrington 1996, p.197-199). This effect was particularly strong in the construction industry as Figure 3 shows. Since the construction of the pipeline was an event unique to Alaska, finding a control group with parallel trends is particularly challenging.

Jones and Marinescu (2022) use the synthetic control method of Abadie, Diamond, and Hainmueller (2010) to create a counterfactual Alaska that matches its trends in employment, part-time rates, and percent in the labor force for years 1977 to 1981, and in hours worked from 1979 to 1981. Despite the short pre-event period, they are able to generate a synthetic Alaska in their analyses of employment and part-time rates. However, they fail to achieve a satisfactory fit in the pre-period for the analysis of hours worked and labor force participation. In addition, as they show in their online appendix, the pool of states and the synthetic control weights appear to be highly sensitive to the chosen dependent variable. No single control group consistently matches Alaska across outcomes in the pre-event period.

First, because of the challenges of having a small number of pre-event years and of the presence of the TAPS project, I turn my attention to finding a control group for the period of time from 1995 to 2020. This period likely allows for results that are more reliable since the analysis is conducted over a longer period of time in which Alaska did not face any major unique economic event. My synthetic control estimates are essentially placebo estimates in which the treatment year is 2021 and the pre-period starts in 1995. I am not interested in estimating the treatment effect by synthetic control. However, this method reveals the states that fit Alaska in the long run. The resulting states are the control group for the DiD analysis of the permanent fund in 1982.

I then use synthetic control to generate a control group to run differences-in-differences regressions on various labor outcomes in the period around 1982. Even though synthetic control generates a different synthetic Alaska for each different labor outcome, my goal is to define a unique control group. The choice of which states to include is based on the states that receive higher weight and appear more often in all of the synthetic control estimations. Weights guide my choice of which state to include in the control group. However, synthetic control weights do not carry over to the DiD estimation – where I instead use population weights.

Synthetic control features a data-driven method for choosing a counterfactual in the context of causal inference in policy evaluation. It estimates the effect of an intervention of interest by comparing the evolution of an aggregate outcome variable for a unit affected by the intervention to the evolution of the same aggregate outcome for a synthetic control group. Synthetic control generates this control group by searching for a weighted combination of control units picked by a donor pool to approximate the unit affected by the intervention in terms of some outcome predictors (Abadie, Diamond, and Hainmueller 2010).

In the context of my analysis, the donor pool contains all U.S. states, and the outcome variables are Alaska's employment rate, part-time rate, percent in the labor force, and average number of hours worked. The predictors, as described in the data section, are the share of the population with less than a high school degree and with a high school degree, the share of population in age groups 16 to age 19, 20 to 24, and 25 to 64, and the relative level of importance of analytical, fundamental, managerial, mechanical, and social skills.

DiD on Labor Outcomes

After finding a control group that consistently matches Alaska in terms of labor market outcomes, I estimate person-level difference-in-difference regressions on four outcomes: employment, part-time employment, labor force participation, and hours worked. The estimation spans the period around the implementation of the permanent fund in 1982. The pre-event period is policy years 1979 to 1981, and the post-event period goes up to 1985. The treated unit is Alaska and the control group includes Minnesota, Nevada, Utah, and Wyoming.

I regress employment, part-time employment, labor force participation and work hours on the interaction between a post-event dummy and an Alaska dummy, with state effects, time effects, and a set of covariates including sex, age, education, race, and monthly employment in construction in the person's state.

$$y_{it} = \alpha + \beta A_{it} \cdot Post_t + \boldsymbol{x}_{it}\boldsymbol{\gamma} + \zeta_t + \delta^s_{it} + \epsilon_{it} \tag{1}$$

for each $i = 1, ..., N_t$ and $t = 1, ..., T$. A_{it} indicates if person i is from Alaska at time t, and $Post_t$ is an indicator variable equal to 1 if the observation is from the post-event time period. ζ_t and δ^s_{it} are year and state fixed effects respectively. $\boldsymbol{x}_{it}$ is a vector of covariates including age, race, sex, marital status, highest grade completed, and a variable that tracks the monthly employment in construction in each state.

For each outcome, I also run a regression that includes a differential time trend. To do so, I interact the Alaska dummy with a continuous variable that measures time in months since July 1979.

In a third set of estimates, I interact Alaska with post-event policy years to estimate year-specific effects of the Alaska Permanent Fund.

$$y_{it} = \alpha + \sum_{t=1982}^{1985} \beta_t A_{it} \cdot I_t + \boldsymbol{x}_{it}\boldsymbol{\gamma} + \zeta_t + \delta_{it}^s + \epsilon_{it} \tag{2}$$

for each $i = 1, ..., N_t$ and $t = 1, ..., T$. I_{it} is is an indicator variable for time equal to 1 if observation i is from year t. Rather than reporting a single treatment effect, I estimate the year-specific estimates $\beta_{1982}, ..., \beta_{1985}$.

I also estimate these specifications by race and sex. For regressions on part-time employment only, I also estimate regressions for married people and married females. These regressions allow me to conduct a by-group analysis and estimate whether the policy affected the race and sex groups differently.

To evaluate parallel trends in the pre-event period, I also interact the Alaska dummy with both pre-event policy years 1979 and 1980. The test for the existence of pre-trends consists in testing the statistical significance of the interaction terms between a dummy for the treatment group and all pre-event years. If these pre-trends or leads are jointly insignificant, then we fail to reject the null hypothesis of parallel trends. As Roth, Sant'Anna, Bilinski, and Poe (2022) explain, this test has several limitations and cannot be relied on fully for testing parallel trends. For this reason, I use the pre-trends test in my DiD regressions only as a plausibility check of the results from applying synthetic control in the years 1995 to 2020.

DiD on Skills

The lack of dependent coding in the CPS in the years of my analysis complicates the study of how UBI affects the skill portfolio of workers. Poletaev and Robinson (2008) and Robinson (2018) use the Displaced Worker Survey to create occupational distance measures to compare the jobs of displaced workers before and after plant closings. If the CPS reported the job each individual had in the year or month preceding their interview — a feature known as dependent coding — I could use a similar technique to analyze whether the Alaska permanent pushed workers to change their occupation and, most importantly, their skill portfolio. Unfortunately, the CPS introduced dependent coding only one year after the permanent fund was implemented in Alaska, making it impossible to use it for my difference-in-differences analysis. A possible alternative could be taking advantage of the Integrated Public Use Microdata Series (IPUMS) unique person identifier to treat the CPS as longitudinal data

Table 3. Synthetic Control Weights

	Employment Rate	Part-Time Rate	Participation Rate	Hours Worked
Colorado	0.040	-	-	-
Minnesota	0.109	0.183	0.413	-
Nevada	0.440	0.364	0.335	0.346
Utah	0.204	0.262	-	-
Wyoming	0.207	0.190	0.252	0.263
District of Columbia	-	-	-	0.069
Texas	-	-	-	0.322

Notes: Entries are weights that synthetic control assigns to each state each outcome variable.
Source: Current Population Survey, 1995-2021.

set. However, as Robinson (2018) explains, the well-known problem of measurement error in occupation coding significantly reduces the accuracy of any measure of occupational mobility. For this reason, I use data on the importance of each skill relative to all skills in each occupation to analyze whether the Alaska permanent fund significantly affects the average skill portfolio of Alaska's labor market.

For each relative skill variable, I present results from four different specifications. First, a simple DiD, a simple DiD regression with a differential linear time trend, a DiD regression with leads, and one with both leads and lags. Because my skill variables add up to one, the sum of the DiD effect on each relative skill must equal zero.

All regressions are weighted with final person-level weights and report standard errors that are clustered at the state level.

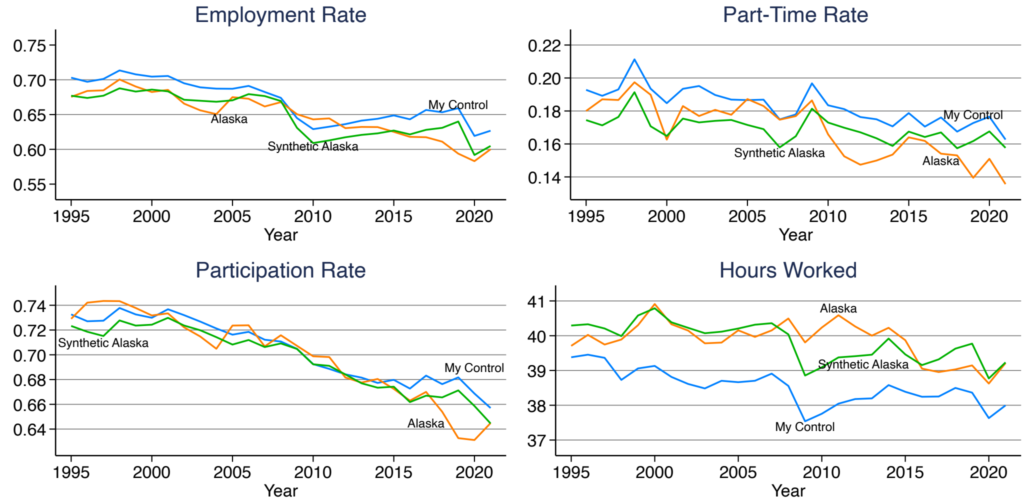
5. Results

Finding a Control Group

Applying synthetic control from 1995 to 2021 allows me to narrow down a control group for Alaska across all of the four labor outcomes. As Table 3 shows, Minnesota, Nevada, Utah, and Wyoming are the top four states for synthetic Alaska for the employment rate, part-time rate, and participation rate, although Utah does not receive any weight in the participation rate estimation. For this reason, these four states belong in the control group. District of Columbia and Texas replace Minnesota and Utah for hours worked. Texas, like Alaska, is a state that heavily relies on oil. Nevertheless, I do not include it since it is an outlier in terms of geography. The same is true for DC that, in addition, is also too dissimilar in terms of geographical characteristics and demographics.

The main pattern that emerges is that some states in the Mountain division

Figure 4. Alaska, Synthetic Alaska, and the Control Group



Notes: The control group is a population-weighted average of Minnesota, Nevada, Utah, and Wyoming.
Source: Current Population Survey, 1995-2021.

are very similar to Alaska. Alaska and the majority of states in the control group are mountainous states with low population densities. This is in line with Jones and Marinescu's (2022) synthetic control estimates. The result seems highly plausible although neither population density nor geographical characteristics are included as covariates in the synthetic control estimation.

Table 4 shows that the synthetic control properly matches Alaska both in terms of its labor outcomes and the included covariates. Column 1 presents the average values for the outcome variables and covariates in Alaska in the period from 1995 to 2020. Each of the four following columns, instead, reports the averages for synthetic Alaska in each of the four labor outcomes. The averages for synthetic Alaska closely match the ones for Alaska in all of the four columns.

Figure 4 shows that both synthetic Alaska and the control group properly fit Alaska's trends in employment, part-time employment, participation rate, and hours worked. In each panel, the orange series represents Alaska, the green series synthetic Alaska, and the blue series is the population weighted average of data from Minnesota, Nevada, Utah, and Wyoming. Despite they do not include the same states and do not use the same weights, the trends for synthetic Alaska and the control group appear to be mostly parallel. In addition, they both effectively fit the trends for Alaska in most of the presented panels. The only exception is a slightly less precise but still satisfactory match in terms of hours worked.

Table 4. Pretreatment Covariate Balance, 1995-2020

		Synthetic Alaska			
	Alaska	Employment-to-Population Ratio	Part-Time Rate	Participation Rate	Hours Worked
Outcome Variables					
Employment	0.651	0.651	-	-	-
Part-Time	0.170	-	0.170	-	-
Participation	0.702	-	-	0.699	-
Hours Worked	390.893	-	-	-	390.851
Demographics					
Age 16-19	0.083	0.075	0.076	0.072	0.072
Age 20-24	0.090	0.092	0.094	0.084	0.089
Age 25-60	0.719	0.678	0.675	0.681	0.683
Female (=1)	0.496	0.503	0.503	0.504	0.508
Education < 11 Years	0.113	0.130	0.128	0.125	0.159
Education = 12 Years	0.327	0.328	0.323	0.322	0.324
Share of Skills					
Analytical	0.184	0.185	0.185	0.185	0.185
Managerial	0.175	0.175	0.175	0.175	0.175
Mechanical	0.180	0.181	0.180	0.180	0.181
Social	0.211	0.211	0.211	0.211	0.211

Notes: Entries are the averages of labor outcome variables and covariates for Alaska and synthetic Alaska. Each labor outcome has its own synthetic Alaska.
Sources: Current Population Survey, 1995-2021, and O*NET, 2003-2005.

DiD on Labor Outcomes

In this section, I present the estimates of the DiD regressions on labor outcomes. Table 5 shows results for employment, Table 6 for part-time employment, Table 7 for labor force participation, and Table 8 for hours worked. Each table reports simple DiD estimates with and without a differential time trend and year-specific DiD estimates.

The estimates of the covariates' coefficients across specifications and tables are sensible. The sign and significance of the coefficients do not vary across specifications for each dependent variable. The employment and participation rates are higher for men, increase with education, and increase at a decreasing rate with age (the effect peaks in magnitude at 39 years for employment and 38 for labor force participation). Part-time employment is higher for women and appears to be unrelated to age and education. Lastly, working hours are lower for women and increase with age until approximately age 45.

Simple DiD. Table 5 shows that the effects of the policy on employment are ambiguous. In the first column, the effect is not statistically significant. However, the estimated DiD coefficient is significant at the 5% level in the specification that includes a differential time trend. The point estimate suggests that the Alaska Permanent Fund increases employment by 2.3%. The result contradicts the expectations of a negative income effect.

Table 6 shows that the Alaska Permanent Fund's impact on part-time employment also depends on a differential time trend. The effect on part-time employment is positive and significant beyond the 1% level in the first column of Table 6. There is evidence for a 1.6% increase in part-time employment due to the policy. With the differential trend, the estimate becomes insignificant.

The policy has no effect on labor force participation. As Table 7 reports, the DiD estimate is not significant regardless of whether I account for differential time trends.

There is also no effect on hours worked. Neither DiD coefficient in columns 1 and 2 of Table 8 is statistically significant. In contrast with the results on part-time employment, these results provide evidence of no effect of the policy on the intensive margins.

Overall, Alaska seems to follow a differential linear trend in two of my four labor outcomes. Compared to the control group of Minnesota, Nevada, Utah, and Wyoming, the treatment group's employment and part-time employment have a differential trend equal to -0.04% and 0.02% per month. The time trend is, instead, insignificant in both the regressions on hours worked and participation.

Multiple Treatment Effects. In column 3 of Tables 5-8, I present coefficients from specifications that include lags in treatment effects for all labor outcomes. These regressions allow me to check whether the effect of the policy follows a specific

Table 5. Effect of the Alaska Permanent Fund on Employment

	(1)	(2)	(3)	(4)
DiD	0.006	0.023**		
	(0.004)	(0.003)		
Linear Trend†		−0.395**		
		(0.070)		
Pre-Event				
1979 DiD				0.004
				(0.003)
1980 DiD				−0.012**
				(0.002)
Post-Event				
1982 DiD			0.024**	0.021**
			(0.004)	(0.003)
1983 DiD			−0.007*	−0.010*
			(0.002)	(0.003)
1984 DiD			0.006	0.004
			(0.004)	(0.005)
1985 DiD			0.003	0.000
			(0.005)	(0.006)
Other Controls				
Female = (=1 female)	−0.221***	−0.221***	−0.221***	−0.221***
	(0.022)	(0.022)	(0.022)	(0.022)
Age	0.043***	0.043***	0.043***	0.043***
	(0.001)	(0.001)	(0.001)	(0.001)
Age Squared	−0.001***	−0.001***	−0.001***	−0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Highest Grade	0.020***	0.020***	0.020***	0.020***
	(0.001)	(0.001)	(0.001)	(0.001)
Construction†	0.505*	0.498*	0.499*	0.501*
	(0.160)	(0.154)	(0.157)	(0.159)
Race Effects	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes
State Effects	Yes	Yes	Yes	Yes
N	430,988	430,988	430,988	430,988
R^2	0.150	0.150	0.150	0.150

Notes: Weighted difference-in-difference estimates with cluster-robust standard errors in parentheses. The sample contains Alaska as the treated group and Minnesota, Nevada, Utah, and Wyoming as the control group. Construction employment is measured in thousands. † signals that the coefficient is multiplied by 1000. If a p-value is less than 0.05, the coefficient has a star (*). If a p-value is less than 0.01, it has 2 stars (**). If a p-value is less than 0.001, it has three stars (***).

Sources: Current Population Survey, 1979-1986, and Current Employment Statistics State and Metro Area, 1979-1986.

Table 6. Effect of the Alaska Permanent Fund on Part-Time Employment

	(1)	(2)	(3)	(4)
DiD	0.016**	0.007		
	(0.004)	(0.005)		
Linear Trend†		0.210*		
		(0.055)		
Pre-Event				
1979 DiD				0.002
				(0.004)
1980 DiD				−0.008
				(0.005)
Post-Event				
1982 DiD			0.012	0.010
			(0.006)	(0.004)
1983 DiD			0.014*	0.012*
			(0.003)	(0.003)
1984 DiD			0.018**	0.016**
			(0.003)	(0.002)
1985 DiD			0.019**	0.017***
			(0.003)	(0.001)
Other Controls				
Female (=1 female)	0.128**	0.128**	0.128**	0.128**
	(0.017)	(0.017)	(0.017)	(0.017)
Age	0.002	0.002	0.002	0.002
	(0.003)	(0.003)	(0.003)	(0.003)
Age Squared	−0.000	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Highest Grade	−0.002	−0.002	−0.002	−0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Construction†	−0.439***	−0.435***	−0.432***	−0.431***
	(0.045)	(0.042)	(0.041)	(0.042)
Race Effects	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes
State Effects	Yes	Yes	Yes	Yes
N	430,988	430,988	430,988	430,988
R^2	0.042	0.042	0.042	0.042

Notes: See Table 5.

Table 7. Effect of the Alaska Permanent Fund on Participation

	(1)	(2)	(3)	(4)
DiD	0.004	0.007		
	(0.003)	(0.004)		
Linear Trend†		−0.078		
		(0.059)		
Pre-Event				
1979 DiD				0.002
				(0.006)
1980 DiD				−0.009
				(0.005)
Post-Event				
1982 DiD			0.011	0.008*
			(0.005)	(0.002)
1983 DiD			−0.004	−0.007*
			(0.003)	(0.002)
1984 DiD			0.008	0.005
			(0.004)	(0.004)
1985 DiD			0.001	−0.001
			(0.004)	(0.006)
Other Controls				
Female (=1 female)	−0.242***	−0.242***	−0.242***	−0.242***
	(0.021)	(0.021)	(0.021)	(0.021)
Age	0.042***	0.042***	0.042***	0.042***
	(0.001)	(0.001)	(0.001)	(0.001)
Age Squared	−0.001***	−0.001***	−0.001***	−0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Highest Grade	0.014***	0.014***	0.014***	0.014***
	(0.001)	(0.001)	(0.001)	(0.001)
Construction†	0.025	0.024	0.024	0.025
	(0.124)	(0.123)	(0.127)	(0.131)
Race Effects	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes
State Effects	Yes	Yes	Yes	Yes
N	430,988	430,988	430,988	430,988
R^2	0.184	0.184	0.184	0.184

Notes: See Table 5

Table 8. Effect of the Alaska Permanent Fund on Hours Worked

	(1)	(2)	(3)	(4)
DiD	−0.469	−0.526		
	(0.219)	(0.210)		
Linear Trend†		1.296		
		(1.434)		
Pre-Event				
1979 DiD				−0.119
				(0.340)
1980 DiD				0.432
				(0.316)
Post-Event				
1982 DiD			−0.463	−0.360*
			(0.305)	(0.123)
1983 DiD			−0.917*	−0.815**
			(0.258)	(0.157)
1984 DiD			−0.272	−0.171
			(0.161)	(0.194)
1985 DiD			−0.300	−0.199
			(0.228)	(0.184)
Other Controls				
Female (=1 female)	−9.745***	−9.745***	−9.745***	−9.745***
	(0.820)	(0.820)	(0.820)	(0.820)
Age	0.544**	0.544**	0.544**	0.544**
	(0.076)	(0.076)	(0.076)	(0.076)
Age Squared	−0.006***	−0.006***	−0.006***	−0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
Highest Grade	0.065	0.065	0.064	0.064
	(0.055)	(0.055)	(0.054)	(0.055)
Construction	0.047***	0.047***	0.047***	0.047***
	(0.004)	(0.004)	(0.004)	(0.004)
Race Effects	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes
State Effects	Yes	Yes	Yes	Yes
N	68,747	68,747	68,747	68,747
R^2	0.116	0.116	0.116	0.116

Notes: See Table 5.

pattern in the first four years after the implementation of the policy.

The estimates in column 3 of Table 5 suggest that the policy has a positive impact on employment in the first year and a negative one in 1983. A test of joint significance shows that the post-event coefficients are significant at the 1% level. There is, however, no clear pattern in terms of sign and magnitude. The Alaska Permanent Fund increases employment by 2.4% in 1982 and then decreases it by 0.7% in 1983. The effect vanishes in the following two years.

Table 6 reveals that the Alaska Permanent Fund builds up a positive effect on part-time employment over time. The dummies are again jointly significant at the 1% level. In contrast with the results for employment, the estimates in column 3 reveal treatment effects that increase in magnitude and significance over time. While the impact is null in 1982, it grows from 1.4% in 1983 to 1.8% in 1984 and 1.9% in 1985. These results suggest that the permanent fund's effect on part-time employment emerges only several years after its implementation.

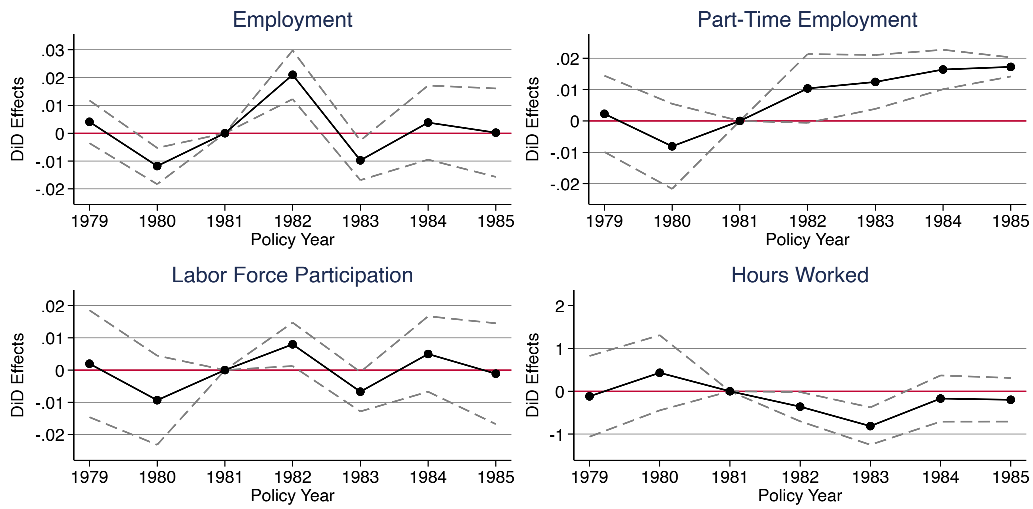
The full effects of the policy on labor force participation are jointly significant at the 1%. Nevertheless, Table 7 shows that the DiD estimates in column 3 are individually statistically insignificant. In addition, their signs follow the same pattern as the ones on employment. In fact, the policy appears to increase participation in the first year and then decrease it in 1983. The signs are then again positive in the last two years. Considering that the DiD estimates from columns 1 and 2 are not significant, the joint significance of the dummies in column 3 is a surprising result. Again, I am not able to identify any clear economic interpretation.

Lastly, as Table 8 reports, there is some evidence for a negative impact of the policy on hours worked. The coefficients are again jointly significant, even though the 1983 dummy is the only significant one. The coefficient suggests that the average hours worked per week decrease by almost one in 1983. While not significant, the other coefficients are also all negative. This result is in line with what I find for part-time employment.

The policy's impact on labor supply happens mainly through a reduction of employment in terms of intensive margins. The coefficients on employment and labor force participation, in fact, suggest an ambiguous impact of the policy on decisions regarding whether to work. I am not confident in concluding that the policy has no effect on these outcomes, but I am also not able to identify an evident economic interpretation. For a graphical representation of all effects, Figure 5 shows results from column 4. These vary only slightly in terms of magnitude and significance from the ones in column 3. The graph, however, provides a useful visual interpretation.

Test for Parallel Trends. Figure 4 shows that the control group including Minnesota, Nevada, Utah, and Wyoming does a good job at matching Alaska in terms of its four labor outcomes in the period from 1995 to 2020. In this section, I evaluate whether the same control group satisfies parallel trends in the years leading to the

Figure 5. Difference-in-Differences Effects by Year



Notes: These are estimates from column (4) of Tables 5-8. Dashed series are the upper and lower limits of the confidence intervals.

implementation of the policy. To do so, I test the joint statistical significance of the pre-event DiD coefficients in Tables 5-8. If the coefficients are jointly significant, I reject the null of parallel trends.

DID regressions with pre-event treatment dummies in column 4 of Tables 5, 6, 7, and 8 indicate that Minnesota, Nevada, Utah, and Wyoming do not comprise a good control group for Alaska's labor market. At a first look, at least in terms of labor force participation, part-time employment and work hours, my control group satisfies parallel trends since all of the lead dummies are individually insignificant. A test of joint significance of the pre-event estimates, however, rejects the null of parallel trends for all regressions but the one on labor force participation. The p-value for this specification, however, is only 0.0574.

Table 9 re-estimates the pre-event dummies without including construction employment and shows that the results vary only minimally. When I do not include this variable, I find that the 1980 coefficient on employment is more significant than the one in column 4 of Table 5. Also, the 1979 coefficients for the regressions on part-time employment and hours worked become statistically significant when I omit the construction variable. Despite these small improvements, the construction variable does not seem to do enough to control for the reduction in employment following the end of the project.

The lack of parallel trends confirms the struggle of finding a control group for Alaska. Figure 4 shows that the task of finding a control group is not as challenging in the span of years 1995 to 2020. The three series, in fact, generally move in the

Table 9. Effect of the Alaska Permanent Fund Without Employment in Construction

	Employment	Part-Time Employment	Labor force Participaton	Hours worked
Pre-Event				
1979 DiD	−0.005	0.010*	0.002	−0.923**
	(0.003)	(0.003)	(0.005)	(0.179)
1980 DiD	−0.017***	−0.004	−0.010	0.006
	(0.002)	(0.004)	(0.004)	(0.244)
Post-Event				
1982 DiD	0.025***	0.007	0.008**	0.017
	(0.003)	(0.003)	(0.002)	(0.111)
1983 DiD	−0.006	0.009	−0.007	−0.498
	(0.003)	(0.003)	(0.003)	(0.214)
1984 DiD	0.004	0.016**	0.005	−0.160
	(0.005)	(0.003)	(0.004)	(0.218)
1985 DiD	−0.002	0.019***	−0.001	−0.443
	(0.006)	(0.001)	(0.005)	(0.203)
Other Controls				
Female (=1 female)	−0.221***	0.128**	−0.242***	−9.748***
	(0.022)	(0.017)	(0.021)	(0.821)
Age	0.043***	0.002	0.042***	0.544**
	(0.001)	(0.003)	(0.001)	(0.077)
Age Squared	−0.001***	−0.000	−0.001***	−0.006***
	(0.000)	(0.000)	(0.000)	(0.001)
Highest Grade	0.020***	−0.002	0.014***	0.062
	(0.001)	(0.001)	(0.001)	(0.055)
Race Effects	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes
State Effects	Yes	Yes	Yes	Yes
N	430,988	430,988	430,988	68,747
R^2	0.150	0.042	0.184	0.115

Notes: See Table 5.

same direction for all labor outcomes, regardless of whether I use synthetic Alaska or the control group with Minnesota, Nevada, Utah, and Wyoming. The end of the construction of the TAPS in 1977 is probably the main reason why the parallel trend assumption fails in the pre-event period.

Even though not reassuring, the conclusions I draw about parallel trends are relevant for any study of the Alaska Permanent Fund. In fact, there seems to be no true group of states that resembles Alaska in the years leading to the implementation of the policy. This means that any result in the context of this natural experiment is not fully reliable.

By-Group Analysis. After estimating the average impact of the policy for all people in Alaska, I turn to estimating effects for some subsets of the sample. Table 10 focuses on employment, Table 11 on part-time employment, Table 12 on labor force participation, and Table 13 on work hours. For each of these labor outcomes, I report effects for four subsets of the sample: men, women, white people, and black people. Table 14 shows by-group estimates of the treatment effects for married people and married females on part-time employment and hours worked only.

Table 10 shows results that are not too different from the ones on the whole sample, with the only exception of black people. It seems that the policy has a positive impact on employment for men, women, and white people in the first year. The effect in 1983, instead, is negative but – in contrast with Table 5 – is not statistically significant for any of the three groups. The effects for the remaining years are not significant for women and white people. Only men in 1984 seem to receive a significant incentive to work. The policy seems to affect black people negatively. The permanent fund decreases employment of blacks by 8% in 1983 and 4.7% 1984.

The results on part-time employment and hours worked are evidence of a strong disincentive to work for females. In table 11 the treatment dummies for 1984 and 1985 are significant and equal to 0.03. These coefficients are greater than the ones on the same dummies for the whole sample. For men and whites, the coefficients are also positive but not as large in magnitude. Again, the outliers are black people whose part-time employment decreases by 1.3% in 1984 due to the policy.

Table 12 shows that the policy significantly affects labor force participation only for men and black people. For men, the effect is positive and significant in all years besides 1983. The increases in labor force participation in 1982, 1984, and 1985 are 0.3%, 1.1%, and 0.7% respectively. In line with what I find for employment, the policy gives men an incentive to join the labor force. The participation rate for black people, instead, decreases by 4.7% in 1983 and increases by 4.0% in 1985. These large but inconsistent results draw no clear economic pattern for black people's labor force participation rates.

As it concerns hours worked, Table 13 shows that the policy pushes women to reduce work hours by 0.9 and 0.5 in 1983 and 1984 respectively. The other columns show effects that are either smaller or insignificant. The only coefficient that stands

Table 10. Effect of the Alaska Permanent Fund on Employment by Sex and Race

	Men	Women	Whites	Blacks
Post-Event				
1982 DiD	0.019***	0.029*	0.024**	0.037
	(0.002)	(0.009)	(0.004)	(0.036)
1983 DiD	−0.005	−0.008	−0.002	−0.080*
	(0.003)	(0.004)	(0.003)	(0.019)
1984 DiD	0.008*	0.007	0.003	−0.047*
	(0.002)	(0.008)	(0.005)	(0.012)
1985 DiD	0.009	0.000	−0.005	0.068
	(0.003)	(0.010)	(0.005)	(0.033)
Other Controls				
Female (=1 female)			−0.227***	−0.107*
			(0.022)	(0.024)
Age	0.044***	0.044***	0.044***	0.030**
	(0.001)	(0.001)	(0.001)	(0.005)
Age Squared	−0.001***	−0.001***	−0.001***	−0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Highest Grade	0.015***	0.026***	0.019***	0.040***
	(0.001)	(0.002)	(0.001)	(0.003)
Construction†	1.061**	−0.067	0.460*	1.832*
	(0.172)	(0.160)	(0.155)	(0.448)
Race Effects	Yes	Yes	No	No
Time Effects	Yes	Yes	Yes	Yes
State Effects	Yes	Yes	Yes	Yes
N	213,474	217,514	399,944	8,716
R^2	0.114	0.078	0.152	0.107

Notes: See Table 5.

Table 11. Effect of the Alaska Permanent Fund on Part-Time Employment by Sex and Race

	Men	Women	Whites	Blacks
Post-Event				
1982 DiD	0.004	0.019	0.008	−0.026
	(0.004)	(0.008)	(0.006)	(0.026)
1983 DiD	0.013**	0.013	0.010	0.006
	(0.002)	(0.005)	(0.004)	(0.019)
1984 DiD	0.009*	0.028**	0.012*	−0.013*
	(0.003)	(0.004)	(0.003)	(0.004)
1985 DiD	0.009	0.026***	0.005	0.019
	(0.005)	(0.001)	(0.002)	(0.016)
Other Controls				
Female (=1 female)			0.133**	0.028**
			(0.017)	(0.005)
Age	−0.012***	0.016*	0.003	−0.018*
	(0.001)	(0.005)	(0.003)	(0.004)
Age Squared	0.000***	−0.000*	−0.000	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
Highest Grade	−0.002	−0.001	−0.002	−0.000
	(0.001)	(0.002)	(0.001)	(0.003)
Construction†	−0.707***	−0.162*	−0.429***	−0.229
	(0.031)	(0.056)	(0.041)	(0.379)
Race Effects	Yes	Yes	No	No
Time Effects	Yes	Yes	Yes	Yes
State Effects	Yes	Yes	Yes	Yes
N	213,474	217,514	399,944	8,716
R^2	0.007	0.013	0.044	0.020

Notes: See Table 5.

Table 12. Effect of the Alaska Permanent Fund on Participation by Sex and Race

	Men	Women	Whites	Blacks
Post-Event				
1982 DiD	0.003*	0.017	0.011	−0.023
	(0.001)	(0.011)	(0.006)	(0.025)
1983 DiD	−0.003	−0.005	0.001	−0.047*
	(0.003)	(0.005)	(0.004)	(0.013)
1984 DiD	0.011*	0.006	0.003	−0.026
	(0.003)	(0.007)	(0.004)	(0.017)
1985 DiD	0.007*	−0.002	−0.007	0.040*
	(0.002)	(0.008)	(0.003)	(0.014)
Other Controls				
Female (=1 female)			−0.246***	−0.126***
			(0.021)	(0.008)
Age	0.042***	0.043***	0.042***	0.036*
	(0.001)	(0.001)	(0.001)	(0.008)
Age Squared	−0.001***	−0.001***	−0.001***	−0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Highest Grade	0.008***	0.023***	0.013***	0.032***
	(0.001)	(0.002)	(0.001)	(0.001)
Construction†	0.285	−0.247	−0.009	0.228
	(0.104)	(0.160)	(0.128)	(0.356)
Race Effects	Yes	Yes	No	No
Time Effects	Yes	Yes	Yes	Yes
State Effects	Yes	Yes	Yes	Yes
N	213,474	217,514	399,944	8,716
R^2	0.158	0.087	0.186	0.140

Notes: See Table 5.

Table 13. Effect of the Alaska Permanent Fund on Hours Worked by Sex and Race

	Men	Women	Whites	Blacks
Post-Event				
1982 DiD	−0.592	−0.389	−0.070	3.669
	(0.289)	(0.407)	(0.327)	(1.439)
1983 DiD	−0.867*	−0.899*	−0.684	3.716***
	(0.256)	(0.282)	(0.260)	(0.308)
1984 DiD	−0.071	−0.514*	0.206	1.171
	(0.219)	(0.172)	(0.176)	(0.656)
1985 DiD	−0.378	−0.154	0.249	1.022
	(0.471)	(0.224)	(0.216)	(0.526)
Other Controls				
Female (=1 female)			−9.986***	−3.642**
			(0.760)	(0.707)
Age	0.922***	0.115	0.529**	0.337*
	(0.064)	(0.159)	(0.077)	(0.078)
Age Squared	−0.011***	−0.001	−0.006***	−0.005**
	(0.001)	(0.001)	(0.001)	(0.001)
Highest Grade	−0.090	0.299*	0.048	0.410
	(0.112)	(0.073)	(0.052)	(0.314)
Construction†	55.767**	35.839**	48.889***	−54.329
	(6.847)	(4.404)	(3.329)	(27.271)
Race Effects	Yes	Yes	No	No
Time Effects	Yes	Yes	Yes	Yes
State Effects	Yes	Yes	Yes	Yes
N	38,062	30,685	64,429	1,445
R^2	0.017	0.012	0.120	0.067

Notes: See Table 5.

Table 14. Effect of the Alaska Permanent Fund on Part-Time Employment and Hours Worked by Marital Status

	Part-Time Employment		Hours Worked	
	Married People	Married Women	Married People	Married Women
Post-Event				
1982 DiD	0.017	0.023	−1.055	−1.329
	(0.008)	(0.012)	(0.427)	(0.791)
1983 DiD	0.022**	0.027*	−1.194*	−2.081*
	(0.004)	(0.007)	(0.301)	(0.573)
1984 DiD	0.031***	0.044***	−1.060*	−1.944**
	(0.003)	(0.003)	(0.272)	(0.358)
1985 DiD	0.029***	0.039***	−0.911*	−1.203*
	(0.002)	(0.003)	(0.299)	(0.323)
Other Controls				
Female (=1 female)	0.159**		−11.821***	
	(0.020)		(0.829)	
Age	0.004	0.019*	0.555**	0.167
	(0.004)	(0.006)	(0.119)	(0.215)
Age Squared	−0.000	−0.000*	−0.007**	−0.002
	(0.000)	(0.000)	(0.001)	(0.002)
Highest Grade	−0.000	0.001	−0.067	0.069
	(0.001)	(0.002)	(0.073)	(0.094)
Construction†	−0.522***	−0.291*	61.037***	56.137**
	(0.054)	(0.094)	(4.708)	(10.203)
Race Effects	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes
State Effects	Yes	Yes	Yes	Yes
N	330,051	166,813	52,093	22,038
R^2	0.061	0.017	0.154	0.013

Notes: See Table 5.

out is the estimated impact of the policy on hours worked for black people in 1983. The coefficient, equal to 3.7, is extremely large.

Table 14's estimates on samples of married people and married women identify even stronger disincentives for these two groups. The Alaska Permanent Fund increases part-time employment for married people by 2.2%, 3.1%, and 2.9% respectively in 1983, 1984, and 1985. The impact on married women is even larger. The three dummies grow to 2.7%, 4.5%, and 4.2%, the largest value I find in all of the part-time regressions. Focusing on hours worked, I find a similar pattern. For married people, the policy decreases weekly work hours by 1.2, 1.0, and 0.8. For married females, the effect almost doubles to 2.0, 1.9, and 1.2.

Based on my results, I conclude that the policy has a significant impact on employment and labor force participation for men and on part-time employment and work hours for females and married people. The permanent fund makes men work more overall and only reduces only slightly their work hours and part-time rates. On the other hand, it creates a large disincentive for women. The impacts of the policy on part-time employment and hours worked for married people, and married females suggest that decisions to work part-time are more likely for those living with a partner. With another person in the same household, receiving unconditional payments pushes people to spend less time at work.

Lastly, it appears that the effects on blacks move in a direction that is opposite to that of all other subgroups. The small number of observations and the lack of a consistent pattern for all outcomes make me less confident on the results for this subgroup.

DiD on Skills

My regressions on skills are useful to investigate whether the permanent fund affects the skill portfolio of Alaska's labor market. Table 15 reports results for managerial skills, Table 16 for social skills, Table 17 for analytical skills, Table 18 for mechanical skills, and Table 19 for fundamental skills. In each table, I present 4 different specifications: a simple DiD, a simple DiD with a differential time trend, a DiD with multiple treatment effects, and a DiD with leads and lags.

Table 15 shows sensible estimates of my covariates in the context of managerial skills that do not vary across specifications. The share of managerial skills is 1 percentage point lower for women. The result underlines how women are less likely to have jobs in management positions. Managerial skills gain importance relative to all skills at a decreasing rate as age increases up to 46 years old. After this threshold, the effect becomes negative. Lastly, the share of managerial skills increases with education at a rate of 0.06 percentage points per grade.

The patterns for social skills are similar. The share of social skills increases at a decreasing rate with age. The coefficient on education is still positive but much larger and equal to 0.36. Also, in contrast with the results for managerial skills,

Table 15. Effect of the Alaska Permanent Fund on Managerial Skills

	(1)	(2)	(3)	(4)
Simple DiD	−0.022*	−0.044		
	(0.005)	(0.017)		
Linear Trend		0.001		
		(0.000)		
Pre-Event				
1979 DiD				0.033
				(0.034)
1980 DiD				0.048
				(0.025)
Post-Event				
1982 DiD			−0.061*	−0.035*
			(0.014)	(0.008)
1983 DiD			−0.021	0.006
			(0.016)	(0.029)
1984 DiD			−0.005	0.022
			(0.008)	(0.025)
1985 DiD			−0.009	0.018
			(0.019)	(0.021)
Other Controls				
Female (=1 female)	−1.067***	−1.067***	−1.067***	−1.067***
	(0.049)	(0.049)	(0.049)	(0.049)
Age	0.014*	0.014*	0.014*	0.013*
	(0.003)	(0.003)	(0.003)	(0.003)
Age Squared	−0.000*	−0.000*	−0.000*	−0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
Highest Grade	0.062***	0.062***	0.062***	0.062***
	(0.006)	(0.006)	(0.006)	(0.006)
Construction†	0.464	0.473	0.504	0.534
	(0.200)	(0.194)	(0.190)	(0.202)
Race Effects	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes
State Effects	Yes	Yes	Yes	Yes
N	342,643	342,643	342,643	342,643
R^2	0.086	0.086	0.086	0.086

Notes: Weighted difference-in-difference estimates with cluster-robust standard errors in parentheses. The sample contains Alaska as the treated group and Minnesota, Nevada, Utah, and Wyoming as the control group. Construction employment is measured in thousands. The dependent skill variable is measured relative to total skills. † signals that the coefficient is multiplied by 1000. If a p-value is less than 0.05, the coefficient has a star (*). If a p-value is less than 0.01, it has 2 stars (**). If a p-value is less than 0.001, it has three stars (***).

Sources: Current Population Survey, 1979-1986, Current Employment Statistics State and Metro Area, 1979-1986, and O*NET, 2003-2006.

Table 16. Effect of the Alaska Permanent Fund on Social Skills

	(1)	(2)	(3)	(4)
Simple DiD	0.075*	0.078**		
	(0.019)	(0.010)		
Linear Trend		−0.000		
		(0.001)		
Pre-Event				
1979 DiD				−0.053
				(0.068)
1980 DiD				−0.098*
				(0.025)
Post-Event				
1982 DiD			0.122*	0.073*
			(0.029)	(0.018)
1983 DiD			0.102*	0.052*
			(0.023)	(0.015)
1984 DiD			0.011	−0.039
			(0.005)	(0.027)
1985 DiD			0.077	0.026
			(0.030)	(0.014)
Other Controls				
Female (=1 female)	2.203***	2.203***	2.203***	2.203***
	(0.075)	(0.075)	(0.075)	(0.075)
Age	0.020**	0.020**	0.020**	0.020**
	(0.003)	(0.003)	(0.003)	(0.003)
Age Squared	−0.000	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Highest Grade	0.358***	0.358***	0.358***	0.358***
	(0.041)	(0.041)	(0.041)	(0.041)
Construction†	−2.635***	−2.636***	−2.692***	−2.741***
	(0.165)	(0.158)	(0.175)	(0.223)
Race Effects	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes
State Effects	Yes	Yes	Yes	Yes
N	342,643	342,643	342,643	342,643
R^2	0.195	0.195	0.195	0.195

Notes: See Table 15.

Table 17. Effect of the Alaska Permanent Fund on Analytical Skills

	(1)	(2)	(3)	(4)
Simple DiD	0.048**	0.071***		
	(0.007)	(0.005)		
Linear Trend		−0.001		
		(0.000)		
Pre-Event				
1979 DiD				−0.024
				(0.032)
1980 DiD				−0.144***
				(0.015)
Post-Event				
1982 DiD			0.092**	0.036
			(0.012)	(0.015)
1983 DiD			0.079*	0.023
			(0.023)	(0.024)
1984 DiD			−0.009	−0.065*
			(0.015)	(0.023)
1985 DiD			0.044	−0.012
			(0.017)	(0.029)
Other Controls				
Female (=1 female)	0.539**	0.539**	0.539**	0.539**
	(0.100)	(0.100)	(0.100)	(0.100)
Age	0.051***	0.051***	0.051***	0.051***
	(0.005)	(0.005)	(0.005)	(0.005)
Age Squared	−0.000*	−0.000*	−0.000*	−0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
Highest Grade	0.124***	0.124***	0.124***	0.124***
	(0.014)	(0.014)	(0.014)	(0.014)
Construction†	3.018*	3.009*	2.958*	2.930*
	(0.822)	(0.815)	(0.808)	(0.822)
Race Effects	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes
State Effects	Yes	Yes	Yes	Yes
N	342,643	342,643	342,643	342,643
R^2	0.031	0.031	0.031	0.031

Notes: See Table 15.

Table 18. Effect of the Alaska Permanent Fund on Mechanical Skills

	(1)	(2)	(3)	(4)
Simple DiD	−0.179*	−0.201**		
	(0.040)	(0.041)		
Linear Trend		0.000		
		(0.001)		
Pre-Event				
1979 DiD				0.062
				(0.105)
1980 DiD				0.249**
				(0.034)
Post-Event				
1982 DiD			−0.256**	−0.153*
			(0.048)	(0.038)
1983 DiD			−0.278**	−0.175***
			(0.057)	(0.017)
1984 DiD			−0.028	0.076
			(0.039)	(0.035)
1985 DiD			−0.185*	−0.081
			(0.049)	(0.042)
Other Controls				
Female (=1 female)	−3.677***	−3.677***	−3.677***	−3.677***
	(0.240)	(0.240)	(0.240)	(0.240)
Age	−0.118***	−0.118***	−0.118***	−0.118***
	(0.008)	(0.008)	(0.008)	(0.008)
Age Squared	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Highest Grade	−0.993***	−0.993***	−0.993***	−0.993***
	(0.055)	(0.055)	(0.055)	(0.055)
Construction†	4.379*	4.388*	4.511*	4.575*
	(1.097)	(1.084)	(1.081)	(1.150)
Race Effects	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes
State Effects	Yes	Yes	Yes	Yes
N	342,643	342,643	342,643	342,643
R^2	0.242	0.242	0.242	0.242

Notes: See Table 15.

Table 19. Effect of the Alaska Permanent Fund on Fundamental Skills

	(1)	(2)	(3)	(4)
Simple DiD	0.079*	0.096**		
	(0.021)	(0.020)		
Linear Trend		−0.000		
		(0.001)		
Pre-Event				
1979 DiD				−0.018
				(0.044)
1980 DiD				−0.055
				(0.024)
Post-Event				
1982 DiD			0.123**	0.079**
			(0.022)	(0.014)
1983 DiD			0.143***	0.094***
			(0.013)	(0.006)
1984 DiD			0.070*	0.005
			(0.019)	(0.009)
1985 DiD			0.102*	0.049*
			(0.027)	(0.012)
Other Controls				
Female (=1 female)	2.002***	2.002***	2.134***	2.002***
	(0.083)	(0.083)	(0.108)	(0.083)
Age	0.034**	0.034**	0.037*	0.034**
	(0.007)	(0.007)	(0.010)	(0.007)
Age Squared	−0.000*	−0.000*	−0.000*	−0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
Highest Grade	0.449***	0.449***	0.414***	0.449***
	(0.026)	(0.026)	(0.026)	(0.026)
Construction†	−5.227***	−5.234***	−5.186***	−5.299***
	(0.250)	(0.238)	(0.264)	(0.263)
Race Effects	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes
State Effects	Yes	Yes	Yes	Yes
N	342,643	342,643	342,643	342,643
R^2	0.266	0.266	0.264	0.266

Notes: See Table 15.

the importance of social skills relative to all skills is much higher for women (2 percentage points). This result reveals that women have jobs in which social skills play a larger role.

The coefficients on the covariates are all positive for analytical skills. The importance that analytical skills have in the overall portfolio increases at a decreasing rate with age, falls by 0.12 percentage points for each grade, and is higher for women (0.5 percentage points). The result is in line with the idea that older and more educated people work in jobs that require more technical skills. Also, it reveals that women have jobs in which analytical skills matter more.

Compared to analytical skills, all of the coefficients reverse sign for mechanical skills in Table 18. They are negative for both age and education. One additional year of school is associated with a decrease by 1 percentage point of the importance of mechanical skills relative to all skills. Also, the share of mechanical skills is 3.7 percentage points lower for women.

The signs for fundamental skills resemble those of analytical skills. The importance of this skill relative to all skills increases at a decreasing rate for age, and goes up by 0.45 with each additional grade of schooling. Compared to that of men, the average skill portfolio of women gives more importance to fundamental skills.

Overall, these results indicate that more educated and older workers are less likely to work in jobs in which mechanical skills have a larger weight. At the same time, there are fewer women in management jobs and in occupations that give large weight to mechanical skills.

Simple DiD. The Alaska Permanent Fund overall has a positive impact on social, analytical, and fundamental skills and reduces the share of managerial and mechanical skills. Since the estimated time trends are very small and statistically insignificant in each table, I focus on the estimates in column 1. Table 15 shows that the coefficient is significant and equals -0.02 for managerial skills. Table 16 shows that the policy has a positive and significant effect on the share of social skills equal to 0.07 percentage points. The estimated treatment effect on analytical skills is also positive and significant. Table 17 shows that the coefficient is equal to 0.05. The DiD estimate for mechanical skills in Table 18 instead is negative. The Alaska Permanent Fund decreases the share of mechanical skills by 0.18 percentage points. The Alaska Permanent Fund also increases fundamental skills. The estimate in Table 19 is equal to 0.08.

Multiple Treatment Effects. The results in column 3 of all tables confirm the presence of a significant impact on Alaska's average skill portfolio. In fact, a test of joint significance shows that the lags in treatment are statistically significant for all regressions.

The reduction in managerial skills happens in the first year after the policy was implemented, with a coefficient equal to -0.06. The lack of individual statistical significance of the other post-event dummies suggests no evidence for lags in treatment. This result suggests that the policy might not affect the share of managerial skills at all. The impact of the Alaska Permanent Fund, however, is systematically significant for the other skills.

Table 17 shows that the share of social skills increases by 0.12 and 0.10 in the first two years of the policy. The effect then vanishes for all of the remaining years. These results could imply that the policy affects social skills only in the very short-run.

A similar pattern emerges for analytical skills. Their share increases by 0.09 in 1982 and then falls to 0.08 in the following year. The effect then exhausts almost immediately. Although not significant, the 1984 coefficient reaches even a negative value.

It seems that mechanical skills, instead, consistently decrease due to the Alaska Permanent Fund. The negative impact equals -0.26 in 1982 and peaks in magnitude at -0.28 in 1983. It loses significance in 1984 and then grows again in 1985, reaching -0.18. Unlike what happens for social and analytical skills, the policy's impact on the share of mechanical skills maintains its significance until 1985.

Fundamental skills seem to consistently increase in all years of my analysis. Similarly to what happens with mechanical skills, the effect peaks in magnitude in 1983 and then decreases over time. In chronological order, the effects are equal to 0.12, 0.14, 0.07, and 0.10.

The Alaska Permanent Fund leads to an increase in the importance of social, analytical, and fundamental skills mainly at the expense of mechanical skills. The increase is largest for fundamental skills. The results are in line with the theory of human capital. If people have their basic needs covered, they decide to retrain and move to high skill jobs that are also more appealing in terms of social interactions and more reliant on fundamental skills. On the other hand, they abandon jobs low-skill characterized by a large share of mechanical skills — a conclusion based on the negative relationship between education on the share of mechanical skills.

The effect on managerial skills is less clear. While the simple DiD estimate points to a decrease in the share of managerial skills, the estimation with multiple treatment effects does not show a pattern that is consistent over time. A reduction in managerial skills would partially contradict human capital accumulation. The results would in fact suggest that unconditional cash transfers provide a disincentive for entrepreneurship.

Test for Parallel Trends. The pre-event dummies in Tables 15–19 are significant in all skill regressions. Similarly to what I find for the regressions on labor outcomes, the variables are jointly significant even when they are individually insignificant. I reject the null at the 1% level for managerial, social, analytical and mechanical skills, and at the 5% for fundamental skills. The takeaway is the same as the one for the labor outcomes regressions. It seems that matching Alaska in terms of skills in the years of the Permanent Fund is very complicated. For the same reason, I am also skeptical about these results.

6. Summary and Conclusions

I study the potential impact of UBI on labor outcomes and human capital by analyzing the natural experiment of the Alaska Permanent Fund: an unconditional cash transfer program in Alaska since 1982. I use data on employment and work hours from the CPS and skill data from O*NET to evaluate whether the unconditional payments give Alaska residents an incentive to work less, and whether they affect the composition of skills of Alaska's labor market.

Because (1) CPS data for Alaska is only available from 1977 and (2) the construction of the Trans-Alaska Pipeline System heavily affected employment in Alaska in the 1970s, it is difficult to find control states using the pre-event data. For this reason, I apply synthetic control from 1995-2020, treating 2021 as a placebo event, to find an appropriate control group. Based on the results of synthetic control, I find that Minnesota, Nevada, Utah, and Wyoming properly fit Alaska's employment and hours series since 1995.

The DiD estimates from 1979 to 1985 suggest that the Alaska Permanent Fund affects employment only through intensive margins. While the impact on employment and on labor force participation is ambiguous, the only significant results show that the policy increases part-time employment and reduces work hours. I also find evidence that the Alaska Permanent Fund affects Alaska's average skill portfolio. Unconditional cash transfers push people towards jobs in which analytical, fundamental, and social skills play a larger role. The increase in importance of these three skills in Alaska's skill portfolio seems to be at the expense of mechanical skills and, with only little evidence, of managerial skills.

Essentially all of the DiD regressions fail to satisfy parallel trends; this result, however, is an important finding on its own. For instance, it could explain why the treatment effects in the context of employment, labor force participation and managerial skills do not follow a meaningful pattern in time. Moreover, the rejection of parallel trends serves as a warning with respect to the estimated coefficients that are neither constant nor increase over the years. Results such as the decrease in employment in the intensive margins and the movement of workers towards high-skill jobs are in line with theory on credit constraints and human capital. However, these patterns may just be due to Alaska's unusual trends in employment and skill composition. Alaska is already an uncommon state, and it was especially uncommon in the years following the construction of its pipeline system.[3]

These conclusions underline the limitations of all studies of the Alaska Per-

[3] "TAPS was an unusual event and Alaska is an unusual place" (Carrington 1996, p. 217).

manent Fund. In particular, there is reason to be skeptical of Jones and Marinescu's (2022) results. Using synthetic control in the pre-event period allows them to match Alaska in some labor outcomes. However, it seems plausible that their fit in the years leading to the policy's implementation arises due to spurious relationships. First, they fail to identify a control group of states that is constant across labor outcomes. Second, some of their estimations assign relevant weights to states that are too qualitatively different from Alaska, such as the District of Columbia and Louisiana in the main specifications, or Hawaii and Pennsylvania in some of the by-group specifications. Lastly, they are only able to achieve a satisfactory fit for employment and part-time employment, but reportedly fail to match Alaska in terms of labor force participation and work hours. For this reason, the treatment effects they estimate could be just capturing patterns in employment that are unique to Alaska.

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