Universal Cash Transfers and Inflation[†]

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Abstract

By stimulating consumer spending, unconditional cash transfers may increase price levels. In Alaska, residents have received an unconditional cash transfer every year since 1982: the Alaska Permanent Fund Dividend. We measure the impact of the dividend using a synthetic control method, which matches Alaska with similar states prior to the introduction of the dividend. The method does not find a good control group for Alaska, likely because of unusual inflation dynamics around 1982. While there is suggestive evidence of positive inflation and price effects, much uncertainty remains regarding the causal effect of unconditional cash transfers on Alaskan inflation and prices.

Keywords: unconditional cash transfers, inflation.

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1 Introduction

During the COVID-19 pandemic, governments around the world have often used cash relief to address income shortfalls: about one third of social protection measures involved cash, and the cash reached about 14% of the world population in 2020 (Gentilini, 2021). In the United States, a number of quasi-universal cash transfers were instituted during the pandemic. These include the stimulus checks ("Economic Impact Payments"), and the Child Tax Credit (CTC). The CTC creates a guaranteed income during childhood, and is a notable shift from the work requirements of the traditional CTC and the Earned Income Tax Credit (EITC). Even before the pandemic, the idea of a universal cash transfer as a universal basic income was popularized in the US by Democratic presidential primary candidate Andrew Yang. While universal or quasi-universal cash transfers can improve health and educational outcomes (Marinescu, 2018), they may also have some unintended consequences. One issue with universal or quasi-universal cash transfers is that, by stimulating consumption, these transfers may increase prices and cause inflation. How big these inflationary effects may be crucially depends on the price elasticity of supply by goods and services producers.

Compelling evidence from randomized control trials (RCTs) in developing countries typically shows small to no price effects, even for very large transfers as a share of the local economy (e.g. Egger et al., 2019). While there are no equivalent studies for developed countries, there are studies examining the price effects of targeted, quasi-cash programs like SNAP in the US (e.g. Goldin et al., 2022). However, these SNAP payments have limited relevance to the question of universal cash transfers because of their smaller scale, and because of the flypaper effect that leads households to focus their extra spending on food. The effect of large scale universal cash transfers on prices and inflation in developed countries is therefore still an open question.

In the US, the state of Alaska introduced a universal, annual cash payment in 1982, which allows us to evaluate the impact of a large scale cash payment on inflation and prices in a developed economy. The payment is a dividend from the Alaska Permanent Fund, a fund that invests part of Alaska's oil and gas revenues in a diversified portfolio. The payment varies from year to year depending on the performance of the fund. Total dividend payments represent about 7.25% of total labor income in Alaska (Jones and Marinescu, 2022). We use data from the Current Population Survey and a newly developed data set on state-level inflation from Hazell et al. (2020) to evaluate the impact of the dividend on inflation and prices. We use a synthetic control method Abadie et al. (2010), which matches Alaska to similar states in the pre-period (prior to 1982): the impact of the dividend can be calculated as the difference between the outcome in Alaska and the outcome in the synthetic control after 1982 and up to 2015. The synthetic control method therefore captures the average, long-run effect of the annual dividend payment in Alaska. A complication that arises here is that US inflation prior to 1982 was very high by historical standards, and declined rapidly just before 1982. This means that the environment was highly non-stationary, which may make it more difficult for the synthetic control method to find good controls for Alaska, and to build reliable counterfactuals.

Our synthetic control estimates show some suggestive evidence of price increases, but the point estimates are typically not statistically significant, and have very wide confidence intervals. We would expect based on theory and prior evidence (Egger et al., 2019; Filmer et al., 2021) that a large scale cash unconditional transfer would have larger effects on the prices of non-tradable goods, because the price elasticity of supply is likely to be smaller in that sector. However, the precision of our estimates does not markedly improve when we estimate separately the impact of the dividend on tradable and non-tradable goods, and the confidence intervals for our estimates of the impact of cash transfers on the inflation or prices of tradable vs. non-tradable goods overlap. Thus, much uncertainty remains. Our priors, based on the compelling evidence from developing countries, suggest that the effects of universal cash transfers on inflation in developed countries may plausibly be small, and the additional evidence from Alaska does little to update beliefs, given the uncertainty.

2 What we know about the impact of cash transfers on prices

2.1 Evidence from Near-Cash Transfers

There is little existing evidence on the effect of direct cash transfers on inflation in developed countries. There are, however, some related studies that look at the prices effects of near-cash transfers, such as the Supplemental Nutrition Assistance Program (SNAP), formerly known as the Food Stamps program. While there are some conflicting findings, the bulk of the evidence points toward negligible prices effects of SNAP benefits. Hastings and Washington (2010) used scanner data on items purchased at a supermarket chain in Nevada, where SNAP benefits are distributed on the same day for all beneficiaries. Quantities purchased are 32% higher during the week of benefit delivery, while prices are only 3% higher during the same time period, suggesting a modest price effect. Goldin et al. (2022), however, fail to find a significant price effect of SNAP timing when using scanner data with nationwide coverage. Their findings may differ in part because their data feature variation in benefit cycles across different states, allowing SNAP effects to be estimated separately from general first-of-the-month effects.

While the previous two studies focus on short-term variation in prices in response to the re-timing of spending withing a month, three other studies look at the effects of SNAP benefit generosity on prices, more generally. Makioka (2018) uses the Nielsen Consumer Panel, which tracks individual household purchases over time, and finds that, following expansions in SNAP benefits due to the American Recovery and Reinvestment Act (ARRA) of 2009, places with higher concentrations of SNAP beneficiaries actually saw slightly lower prices. Similarly, Jaravel (2018) finds that in states with larger growth in SNAP receipt, SNAP beneficiaries experience less inflation in prices, relative to non-beneficiaries. In this case, the author argues that greater purchasing power could increase prices, but could also cause prices to decrease if it spurs greater product variation. Leung and Seo (2018) also use variation in SNAP generosity across states following the ARRA expansion and, contrary to Makioka (2018), find that a 1% increase in SNAP benefits results in an 0.8% increase in grocery prices.

While these studies may shed light on the potential impacts of cash transfers on prices, there are some limitations. SNAP transfers can only be spent on food to be prepared at home. Although theory suggests that the benefits should be cash-like for recipients whose total expenditures exceed their SNAP benefits, Hastings and Shapiro (2018) show that the likelihood of purchasing food when a budget constraint is relaxed by SNAP receipt greatly exceeds what would be expected if the benefits were in fact fungible. The authors attribute this "fly-paper effect" to mental accounting behavior. SNAP benefits, therefore, tend to be spent on a specific subset of goods, and, thus, whatever price effects may be detectable in the case of SNAP may overstate what would happen in the case of an unconditional cash transfer that can be spent on a much broader set of goods. While there is a lack of such unconditional transfers in the U.S. to draw from for evidence, we next survey studies based in developing countries where much more progress on this question has been made, including with evidence from randomized controlled trials (RCTs).

2.2 Evidence from Developing Countries

In contrast to the setting of developed countries, multiple compelling studies on the price effects of cash transfers exist for developing countries, which we summarize in Table 1. We include in our literature review both conditional cash transfers (Angelucci and De Giorgi, 2009; Beegle et al., 2017; Filmer et al., 2021) and unconditional cash transfers (Handa et al., 2018; Egger et al., 2019), along with a program studied by Cunha et al. (2019), which was technically conditional, but had very little enforcement in practice. All of the studies in this initial set are randomized controlled trials, with the exception of one of the 5 programs used to estimate price effects in Handa et al. (2018). The unit of randomization or observation in these studies is the village level, which is critical to understanding the impact of cash transfers on prices. Since price changes affect both treated and untreated individuals in a village, a within-village comparison could not detect these price effects. Instead, these studies compare prices in villages that received cash transfers to prices in villages that did not, and in one case, Egger et al. (2019) also vary the probability that neighboring villages are treated, in order to capture even broader spillovers across villages.

To better compare price effects across studies, we use the details of the study in each case to report in Table 1 a standardized size of the transfer in terms of share of village level expenditures, income, food consumption. In most cases, the size of the village-level transfer was approximated by multiplying the percent increase in consumption or expenditures for eligible households and the share of households in the village who received the transfer. According to this standardization, the smallest transfer was observed in a study by Beegle et al. (2017), with about 3% of village consumption in Malawi, while the largest transfer was studied by Handa et al. (2018) in Zambia, with 27% of village expenditure.

Across studies, cash transfers generally had no effect on prices, with the exception of one study in the Philippines (Filmer et al., 2021), which found no effect on average, or for rice or sugar, but price increases of 6% to 8% for eggs, in villages where the share of treated households neared 100%. Remarkably, a transfer on the order of 25% of village-level expenditures in Kenya had positive, but economically insignificant effect of 0.12% on the prices of non-tradable goods, and no effect on the price of tradable goods (Egger et al., 2019). This finding corresponds to an elasticity of non-tradable goods prices with respect to cash transfers of 0.005. It is therefore no surprise that experiments with smaller transfers could not detect any statistically significant effect on prices. One potential reason for negligible price effects in these cases is a high elasticity of supply. In Egger et al. (2019), with a 25% increase in village expenditure, there was also a significant increase in village GDP, with a fiscal multiplier of 2.7. Thus, supply was able to increase and meet the demand that cash transfers generated. Such a strong supply response likely played a role in the limited price effects observed.

In extrapolating from developing countries to a developed country context, a key question concerns the relative elasticity of supply in developed countries vs. developing countries. On the one hand, developed countries are better integrated into global supply chains, which predicts a higher elasticity of supply, especially for tradable goods. On the other hand, developed countries may have less slack, e.g. in labor markets, making it more difficult to mount a strong supply response in response to a large cash infusion. Thus, we turn to empirical evidence from Alaska to learn more about the impacts of unconditional cash transfers on prices in developed countries.

Table 1: **Table 1: Studies Summary (Developing Countries)**

Study	**Country**	**Identification**	**Size of Transfer**	**Price Effect**
Angelucci and De Giorgi (2009)	Mexico	RCT randomized at the village level	19% of village food consumption	No price increase for 31 out of 36 food items, including for staples like rice, beans, and chicken. Small price increase for 5 items including onions, lemons, eggs, and coffee.
Beegle, Galasso, and Goldberg (2017)	Malawi	RCT randomized at the villiage level	3% of annual village consumption, 4.5% of annual village expenditures on food.	No price increase for a price index baesd on the 5 most consumed items
Handa, Daidone, Peterman, Davis, Pereira, Palermo, and Yablonski (2018)	Lesotho, Malawi, Zambia (2 studies), and Zimbabwe	Lesotho, Malawi, and Zambia (RCT); Zimbabwe: (matching)	17%-27% of village consumption.	No price increase for a standard basket of goods
Egger, Haushofer, Miguel, Niehaus, and Walker (2019)	Kenya	RCT randomized at the village level	25% of village household expenditure.	0.13% price increase for all goods. 0.12% price increase for non-tradable goods. No price increase for tradable goods.
Cunha, De Giorgi, and Jayachandran (2019)	Mexico	RCT randomized at the village level	16% of village food expenditure, 10% of total village expenditure. 8% of total village income.	No price increase for food
Filmer, Friedman, Kandpal, and Onishi (2021)	Phillipines	RCT randomized at the village level	15% of village income	No significant price increase on average. 6-8% increase for non-tradable goods, only when a high share of population is treated.

3 Institutional background, empirical approach, and data

3.1 Background on the Alaska Permanent Fund

During the 1970s, the state of Alaska experienced a windfall in revenues, driven by the production and sale of oil from Alaska's North Slope region. After the initial surplus in revenue was nearly spent down by government officials, the citizens of Alaska took recourse to better manage the funds, creating the Alaska Permanent Fund (O'Brien and Olson, 1990). The fund invested the proceeds for the sale of oil royalties into a diversified wealth fund, managed by the Alaska Permanent Fund Corporation. As of January, 2022, the total value of the fund was $81.1 billion.[1]

Starting in 1982, a portion of the returns to the fund have been paid out to every resident of Alaska, known as the Alaska Permanent Fund Dividend (PFD). The level of dividend in each year generally follows a fixed formula that is based on the average returns during the last 5 years. Figure 1 plots the level of the dividend over time, with values ranging from $331 in 1984 to $2,072 in 2015, with average in recent years hovering around $1,000 since 1996.[2] The payment is effectively universal and paid out to each resident who has lived in the state for at least 12 months, including children.

Total dividend payments represent about 7.25% of total labor income in Alaska (Jones and Marinescu, 2022). The transfer is therefore smaller as a share of the Alaskan economy than most of the transfers studied in the literature covered above, which range from 3% to 27%. Given that only modest price effects, for a subset of goods, were found in those previous studies with generally larger transfers (see section 2), we might similarly expect to find negligible effects in the case of Alaska.

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

[2] https://pfd.alaska.gov/Division-Info/summary-of-dividend-applications-payments

3.2 Empirical Approach

In order to assess the impacts of the Alaska Permanent Fund Dividend on prices, we construct a counterfactual for Alaska using the synthetic control method as in Abadie et al. (2010). Our implementation of the method closely follows the approach in Jones and Marinescu (2022), and we direct the reader there for a more detailed discussion. We have a panel of S state observations spanning T periods and indexed by s and t. A treatment is introduced in the treatment state, $s = 1$, in period $T_0 + 1 < T$. Using a potential outcomes framework, let $y_{st}(0)$ be the outcome of interest for an untreated state and $y_{st}(1)$ be the outcome of interest when a treatment has occurred. Our goal is to estimate $\alpha_{1t} = y_{1t}(1) - y_{1t}(0)$ for $t \in \{T_0 + 1, \ldots, T\}$, i.e. the treatment effect for the treated state in the post-treament period.

The synthetic control method produces $S - 1$ weights, $\mathbf{w} = (w_2, \ldots, w_S)$, that when applied to the control states provide an estimate of how outcomes would have evolved in the treated state had there been no policy implemented. The weights are constrained to sum to one and to be non-negative: $\sum w_s = 1$ and $w_s \geq 0$ for all $s \in \{2, \ldots, S\}$. As in Abadie et al. (2010), we choose the weights via the following minimization:

$$\mathbf{w}^* = \arg\min_{\mathbf{w}} \left(\mathbf{X}_1 - \sum_{s=2}^{S} w_s \cdot \mathbf{X}_s \right)' \mathbf{V} \left(\mathbf{X}_1 - \sum_{s=2}^{S} w_s \cdot \mathbf{X}_s \right) \tag{1}$$

where $\mathbf{X}_s$ $(K \times 1)$ is a vector of observables used for matching. The matrix $\mathbf{V}$ is a positive definite and diagonal $K \times K$ matrix, with diagonal elements v_k chosen using a regression-based method. We regress each outcome variable y_{st} in the pre-treatment period on the $K \times 1$ vector of prediction variables, X_s. This produces K coefficients, β_{kt}, for each pre-treatment period t. We then use these coefficients to construct the v_k:

$$v_k = \frac{\sum_t \beta_{kt}^2}{\sum_k \sum_t \beta_{kt}^2} \tag{2}$$

Our estimator for α_{1t} is: $\hat{\alpha}_{1t} \equiv y_{1t} - \sum_{s=2}^{S} w_s^* \cdot y_{st}$ for $t \in \{T_0+1, \ldots, T\}$. In our output tables, we provide as a summary, the average difference between the treatment unit and the synthetic control during the post-treatment period:

$$\hat{\alpha}_1 \equiv \frac{1}{T - T_0} \sum_{t=T_0+1}^{T} \hat{\alpha}_{1t} \tag{3}$$

We implement this method using the "synth" command in Stata.

Next, we use a permutation test, as suggested by Abadie et al. (2010), to assess whether the difference between the treated state and its synthetic counterpart in the post-period is remarkable relative to what would be estimated using the same method in a period or state when no policy is implemented. For each state with price data other than Alaska, for the years 1979 to 2014—and also for Alaska itself in years other than 1982—we re-estimate the weights in (1) and calculate an estimate as in (3). In order to remain comparable to our main estimate for Alaska in 1982, we use the same set of potential control states, the same set of matching variables $\mathbf{X}$, and the same number of pre-treatment years for matching for each placebo. Denote $\tilde{\alpha}_{st}$ as the estimate for state s with placebo treatment year t. We report a "p-value" for our main estimate, defined as follows:

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

where $s = 1$ denotes Alaska, the 1982 represents the actual year of treatment, and N_{st} is the total number of placebo estimates.

The statistic p therefore measures the share of the placebo effects that are larger in absolute value than that of the treatment state. Suppose, for example, that the synthetic control method generally produced good counterfactuals. Then we would expect most of the placebo estimates to be close to zero, lending more credence to any difference found between Alaska and its synthetic control after 1982. On the other hand, if the variables we use generally produced a poor match for inflation or prices, then it may be common to find differences, even in placebo states and time periods. In that case, we may be less likely to attribute differences between Alaska and its synthetic control to treatment, but rather, to the noisiness of the data and the method. Though the treatment is not in fact randomized, we nonetheless implement this procedure in the spirit of randomization inference, as in Bertrand et al. (2002).

We additionally calculate confidence intervals by inverting our permutation test (see e.g. Imbens and Rubin, 2015). Take a given null hypothesis for the difference between Alaska and its counterfactual, α^*. We transform the data by subtracting this effect from the outcome variable for Alaska during post-treatment years, as follows:

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

If the data is consistent with this null effect, then using this transformed version to calculate the p statistic from (4) should result in a relatively high value. We define a 95 percent confidence interval as the set of null values for which the associated p statistic is above 0.05, that is, the set of values that cannot be rejected by the data. Intuitively, noisier data or less precise estimates will result in wider confidence intervals.

Following Abadie et al. (2010), we use an additional diagnostic to assess the quality of the match between a state and its synthetic control. Looking at (1), if the weights $w_s \geq 0$ are constrained to be non-negative, then the match depends on whether the vector $\mathbf{X}_1$ is within the convex hull of the control states' vectors $\mathbf{X}_s$. This is not guaranteed to be the case, and thus we compare the values of $\mathbf{X}$ for Alaska and its synthetic control for each specification below. Since we are ultimately concerned with modeling the outcome variable y_{st}, we further calculate how well the outcomes track using the root-mean-square error (RMSE) of the outcome variable in the pre-treatment period:

$$RMSE = \sqrt{\frac{1}{T_0} \sum_{t=1}^{T_0} \left(y_{1t} - \sum_{s=2}^{S} w_s^* \cdot y_{st} \right)^2} \tag{6}$$

We then rank the RMSE for our actual treatment state against all placebos to assess whether the fit for our treatment state is poor relative to what is generally achievable in our sample. So, for example, if Alaska has consistently higher inflation than all other states in the pre-period, its RMSE will likely have a higher rank, indicated a relatively poor fit.

3.3 Data

We use a dataset constructed by Hazell et al. (2020), which aggregates micro-level price data collected by the Bureau of Labor Statistics (BLS) for the purpose of constructing the Consumer Price Index (CPI). The resulting dataset is a panel of state-level inflation rates, at an annual frequency, separately for tradable and nontradable products, and for both products combined. The data span the period from 1978 to 2018.

The prices were collected for thousands of individual products and services, which comprise about 70% of consumer spending. They were collected in 87 geographical areas across the United States, and aggregated within a state to form the state-level panel. The data are not, however, available in all years for all states. First, for all states, data are missing for 1987 and 1988. In addition, many states do not have continuous data outside of those two years, and a few states do not have any price data at all. Appendix Figure A.1 shows when and where data are missing, and because the synthetic control method requires a balanced panel, we only use states that, like Alaska, have continuous data from 1978 through 1986, and from 1989 onward. In total, there are 23 states that fit these criteria. For more details on this data set and the calculation of state-level inflation rates, we refer the reader to Hazell et al. (2020).

We conduct analysis both on state-level inflation rates directly, and also use annual inflation rates to construct state-level CPIs. We normalize prices to a level of 100 in 1981, the last year before the introduction of the Alaska PFD, and then chain together inflation rates to recover CPI levels back to 1978 and forward to 1986, where the gap in the data prevents moving any further. For this reason, our specifications for CPI only use 5 post-treatment periods. In that case, when we conduct placebos, we re-normalize price levels to be 100 in the last pre-period year before each placebo treatment year.

We merge the data on inflation and price levels at the state-by-year level with data from the Current Population Surveys (CPS), aggregated in the same fashion. We restrict our analysis to data for those who are 16 years old or above and collapse the data using survey weights, to create annual averages for the 50 states and the District of Columbia. These data, for the years 1978 to 2015, were downloaded from the Integrated Public Use Microdata Series (IPUMS) CPS website (King et al., 2010).

From the CPS, we measure the share of population in three educational categories: less than a high school degree, high school degree, and at least some college. We additionally measure the share female and the share of the population in four age groups: age 16 to age 19, age 20 to age 24, age 25 to age 64, and age 65 or older. Finally, we take into account the industrial composition of the workforce using five broad categories of industry codes: (1) agriculture, forestry, fisheries, mining, and construction; (2) manufacturing; (3) transportation, communications, utilities, wholesale, and retail trade; (4) finance, insurance, real estate, business, repair, and personal services; and (5) entertainment and recreation, professional and related services, public administration, and active duty military. We use these time-varying, state-level variables as predictors in our synthetic control estimation.

Examining the case of Alaska is informative about what the price and inflation effects of unconditional cash transfers in developed countries. While Alaska is no doubt more similar to the US and other developed countries than to developing countries, it is still fairly unique within the US, and this must be kept in mind when interpreting the results. In particular, Alaska is in the top three states with the lowest shares of employment in industries 1 (agriculture, forestry, fisheries, mining, and construction) and 3 (transportation, communications, utilities, wholesale, and retail trade). It has the highest oil to GDP ratio of all states, the second-highest unemployment rate, and Alaska also has the lowest share of high school educated workers (see appendix Table A.1).

4 Synthetic Control Estimates

We start with presenting the results from an estimation where we match Alaska to synthetic controls based on the *average* inflation or prices in the pre-period, as well as the average of other covariates listed above over the pre-period. Because it is difficult to match the time-series of Alaskan inflation in the pre-period, we repeat the analysis by matching *only* on inflation or prices in each of the pre-periods. This second approach allows for a slightly better match of the pre-trends in the outcomes of interest, at the cost of ignoring potentially relevant drivers of long-run inflation that covariates can capture.[3]

4.1 Matching on average pre-period inflation and additional covariates

We begin with the estimation of the effects of the Alaska PFD on annual state-level inflation rates. As shown in Table 3, when using the synthetic control method with overall inflation as the key outcome, we arrive at a "synthetic Alaska" that is a weighted combination of Minnesota (56%), Maryland (35%), and Colorado (9%). Column (2) of Table 2 shows the average value of key observables for this "synthetic Alaska." The average rate of inflation in the pre-period was 5% in Alaska, and was 5.1% for its synthetic counterpart. The remaining observables tend to also be well-matched, with the exception of the share of workers in

[3]Drawing from the literature on Phillips Curve estimation, we have also considered unemployment in the pre-period as an additional variable that might yield better matches, along with a measure of oil revenues to GDP, given Alaska's unique reliance on oil during this period. In neither case did our results materially change. Results are available from the authors upon request.

Industry Groups 2 (manufacturing) and 5 (entertainment and recreation, professional and related services, public administration, and active duty military).

Figure 2a plots actual annual inflation for Alaska (solid line), and the same for its synthetic control (dotted line). While we are able to match the average inflation in the pre-period, it is also the case that trends in inflation prior to 1982 are quite different between Alaska and the synthetic control. Synthetic Alaska starts at a lower rate of inflation in 1978, and has a much steeper rise in 1980. This relatively poor fit leaves us with considerable uncertainty regarding our ability to estimate the causal effect of the Alaska Permanent Dividend on inflation.

As we follow the plot past 1982 (Figure 2a), when the Alaska PFD was introduced, we see higher inflation early on for Alaska. Thereafter, we see a very similar pattern for both Alaska and synthetic Alaska [4]. Table 4 shows, in Column (1), that the average difference between the two in the post period is 0.2 percentage points, with a pseudo p-value of 0.4, meaning that the difference is not noticeably larger, in absolute terms, than what is normally found when placebos are estimated. In other terms, we estimate a positive but insignificant effect of the Alaska Permanent Fund Dividend on inflation. Figure 2b demonstrates this visually: the solid black line shows the difference between Alaska and the synthetic control over time, which generally lies within the interior of the gray lines that plot the same difference for each placebo.

The insignificance of the result is in part due to the considerable variation in placebo estimates, as captured by our confidence intervals, which range from negative 0.5 to a positive 1 percentage point difference. This suggests that the data do not permit a very precise estimate. Moreover, at the bottom of Table 4, we see that pre-period RMSE in column 1 is larger than that of 97% of the placebo estimates, which more formally confirms the concerns discussed above when visually assessing the poor pre-period fit in Figure 2a. It was indeed relatively difficult to find a good pre-period match for Alaska.

[4] The gap in data for 1986 and 1987 is reflected in the figure.

In columns (3) and (4) of Table 2, we present the average values for the *pre-period* covariates of the synthetic control when separately estimating inflation for goods and services that are nontradable and tradable, respectively. Note that the method chooses a different set of states for control for each outcome, as summarized in Table 3. As compared to the case of overall inflation, in Table 2, Column (2), the difference in average, pre-period inflation between Alaska and the synthetic control is slightly larger when we separately look at nontradable inflation (column 3). Meanwhile, the remaining observable characteristics have a fit similar to that of the overall inflation model in column (2). Figures 3a and 4a, which plot inflation in each period, reveal divergence in the quality of pre-period match between these two outcomes. While non-tradable inflation trends in the opposite direction of the synthetic control in the pre-period, inflation for tradable goods matches its synthetic counterpart quite well. This pattern continues in the post-treatment period.

After the dividend was introduced, for non-tradable and tradable inflation, respectively in columns (2) and (3) of Table 4, the average difference between Alaska and the synthetic control are both positive and noisily estimated. In principle, one might expect higher inflation in the non-tradable sector, since the prices of those goods should respond more to Alaskan demand. Tradable goods, in contrast, are traded on national or international markets, and Alaska is small relative to the US and to the world economy. While our point estimates suggest the opposite, we remind the reader that our pre-period fit for non-tradable inflation is particularly poor, with an RMSE greater than in 97% of the placebo cases. We are ultimately able to draw few strong conclusions from this initial analysis, given the challenges in finding an adequate synthetic control. Below, we will consider refinements that help to achieve a better pre-period fit.

We next move on to estimating the impact of the Alska Permanent Fund Dividend on the price *level*, the CPI, instead of inflation. Columns (5) through (7) of Table 2 show the balance in pre-period covariates for models that estimate overall CPI, and tradable and non-tradable CPI for Alaska. In all three cases, the average of the key outcome in the period among the synthetic control matches that in actual Alaska. As in the case with annual inflation, the other covariates tend to be well-balanced, with the exception of the share of workers in industrial groups 2 and 5. The combination of states and weights chosen in each case are provided in Table 5. While the states chosen change from outcome to outcome, Minnesota remains a part of the synthetic control in most cases. Figures 5a, 6a, and 7a suggest, at least visually, that we are able to achieve a more reasonable fit in the pre-period. This is more formally demonstrated in Table 6, in the final row, where the synthetic control for CPI in Alaska provides a slightly better fit than in the case of annual inflation. However, in two out of three cases, the pre-period RMSE is still relatively large relative to our placebos (above the 75th percentile), implying that it is still difficult to find a good pre-period match for Alaska, even when we focus on CPI instead of inflation.

In all three Figures, 5a through 7a, we observe a higher CPI in Alaska, relative to its synthetic counterpart, in the period following 1982. Recall that in this case, as we calculate CPI by chaining successive years together, we can only follow CPI until 1986, after which a gap in data on inflation precludes going any further. In Table 6, we summarize this positive effect for all three outcomes, and in the case of all prices and prices for tradable goods, we find the effect to have a "p-value" below 0.05. In other words, the average difference in the post-period is large relative to what we observe among many placebo cases. One difference that may contribute to the more significant effect in this case is that in the short-run, Alaska experiences higher CPI and inflation rates than many other states, while our estimates for inflation average outcomes of a much longer period, over which conditions in Alaska and its synthetic control begin to largely converge. Qualitatively, the results for CPI are similar to those for annual inflation, including the fact that the point estimate for tradable goods is larger than that for non-tradable ones. However, there is substantial overlap in the confidence intervals of each, and we continue to treat the estimates for CPI with a similar level of caution, since the pre-period fit is still on the relatively poor end, when compared to the other placebos.

4.2 Matching on price and inflation in all pre-periods

Because there are large and rapid changes in inflation and the CPI leading up to 1982, we also present results that allow for a closer match on pre-period trends in the outcome variables. Specifically, for these additional results, we match Alaska based on the values of the outcome variables in each of the years from 1978-1981 (i.e. an "all pre-periods" match), and ignoring all other covariates. These estimates represent a trade-off: they better match the pre-trend in the outcome of interest, while neglecting other potentially influential differences between Alaska and its control states.

In the case of annual inflation, Figures A.2a, A.3a, and A.4a suggest that we are able to better match the trends in annual inflation prior to 1982 with this alternative approach. Table A.4 summarizes this in more detail, in columns (1) through (6). One way to quantify this improvement in fit is to note that the pre-period RMSE for these models in Table 7 are smaller by a factor of at least three, when compared to those in the initial models in Table 4. On the other hand, the pre-period RMSE for all placebo models have decreased as well, which means that in bottom row of Table 7, the relative RMSE rank has only improved marginally, if at all across the three models. We therefore continue to face challenges in finding a relatively good match for Alaska in the pre-period.[5] Turning to the estimated impact of the Alaska PFD on inflation, we find in Table 7 an increase in the estimated effect on overall inflation, and in the case of non-tradable goods, but, for all three outcomes, we continue to find no indication of statistical significance of the effects.

When we apply this alternative method to estimating the effect on CPI rather than inflation, we find again in Table 8 that we are able to achieve a reduction in the pre-period RMSE as compared to the initial model in Table 6, especially in the case of non-tradable and tradable prices. There is also some improvement in the relative RMSE rank across the three models.[6] For CPI non-tradable and CPI-tradable, the point estimates are about half what

[5]See Appendix Table A.2 for the weights chosen for the synthetic control in this case.

[6]See Appendix Table A.3 for the weights, and Appendix Figures A.5, A.6 and A.7 for the evolution of the CPI in Alaska vs. the synthetic control.

they were in our main results, and they are not statistically significant, though confidence intervals remain very large. Meanwhile, the estimate for overall CPI has increased, and remains significant in magnitude. However, the relative fit, as measured by RMSE percentile, remains weak for the model.

5 Conclusion

What is the impact of large-scale cash transfers on prices and inflation? Our systematic review of village-level randomized controlled trials in developing countries reveals that there are small to no price effects, even for transfers as large as 25% of the local economy. We then bring in evidence from the US, where the Alaska Permanent Fund has provided a yearly cash dividend to essentially every Alaskan since 1982. Using a synthetic control method, we find some suggestive evidence that this transfer may have increased prices and inflation in Alaska, but our point estimates are typically not statistically significant and extremely imprecise. The statistical limitations of our estimates may be related to the fact that inflation was high and decreasing rapidly around 1982, the time when the Alaskan dividend was first introduced: this may make it more difficult for the synthetic control method to give precise and reliable estimates of price and inflation effects.

In most of our specifications, we find that universal cash transfers in Alaska do not have a statistically significant effect on prices or inflation. Our point estimates tend to be positive, but much uncertainty remains about the true size of the effects. Given the relatively modest size of the transfer and prior estimates from RCTs in developing countries, we expected to find small price effects. Based on our findings and the uncertainty that surrounds them—our confidence intervals are rather wide—there is not much reason to update our prior of small inflationary effects. However, much uncertainty remains, and more evidence is needed to determine the causal effect of large scale cash transfers on prices and inflation in developed countries. More precise estimates will no doubt be of value in learning more about the developed country context.

Additional evidence would be especially useful given the high level of inflation in the US that began to emerge near the end of 2021. The COVID-19 pandemic has led to an increase in cash transfers, including the quasi-universal economic impact payments (stimulus checks). If we were to rely on the relatively precise evidence from developing countries, we might conclude that the cash transfers related to COVID-19 similarly had small impacts on prices and inflation. The imprecision of our estimates for Alaska constrain our ability to update those predictions much for a more developed economy. Moreover, the pandemic period is sufficiently different from prior situations that we should be cautious about drawing firm conclusions. For example, there were unique pandemic-related supply shocks that reduced the elasticity of supply, which may have increased inflation in the face of increased demand. On the other hand, unlike the Alaska Permanent Fund, the COVID-19 transfers were temporary, which may suggest lower inflationary effects.

Ultimately, the price effects are only part of what is needed to fully evaluate such cash transfers. Other questions include the impact on labor supply (Jones and Marinescu, 2022), and also the net effect of any price increase, considering the fact that wages may also increase. Therefore, future work should include an examination of the wage impacts, along with price impacts, of cash transfers.

Table 2: Pre-Treatment Covariate Balance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Synthetic Control Outcome					
	Alaska	Inflation	Inflation Nontradable	Inflation Tradable	CPI	CPI Nontradable	CPI Tradable
Inflation	5.00	5.07	-	-	-	-	-
Inflation - Nontradable	5.51	-	6.09	-	-	-	-
Inflation - Tradable	4.42	-	-	5.06	-	-	-
CPI	92.52	-	-	-	92.27	-	-
CPI - Nontradable	93.20	-	-	-	-	93.08	-
CPI - Tradable	92.49	-	-	-	-	-	92.46
Age 16 - 19	0.108	0.104	0.105	0.104	0.102	0.095	0.101
Age 20 - 24	0.151	0.129	0.128	0.128	0.127	0.118	0.130
Age 25 - 65	0.693	0.642	0.645	0.645	0.630	0.647	0.641
Share Women	0.504	0.518	0.520	0.519	0.515	0.525	0.514
Industry Group 1	0.097	0.076	0.067	0.070	0.078	0.067	0.089
Industry Group 2	0.036	0.111	0.106	0.106	0.137	0.092	0.116
Industry Group 3	0.189	0.182	0.177	0.179	0.180	0.187	0.187
Industry Group 4	0.078	0.087	0.085	0.086	0.089	0.107	0.090
Industry Group 5	0.239	0.196	0.212	0.207	0.171	0.172	0.169
Education $\leq$ 11 years	0.226	0.270	0.282	0.276	0.266	0.293	0.249
Education = 12 years	0.397	0.397	0.389	0.387	0.413	0.357	0.400

Notes: Table reports average value of variables during the pre-treatment period for Alaska and the synthetic control constructed using the method in Section 3.2. Columns (2) - (7) differ in the outcome matched on in equation (2). The omitted category for age groups is 65 and older. The omitted category for industry groups are not working. The omitted group for education is more than 12 years. The pre-treatment period covers 1978-1981. See Tables 3 and 5 for the combination of states and weights that comprise each synthetic control.

Table 3: State Weights for Synthetic Alaska when the Outcome is Inflation

State	Weight
Panel A: Inflation	
Minnesota	0.559
Maryland	0.347
Colorado	0.094
Panel B: Inflation - Non-Tradeable	
Maryland	0.570
Minnesota	0.430
Panel C: Inflation - Tradeable	
Maryland	0.506
Minnesota	0.412
Colorado	0.081

Notes: Table reports the combination of states and weights chosen using the method in Section 3.2 to construct a synthetic control for Alaska. Panels A through C correspond to columns (1) through (3) in Table 4.

Table 4: Synthetic Control Estimates, Average Difference - Inflation, 1982-2015

	(1)	(2)	(3)
	Inflation	Inflation Non-Tradable	Inflation Tradable
$\hat{\alpha}_0$	0.203	0.031	0.346
p-value	0.435	0.927	0.270
95% CI	[-0.486,0.976]	[-1.090,1.048]	[-0.452,1.389]
Number of placebos	782	782	782
Pre-Period RMSE	2.777	3.321	0.751
RMSE Percentile	0.969	0.973	0.540

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes, using the synthetic control method outlined in Section 3.2. The treatment effect is averaged over the years 1982 to 2015. The p-value and confidence intervals are constructed using the permutation test also described in Section 3.2. Root mean squared error (RMSE) is calculated using up to 5 years of pre-treatment data, and percentile is based on a comparison among all placebo estimates. See Table 3 for the combination of states and weights that comprise each synthetic control.

Table 5: State Weights for Synthetic Alaska when the Outcome is the CPI

State	Weight
Panel A: CPI	
Minnesota	0.813
Connecticut	0.180
District of Columbia	0.006
Panel B: CPI - Non-Tradeable	
Hawaii	0.618
Connecticut	0.243
Colorado	0.094
Minnesota	0.045
Panel C: CPI - Tradeable	
Minnesota	0.595
Colorado	0.258
Washington	0.133
Oregon	0.014

Notes: Table reports the combination of states and weights chosen using the method in Section 3.2 to construct a synthetic control for Alaska. Panels A through C correspond to columns (1) through (3) in Table 6.

Table 6: Synthetic Control Estimates, Average Difference - CPI, 1982-1986

	(1)	(2)	(3)
	CPI	CPI - Non-Tradable	CPI - Tradable
$\hat{\alpha}_0$	4.548	3.321	5.681
p-value	0.048	0.139	0.046
95% CI	[0.394,8.854]	[-2.051,8.796]	[0.254,11.108]
Number of placebos	690	690	690
Pre-Period RMSE	0.628	0.650	0.316
RMSE Percentile	0.871	0.746	0.488

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes, using the synthetic control method outlined in Section 3.2. The treatment effect is averaged over the years 1982 to 1986. The p-value and confidence intervals are constructed using the permutation test also described in Section 3.2. Root mean squared error (RMSE) is calculated using up to 5 years of pre-treatment data, and percentile is based on a comparison among all placebo estimates. See Table 5 for the combination of states and weights that comprise each synthetic control.

Table 7: Synthetic Control Estimates, Average Difference - Inflation, 1982-2015 - All Pre-Periods Matched

	(1)	(2)	(3)
	Inflation	Inflation Non-Tradable	Inflation Tradable
$\hat{\alpha}_0$	0.407	0.115	0.306
p-value	0.153	0.754	0.326
95% CI	[-0.219,1.124]	[-0.921,0.964]	[-0.462,1.340]
Number of placebos	782	782	782
Pre-Period RMSE	0.708	0.958	0.201
RMSE Percentile	0.908	0.893	0.630

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes, using the synthetic control method outlined in Section 3.2. The treatment effect is averaged over the years 1982 to 2015. The p-value and confidence intervals are constructed using the permutation test also described in Section 3.2. Root mean squared error (RMSE) is calculated using up to 5 years of pre-treatment data, and percentile is based on a comparison among all placebo estimates. See Appendix Table A.2 for the combination of states and weights that comprise each synthetic control.

Table 8: Synthetic Control Estimates, Average Difference - Inflation, 1982-1986- All Pre-Periods Matched

	(1)	(2)	(3)
	CPI	CPI - Non-Tradable	CPI - Tradable
$\hat{\alpha}_0$	6.271	1.001	3.028
p-value	0.025	0.632	0.155
95% CI	[1.840,10.549]	[-4.615,6.828]	[-2.549,8.791]
Number of placebos	690	690	690
Pre-Period RMSE	0.535	0.206	0.091
RMSE Percentile	0.932	0.751	0.236

Notes: Table presents estimates of effect of Alaska Permanent Fund Dividend on several outcomes, using the synthetic control method outlined in Section 3.2. The treatment effect is averaged over the years 1982 to 1986. The p-value and confidence intervals are constructed using the permutation test also described in Section 3.2. Root mean squared error (RMSE) is calculated using up to 5 years of pre-treatment data, and percentile is based on a comparison among all placebo estimates. See Appendix Table A.3 for the combination of states and weights that comprise each synthetic control.

Figure 1: Alaska Permanent Fund Dividend: nominal and real amounts

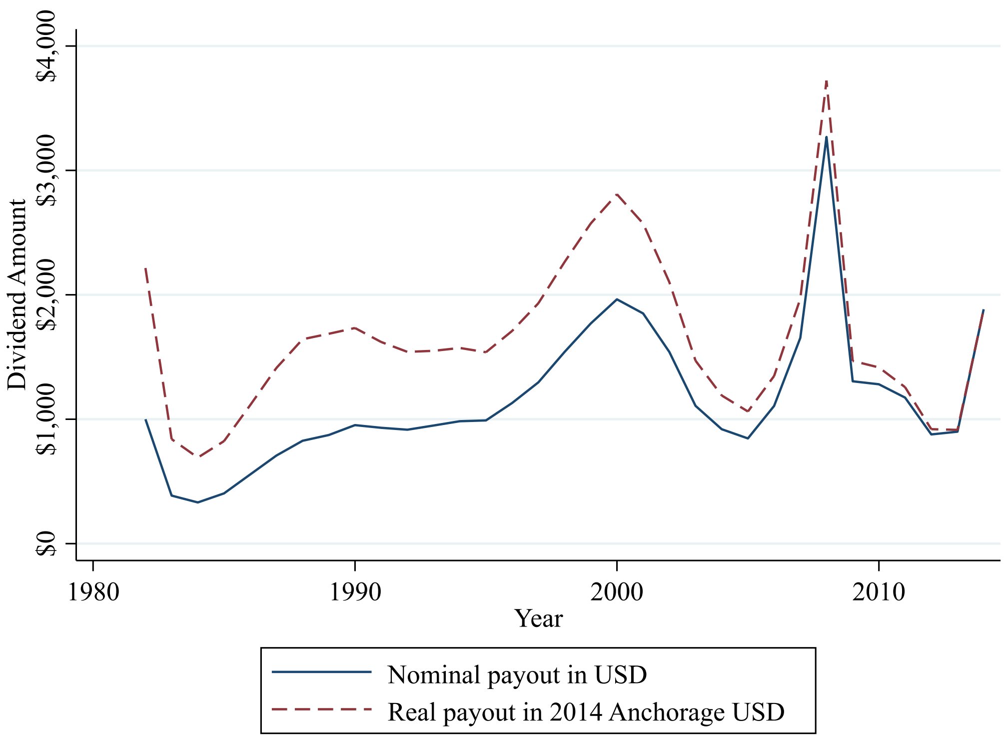
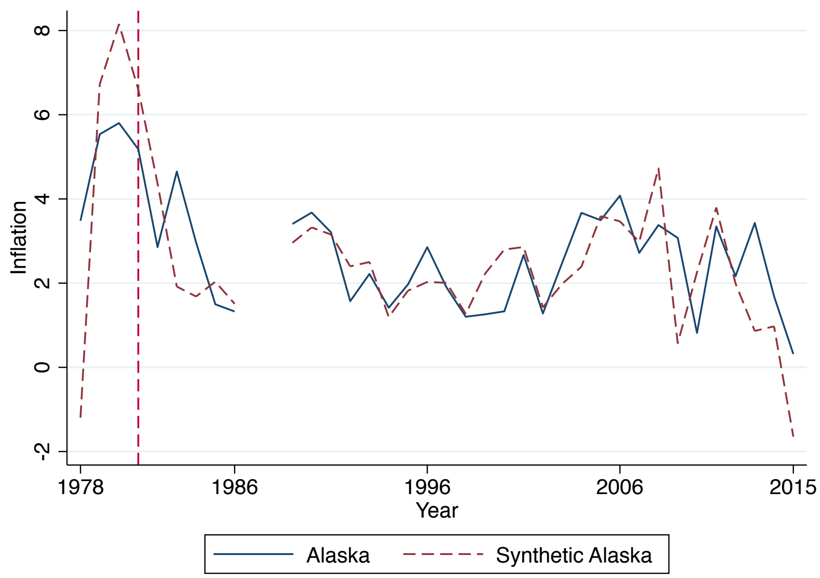
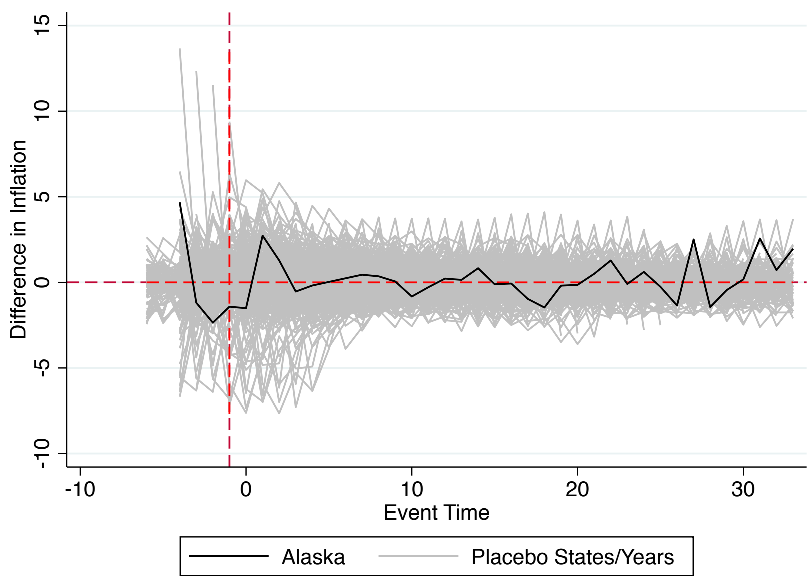


Figure 2: Inflation, 1978-2015



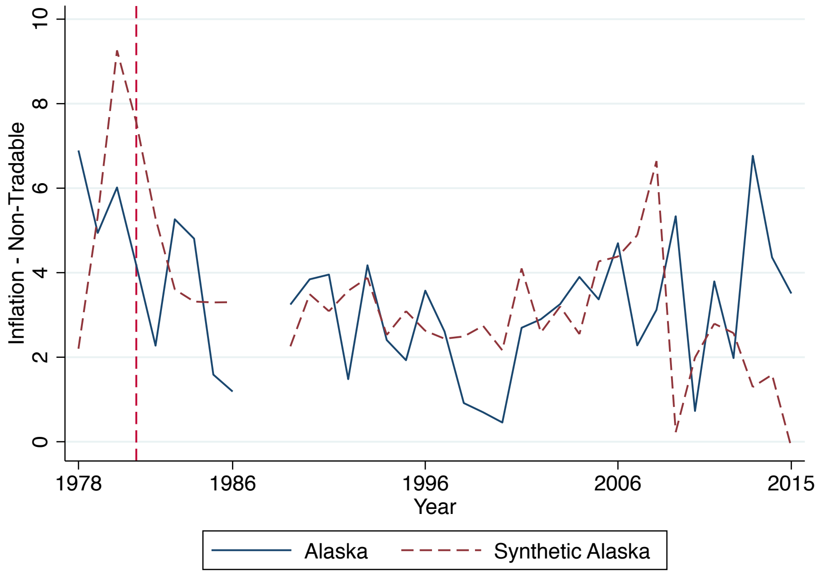
(a) Inflation: Alaska vs. Synthetic Alaska



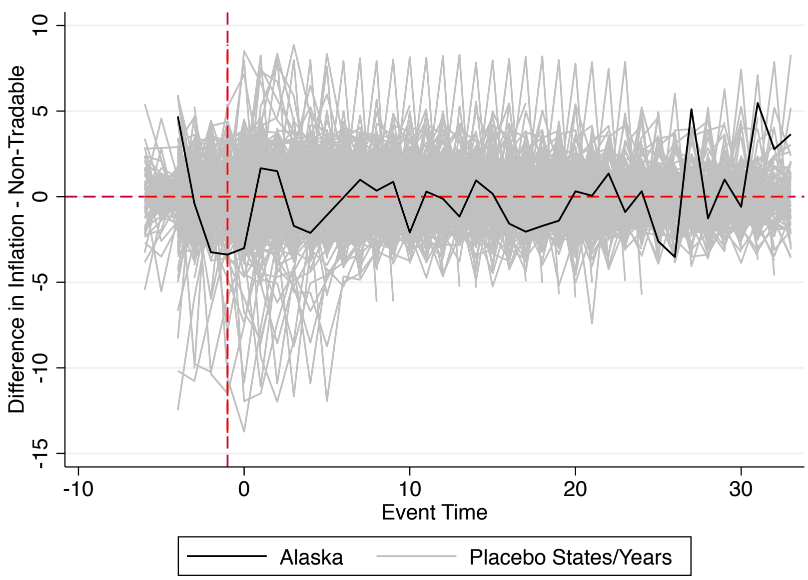
(b) Synthetic Difference in Inflation, Alaska vs. Placebo States

Notes: Panel (a) plots the synthetic control estimates of the inflation rate for Alaska from 1978 to 2015. The solid line plots the actual inflation rate in Alaska, while the dotted line plots the synthetic control estimate. The vertical dashed line indicates 1981, the year before the onset of the Alaska Permanent Fund Dividend. Panel (b) plots the results of a permutation test of the significance of the difference between Alaska and synthetic Alaska. The solid dark line plots the difference for Alaska using the true introduction of the treatment in 1982. The light grey lines plot the difference using other states and or other treatment years. See Table 3 for the combination of states and weights that comprise each synthetic control.

Figure 3: Inflation (Non-Tradables), 1978-2015



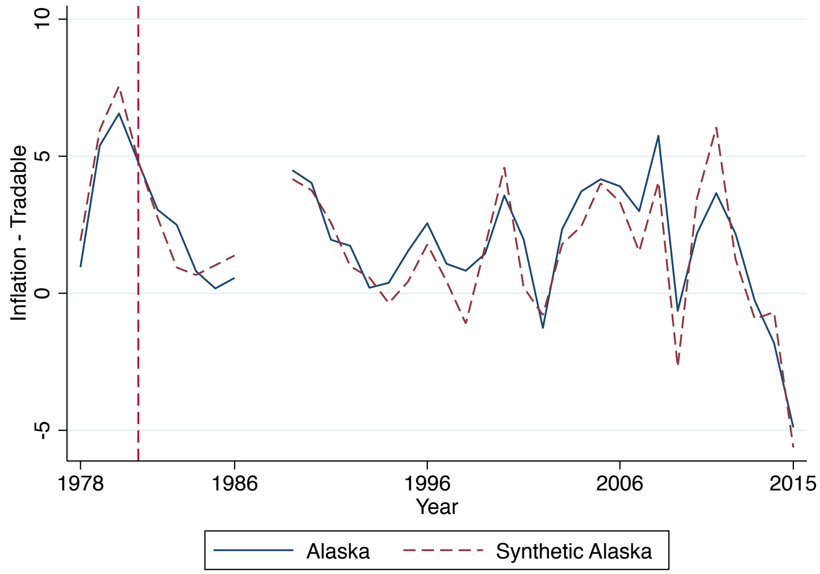
(a) Inflation (Non-Tradables): Alaska vs. Synthetic Alaska



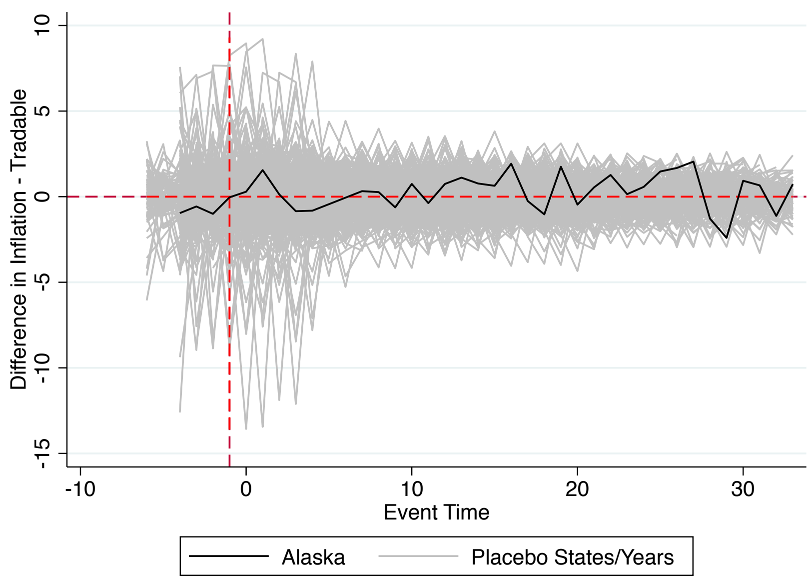
(b) Synthetic Difference in Inflation (Non-Tradables), Alaska vs. Placebo States

Notes: Panel (a) plots the synthetic control estimates of the inflation for non-tradables for Alaska from 1978 to 2015. The solid line plots the actual inflation rate of non-tradables in Alaska, while the dotted line plots the synthetic control estimate. The vertical dashed line indicates 1981, the year before the onset of the Alaska Permanent Fund Dividend. Panel (b) plots the results of a permutation test of the significance of the difference between Alaska and synthetic Alaska. The solid dark line plots the difference for Alaska using the true introduction of the treatment in 1982. The light grey lines plot the difference using other states and or other treatment years. See Table 3 for the combination of states and weights that comprise each synthetic control.

Figure 4: Inflation (Tradables), 1978-2015



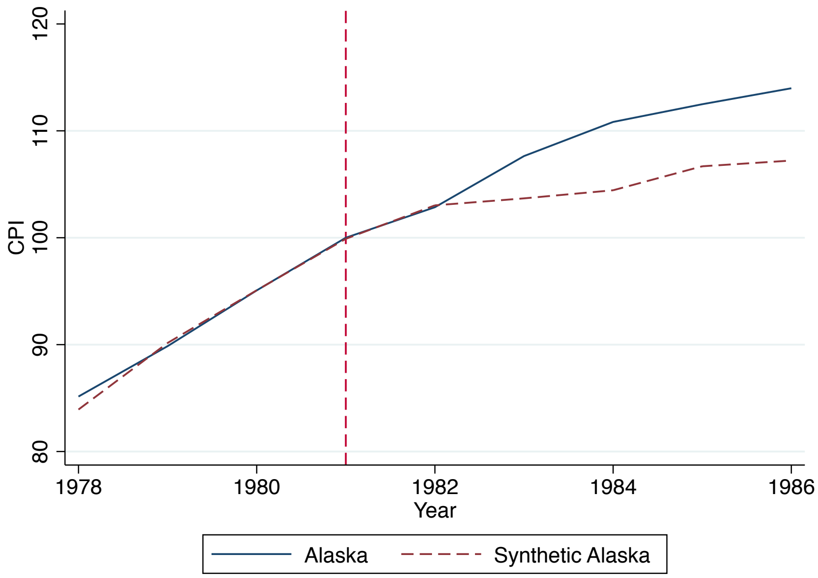
(a) Inflation (Tradables): Alaska vs. Synthetic Alaska



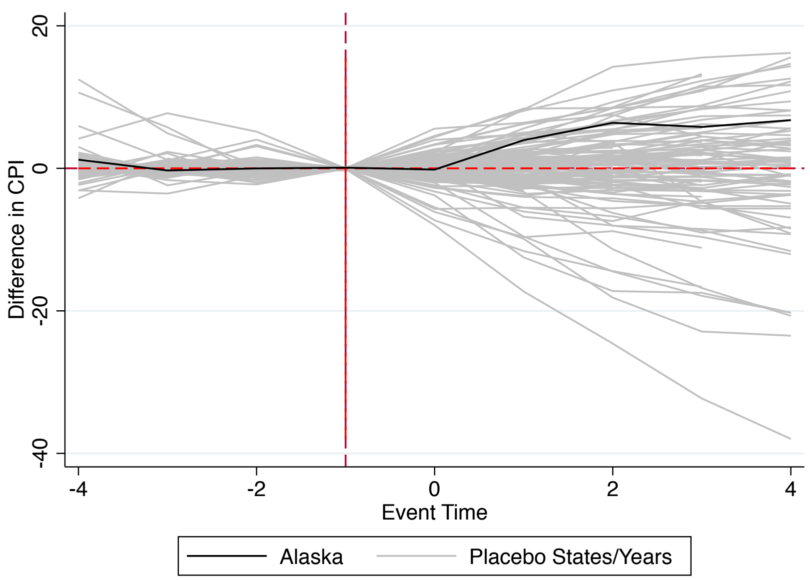
(b) Synthetic Difference in Inflation (Tradables), Alaska vs. Placebo States

Notes: Panel (a) plots the synthetic control estimates of the inflation for tradables for Alaska from 1978 to 2015. The solid line plots the actual inflation rate of tradables in Alaska, while the dotted line plots the synthetic control estimate. The vertical dashed line indicates 1981, the year before the onset of the Alaska Permanent Fund Dividend. Panel (b) plots the results of a permutation test of the significance of the difference between Alaska and synthetic Alaska. The solid dark line plots the difference for Alaska using the true introduction of the treatment in 1982. The light grey lines plot the difference using other states and or other treatment years. See Table 3 for the combination of states and weights that comprise each synthetic control.

Figure 5: CPI, 1978-1986



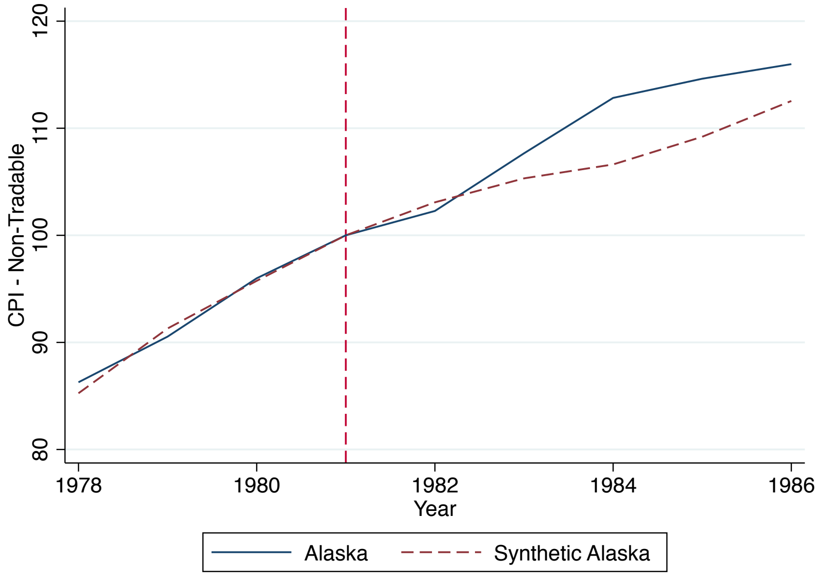
(a) CPI: Alaska vs. Synthetic Alaska



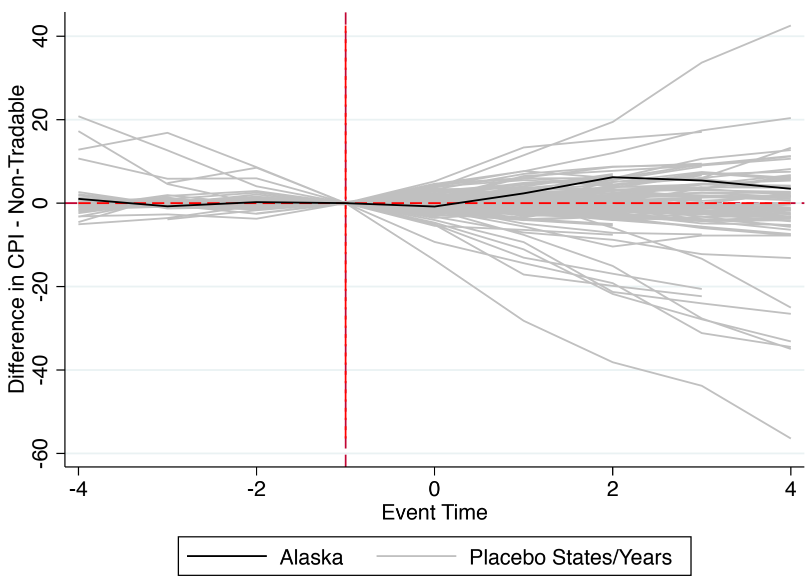
(b) Synthetic Difference in CPI, Alaska vs. Placebo States

Notes: Panel (a) plots the synthetic control estimates of prices for Alaska from 1978 to 1986. The solid line plots the actual prices in Alaska, while the dotted line plots the synthetic control estimate. The vertical dashed line indicates 1981, the year before the onset of the Alaska Permanent Fund Dividend. Panel (b) plots the results of a permutation test of the significance of the difference between Alaska and synthetic Alaska. The solid dark line plots the difference for Alaska using the true introduction of the treatment in 1982. The light grey lines plot the difference using other states and or other treatment years. See Table 5 for the combination of states and weights that comprise each synthetic control.

Figure 6: CPI (Non-Tradables), 1978-1986



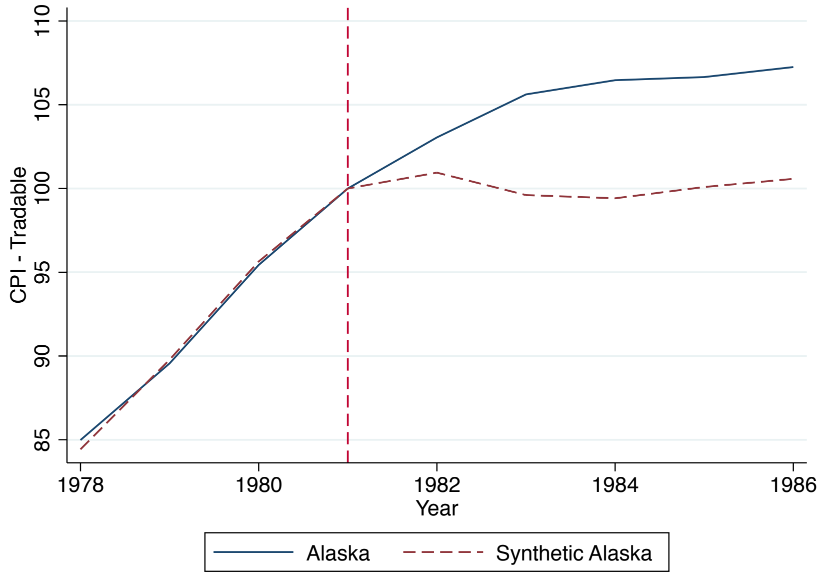
(a) CPI (Non-Tradables): Alaska vs. Synthetic Alaska



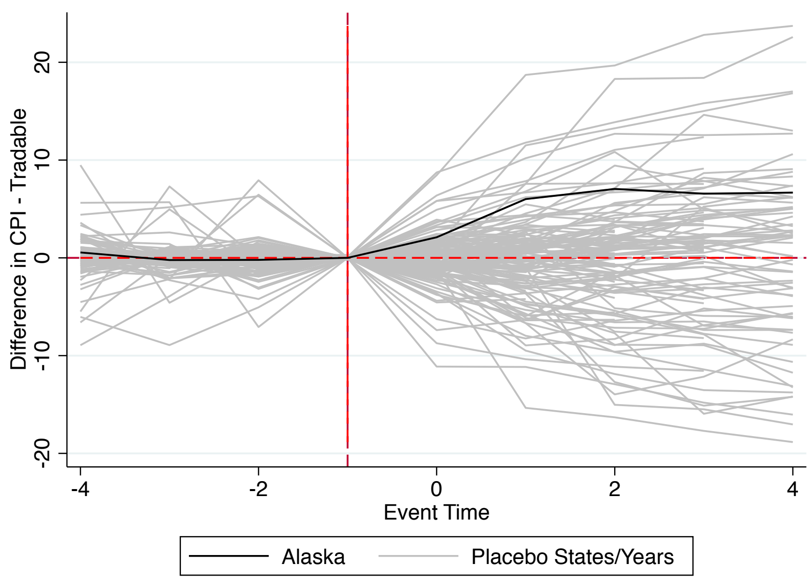
(b) Synthetic Difference in CPI (Non-Tradables), Alaska vs. Placebo States

Notes: Panel (a) plots the synthetic control estimates of prices for non-tradables for Alaska from 1978 to 1986. The solid line plots the actual prices of non-tradables in Alaska, while the dotted line plots the synthetic control estimate. The vertical dashed line indicates 1981, the year before the onset of the Alaska Permanent Fund Dividend. Panel (b) plots the results of a permutation test of the significance of the difference between Alaska and synthetic Alaska. The solid dark line plots the difference for Alaska using the true introduction of the treatment in 1982. The light grey lines plot the difference using other states and or other treatment years. See Table 5 for the combination of states and weights that comprise each synthetic control.

Figure 7: CPI (Tradables), 1978-1986



(a) CPI (Tradables): Alaska vs. Synthetic Alaska



(b) Synthetic Difference in CPI (Tradables), Alaska vs. Placebo States

Notes: Panel (a) plots the synthetic control estimates of prices for tradables for Alaska from 1978 to 1986. The solid line plots the actual prices of tradables in Alaska, while the dotted line plots the synthetic control estimate. The vertical dashed line indicates 1981, the year before the onset of the Alaska Permanent Fund Dividend. Panel (b) plots the results of a permutation test of the significance of the difference between Alaska and synthetic Alaska. The solid dark line plots the difference for Alaska using the true introduction of the treatment in 1982. The light grey lines plot the difference using other states and or other treatment years. See Table 5 for the combination of states and weights that comprise each synthetic control.

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Universal Cash Transfers and Inflation

Damon Jones and Ioana Marinescu

Online Appendix

Appendix A Appendix Tables and Figures

Table A.1: Average State Characteristics & Alaska Ranking (1978-1981)

	Alaska	**Average Among Other States**	**Alaska Rank Among All States**
Industry 1	0.360	0.397	3
Industry 2	0.097	0.059	21
Industry 3	0.036	0.128	2
Industry 4	0.189	0.166	21
Industry 5	0.078	0.084	8
Education 1	0.226	0.314	1
Education 2	0.397	0.369	19
Education 3	0.376	0.317	20
Unemployment Rate (%)	9.625	6.676	22
Oil to GDP	1141.460	10.883	23

Notes: Industry shares are: (1) agriculture, forestry, fisheries, mining, and construction; (2) manufacturing; (3) transportation, communications, utilities, wholesale, and retail trade; (4) finance, insurance, real estate, business, repair, and personal services; and (5) entertainment and recreation, professional and related services, public administration, and active duty military. Education shares are: less than a high school degree (1), high school degree (2), and at least some college (3). Oil to GDP Ratio is interpreted as dollars of oil production per 1,000 dollars of GDP.

Table A.2: State Weights for Synthetic Alaska - All Pre-Periods Matched

State	Weight
Panel A: Inflation - All Pre-Periods Matched	
New Jersey	0.584
Connecticut	0.332
Minnesota	0.084
Panel B: Inflation - Non-Tradeable - All Pre-Periods Matched	
Texas	0.688
Connecticut	0.213
New Jersey	0.099
Panel C: Inflation - Tradeable - All Pre-Periods Matched	
Illinois	0.715
Maryland	0.256
Tennessee	0.029

Notes: Table reports the combination of states and weights chosen using the method in Section 3.2 to construct a synthetic control for Alaska. Panels A through C correspond to columns (1) through (3) in Table 7.

Table A.3: State Weights for Synthetic Alaska- All Pre-Periods Matched

State	Weight
Panel A: CPI - All Pre-Periods Matched	
New Jersey	0.430
Connecticut	0.337
Minnesota	0.233
Panel B: CPI - Non-Tradeable - All Pre-Periods Matched	
Maryland	0.391
District of Columbia	0.381
Connecticut	0.228
Panel C: CPI - Tradeable - All Pre-Periods Matched	
Minnesota	0.377
New Jersey	0.369
Tennessee	0.057
Illinois	0.048
Massachusetts	0.022
Florida	0.016
Washington	0.010
Maryland	0.010
District of Columbia	0.009
Michigan	0.009
Texas	0.009
Pennsylvania	0.008
Hawaii	0.008
Wisconsin	0.008
Oregon	0.008
Georgia	0.006
New York	0.006
Missouri	0.006
Connecticut	0.005
Ohio	0.005
California	0.003
Colorado	0.002

Notes: Table reports the combination of states and weights chosen using the method in Section 3.2 to construct a synthetic control for Alaska. Panels A through C correspond to columns (1) through (3) in Table 8.

Table A.4: Pre-Treatment Covariate Balance - All Pre-Periods Matched

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Inflation		Inflation Nontradable		Inflation Tradable		CPI		CPI Nontradable		CPI Tradable	
Years	Alaska	Synthetic Alaska	Alaska	Synthetic Alaska	Alaska	Synthetic Alaska	Alaska	Synthetic Alaska	Alaska	Synthetic Alaska	Alaska	Synthetic Alaska
1978	3.48	3.39	6.89	6.60	0.95	1.20	85.15	84.26	86.28	86.04	84.98	85.07
1979	5.54	6.53	4.94	6.61	5.38	5.48	89.86	90.11	90.54	90.82	89.56	89.65
1980	5.80	6.78	6.02	5.82	6.56	6.76	95.07	95.63	95.99	95.80	95.44	95.53
1981	5.18	4.94	4.18	5.05	4.78	5.01	100.00	100.00	100.00	100.00	100.00	100.00

Notes: Table reports average value of variables during the pre-treatment period for Alaska and the synthetic control constructed using the method in Section 3.2. See Appendix Tables **??** and **??** for the combination of states and weights that comprise each synthetic control.

Figure A.1: Missing Data (PI)

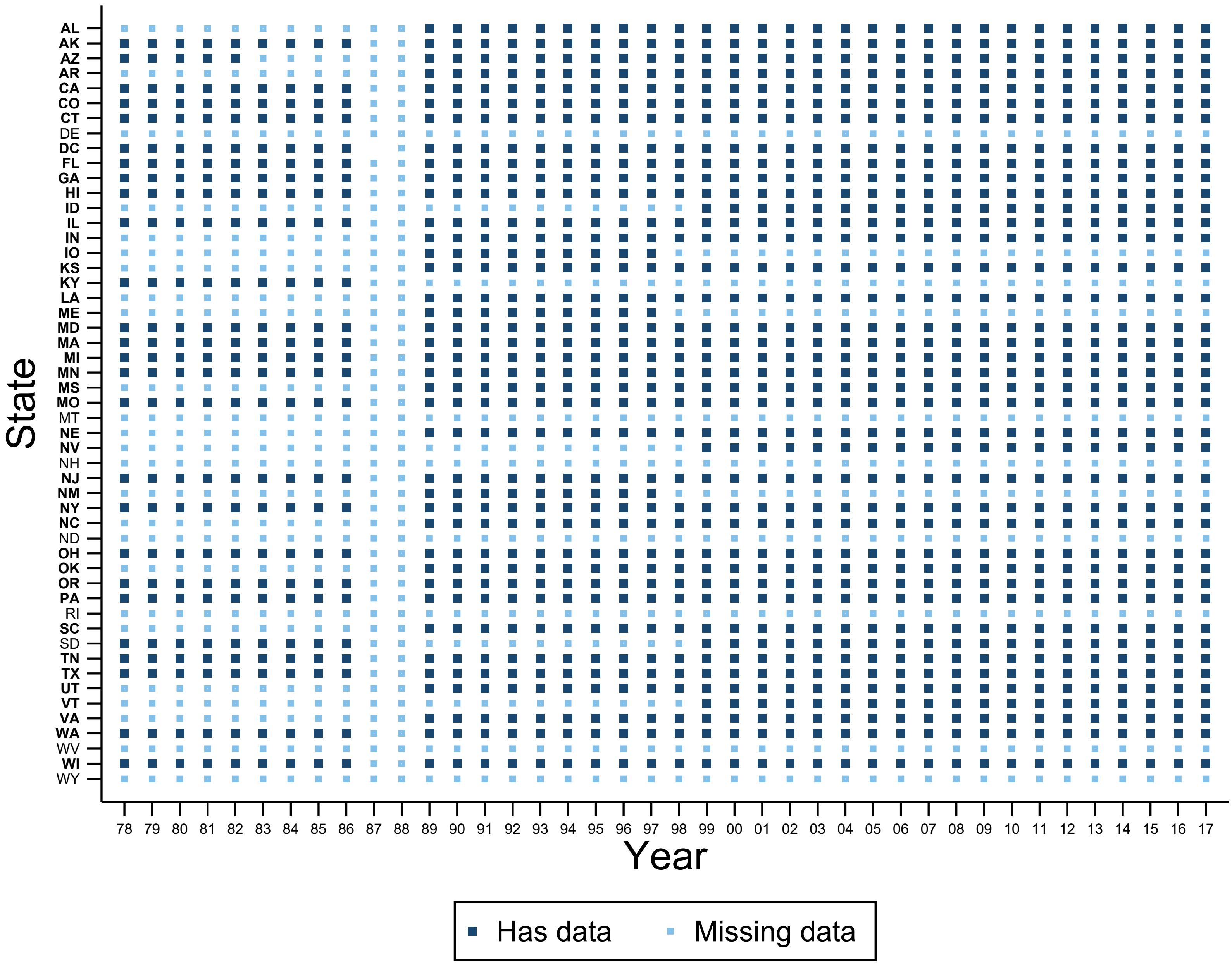
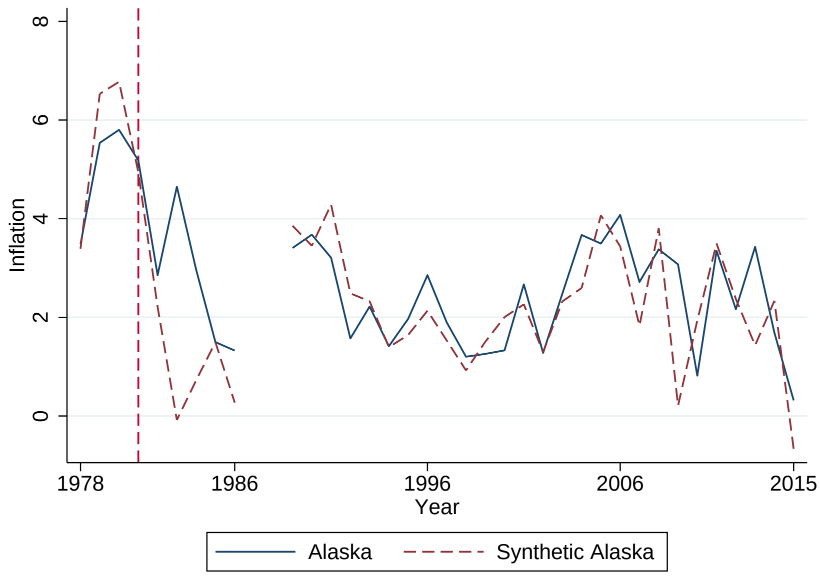
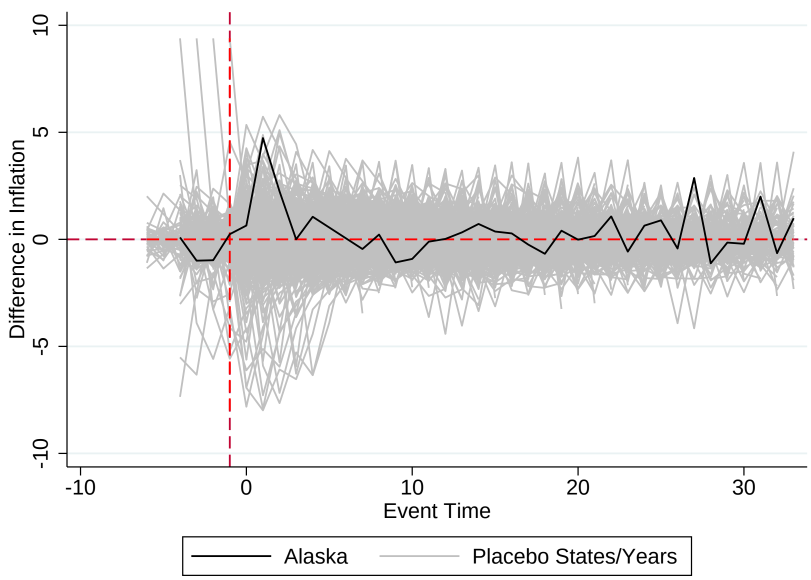


Figure A.2: Inflation: All Pre-Periods Matched

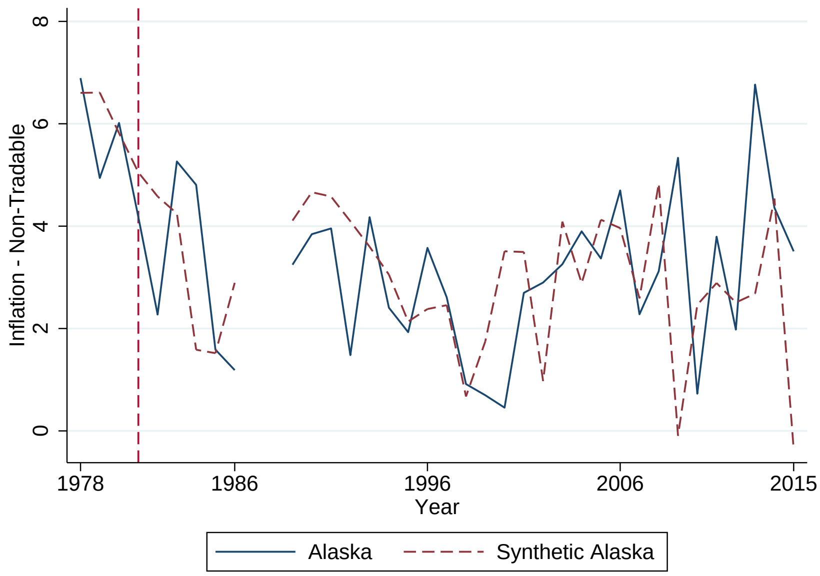


(a) Inflation

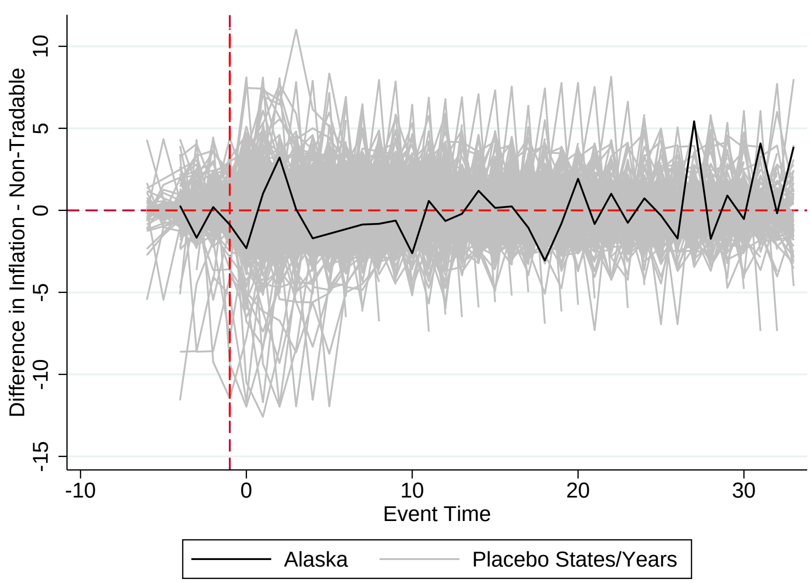


(b) Difference in Inflation

Figure A.3: Inflation (Non-Tradeables): All Pre-Periods Matched

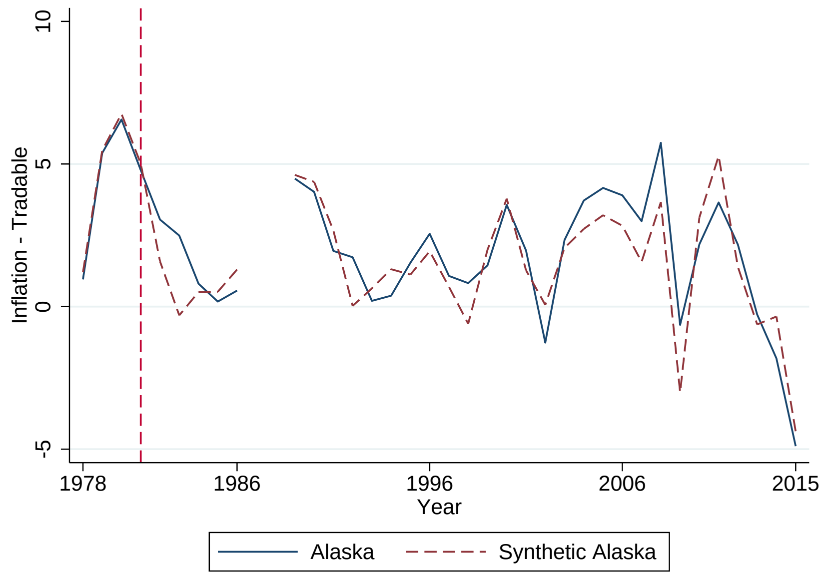


(a) Inflation (Non-Tradeables)

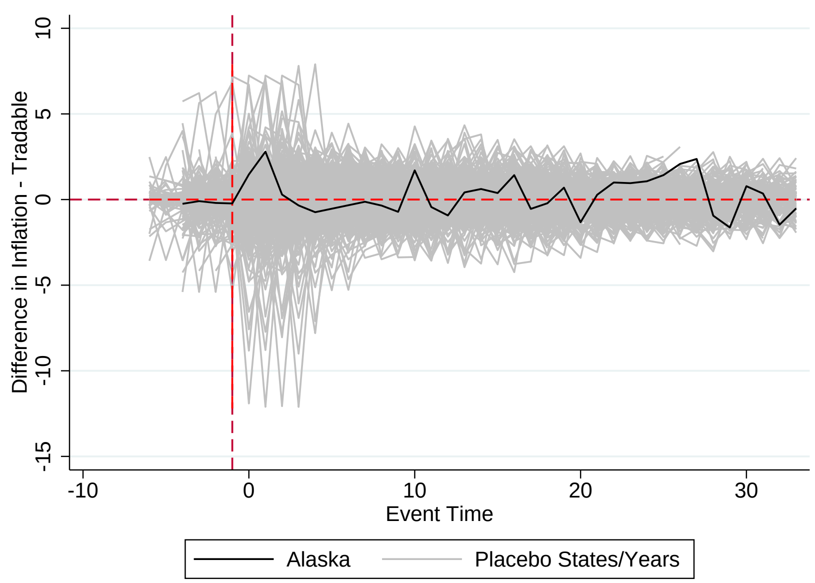


(b) Difference in Inflation (Non-Tradeables)

Figure A.4: Inflation (Tradeables): All Pre-Periods Matched

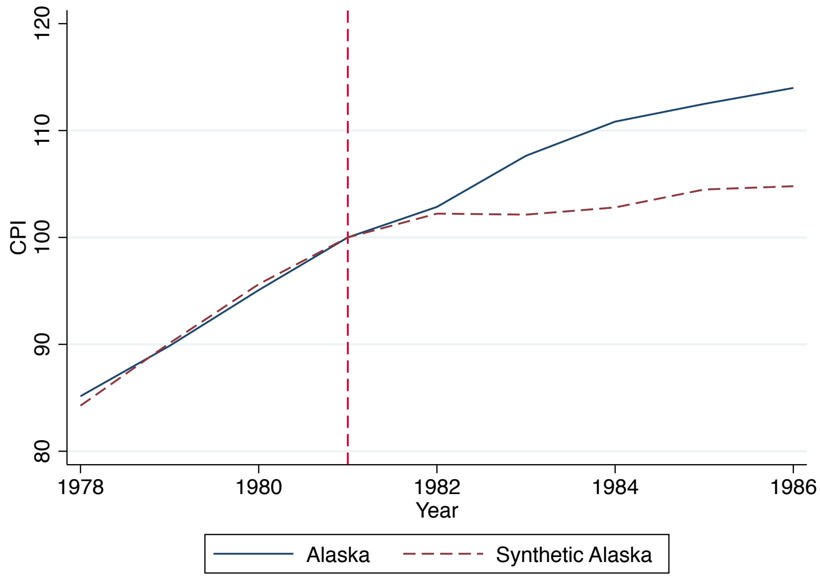


(a) Inflation (Tradeables)

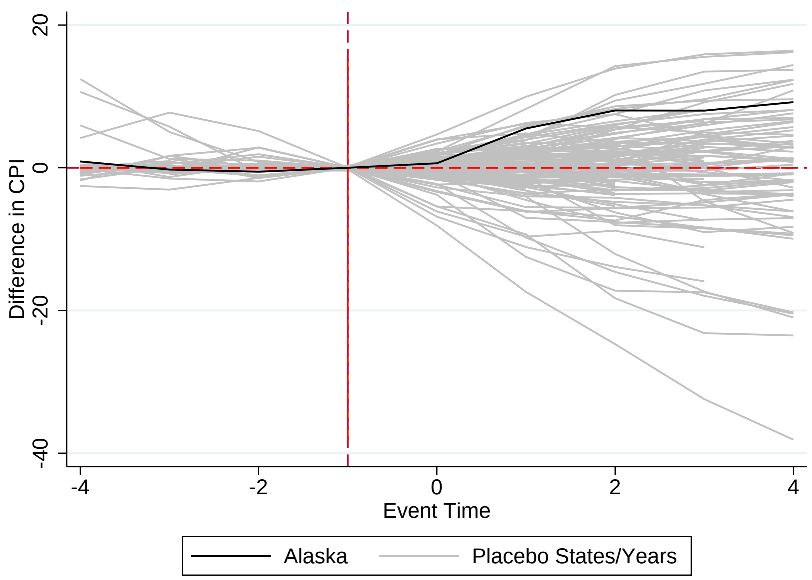


(b) Difference in Inflation (Tradeables)

Figure A.5: CPI: All Pre-Periods Matched

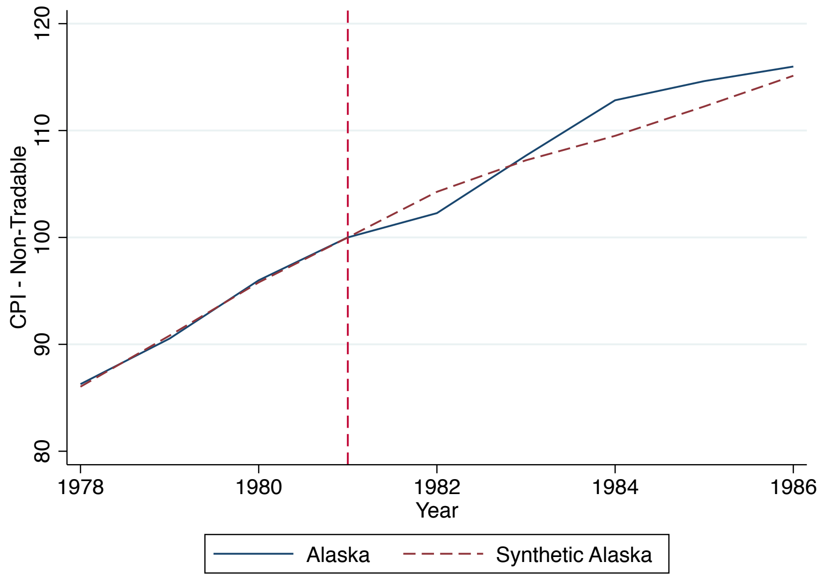


(a) CPI

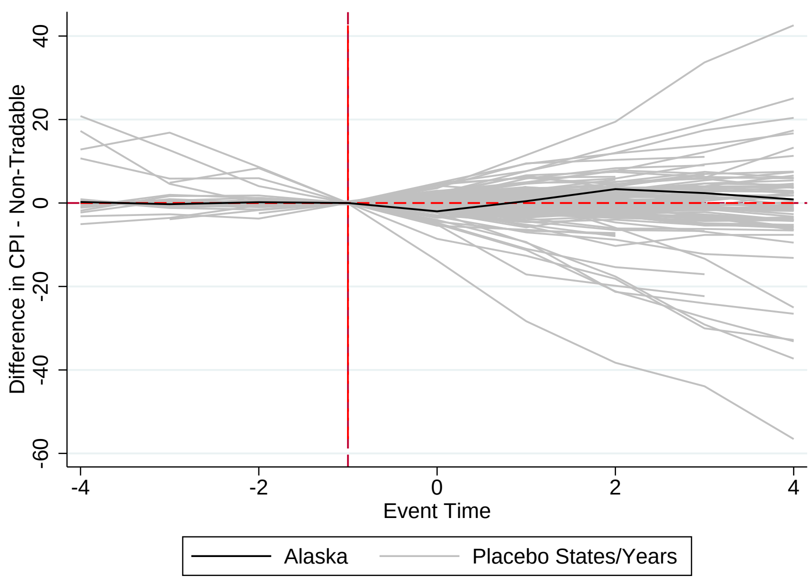


(b) Difference in CPI

Figure A.6: CPI (Non-Tradeables): All Pre-Periods Matched

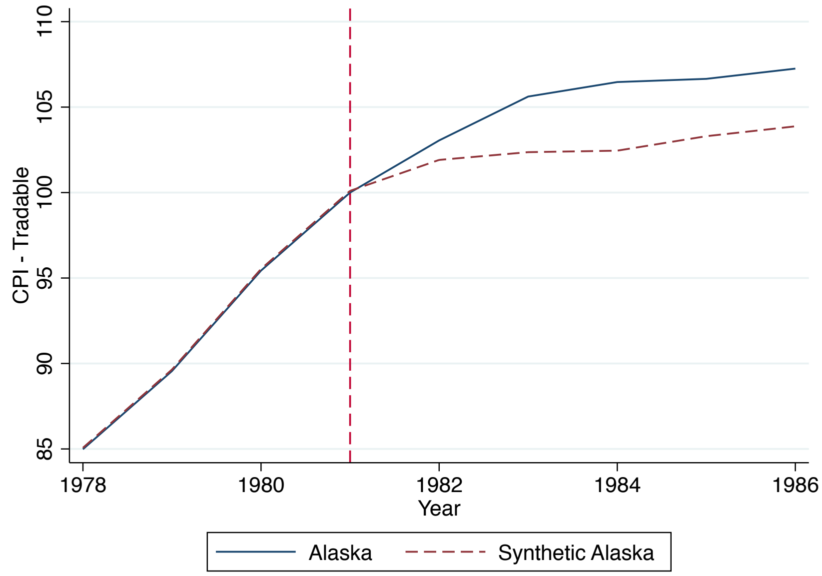


(a) CPI

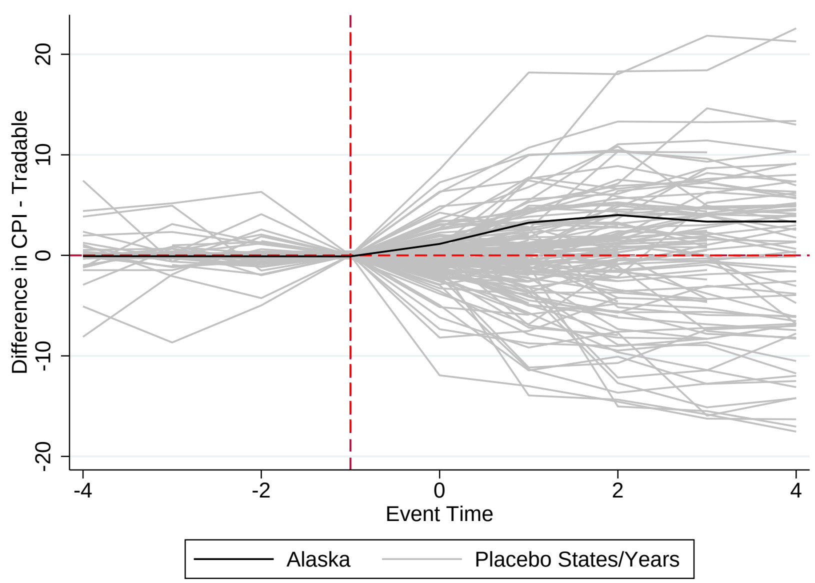


(b) Difference in CPI (Non-Tradeables)

Figure A.7: CPI (Tradeables): All Pre-Periods Matched



(a) CPI



(b) Difference in CPI (Tradeables)