

# Dynamic Responses to Labor Demand Shocks: Evidence from the Financial Industry in Delaware\*

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## Abstract

This paper analyzes the dynamic response to a large exogenous labor demand shock: the relocation of financial firms to Delaware following a Supreme Court ruling and state legislation in the 1980s. Using synthetic control methods I find significant effects on employment growth and the unemployment rate five years after the policy was passed. Thirty years later, there was much convergence to the pre-policy equilibrium. However, there were persistent effects on the unemployment rate and wages. The results suggest that while large short-run effects are not sustainable due to in-migration, smaller long-run effects persist due to direct productivity effects or agglomeration.

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# 1 Introduction

Jurisdictions regularly engage in fiscal or regulatory races to attract firms and stimulate labor demand.<sup>1</sup> While in the short run attracting new firms should decrease local unemployment and increase local wages, the long-run impact is less obvious. In particular, these policies may have no long-run effect if the economy returns to its pre-policy spatial equilibrium. Previous literature has suggested several potential long-run adjustment mechanisms (Bartik 1991, Blanchard and Katz 1992). Higher wages, resulting from the positive labor demand shock, may force some firms to exit, increasing the unemployment rate and decreasing wages. Alternatively, higher wages and lower unemployment rates may encourage an in-migration of workers. This puts downward pressure on wages, and increases the unemployment rate, but results in a permanently higher employment level. Short-run effects may also persist if the policy increases worker productivity, either directly or through agglomeration.

Given the prevalence of these policies as an economic development tool, understanding their long-run economic impact is crucial for policymakers. While previous papers have studied shocks to manufacturing and energy,<sup>2</sup> there is a particular lack of evidence on policies attracting white-collar jobs, an important target for local jurisdictions. Reflecting this importance, Prudential Financial and Royal Bank of Scotland each received more than 100 million dollars in state grants from 2007 to 2012 (Story, Fehr, and Watkins 2012).<sup>3</sup> Wages, geographic mobility, and agglomeration economies may be quite different in these jobs relative to manufacturing and energy jobs. These differences may yield important differences in short- and long-run policy effects.

This paper studies the dynamic effects of an exogenous increase in local

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<sup>1</sup>A recent investigation found that states, counties, and cities give more than \$80 billion a year to companies in incentives (Story 2012). Carruthers and Lamoreaux (2014) survey the literature on regulatory races.

<sup>2</sup>This literature is reviewed at the end of the section.

<sup>3</sup>Based on these data, Prudential Financial was awarded \$224 million in state grants from 2007 to 2012, and Royal Bank of Scotland was awarded \$121 million. Out of 48 companies identified to have received more than \$100 million dollars in state grants from 2007 to 2012, Prudential was ranked 11th and Royal Bank of Scotland 39th (Story, Fehr, and Watkins 2012).

labor demand affecting the finance sector, resulting from a landmark United States Supreme Court decision. In 1978, the US Supreme Court ruled in *Marquette National Bank of Minneapolis v. First Omaha Service Corp.* that a bank could export the highest interest rate allowed by the state in which it is headquartered. Previously, state usury laws determined the maximum interest rate that banks could charge customers residing in that state (regardless of where the bank was headquartered). This effectively deregulated the bank credit card market in the United States. An existing literature studies *Marquette's* impact on credit card interest rates, profits, consumer finance, and entrepreneurship (Ausubel 1991, Chatterji and Seamans 2012, Knittel and Stango 2003, Zinman 2003). However, this is the first paper to study the exogenous increase in local labor demand, arising from bank relocation, following *Marquette*.

*Marquette* implied that if one state eliminated its usury laws, then banks could relocate to that state and charge unlimited interest to customers around the country. South Dakota eliminated its usury laws in 1980, and Delaware followed in 1981 with the Financial Center Development Act (FCDA). This legislation had several provisions, including eliminating the usury laws, introducing a regressive tax structure for banks, and reducing other regulation of the finance industry. Likely because of its proximity to New York and its regressive tax, many more banks and credit card companies opened subsidiaries in Delaware than in South Dakota. I study the economic adjustment to, and the long-run impact of, this exogenous increase in local labor demand.<sup>4</sup>

The ideal estimate of the policy's "treatment effect" would compare outcomes in Delaware in year  $t$ , to the outcome in Delaware in year  $t$  if the policy had not been implemented. Because this control is not observed, I use synthetic control methods to create a weighted composition of states that ap-

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<sup>4</sup>Several earlier papers study the effect of the FCDA (Butkiewicz and Latham 1991 and Abrams and Butkiewicz 2007). These papers find positive effects of the FCDA on Delaware's economy. I extend their study of the FCDA by focusing more on the economic adjustment mechanism, identifying a control group to Delaware, using micro-level wage data, and testing for agglomeration. Weinstein (2015) studies whether this sector-specific increase in local labor demand affects choice of college major.

proximate Delaware's economy had the policy not been implemented (Abadie, Diamond, and Hainmueller 2010, 2014). If Delaware's policy directly affected other states, they would no longer be good approximations of Delaware in the absence of the policy. I present several restrictions on the set of potential control states to address these concerns.

I construct a dataset from 1960 to 2013 using the Current Employment Statistics (CES), Local Area Unemployment Statistics (LAUS), Federal Housing Finance Agency (FHFA) Index, CPS Microdata, and various data from the US Census. Because the wage data are at the individual level, the synthetic control method employed for the state-level data is not appropriate. Instead, I estimate regressions controlling for state characteristics in the pre-policy period.

The results suggest the policy had large effects on Delaware's economy approximately five years after it was passed. By this time, employment growth was higher in Delaware than in the synthetic control. This was partly due to a decrease in the unemployment rate, an increase in the participation rate, and an in-migration of new workers. There is some evidence of temporary effects on wages in this first five year period, which disappear the following year. That these wage effects are only temporary is consistent with increases in participation and population growth. Housing prices also rise during this period immediately following the policy.

By the late 1980s, just under ten years after the policy was implemented, employment growth in Finance, Insurance, and Real Estate (FIRE) slowed relative to the synthetic control, as did total employment growth. Despite this lower employment growth, population growth continued, likely explaining the increase in the unemployment rate and decrease in the participation rate relative to the synthetic control. Layoffs and firm exits cannot explain this convergence, since employment growth does not fall significantly or persistently relative to the synthetic control.

Thirty years after the policy was implemented, employment growth, housing price growth, and participation had all converged to the synthetic control. However, the exogenous increase in labor demand had local long-run benefits.

Delaware's unemployment rate was lower than had the policy not been implemented (though closer to the pre-policy equilibrium than it was in the first post-policy decade) and wages were higher through the 1990s. In addition, economic adjustment occurred through population growth, implying that the employment level, and tax base, is permanently higher in Delaware relative to the synthetic control. Despite employment and population growth, housing price growth is lower in the second decade following the policy, suggesting a flat long-run housing supply curve.

I investigate whether renewed FIRE growth in the 1990s, and the long-run effects on the unemployment rate and wages are due to agglomeration, or another shock to labor demand. If workers are paid their marginal product and marginal product increases as a direct result of the policy, then wages and unemployment should not return to the pre-policy equilibrium, even with population growth. Testing for agglomeration requires controlling for these direct policy effects. I compare FIRE employment growth in Delaware to South Dakota, which passed a policy similar to the FCDA one year earlier but attracted fewer firms. I find FIRE employment growth was lower in Delaware than South Dakota starting in the mid 1990s. While this does not rule out agglomeration, it does suggest the size of the agglomerative effects does not depend on the size of the FIRE sector, over this particular employment range.

The results have an important policy implication: large short-run effects from attracting firms are not sustainable due to in-migration. However, smaller long-run effects are possible if the policy has a direct effect, or an indirect agglomerative effect, on worker productivity.

The paper is related to Blanchard and Katz (1992) and Bartik (1991),<sup>5</sup> which study regional cycles and dynamic responses to labor demand shocks. I build on the empirics in these papers by identifying a more clearly exogenous shock to labor demand and using synthetic control methods. The paper also contributes to a more recent literature on place-based policies. Most notably, the paper complements a recent paper by Kline and Moretti (2014), which studies the long-run impact of the Tennessee Valley Authority (TVA),

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<sup>5</sup>Bartik (1991) also provides a review of the literature.

a place-based policy started in 1933. Kline and Moretti (2014) study a policy mainly affecting the manufacturing sector, and in a dramatically different geographical and historical setting. As acknowledged in their paper, the effects of more recent development efforts, in sectors other than manufacturing, may be very different.<sup>6</sup> A related literature studies whether temporary, local shocks can have long-run effects (Carrington 1996, Davis and Weinstein 2002, 2008, Hanlon 2015, Miguel and Roland 2011, Redding, Sturm, and Wolf 2011). This is among the first studies, to my knowledge, of the short- and long-run impact of an exogenous shock to labor demand affecting white-collar jobs.

## 2 Exogenous Shift in Labor Demand in Delaware

Prior to 1978, state usury laws determined the interest rate that credit card companies could charge residents of the state.<sup>7</sup> The US Supreme Court's ruling in *Marquette* allowed a bank to export the highest interest rate allowed by the state in which it is headquartered. At the time, large banks claimed they were incurring losses in their credit card divisions due to high interest rates, coupled with ceilings on the interest rates they could charge customers. After the *Marquette* ruling, banks were eager to find a state that would allow them to charge higher interest rates to customers around the country.

In 1980, South Dakota eliminated its usury laws, and Citibank subsequently moved its credit card operations to South Dakota. Delaware, which had historically provided a favorable business climate, was looking to diversify its economy from the automotive and chemical industry.<sup>8</sup> After the *Marquette*

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<sup>6</sup>The nature of the TVA and the Delaware policy are also quite different. While the TVA involved government spending on infrastructure projects, the Delaware policy involved a change in regulations, along with a change in tax structure. Regulatory and tax policies aimed at attracting firms are very prevalent today (Story 2013; Carruthers and Lamoreaux 2014). Second, while the TVA spanned state and county borders, Delaware's policy required that firms, and most likely labor, move across state lines to benefit. Analyzing the Delaware policy has implications for the many state-based economic development policies.

<sup>7</sup>The description of the FCDA in this section is based on Moulton (1983).

<sup>8</sup>Delaware was historically a favored location for business incorporation, due to its corporation law, Court of Chancery (corporations court), and a traditionally business-friendly government (Black 2007).

ruling, the state recognized the opportunity to attract the finance industry. In 1981, Delaware eliminated its usury laws, with the passage of the Financial Center Development Act (FCDA). In addition to eliminating ceilings on interest rates for most kinds of loans, the FCDA reduced other industry regulation and introduced a regressive tax structure for banks.<sup>9</sup>

While South Dakota was the first to eliminate its usury laws, Delaware was closer to the major financial centers of the Northeast. In addition, unlike Delaware, South Dakota did not introduce a regressive tax structure for banks (South Dakota Session Laws 1979).<sup>10</sup> As a result, many companies moved their finance or credit operations to Delaware, starting with J.P. Morgan in 1981. Many of the banks that moved operations to Delaware came from nearby states. As of March, 1983, seven banks from New York, four from Maryland, and three from Pennsylvania had established Delaware subsidiaries (Moulton 1983). Other states responded to Delaware's legislation, but either the legislation was too late or was not as generous as Delaware's legislation (Erdevig 1988).<sup>11</sup> The empirical strategy addresses the concern that other states were directly affected by Delaware's law.

The Supreme Court ruling in *Marquette*, followed by Delaware legislation, resulted in an arguably exogenous increase in labor demand in Delaware. Figure 1 shows that around the time of the policy there were clear increases in the share of Delaware's employment in FIRE. The synthetic control in Figure

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<sup>9</sup>There were capitalization and employment requirements for these FCDA banks. Other provisions of the FCDA include allowing borrowers and lenders to negotiate terms without interference from regulators, and banks to charge certain fees for credit accounts.

<sup>10</sup>From 1979 until 1991, South Dakota imposed a tax of 6% on the net income of financial institutions (South Dakota Session Laws 1979). Delaware's tax was 8.7% on the first \$20 million of net income, 6.7% on net income from \$20 to \$25 million, 4.7% on net income from \$25 to \$30 million, and 2.7% on net income over \$30 million (Moulton 1983). In 1991, South Dakota introduced a regressive tax on the net income of financial institutions (South Dakota Session Laws 1991).

<sup>11</sup>New York passed a law in 1981 eliminating its usury laws and allowing companies to charge fees, but did not restructure the taxes. Virginia eliminated interest rate ceilings on credit card loans in 1983. Maryland raised, but did not eliminate, their interest rate ceiling in 1982, and allowed fees on credit cards and invited out-of-state banks in 1983. Pennsylvania raised, but did not eliminate, the interest rate ceiling in 1982, and also allowed banks to charge certain fees (Erdevig 1988).

1 will be explained fully in a subsequent section. I study the dynamic effects of this exogenous shock.

### 3 Data

I obtain annual data from 1960 through 2000 on non-farm employment by state and SIC industry from the Bureau of Labor Statistics (BLS) Current Employment Statistics (CES). Since the SIC-basis estimates are only available until 2001, to obtain a longer time series for total employment I use total employment from the CES, NAICS basis. These data are available until 2013.<sup>12</sup> When constructing shares of total employment by industry, the denominator is total employment, SIC basis.<sup>13</sup> From the BLS Local Area Unemployment Statistics (LAUS), I obtain annual data from 1976 through 2013 on the labor force participation and unemployment rate by state. I obtain state unemployment rates from 1970 through 1976 constructed from labor market areas.<sup>14</sup> These unemployment rates were normalized to equal the LAUS unemployment rate in 1976.

I obtain population by state and year from the intercensal estimates of the US Census. The population numbers are the actual census population numbers in the Census years. I obtain several demographic measures at the state level from the pre-FCDA US Censuses in 1960, 1970, and 1980: percent with at least a high school diploma, percent of the population age 15 to 64, and percent living in metropolitan areas. I look at migration flows across states to identify other states affected by the FCDA . The measure of migration flows comes from the 1980 and 1990 Censuses. I obtain data on housing prices from the Federal Housing Finance Agency All-Transactions Index, which begins in 1975. I adjust the index using the Consumer Price Index for All Urban Consumers (CPI-U). To analyze the effect of the labor demand increase on wages, I use individual level data from the 1950, 1960, 1970, 1980 Censuses

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<sup>12</sup>Unlike the NAICS-basis data for total employment, NAICS-basis data by industry are only available starting in 1990.

<sup>13</sup>As a result, I measure employment by industry as a share of total non-farm employment.

<sup>14</sup>These data were provided by Larry Katz, and were used in Blanchard and Katz (1992).



and the 1977-2013 March Current Population Survey (CPS) Microdata (King et al. 2010, Ruggles et al. 2010). These data contain information on wages, occupation, industry, geographic location, and individual demographics.

## 4 Synthetic Control Method

The “treatment” effect of this policy in year  $t$  is  $Y_{DE,t} - Y_{DE,t}^N$ , where  $Y_{DE,t}^N$  is the outcome in Delaware if the policy had not been implemented. Clearly,  $Y_{DE,t}^N$  is not observed. Following Abadie, Diamond, and Hainmuller (2010, 2014), I estimate the treatment effect using  $Y_{DE,t} - \sum_{j=1}^J w_j^* Y_{jt}$ , where  $\sum_{j=1}^J w_j^* Y_{jt}$  is referred to as a synthetic control. The synthetic control is a weighted average of the outcomes in other states, where the weights are chosen to approximate Delaware in the absence of the policy. The synthetic control is chosen so that pre-policy predictors of the outcome are the same as in Delaware. Weights on the predictor variables are determined using a regression-based method, described below.

I analyze the response of several variables to this exogenous increase in labor demand: employment growth (both total and by industry), unemployment rate, participation rate, population growth, and housing price growth. I allow the synthetic control to vary with the outcome of interest. Delaware’s unemployment rate may have looked like state X’s in the absence of the policy, but the same may not be true of population growth. For robustness, I hold constant the synthetic control across outcomes.

### Adjusting for Policy Effects in the Control States

The synthetic control is constructed using states that looked similar to Delaware prior to the policy. The migration of firms and people to Delaware will yield negative effects in control states. This will lead me to double count the effect of the policy in Delaware: comparing the positive effect in Delaware to the negative effect in the control states.

Using data from the US Census, for each state I compare the fraction of

the 1985 population that had moved to Delaware by 1990, and the fraction of the 1975 population that had moved to Delaware by 1980. Looking at the difference in mobility across these years allows me to infer the effect of the policy on mobility. The principal results do not use the top five states losing population to Delaware from 1985 to 1990, relative to 1975 to 1980. These include (with difference in the fraction of population lost to Delaware in parentheses): Maryland (.00043), Pennsylvania (.00039), New Jersey (.00035), West Virginia (.00025), and Rhode Island (.00023). These differences are all very small. For example, from 1975 to 1980, Maryland lost .28% of its population to Delaware (approximately 11,600 people). From 1985 to 1990, Maryland lost .32% of its population to Delaware (approximately 14,200 people). For robustness, I construct the synthetic control without using the top 10 states losing population to Delaware, and also the top 15 states.

The states losing the most population to Delaware as a result of the policy include all of Delaware's bordering states. For robustness, I also estimate the synthetic control without using as controls any states that border Delaware or that border Delaware's neighboring states. These include: New Jersey, Pennsylvania, Maryland, Washington, DC, Virginia, West Virginia, Ohio, and New York.

## **Choosing the Predictor Variables for Matching States**

The synthetic control method involves, for each outcome of interest, matching Delaware to states with similar pre-policy outcome predictors. The weighting of these predictors is determined by a data-driven regression-based method, minimizing the mean squared prediction error of the outcome in the pre-policy period (Abadie, Diamond, and Hainmueller 2010). This section discusses the choice of the predictor variables.

One possible set of predictors would be the value of the outcome in each pre-policy year. For example, to observe the policy's effect on population growth in Delaware, I could match Delaware to a combination of states that would yield similar values of population growth in each pre-policy year. This

assumes that if pre-policy outcomes are similar, post-policy outcomes would have been similar in the absence of the policy.

This assumption would be violated if there is a change in the 1980s that differentially affects states with similar pre-policy outcomes. One likely characteristic which could yield differential effects is sectoral composition. If Delaware and Hawaii had the same population growth in each pre-policy year, then Hawaii would be the synthetic control. If Delaware's economy had more manufacturing in the 1970s, and manufacturing experienced national declines in the 1980s, then Delaware's population growth in the 1980s would likely be affected in the absence of the policy. This will not be reflected in Hawaii's population growth.

To allow for structural changes differentially affecting states with similar pre-policy outcomes, I include as predictors the outcome in each pre-policy year along with five-year averages of the following sectoral, economic, and demographic predictors in the pre-policy period: share of employment in construction; FIRE; manufacturing; trade; services; transportation and utilities; government; as well as the unemployment rate; labor force participation rate; and population growth.<sup>15</sup> I also include as predictors the 1960, 1970, and 1980 Census values for the percent living in metropolitan areas; percent of the population 15 to 64; and percent with at least a high school diploma.

Table 1 shows the control states comprising the synthetic control when excluding the top five states losing population to Delaware as a result of the policy. Each column presents the synthetic control for that particular dependent variable. It is clear that there is variation across outcomes. For robustness, I hold the composition constant.

Motivating the use of the synthetic control, columns 1 and 2 of Table 2 convey several important pre-policy differences between Delaware and the

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<sup>15</sup>Specifically, I include five year averages from 1960 through 1979, as well as the value in 1980 of the following variables: share of employment in construction; FIRE; manufacturing; trade; services; transportation and utilities; government. I include five year averages from 1970 through 1979, and the value in 1980 of the unemployment rate. I include five year averages from 1961 through 1980 of population growth, and the average from 1976 through 1980 of labor force over population.

average of all the other states. Most notably, a larger share of Delaware’s workforce was employed in manufacturing, its population was more likely to be living in metropolitan areas, and Delaware’s unemployment rate was higher. Figure 2a further shows that before the policy, the average unemployment rate in the other states differs considerably from that in Delaware. Differences in the predictors and the pre-policy outcomes suggest that the average of the other states does not approximate Delaware in the absence of the policy.

The third column of Table 2 shows the predictors when including the unemployment rate in each pre-policy year. The share employed in manufacturing is much lower in the synthetic control than in Delaware. Furthermore, the percent living in metropolitan areas (except in 1980) and the percent with at least a high school diploma are also lower in this synthetic control. However, Figure 2b shows that the unemployment rate in the synthetic control very closely matches the pre-policy unemployment rate in Delaware. Including the pre-policy outcome in each year results in a very good pre-policy fit, but at the expense of other potentially important predictors. For robustness I also estimate the synthetic control using five-year averages of the pre-policy dependent variable as predictors, rather than the value in each year. This should allow for more weight on sectoral composition, addressing concerns that controlling for pre-policy outcomes, sectoral composition may affect later outcomes due to other shocks.

## 5 Dynamic Responses to a Labor Demand Shock

### 5.1 Policy Impact on Sectoral Composition

Figure 3 shows how the policy affected the sectoral composition of Delaware’s economy. The first plot shows the dramatic increase in the share employed in FIRE. Before the policy, approximately 5% of Delaware’s economy, and that of the synthetic control, was employed in FIRE. However, immediately after the policy, the percent of Delaware’s economy employed in FIRE grew dramatically, reaching 12% by 2000. In the synthetic control, the percent

employed in FIRE remained constant. Trade, transportation and utilities, and government became a smaller percentage of Delaware employment relative to the synthetic control.

If the additional FIRE employment was from in-migration, this would suggest FIRE employment was growing in Delaware, while employment in these industries remained constant. In the next section, I analyze the extent to which FIRE employment was fueled by substitution across sectors, from unemployment, or from in-migration. Finally, the share employed in construction increased in Delaware relative to the synthetic control after the policy. The arrival of new firms likely necessitated new office locations and housing.

## 5.2 Policy Impact on Economic Variables

Figure 4 shows the principal results of the paper. The plot in the top left shows FIRE employment growth in levels as a share of employment. Starting in 1981, the year the law was passed, the FIRE industry grows in Delaware relative to the synthetic control. Prior to the policy FIRE was a small share of Delaware employment. Thus, despite large percentage growth in FIRE in the first five years, this is only a small percent of Delaware's total employment. The other plots suggest that in the first two years this FIRE employment growth led to total employment growth, fueled by a decrease in unemployment and increase in the participation rate. This suggests employed people are not simply moving across sectors.

In 1982, immediately after the policy, employment growth in Delaware is about 1.8 percentage points higher than in the synthetic control. This drops over the next four years to a differential of .9 percentage points. However, by 1987, this difference reaches over 2.7 percentage points. Part of this employment growth is from a decrease in the unemployment rate. Delaware's unemployment rate drops to about three percentage points below that of the synthetic control during this period. Some of the employment growth also seems to be fueled by in-migration, as population growth in Delaware grows to approximately one percentage point larger than in the synthetic control.

Finally, the participation rate increases relative to the synthetic control over this period, to a differential of approximately 3 percentage points.

By the end of the 1980s and through the early 1990s, FIRE employment growth had slowed considerably. Total employment growth in Delaware becomes more similar to the synthetic control, and in some years is even lower. The plots show that despite slower total employment growth, population growth continues at a high rate. This arguably explains the increase in the unemployment rate, and the decrease in the participation rate in Delaware relative to the synthetic control. The period from the late 1980s through the early 1990s presents evidence that large short-term drops in the unemployment rate and increases in the participation rate due to an exogenous increase in labor demand are not sustainable. While some of this is due to lost jobs, this alone cannot explain the convergence towards the pre-policy equilibrium. Total employment growth never falls dramatically nor persistently. It appears that the principal adjustment mechanism is population growth.

Figure 4 shows Delaware experienced renewed FIRE growth in the mid to late 1990s, and this translated into total employment growth. I explore whether this renewed growth is due to agglomeration later in the paper. Continued in-migration is likely the explanation for why this employment growth does not yield decreases in the unemployment rate. By 2000, FIRE growth had subsided, and total employment growth does as well. Continued in-migration in this environment leads the unemployment rate and participation rate to further converge to that of the synthetic control by the year 2013.<sup>16</sup>

Housing price growth in the 1990s was slower than in the synthetic control, despite employment growth in the mid to late 1990s, and continued population growth. Relatively inelastic short-run housing supply may explain why housing prices grew faster in the 1980s, as Delaware experienced employment and population growth. However, by 1990 new construction may have shifted

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<sup>16</sup>Participation drops below that of the synthetic control by the end of the sample period. Appendix Figure A4 in the Online Appendix shows, based on CPS data, that in these years (2006 until 2010) there was also an increase in the percent of new residents in Delaware who were 55 and older. In 2014, Kiplinger ranked Delaware as the 7th most tax-friendly state for retirees, and the tax-friendliest in the Northeast (*10 Most* 2014).

out the housing supply curve and reduced pressure on prices. If housing supply growth continued throughout the 1990s, this would explain lower housing price growth in Delaware despite employment and population growth. These results suggest a flat long-run housing supply curve, consistent with previous findings (Blanchard and Katz 1992, Bartik 1991). The results are subject to the caveat that this synthetic control is based on only a few pre-policy years.

In sum, the exogenous increase in labor demand has significant effects on employment growth and the unemployment rate in the short term. However, the substantial decline in the unemployment rate relative to the synthetic control cannot be sustained in the long run due to in-migration. Importantly, the unemployment rate does remain below the pre-policy equilibrium in the long run, an effect explored later in the paper. Employment growth never falls in a significant and sustained way relative to the synthetic control. As in Blanchard and Katz (1992), the shock leads to a permanently higher employment level, beneficial to policymakers in the form of a larger tax base.

Following Abadie, Diamond, and Hainmueller (2010), I assess whether these effects are statistically significant through the use of placebo tests. I estimate the treatment effects from assuming each of the states in the donor pool (excluding the top five states losing population to Delaware as a result of the policy) is the treated state. For each state, I construct a synthetic control using the principal synthetic control specification. If the differences between Delaware and the synthetic control are much larger than the differences between the other states and their synthetic controls, the results are less likely due to chance alone.

Figure 5 shows the policy's effects on the growth of FIRE employment as a share of total employment, as well as the unemployment rate, appear nonrandom over the entire post-policy period. There is also strong evidence that the increase in total employment growth, population growth, labor force over population, and housing price growth from the mid-1980s to the late-1980s was nonrandom. The continued effects on population growth, as well as total employment growth in the 1990s, are also nonrandom.

## Robustness

Using the pre-policy outcomes in each year as predictors yields low weights on the sectoral and demographic predictors. This is problematic if there is a post-policy trend affecting a particular economic sector. To address this concern, I estimate the synthetic control using five-year averages of the pre-policy outcome as the predictor, rather than the value in each year. This should allow for greater weights on the sectoral predictors, and thus smaller differences in these predictors between Delaware and the synthetic control. The fourth column of Table 2 shows that when the dependent variable is the unemployment rate, this procedure yields predictors that are much more similar to Delaware, especially the share employed in manufacturing. The tradeoff is more dissimilar pre-policy outcomes.

Appendix Figure A1 in the Online Appendix shows this robustness specification yields similar results. The largest differences are in the size of the effects in the 1990s. The robustness specification shows no employment gains in the 1990s, stronger convergence of the unemployment rate in the 1990s, and a smaller population growth differential. In addition, employment growth relative to the synthetic control occurs only in 1982 and 1987, and not in between. However, the synthetic control looks quite different from Delaware in the pre-policy years.

The principal results estimate a different synthetic control for each outcome. As an additional robustness check, I estimate the synthetic control holding fixed the weights on each predictor. This implies that the composition of the synthetic control remains constant across outcomes. I obtain the weights on the predictors by constructing the synthetic control for the share employed in manufacturing. Manufacturing was important to Delaware's economy in the pre-policy period, and American manufacturing experienced significant declines in the 1980s. As a result, states where manufacturing was similarly important may be the best approximation of Delaware in the absence of the policy. Appendix Table A1 in the Online Appendix shows the states comprising this synthetic control, and Appendix Figure A2 in the Online Appendix shows the principal results are robust to using the manufacturing synthetic



control. One slight difference is that population growth appears to begin sooner after the policy when using this robustness synthetic control.

Finally, I estimate three alternative specifications of the synthetic control in which I exclude the top 10 and top 15 states losing population to Delaware, and the states within two states of Delaware. The results from these specifications are very similar to those shown in Figure 4 (not shown). One minor exception is that there are slightly smaller differences in total employment growth in the early 1980s when excluding the top 10 and 15 states losing population to Delaware. Similarly, the post-policy differences in population growth are slightly smaller when excluding the top 10 states losing population to Delaware.

### Regression-adjusted estimates

As an alternative to the synthetic control estimation, I estimate a regression controlling for averages of the predictors in the pre-policy period. Specifically, I estimate:

$$y_{st} = Z_s\eta + \gamma_t + \delta_t year\_t * DE_s + u_{st} \quad (1)$$

The vector  $Z_s$  consists of the predictor averages over five-year periods, starting in 1960 through 1979. The value of the predictor in 1980 is also included. These predictors include share employed in FIRE, manufacturing, trade, transportation and utilities, services, construction, government, as well as the unemployment rate, population growth, labor force over population, employment growth, housing price growth, and FIRE employment growth as a share of employment. As these variables are available starting in different time periods, their averages begin in the first five-year period in which there are data. I also include the demographic variables from the Census which were used in the synthetic control specifications.

The variable  $year\_t$  is an indicator for whether  $year$  is equal to  $t$ . The variable  $DE_s$  is an indicator for state  $s = Delaware$ . I do not estimate a constant term so that I obtain coefficients on each of the year indicator

variables. Each observation is a unique state/year pair, and so I estimate the standard errors using the traditional heteroskedasticity-robust formula.

The coefficient  $\delta_t$  measures the difference between the outcome in Delaware and the average outcome outside of Delaware in year  $t$ , controlling for trends in multiple state characteristics over the course of the pre-policy period (through controlling for averages over five-year periods). To avoid capturing policy effects in the control states, I exclude the top five states losing population to Delaware from 1985 to 1990 relative to 1975 to 1980. Appendix Figure A3 presents the coefficients  $\delta_t$  from equation (1), and their 95% confidence intervals for the main outcomes.

The general patterns are very similar to those in Figure 4, with large effects in the mid-1980s, and some convergence by the late 1980s and 1990s. There is also a persistent effect on the unemployment rate. Given that I have controlled for the five-year average of the pre-policy outcome, the effects should be close to zero in the pre-policy period. However, several of the outcomes convey a non-zero difference between Delaware and other states in this pre-period. To determine the magnitude of the policy's effect on these variables, I compare the pre- and post-difference. The magnitudes are generally similar to those in the synthetic control.

## 6 Dynamic Response of Wages

I use the 1950 and 1960 1% sample, 1970 1% Form 1 State sample, and 1980 5% state sample, along with the CPS March Supplement Microdata from 1977 through 2013, to determine the effect of the labor demand shock on wages. Because these data are at the individual level, I do not use the synthetic control method. Instead, I control for state characteristics in the pre-policy period. I estimate the following regression:

$$\ln(w_{ist}) = X_{ist}\beta + Z_s\eta + \gamma_t + \delta_t year\_t * DE_{ist} + u_{ist} \quad (2)$$

The dependent variable is the log of the individual’s wage and salary income from the previous year, in 1999 dollars. For each year, I exclude individuals with wages below the 1st percentile of the non-zero wages, or above the 99th percentile of the non-zero wages. The vector  $X_{ist}$  contains individual characteristics, including potential years of experience, potential years of experience squared, indicators for grouping of usual hours worked per week last year and weeks worked last year, years of education, and indicators for white, black, Asian, male, and married.<sup>17</sup>

$Z_s$  is a vector of state characteristics in the pre-policy period, including the value in 1980 as well as five-year averages from 1960 through 1964, and 1970 through 1974 of the following variables: share employed in FIRE, manufacturing, trade, and services, as well as the unemployment rate, and population growth.<sup>18</sup> Because labor force participation rate is available only in 1976, I include the value in 1980 and the average from 1976 through 1979. I also include the same state demographic variables from the Census as in the synthetic control specifications. To further improve the comparison I include the mean of the outcome variable in each state for each of the pre-policy years (1950, 1960, 1970, and 1977 through 1981).<sup>19</sup>

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<sup>17</sup>See Online Appendix for details on variable construction.

<sup>18</sup>I do not include each of the five-year pre-policy averages because of the limited number of individuals in the sample who are working in Delaware.

<sup>19</sup>Because these regressors are estimated with some error, they may induce measurement error bias into the results. To determine if this could be problematic, I calculate a rough approximation of the measurement error and the attenuation bias in the coefficients. I regress the outcome (log wage) in each pre-policy year on indicator variables for each state. Assuming classical measurement error, I calculate the reliability measure on the pre-policy average wage as  $(1 - \frac{\text{var}(SE_s)}{\text{var}(\text{meanincome}_{-t_s})})$  for each  $t$  in the pre-policy years.  $SE_s$  denotes the robust standard error on the state indicator variable for state  $s$ , and  $\text{meanincome}_{-t_s}$  is the average log income in state  $s$  in year  $t$ , for  $t = 1950, 1960, 1970$ , and 1977 through 1981. Because the banking and credit specification only includes the mean wage in the banking and credit sector for the year group 1977 through 1981, I estimate the reliability measure for this year group as a whole. The reliability measures are all very high, greater than .9, with a majority greater than .99. This suggests the coefficients suffer from very little attenuation bias, and should not greatly affect the other coefficients. These are rough approximations of the reliability measures given that when measurement error is present in multiple explanatory variables (as may be the case here), it is not the variance of the mismeasured variable that affects the plim of the coefficient, but the variance after netting out the other explanatory variables. Deriving the inconsistency of the estimators in this

The variable  $DE_{ist}$  is an indicator for whether individual  $i$  living in state  $s$  in year  $t$  was living in Delaware in that year. The variable  $year\_t$  is an indicator for whether  $year$  is equal to  $t$ . I do not estimate a constant term so that I obtain coefficients on each year indicator variable.

I estimate this specification using the full sample (including industry and occupation fixed effects), and separately for groups whose wages could be particularly impacted by this policy. I estimate a specification only including individuals whose industry was “Banking and credit agencies,”<sup>20</sup> and another specification only including “Accountants and auditors,”<sup>21</sup> and clerks and managers who would be relevant to the banking and credit industries.<sup>22</sup> While individuals with these occupations may not have been working in the finance industry, their wages may have increased because of demand from financial firms. When the sample is limited to those in the banking and credit industry, I include occupation fixed effects. When the sample is limited to accountants, auditors, relevant clerks, and relevant managers, I include occupation and industry fixed effects.

Due to the smaller sample sizes in the regressions including individuals in the banking and credit industry, I include an indicator for 1950, 1960, 1970, the five-year group from 1977 through 1981, and five-year groups of post-policy years and interact these with Delaware.<sup>23</sup> I only include the mean wage over the years 1977 through 1981 because of small sample sizes in early years.

The coefficient  $\delta_t$  measures the difference in the log wage in year  $t$  between an individual in Delaware and a similar individual, working in a state similar to Delaware before the policy. The year of post-policy wages is 1982, since the CPS asks about wages in the previous year. For each year  $t \geq 1982$ , I compare

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case is more complicated (Wooldridge 2002).

<sup>20</sup>coded as 716 using the 1950 Census Bureau industrial classification system.

<sup>21</sup>coded as 0 using the 1950 Census Bureau occupational classification system.

<sup>22</sup>I include the following 1950 Census Bureau occupation codes as clerks relevant in the banking and credit industries: 310 (“Bookkeepers”), 321 (“Collectors, bill and account”), 341 (“Office machine operators”), and 390 (“Clerical and kindred workers (n.e.c.)”). See Appendix Table 2 in the Online Appendix for the list of occupation codes included as managers relevant in banking and credit.

<sup>23</sup>The last group has two years: 2012 and 2013.

$\widehat{\delta}_t$  to  $\widehat{\delta}_{1981}$ . This compares the post-policy difference in log wages (in year  $t$ ) between Delaware and similar states, controlling for individual covariates, to the difference in log wages between Delaware and similar states in the year before the policy was implemented. I estimate the regression with standard errors clustered at the state level, state/year level, and unclustered but robust to heteroskedasticity. For the full sample, and the sample of workers in banking and credit, I report the standard errors clustered at the state/year level, as these are the largest.<sup>24</sup> For the sample of clerks, accountants, and managers, I report the unclustered standard errors, robust to heteroskedasticity, as these are the largest.

Figure 6a shows the wage results using all occupations and industries. The difference in wages between workers in Delaware and workers in observationally similar states is approximately 1.5% in the year before the policy. For five years after the policy, wages increase temporarily but these differentials are eliminated the following year. However, starting in 1987, wages start to increase in a more steady manner, and by 1989 the difference-in-difference reaches nearly 12 percentage points and is statistically significant. While the difference does not remain this large, it continues to be substantial. The difference-in-difference remains at a level of approximately 7 to 8 percentage points through 2007 with very few exceptions. Starting in 2008, the effect decreases but remains positive and statistically significantly different from the pre-policy effect.

These effects largely coincide with the pattern in employment growth seen in Figure 4. Combined with evidence in the previous section, the immediate wage effects were likely temporary due to increases in participation and increases in population growth. These short-run effects are consistent with previous findings (Blanchard and Katz 1992). In the next section I explore whether the later, more permanent effect on wages is due to another shock or

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<sup>24</sup>While the standard errors clustered at the state/year level are the largest, the F-tests for whether the effect in each year is equivalent to the effect in the year before the policy are also at times larger, especially with the full sample, resulting in lower p-values. This is because the covariances are higher when the standard errors are clustered at the state/year level.

agglomeration.

Figure 6b shows the wage results for those in the banking and credit industry. There is no immediate effect on wages, though similar to the full sample there are effects beginning in the late 1980s that are long lasting. Controlling for covariates, Delaware wages are approximately 9% less than in other similar states in the period immediately preceding the policy. While the magnitude suggests that wages decrease immediately following the policy, the difference relative to the pre-policy period is not statistically significant. This negative wage differential begins to narrow starting in the 1987-1991 period, years in which FIRE employment growth is very high in Delaware. The point estimate suggests the negative wage differential in Delaware falls considerably to 5%, though the difference relative to the pre-policy period is not statistically significant. The negative wage differential is eliminated completely by the 1992 to 1996 period, and is statistically significantly different from the pre-policy period. With the exception of 1997-2001, this difference remains statistically significant through 2011, at the 5% or 10% level.

The peak difference-in-difference for all occupations and industries was approximately 12 percentage points in 1989, from roughly a 1.5% to a 13.5% differential. The difference-in-difference for wages in the banking and credit industry in 1987-1991 was approximately 4 percentage points, and in 1992-1996 was approximately 11 percentage points, from a -9% to a +2% differential.

Figure 6c shows the results for relevant clerks, accountants, and managers. Similar to the banking and credit industry, there is no effect on wages until the late 1980s. However, there is no statistically significant long-run persistent increase in wages relative to other states. The point estimates do suggest a persistent effect, and in several years throughout the 1990s wages do increase significantly relative to other states.

As noted in Blanchard and Katz (1992), migration decisions are based not on nominal wages, but on consumption wages. Using the specification with all occupations and industries, wages reported in 1989 in Delaware are 11.6% higher than in similar states relative to their pre-policy levels in 1981.<sup>25</sup> Using

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<sup>25</sup>Subject to the log approximation, the results suggest that reported wages in Delaware

the results from the synthetic control, Delaware housing prices become 6.9% more expensive than in the synthetic control in 1988 (the relevant year for reported wages in 1989), relative to their levels in 1987.<sup>26</sup> Assuming a share of housing services of 15% as in Blanchard and Katz (1992) and ignoring that other prices may also go up, consumption wages increase by approximately 10.6% in 1989. Thus, while housing price growth dampens the incentive for individuals to migrate to Delaware, this effect is not large.

Previous literature has found that while employment increases immediately after a labor demand shock, wages increase slowly (Kline 2008). While the greatest wage growth for all occupations occurred in years with significant employment growth, it did not occur in the year with the largest employment growth of the decade, 1987 (1988 in the wage results). This presents some evidence of lagged wage growth, although the wage estimates are very imprecise. Also, we do see large wage growth for clerks, accountants, and managers in 1988.

## 7 A Second Labor Demand Shock or Agglomeration?

A crucial policy question is whether the persistent local effects in Delaware represent agglomerative effects, rather than direct policy effects or a separate labor demand shock. After considerable research to identify whether these later effects were the result of a second labor demand shock affecting Delaware's FIRE sector, one major candidate emerged. In 1990, Delaware enacted another law creating a regulatory environment for banks that differed from most other states, with the potential to dramatically change the banking

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are 1.5% higher than in other states in 1981 (the year preceding the policy since reported wages in 1981 are wages earned in 1980). Reported wages are 13.3% higher in Delaware than other states in 1989 (this reflects differences in 1988 wages). I divide 1.133 by 1.015 to get 1.116.

<sup>26</sup>In 1988, housing price growth was 6.4% in Delaware and -.005% in the synthetic control. The ratio of housing prices in Delaware to the synthetic control is now the 1987 ratio multiplied by 1.064/.995, or 106.9%.

industry. The Bank and Trust Company Insurance Powers Act of 1989 allowed state-chartered banks to enter the insurance business and to exercise powers incidental to banking (Nolen and Yemc 2011, Swayze and Schiltz 2005).<sup>27</sup> After the resolution of some policy, legal, and regulatory uncertainty, several banks initiated insurance operations in Delaware in the 1990s.<sup>28</sup> However, a thorough review of newspaper articles and trade journals, as well as a conversation with a corporate attorney involved with this policy, conveyed the response was not large enough to explain the FIRE growth in the 1990s.<sup>29</sup> This suggests the long-run effects were due to either direct policy effects or agglomeration.

Figure 7 shows that depending on the series, FIRE employment increased in Delaware by between 10,000 and 20,000 jobs from 1990 to 2000. I identified that much of this renewed growth can be attributed to the dramatic growth of two firms: MBNA and AIG. The credit card company MBNA grew from 1,000 employees in 1987 to 10,500 in 2006 (Griffith 2006). The insurance firm AIG grew from 150 Delaware employees in the mid-1980s to 2700 Delaware employees in 2001 (Epstein 2001).<sup>30</sup>

MBNA was one of the world's largest credit card companies, and spun off one of the original firms relocating in Delaware after the FCDA. MBNA grew

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<sup>27</sup>Nationally-chartered banks and subsidiaries of bank holding companies had limited power to engage in insurance activities at the time, and only three other states (including South Dakota) broadly allowed banks to underwrite insurance (Schrader 1990). Prior to this legislation, Delaware allowed grandfathered banks to engage in underwriting insurance, as did Utah. In Delaware, these were banks chartered before 1933 (Swayze and Ripsom 1988). Several other states allowed banks to engage in underwriting, but in a much more restrictive fashion (Schrader 1990).

<sup>28</sup>For example, in 1985 the Federal Reserve Board denied Citicorp's application to acquire a South Dakota-chartered bank and engage in insurance activities through that bank, as allowed by South Dakota law (Schrader 1990). The federal FDIC Improvement Act of 1991 prohibited state-chartered banks from underwriting (though not from selling) insurance, except for grandfathered banks (this included Chase Manhattan and Citicorp in Delaware (Talley 1994)). Finally, the US Supreme Court let stand a ruling in 1992 allowing state-chartered banks to engage in insurance (*Bank Ruling* 1992). This ended legal uncertainty regarding whether these grandfathered banks could engage in insurance activities.

<sup>29</sup>At its peak, Citicorp, which was one of the banks most interested in entering insurance, had 200 employees in its insurance group in Delaware (Chuang 2000).

<sup>30</sup>There does not appear to be a change in the tax on insurance companies that is timed with AIG's growth (Delaware Code 2015).



by marketing popular affinity credit cards, which attracted a less-risky pool of consumers. Interestingly, AIG pursued marketing insurance through affinity groups (O Connell 1996), and pioneered the use of direct marketing in the insurance industry (Jackson 1992). The similar marketing techniques used by AIG and MBNA, and the location of AIG’s marketing division in Delaware (Epstein 1999b), suggest technological externalities. In the late 1990s, aided by changes in Delaware and national law, MBNA partnered with AIG to sell insurance (Epstein 1999a), and AIG opened a bank (Epstein 1999b). This also suggests the potential importance of technological externalities, social learning, and thick labor markets, mechanisms yielding agglomeration economies (Glaeser et al., 1992, Moretti 2011).

Tests for agglomeration require controlling for the policy’s direct effects on productivity.<sup>31</sup> Comparing long-run FIRE employment growth in Delaware and South Dakota provides a unique, though informal, strategy to control for these direct effects and test for agglomeration. As discussed earlier, South Dakota was the first state to pass legislation eliminating interest rate ceilings. Several firms moved to South Dakota following this legislation, but Delaware soon followed with a similar policy. As seen in Panel (a) of Figure 7, many more jobs were created in Delaware than South Dakota.

Since both Delaware and South Dakota adopted similar policies, the direct effects should be the same. However, if agglomerative effects increase monotonically with FIRE employment, then the larger initial FIRE employment increase in Delaware should have resulted in greater agglomerative benefits, and created larger long-run FIRE employment growth.<sup>32</sup> If long-run FIRE employment growth in Delaware is no larger than in South Dakota, this does not rule out agglomeration. Agglomerative effects may be present, but may not be any larger in Delaware even though its FIRE sector is larger. This

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<sup>31</sup>One probable direct productivity effect is that employees no longer had to tailor credit card offers to the customers’ state of residence. This effect of *Marquette* is also discussed in Evans and Schmalensee (2005).

<sup>32</sup>If firms in Delaware and South Dakota would grow at the same rate in the absence of agglomeration, then Delaware’s initial advantage in levels would grow over time absent agglomeration.

would provide important insight into the shape of the agglomeration function for the finance sector.

Panel (a) of Figure 7 shows that FIRE employment levels were nearly the same in Delaware and South Dakota in 1980, before either state enacted its policy. This suggests similar pre-policy environments in each state. Starting immediately after Delaware enacted its policy in 1981, FIRE employment growth was much higher in Delaware than in South Dakota. However, by the late 1980s and early 1990s, FIRE employment in both states was experiencing similar growth rates. By the late 1990s, the growth rate was lower in Delaware. Panel (b) of Figure 7 uses the NAICS classification, available from 1990 through 2013. This series also shows faster growth in Delaware in 1993 and 1994, followed by lower growth rates in Delaware.

The larger initial FIRE workforce in Delaware in the late 1980s does not appear to yield higher long-run employment growth rates (even in the years before the Great Recession). While this does not rule out agglomerative effects, and while the strategy is informal, it does provide suggestive evidence that agglomerative effects are no larger in Delaware than South Dakota, despite a larger FIRE sector in Delaware.<sup>33</sup>

## 8 Conclusion

This paper analyzes the short- and long-term impact of an exogenous shock to labor demand, using the relocation of finance companies to Delaware in the early 1980s. Policies aimed at attracting firms to a particular jurisdiction are prevalent. Understanding whether short-run impacts of such policies can be sustained, or whether they disappear as the spatial economy returns to equilibrium, is of critical importance. In particular, there is little evidence on policies targeting white collar jobs.

Using the synthetic control framework, this paper finds the increase in

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<sup>33</sup>Agglomerative effects may increase in FIRE employment over other employment ranges. I also look at differences in wages for workers in banking and credit in Delaware and South Dakota, though the results are imprecise (not shown).

FIRE employment led to several years of higher total employment growth. This was due to lower unemployment rates, higher participation rates, and population growth. There is also evidence of short-run, temporary increases in wages. However, as FIRE growth subsided, employment growth and the participation rate converged to the synthetic control. Continued in-migration led to some convergence in the unemployment rate; adjustment does not appear to have occurred through falling employment.

The local labor demand shock had long-run local effects. Thirty years after the policy, the unemployment rate is still below the synthetic control (though closer than immediately following the policy), and wages were higher through the 1990s. Furthermore, continued in-migration implies a permanent increase in employment levels and thus the tax base.

The implication for policymakers is that large short-run effects from attracting firms cannot be sustained. Smaller long-run effects are possible if the policy has a direct effect, or an indirect agglomerative effect, on worker productivity.

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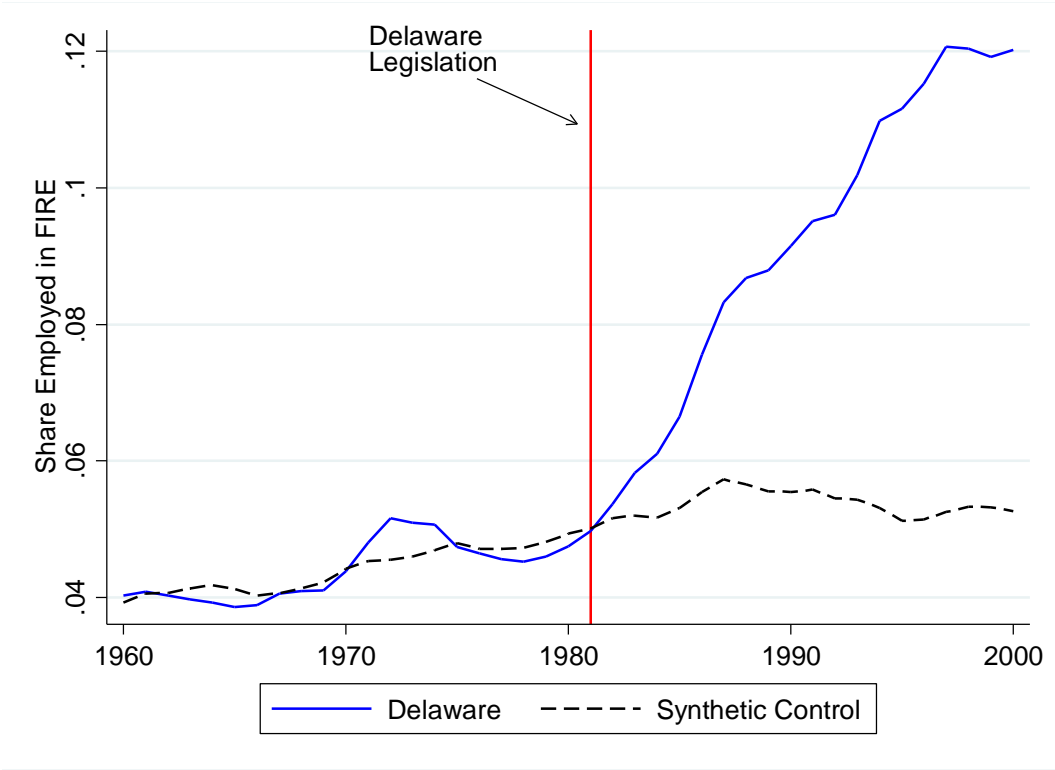
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Figure 1: Policy Impact on FIRE Employment in Delaware

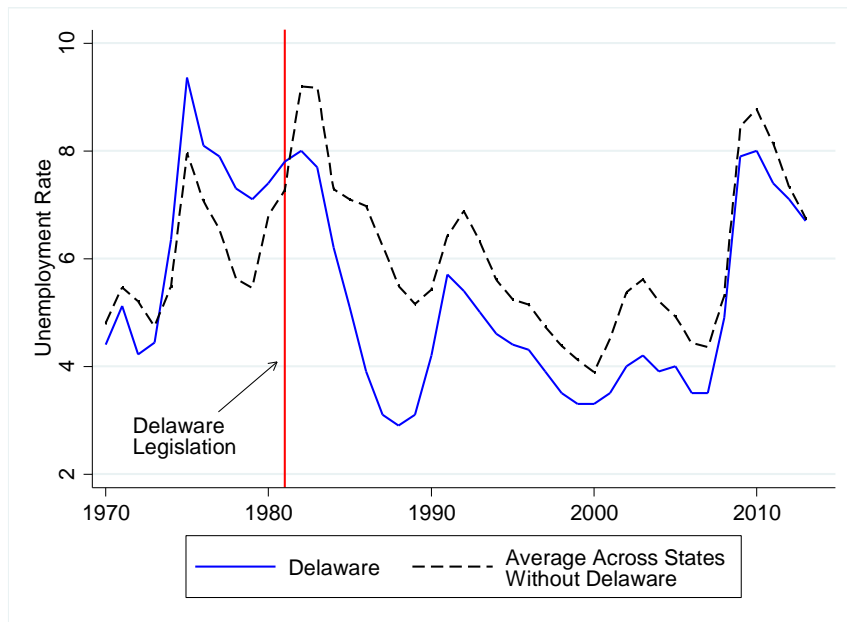


Note: See text for details on construction of the synthetic control.

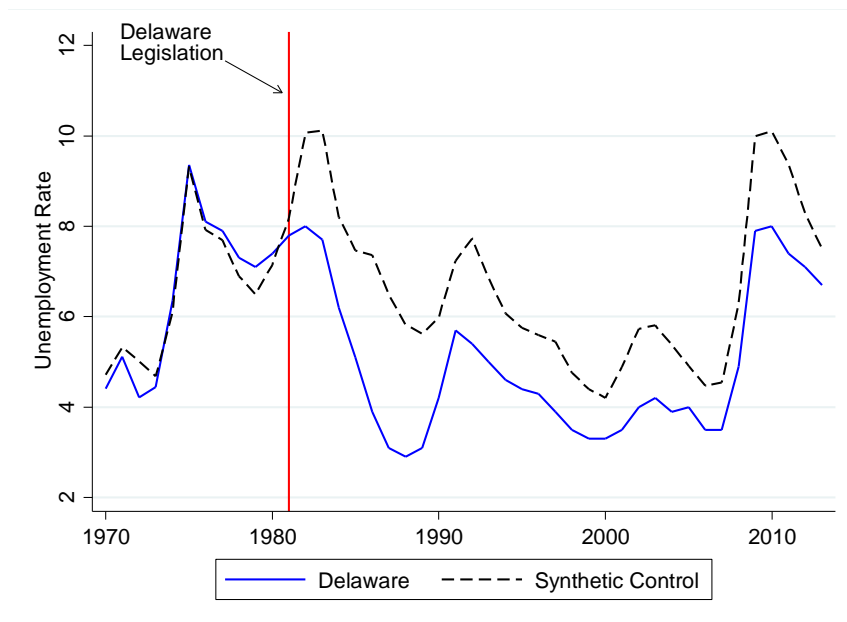


Figure 2: Different Controls for Delaware's Unemployment Rate

(a) Average Unemployment Rate of all Other States and Washington, DC

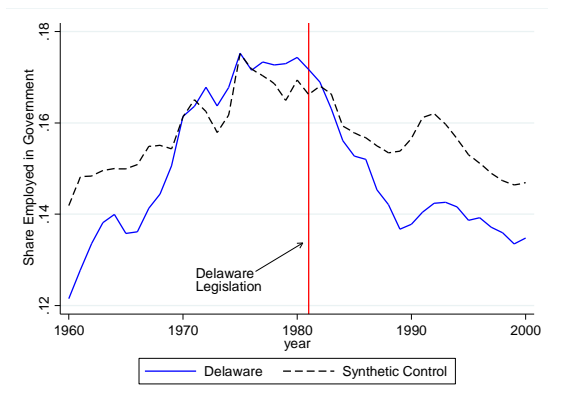
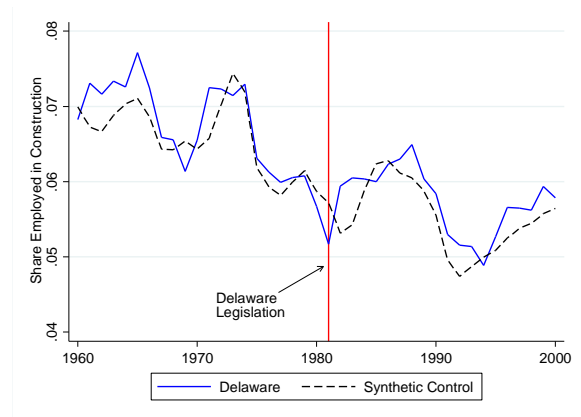
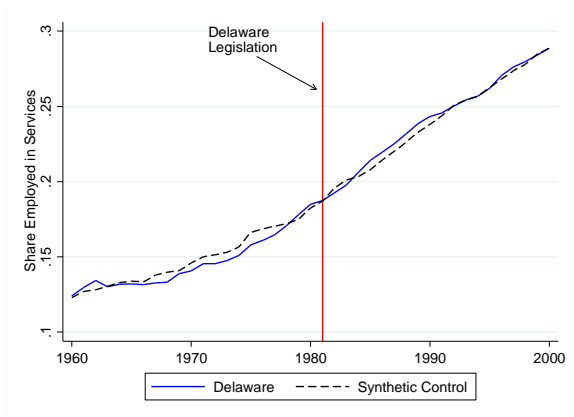
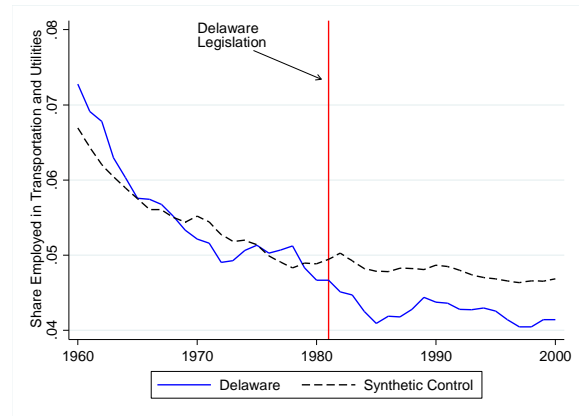
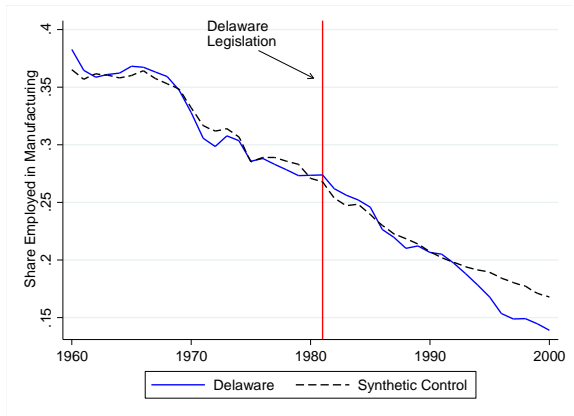
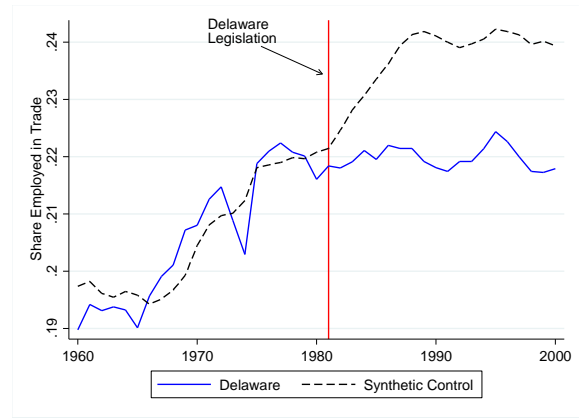
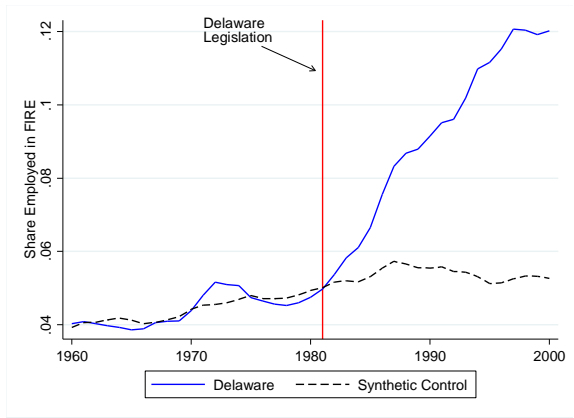


(b) Synthetic Control, with Pre-Policy Unemployment Rates in Each Year as Predictors



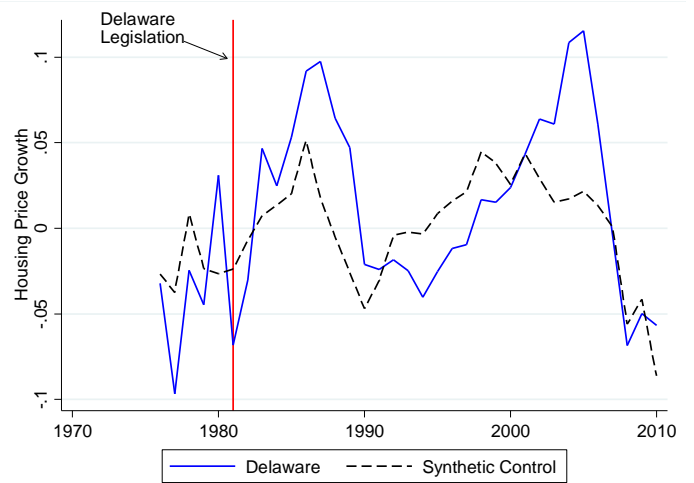
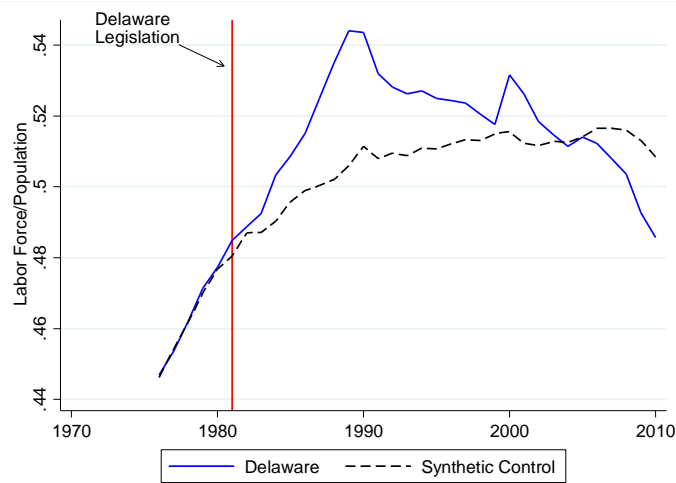
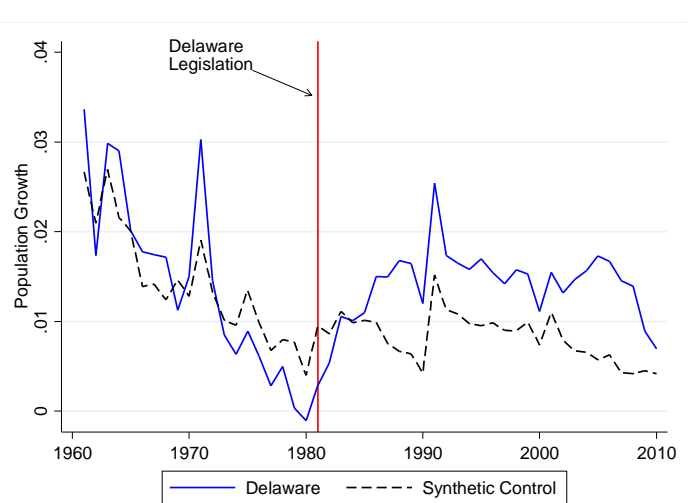
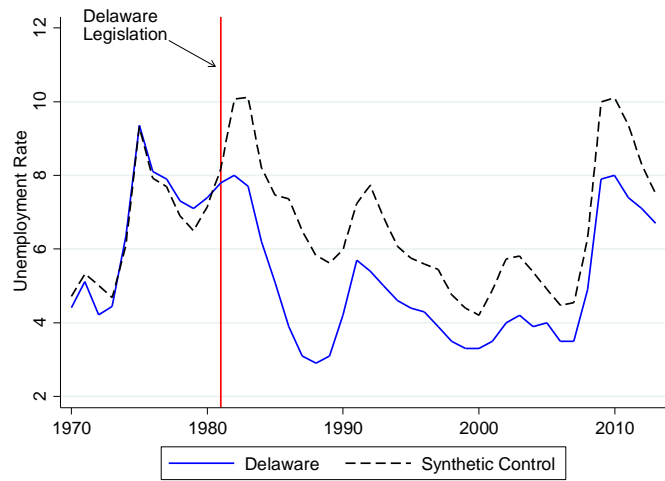
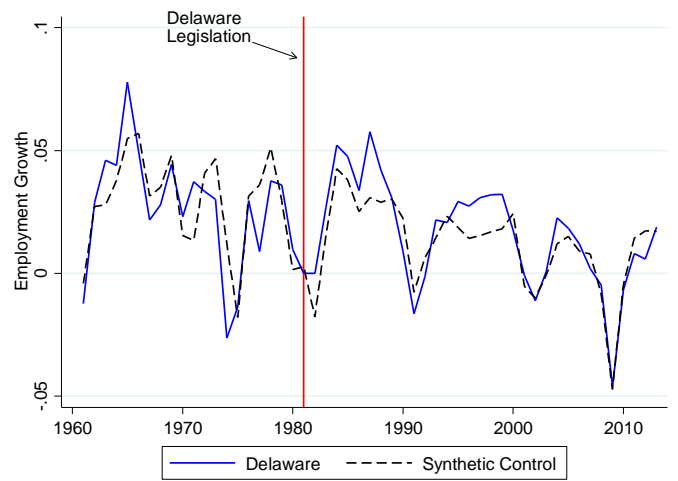
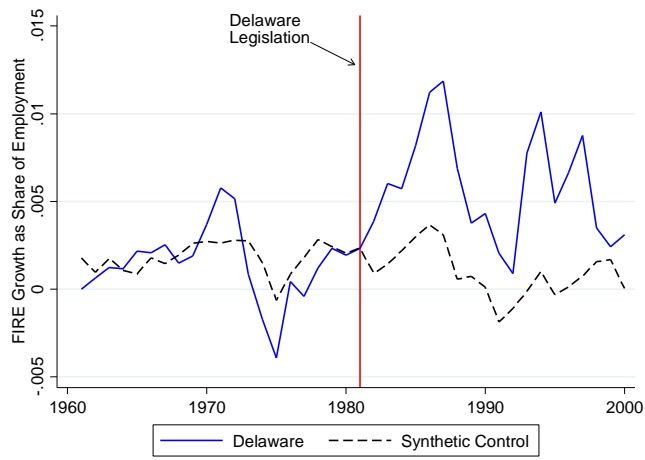
Note: See text for details on construction of the synthetic control.

Figure 3: Policy Impact on Sectoral Composition



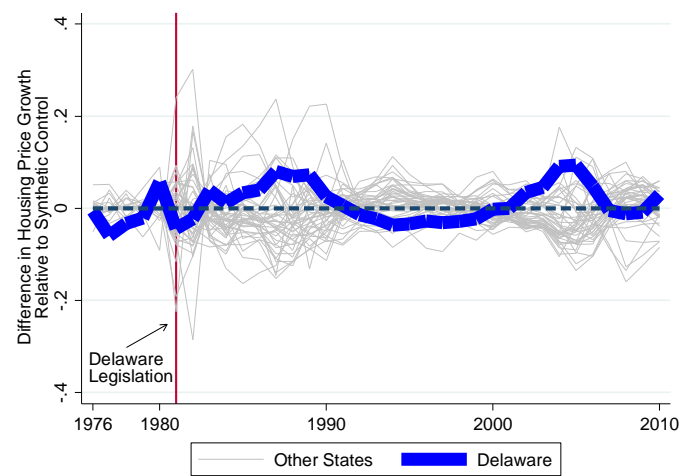
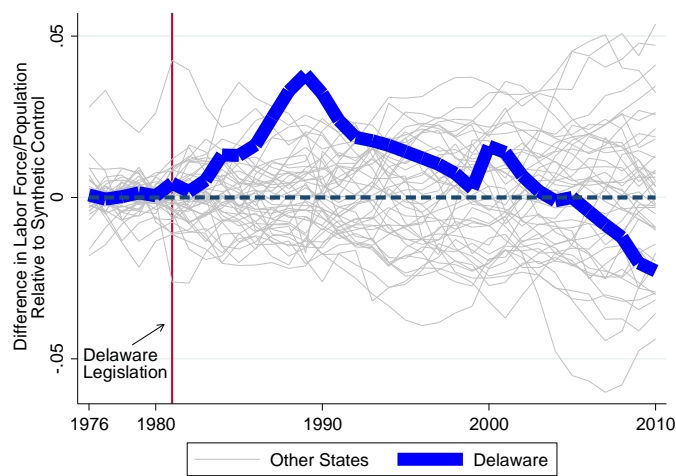
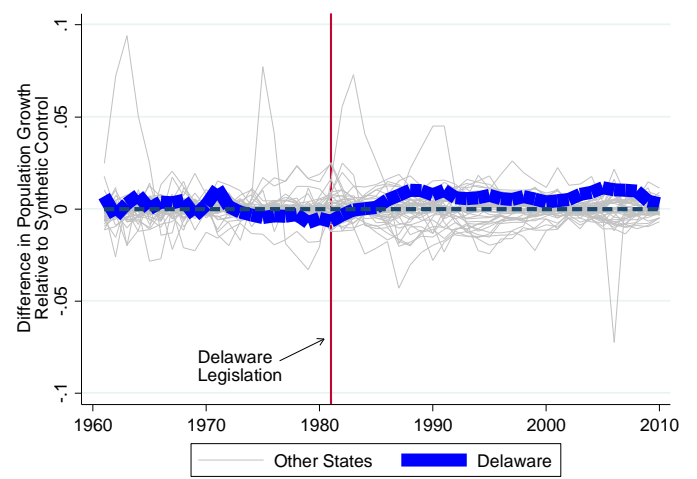
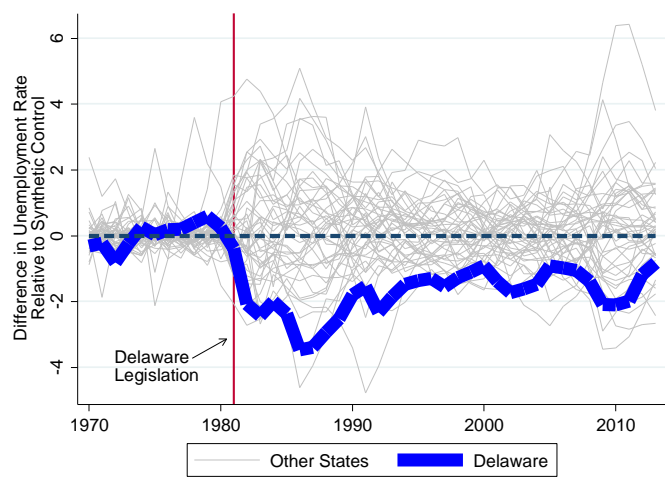
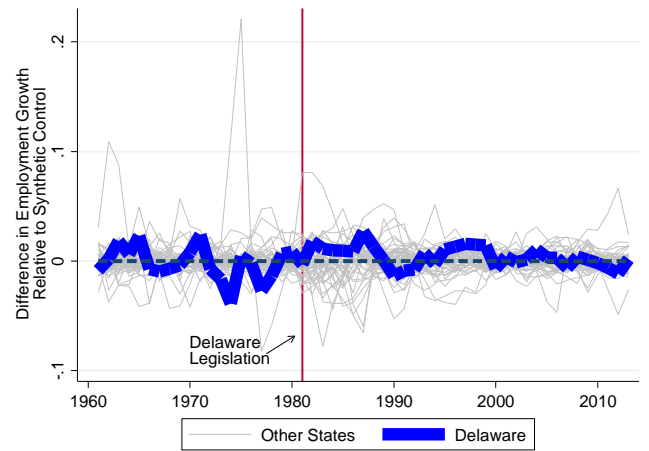
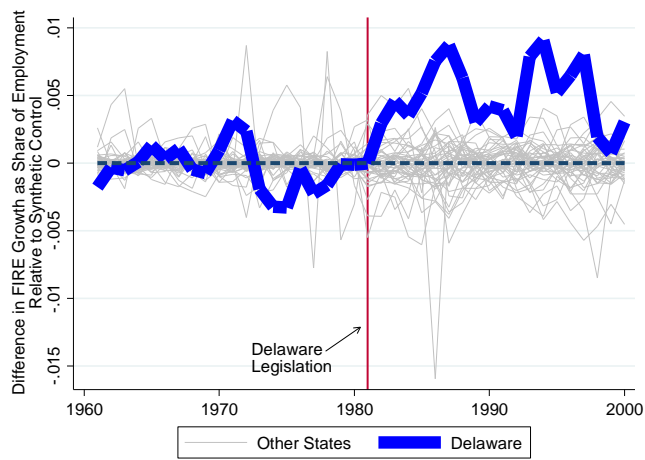
Note: See text for details on construction of the synthetic control

Figure 4: Policy Effects in Delaware Relative to the Synthetic Control



Note: Each plot shows the outcome in Delaware relative to the synthetic control. See paper for details on construction of the synthetic control.

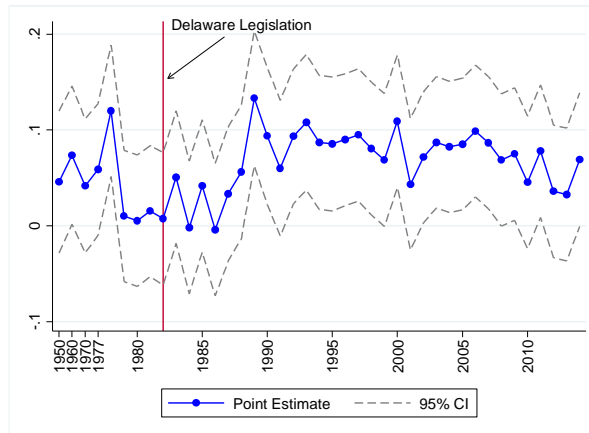
Figure 5: Policy Effects in Delaware Relative to the Synthetic Control, and Placebo Effects for all Control States



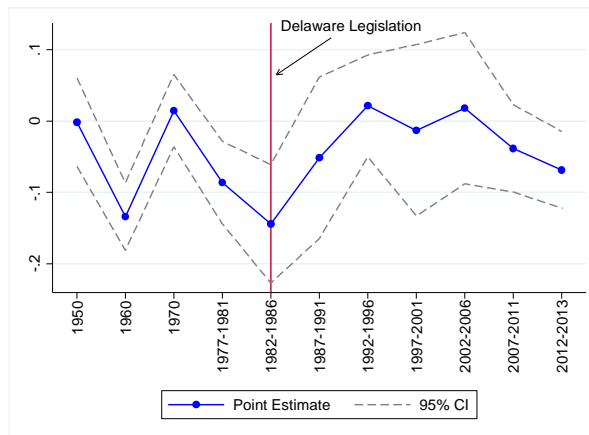
Note: The dark line in each plot shows the difference in the outcome between Delaware and the synthetic control. The gray lines show the difference in the outcome between every other state and their respective synthetic control. See paper for details.

Figure 6: Difference in Log Wages, Delaware Relative to Other States

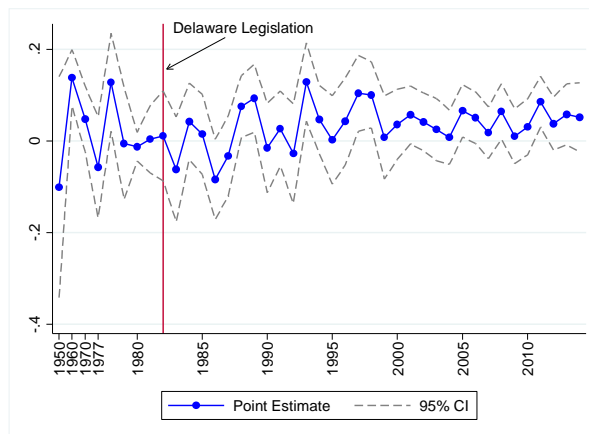
(a) All Occupations and Industries, Controlling for Industry and Occupation Fixed Effects



(b) Banking and Credit Industry



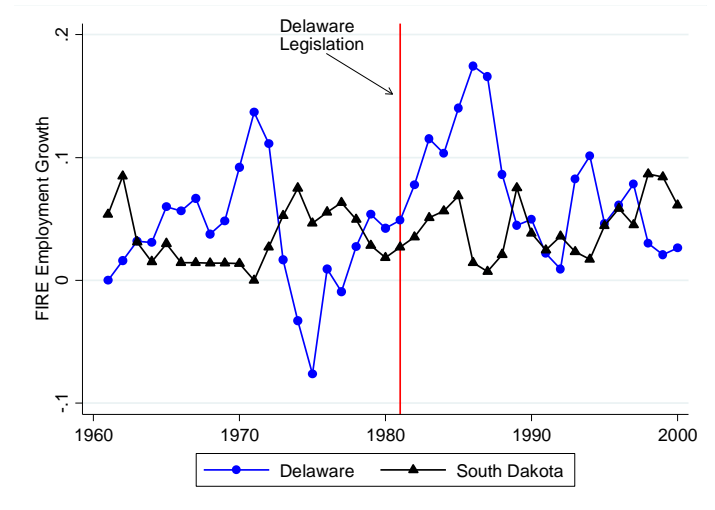
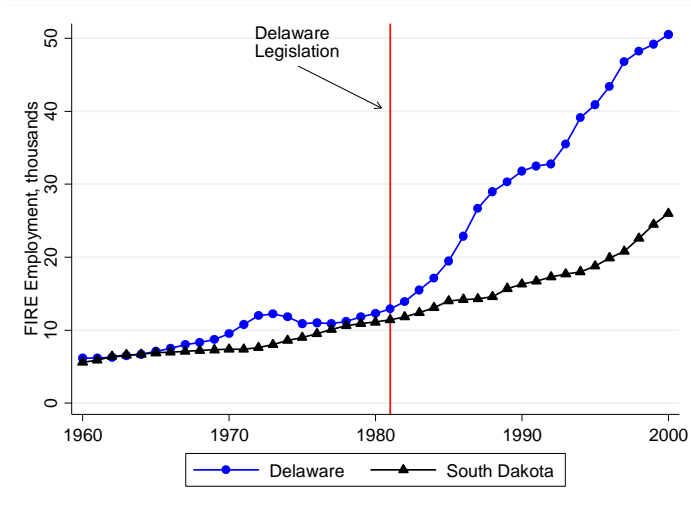
(c) Clerks, Accountants, and Managers



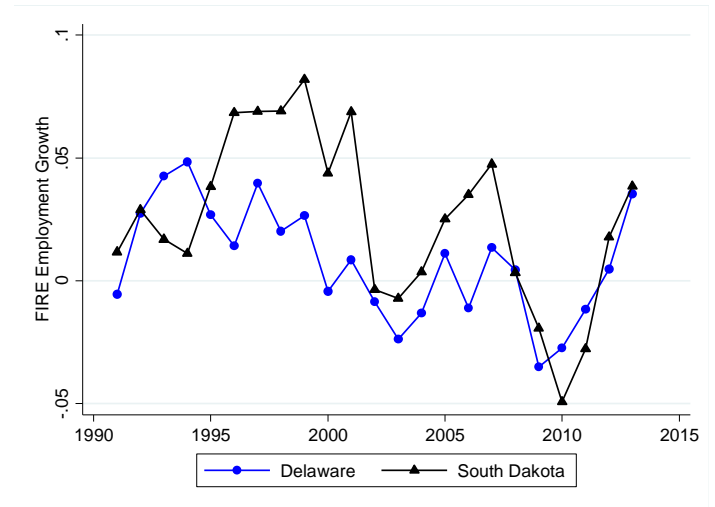
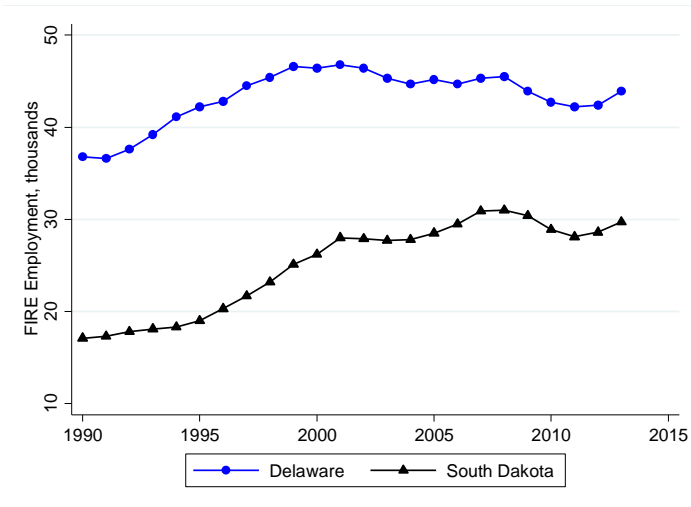
Note: These plots show the coefficient on  $DE \cdot Year$  in the wage regressions. Each includes controls for state characteristics in the pre-policy period, individual characteristics, and occupation and/or industry fixed effects. See text of the paper for further details.

Figure 7: Difference in FIRE Employment between Delaware and South Dakota

(a) 1960-2001



(b) 1990-2013



Note: These plots shows the level, and growth, of FIRE Employment (in thousands) in Delaware and South Dakota. Panel (a) uses the SIC industry classifications, while Panel (b) uses the NAICS industry classifications. See text for details.

Table 1: Composition of Synthetic Control by Outcome

State	FIRE Employment					
	Employment Growth	Growth as a Share of Employment	Unemployment Rate	Population Growth	Labor Force/Population	Housing Price Growth
Alabama	0	0	0.05	0	0	0
Alaska	0	0	0.10	0.09	0.06	0
Arizona	0	0	0	0.05	0.06	0
Arkansas	0	0.02	0	0	0	0
Colorado	0	0	0	0.08	0.06	0
Connecticut	0	0	0	0.31	0.13	0
Washington, DC	0.03	0.20	0.01	0.04	0	0
Florida	0	0.29	0.38	0	0	0
Georgia	0.24	0	0	0	0	1
Hawaii	0.26	0	0	0	0	0
Indiana	0	0	0	0.04	0	0
Louisiana	0.07	0	0	0	0	0
Michigan	0.24	0	0.004	0.36	0.14	0
Nevada	0	0	0	0.03	0	0
New Hampshire	0	0	0	0	0.10	0
New York	0.13	0.05	0.09	0	0	0
Ohio	0	0	0	0	0.31	0
South Carolina	0	0	0	0	0.13	0
Tennessee	0	0	0.37	0	0	0
Vermont	0	0.44	0	0	0	0
Virginia	0.03	0	0	0	0.02	0

Note: Each column denotes the states comprising the synthetic control (and their share of the control) for the given outcome. The procedure used to construct the synthetic control is described in the paper.

Table 2: Balance of Predictors in Delaware and the Synthetic Control: Unemployment Rate

	(1)	(2)	(3)	(4)
		<b>Average of 49 control states and DC</b>		
<b>Predictors</b>	<b>Delaware</b>		<b>Synthetic Control</b>	<b>Synthetic Control</b>
			<b>Every year from 1960 through 1980</b>	<b>Average from 1970-1974, 1975-1979, 1980</b>
<b>Unemployment Rate</b>				
<b>Share Employed in</b>				
<b>FIRE</b>				
1965-1969	0.04	0.04	0.05	0.04
1970-1974	0.05	0.05	0.05	0.05
1975-1979	0.05	0.05	0.06	0.05
<b>Manufacturing</b>				
1965-1969	0.36	0.25	0.24	0.34
1970-1974	0.31	0.23	0.22	0.30
1975-1979	0.28	0.21	0.20	0.27
<b>Trade</b>				
1965-1969	0.20	0.21	0.21	0.20
1970-1974	0.21	0.22	0.22	0.21
1975-1979	0.22	0.22	0.23	0.22
<b>Services</b>				
1965-1969	0.13	0.15	0.16	0.14
1970-1974	0.15	0.17	0.17	0.15
1975-1979	0.17	0.18	0.19	0.17
<b>% Metropolitan</b>				
1960	68.9	47.5	52.9	57.6
1970	70.4	52.5	55.6	64.3
1980	67.0	60.8	73.2	71.2
<b>% with ≥ a HS Diploma</b>				
1960	43.3	41.6	38.6	40.5
1970	54.6	52.9	49.5	51.2
1980	68.6	67.3	63.8	65.7
<b>Unemployment Rate</b>				
1970-1974	4.9	5.2	5.2	5.3
1975-1979	8.0	6.5	7.7	7.2
1980	7.4	6.8	7.2	7.4
<b>Population Growth</b>				
1966-1970	0.02	0.01	0.02	0.01
1976-1980	0.00	0.01	0.02	0.01

Note: This table compares the balance of predictor variables when the outcome is unemployment rate, for Delaware, the average of the 49 other states and Washington, DC, and two different synthetic control specifications. Not all predictors are shown. For the full set of predictors, see text.



Dynamic Responses to Labor Demand Shocks: Evidence  
from the Financial Industry in Delaware  
*Appendix: For Online Publication*

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## Construction of Variables

I calculate potential years of experience from the Census and CPS as  $Age - Education - 6$ , and set this to zero if it is less than zero. I code education as 0 if the *educ* variable from the CPS denoted the respondent received no education, preschool, or kindergarten (CPS code 2). I code education as 4 if the respondent attained grades one through 4 (CPS code 10), six if the respondent attained grades 5 or 6 (CPS code 20), 8 if the respondent attained grades 7 or 8 (CPS code 30), and for grades 9 and 10 I code the education variable as the grade attained. I code the education variable as 11 if the respondent attained 12th grade without a diploma (CPS code 71), and 12 if the respondent attained 12th grade with diploma unclear or diploma/equivalent (CPS codes 72 and 73). I code the education variable as 13 for 1 year of college or some college but no degree (CPS codes 80 and 81). I code the education variable as 14 for 2 years of college or associate's degree, occupational/vocational program, academic program (CPS codes 90, 91, and 92). I code the education variable as 15 for 3 years of college, 16 for 4 years of college or Bachelor's degree (CPS codes 110 and 111), and 18 for anything more than 4 years of college (CPS codes 120 through 125).

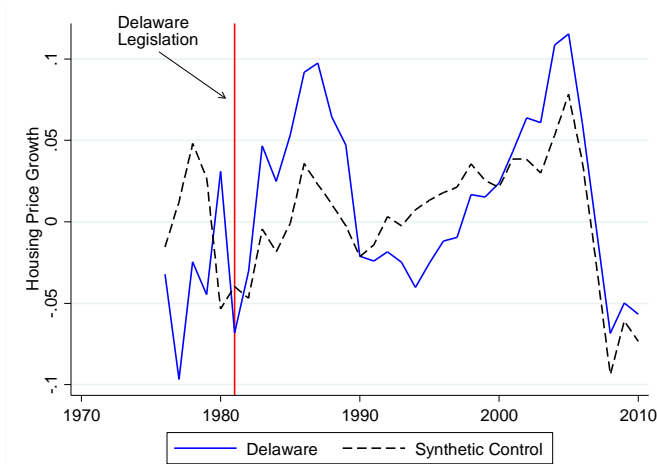
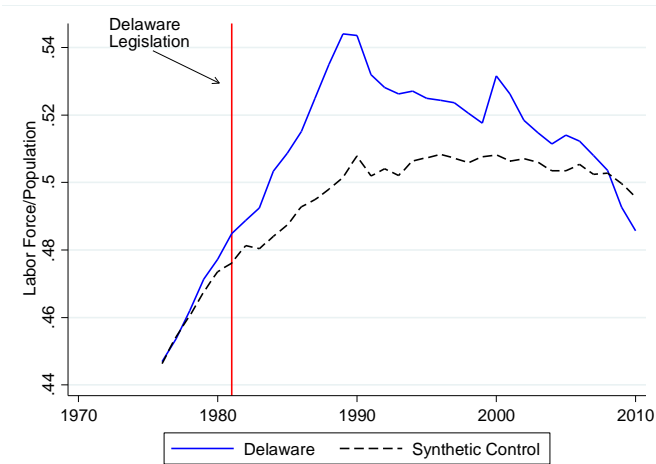
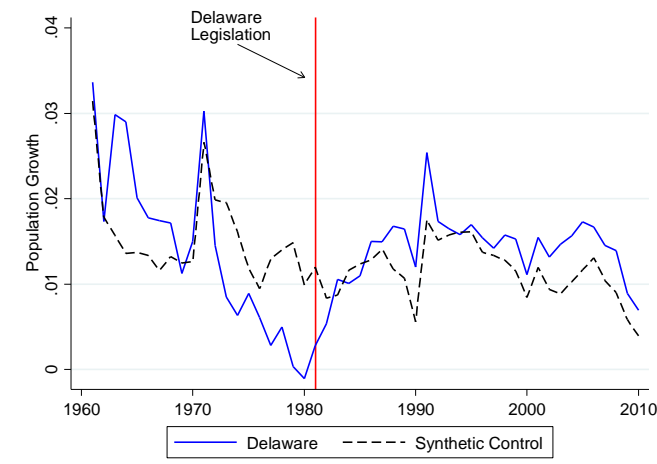
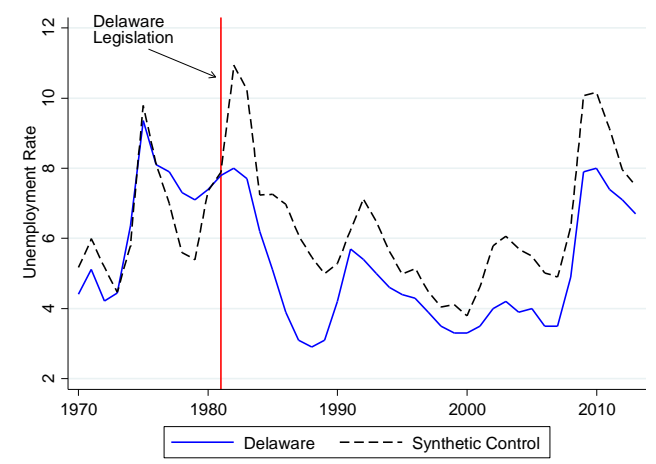
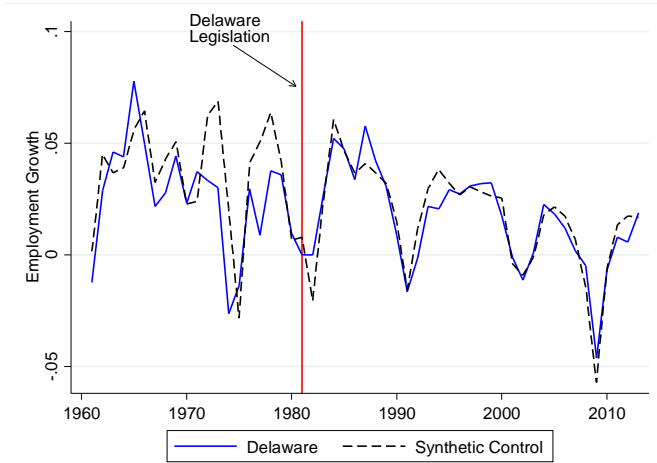
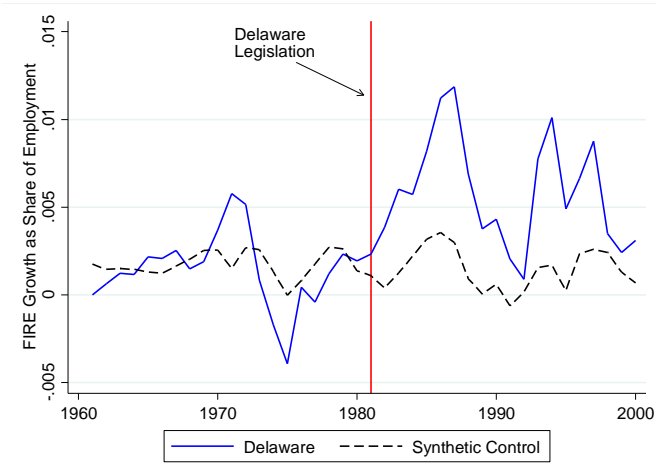
The education codes for the census are slightly different. I code education as 4 if the individual attained nursery school to grade 4 (Census code 1); 8 if grades 5, 6, 7, or 8 (Census code 2); for grades 9 through 12 I code the education variable as the grade attained. I code education as 13 for 1 year of college; 14 for two years of college; 15 for 3 years of college; 16 for 4 years of college; and 17 for 5+ years of college (Census code 11).

I code as married those who respond they are married with spouse absent in addition to those who are married with spouse present.

I code groupings of hours and weeks worked in the CPS to be consistent with the census variable. I include indicators for the following groups of usual hours worked per week last year: 1 through 14 hours, 15 through 29 hours, 30 through 34 hours, 35 through 39 hours, 40 hours, 41 through 49 hours, 49 through 59 hours, and 60 hours. I include indicators for the following groups of weeks worked last year: 1 through 13, 14 through 26; 27 through 39; 40 through 47; 48 through 49; 50

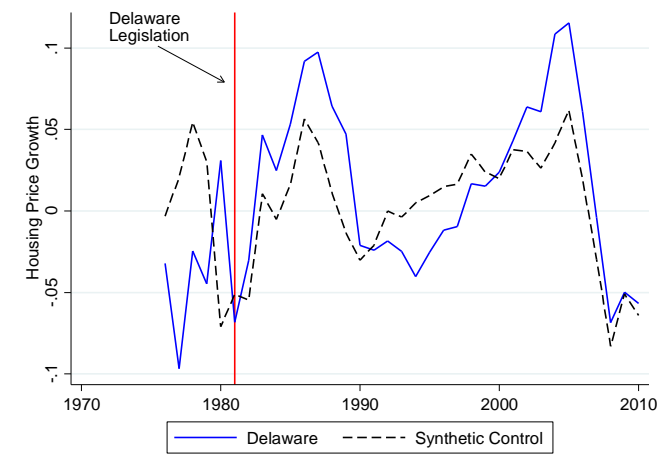
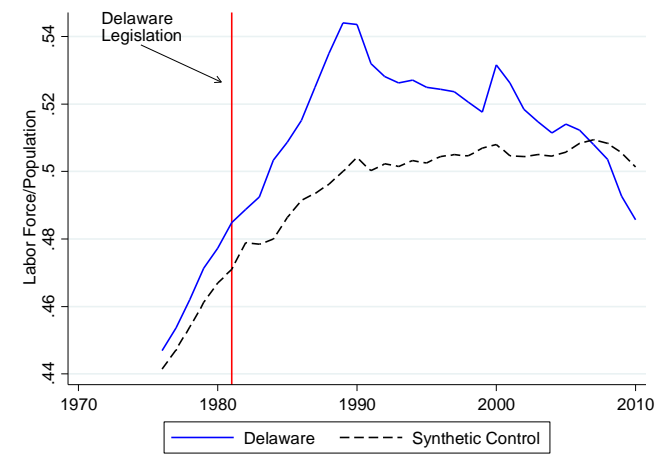
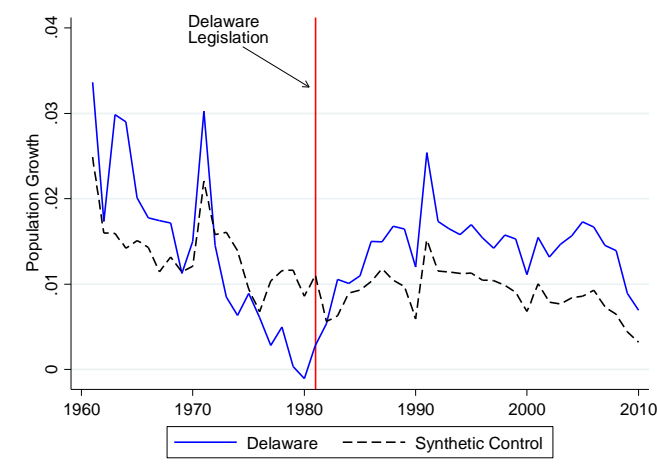
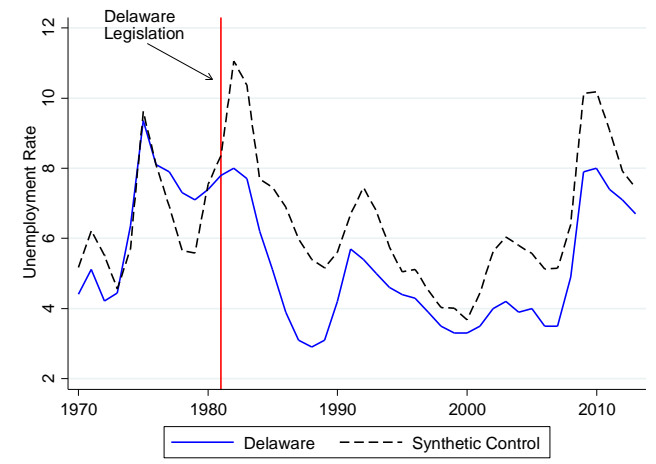
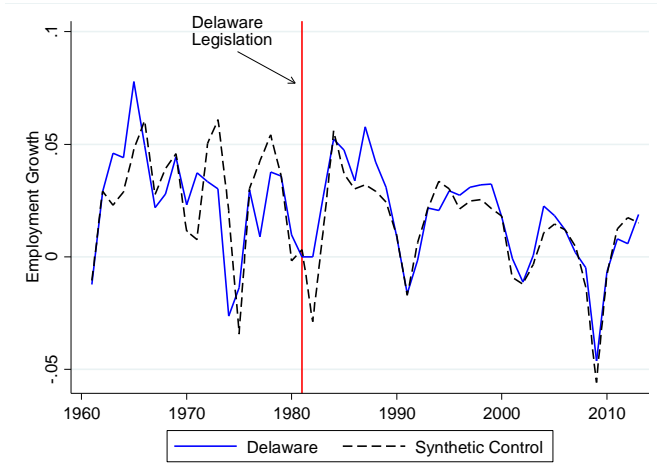
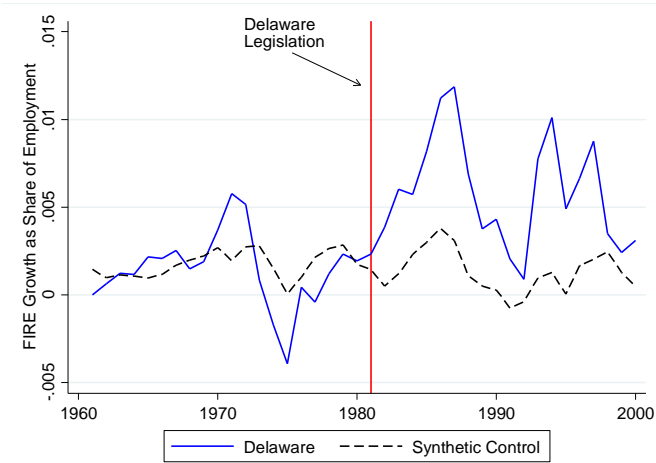
through 52.

Appendix Figure A1: Policy Effects in Delaware Relative to the Synthetic Control, Using Five-Year Averages of Pre-Policy Outcome as Predictors



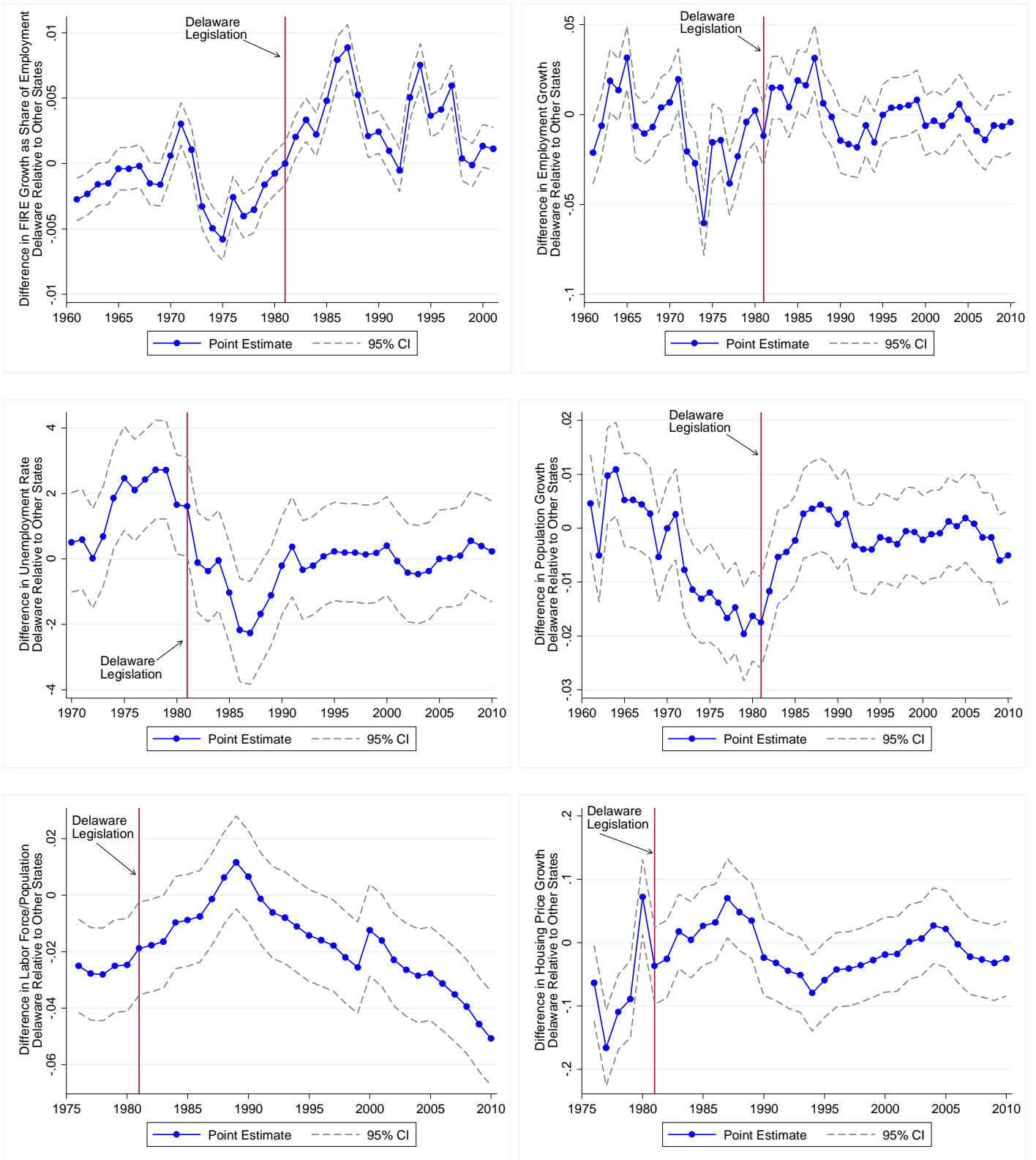
Note: Each plot shows the outcome in Delaware relative to the synthetic control. See paper for details on construction of the synthetic control.

Appendix Figure A2: Policy Effects in Delaware Relative to the Synthetic Control, Using the Synthetic Control from the Share Employed in Manufacturing



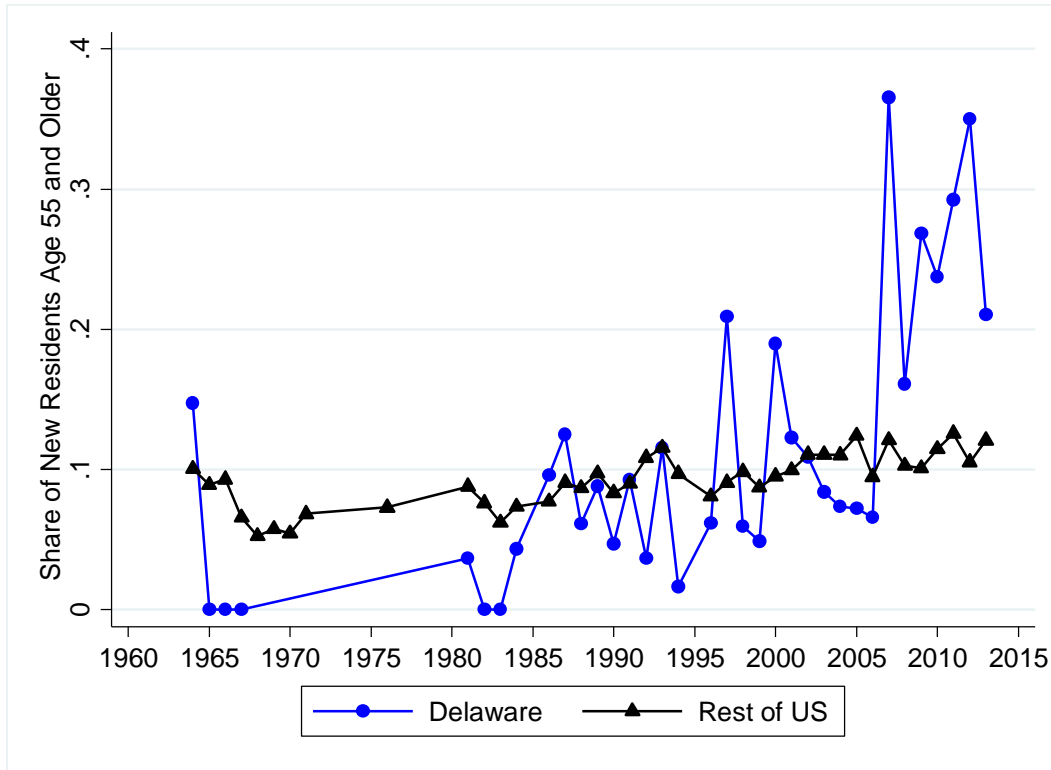
Note: Each plot shows the outcome in Delaware relative to the synthetic control. See paper for details on construction of the synthetic control.

Appendix Figure A3: Regression Estimates of Policy Effects in Delaware, Controlling for Pre-Policy Characteristics



Note: Each plot shows the coefficients on the interactions between Delaware and the year fixed effects, and their 95% confidence intervals. The regression controls for state characteristics in the pre-policy period. See paper for details.

Appendix Figure A4: Share of New Residents 55 and Older



Note: This plot is based on CPS Microdata and compares the weighted share of residents who migrated across states last year who are 55 and older. The data are missing for Delaware from 1968 through 1976, and there were no new Delaware residents in the CPS from 1977 through 1980, or in 1985 and 1995. Sample sizes for Delaware are small, from 1981 through 2013 they range from 33 to 99.

Appendix Table A1: Synthetic Control for Share Employed in Manufacturing

State	Weight in Synthetic Control
Arizona	0.09
Connecticut	0.09
Florida	0.09
Michigan	0.05
New Hampshire	0.06
Ohio	0.47
South Carolina	0.16

Note: This table denotes the states comprising the synthetic control (and their share in the control) when the outcome is the share employed in manufacturing. See text for details.

Appendix Table A2: 1950 Census Bureau Occupational Codes Included as Relevant Managers

200 "Buyers and department heads, store"

201 "Buyers and shippers, farm products"

204 "Credit men"

205 "Floormen and floor managers, store"

210 "Inspectors, public administration"

250 "Officials and administrators (n.e.c.), public administration"

280 "Purchasing agents and buyers (n.e.c.)"

290 "Managers, officials, and proprietors (n.e.c.)"