The Impacts of the Alaska Permanent Fund Dividend on High School Status Completion Rates

A Synthetic Control Study

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Abstract

Direct cash transfer programs have shown success as poverty interventions in both the developing and developed world, yet little research exists examining the society-wide outcomes of an unconditional cash transfer program disbursed without means-testing. This paper attempts to determine the impact of direct cash transfers on educational outcomes in a developed society by investigating the impacts of the Alaska Permanent Fund Dividend, which was launched in 1982 and continues to be disbursed on an annual basis to every Alaskan. A synthetic control model is deployed to examine the path of educational attainment among Alaskans between 1977 and 1991 in order to determine if high school status completion rates after the launch of the dividend diverge from the synthetic in a manner suggestive of a treatment effect.

Introduction

In 1976, the people of Alaska voted to amend their state's constitution in order to allow the creation of a dedicated fund that would be managed independently from the day-to-day spending of the government. The purpose of the fund was to hold and invest the oil revenues that would begin filling the state's coffers with the completion of the Trans Alaska Pipeline the following year. The vote enshrined Article IX, Section 15 of the Alaska Constitution, which holds that a minimum of twenty-five percent of Alaska's mineral revenue are to flow directly into the Permanent Fund.

In 1980, the Alaskan legislature created the Alaska Permanent Fund Corporation, a quasi-independent entity tasked with managing the growing assets of the Fund. The same year, the legislature established the Permanent Fund Dividend (PFD), which pays a cash sum based on a five-year average of the Fund's performance to all state residents who have lived in Alaska for at least a full calendar year. Its first year, the fund paid out $1000 USD to all eligible residents—a sum roughly equal to $2600 today. The Alaska Permanent Fund is now valued at over $60 billion.

Dividends from the Fund are issued annually to every man, woman and child in the state of Alaska, and have ranged over the course of its existence from approximately $300 to approximately $2000. Since petroleum production is the source of the fund's value, dividend payouts vary with the price of oil. In addition, the State has some latitude to divert money from the fund and to influence the size of the dividend. In 2008, Alaska's state legislature approved an additional one-time payment of $1200 from the fund to all the state's residents.

In-depth empirical investigation of the Alaska Permanent Fund holds substantial interest for contemporary political discourse. Though the size of the dividend has historically been too small to constitute a living wage, mention of the Fund has begun to appear in recent conversations about the concept of universal basic income and, more broadly, in discussions about the role of cash transfers in shaping development outcomes. Modest basic income trials have recently been initiated in several locales (e.g. Goodman, 2018). The ideological versatility of these programs has been widely lauded: American libertarians view cash transfers as a salve for the inefficiencies of the bureaucratic welfare state (Zwolinski, 2014) and democratic socialists see social wealth funds (a scenario in which dividends would come from funds built through government purchases of ownership in private companies using tax revenue from high earners) as a pragmatic path to public ownership of the means of production (Bruenig, 2017).

Some previous research into the virtues of Alaska's dividend has focused on labor market outcomes: since a common criticism of unconditional cash transfers is that they reduce incentives to work, economists have sought relationships between changes in part- and full-time employment and the disbursement of the dividend. An NBER working paper from this year (Jones & Marinescu, 2018) found no significant reduction in aggregate employment as a result of the dividend. Another recent study (Berman, 2018) identified significant reductions in the poverty rate among Alaska's Native population.

There is considerable research on the impact of cash transfers on a wide variety of vital measures in developed and developing economies, from birth outcomes (Amarante, Manacorda, Miguel & Vigorito, 2011) and education (Edmonds, 2006) to entrepreneurship (de Mel, McKenzie & Woodruff, 2007) and mental health (Baird, Feirreira, Özler & Woolcock, 2013). In North America, this research is augmented by a large body of studies investigating the impact of the Earned Income Tax Credit (EITC), the results of trials designed to assess the impacts of a negative income tax (NIT), and analysis of data produced by the Manitoba Basic Annual Income Experiment ("Mincome").

In this paper, I investigate one avenue by which the disbursement of the PFD could potentially influence social welfare: educational attainment. As an outcome variable, I use high school status completion rate, which I calculate using the Current Population Survey for each available state and year in the timeframe 1977-1991. The high school status completion rate is defined as the proportion of 18- to 24-year-olds who hold a high school diploma or alternative credential. Using a synthetic control model, I attempt to discern differences between educational attainment trends in Alaska and in a "synthetic Alaska"—a counterfactual version of the state in which no dividend is disbursed.

Literature Review

Earned Income Tax Credit

Though no directly comparable program to the Alaska Permanent Fund Dividend exists elsewhere in the United States, the Earned Income Tax Credit (EITC) arguably provides a strong prior basis for believing that the PFD might result in positive social welfare impacts. The EITC is a federal tax credit, begun in 1975, and is considered by some to be the "cornerstone of U.S. anti-poverty policy" (Hoynes 2014) and the "most effective anti-poverty program for working families" (Eckholm 2007). This latter claim is important: the EITC is effectively an earnings subsidy, and individuals who do not work are not eligible to receive the transfer. The EITC initially came about, in part, as a reaction to proposals for a negative income tax (NIT), a policy most forcefully advocated for by Milton Friedman (Moffitt 2003). Though NIT payments would have reduced with increased work hours, early EITC proponents believed that the main benefits of the NIT would accrue to those without employment, and would therefore reduce work incentives (Hotz & Scholz 2003); the EITC proved to be a more politically palatable alternative.

In its four decades of existence, the EITC has been seen by many to have been a resounding success. In 1997 and 1998, for example, the program lifted more than 4 million families out of poverty (Council of Economic Advisers 1998). Perhaps more compellingly for the current research, a marginal $1000 EITC exposure for children between 13 and 18 is associated with increased odds of completing high school and college (Bastian & Michelmore 2018). This is congruent with other research suggesting that EITC exposure in a student's senior year of high school is associated with an increase in college enrollment the following year (Manoli & Turner 2018), and that the credit seems to improve elementary school students' test scores (Chetty, Saez & Rockoff 2011). A larger body of research, though outside the scope of the present study, connects the EITC to a variety of other positive welfare outcomes, such as a decreased number of low-birthweight births (Hoynes, Miller & Simon, 2017).

Manoli & Turner (2018) propose that credit constraints and excess sensitivity to cash-on-hand partially explain the apparently large effect of EITC payments on educational attainment in some settings. They observe that university enrollment involves up-front, out-of-pocket costs that may not be anticipated or planned for by families in financially constrained circumstances, and note that this observation is consistent with the finding that the apparent income effects on college enrollment do not persist at higher income levels. Support for an analogous circumstance in the case of high school completion can be found in the educational literature, such as the work of Doll, Eslami & Walters (2013), who conduct an exhaustive review of research on high school non-completion. These authors note that that students who drop out toward the end of their high school educations often cite the incompatibility of work and school as their reason for dropping out. If the EITC and other comparable programs provide sufficient financial cushion in order to bring work and school out of conflict, an increase in completion rates could result.

NIT Trials

The EITC as it currently exists and the NIT as it was proposed are subtly different programs. In its simplest initial formulations by Milton Friedman (Friedman 2002; Friedman 1987), the NIT would have entailed a fixed amount of allowances—essentially a minimum income level—and a subsidy rate governing how much of the gap between a family's earned income and the minimium income level would be made up with government assistance. For example, with an allowance of $20,000 and a subsidy rate of 20%, a family earning $10,000 would receive $2,000 from the government; a family earning $15,000 would receive $1,000 and a family earning $19,000 would receive $200.

Friedman saw this setup as a potential replacement for a bricolage of federal welfare programs that seemed to create situations in which working more resulted in overall benefit reductions for poor families.

Yet the simplicity of a single, federally mandated minimum income level may have been the death of the idea: income levels high enough to protect urban working families would have been enough to comfortably (if not luxuriously) support rural loafers, creating a politically unsustainable state of affairs (Frank 2006).

During the heyday of the NIT debate in the United States, several small-scale experiments were conducted to investigate the impact of such a program on social welfare. One such program was conducted in Gary, Indiana, where positive effects on school performance were found for fourth- through seventh-graders, with increased effects associated with longer time spent in the program (Maynard & Murnane, 1979). Another study, known as the Seattle-Denver Income Maintenance Experiment (SIME/DIME), found increases in school attendance associated with participation in a similar minimum income program (Venti, 1984). The New Jersey Experiment in Income Maintenance, conducted around the same time, found increases in educational attainment associated with program participation (Hanushek, 2003). Hanushek (2003) notes that one possible cause of increased school attendance may be that income subsidies reduce the opportunity cost of attending school.

The findings of Venti (1984), in particular, lend credence to the suggestion, made above, that income support can lead to a substitution of school for work in high school students; Venti finds strong and statistically significant reductions in the probability of working for 16- to 21-year-olds that are offset by an increase in school attendance. In contrast, Maynard & Murnane (1979) find no effect on older children, but they stress that the samples under review—in Gary, Indiana, and in rural North Carolina—are not representative of the U.S. student population as a whole.

The transferability of results from NIT trials to the PFD case is promising but unclear, since the transfers are of qualitatively different kinds. The NIT, like the EITC, is not universal; indeed, few cash transfer programs are. More importantly, however, the NIT experiments were explicitly short-term experiments, and it is unclear whether families responded differently in that knowledge. Would these families have smoothed their consumption differently if the trial had instead been a permanent program? The answer to this question is impossible to know, but bears directly on the generalizability of these findings to the Alaskan case.

Non-EITC Cash Transfer Programs

A growing body of research, largely in the developing world but including some more recent work in developed countries, provides evidence for various positive welfare impacts of unconditional cash transfer programs. In Uruguay, researchers found that a series of transfers amounting to roughly a doubling of monthly income was associated with a 1.7% reduction in low-birthweight births among mothers who received the transfer (Amarante, Manacorda, Miguel & Vigorito, 2011), and similar studies elsewhere have found that cash transfers can positively influence structural determinants of overall health (Owusu-Addo, Renzaho & Smith, 2018). However, meta-analyses have found inconclusive evidence of summary health effects of cash transfer programs, at least in lower-middle-income countries (Pega et al., 2017).

With respect to education, some evidence from the developing world points to substantial positive impacts on attendance and enrollment from unconditional transfer programs (Benhassine, Devoto, Duflo, Dupas & Pouliquen, 2013), while other research indicates limited enrollment effects and slight positive impacts on completion rates (Araujo, Bosch & Schady, 2016).

Unconditional cash transfer programs have also been attempted on a preliminary basis in the developed world. A recent basic income trial in Finland yields some positive social welfare impacts (for example, on well-being) but no positive conclusions regarding education (Finnish Ministry of Social Affairs and Health, 2019). In the United States, the Eastern Band of Cherokee Indians, located in North Carolina, has distributed a portion of casino profits to all adult tribal members since 1997; these disbursements are associated with a significant positive impact on educational attainment on the basis of an additional income (on average) of $4000 per year. Akee, Copeland, Keeler, Angold & Costello (2010) find that this additional income increases educational attainment by 1 year at age 21.

One of the most well-known early experiments with unconditional cash transfers in the developed world was conducted in Manitoba between 1974 and 1979. This program is more closely comparable to the Alaskan case because the experiment included a saturation site—the town of Dauphin—in which all eligible residents were entitled to receive a cash transfer; nevertheless, only a third of residents qualified. This experiment seems to have resulted in modestly higher educational attainment among treated households, with an increase in the proportion of students progressing from Grade 11 to Grade 12 (Forget, 2011); research also indicated a decrease in hospitalizations, particularly those related to mental health and accidental injury.

Forget (2011) notes a puzzle in the Mincome data that has direct applicability to the Alaska case: though only a third of families in Dauphin (the saturation site) qualified for payments, and though many of these payments were small, an education effect is apparent and statistically significant. Forget observes that the students most likely to leave school came from the same low-income families that were eligible for the Mincome stipend; high-income students were already relatively likely to finish school. The same logic applies to the Alaska case: though all families receive the dividend, an effect an education could still be perceptible due to the payment's differential effect on low-income students, e.g. those most likely not to finish high school.

Previous research on the PFD

In the three-and-a-half decades of the Alaska Permanent Fund Dividend, researchers have investigated various aspects of the program. Two papers in particular can be considered important precedents for my own: the first is a well-known NBER working paper by Damon Jones and Ioana Marinescu (2018), who fit a synthetic control model (upon which I closely model my own) in order to estimate the effects of the PFD on part- and full-time employment in Alaska. The second, by Matthew Berman (2018), finds that the PFD has had a significant positive impact on poverty among indigenous people in Alaska.

Jones and Marinescu use data from the Current Population Survey (CPS) to form a synthetic control for Alaska from 1977 (the earliest year for which CPS data is available specifically for Alaska) through 2014. They find no difference in employment, labor force participation or hours worked as a result of the PFD; however, they do find a statistically significant increase in the part-time rate. In particular, the apparent null effect on hours worked suggests that the hypothesized causal pathway in which the PFD reduces work hours for high school students, thereby bringing work and school out of conflict and increasing status completion rates, may not be supported by the evidence.

Berman (2018) uses Census data to identify the portion of individuals' income constituted by the PFD and compares it to their incomes without the PFD; in this manner, he identifies the portion of indigenous Alaskans moved above the poverty threshold by means of the dividend: for some time periods, Berman finds differences as large as 10 percentage points between Native poverty rates with and without the PFD.

Hsieh (2003) uses the PFD as a means to examine the permanent income hypothesis, which holds that consumers should not respond to previously anticipated changes in their income. Using data from the Consumer Expenditure Survey (CEX), Hsieh finds evidence that Alaskan households smooth their spending in a way that accords with the predictions of the life-cycle/permanent income hypothesis. This finding is accompanied by evidence that the same sample of houses do *not* appear to smooth their incomes in response to expected income tax refunds. Hsieh attributes this seeming discrepancy to bounded rationality, citing the differing cognitive demands placed on households when they consider tax refunds and the PFD, respectively. The Permanent Fund Dividend is distributed annually and without fail; moreover, since its amounts are based on a five-year average of oil revenues, year-to-year amounts are not extremely volatile and are therefore easily foreseen even by unsophisticated individuals.

Hsieh's work is therefore in some tension with the mechanism proposed for the educational impact of the EITC by Manoli & Turner (2013). If Alaskan households successfully smooth their consumption in anticipation of receipt of the PFD, then the dividend funds are not available as excess cash-on-hand in event of unanticipated expenses at the time of college enrollment, or at any other time. More broadly, if the permanent income hypothesis holds with respect to the PFD, then the availability of excess cash-on-hand is unlikely to be the potential mechanism for any kind of social welfare impact of the PFD.

This preceding work on the PFD is important in the present context since it helps to provide a basis for believing—along with aforementioned data on the EITC and NIT experiments—that the relatively modest size of the dividend can have statistically perceptible effects at a large scale. At the same time, Hsieh (2003) provides reason for believing that the mechanisms that might lead to a positive education effect for the EITC might not hold for the PFD.

Methodology

In this paper, I work largely along the lines of Jones and Marinescu, deploying the synthetic control model popularized Abadie and Gardeazabal (2003) and extended by Abadie, Diamond & Hainmueller (2010). For mathematical details on the method, readers should refer to those papers. In this paper, I use the R package *Synth* (Abadie, Diamond & Hainmueller, 2011).

My choice of methodology here results from the challenges of causal inference in the PFD scenario. The variable under investigation is a state-level statistic (youth educational attainment), and we have only one treated unit (the state of Alaska). For this reason, a conventional difference-in-differences approach is difficult to implement—and, in any case, the required parallel trends assumption is difficult to defend: state-level policies that affect educational attainment and enrollment are continually being instituted, extended, modified, amended and occasionally rolled back throughout the country; there is no reason to accept the strong assumption that parallel trends in the pretreatment would, even in the absence of the specified treatment, continue to hold in control units in the post-treatment period. The synthetic control method is more robust to this concern, for reasons elaborated on below.

The synthetic control method is a quasi-experimental technique used to determine the effects, over time, of a policy implemented at a single point in time on an experimental unit for which there is no suitable real-life control. A “synthetic control” is constructed using a data from a weighted pool of donor states. A broad outline of the method follows below.

In the synthetic control method, the treated unit is compared to a single synthetic control unit, which is constructed using a weighted average of m candidate donor units. The goal is to determine the effect of a treatment administered at time t, using data from k pretreatment periods and q post-treatment periods, on some variable of interest $\mathbf{Y}$. For a given outcome metric, a set of p prognostic covariates $\mathbf{V}$ is selected; conventionally, this set includes lagged values of the variable of interest, which are naturally highly predictive of current values of a given time-varying measurement. An $(m \times 1)$ vector of weights $\mathbf{W}$ is selected to minimize $||\mathbf{X_1} - \mathbf{W}\mathbf{X_0}||$, where $\mathbf{X_1}$ is a $(p \times 1)$ vector in which each X_{1_n} is some linear combination of the values of covariates $\mathbf{V}$ over the pretreatment period $(t-k, t)$; $\mathbf{X_0}$ is a $(p \times m)$ matrix of covariates for the pool of candidate regions with the same makeup. A separate set of predictor weights is selected in order to minimize the mean squared prediction error between the weighted predictors and the values of $\mathbf{Y}$ over the pretreatment period.

After goodness-of-fit testing, a placebo test is conducted on states in the donor pool in order to determine the significance of the results calculated for the region under investigation. Separate synthetic controls are fit for all possible counterfactual treatment regions; for each model, the root mean squared error (RMSE) is recorded and the observed RMSE of the original model is compared with the distribution of counterfactual RMSEs. In this way, a "p-value" is recorded that suggests the likelihood of observing a difference as large as the observed treatment-synthetic gap. A null hypothesis of no effect is not tested; rather, a large difference is treated as being "suggestive" of a treatment effect (McClelland and Gault 2017).

Data

Data Description

This study uses two main data sources. The first is an extract from the Current Population Survey (CPS) for the years 1977 to 1991. The CPS is a monthly survey of the civilian non-institutionalized population conducted by the U.S. Bureau of Census on behalf of the the U.S. Bureau of Labor Statistics. It collects a wide variety of household- and individual-level variables relevant for statistical evaluation of the U.S. labor force. 1977 is the earliest year available from CPS for the purposes of this research: although a variable identifying a CPS respondent's state of residence is available going back to 1962, CPS did not disaggregate Alaska between 1968 and 1976 (inclusive); for those years, it was included as an component of multi-state groupings including, in the first case, Alaska, Washington, and Hawaii (1968-1972) and, in the second case, Washington, Oregon, Hawaii, and Alaska (1973-1976). Since Alaska cannot be meaningfully disaggregated for this period, it is not available for use in the synthetic control model deployed here.

The timeframe under investigation for this study ends in 1991. In 1992, CPS switched its method for recording educational attainment. Though recent CPS publications offer a recoded variable that incorporates both pre- and post-1992 measurements, this recode does not allow for comparability across time periods that include the 1992 measurement change.

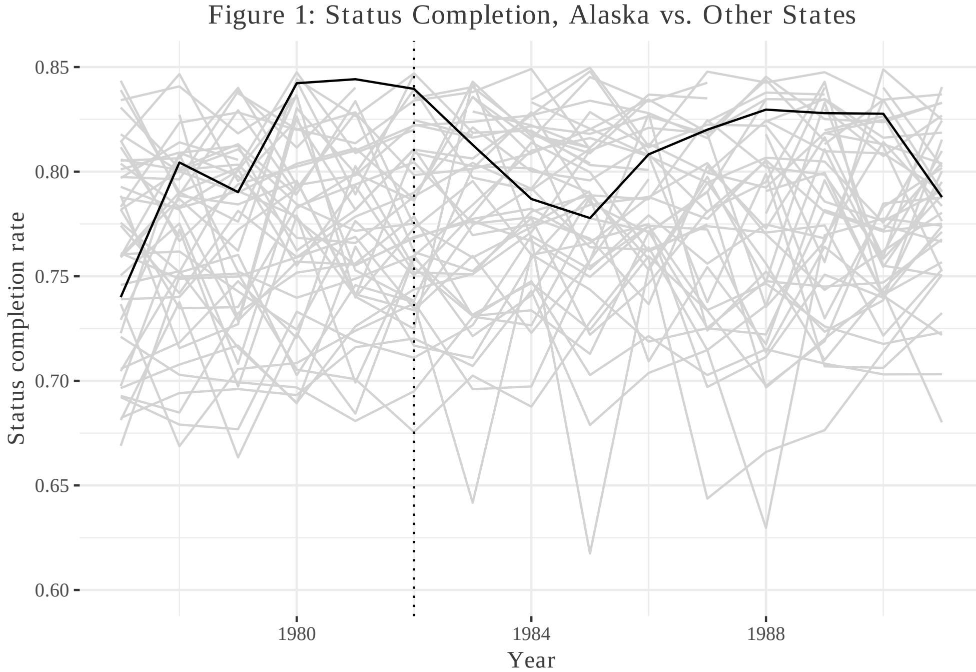
This study also uses the Correlates of State Policy dataset produced by Michigan State University. The main utility of this data in the present study is in providing predictors for the outcome variables for which I create a synthetic control. The dataset aggregates state-level indicators and metrics from a variety of sources for the years 1900 to 2016. However, few variables are available for the entire range of the study (1977-1991), so only relatively modest use is made of this dataset.

Variable of interest

The time period under investigation offers some challenges in terms of finding a suitable outcome variable due to measurement changes in the United States over the past several decades. The National Center for Education Statistics uses several similar but distinct metrics for analyzing high school completion. At present, these are the event dropout rate, the status dropout rate, the status completion rate, the adjusted cohort graduation rate (ACGR), and the averaged freshman graduation rate (AFGR). More recent iterations of the CPS have included questions about present school enrollment or attendance; these questions are not available prior to 1992, when a redesign of the CPS introduced a new question intended to enable better calculation of the various educational statistics listed above.

The status completion rate is here defined as the percentage of young adults between the ages of 18 and 24 who hold a high school credential. The primary weakness of this metric is that it makes no distinction between individuals who were born and/or raised in Alaska and those who migrated to the state. This weakness may be the cause of the somewhat surprising shape of **Figure 1**, which shows a dip in status completion among Alaskans around the time of the initiation of the PFD, which is denoted by a vertical dotted line:

Figure 1: Status Completion, Alaska vs. Other States



This plot also demonstrates the volatile nature of status completion rates across states. Note the unusual increase in status completion rates in the pretreatment period, e.g. the first five years following the completion of the Trans-Alaska Pipeline. This education spike can perhaps be explained by the departure of pipeline workers in 1977 (Sandberg, 2013). As I will explain in more detail in the next section, many of the laborers who moved to Alaska to work on the pipeline project had relatively low levels of education; they came in very large numbers and their gradual departure–as well as their aging out of the sample of 18- to 24-year-olds used to calculate status completion–may well have resulted in an increase in that statistic.

This is just one of many analytical challenges that I will describe in detail in the next section. Nevertheless, for the time period under investigation this is the best available cross-state proxy measure for high school completion, and it is the the statistic that I calculate for each state using CPS data with appropriate weights and use in my analysis below.

Challenges and Idiosyncracies

This research is complicated by two factors: the limitations of the available data, and the somewhat unusual nature of the scenario under investigation. As I detailed in the Introduction to this study, the Permanent Fund Dividend came about as a consequence of the expected completion of the Trans-Alaska Pipeline System (TAPS), which began operating in 1977—perhaps not coincidentally the first year for which disaggregated data is available for Alaska in the Current Population Survey.

The introduction of oil revenues into the Alaskan economy was an economic shock with far-reaching impacts, including, possibly, effects on educational attainment that could plausibly swamp the influence of the PFD itself in any state-level analysis. Yet the state is by no means unique: at the time of the opening of the TAPS, several states produced substantially more crude oil than Alaska; Texas alone produced more than twice as much. Around 1977, Texas, Oklahoma, and New Mexico saw comparable increases in crude oil production (in absolute terms) to that experienced in Alaska. The value of Alaska's 1977 crude oil production came out to roughly 0.7% of the state's GDP in that year.

Nevertheless, TAPS was "the most expensive privately financed construction project in world history" and therefore, given the small size of the Alaskan economy, "the largest localized demand shock in postwar U.S. history" (Carrington, 1996, p. 187). Thus, the large-scale labor force and population changes wrought by the construction of the pipeline and by the rapid growth of the petroleum industry in Alaska are of considerable concern for the present analysis.

Construction on the TAPS began in 1974, at which time the Alyeska Pipeline Service Company, which constructed and continues to operate the pipeline, began to hire staff to work on the pipeline. Each summer of the project, Alyeska and its subcontractors employed a total of roughly 50,000 individuals who fell, generally, into one of two groups. The first group were primarily skilled welders and pipefitters, and generally hailed from out-of-state (in particular from Texas and Oklahoma). The second group consisted of unskilled laborers, and were more likely to be Alaskans (Carrington, 1996). Together, these workers would have constituted more than a tenth of Alaska's 1977 population.

Though the U.S. Census identifies respondents' birth states, it is conducted only once every decade. The Current Population Survey does not collect respondents' U.S. state of birth and only began to collect respondents' one-year interstate migration data in 1982. It is therefore difficult to obtain estimates of the proportion of pipeline workers who remained in Alaska after the completion of the pipeline in 1977, the rate at which those who left the state did so, and the amount that stayed indefinitely.

This is a potential issue for the analysis I conduct below. Though welding was and is considered skilled labor, CPS data shows that, in 1977, 57% of welders had completed twelfth grade, in contrast to 67% of the U.S. population over 18 and 83% of Alaskans over 18. The median age of a U.S. welder in 1977 was 34 and 20% were under 24. By 1982, almost all of the migrant welders who were under 24 in 1977 would have aged out of the sample of 18-to-24 year olds I use to calculate total status completion. However, if some nontrivial portion of them remained in Alaska after the completion of the pipeline, there remains the concern that their presence could artificially suppress status completion rates in the pretreatment period.

Results

In this section, I compare educational outcomes in a synthetic Alaska formed using a weighted combination of donor states (hereafter the "control") to those observed in the real Alaska (the "treated unit"). In **Table 1** I compare the treated unit to its control using the averaged values of predictive covariates over the course of the pre-treatment period, while **Table 2** contains the states used to form the control and their accompanying weights.

Table 1: Pretreatment Predictor Values

	Treated	Synthetic	Sample Mean
Log of Gross State Product	23.209	23.042	24.134
Unemployment rate	9.720	6.476	6.290
Percent of population over 24 with at least 4 years of college	0.210	0.186	0.162
State GDP per capita	33888.804	14302.081	11107.228

	Treated	Synthetic	Sample Mean
Status completion rate, 1977	0.740	0.757	0.776
Status completion rate, 1981	0.844	0.841	0.782

Table 2: Nonzero State Weights

Weight	State
0.169	Connecticut
0.543	Delaware
0.057	Hawaii
0.230	Wyoming

Figure 2 shows the actual paths of the treated unit and the placebos used to form its synthetic control. **Figure 3** plots the path of Alaska against its synthetic control, with a dotted vertical lign at the launch of the PFD in 1982, while **Figure 4** plots the gaps between the synthetic and the treated unit.

Figure 2: Placebos and Alaska (actual units)

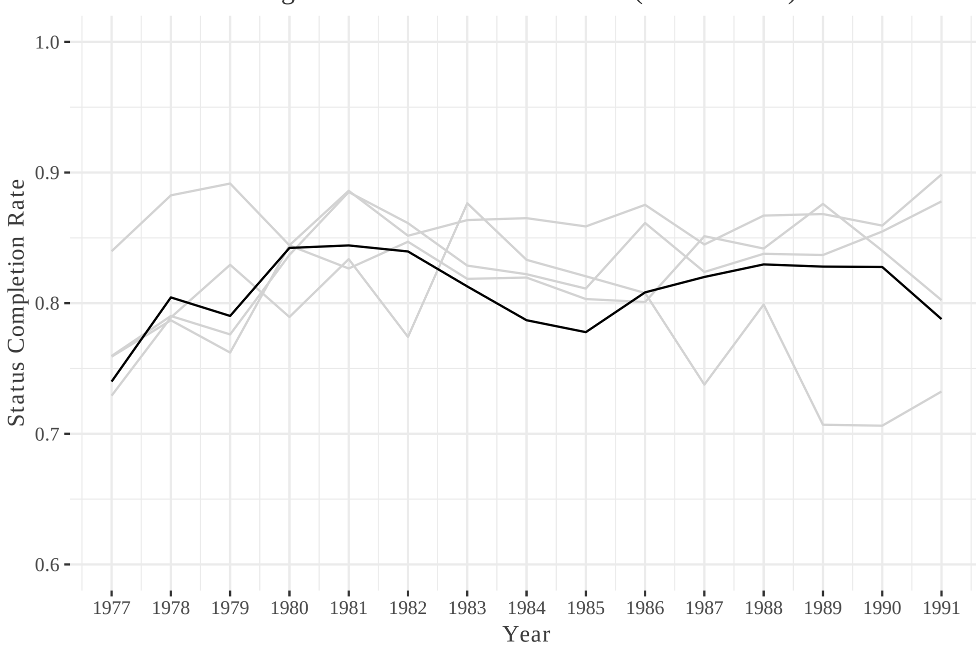


Figure 3: Actual vs synthetic Alaska

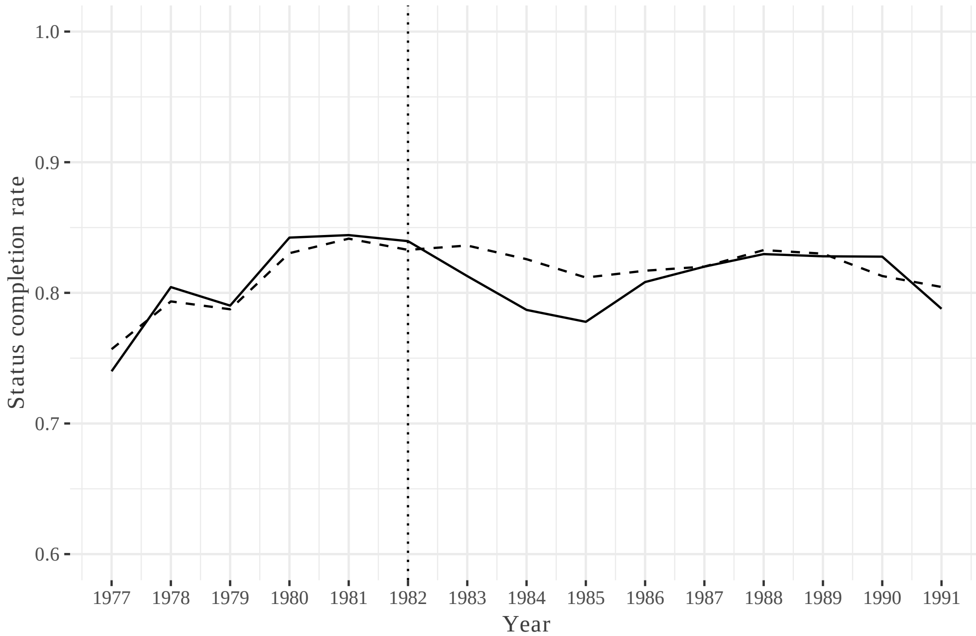
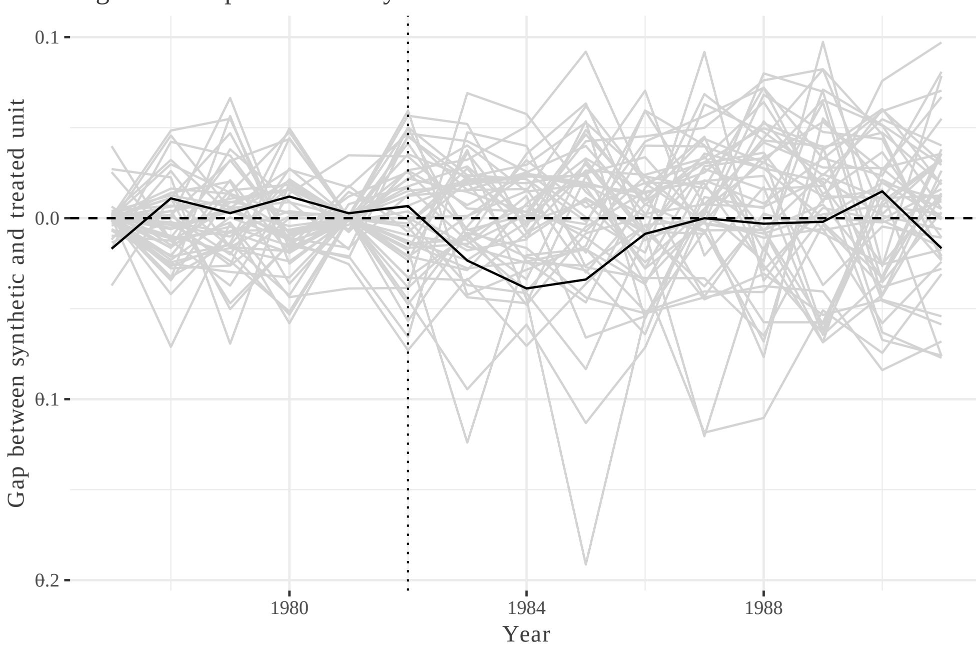


Figure 4: Gaps between Synthetic and Actual Values: Alaska and Placebos



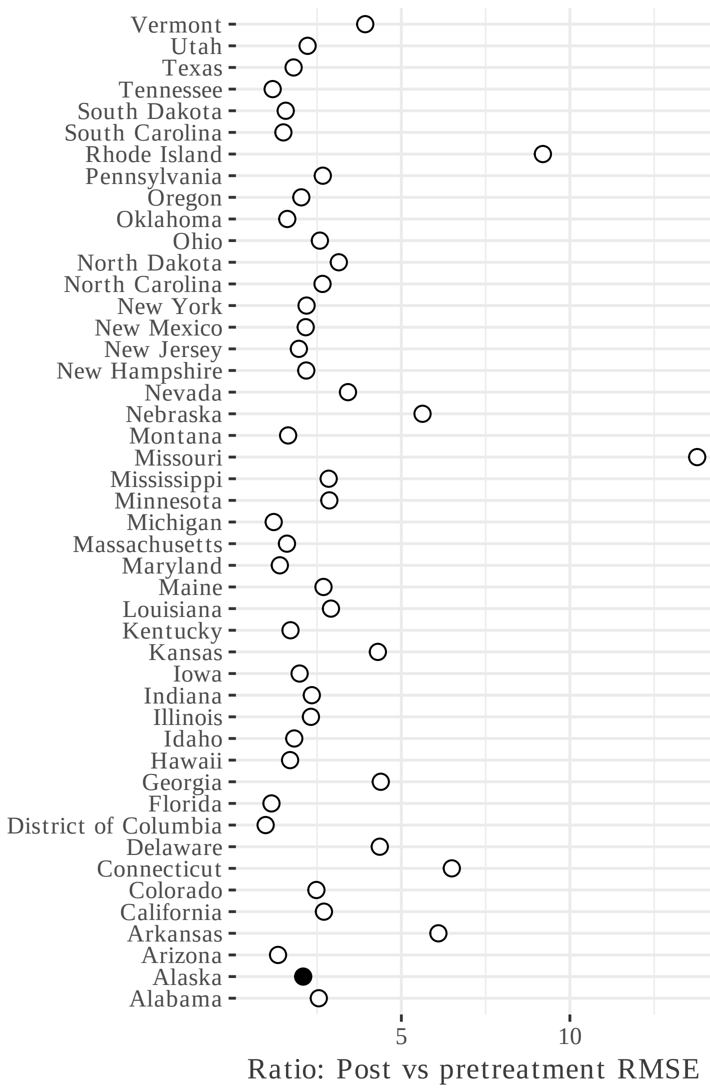
Visual inspection of **Figure 3** and **Figure 4** reveals no easily discernible difference in outcome variable values between the treated unit and its control. To quantify the degree to which this is the case, I use the placebo method: for each actually untreated state in the donor pool, I create a unique synthetic control model using the same specifications as those for Alaska, determining the deviation between the donor state and its control for the counterfactual situation in which each of the donor states received the treatment. By taking as the sampling distribution the small universe of possible synthetic-treated gaps, I develop a “p-value” that suggests the likelihood of observing a gap at least as large as the observed one (between Alaska and its control) in event of a null effect.

In order to develop a usable p-value, I calculate the root mean squared error (RMSE) for each placebo, and for the Alaska-control pair, using the difference between synthetic and actual values for each year of the post-treatment period under investigation (1982-1991).

With a placebo-based p-value of 0.95, I find what amounts to a null effect: in placebo trials, RMSEs at least as large as the observed gap between Alaska and its control are observed in 95% of cases. The path of educational attainment in Alaska between 1977 and 1991 therefore closely tracks the experience of the real Alaska and does not meaningfully depart with the launch of the Permanent Fund Dividend in 1982 or at any point thereafter.

As a further check, I report the ratio between post- and pre-treatment RMSE for all possible synthetic controls, as suggested by Abadie, Diamond & Hainmueller (2015), in order to contextualize the Alaska case. The principle at work here is that, under a non-null effect, that paths of the posttreatment synthetic and treated unit will diverge, resulting in a high ratio relative to the placebo regions. **Figure 5** displays the RMSE ratios for Alaska and all placebos.

Figure 5: RMSE Ratios



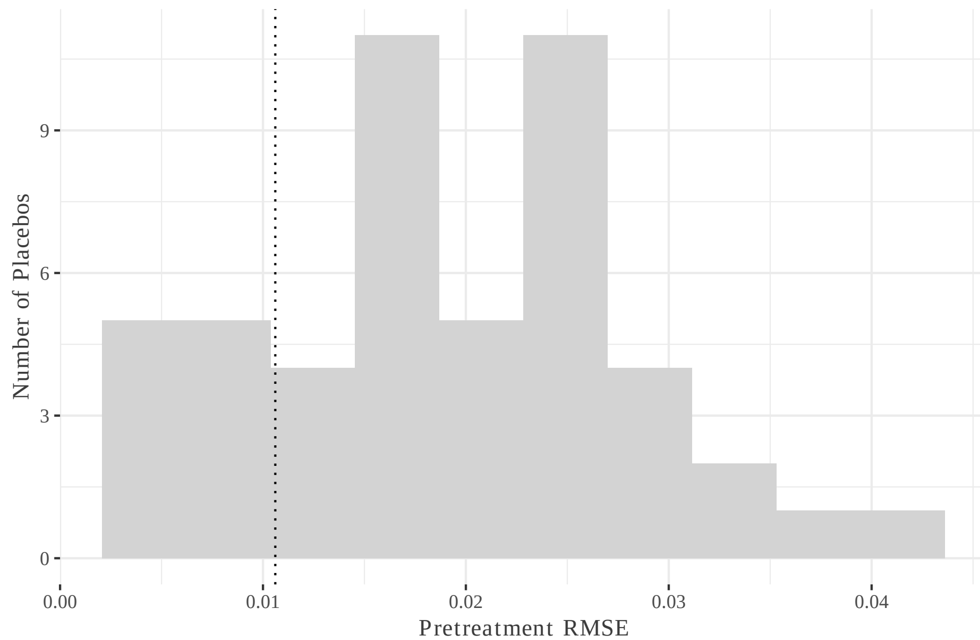
This plot demonstrates that the gap between Alaska and its synthetic control unit does not meaningfully increase in the posttreatment period as compared with a set of placebos fit to their controls using the same set of covariates. In conjunction with the placebo tests and informal p-value derived above, I find that the results of my analysis are not suggestive of a positive effect of the initiation of the Alaska Permanent Fund Dividend on high school status completion rates.

Model Fit

There is no widely agreed-upon rigorous criterion for assessing the fit of a synthetic control model. In this particular case, the challenge is complicated further by the relatively short (five-year) length of the pretreatment period (an obstacle also faced by Jones and Marinescu (2018)). In this section, I present several different measures of model fit and present a rationale for why these metrics suggest that the synthetic control model applied above is a good fit.

The first metric I explore is the pretreatment root mean squared error (RMSE), or the square root of the sum of squared differences between the synthetic control and the treated unit in the pretreatment period. In the histogram below, I plot the distribution of the RMSE for Alaska and for all placebo regions; the dotted vertical line in **Figure 6** denotes the RMSE of the synthetic Alaska.

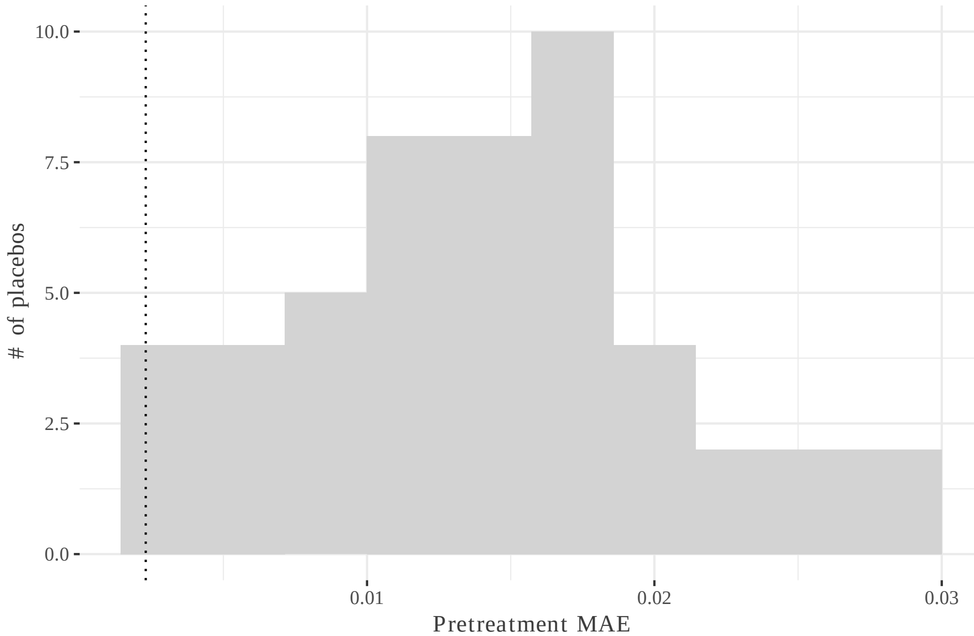
Figure 6: Distribution of pretreatment RMSE



RMSEs for Alaska and its placebos range between 0.001 and 0.04, with a mean of 0.02 and a value of 0.01 for Alaska itself. Thus, during the pretreatment period for all placebos, the set of covariates selected for the model produces synthetic control units within 0.1% and 4% of the actual status completion rates for all regions, and within 1% for Alaska.

In order to complement the RMSE measurements above, I also include the same set of statistics with respect to mean absolute error. Willmott & Matsuura (2005) argue that RMSE varies not only with the average error magnitude but also with the square root of the number of errors and the variability of the distribution of error magnitudes. I present MAE here in order to preëmpt these concerns and for purposes of comparison with RMSE.

Figure 7: Distribution of pretreatment MAE



MAEs for Alaska and its placebos range between 0.003 and 0.028, with a mean of 0.014; synthetic control units fall within 0% and 2.8% of the actual status completion rates for all regions.

In order to offer a framework for interpreting these fit statistics, I suggest considering RMSE within the context of the actual period-to-period variation of the outcome metric for the treated unit in the pretreatment period. Many practitioners of the synthetic control method have tended to offer *de facto* visual arguments for model fit; Abadie, Diamond & Hainmueller (2010), for example, offer convincing plots depicting the apparently parallel pretreatment trajectories of synthetic and treated units as an informal argument for good model fit.

RMSE does not offer useful information about model fit in the absence of a comparison with a competing model and that model's RMSE. However, the relationship between this deviation and the actual variability of the treated unit can be considered a good formal proxy for visually assessed "fit." In the present context, for example, a synthetic control RMSE of 1 percentage point would make for an impressive fit if the actual treated unit had a standard deviation 5 percentage points, but would represent an extremely poor fit if the unit were to tend to vary only within about 0.1 percentage points.

This perspective offers a simple rule of thumb for considering whether a synthetic control is a good fit for the treated unit: whether the RMSE of the synthetic unit is smaller—preferably much smaller—than the standard deviation of the treated unit in the pretreatment period. This rule of thumb is well-suited to scenarios, such as the present one, in which the available pretreatment period is short. For studies with more extensive data, Li (2017) has proposed a subsampling method based on the bootstrap that is suitable for use with a synthetic control.

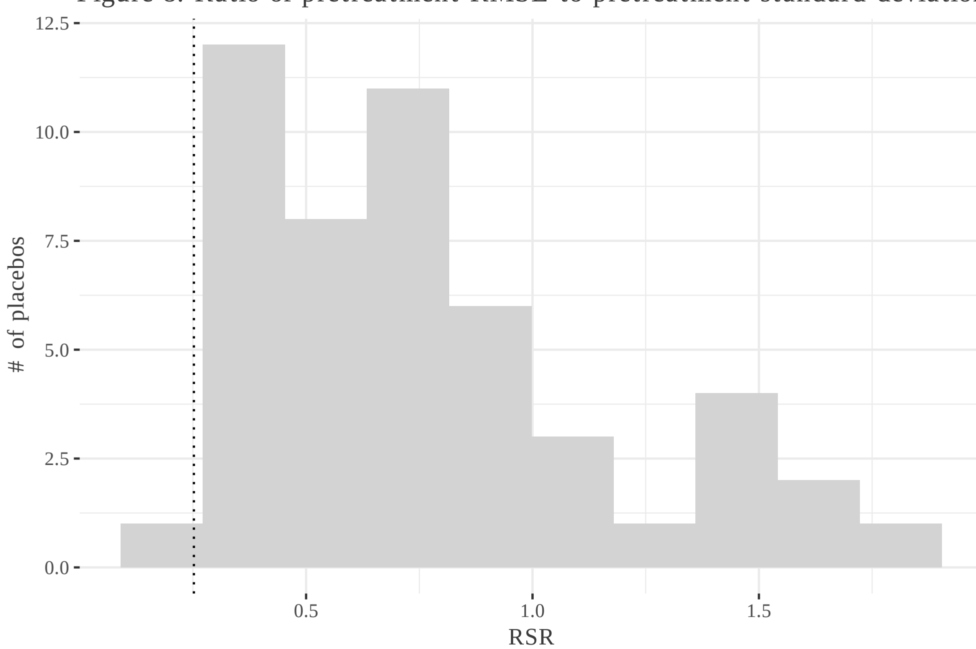
The statistic I deploy below has been previously been suggested in the statistical hydrology literature as a method for evaluating performance of watershed models, which entail the simulation of monthly discharge rates for a given waterway for the purposes of future flow forecasting and attendant practical goals such as land management (Moriasi et al 2007). In this literature the figure is referred to as the RMSE-observations standard deviation ratio, or RSR.

For clarity, I present this formally below: I calculate the statistic RSR for a given state over T time periods as below, where y_t represents the value of the variable of interest for the treated unit at time t and $\hat{y}_t$ is the value of the synthetic unit at time t.

$$RSR = \frac{\sqrt{\frac{1}{T}\sum_{j=0}^{T}(\hat{y}_t - y_t)^2}}{\sqrt{\frac{1}{T-1}\sum_{t=0}^{T}(y_t - \overline{y})^2}}$$

The distribution of this statistic for the pretreatment period of the synthetic Alaska is depicted below in Figure X, with a dotted vertical line representing the value of this statistic for Alaska.

Figure 8: Ratio of pretreatment RMSE to pretreatment standard deviation



This histogram suggests that the collection of covariates used in this synthetic control method is a good fit not just for Alaska, but for the bulk of placebos as well. For nearly all regions, RSR values of less than one indicate that the pretreatment variation between synthetic and treated units is less, and often much less, than the natural variation of the treated unit alone.

Discussion

This study grows out of a large previous body of research on the social welfare impacts of cash transfers. As I have detailed in my literature review above, this research includes several distinct sub-areas. One group of studies examines the effects of the Earned Income Tax Credit (EITC), arguably the most successful antipoverty program in the history of the United States. Researchers have found that even modest exposure to the EITC in childhood can improve educational outcomes for low-income children, and a host of other positive benefits attested in the academic literature contrasts with the relatively limited amounts afforded to low-income families by the disbursement of the EITC. This lends credence to the idea that even modest cash transfers can have substantial effects on affected populations.

The second group of studies includes the negative income tax trials conducted in the United States in the 1970s. The results of many of these studies indicated modest positive impacts of cash transfers on the educational outcomes of low-income children, teenagers, and young adults.

Lastly, there is the heterogeneous collection of various "one-off" studies on unconditional cash transfers. These include the recent and substantial body of research on cash transfers in the developing world, the Manitoba Basic Annual Income Experiment ("Mincome"), and programs such as the annual payment disbursed by the Eastern Band of Cherokee Indians in North Carolina.

This collection of disparate analyses, conducted over the course of nearly half a century, provides strong prior evidence for the hypothesis that relatively modest cash transfers—those, loosely speaking, between \$1000 and \$5000—can positively impact educational attainment among low-income youth, with an effect size on the order of about one extra year of education.

The results of the synthetic control model above fail to provide substantive support for this hypothesis. In this section, I address the possibility of a true null effect, offer explanations for the null finding in the hypothetical presence of a true non-null effect, and offer methodological suggestions for future researchers.

Considerations about the null finding

The simplest explanation for the null finding of my analysis is that the synthetic control method that I use in this study is too blunt an instrument to detect what could be a very small effect. With better covariates or a superior dataset, it might be possible to develop a tighter-fitting control unit, and therefore to discern a positive effect. There is also space here to deploy other econometric methods: in particular, it may be possible to use data from the American Community Survey to understand the possible correlation between years of exposure to the dividend and high school status completion.

Another possible explanation for the null finding of this analysis may be that, if there are educational effects resulting from the disbursement of the PFD, they are distributed heterogeneously across the population. The research discussed in the previous section has focused largely on the educational attainment of students from low income families; it is plausible, if the distribution of the PFD has any effect at all, that this effect is heterogeneous and acts only on low-income students. Yet this explanation itself requires an explanation. Holding other factors constant, individuals with higher household income are generally more likely to be high school status completers than those with low household income. Even if the disbursement of the Alaska Permanent Fund Dividend displays a heterogeneous treatment effect on educational attainment, with a trivial or null effect for middle- and upper-income students, it would still be reasonable to expect overall status completion rates to increase due to the increased completion rates of low-income students.

A third possibility is that the size of the dividend is simply not large enough to have a measurable effect on educational attainment. Yet this presents a puzzle as well. Some literature on the EITC, for example, suggests sizeable effects on high school completion associated with an exposure to the credit, between the ages of 13 and 18, of approximately $1000 in 2018 dollars (Bastian & Michelmore 2018). The families of Alaskan children who were thirteen years old at the initiation of the dividend in 1982 would have received the equivalent of more than $6000 by the time those children turned 18 (Alaska Department of Revenue 2019), but, per the synthetic control model applied here, no positive effect begins to reveals itself even as late as 1987.

A final possibility is that the prior evidence provided by research on the NIT and the EITC is itself misleading. In my literature review, I uncovered relatively few studies reporting null effects of the NIT or the EITC. This could be an indication of the phenomenon known as the file drawer effect or the file drawer problem (Rosenthal, 1979), in which null results are not considered interesting and do not find their way to publication. Those studies claiming education effects of the EITC could be simply the non-null tip of a vast iceberg, the null bulk of which is submerged. I am unconvinced by this possibility, given that studies on EITC, NIT and MINCOME demonstrate broad concordance with the respect to their reported effect sizes.

Parameter of interest

In previous sections, I have cited various challenges related to the measurement of substantive state-level education statistics for the time period in question. I feel that this is the primary obstacle to studying this topic, and to my use of the synthetic control method in particular. Status completion rate is the only available statistic for the period under review that can be considered a good proxy for high school completion, as well as an unusually blunt measure of that underlying statistic in the case of Alaska. Yet status completion writ large is not, perhaps, the most instructive statistic with respect to Alaskans' educational attainment during the post-pipeline period: at the time of the PFD's launch, Alaska had just wrapped up the largest privately financed construction project in world history. Young men and a few young women of differing levels of education were rapidly moving to and from the state, and no effort was made by the Current Population Survey to distinguish migrants from non-migrants (throughout the country) until the greatest upheavals had already ended. For this reason, status completion may not capture the underlying quantity of interest, which might be summarized as "the educational attainment of native-born Alaskans"; sadly for my purposes, states of birth were not colleced by the CPS during this time period, either.

The question of whether or not status completion is a suitable parameter of interest therefore casts something of a pall over this research. Do changes in the status completion rate reflect the impact of the PFD on educational attainment, in whole or in part? The answer is surely affirmative to some small extent, but numerous other factors affect status completion—particularly so for the time and place I analyze here. More importantly, the educational attainment of migrant laborers, the vast majority of whom moved to Alaska after the age of 18, is not really of interest, since none of the causal mechanisms proposed for the apparent educational impacts of previous cash transfer programs could have affected them. Nevertheless, there is no rigorous way to remove these individuals from the data. For this reason, the question of whether changes in status completion can measure effects on educational attainment for those Alaskans who we could plausibly expect to be affected by Permanent Fund Dividend payments remains an open one.

Conclusion

The disbursement of the Permanent Fund Dividend relies on Alaska's oil revenues, the existence of which indexes a unique state of affairs that makes comparison difficult. The launch of Alaska's sovereign wealth fund and the initiation of the dividend in 1982 occurred in a period that brought enormous upheaval to Alaska. With the start of construction on the Trans-Alaska Pipeline System in 1974, workers from all over the country swelled Alaska's population. At the same time, well-paid pipeline jobs became available to native-born Alaskans and the state's economy experienced a demand shock of rare magnitude. The Current Population Survey only began disaggregating data for Alaska in 1977, and only began to collect information about one-year interstate migration patterns in 1981. At the time of the launch of the PFD, Alaska was a unique region, and to some degree it is still today: it is geographically isolated from the contiguous United States, sparsely populated, and climactically inhospitable.

All of these factors represented challenges to my effort to study the impact of the Permanent Fund Dividend on high school status completion rates in the period following the start of payments in 1982. In this paper, I fit a synthetic control to Alaska in the pretreatment period 1977-1981 and tracked this control in parallel with the treated unit in order to identify possible divergences, which would be suggestive of a treatment effect. I found no such effect, and verified the lack of a notable divergence between treated and synthetic units by conducting placebo tests.

Despite the methodological and data difficulties encountered over the course of this study, I contend that my results are suggestive of a lack of a substantive effect of the PFD on educational attainment in Alaska, but do not suggest that there is no meaninful effect at all. As I discuss in the Model Fit section above, the synthetic control closely tracks the path of the actual Alaska during both the pretreatment and post-treatment periods. The gap between Alaska and its synthetic control is not notably higher than the gap between any placebo region and its own synthetic during the post-treatment period, suggesting at the very least that the treatment effect of the dividend does not exceed the variation in the estimator.

However, as the root mean-squared error for Alaska's synthetic control in the post-treatment period is approximately 2%, it remains possible that the PFD could have had a true effect on high school status completion rates of less than 2%. This would be a non-neglible effect size, given the persistent challenges of ensuring that students graduate high school.

Future researchers who hope to address this question or similar ones would, perhaps, be better served by investing time and effort in securing novel or unexploited data in order to conduct their analyses of the effects of the PFD. In my own explorations of this matter, I feel I have exhausted the limited ability of the Current Population Survey to answer questions about this place and this time. My own abortive experiments with the Panel Study of Income Dynamics and the American Community Survey have led me to conclude that Alaska's idiosyncracies—in particular, its status as a kind of statistical backwater in the 1970s—make the use of large-scale survey data an unpromising avenue of investigation.

Recently, as related by Forget (2011), social scientists have been able to undertake an exhaustive reinvestigation of the Mincome experiment using not the datasets originally produced by the project but, instead, granular Canadian administrative data from the same place and time. I recommend this as a path forward for future research into the welfare effects of the Permanent Fund Dividend. For rigorous estimates of the impact of the PFD, population-level statistics, as opposed to survey-based estimates, may be necessary; it may also be necessary to have access to official data in order to disentangle the population of interest from Alaska's many comers and goers during the pipeline era.

For researchers interested in promoting or investigating the Alaska Permanent Fund as a lens through which to view a possible future basic income guarantee, I recommend not viewing the PFD as a panacea, if only due to the many challenges of studying it. However, the PFD remains an untapped well of data on the possible impacts of a society-wide unconditional cash transfer program. It is the only program of its type in the developed world, and research on its welfare impacts is remarkably scarce. The same prior literature that led me to embark on this path of research continues to provide strong evidence that cash transfers are an effective means of improving outcomes, at least for low-income households. What remains to be seen is whether means testing and work requirements are cost-effective ways of disbursing such programs, whether cash transfers can also be effective supports for middle-income families, and whether society-wide transfer programs have large-scale effects beyond their aggregate impact on individual households. I hope future researchers will exploit the Alaska Permanent Fund Dividend to help answer these questions.

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