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ABSTRACT

This paper analyzes an important shock to local labor demand in the financial services sector: firm relocation to Delaware following a Supreme Court ruling and state legislation in the 1980s. Using synthetic controls and bordering states, I find significant effects on employment growth, the unemployment rate, and participation in the first decade. Employment spillovers to the nontradable sector and migration appear larger than estimates from shocks to the tradable sector. Effects persist for 10 to 20 years after Delaware loses its original policy-induced advantage. The shift towards a low unemployment sector explains this persistence, rather than direct productivity effects or agglomeration.

JEL Classification Codes: R10, J20, G20

Key Words: Labor demand shocks, regulatory competition, migration, local labor markets

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

Russell Weinstein†

May 23, 2017

Abstract

This paper analyzes an important shock to local labor demand in the financial services sector: firm relocation to Delaware following a Supreme Court ruling and state legislation in the 1980s. Using synthetic controls and bordering states, I find significant effects on employment growth, the unemployment rate, and participation in the first decade. Employment spillovers to the nontradable sector and migration appear larger than estimates from shocks to the tradable sector. Effects persist for 10 to 20 years after Delaware loses its original policy-induced advantage. The shift towards a low unemployment sector explains this persistence, rather than direct productivity effects or agglomeration.

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1 Introduction

Local governments in the United States are estimated to spend 80 billion dollars per year on incentives to attract or retain companies (Story 2012).[1] Local governments in developing countries, especially Brazil, India, and China, also extensively compete for firms through offering fiscal incentives. While local governments in Europe are currently limited in their power to offer these incentives, this issue has recently arisen in the courts (Markusen and Nesse 2007). Given the large costs and prevalence of these policies around the world, understanding their economic impact is crucial.

The local policy effects depend on whether the policy meaningfully stimulates labor demand, directly affects worker productivity, or yields local spillovers to other industries. The policy effects also depend on how the economy adjusts to the shock. For example, the impact will be affected by whether and how quickly individuals migrate in response (Bartik 1991; Blanchard and Katz 1992). Importantly, the local, long-run impact also depends on whether companies remain in their new jurisdiction or eventually leave for another jurisdiction offering a more attractive package. This is one potentially important distinction between local governments individually offering incentives and a central government offering incentives to locate in a particular region (for example, federal Empowerment Zones in the United States or Regional Selective Assistance [RSA] in Great Britain).[2]

These factors affecting the policy impact may vary by industry. Industries pay different wages and employ people with different characteristics, including different mobility frictions. Spillover effects may also depend on which industry was targeted. Thus, the impact of a policy attracting a manufacturing firm may be quite different from that of a policy attracting a financial services company.

This paper makes two important contributions. First, I study the short-run impact of a well-known policy seeking to create an international center for financial services in one jurisdiction. While previous papers have studied shocks to manufacturing and energy,[3] there is a particular lack of evidence on policies attracting white-collar jobs, and no papers to my knowledge studying policies targeting financial services. These

[1] Carruthers and Lamoreaux (2014) survey the literature on regulatory races.

[2] Devereux, Griffith, and Simpson (2007) and Criscuolo et al. (2012) analyze the RSA policy. Busso, Gregory, and Kline (2013) study Empowerment Zones. See Neumark and Simpson (2015) for a review of studies analyzing place-based policies.

[3] This literature is reviewed at the end of the section.

are 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).[4]

I present estimates, the first of which I am aware, of local multiplier effects from a nontradable sector to other nontradable sectors, and more specifically from finance to other nontradable sectors. I then compare these magnitudes to recent estimates of local multiplier effects from tradable to nontradable sectors (Moretti 2010; Moretti and Wilson 2013).[5] These comparisons are relevant for local policymakers deciding which sectors to target. By focusing on a shock to financial services, the results uniquely contribute to policy and academic discussion of how the finance sector benefits society (Zingales 2015). I also apply the relatively new, though increasingly used, synthetic control method to the local labor-market literature.

Second, I study a unique setting in which a short-run policy-induced advantage weakens over time. Given significant competition between jurisdictions, it is necessary to understand the robustness of local policies to future competition. If firms remain in the jurisdiction even after the jurisdiction's policy-induced advantage disappears, this may suggest agglomerative effects or high fixed costs of relocation.

I study the dynamic effects of an exogenous increase in local labor demand affecting the finance sector–an increase resulting from a landmark United States Supreme Court decision. In 1978, the U.S. 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).

Marquette implied that if one state eliminated its usury laws, banks could relocate to that state and charge unlimited interest to customers around the country. South Dakota eliminated its usury laws in 1980. Delaware followed in 1981 with the Financial Center Development Act (FCDA), which also introduced a regressive tax for banks. Likely because of its proximity to New York and its regressive tax, many

[4]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 as having received more than $100 million dollars in state grants from 2007 to 2012, Prudential was ranked eleventh and Royal Bank of Scotland thirty-ninth (Story, Fehr, and Watkins 2012).

[5]As in Moretti (2010), I use tradable employment to refer to manufacturing and nontradable to refer to other industries, excluding agriculture, mining, government, and the military.

more banks and credit card companies opened subsidiaries in Delaware than in South Dakota.

Within just a few years, other states–including Delaware's neighbors–responded with similar policies eliminating or increasing the limit on interest rates. As more states with low taxes passed these policies, Delaware's tax advantage weakened too. Within 10 years of Delaware enacting its policy, the original policy-induced advantage was eliminated.

The *Marquette* decision effectively deregulated the bank credit-card market in the United States. Given its importance, an existing literature studies its impact on credit-card interest rates, profits, consumer finance, and entrepreneurship (Ausubel 1991; Chatterji and Seamans 2012; Knittel and Stango 2003; Zinman 2003). Similarly, Delaware's legislative action is well known for its impact on bank relocation (Evans and Schmalensee 2005). However, this is among the first papers to study the exogenous increase in local labor demand following *Marquette*, which created an important center for financial services in Delaware.[6]

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 the states bordering Delaware as a counterfactual, as well as synthetic control methods. The latter create a weighted composition of states that approximate what Delaware's economy would have been had the policy not been implemented (Abadie, Diamond, and Hainmueller 2010, 2014). The relative value of these methods depends on whether prepolicy predictor variables, or geographic proximities, are a better predictor of postpolicy outcomes. Where these two methods differ, they can be seen as informally bounding the effects.

I construct a data set from 1960 to 2013 using the Current Employment Statistics (CES), Local Area Unemployment Statistics (LAUS), the Federal Housing Finance Agency (FHFA) Index, CPS microdata, and various data from the U.S. census. Because the wage data are at the individual level, the synthetic control method employed

[6]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 evidence of productivity effects, direct or indirect through agglomeration. Weinstein (2015) studies whether this sector-specific increase in local labor demand affects choice of college major.

for the state-level data is not appropriate. Instead, I estimate regressions controlling for state characteristics in the prepolicy period.

The policy had large effects within the first seven years. Total employment growth increased approximately 1.5 percentage points per year relative to the controls in this period, suggesting new jobs are not all filled by substitution across sector. Of this increase, approximately 0.5 percentage points per year are due to Finance, Insurance, and Real Estate (FIRE); transportation/utilities and construction appear to have grown also. There is an initial migration response; population growth is 0.6 percentage points higher per year than in the synthetic control. The unemployment rate falls by an average of 2.3 percentage points below the synthetic control, and the participation rate increases by an average of 1.3 percentage points relative to the controls. I cannot rule out that this is partly driven by workers leaving unemployment or reentering the labor force. While wages are not higher in this first period, there is a one-year wage effect, consistent with increases in labor supply.

By the end of the first postpolicy decade, these effects persisted at similar magnitudes, and the effects on population growth and participation rate were larger. By 1992, each FIRE job created from 1980 to 1987 is associated with an additional three jobs in FIRE (from 1987 to 1992), trade, transportation/utilities, and construction. Delaware wages are 8 percent higher than in other states, whereas just before the policy they were equivalent, controlling for covariates. In the banking and credit industry, Delaware wages are now 7 percent higher than in other states, whereas before the policy they were 7 percent lower.

Evaluating the policy's long-run impact requires the stronger assumption that any long-run difference-in-difference derives from the initial shock. With this caveat, the results suggest Delaware's original advantage provided long-run effects, even after other states had competed it away. Ten to twenty years after the policy, when Delaware's policy-induced advantage had arguably disappeared, the policy's effects strongly persist. Local multipliers suggest that for every FIRE job created from 1980 to 1987, by 2000 there were an additional 6.8 jobs in FIRE (from 1987 to 2000), trade, transportation/utilities, and construction. Excluding the spillover FIRE jobs, this multiplier is 4.1.

Twenty to thirty years after the policy, total employment growth appears to have slowed. However, the lower unemployment rate, higher population growth, and higher wages persist even 30 years after the policy, though with some convergence toward

the control. This persistence differs dramatically from that found in Blanchard and Katz (1992).

I test whether these long-run effects on the unemployment rate are explained by the increased share of workers employed in finance, a sector with higher wages and lower unemployment. This is also an informal test of whether agglomerative effects in Delaware are stronger than in finance nationally. Specifically, I compare the unemployment rate in Delaware to the predicted unemployment rate based on national sectoral unemployment rates from the Bureau of Labor Statistics (BLS). By approximately 15 years after the shock, I find that the persistent effect on the unemployment rate is almost all explained by the shift in sectoral composition. This suggests that while the policy had positive effects in Delaware, it was inefficient on the aggregate level.

The effects of this local financial services shock differ from the effects of policies not specifically targeting white-collar industries.[7] First, the local multipliers from finance to nontradable employment are larger than recent estimates from tradable to nontradable employment (Moretti 2010), though smaller than the very short-run local multiplier from biotech to nontradable employment (Moretti and Wilson 2013).

Second, the migration response is larger and more significant than two other important local development policies: 1) the Tennessee Valley Authority (TVA) targeting manufacturing and agriculture (Kline and Moretti 2014) and 2) federal urban Empowerment Zones (EZs), which did not target specific industries (Busso, Gregory, and Kline 2013). This is consistent with higher education levels of finance employees, which causes higher geographically mobility (Malamud and Wozniak 2012). Changes in the number and types of workers in the jurisdiction have important implications for incidence of the policy, but also for the industries that may experience spillovers.

Third, the finance shock's effects in Delaware appear more robust than those of the TVA to a weakening policy-induced advantage, though the time series for the TVA study is considerably longer (40 years after a weakened advantage relative to 20).[8] This may suggest stronger agglomeration economies from finance, or it may suggest

[7] 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).

[8] After federal subsidies decreased, TVA regions experienced no population growth, reversal in agriculture employment growth, and positive though much reduced growth in manufacturing (with smaller magnitudes than the continued finance growth in Delaware) (Kline and Moretti 2014).

larger moving costs for finance firms. Alternatively, this could simply reflect the value of targeting an industry that performs well in the long run, and manufacturing did not. Finally, the wage effects in Delaware also contrast with lack of wage growth from the TVA, though there are wage effects from EZs.

The results have important policy implications: local policies can successfully incentivize firms to relocate, producing significant local wage and employment effects, and these effects can be sustained in the longer run. However, this is conditional on the policy shifting the economy's composition toward sectors with low unemployment and high wages in the long run. It is especially notable that this shift was lasting in Delaware, despite other states later passing similar legislation.

2 Exogenous Shift in Labor Demand in Delaware: A Temporary Policy-Induced Advantage

Prior to 1978, state usury laws determined the interest rate that credit card companies could charge residents of the state.[9] The U.S. 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 losses in their credit card divisions because of high interest rates, coupled with ceilings on the interest rates they could charge. After *Marquette*, banks were eager to find a state that would allow them to charge higher interest rates nationwide.

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 industries.[10] After *Marquette*, the state recognized the opportunity to attract the finance industry. In 1981, Delaware eliminated its usury laws by passing the 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.[11]

[9] The description of the FCDA in this section is based on Moulton (1983).

[10] 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).

[11] There were capitalization and employment requirements for these FCDA banks. In the subsequent years, Delaware passed two other pieces of legislation aimed at helping smaller banks and

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 until 1991 (South Dakota Session Laws 1979, 1991).[12] As a result, many companies moved their finance or credit operations to Delaware, starting with J.P. Morgan in 1981. By 1987, 27 banks and nonbanks had been opened or been acquired through the FCDA (Figure 1, Panel A).[13] Eighteen were focused in part on consumer credit and credit/debit cards, while the remainder generally focused on wholesale banking. Of the 27 banks, 12 were from New York, and these were focused generally on wholesale banking rather than credit cards. By 1987, four banks had moved from Pennsylvania, four from Maryland, and three from Illinois. Figure 2 shows that around the time of the policy there were clear increases in the share of Delaware's employment in FIRE.

The tax and interest-rate advantages the FCDA conferred upon Delaware were ultimately only temporary. After the policy, Delaware remained an attractive place to relocate for banks from New York, Maryland, and Pennsylvania, despite these states responding with similar legislation. The taxes on banks remained lower in Delaware, and the interest-rate ceiling higher when compared to Maryland or Pennsylvania.[14] However, after Delaware's legislation, many other states responded similarly by eliminating interest-rate ceilings, and also offered low taxes on financial institutions. By

international banks to take advantage of the FCDA provisions. These had smaller initial impacts than the FCDA (Erdevig 1988). The effects in this paper can be seen as the combined effects of the FCDA and these additional laws extending the FCDA provisions. 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.

[12]From 1979 until 1991, South Dakota imposed a tax of 6 percent on the net income of financial institutions (South Dakota Session Laws 1979). Delaware's tax was 8.7 percent on the first $20 million of net income, 6.7 percent on net income from $20 to $25 million, 4.7 percent on net income from $25 to $30 million, and 2.7 percent 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).

[13]The source for the description of the banks and nonbanks opening through the FCDA through 1987 is Swayze and Ripsom (1988).

[14]See appendix for a full description of the resulting competition between states. New York passed a law in 1981 eliminating its usury laws and allowing companies to charge fees, but did not significantly reduce its high taxes on banks (Rubin 2011). In 1983, Virginia eliminated interest-rate ceilings on credit card loans, allowed unlimited annual fees, and invited out-of-state bank holding companies to acquire a bank. By 1983, Maryland raised but did not eliminate the interest-rate ceiling and allowed credit card fees, but the bank tax rate of 7 percent was not changed (Maryland Session Laws 1968; Michie's Annotated Code 2004). In 1982, Pennsylvania raised but did not eliminate the interest-rate ceiling, allowed some bank fees (Erdevig 1988), and reduced taxes on banks, though they were not lower than in Delaware (Pennsyvlania Department of Revenue 2008, 2015).

1982, most states with large banking sectors had relaxed or repealed their interest-rate ceilings (Ausubel 1981). By 1985, 15 states had eliminated the ceiling on credit card interest rates (Chatterji and Seamans 2012).[15]

These changes implied that banks from New York, Maryland, and Pennsylvania now had many other options for relocation besides Delaware. In addition, the costs of remaining in their own state were lower, given relaxation in interest-rate ceilings. Most directly, Delaware's advantage had disappeared by 1991, when South Dakota introduced a regressive tax rate for financial institutions, with a lower top rate and a lower bottom rate than Delaware (South Dakota Session Laws 1991). Consistent with the policy advantage being only temporary, Figure 1, Panel B, shows that the number of new banks remained relatively flat in Delaware during the early 1990s, some 10 years after the policy went into effect. Furthermore, starting in 1990, banks that had moved to Delaware in the early 1980s began to leave the state. By the mid-1990s, seven such banks had left.

This suggests that after 1991, banks and credit card companies that were relocating or adding jobs in Delaware were not doing so because Delaware still offered a policy advantage. Thus, policy effects 30 years after the policy's enactment can be interpreted as long-run effects of the policy, rather than continual short-run effects from a policy advantage. The erosion of Delaware's original advantage uniquely allows me to study whether there are long-run benefits to an initial advantage after that policy-induced advantage has disappeared.

3 Data

I obtain annual data from 1960 through 2000 on nonfarm employment by state and SIC industry from the BLS Current Employment Statistics (CES). Since the Standard Industrial Classification (SIC)-basis estimates are only available until 2001, to obtain a longer time series for total employment I use total employment from the CES, North American Industry Classification System (NAICS) basis. These data are

[15]Some of these states may not have formally invited out-of-state bank holding companies as Delaware had, implying that relocation there would be more difficult. This also was changing during the 1980s, and by 1990, 46 states allowed out-of-state bank holding companies to acquire in-state banks in certain circumstances. Furthermore, the Riegle Neal Banking Act of 1994 implied that this was no longer a necessary requirement (Medley 1994).

available until 2013.[16] When constructing shares of total employment by industry, the denominator is total employment, SIC basis.[17] 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.[18] 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 U.S. census, available through 2010. 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 U.S. censuses in 1960, 1970, and 1980: percentage with at least a high school diploma, percentage of the population aged 15 to 64, and percentage living in metropolitan areas.

I obtain data on housing prices from the Federal Housing Finance Agency's 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, and 1980 censuses and the 1977–2014 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 Empirical Methods

4.1 Dynamic Response of Employment-Related Variables

The "treatment" effect of this policy in year t is $Y_{DE,t} - Y^N_{DE,t}$, where $Y^N_{DE,t}$ is the outcome in Delaware if the policy had not been implemented. Clearly, $Y^N_{DE,t}$ is not observed, but instead must be approximated by a control representing Delaware absent the policy. There are several possible ways in which to construct the control group. Perhaps most obviously, I could estimate a differences-in-differences model using bordering states as a control group. This strategy is appropriate if, absent the policy, Delaware would not have experienced any differential shock in the postpolicy

[16] Unlike the NAICS-basis data for total employment, NAICS-basis data by industry are only available starting in 1990.

[17] As a result, I measure employment by industry as a share of total nonfarm employment.

[18] These data were provided by Larry Katz, and were used in Blanchard and Katz (1992).

years relative to these states. While this seems reasonable, it is not obvious that bordering states are the best control, and the choice is somewhat arbitrary. In addition, potentially negative policy effects in bordering states would imply this strategy may double count the policy's impact.

A second possibility is to use the data to identify states that appear similar before the policy, using the synthetic control method (Abadie, Diamond, and Hainmuller 2010, 2014). This method involves constructing a weighted combination of states so that the outcome predictor variables match those in Delaware before the policy. The assumption is that if the prepolicy trends appear similar in these states, their postpolicy trends should have been similar in the absence of the policy.

I present results using both the bordering states and the synthetic control as a counterfactual Delaware. While in many cases they yield similar results, there are important dissimilarities, which I discuss. In cases where there are differences, the two methods can be seen as informally bounding the effects.

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.

Synthetic Control

I construct a synthetic control with similar sectoral, economic, and demographic characteristics as Delaware in the prepolicy period. Specifically, I include as predictors five-year averages of the following variables in the prepolicy period: share of employment in construction, FIRE, manufacturing, trade, services, transportation and utilities, and government, as well as the unemployment rate, labor force participation rate, housing price growth, employment growth, and population growth.[19] I also include as predictors the 1960, 1970, and 1980 census values for the percentage living in metropolitan areas, percentage of the population 15 to 64, and percentage with at least a high school diploma. By matching Delaware to a control with similar five-year averages of these variables, often starting in 1960 and going through 1980, I capture

[19] 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, and 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 employment growth, and the average from 1976 through 1980 of labor force over population.

not only prepolicy levels but prepolicy trends.

By assigning equal weight to each of these predictors, I hold constant the composition of the synthetic control across outcomes. For robustness, I allow the weight on the predictors to vary with each outcome, and I estimate a separate synthetic control for each outcome variable. I include each state and Washington, D.C., as potential components of the synthetic control.[20] Robustness specifications address the concern that the policy negatively affected certain control states.

Differences-in-Differences Using Synthetic Control Estimates. Following Bohn, Lofstrom, and Raphael (2014), I obtain differences-in-differences estimates by comparing Delaware to the synthetic control in the period before the policy and in each period after the policy. Specifically,

$$DD_{DE,t} = (Outcome_t^{DE} - Outcome_t^{synth}) - (Outcome_{pre}^{DE} - Outcome_{pre}^{synth}). \quad (1)$$

I define the preperiod as the five years immediately preceding the policy. For ease of presentation, I divide years into the following groups: pre-1976, 1976–1980, 1981–1987, 1988–1992, 1993–2000, and post-2000. As will be evident, these groups were chosen to match the different stages of economic adjustment. Specifically, I estimate

$$DiffY_{DE,t} = \alpha_0 + \beta_g YearGroup\_g_t + u_{st}. \quad (2)$$

The variable $DiffY_{DE,t}$ is the difference in outcome Y between Delaware and the synthetic control in year t. The variable $YearGroup\_g_t$ equals one if year t is in year group g, and zero otherwise. The omitted year group consists of the years immediately preceding the policy, 1976 through 1980.

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 is the treated state. For each state, I construct a synthetic control using the principal synthetic control specification.[21] As in Bohn, Lofstrom, and Raphael (2014), I obtain the

[20] I exclude Oregon since it is missing data in some prepolicy years.

[21] I do not allow Delaware to be in the synthetic control of the placebo treatment states, because of the large policy effects in Delaware.

differences-in-differences estimates for each of these placebo states. 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. More formally, following Bohn, Lofstrom, and Raphael (2014), the placebo differences-in-differences can be interpreted as the sampling distribution for the estimate $DD_{DE,t}$. If the cumulative density function of all the differences-in-differences estimates is $F(.)$, then the p-value of the one-tailed test that $DD_{DE,t} > 0$ is $1 - F(DD_{DE,t})$.

Differences-in-Differences Relative to Bordering States. I estimate the following specification, including Delaware and bordering states (Maryland, New Jersey, and Pennsylvania):

$$\begin{aligned} Y_{st} &= \alpha_0 + \sum_{g=1}^{5} (\beta_g YearGroup\_g_t + \gamma_g YearGroup\_g_t * DE_s) \\ &\quad + \delta StateDE_s + u_{st}. \end{aligned} \tag{3}$$

4.2 Spillover Employment Effects

Spillover employment effects may operate with a lag. I compare sector-level employment growth over several multiyear periods in Delaware to that of the synthetic control and compare these with prepolicy differences. For $(t, t') = (1980, 1976)$, (1987,1980), (1992,1987), and (2000,1992), I estimate

$$DiffY_t - DiffY_{t'} = \alpha + \beta_{1987} + \beta_{1992} + \beta_{2000} + u_t. \tag{4}$$

The dependent variable $DiffY_t - DiffY_{t'} \equiv$

$$\left[Ln(Empl_{DE,t}) - Ln(Empl_{synth,t})\right] - \left[Ln(Empl_{DE,t'}) - Ln(Empl_{synth,t'})\right].$$

There are four observations in the regression, and α denotes the difference in sectoral growth between 1976 and 1980 in Delaware and the synthetic control. The coefficients β_t measure the additional difference in sectoral growth between Delaware and the synthetic control from t to t' (i.e., β_{2000} gives the additional difference in sectoral growth between Delaware and the synthetic control from 1992 to 2000).

Given that there are three states in the control group when comparing it to bordering states, I specify the regression slightly differently. However, this produces the same difference-in-difference effects as above. Specifically, I estimate the following for state s and year t:

$$\begin{aligned} Ln(Empl_{st}) - Ln(Empl_{st'}) &= \alpha + \gamma_{DE} + \beta_{1987} + \beta_{1992} + \beta_{2000} + \\ &\quad \delta_1 DE * 1987 + \delta_2 DE * 1992 + \\ &\quad \delta_3 DE * 2000 + u_{st}. \end{aligned} \tag{5}$$

4.3 Dynamic Response of Wages

I use the 1950 and 1960 1 percent sample, 1970 1 percent Form 1 state sample, and 1980 5 percent state sample, along with the CPS March Supplement microdata from 1977 through 2014, 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 prepolicy period. I estimate the following regression:

$$\begin{aligned} ln(w_{ist}) &= \alpha_0 + \gamma_t + \delta DE_{ist} + \sum_{g=1}^{7} \beta_g YearGroup\_g_t * DE_{ist} \\ &\quad + X_{ist}\kappa + Z_s\eta + u_{ist}. \end{aligned} \tag{6}$$

The dependent variable is the log of the individual's wage and salary income from the previous year, in 1999 dollars.[22] The vector X_{ist} contains individual characteristics.[23] Z_s is a vector of state characteristics in the prepolicy 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.[24] Because labor

[22] For data from the CPS March Supplement, I exclude individuals with wages below the first percentile of the nonzero wages in each year. In the census, the codes are the midpoints of intervals, and so I do not employ the same restriction. Both the CPS and census data are top-coded.

[23] These include 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. See appendix for details on variable construction.

[24] I do not include each of the five-year prepolicy averages because of the limited number of

force participation rate is available only in 1976, I include the value in 1980 and the average from 1976 through 1979. I include the same state-level demographic variables as in the synthetic control, and the mean of the outcome in each state in each prepolicy year.[25]

The variable DE_{ist} indicates whether individual i living in state s in year t was living in Delaware in that year. The year groups are similar to those above, but adjusted by one year since respondents in the CPS report retrospective wages. I also include as year groups the prepolicy census years 1960 and 1970, omitting an indicator for 1950. I omit the interaction between DE_{ist} and 1950, since I include DE_{ist}. Thus, I interact DE_{ist} with the year groups g, including 1960, 1970, 1977–1981, 1982–1989, 1990–1993, 1994–2001, and 2002–2014.

I also estimate a regression comparing wages in Delaware to wages in bordering states:

$$\begin{aligned} ln(w_{ist}) &= \alpha_0 + \gamma_t + \delta DE_{ist} + \sum_{g=1}^{7} \beta_g YearGroup\_g_t * DE_{ist} \\ &\quad + X_{ist}\kappa + u_{ist}. \end{aligned} \tag{7}$$

Similar to the regressions in the earlier sections of this paper, I compare Delaware to bordering states without controlling for other prepolicy state characteristics.

I estimate these specifications using the full sample (including industry and occupation fixed effects), and separately for individuals whose industry was "Banking

individuals in the sample who are working in Delaware.

[25]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 prepolicy year on indicator variables for each state. Assuming classical measurement error, I calculate the reliability measure on the prepolicy average wage as (1-$\frac{var(SE_s)}{var(meanincome\_t_s)}$) for each t in the prepolicy years. SE_s denotes the robust standard error on the state indicator variable for state s, and $meanincome\_t_s$ 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 0.9, with a majority greater than 0.99. This suggests that 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 case is more complicated (Wooldridge 2002).

and credit agencies,"[26] as well as for "Accountants and auditors,"[27] and for clerks and managers who would be relevant to the banking and credit industries.[28] Wages for these occupations, regardless of industry, may have increased because of demand from financial firms.

I include occupation fixed effects when only including those in the banking and credit industry. I include occupation and industry fixed effects when the sample is limited to relevant occupations. In the regressions limited to individuals in the banking and credit industry, I only include the mean wage over the years 1977 through 1981, because of small sample sizes in early years.

The coefficient β_g measures the average difference in log wages for years in year group g between an individual in Delaware and a similar individual working in a state similar to Delaware before the policy (regression 6) or in a bordering state (regression 7). The first year the policy may affect wages is 1982, since respondents report the previous year's wage. For each year group after 1982, I compare $\widehat{\gamma}_g$ to $\widehat{\gamma}_{1977-1981}$; this compares postpolicy differences to prepolicy differences. I estimate the regression with standard errors clustered at the state level, state/year level, and unclustered but robust to heteroskedasticity. To be conservative, I present the standard errors when correcting only for heteroskedasticity, since these are larger than the clustered standard errors.[29]

[26] Coded as 716 using the 1950 Census Bureau industrial classification system.

[27] Coded as 0 using the 1950 Census Bureau occupational classification system.

[28] 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 A.12 in the on-line appendix for the list of occupation codes included as managers relevant in banking and credit.

[29] While I report the largest standard errors, the F-tests for whether the effects are equivalent to the effects in the years before the policy are also at times larger. This results in lower p-values. When comparing to bordering states, and including all occupations and industries, the F-tests comparing the effects in 1994–2001 and 2002–2014 to 1977–1981 using the unclustered standard errors imply that the differences-in-differences are statistically significant. However, they are not statistically significant based on the F-test using the standard errors clustered at the state/year level. There is a similar result when comparing Delaware to bordering states and including relevant clerks, accountants, and managers, and testing the equivalence of the effects in 1994–2001 and 1977–1981. There is also a similar result when comparing Delaware to all states and including relevant clerks, accountants, and managers, and testing the equivalence of the effects in 1989–1993 and 1977–1981. This is because the covariances are higher in these instances when the standard errors are unclustered but robust to heteroskedasticity than when they are clustered at the state/year level.

5 Prepolicy Differences: Delaware, Bordering States, and the Synthetic Control

Two of the top three states comprising the synthetic control are neighboring states to Delaware (Maryland and Pennsylvania), accounting for approximately 36 percent of the synthetic control (Table 1). The potential advantage of the synthetic control is that it identifies states similar to the treatment state, but not necessarily geographically close. Most of the other states in the control are those where manufacturing is similarly important as it was in Delaware before the policy (27 percent of employment in 1980). Other than Alaska, Florida, and Vermont (which make up only 16 percent of the synthetic control), all of the nonbordering states in the synthetic control had between 29 percent and 33 percent of their economy employed in manufacturing in 1980.[30] American manufacturing experienced significant declines in the 1980s. As a result, states where manufacturing was similarly important may best approximate Delaware in the absence of the policy.

Table 2 shows that in 1975–1979 manufacturing comprised 28 percent of Delaware's employment, but only 24 percent of bordering states' employment. The synthetic control more closely matches this high percentage in manufacturing, with 26 percent employed in manufacturing. Similarly, in this period, services made up 17 percent of Delaware's employment, but 19 percent of bordering states' employment. The synthetic control more closely matches this lower percentage in services, with 17 percent employed in that sector.

Figure 3 shows that for the main outcomes of interest, pretrends generally look similar in the synthetic control and bordering states, and these fairly closely approximate the prepolicy outcome in Delaware.

Thus, the principal advantage of the synthetic control over the bordering states is greater similarity in the economy's sectoral composition: more reliance on manufacturing and less on services. Because of the important shock to manufacturing in the 1980s, comparing Delaware to other manufacturing states may be especially important in satisfying the parallel trends assumption. However, if postpolicy trends are driven mainly by geography rather than by these differences in sectoral composition,

[30] States with percentage employed in manufacturing: South Carolina (33 percent), Michigan (29 percent), North Carolina (34 percent), Indiana (31 percent), Connecticut (31 percent), Ohio (29 percent), Alaska (27 percent), Vermont (25 percent), and Florida (13 percent).

then comparing Delaware to bordering states will more likely identify the policy's causal effect. As mentioned, where these methods yield different results, they can be seen as informally bounding the policy's effect.

6 Adjustment to a Labor Demand Shock

6.1 Short-Run Adjustment

I focus first on the policy's effects over approximately the first decade. This is a period in which Delaware's policy-induced advantage over other states had not completely disappeared. Also, because additional shocks are always possible in the long run, attributing short-run effects to the policy is more straightforward. Within this first decade, I analyze effects within the first seven years (through 1987), and then from 1988 to 1992.

Employment

Relative to before the policy, employment growth in Delaware immediately after the policy is about 1.5 percentage points higher per year than in the synthetic control (Figure 3; Table 3, column 1). This effect is larger than all but two of the placebo estimates, yielding a p-value of 0.06. While the magnitude is similar when comparing Delaware to bordering states, the estimate is not statistically significant. However, Appendix Table A.3 shows that effects on employment growth relative to bordering states were large and statistically significant in 1985 and 1987. The increase in employment growth suggests that the policy did not simply induce workers to substitute across sectors, without an effect on total employment.

Column 2 shows that of the 1.5 percentage point annual difference in employment growth, 0.6 percentage points are due to FIRE employment growth. This FIRE growth relative to the synthetic control is larger than all of the placebo estimates. The magnitude is similar when comparing Delaware to bordering states.[31] Delaware's legislation required that banks opening as a result of the state's policy must employ at least 100 people by the end of the first year of operation. One way of measuring spillovers within finance is to compare the number of new FIRE jobs with the number

[31] Appendix Table A.2 presents the difference-in-difference for each postpolicy year, relative to the five years immediately preceding the policy.

of jobs required given the number of new firms. Twenty-one new banks had been opened through the FCDA by 1987.[32] Thus, between 1980 and 1987 there should have been at least 2,100 new FIRE jobs in Delaware. In fact, there were 14,400 new FIRE jobs in this period.[33]

Population growth increases immediately after enactment of the policy, suggesting out-of-state residents are attracted to the newly created jobs. Relative to the prepolicy period, population growth is 0.6 percentage points higher per year in Delaware compared to the synthetic control (significant at the 10 percent level) and 0.3 percentage points higher per year compared to bordering states (not statistically significant) (column 4). This migration is consistent with immediate increases in housing price growth relative to the controls, although these effects are not statistically significant (column 6). The results suggest population increases by an additional 1 percent for a given 1 percent increase in FIRE jobs as a share of total employment in that year.[34]

There are large, immediate effects on the unemployment and participation rate. Relative to the period preceding the policy, Delaware's unemployment rate in the first postpolicy years is, on average, about 2.3 percentage points below that of the synthetic control, a larger effect than all but two of the placebo estimates. This effect is approximately 1 percentage point larger—and more statistically significant—than the estimate using the bordering states. Similarly, compared to the prepolicy period, the participation rate in Delaware immediately after the policy is, on average, about 1.3 percentage points higher than in the synthetic control and the bordering states, and is statistically significant at the 10 percent level.

If the new residents in Delaware are moving directly into jobs, this would decrease the unemployment rate and increase participation, without implying that the policy helped individuals to leave unemployment or reenter the labor force. Immediately after enactment of the policy, the growth rate of the number of unemployed and people out of the labor force is more negative in Delaware than the synthetic control

[32]Figure 1 shows that 27 banks and nonbanks had been opened or acquired through the FCDA by 1987. Of these, six had been acquired.

[33]While this includes new jobs in FIRE outside of banks and credit card companies (for example, insurance and real estate), Erdevig (1988) reports 13,536 new jobs in Delaware commercial banks from 1980 through 1987. Even including an additional 100 employees for the acquired banks (which may not have faced the requirement if they already had 100 employees) implies many more jobs were created than required.

[34]The coefficients suggest the policy yields an additional 0.6 percent increase in FIRE jobs as a share of employment, and an additional 0.6 percent increase in population.

and bordering states, relative to before the policy (Appendix Figure A.5, Appendix Table A.13). While these effects are not statistically significant, we cannot rule out nontrivial effects.

By the end of the 10-year period following enactment of Delaware's policy, total employment growth in Delaware was still nearly 1 percentage point higher per year than in the synthetic control and bordering states, relative to the prepolicy difference. However, this is smaller and less statistically significant than the immediate effect. FIRE employment growth remained large and statistically significant, though also smaller than the immediate effect. The difference between columns (1) and (2) suggests important employment spillovers to industries other than FIRE, as is discussed below.

Similarly, by the end of the 10-year period following enactment of the policy, the unemployment rate remained significantly lower relative to the controls (2.2 percentage points relative to the synthetic control and 1.5 relative to bordering states), and participation was significantly higher (about 2.5 percentage points more than both controls, relative to before the policy). Population growth was also much higher relative to the controls (roughly 1 percentage point per year more than both controls, relative to the prepolicy period). Consistent with the population increase, housing price growth was higher than in the controls, though only significant relative to the synthetic control.

In the years immediately following the policy, there are no statistically significant spillover effects to other industries, though the effects on transportation/utilities and construction are quite large (Figure 6; Table 5). However, by the end of the 10-year period following the policy, there is statistically significant evidence suggesting spillovers to trade and transportation/utilities. The coefficients suggest that a 22 percent increase in FIRE jobs (and a 55 percent increase over the previous years) is associated with an additional 7.6 percent growth in trade employment and an additional 11 percent growth in transportation and utilities employment. These effects are very similar when compared to bordering states, and the effects on construction employment are significant.

The coefficients in Panel A of Table 5 suggest the policy created an additional 12,680 FIRE jobs from 1980 to 1992, and that these jobs are associated with an additional 1,474 transportation/utilities jobs and 5,396 retail/wholesale trade jobs.[35]

[35]This is based on Delaware employment in these industries in 1980 and 1987. In 1980 (1987)

This suggests that for every 1 FIRE job created from the start of the policy through 1992, 0.54 jobs were created in trade and transportation/utilities.[36] The coefficients relative to bordering states suggest an additional 11,626 FIRE jobs from 1980 to 1992, and these are associated with an additional 1,487 transportation/utilities jobs, 7,171 trade jobs, and 4,363 construction jobs. This multiplier is 1.12 (and 0.74 when excluding construction).

Figure 1 suggests that by 1987, bank relocations to Delaware had nearly ended. If I treat the additional FIRE jobs from 1987 to 1992 as spillovers rather than a direct response to the policy, results suggest that for every FIRE job created from 1980 to 1987, by 1992 there were an additional 1.9 jobs in FIRE (from 1987 to 1992), transportation/utilities, and trade (1.02 excluding FIRE). The multiplier is 3.02 based on the bordering states specification (including construction), and 2.13 excluding FIRE.

The multipliers suggest the finance shock in Delaware added a large number of jobs in other sectors within the first 10 years. Moretti (2010) estimates local multipliers from tradable to nontradable employment. These multipliers are not directly comparable given that I look at multipliers by sector rather than by all nontradable jobs. However, the multipliers relative to added FIRE jobs from 1980 to 1987 are larger than the general tradable-to-nontradable multiplier in Moretti (2010), though smaller than the high-skilled tradable-to-nontradable multiplier (Moretti 2010). They are also smaller than multipliers from biotech to nontradables (Moretti and Wilson 2013). The multipliers relative to added FIRE jobs from 1980 to 1992 are smaller than these other estimates.

Interestingly, comparing Delaware to bordering states suggests effects on manufacturing, though these are not evident when compared to the synthetic control. This likely reflects the importance of comparing Delaware to other manufacturing states in the synthetic control, rather than to bordering states where manufacturing was less important.

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there were 12,300 (26,700) FIRE jobs, 12,100 (13,400) transportation/utilities jobs, 56,000 (71,000) trade jobs, and 14,700 (20,200) construction jobs.

[36] To construct the multipliers, I only include additional jobs that were statistically significant in Table 5.

Wages

The difference-in-difference in wages after the policy (1982–1988) relative to just before the policy (1977–1981), suggests a small, statistically insignificant policy effect on wages in Delaware (Table 4, column 1). This pre/post difference-in-difference is also small and statistically insignificant when comparing Delaware to bordering states (Table 4, column 2) and when limiting the sample to individuals working in banking and credit, or clerks, accountants, and managers (Table 4, columns 3–6). Including indicator variables for each postpolicy year interacted with Delaware, there is evidence of a temporary increase in wages for all occupations and industries one year after the policy (1983) that lasts for only one year (Appendix Table A.4), consistent with Blanchard and Katz (1992). This is also consistent with increases in labor supply. The effect on population growth, without continued wage effects in the first seven years, suggests that migration decisions are more strongly determined by employment than by wages, as in Beaudry, Green, and Sand (2014).

By the end of the 10-year period following Delaware's policy, there are large, statistically significant effects on wages. By 1989–1993, the difference-in-difference relative to just before the policy is 8 percentage points. This implies that Delaware wages are now 8 percent higher than in other states, whereas just before the policy they were equivalent, controlling for covariates. The difference-in-difference relative to bordering states is also positive, though not statistically significant. These effects were especially large among workers in the banking and credit industry. By 1989–1993, the difference-in-difference in banking and credit wages compared to just before the policy is 14 percentage points relative to all states, and 10 percentage points relative to bordering states. Among clerks, accountants, and managers, the effects are smaller and less statistically significant, though still a nontrivial 4 percentage points when compared to all states.

The results in this first decade suggest that fiscal or regulatory competition can effectively incentivize employers to relocate. Importantly, these employment and wage effects are robust to attempts from other states to compete away the advantage, and also robust to large population growth. The short-run effect on population growth differs from place-based policies targeting other industries (Busso, Gregory, and Kline 2013; Kline and Moretti 2014). This may imply the policy is less beneficial in helping unemployed or out-of-the-labor-force workers in Delaware. However, it does cause spillover effects to other industries—effects that appear larger than multipliers from

tradable to nontradable employment (Moretti 2010), and there is some evidence of reductions in the number of unemployed.

Given that firms continue to arrive in Delaware in this first decade after policy adoption, the exercise is not perfectly comparable to the Blanchard and Katz (1992) study of adjustment to a one-time shock. However, similar to their findings, there is an effect on migration and not on firm exit. The next section will consider whether the effects within the first decade can persist after Delaware has lost its policy-induced advantage.

6.2 Long-Run Adjustment

By 1991, Delaware's tax and usury law advantage had disappeared because of competition from other states and federal legislation. This period provides a unique setting to study the robustness of place-based policies to fiscal and regulatory competition from other jurisdictions. If companies remain in Delaware, this may suggest that the policy yielded agglomerative effects. Alternatively, it may suggest small differences in regulatory environments and substantial relocation costs for companies.

Analyzing the policy's long-run impact requires that any difference-in-difference must derive from the initial shock, ruling out alternative shocks in Delaware and control states. Bordering states may be a better control in the long run, if long-run economic trends are driven more by geography than by initial sectoral composition.

During the 1990s, total employment growth in Delaware remains about 1 percentage point higher per year than in the controls, relative to before the policy (Figure 3; Table 3). FIRE growth is contributing more to total employment growth than it did immediately after the policy. Figure 3 shows a clear resurgence in FIRE growth in this period. Figure 5 indicates that this appears to be mainly due to the growth of MBNA, a credit card company that spun off from one of the original FCDA firms relocating to Delaware.[37] This suggests that persistent effects are due to the original policy, rather than a second shock affecting finance labor demand in this period.[38]

[37]The insurance firm AIG also grew from 150 Delaware employees in the mid-1980s to 2,700 Delaware employees in 2001 (Epstein 2001b). This growth suggests the importance of agglomeration economies. AIG located its marketing division in Delaware (Epstein 1999b). It pioneered the use of direct marketing in the insurance industry (Jackson 1992), using strategies similar to those used by Delaware's credit card companies. In the late 1990s, MBNA partnered with AIG to sell insurance (Epstein 1999a), and AIG opened a bank (Epstein 1999b). Below, I test whether agglomerative forces in Delaware appear stronger than in the industry nationally.

[38]Newspaper articles have attributed persistent effects on Delaware's economy to the original 1981

The difference between columns (1) and (2) suggests important employment spillovers to industries other than FIRE, discussed below.

From 1993 to 2000, Delaware's unemployment rate is 1.5 percentage points below that of both the synthetic control and bordering states, relative to before the policy. While this effect represents convergence towards the synthetic control, it is similar in size to the effect in the 1980s and early 1990s when compared to bordering states. This very incomplete convergence may be due to continued population growth in Delaware, which is larger than all but two of the placebo estimates during this period. Participation converges toward the controls during this period as well, though remaining over 1 percentage point larger in Delaware than before the policy.[39]

The positive effect on trade employment growth continues during the 1990s with a similar or larger magnitude (though only significant relative to bordering states) (Table 5). The employment effects in transportation and utilities are now smaller, though they still imply that the policy increased employment in this sector by 7 percent. Effects on construction employment growth are smaller than the preceding period but still imply the policy increased employment in this sector by an additional 12–16 percent (only significant relative to bordering states).

To calculate the multiplier, I include the additional growth in each sector from 1980 to 2000, as well as the additional growth in FIRE from 1992 to 2000. As Figure 1 shows, this FIRE growth is no longer due to new firms relocating as a direct result of the 1981 policy, and so I interpret it as spillover effects within FIRE. The coefficients from the bordering states specification imply that the policy created an additional 11,626 FIRE jobs from 1980 to 1992, and that these were associated with an additional 11,218 FIRE jobs (from 1992 to 2000), 2,509 transportation/utilities jobs, 15,474 trade jobs, and 7,162 construction jobs.

These results imply that for every FIRE job created from 1980 to 1992, by 2000 there were an additional 3.13 jobs in FIRE, transportation/utilities, trade, and con-

policy (Epstein 2001a). After extensively studying newspaper and trade journal articles, as well as interviewing a knowledgeable party (chief of staff and legal counsel to Governor du Pont, who signed the FCDA into law), I found no evidence that this later growth at MBNA was the result of a second Delaware policy. See the on-line appendix for information on an unsuccessful attempt to create another regulatory advantage for Delaware banks.

[39]Participation drops below that of the synthetic control by the end of the sample period. Appendix Figure A.4 in the on-line appendix shows, based on CPS data, that in these years (2006 until 2010) there was also an increase in the percentage of new residents in Delaware who were 55 and older. In 2014, Kiplinger ranked Delaware as the seventh most tax-friendly state for retirees, and the tax-friendliest in the Northeast (*10 Most* 2014).

struction relative to 1980. Not counting the FIRE jobs implies a multiplier of 2.16. As before, the additional FIRE jobs from 1987 to 1992 are arguably the result of within-FIRE spillovers. Accounting for this, the multiplier implies that for every FIRE job created from 1980 to 1987, by 2000 there were an additional 6.83 jobs in FIRE (from 1987 to 2000), transportation/utilities, trade, and construction relative to 1980 (4.11 excluding FIRE). This is smaller than the short-run local multiplier from biotech jobs (Moretti and Wilson 2013), but larger than recent estimates of the local multiplier from tradable jobs to nontradable jobs (Moretti 2010).

With the caveat of having few prepolicy years, the positive effect on housing price growth falls considerably from the late 1980s to the mid-to-late 1990s. This may be explained by new construction and a flat long-run housing supply curve, consistent with previous findings (Bartik 1991; Blanchard and Katz 1992). However, the effect increases again when compared to the synthetic control in the 2000s.

After 2000, there continues to be a negative policy effect on the unemployment rate (2 percentage points lower relative to the synthetic control, and 1 percentage point lower relative to bordering states, but not statistically significant). Population growth also continues to be large and statistically significant relative to the controls. There is no longer any effect on participation relative to either control.

Wages

The large effect on wages persists in the 1990s. The coefficient implies that Delaware wages are now 6 percent higher than in other states, whereas just before the policy they were equivalent, controlling for covariates. The difference-in-difference relative to bordering states is also positive, suggesting wages are 2 percent higher in Delaware in this period, whereas they were equivalent just before the policy. These effects are only slightly larger for workers in the banking and credit industry, whereas in the earlier period they were considerably larger for these workers. In contrast, the effect on clerks, accountants, and managers is now larger than it was in the period from 1989 to 1993.

These large wage effects also generally persist from 2002 to 2014, though magnitudes and statistical significance are slightly smaller.

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 1989–1993 in Delaware are 8.2 percent higher

than in similar states relative to their prepolicy levels in 1977–1981.[40] Using the results from the synthetic control, Delaware housing prices become 18.2 percent more expensive than in the synthetic control in 1988 (the relevant year for reported wages in 1989), relative to their levels in 1980.[41] Assuming a share of housing services of 15 percent, as in Blanchard and Katz (1992), and ignoring that other prices may also go up, consumption wages increase by approximately 5.5 percent in 1989–1993. Thus, while housing price growth dampens the incentive for individuals to migrate to Delaware, this effect does not eliminate the wage growth.

In sum, even after Delaware had lost its policy-induced advantage relative to other states, the policy effects strongly persisted for the next decade. Twenty to thirty years after the policy, total employment growth appears to have slowed considerably. However, the lower unemployment rate, higher population growth, and higher wages persist even 30 years after the policy, though with some convergence toward the control group. The results suggest Delaware's original advantage provided long-run effects, even after that advantage had disappeared.

Notably, the results differ dramatically from Blanchard and Katz (1992), who find that unemployment and participation converge to the prepolicy equilibrium five to seven years after the shock. Below, I consider the extent to which these long-run effects can be explained by the greater share of Delaware's economy employed in finance, a sector with lower unemployment. An alternative explanation is that the policy directly affected worker productivity in the finance sector, or indirectly affected productivity through agglomeration.

Robustness

Predictor variables. The principal results estimate one synthetic control for each outcome variable. However, 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

[40] Subject to the log approximation, the results suggest that controlling for covariates, reported wages in Delaware are 3 percent lower than those in other states in 1977–1981. Reported wages are 5 percent higher in Delaware than other states in 1989–1993. I divide 1.05 by 0.97 to get the 8.2 percent difference in 1989–1993 relative to 1977–1981.

[41] Normalizing the value of housing in 1980 to 1.0 in Delaware and the synthetic control, I determine the value in 1988, given the price growth in each year from 1981 through 1988. I obtain these values from the principal synthetic control estimation. By 1988, the value was 1.3 in Delaware and 1.1 in the synthetic control. The ratio of housing prices in Delaware to the synthetic control is the 1980 ratio multiplied by 1.3/1.1, or 118.2 percent.

growth. For robustness, I allow for the synthetic control to differ for each outcome variable. Specifically, as described in Abadie, Diamond, and Hainmueller (2010), for each outcome variable, I find the weights on the predictors that minimize the mean squared prediction error of the outcome variable in the prepolicy period.

Overall, the composition of the synthetic control looks quite similar across outcome variables (Appendix Table A.6). Several states make up a clear majority of the control for most outcomes (Indiana, Connecticut, and Maryland). Relative to the main synthetic control, there is significantly more weight placed on Indiana and Connecticut in this robustness control.

Allowing for the composition of the synthetic control to differ for each dependent variable yields some important differences in the control across outcomes. For example, while Indiana comprises over 35 percent of the control for FIRE employment growth, total employment growth, and the unemployment rate, it is only 9.5 percent of the control for population growth and 0 percent of the control for participation. It appears that relative to these other variables, population growth in Delaware looks more similar to South Carolina and Ohio. Similarly, relative to these other variables, participation in Delaware looks more similar to Arizona and Ohio.

The results are largely unchanged, though the effect on population growth is slightly smaller and less significant (Appendix Figure A.2, Appendix Table A.8). This suggests that while the synthetic control may change, it shifts to states that look sufficiently similar so that overall results do not dramatically change.

Adjusting for Negative Policy Effects in Control States. The migration of firms and people to Delaware will yield negative effects in control states. If these negative effects are large in particular states, and those states are influential in the synthetic control, then the policy's effect will be overestimated. I will be double counting 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 U.S. 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.[42] Looking at the difference in mobility across these years allows me to infer the effect of the policy on mobility. For robustness, I estimate the synthetic control excluding the top five states losing population to

[42]State-to-state migration flows are only available in the decennial years of the census.

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 (0.00043), Pennsylvania (0.00039), New Jersey (0.00035), West Virginia (0.00025), and Rhode Island (0.00023). While these states are important components of the synthetic control (Table 1), the population loss to Delaware is very small. This mitigates concerns that negative policy effects in control states bias the principal results. For example, from 1975 to 1980, Maryland lost 0.28 percent of its population to Delaware (approximately 11,600 people). From 1985 to 1990, Maryland lost 0.32 percent of its population to Delaware (approximately 14,200 people).

I construct the synthetic control but do not allow these five states to be in the synthetic control for Delaware. I also exclude them from the placebo analysis. While the composition of the synthetic control changes (Appendix Table A.9), the differences-in-differences estimates do not dramatically differ from the principal results. However, the effects on unemployment and population growth are larger when excluding states losing population to Delaware (Appendix Figure A.3, Appendix Table A.11). There does not appear to be evidence that the principal results are overestimated, which is not surprising given the small effects on population in nearby states.

Compositional Effects

The persistently lower unemployment rate and higher wages in Delaware may be explained by the policy's impact on sectoral composition. The policy resulted in a shift to finance, a sector with lower unemployment and higher wages. I compare Delaware's actual unemployment rate to the predicted rate based on sectoral composition, using national sectoral unemployment rates from the BLS labor force statistics (based on the CPS). If Delaware's unemployment is lower than this predicted rate, this suggests the policy brought sectoral unemployment rates (in finance or other sectors) lower than the national rates. This could be due to the policy's direct effect on worker productivity in the finance sector, or to a potential agglomerative effect of the policy.[43]

I obtain the predicted number of unemployed people by sector (s) in the following way: $UR_{National,s} = \frac{U_{DE,s}}{U_{DE,s}+E_{DE,s}}$. The values of $E_{DE,s}$ (number employed in sector s) and $UR_{National,s}$ (national unemployment rate in sector s) are known, and I solve

[43] As discussed in Evans and Schmalensee (2005), Marquette may have directly affected worker productivity in the finance sector because employees no longer had to tailor credit card offers to the customers' state of residence.

for $U_{DE,s}$ (number unemployed in sector s). I then add the number of predicted unemployed across all sectors, and divide by this number plus the total employed.[44] The BLS started reporting sectoral unemployment rates in 1976, and so I present predicted unemployment rates starting in 1976. The sectoral unemployment rates use the SIC definitions, which are not available starting in the early 2000s, when the BLS exclusively adopted the NAICS definitions. Consistent with the results presented earlier, I present these predicted unemployment rates through 2000.

In the years immediately after the policy, there is a large difference between Delaware's actual and predicted unemployment rates (Figure 7). The difference reaches up to 2 percentage points in the mid- to late 1980s. Lower unemployment rates than national averages are consistent with a dramatic, exogenous increase in labor demand. During this initial period, there are transitions to employment from unemployment and from being out of the labor force, and new residents arrive to immediately take jobs. In this setting, we would expect that the unemployment rate in Delaware is even lower than the new economic composition would suggest.

The actual rate remains considerably below the predicted rate until approximately 1994, nearly 15 years after the policy was enacted. This could be evidence of the policy's agglomerative effects, which drive unemployment rates below national sectoral rates in finance or other sectors.

During the mid-1990s, the unemployment rate is 1 to 2 percentage points lower in Delaware than in bordering states and in the synthetic control. However, the difference between Delaware's unemployment rate and the predicted rate based on sectoral composition is significantly smaller in this period—approximately 0.2 percentage points. This suggests that much of the persistent effect on the unemployment rate in the longer run is due to the changed sectoral composition.

This exercise suggests the policy's long-run effects are not due to its direct impact on worker productivity in the finance sector. Rather, the results suggest the long-run policy effects can be attributed to the policy's impact on sectoral composition. While this shift towards finance may have yielded agglomerative effects in Delaware, the results suggest these agglomerative effects were not stronger than those that exist in finance nationally. Delaware largely attracted firms from New York, an important

[44]The employment data used in the paper are only for nonfarm employment. However, the state unemployment rate is constructed by the BLS using CPS data, which includes workers in the agricultural sector. Thus, to compare the predicted unemployment rate to the actual rate, I use data on agricultural employment from the Bureau of Economic Analysis (BEA).

financial center with agglomerative effects likely equal to at least the national average in the industry. The results then suggest that while Delaware's policy yielded positive effects in Delaware, it was inefficient at the aggregate level.

7 Conclusion

This paper analyzes the short- and long-run impact of an exogenous shock to labor demand in the financial services sector, using the relocation of finance companies to Delaware in the early 1980s. Policies aimed at attracting firms to a particular jurisdiction are prevalent, though much of the recent literature has focused on policies targeting jobs in the tradable sector—for example, manufacturing. The response to these policies may depend on the targeted industry because of differences in wages, mobility frictions, and spillover effects.

The first contribution of the paper is to study the short-run impact of a policy targeting financial services, and to compare this to recent studies of policies targeting the tradable sector. The second contribution is to study the long-run impact of the policy, after the original policy-induced advantage had disappeared. Given intense competition between local jurisdictions, it is important to understand whether newly attracted firms will remain in the new jurisdiction or will leave for a more attractive package.

Using bordering states, as well as the synthetic control framework, the findings suggest that fiscal or regulatory competition can effectively incentivize employers to relocate, and that this has positive effects on the local economy. By the end of the first decade, total employment growth, wages, population growth, and participation were higher, while the unemployment rate was lower. Furthermore, for every FIRE job created from 1980 to 1987, by 1992 there were up to three additional jobs in FIRE (from 1987 to 1992), trade, transportation/utilities, and construction. These effects largely persisted for the two decades after Delaware lost its original policy-induced advantage, with the exception of total employment growth in this latter decade. In addition, by 2000 the multiplier was nearly seven.

These persistent effects differ dramatically from the Blanchard and Katz (1992) finding that unemployment and participation adjust within five to seven years of the shock. The policy's lasting impact on sectoral composition appears to explain these persistent effects. The lasting impact on sectoral composition is noteworthy given

that other states passed similar legislation to that of Delaware in the following years.

The effects differ from recent studies of policies that do not specifically target white-collar jobs. The principal differences are a stronger migration response to the finance shock, and more persistent effects after the original policy-induced advantage disappeared. The multiplier effects are larger than most estimates of local multipliers from tradable to nontradable employment (Moretti 2010). One important exception is that they are smaller than the estimated local multiplier from biotech to nontradable emploment (Moretti and Wilson 2013).

The implication for policymakers is that short-run effects from attracting firms can be sustained if the policy shifts the economic composition toward a low-unemployment and high-wage sector in the long run. However, at least in this setting, the successful local policy appears inefficient at the aggregate level. Agglomerative effects do not appear stronger than in the industry nationally.

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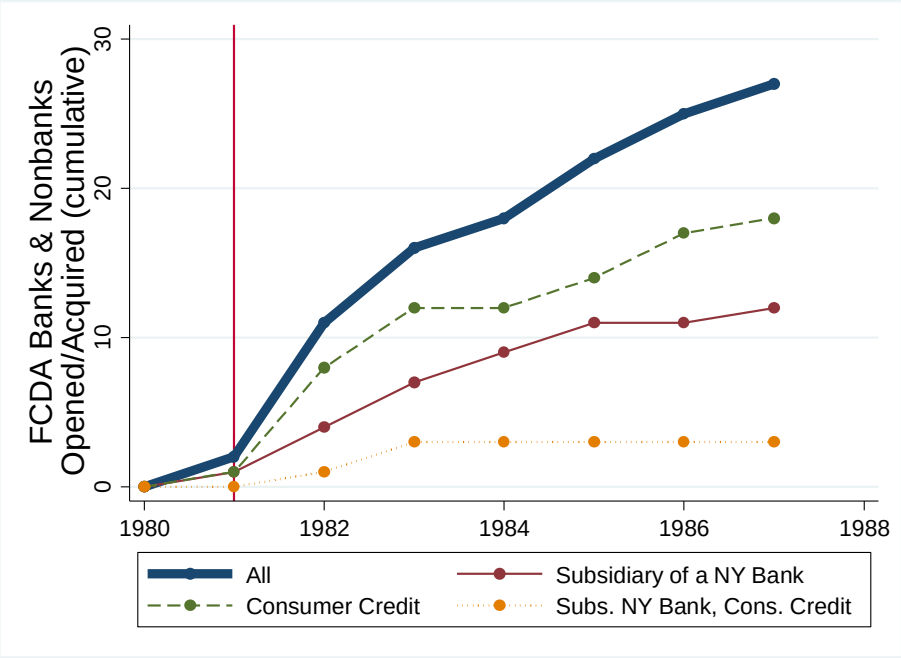
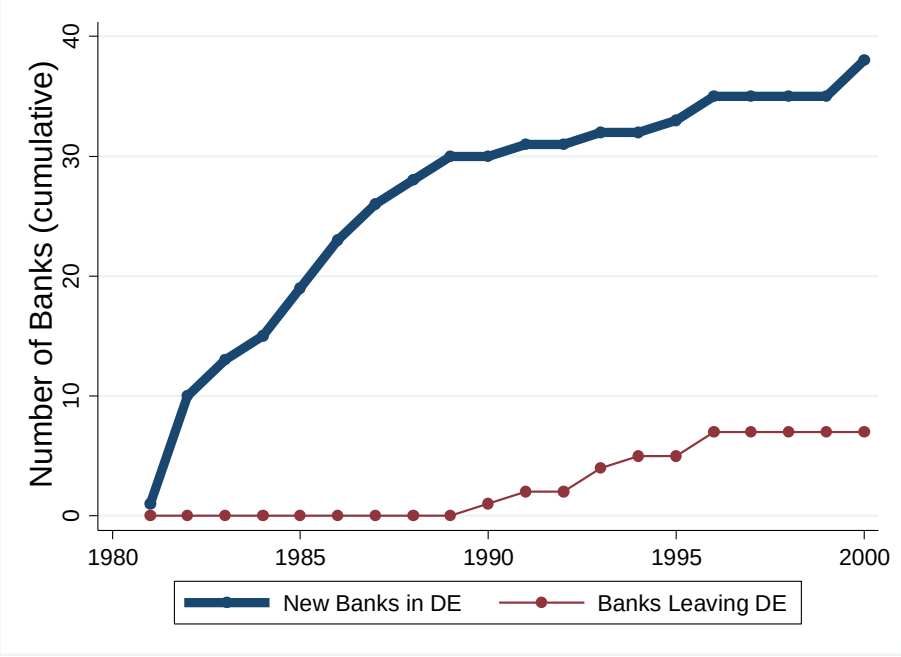
Figure 1: Bank Relocations Following Delaware's Policy

Panel A: FCDA Banks Opened/Acquired Through 1987, by Type and Origin

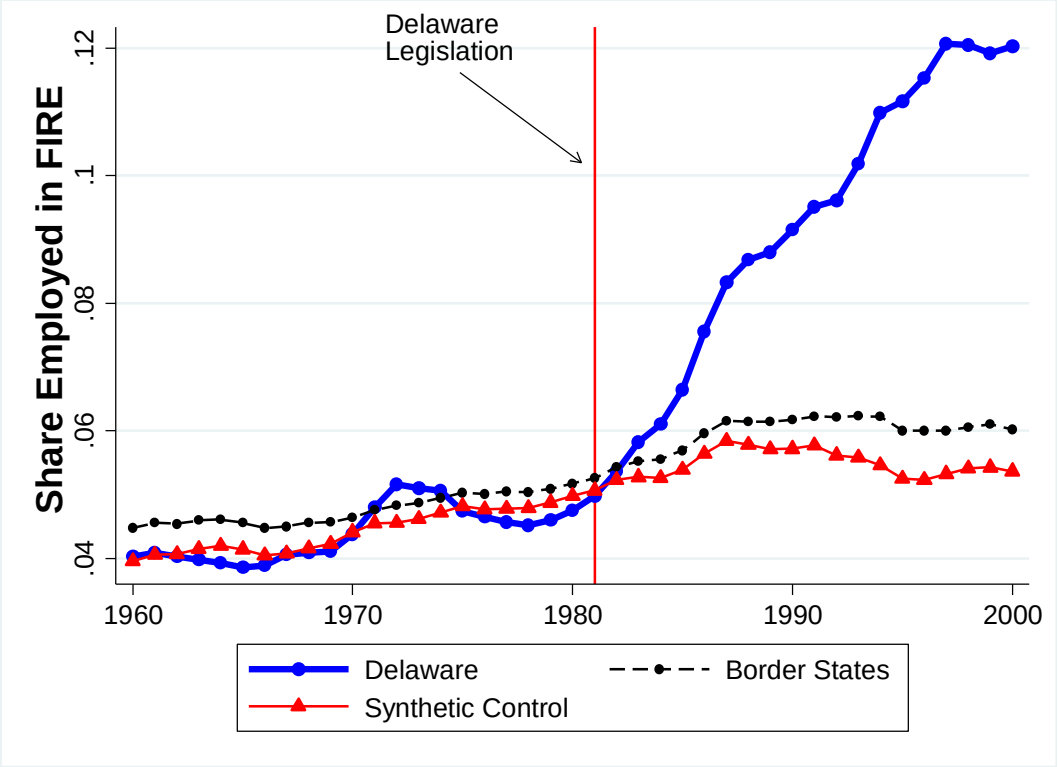


Panel B: Banks Opening and Closing in Delaware, Through 2000



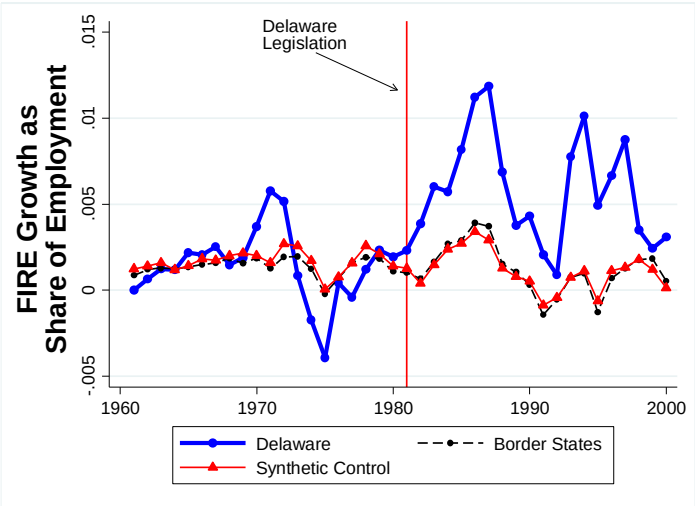
Note: The source for Panel A is Swayze and Ripsom (1988). The source for Panel B is Epstein (2001a). There are slight differences because Panel A shows only FCDA banks and nonbanks, rather than all new banks (in particular leaving out Community Credit Bank Act banks created through 1983 Delaware legislation). Further, Panel A includes acquisitions, rather than only new banks that were opened.

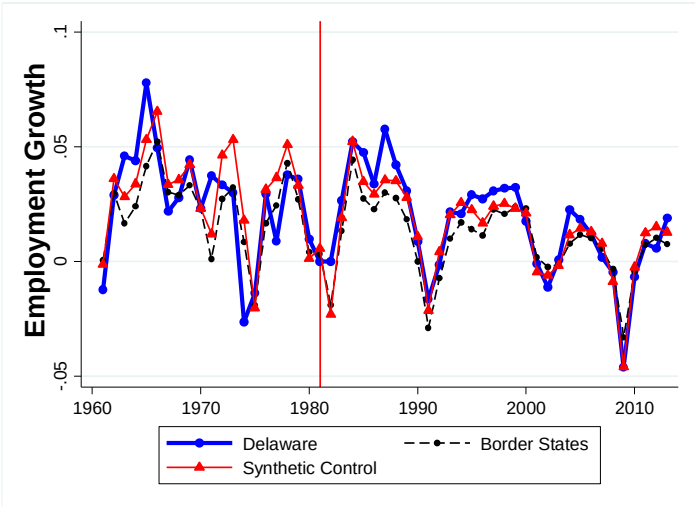
Figure 2: Policy Impact on FIRE Employment in Delaware

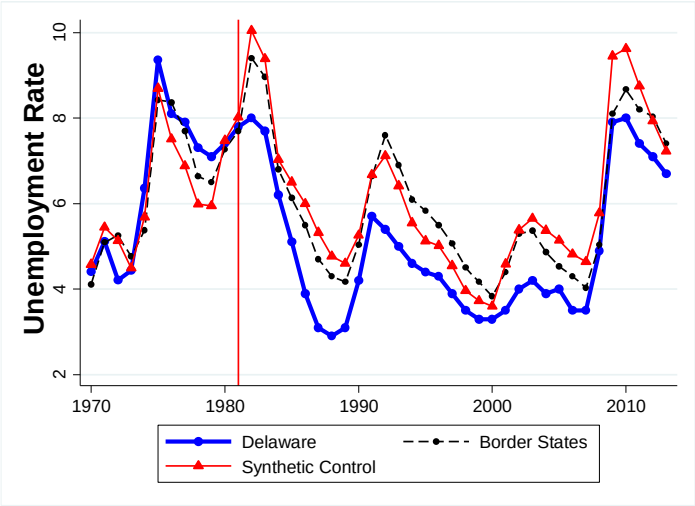


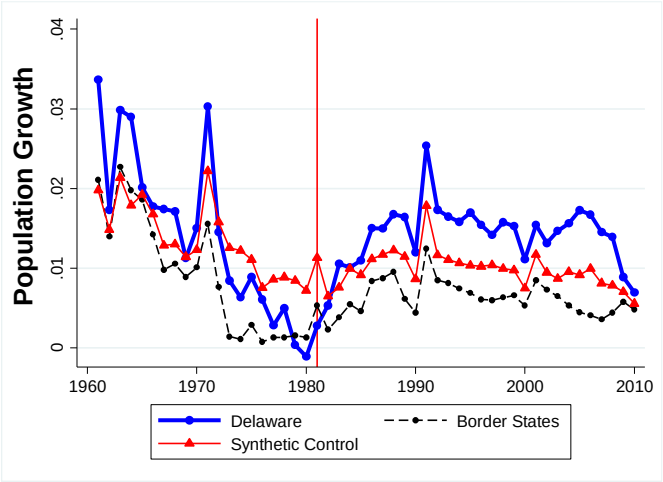
Note: See text for details on construction of the synthetic control.

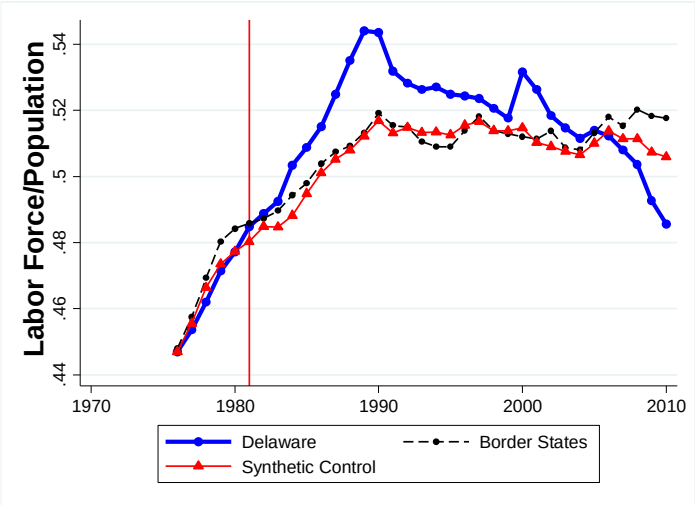
Figure 3: Policy Effect on Employment, Unemployment, and Population:
Delaware Relative to the Synthetic Control and Bordering States

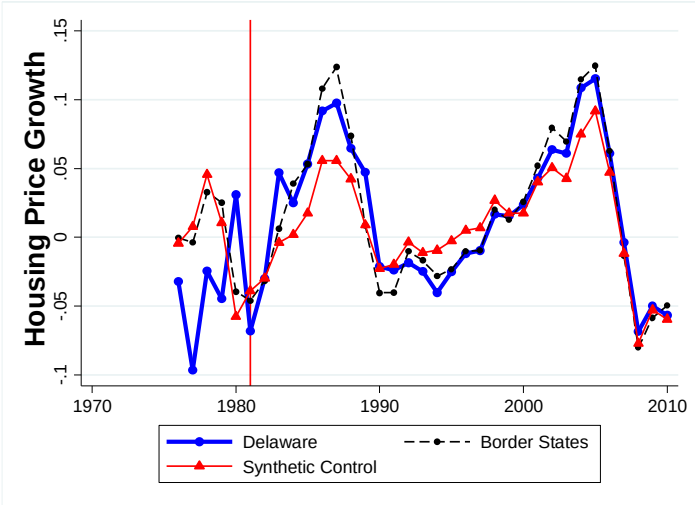






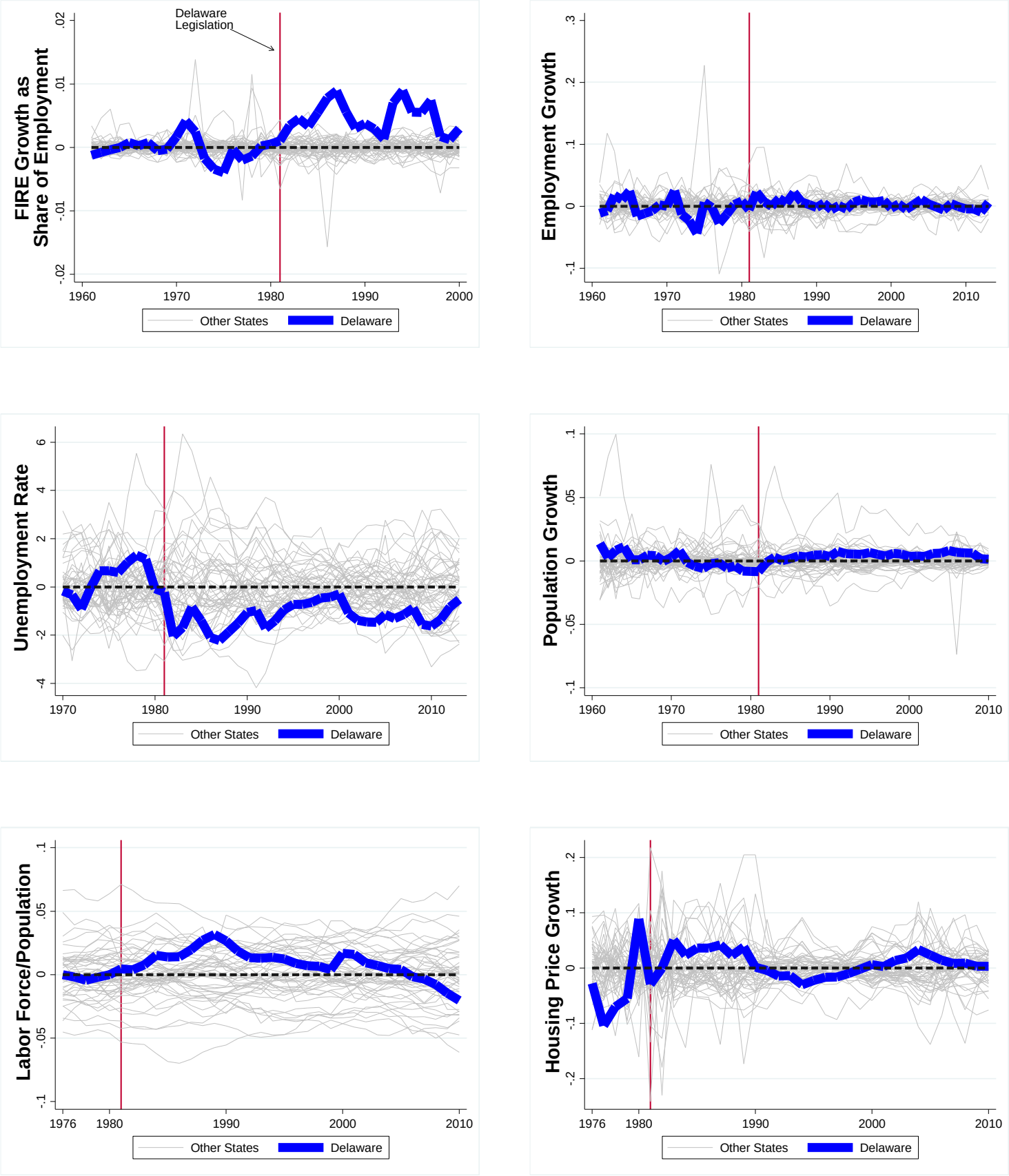






Note: See paper for details on construction of the synthetic control.

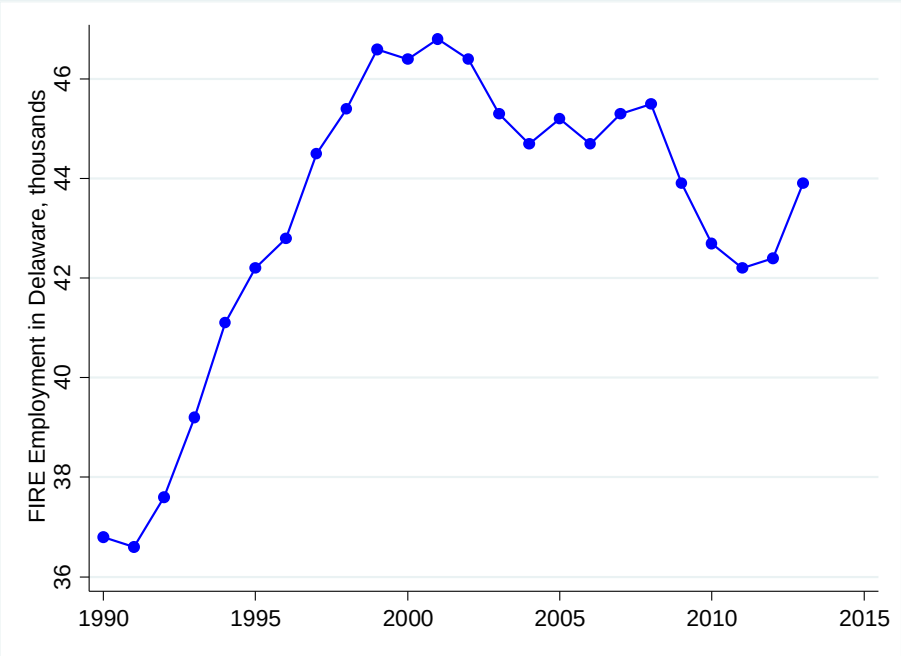
Figure 4: Policy Effects on Employment, Unemployment, and Population:
Estimated Effects in Delaware Relative to Placebos



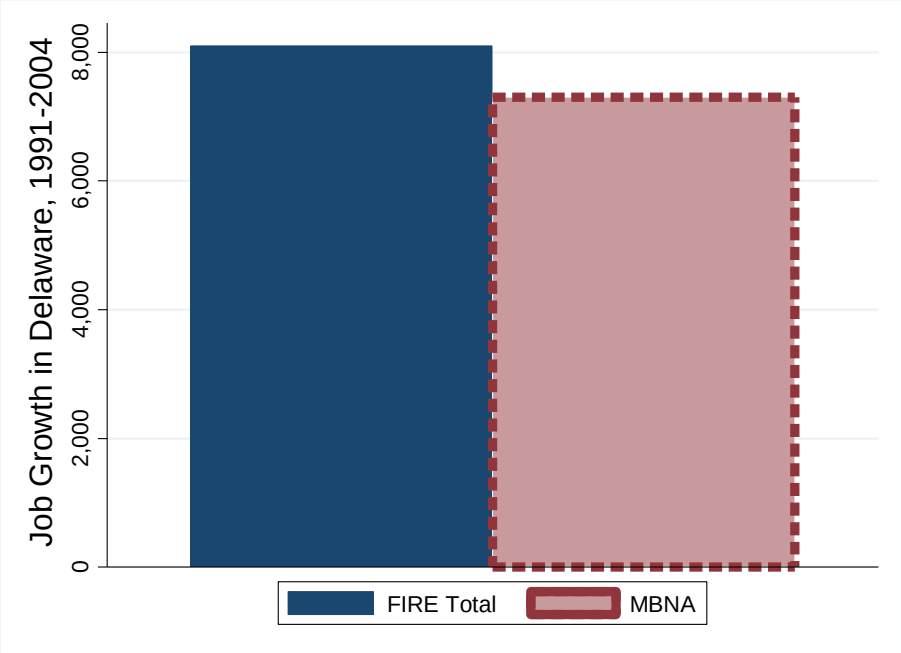
Note: See paper for details on construction of the synthetic control.

Figure 5: Decomposition of FIRE Growth in Delaware, 1990s

Panel A: FIRE Employment in Delaware, 1990-2015

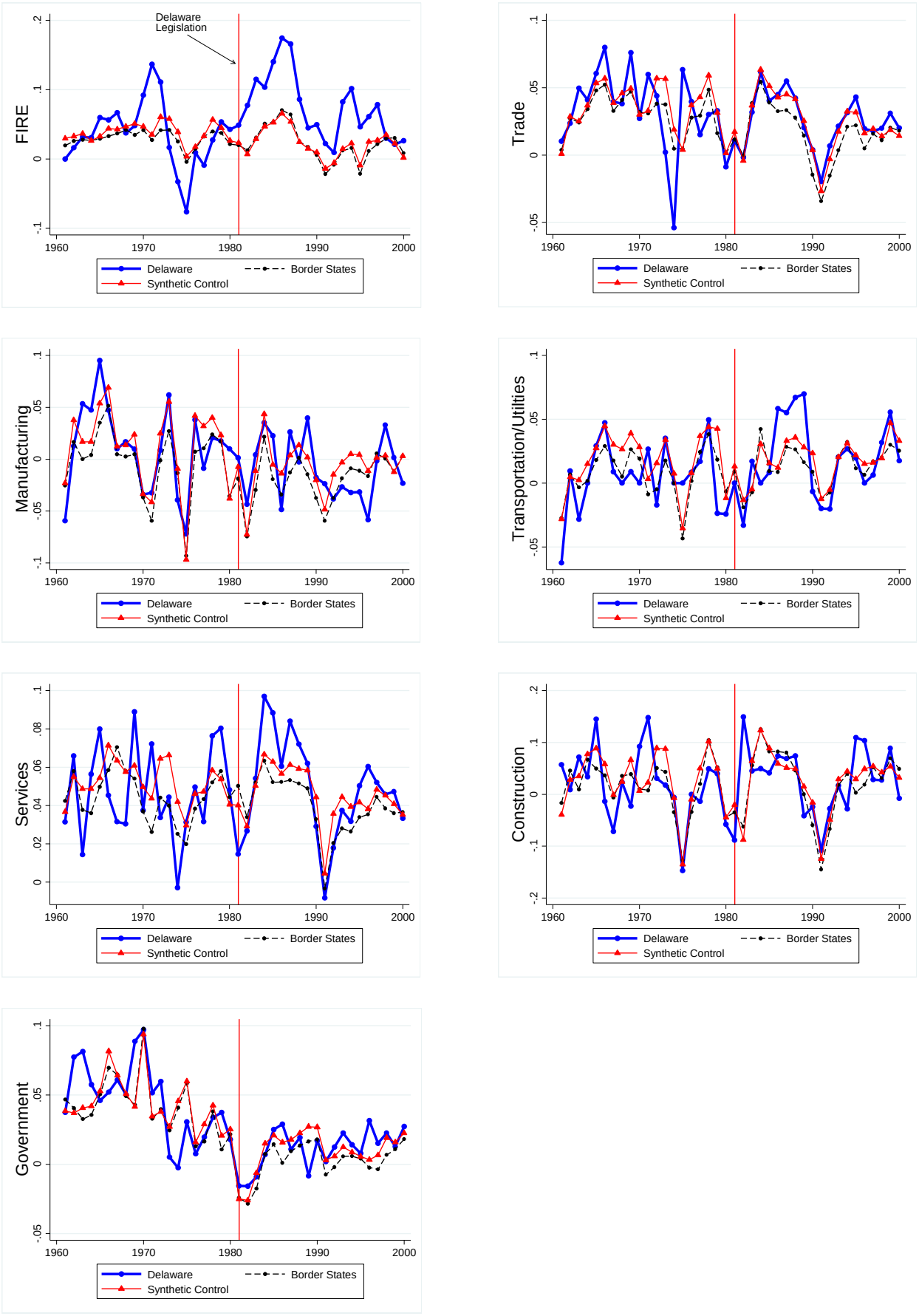


Panel B: FIRE Employment Growth Explained by MBNA Employment Growth



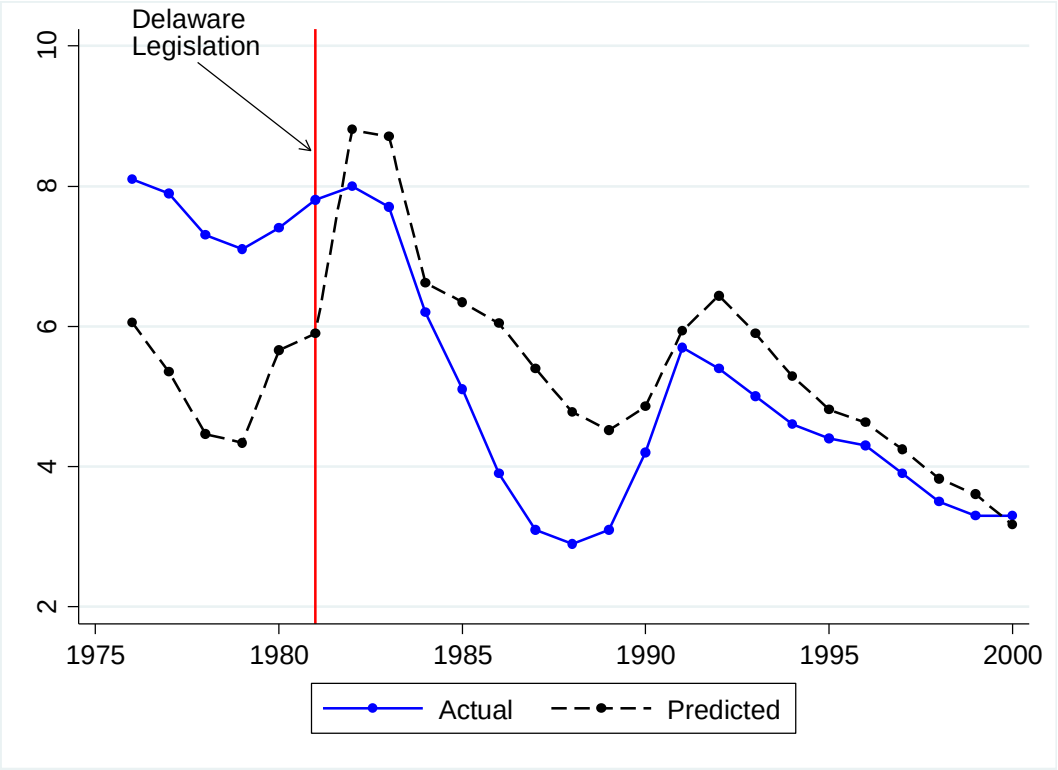
Note: Panel A, and the left bar of Panel B are constructed using the CES, based on the NAICS industry codes. The right bar of Panel B is constructed based on Boyer and Ratledge (2009).

Figure 6: Policy Effects on Employment Growth in FIRE and Other Industries:
Delaware vs. Synthetic Control and Bordering States



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

Figure 7: Effect of Sectoral Composition on Delaware's Unemployment Rate



Note: The predicted unemployment rate is constructed using the share of Delaware's employment in each sector from the CES, and national sectoral unemployment rates from the Bureau of Labor Statistics. See paper for details.

Table 1: Composition of Synthetic Control

Maryland	0.262
South Carolina	0.154
Pennsylvania	0.099
Michigan	0.091
North Carolina	0.084
Florida	0.082
Indiana	0.077
Vermont	0.057
Connecticut	0.044
Ohio	0.031
Alaska	0.02

Note: This table shows the composition of the synthetic control for Delaware. See paper for details.

Table 2: Pre-policy Characteristics of Delaware, Bordering States, and the Synthetic Control

	(1)	(2)	(3)
Predictors	**Delaware**	**Bordering States**	**Synthetic Control**
Share Employed in			
FIRE			
1965-1969	0.04	0.05	0.04
1970-1974	0.05	0.05	0.05
1975-1979	0.05	0.05	0.05
Manufacturing			
1965-1969	0.36	0.32	0.33
1970-1974	0.31	0.28	0.29
1975-1979	0.28	0.24	0.26
Trade			
1965-1969	0.2	0.2	0.20
1970-1974	0.21	0.21	0.21
1975-1979	0.22	0.22	0.22
Services			
1965-1969	0.13	0.15	0.14
1970-1974	0.15	0.17	0.16
1975-1979	0.17	0.19	0.17
% Metropolitan			
1960	68.9	81.5	56.5
1970	70.4	83.8	62.5
1980	67.0	87.4	73.6
% with ≥ a HS Diploma			
1960	43.3	39.6	38.8
1970	54.6	51.7	49.6
1980	68.6	66.5	64.6
Unemployment Rate			
1970-1974	4.9	4.9	5.1
1975-1979	8.0	7.5	7.0
1980	7.4	7.3	7.5
Population Growth			
1966-1970	0.02	0.01	0.01
1976-1980	0	0	0.01

Note: This table compares the balance of predictor variables in the synthetic control, Delaware, and the states bordering Delaware. Not all predictors are shown. For the full set of predictors, see text.

Table 3: Policy Effects on Employment, Unemployment, and Population:
Differences-in-Differences Relative to Five Years Pre-Policy

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Employment Growth	FIRE Employment Growth as a Share of Employment	Unemployment Rate	Population Growth	Labor Force/ Population	Housing Price Growth
Panel A: Delaware Relative to Synthetic Control						
Pre-1976	0.003	0	-0.823*	0.008*		
	(15/50)	(26/50)	(5/50)	(4/50)		
	[.3]	[.52]	[.1]	[.08]		
1981-1987	0.015*	0.006**	-2.3*	0.006*	0.013*	0.056
	(3/50)	(1/50)	(3/50)	(5/50)	(5/50)	(6/50)
	[.06]	[.02]	[.06]	[.1]	[.1]	[.12]
1988-1992	0.008	0.004**	-2.224*	0.011**	0.025*	0.042*
	(15/50)	(1/50)	(3/50)	(2/50)	(1/50)	(5/50)
	[.3]	[.02]	[.06]	[.04]	[.02]	[.1]
1993-2000	0.01	0.006**	-1.505*	0.011*	0.012	0.021
	(6/50)	(1/50)	(3/50)	(3/50)	(13/50)	(8/50)
	[.12]	[.02]	[.06]	[.06]	[.26]	[.16]
2001+	0.006		-2.011**	0.011*	0.001	0.047**
	(12/50)		(1/50)	(3/50)	(26/50)	(2/50)
	[.24]		[.02]	[.06]	[.52]	[.04]
Panel B: Delaware Relative to Bordering States						
Pre-1976	0.004	0.001	-0.122	0.005*		
	(0.010)	(0.001)	(0.941)	(0.003)		
1981-1987	0.012	0.005***	-1.324	0.003	0.013	0.026
	(0.012)	(0.001)	(0.940)	(0.002)	(0.011)	(0.033)
1988-1992	0.010	0.004***	-1.553**	0.008***	0.028***	0.043
	(0.013)	(0.001)	(0.756)	(0.003)	(0.010)	(0.029)
1993-2000	0.007	0.005***	-1.467***	0.007***	0.018**	0.028
	(0.007)	(0.001)	(0.491)	(0.002)	(0.009)	(0.021)
2001+	-0.001		-1.008	0.007***	-0.000	0.027
	(0.009)		(0.695)	(0.002)	(0.009)	(0.029)

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variables are at the state/year level. Both panels present differences-in-differences estimates. Coefficients on year groups reflect the average difference-in-difference for a given year in the year group. In Panel A, the rank of the effect relative to placebo estimates is in parentheses and the p-value based on this rank is in brackets. In Panel B, robust standard error in parentheses. Estimates in Panel B are the coefficients on year group interacted with Delaware. See paper for details.

Table 4: Policy Effect on Wages: Delaware Relative to Observationally Similar and Bordering States

	All		Banking and Credit		Clerks, Accountants, Managers	
Delaware	0.03	-0.05	0.00	-0.02	-0.10	-0.17
	(0.04)	(0.04)	(0.22)	(0.22)	(0.12)	(0.12)
DE*1960	0.03***	0.02	-0.11	-0.10	0.24***	0.21***
	(0.04)	(0.04)	(0.22)	(0.23)	(0.12)	(0.13)
DE*1970	-0.003*	0.01	0.04	0.02	0.16	0.14
	(0.04)	(0.04)	(0.23)	(0.23)	(0.13)	(0.13)
DE*1977-1981	-0.03	0.002	-0.07	-0.05	0.11	0.09
	(0.04)	(0.04)	(0.22)	(0.22)	(0.12)	(0.12)
DE*1982-1988	-0.02	-0.01	-0.10	-0.08	0.11	0.08
	(0.04)	(0.04)	(0.22)	(0.22)	(0.12)	(0.12)
DE*1989-1993	0.05***	0.01	0.07***	0.05*	0.15*	0.09
	(0.04)	(0.04)	(0.22)	(0.22)	(0.12)	(0.12)
DE*1994-2001	0.03***	0.02**	0.005*	-0.02	0.16***	0.13*
	(0.04)	(0.04)	(0.22)	(0.22)	(0.12)	(0.12)
DE*2002-2014	0.02***	0.01*	0.0008*	-0.01	0.15***	0.11
	(0.04)	(0.04)	(0.22)	(0.22)	(0.12)	(0.12)
Control	All	Bordering	All	Bordering	All	Bordering
N	10,089,651	1,075,720	216,887	23,726	1,961,047	214,560

Note: This table shows the coefficients on the interaction between Delaware and year group, relative to all states (Columns 1, 3, 5) and bordering states (Columns 2, 4, 6). These coefficients are from a regression including an indicator for Delaware, each post-policy year, the year group 1977-1981, the earlier census years (omitting 1950), and the interactions listed in the table above. The first two columns show the results for all occupations and industries, columns three and four show the results for individuals in the banking and credit industry, and the last two columns show the results for clerks, accountants, and managers relevant to the banking and credit industry. Asterisks denote whether the interaction between Delaware and year group relative to the coefficient on Delaware*1977-1981 is statistically significant. *** p<0.01, ** p<0.05, * p<0.1. In the regressions including all states, I control for state characteristics in the pre-policy period, individual characteristics, and occupation and/or industry fixed effects. In the regressions including bordering states, I control for individual characteristics and occupation and/or industry fixed effects. Robust standard errors are in parentheses as these are larger than the clustered standard errors. See text of the paper for further details.

Table 5: Policy Effect on Employment Growth in FIRE and Other Industries:
Differences-in-Differences Relative to Five Years Pre-Policy

	FIRE	Trade	Transportation/ Utilties	Services	Construction	Manufacturing	Government
Panel A: Delaware Relative to Synthetic Control							
1980-1987	0.549**	0.055	0.11	0.019	0.195	0.077	0.028
	(1/50)	(16/50)	(10/50)	(18/50)	(8/50)	(10/50)	(16/50)
	[.02]	[.32]	[.2]	[.36]	[.16]	[.2]	[.32]
1987-1992	0.222**	0.076*	0.11*	-0.064	0.13	0.04	-0.035
	(1/50)	(5/50)	(4/50)	(42/50)	(14/50)	(15/50)	(41/50)
	[.02]	[.1]	[.08]	[.84]	[.28]	[.3]	[.82]
1992-2000	0.34**	0.103	0.067	-0.01	0.124	-0.119	0.067
	(1/50)	(7/50)	(16/50)	(26/50)	(9/50)	(45/50)	(7/50)
	[.02]	[.14]	[.32]	[.52]	[.18]	[.9]	[.14]
Panel B: Delaware Relative to Bordering States							
1980-1987	0.498***	0.058	0.089	0.026	0.124	0.143***	0.047
	(0.052)	(0.048)	(0.087)	(0.054)	(0.105)	(0.042)	(0.069)
1987-1992	0.206***	0.101**	0.111**	-0.015	0.216*	0.083	-0.019
	(0.014)	(0.031)	(0.041)	(0.032)	(0.107)	(0.052)	(0.054)
1992-2000	0.342***	0.111***	0.070**	0.040	0.159**	-0.109**	0.085
	(0.030)	(0.020)	(0.029)	(0.038)	(0.057)	(0.045)	(0.054)

Note: *** p<0.01, ** p<0.05, * p<0.1. Both panels present the additional employment growth in Delaware relative to the control over the given period relative to this difference in employment growth from 1976 to 1980. The dependent variables in Panel A are (Ln(Empl_t,DE) - Ln(Empl_t,synth)) - (Ln(Empl_t',DE) - Ln(Empl_t',synth)), for (t,t') = (1976,1980), (1980,1987), (1992,1987), (2000,1992). The explanatory variables in Panel A are indicators for 1987, 1992, and 2000 (with 1980 the left-out group). The dependent variables in Panel B are Ln(Empl_t,s) - Ln(Empl_t',s) and the explanatory variables include year fixed effects, and these effects interacted with Delaware. In Panel A, the rank of the effect relative to placebo estimates is in parentheses and the p-value based on this rank is in brackets. In Panel B, robust standard error in parentheses. Estimates in Panel B are the coefficients on year group interacted with Delaware. See paper for details.

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

May 23, 2017

Fiscal and Regulatory Competition from Other States

New York passed a law in 1981 eliminating its usury laws and allowing companies to charge fees, but did not restructure the taxes. In fact, in 1981, there was a temporary 18 percent surchage on tax liability attributable to business in the Metropolitan Commuter Transportation District, later reduced to 17 percent in 1982. This surcharge remains in place today. The tax rate in 1981 for banks was 12 percent, reduced to 9 percent in 1985, to 7.5 percent in 1999 (over three years), and to 7.1 percent in 2007 (Rubin 2011).

In 1983, Virginia eliminated interest rate ceilings on credit card loans, as well as allowed unlimited annual fees and invited out-of-state bank holding companies to acquire a bank. In 1982, Maryland raised the interest rate ceiling to 24 percent but did not allow fees on credit cards or invite out-of-state banks until 1983. The tax on financial institutions in Maryland was 7 percent, both at the time of introduction in 1968 and at repeal in 2000 (Maryland Session Laws 1968, Michie's Annotated Code 2004).

While Pennsyvlania responded as well, it did not match Delaware's policy. In 1982, Pennsylvania raised the interest rate ceiling from 15 percent to 18 percent and also allowed banks to charge a fee of up to $15 per year (Erdevig 1988). Pennsyvlania taxes banks and financial institutions based on their equity capital rather than on their net income (as in Delaware and most other states) (Pennsyvlania Department

of Revenue 2015). From 1971 through 1983, this tax rate was 1.5 percent. In 1984, it was reduced to 1.075 percent, in 1990 it increased to 1.25 percent, and in 2014 it was reduced to 0.89 percent (Pennsylvania Department of Revenue 2008, 2015). The effective rate on net income was estimated to be 9.84 percent based on the 2014 rate, making it higher than the top rate in Delaware (Pennsyvlania Department of Revenue 2015).

A Failed Attempt at Another Regulatory Advantage

The Bank and Trust Company Insurance Powers Act of 1989 allowed state-chartered banks in Delaware to enter the insurance business and to exercise powers incidental to banking (Nolen and Yemc 2011; Swayze and Schiltz 2005). Few other states allowed such powers to banks (Schrader 1990). After the resolution of some policy, legal, and regulatory uncertainty, several banks initiated insurance operations in Delaware in the 1990s. However, a thorough review of newspaper articles and trade journals, as well as a conversation with a corporate attorney involved with this policy, indicated that the response was not large enough to explain the FIRE growth in the 1990s. 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).

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 that 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 one year of college or some college but no degree (CPS codes 80 and 81). I code the education variable as 14 for two years of college or associate's degree, occupational/vocational program, or

academic program (CPS codes 90, 91, and 92). I code the education variable as 15 for three years of college, 16 for four years of college or bachelor's degree (CPS codes 110 and 111), and 18 for anything more than four years of college (CPS codes 120 through 125).

The education codes for the census are slightly different. I code the education variable 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 the education variable as 13 for one year of college; 14 for two years of college; 15 for three years of college; 16 for four years of college; and 17 for five or more years of college (census code 11).

I code as married those who respond that 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.

Policy Impact on Sectoral Composition

Appendix Figure A.1 shows the policy's effect on sectoral composition, and Appendix Table A.14 presents differences-in-differences estimates. The first plot in Appendix Figure A.1 shows the dramatic increase in the share employed in FIRE. Before the policy, approximately 5 percent of nonfarm employment in Delaware, bordering states, and the synthetic control was employed in FIRE. Immediately after the policy, the percentage employed in FIRE grew dramatically in Delaware, reaching 12 percent by 2000. The differences-in-differences estimates are statistically significant, and approximately the same magnitude using both the synthetic control and the bordering states (Appendix Table A.14). In the postpolicy years, the differences-in-differences using the synthetic control are larger than all of the placebo estimates.

Trade, and transportation and utilities became a smaller share of employment in Delaware relative to both the synthetic control and the bordering states. These effects began immediately after the policy and persisted for the next twenty years.

The magnitudes are fairly similar using both control groups, though the synthetic control implies a more negative effect on the share employed in trade in 1993–2000. The effect on the share employed in services is one of the most striking differences between the estimates using the synthetic control and the bordering states. While there is no effect relative to the synthetic control, the results suggest a negative policy effect in Delaware relative to the bordering states (especially in later years). Finally, there is some evidence the policy immediately increased the share employed in construction, which is not captured in Appendix Table A.14 because it was only a short-term effect.

References

[1] Chuang, Tamara. 2000. "Citicorp Will Close in Dover." *News Journal* [Wilmington, DE], January 14, 2000.

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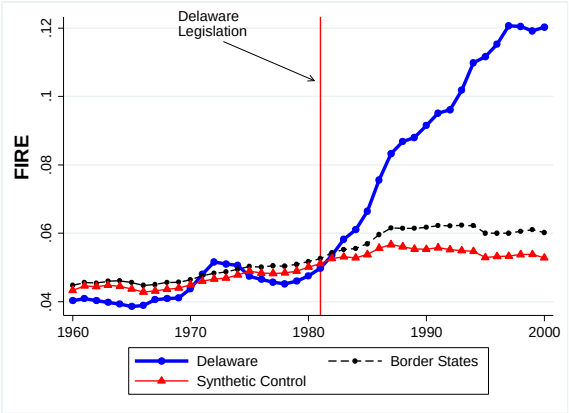
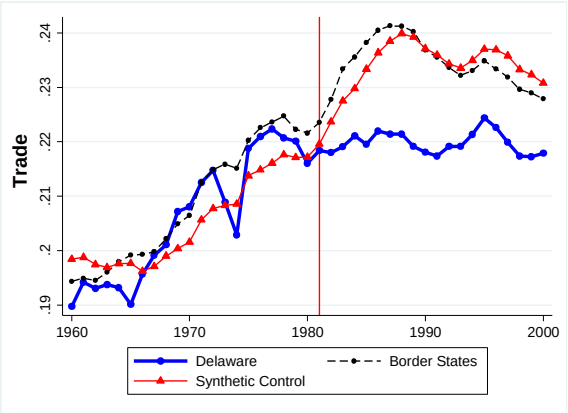
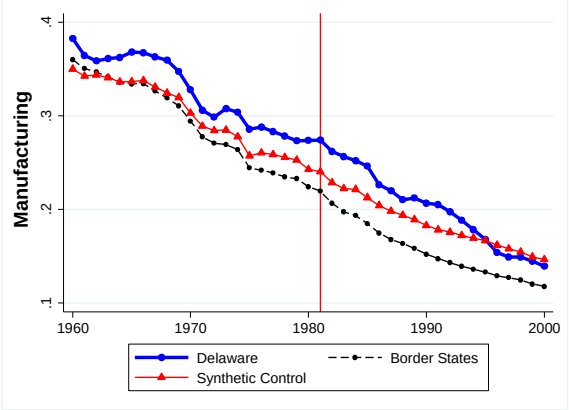
[3] Schrader, M. (1990): "Dual Banking and State Bank Insurance Powers: Diversifying Financial Services Through the Back Door," *Indiana Law Journal,* 66(1).

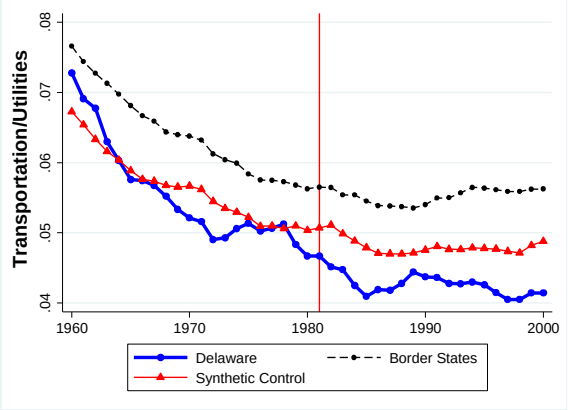
[4] Swayze, D. and C. Schiltz (2005): "The Evolution of Banking in Delaware," *Delaware Banker*, Vo. 1 (4).

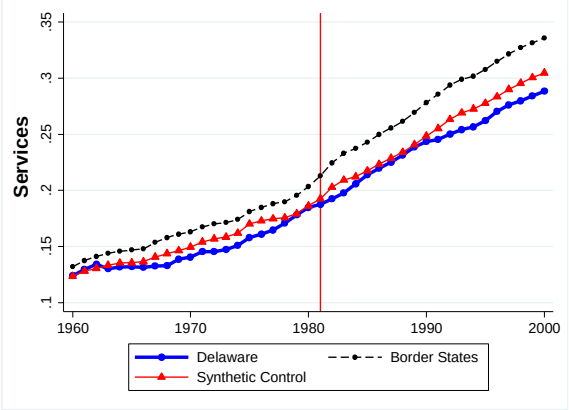
Appendix Figure A.1: Policy Effects on Sectoral Composition:
Delaware vs. Synthetic Control and Bordering States

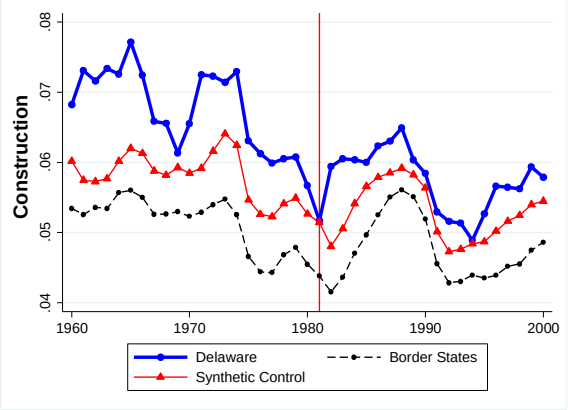


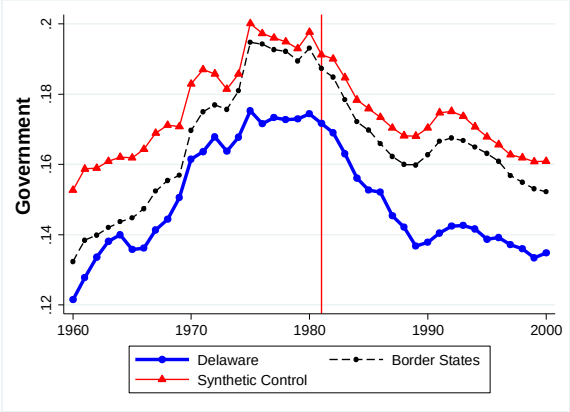






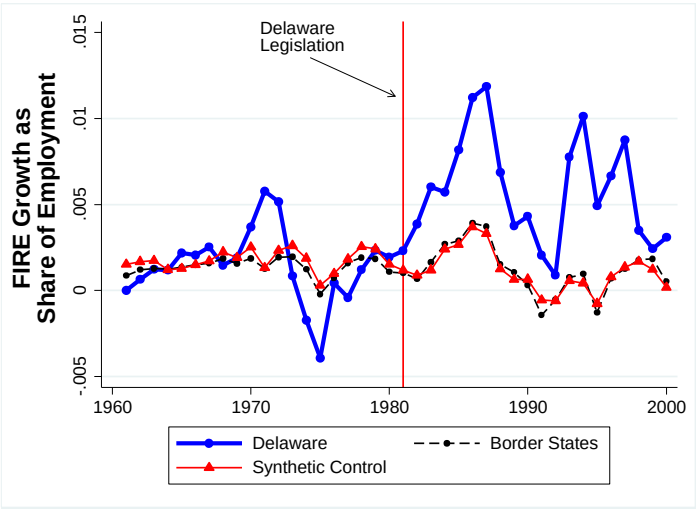


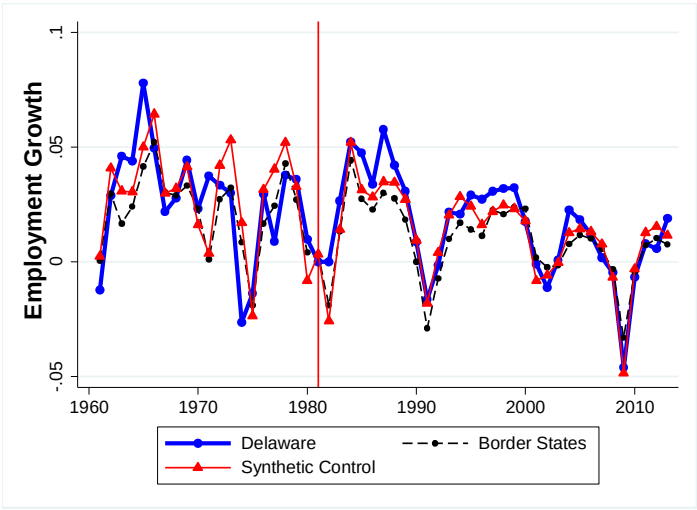


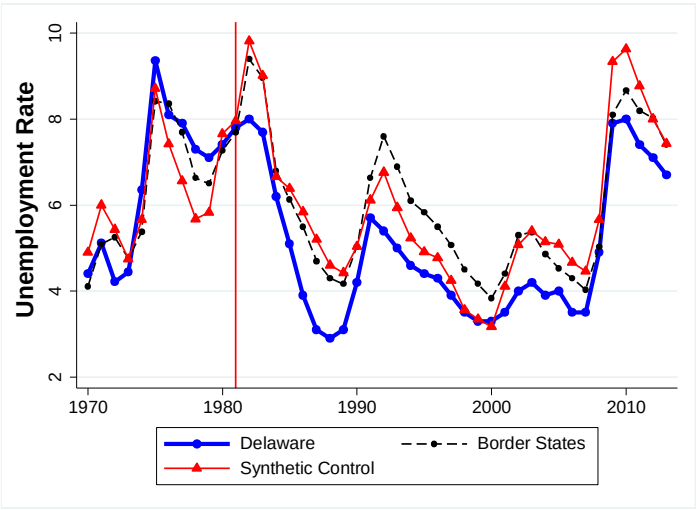


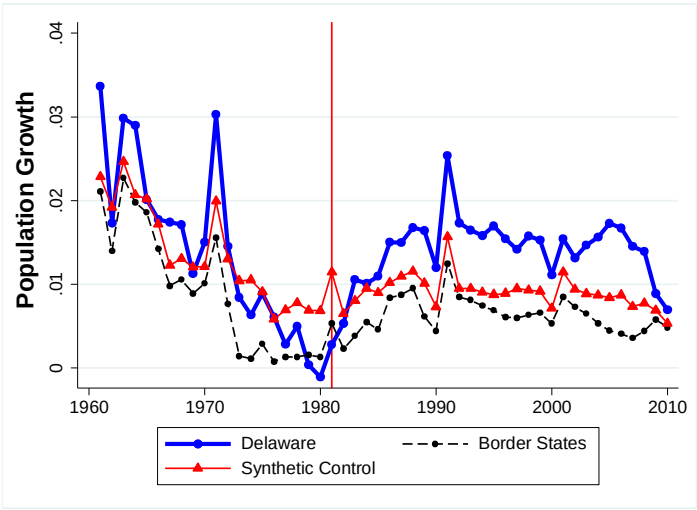
Note: See text for details on construction of the synthetic control.

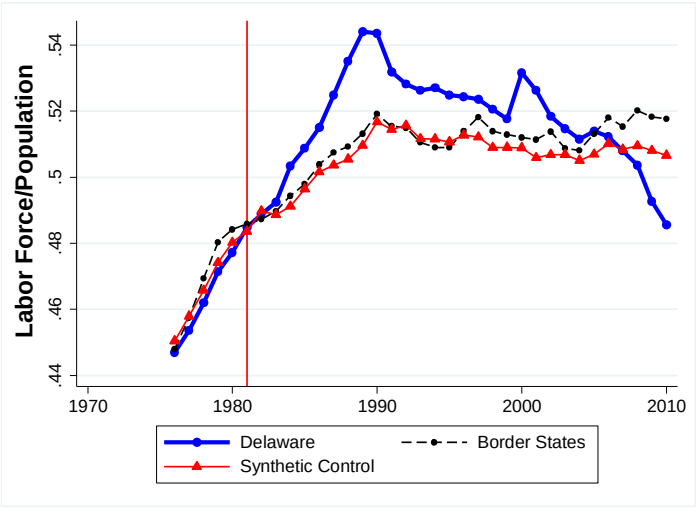
Appendix Figure A.2: Policy Effect on Employment, Unemployment, and Population:
Delaware Relative to the Synthetic Control Allowing the Control to Vary by Outcome

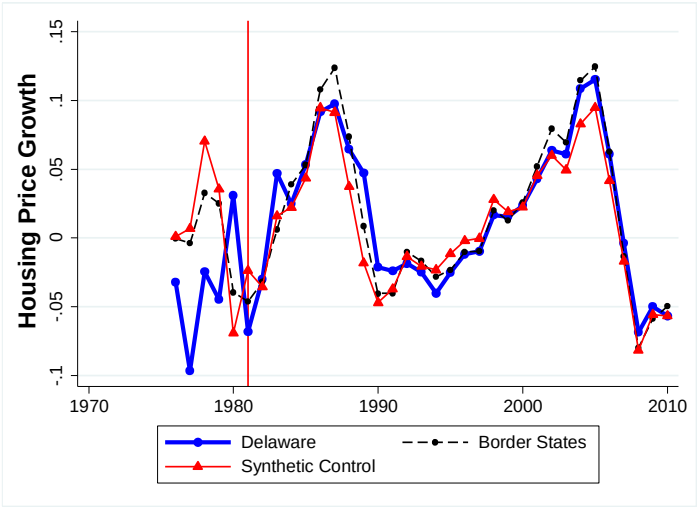






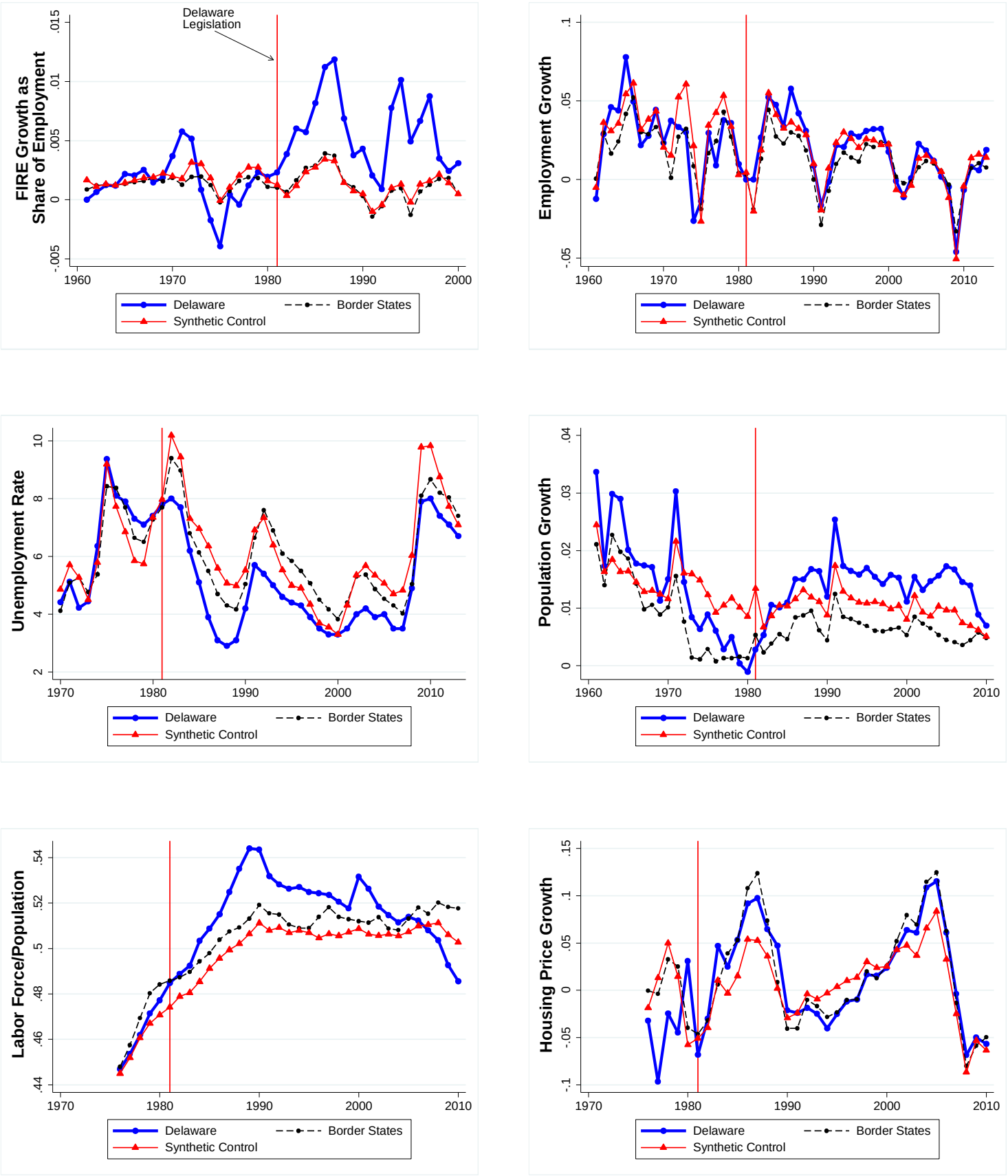






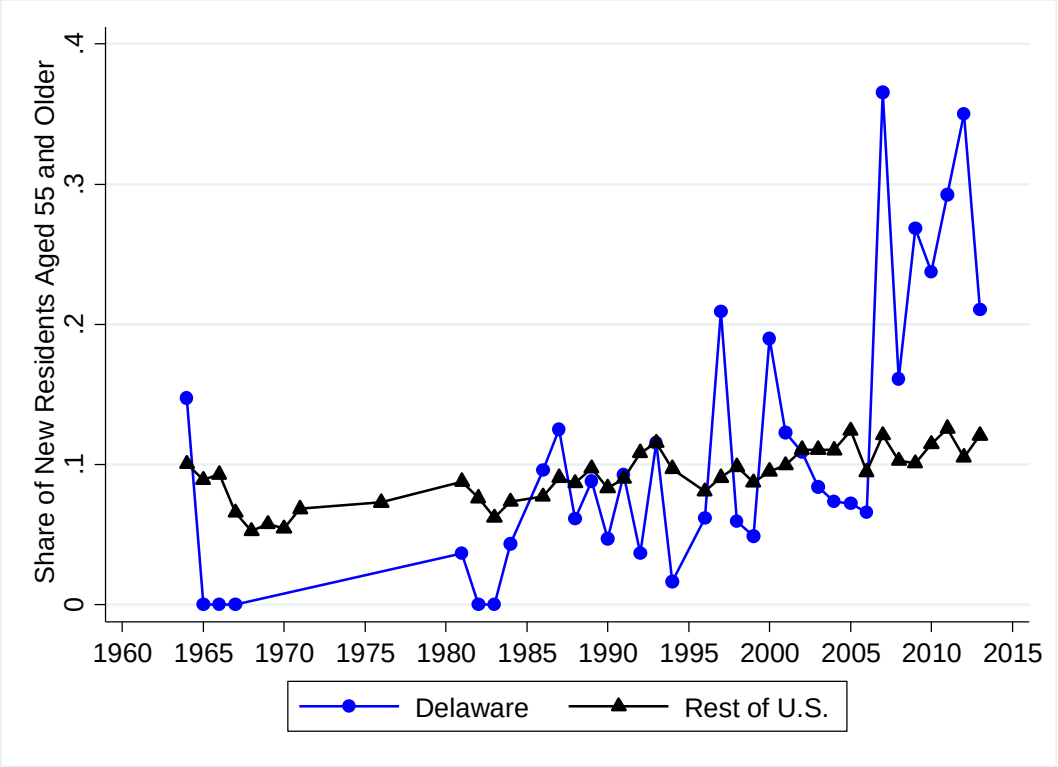
Note: The procedure for constructing the synthetic controls in this figure allows the composition of the control to vary across outcome. See paper for details.

Appendix Figure A.3: Policy Effect on Employment, Unemployment, and Population:
Delaware Relative to the Synthetic Control Excluding States Losing Population to Delaware



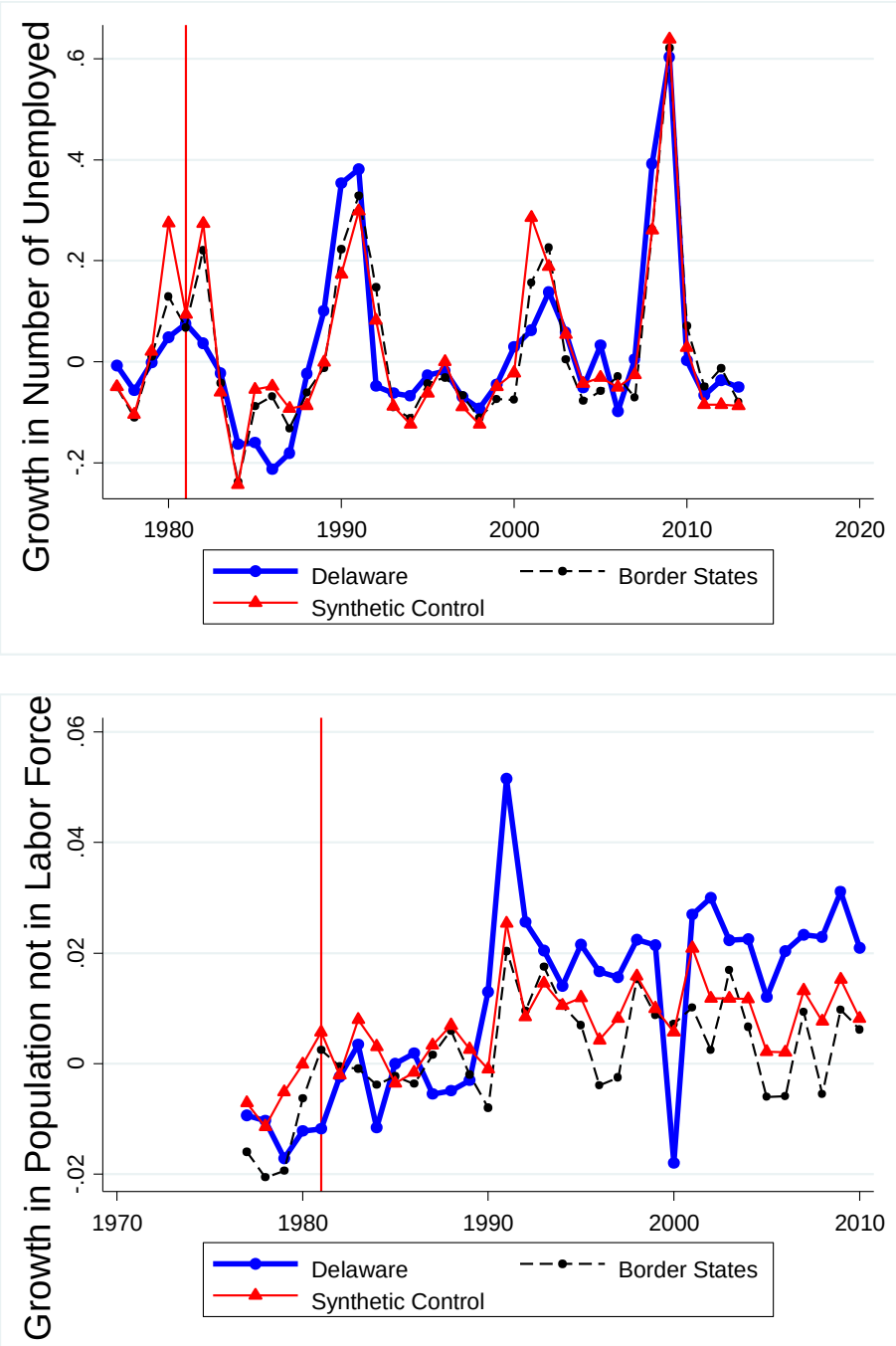
Note: When constructing the synthetic control in this figure, I do not allow the five states losing the most population to Delaware after the policy to enter the control. These include Maryland, Pennsylvania, New Jersey, West Virginia, and Rhode Island. See paper for details.

Appendix Figure A.4: 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 Figure A.5: Growth Rates of Number of Individuals Unemployed and Out-of-Labor Force



Note: See paper for details on construction of synthetic control.

Appendix Table A.1: Policy Effect on Employment, Unemployment, and Population, Delaware vs. Bordering States

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Employment Growth	FIRE Employment Growth as a Share of Employment	Unemployment Rate	Population Growth	Labor Force/ Population	Housing Price Growth
Pre-1976	-0.001	-0.000	-1.789***	0.011***		
	(0.005)	(0.000)	(0.518)	(0.002)		
1981-1987	-0.006	0.001***	-0.265	0.004***	0.027***	0.032*
	(0.006)	(0.000)	(0.577)	(0.001)	(0.008)	(0.018)
1988-1992	-0.021***	-0.001***	-1.747***	0.007***	0.047***	-0.006
	(0.007)	(0.000)	(0.508)	(0.002)	(0.008)	(0.015)
1993-2000	-0.005	-0.001**	-2.056***	0.005***	0.045***	-0.008
	(0.004)	(0.000)	(0.403)	(0.001)	(0.007)	(0.009)
2001+	-0.022***		-1.275***	0.004***	0.047***	0.012
	(0.005)		(0.445)	(0.001)	(0.006)	(0.014)
Pre-1976*DE	0.004	0.001	-0.122	0.005*		
	(0.010)	(0.001)	(0.941)	(0.003)		
1981-1987*DE	0.012	0.005***	-1.324	0.003	0.013	0.026
	(0.012)	(0.001)	(0.940)	(0.002)	(0.011)	(0.033)
1988-1992*DE	0.010	0.004***	-1.553**	0.008***	0.028***	0.043
	(0.013)	(0.001)	(0.756)	(0.003)	(0.010)	(0.029)
1993-2000*DE	0.007	0.005***	-1.467***	0.007***	0.018**	0.028
	(0.007)	(0.001)	(0.491)	(0.002)	(0.009)	(0.021)
2001+*DE	-0.001		-1.008	0.007***	-0.000	0.027
	(0.009)		(0.695)	(0.002)	(0.009)	(0.029)
DE	0.001	-0.000	0.267	0.001	-0.006	-0.031*
	(0.007)	(0.000)	(0.375)	(0.001)	(0.008)	(0.019)
Observations	212	160	176	200	140	156
R-squared	0.241	0.536	0.230	0.359	0.546	0.072

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors in parentheses. Dependent variables are measured at the state/year level. These estimates are from a differences-in-differences regression comparing Delaware to bordering states: New Jersey, Maryland, and Pennsylvania. No data exist for participation and housing price growth before 1976, and no data exist for FIRE growth (using the SIC industry definition) after 2000. See paper for details.

Appendix Table A.2: Policy Effects on Employment, Unemployment, and Population by Year:

Relative to Five Years Pre-Policy, Delaware vs. Synthetic Control

	FIRE Employment Growth as a Share of Employment	Total Employment Growth	Unemployment Rate	Population Growth	Labor Force/ Population	Housing Price Growth
1981	0.002	0.001	-1.019	-0.003	0.006	0.005
	(5/50)	(22/50)	(9/50)	(36/50)	(11/50)	(26/50)
	[.1]	[.44]	[.18]	[.72]	[.22]	[.52]
1982	0.004	0.029	-2.84	0.004	0.006	0.033
	(2/50)	(4/50)	(3/50)	(10/50)	(12/50)	(13/50)
	[.04]	[.08]	[.06]	[.2]	[.24]	[.26]
1983	0.005	0.014	-2.486	0.008	0.009	0.084
	(1/50)	(9/50)	(3/50)	(7/50)	(9/50)	(3/50)
	[.02]	[.18]	[.06]	[.14]	[.18]	[.06]
1984	0.004	0.006	-1.631	0.006	0.017	0.057
	(2/50)	(18/50)	(5/50)	(9/50)	(4/50)	(8/50)
	[.04]	[.36]	[.1]	[.18]	[.08]	[.16]
1985	0.006	0.019	-2.2	0.007	0.016	0.07
	(1/50)	(4/50)	(3/50)	(8/50)	(6/50)	(5/50)
	[.02]	[.08]	[.06]	[.16]	[.12]	[.1]
1986	0.008	0.011	-2.897	0.009	0.016	0.07
	(1/50)	(12/50)	(2/50)	(8/50)	(2/50)	(5/50)
	[.02]	[.24]	[.04]	[.16]	[.04]	[.1]
1987	0.01	0.028	-3.023	0.009	0.021	0.076
	(1/50)	(2/50)	(2/50)	(9/50)	(3/50)	(4/50)
	[.02]	[.04]	[.04]	[.18]	[.06]	[.08]
1988	0.006	0.013	-2.668	0.01	0.029	0.056
	(1/50)	(9/50)	(2/50)	(5/50)	(1/50)	(7/50)
	[.02]	[.18]	[.04]	[.1]	[.02]	[.14]
1989	0.004	0.009	-2.301	0.011	0.033	0.072
	(2/50)	(15/50)	(4/50)	(3/50)	(1/50)	(3/50)
	[.04]	[.3]	[.08]	[.06]	[.02]	[.06]
1990	0.004	0.004	-1.86	0.009	0.028	0.035
	(1/50)	(21/50)	(4/50)	(5/50)	(1/50)	(6/50)
	[.02]	[.42]	[.08]	[.1]	[.02]	[.12]
1991	0.004	0.011	-1.773	0.013	0.02	0.03
	(1/50)	(15/50)	(3/50)	(3/50)	(2/50)	(8/50)
	[.02]	[.3]	[.06]	[.06]	[.04]	[.16]
1992	0.002	0.001	-2.517	0.011	0.015	0.019
	(4/50)	(26/50)	(2/50)	(3/50)	(6/50)	(13/50)
	[.08]	[.52]	[.04]	[.06]	[.12]	[.26]
1993	0.008	0.008	-2.214	0.011	0.015	0.02
	(1/50)	(14/50)	(1/50)	(4/50)	(7/50)	(13/50)
	[.02]	[.28]	[.02]	[.08]	[.14]	[.26]

	FIRE Employment Growth as a Share of Employment	Total Employment Growth	Unemployment Rate	Population Growth	Labor Force/ Population	Housing Price Growth
1994	0.01	0.001	-1.75	0.011	0.015	0.003
	(1/50)	(25/50)	(2/50)	(3/50)	(9/50)	(24/50)
	[.02]	[.5]	[.04]	[.06]	[.18]	[.48]
1995	0.006	0.013	-1.527	0.012	0.014	0.011
	(1/50)	(6/50)	(3/50)	(3/50)	(14/50)	(15/50)
	[.02]	[.12]	[.06]	[.06]	[.28]	[.3]
1996	0.006	0.017	-1.519	0.011	0.011	0.017
	(1/50)	(3/50)	(4/50)	(2/50)	(15/50)	(11/50)
	[.02]	[.06]	[.08]	[.04]	[.3]	[.22]
1997	0.008	0.013	-1.446	0.009	0.009	0.017
	(1/50)	(4/50)	(3/50)	(3/50)	(16/50)	(12/50)
	[.02]	[.08]	[.06]	[.06]	[.32]	[.24]
1998	0.002	0.013	-1.263	0.011	0.008	0.024
	(5/50)	(4/50)	(3/50)	(3/50)	(17/50)	(7/50)
	[.1]	[.08]	[.06]	[.06]	[.34]	[.14]
1999	0.002	0.015	-1.225	0.011	0.006	0.032
	(5/50)	(4/50)	(3/50)	(4/50)	(20/50)	(7/50)
	[.1]	[.08]	[.06]	[.08]	[.4]	[.14]
2000	0.004	0.003	-1.098	0.009	0.019	0.04
	(2/50)	(22/50)	(6/50)	(3/50)	(5/50)	(7/50)
	[.04]	[.44]	[.12]	[.06]	[.1]	[.14]
2001		0.01	-1.886	0.009	0.018	0.037
		(12/50)	(2/50)	(4/50)	(7/50)	(4/50)
		[.24]	[.04]	[.08]	[.14]	[.08]
2002		0.001	-2.183	0.009	0.011	0.047
		(23/50)	(1/50)	(4/50)	(11/50)	(6/50)
		[.46]	[.02]	[.08]	[.22]	[.12]
2003		0.009	-2.253	0.011	0.009	0.052
		(10/50)	(1/50)	(3/50)	(13/50)	(4/50)
		[.2]	[.02]	[.06]	[.26]	[.08]
2004		0.017	-2.272	0.012	0.007	0.067
		(2/50)	(1/50)	(3/50)	(18/50)	(5/50)
		[.04]	[.02]	[.06]	[.36]	[.1]
2005		0.01	-1.943	0.014	0.006	0.057
		(10/50)	(2/50)	(2/50)	(20/50)	(2/50)
		[.2]	[.04]	[.04]	[.4]	[.04]
2006		0.005	-2.114	0.012	0	0.048
		(17/50)	(2/50)	(4/50)	(28/50)	(6/50)
		[.34]	[.04]	[.08]	[.56]	[.12]
2007		0	-1.939	0.012	-0.002	0.042
		(27/50)	(2/50)	(3/50)	(30/50)	(5/50)
		[.54]	[.04]	[.06]	[.6]	[.1]

	FIRE Employment Growth as a Share of Employment	Total Employment Growth	Unemployment Rate	Population Growth	Labor Force/ Population	Housing Price Growth
2008		0.01	-1.681	0.012	-0.006	0.042
		(10/50)	(3/50)	(2/50)	(31/50)	(8/50)
		[.2]	[.06]	[.04]	[.62]	[.16]
2009		0.006	-2.348	0.007	-0.013	0.037
		(17/50)	(3/50)	(5/50)	(37/50)	(7/50)
		[.34]	[.06]	[.1]	[.74]	[.14]
2010		0.002	-2.423	0.007	-0.019	0.037
		(22/50)	(4/50)	(6/50)	(42/50)	(4/50)
		[.44]	[.08]	[.12]	[.84]	[.08]
2011		0.002	-2.148			
		(21/50)	(6/50)			
		[.42]	[.12]			
2012		-0.003	-1.633			
		(30/50)	(6/50)			
		[.6]	[.12]			
2013		0.012	-1.325			
		(7/50)	(10/50)			
		[.14]	[.2]			

Note: These estimates are from a differences-in-differences specification comparing Delaware to the synthetic control in each post-policy year, relative to the five years preceding the policy. The rank of the effect relative to placebo estimates is in parentheses. P-value based on this rank is in brackets. See paper for details.

Appendix Table A.3: Policy Effects on Employment, Unemployment, and Population by Year:

Relative to Five Years Pre-Policy, Delaware vs. Bordering States

	Total Employment Growth	FIRE Employment Growth as a Share of Employment	Unemployment Rate	Population Growth	Labor Force/ Population	Housing Price Growth
1981	-0.005	0.002***	-0.167	-0.004	0.005	0.009
	(0.010)	(0.001)	(0.601)	(0.004)	(0.018)	(0.028)
1982	0.018	0.004***	-1.667	0.002	0.007	0.033
	(0.012)	(0.001)	(1.113)	(0.002)	(0.018)	(0.026)
1983	0.012	0.005***	-1.533	0.005*	0.008	0.072***
	(0.015)	(0.001)	(1.477)	(0.003)	(0.018)	(0.025)
1984	0.007	0.003***	-0.867	0.003	0.015	0.017
	(0.011)	(0.001)	(1.339)	(0.004)	(0.019)	(0.038)
1985	0.019*	0.006***	-1.300	0.005	0.016	0.032
	(0.011)	(0.001)	(1.167)	(0.005)	(0.018)	(0.040)
1986	0.010	0.008***	-1.867**	0.005	0.017	0.015
	(0.010)	(0.001)	(0.880)	(0.005)	(0.018)	(0.049)
1987	0.026***	0.008***	-1.867***	0.005	0.023	0.005
	(0.010)	(0.001)	(0.706)	(0.005)	(0.019)	(0.039)
1988	0.013	0.006***	-1.667***	0.006	0.031*	0.022
	(0.010)	(0.001)	(0.601)	(0.006)	(0.018)	(0.027)
1989	0.011	0.003***	-1.333**	0.009*	0.036**	0.070*
	(0.009)	(0.001)	(0.558)	(0.005)	(0.017)	(0.038)
1990	0.007	0.004***	-1.100**	0.006	0.030	0.051
	(0.011)	(0.001)	(0.527)	(0.004)	(0.019)	(0.034)
1991	0.011	0.004***	-1.200**	0.012***	0.022	0.047
	(0.010)	(0.001)	(0.520)	(0.003)	(0.020)	(0.030)
1992	0.005	0.002***	-2.467***	0.007***	0.019	0.023
	(0.009)	(0.001)	(0.744)	(0.002)	(0.019)	(0.024)
1993	0.011	0.007***	-2.167***	0.007***	0.021	0.023
	(0.008)	(0.001)	(0.682)	(0.002)	(0.018)	(0.024)
1994	0.002	0.009***	-1.767**	0.007***	0.024	0.019
	(0.009)	(0.001)	(0.682)	(0.002)	(0.019)	(0.024)
1995	0.014	0.007***	-1.700***	0.009***	0.022	0.029
	(0.009)	(0.001)	(0.632)	(0.003)	(0.019)	(0.024)
1996	0.015*	0.006***	-1.467**	0.008***	0.016	0.030
	(0.008)	(0.001)	(0.613)	(0.003)	(0.018)	(0.024)
1997	0.007	0.008***	-1.433***	0.007**	0.011	0.031
	(0.009)	(0.001)	(0.491)	(0.003)	(0.016)	(0.024)
1998	0.010	0.002***	-1.267***	0.008***	0.012	0.028
	(0.009)	(0.001)	(0.478)	(0.003)	(0.016)	(0.024)
1999	0.007	0.001	-1.133**	0.007**	0.010	0.034
	(0.009)	(0.001)	(0.554)	(0.003)	(0.015)	(0.025)

	Total Employment Growth	FIRE Employment Growth as a Share of Employment	Unemployment Rate	Population Growth	Labor Force/ Population	Housing Price Growth
2000	-0.007	0.003***	-0.800	0.004*	0.025	0.029
	(0.009)	(0.001)	(0.505)	(0.003)	(0.015)	(0.029)
2001	-0.004		-1.167**	0.006	0.021	0.022
	(0.009)		(0.515)	(0.004)	(0.013)	(0.026)
2002	-0.010		-1.567**	0.004	0.010	0.016
	(0.009)		(0.631)	(0.003)	(0.012)	(0.030)
2003	0.001		-1.433**	0.007***	0.012	0.023
	(0.009)		(0.655)	(0.003)	(0.013)	(0.028)
2004	0.014		-1.233**	0.009***	0.009	0.025
	(0.009)		(0.574)	(0.003)	(0.012)	(0.034)
2005	0.005		-0.800	0.011***	0.006	0.022
	(0.009)		(0.541)	(0.002)	(0.013)	(0.038)
2006	0.001		-1.067**	0.011***	-0.000	0.030
	(0.009)		(0.536)	(0.002)	(0.014)	(0.029)
2007	-0.005		-0.800	0.010***	-0.002	0.041
	(0.009)		(0.574)	(0.002)	(0.013)	(0.026)
2008	-0.003		-0.400	0.008***	-0.011	0.043
	(0.009)		(0.608)	(0.002)	(0.012)	(0.031)
2009	-0.014		-0.467	0.002	-0.020	0.040
	(0.009)		(0.682)	(0.002)	(0.013)	(0.032)
2010	-0.004		-0.933	0.001	-0.026*	0.024
	(0.009)		(0.701)	(0.002)	(0.014)	(0.025)
2011	-0.000		-1.067			0.009
	(0.009)		(0.773)			(0.025)
2012	-0.006		-1.200			0.026
	(0.009)		(0.868)			(0.025)
2013	0.010		-0.967			0.029
	(0.009)		(0.673)			(0.024)
Observations	212	160	176	200	140	156
R-squared	0.567	0.817	0.736	0.409	0.607	0.832

Note: *** p<0.01, ** p<0.05, * p<0.1. These coefficients are the interactions between the year and Delaware. They are from a differences-in-differences specification comparing Delaware to bordering states in each post-policy year relative to the five years preceding the policy. Robust standard errors in parentheses. See paper for details.

Appendix Table A.4: Policy Effect on Wages: Yearly Estimates

	(1)	(2)
1960	0.03***	0.02
	(0.04)	(0.04)
1970	-0.002**	0.01
	(0.04)	(0.04)
1977-1981	-0.04	-0.003
	(0.04)	(0.04)
1982	-0.03	-0.002
	(0.05)	(0.05)
1983	0.01**	0.03
	(0.05)	(0.05)
1984	-0.04	-0.02
	(0.05)	(0.05)
1985	-0.003	0.03
	(0.05)	(0.05)
1986	-0.05	-0.06*
	(0.05)	(0.05)
1987	-0.01	-0.02
	(0.05)	(0.05)
1988	0.005*	-0.0004
	(0.05)	(0.05)
Control	All States	Bordering States
N	10,089,651	1,075,720

Note: This table shows the coefficients on the interaction between Delaware and year group, relative to all states (column 1) and bordering states (column 2). These coefficients are from a regression including an indicator for Delaware, each post-policy year, the year group 1977-1981, the earlier census years (omitting 1950), and those indicators interacted with Delaware (omitting the interaction with 1950). I show in this table only the interactions for the pre-policy years and the first eight post-policy years. Asterisks denote whether the difference relative to the coefficient on Delaware*1977-1981 is statistically significant. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses, as these are larger than clustered standard errors. In the regressions including all states, I control for state characteristics in the pre-policy period, individual characteristics, and occupation and industry fixed effects. In the regressions including bordering states, I control for individual characteristics and occupation and industry fixed effects. See text of the paper for further details.

Appendix Table A.5: Policy Effect on Employment Growth in FIRE and Other Industries:
Differences-in-Differences Relative to Five Years Pre-Policy

	FIRE	Trade	Transportation/ Utilties	Services	Construction	Manufacturing	Government
1980-1987	0.165**	0.111**	-0.004	0.155**	0.181	-0.188***	-0.124
	(0.052)	(0.048)	(0.087)	(0.054)	(0.105)	(0.042)	(0.069)
1987-1992	-0.112***	-0.117***	-0.042	-0.046	-0.368***	-0.167**	-0.047
	(0.014)	(0.031)	(0.041)	(0.032)	(0.107)	(0.052)	(0.054)
1992-2000	-0.022	0.022	0.089**	0.082*	0.150**	-0.073	-0.040
	(0.030)	(0.020)	(0.029)	(0.038)	(0.057)	(0.045)	(0.054)
1980-1987*DE	0.498***	0.058	0.089	0.026	0.124	0.143***	0.047
	(0.052)	(0.048)	(0.087)	(0.054)	(0.105)	(0.042)	(0.069)
1987-1992*DE	0.206***	0.101**	0.111**	-0.015	0.216*	0.083	-0.019
	(0.014)	(0.031)	(0.041)	(0.032)	(0.107)	(0.052)	(0.054)
1992-2000*DE	0.342***	0.111***	0.070**	0.040	0.159**	-0.109**	0.085
	(0.030)	(0.020)	(0.029)	(0.038)	(0.057)	(0.045)	(0.054)
DE	-0.017*	-0.025	-0.056*	0.036	-0.110**	0.024*	0.022
	(0.009)	(0.014)	(0.027)	(0.026)	(0.042)	(0.013)	(0.052)
Observations	16	16	16	16	16	16	16
R-squared	0.971	0.883	0.510	0.862	0.880	0.771	0.646

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors in parentheses. These estimates are from a differences-in-differences regression comparing Delaware to bordering states: New Jersey, Maryland, and Pennsylvania. The dependent variables are Ln(Empl_t,s) - Ln(Empl_t',s) for (t,t') = (1976,1980), (1980,1987), (1992,1987), (2000,1992). The explanatory variables include year fixed effects, and these effects interacted with Delaware. Coefficients on the interaction with Delaware represent the additional employment growth in Delaware relative to the control over the given period relative to this difference in employment growth from 1976 to 1980. See paper for details.

Appendix Table A.6: Composition of Synthetic Control by Outcome

	FIRE Employment Growth as a Share of Employment	Total Employment Growth	Unemployment Rate	Population Growth	Labor Force/ Population	Housing Price Growth
Indiana	0.393	0.371	0.356	0.095	0	0.205
Connecticut	0.22	0.174	0.216	0.257	0.198	0.358
Maryland	0.205	0.243	0.167	0.228	0.159	0.214
South Carolina	0.066	0.074	0.063	0.129	0.09	0.065
Arizona	0.041	0.04	0.063	0	0.129	0
Ohio	0.027	0.039	0.056	0.15	0.259	0
Florida	0	0	0.029	0.08	0	0.084
Nevada	0.036	0.033	0.021	0.006	0.003	0.008
Alaska	0.012	0.018	0.017	0.016	0	0.032
Vermont	0	0	0.012	0	0	0
Louisiana	0	0	0	0	0.02	0
New Hampshire	0	0.009	0	0	0	0
North Carolina	0	0	0	0.039	0.142	0.034

Note: The construction of the synthetic controls in this table allows for the composition of the control to vary across outcomes. In particular, the weights on the predictor variables are determined by a regression-based method, rather than each being assigned a weight of one (as in the principal results). See paper for details.

Appendix Table A.7: Pre-policy Characteristics of Delaware, Bordering States, and the Synthetic Control
Allowing the Synthetic Control to Vary Across Outcomes

	(1)	(2)	(3)	(4)	(5)
Predictors	**Delaware**	**Bordering States**	**Synthetic Control for Unemployment Rate**	**Synthetic Control for Population Growth**	**Synthetic Control for Employment Growth**
Share Employed in					
FIRE					
1965-1969	0.04	0.05	0.05	0.05	0.04
1970-1974	0.05	0.05	0.05	0.05	0.05
1975-1979	0.05	0.05	0.05	0.05	0.05
Manufacturing					
1965-1969	0.36	0.32	0.34	0.34	0.34
1970-1974	0.31	0.28	0.30	0.30	0.30
1975-1979	0.28	0.24	0.27	0.27	0.27
Trade					
1965-1969	0.2	0.2	0.20	0.20	0.20
1970-1974	0.21	0.21	0.21	0.21	0.21
1975-1979	0.22	0.22	0.22	0.22	0.22
Services					
1965-1969	0.13	0.15	0.14	0.14	0.14
1970-1974	0.15	0.17	0.16	0.16	0.16
1975-1979	0.17	0.19	0.17	0.18	0.18
% Metropolitan					
1960	68.9	81.5	60.8	63.6	60.9
1970	70.4	83.8	69.2	70.5	69.6
1980	67.0	87.4	77.0	79.6	77.3
% with ≥ a HS Diploma					
1960	43.3	39.6	42.0	40.4	41.7
1970	54.6	51.7	53.4	51.4	53.1
1980	68.6	66.5	67.5	66.0	67.4
Unemployment Rate					
1970-1974	4.9	4.9	5.3	5.3	5.2
1975-1979	8.0	7.5	6.8	6.8	6.7
1980	7.4	7.3	7.7	7.0	7.7
Population Growth					
1966-1970	0.02	0.01	0.01	0.01	0.01
1976-1980	0	0	0.01	0.01	0.01

Note: This table compares the balance of predictor variables in the synthetic control, Delaware, and the states bordering Delaware. The procedure for constructing the synthetic controls allows the composition of the control to vary across outcomes, by setting weights on predictor variables based on a regression-based method. Not all predictors are shown. For the full set of predictors, see text.

Appendix Table A.8: Policy Effect on Employment, Unemployment, and Population:
Differences-in-Differences Relative to Five Years Pre-Policy, Allowing the Synthetic Control to Vary Across Outcomes

	Total Employment Growth	FIRE Employment Growth as a Share of Employment	Unemployment Rate	Population Growth	Labor Force/ Population	Housing Price Growth
Panel A: Delaware Relative to Synthetic Control						
Pre-1976	0.004	0.001	-1.19*	0.007*		
	(12/50)	(6/50)	(3/50)	(5/50)		
	[.24]	[.12]	[.06]	[.1]		
1981-1987	0.017*	0.006**	-2.228**	0.005	0.013*	0.043
	(4/50)	(1/50)	(2/50)	(8/50)	(5/50)	(9/50)
	[.08]	[.02]	[.04]	[.16]	[.1]	[.18]
1988-1992	0.007	0.004**	-2.059*	0.011**	0.028**	0.068**
	(17/50)	(1/50)	(3/50)	(2/50)	(1/50)	(2/50)
	[.34]	[.02]	[.06]	[.04]	[.02]	[.04]
1993-2000	0.01	0.006**	-1.292*	0.01*	0.017	0.034
	(8/50)	(1/50)	(5/50)	(4/50)	(6/50)	(8/50)
	[.16]	[.02]	[.1]	[.08]	[.12]	[.16]
2001+	0.006		-2.023**	0.01*	0.005	0.053**
	(10/50)		(2/50)	(3/50)	(23/50)	(2/50)
	[.2]		[.04]	[.06]	[.46]	[.04]
Panel B: Delaware Relative to Bordering States						
Pre-1976	0.004	0.001	-0.122	0.005*		
	(0.010)	(0.001)	(0.941)	(0.003)		
1981-1987	0.012	0.005***	-1.324	0.003	0.013	0.026
	(0.012)	(0.001)	(0.940)	(0.002)	(0.011)	(0.033)
1988-1992	0.010	0.004***	-1.553**	0.008***	0.028***	0.043
	(0.013)	(0.001)	(0.756)	(0.003)	(0.010)	(0.029)
1993-2000	0.007	0.005***	-1.467***	0.007***	0.018**	0.028
	(0.007)	(0.001)	(0.491)	(0.002)	(0.009)	(0.021)
2001+	-0.001		-1.008	0.007***	-0.000	0.027
	(0.009)		(0.695)	(0.002)	(0.009)	(0.029)

Note: *** p<0.01, ** p<0.05, * p<0.1. Both panels present differences-in-differences estimates. In Panel A, rank of effect relative to placebo estimates in parentheses and p-value based on this rank in brackets. In Panel B, robust standard error in parentheses. The construction of the synthetic control used in Panel A allows for the composition to vary across outcomes. In particular, the weights on the predictor variables are determined by a regression-based method, rather than each being assigned a weight of one (as in the principal results). Estimates in Panel B are the coefficients on year group interacted with Delaware. See paper for details.

Appendix Table A.9: Composition of Synthetic Control:

Excluding Five States Losing Most Population to Delaware After the Policy

Virginia	0.243
Ohio	0.17
Michigan	0.159
Florida	0.131
South Carolina	0.097
Connecticut	0.078
North Carolina	0.068
Vermont	0.041
Alaska	0.013

Note: This table presents the composition of the synthetic control when excluding the five states losing the most population to Delaware after the policy (relative to before the policy). This composition is the same across all outcomes, since the weight on the predictor variables is set to one for each variable. See paper for details.

Appendix Table A.10: Pre-policy Characteristics of Delaware, Bordering States, and the Synthetic Control

Excluding from the Synthetic Control the States Losing the Most Population to Delaware After the Policy

	(1)	(2)	(3)
Predictors	**Delaware**	**Bordering States**	**Synthetic Control**
Share Employed in			
FIRE			
1965-1969	0.04	0.05	0.04
1970-1974	0.05	0.05	0.05
1975-1979	0.05	0.05	0.05
Manufacturing			
1965-1969	0.36	0.32	0.33
1970-1974	0.31	0.28	0.29
1975-1979	0.28	0.24	0.26
Trade			
1965-1969	0.2	0.2	0.20
1970-1974	0.21	0.21	0.20
1975-1979	0.22	0.22	0.21
Services			
1965-1969	0.13	0.15	0.14
1970-1974	0.15	0.17	0.15
1975-1979	0.17	0.19	0.17
% Metropolitan			
1960	68.9	81.5	55.2
1970	70.4	83.8	62.1
1980	67.0	87.4	73.0
% with ≥ a HS Diploma			
1960	43.3	39.6	39.4
1970	54.6	51.7	49.8
1980	68.6	66.5	64.5
Unemployment Rate			
1970-1974	4.9	4.9	5.2
1975-1979	8.0	7.5	7.1
1980	7.4	7.3	7.3
Population Growth			
1966-1970	0.02	0.01	0.01
1976-1980	0	0	0.01

Note: This table compares the balance of predictor variables in the synthetic control, Delaware, and the states bordering Delaware. The procedure for constructing the synthetic control did not allow the five states losing the most population to Delaware after the policy (relative to before) to enter the control. Not all predictors are shown. For the full set of predictors, see text.

Appendix Table A.11: Policy Effect on Employment, Unemployment, and Population:
Differences-in-Differences Relative to Five Years Pre-Policy, Excluding States Losing Most Population to Delaware from Synthetic Control

	Total Employment Growth	FIRE Employment Growth as a Share of Employment	Unemployment Rate	Population Growth	Labor Force/ Population	Housing Price Growth
Panel A: Delaware Relative to Synthetic Control						
Pre-1976	0.005	0.001	-1.093*	0.01**		
	(9/45)	(5/45)	(3/45)	(2/45)		
	[.2]	[.111]	[.067]	[.044]		
1981-1987	0.016*	0.006**	-2.573**	0.007	0.013	0.059
	(3/45)	(1/45)	(2/45)	(5/45)	(5/45)	(5/45)
	[.067]	[.022]	[.044]	[.111]	[.111]	[.111]
1988-1992	0.01	0.004**	-2.562**	0.013**	0.026**	0.047*
	(13/45)	(1/45)	(2/45)	(2/45)	(1/45)	(3/45)
	[.289]	[.022]	[.044]	[.044]	[.022]	[.067]
1993-2000	0.011*	0.006**	-1.406*	0.012**	0.015	0.015
	(4/45)	(1/45)	(3/45)	(2/45)	(10/45)	(11/45)
	[.089]	[.022]	[.067]	[.044]	[.222]	[.244]
2001+	0.01		-2.081**	0.013**	-0.002	0.053**
	(6/45)		(1/45)	(2/45)	(28/45)	(2/45)
	[.133]		[.022]	[.044]	[.622]	[.044]
Panel B: Delaware Relative to Bordering States						
Pre-1976	0.004	0.001	-0.122	0.005*		
	(0.010)	(0.001)	(0.941)	(0.003)		
1981-1987	0.012	0.005***	-1.324	0.003	0.013	0.026
	(0.012)	(0.001)	(0.940)	(0.002)	(0.011)	(0.033)
1988-1992	0.010	0.004***	-1.553**	0.008***	0.028***	0.043
	(0.013)	(0.001)	(0.756)	(0.003)	(0.010)	(0.029)
1993-2000	0.007	0.005***	-1.467***	0.007***	0.018**	0.028
	(0.007)	(0.001)	(0.491)	(0.002)	(0.009)	(0.021)
2001+	-0.001		-1.008	0.007***	-0.000	0.027
	(0.009)		(0.695)	(0.002)	(0.009)	(0.029)

Note: *** p<0.01, ** p<0.05, * p<0.1. Both panels present differences-in-differences estimates. In Panel A, rank of effect relative to placebo estimates in parentheses and p-value based on this rank in brackets. In Panel B, robust standard error in parentheses. The construction of the synthetic control used in Panel A does not allow the five states losing the most population to Delaware after the policy to enter the control (Maryland, Pennsylvania, New Jersey, West Virginia, and Rhode Island). Estimates in Panel B are the coefficients on year group interacted with Delaware. See paper for details.

Appendix Table A.12: 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.)”

Appendix Table A.13: Policy Effect on Growth in Number Unemployed and out-of-the Labor Force:

Differences-in-Differences Relative to Five Years Pre-Policy

	Unemployment Growth	Not in Labor Force Growth
Panel A: Delaware Relative to Synthetic Control		
1981-1987	-0.031	0.001
	(37/50)	(21/50)
	[.74]	[.42]
1988-1992	0.099	0.014
	(6/50)	(3/50)
	[.12]	[.06]
1993-2000	0.066	0.01
	(5/50)	(8/50)
	[.1]	[.16]
2001+	0.035	0.019
	(13/50)	(2/50)
	[.26]	[.04]
Panel B: Delaware Relative to Bordering States		
1981-1987	-0.050	-0.006
	(0.064)	(0.004)
1988-1992	0.026	0.008
	(0.101)	(0.011)
1993-2000	0.031	0.004
	(0.039)	(0.006)
2001+	0.002	0.016***
	(0.074)	(0.004)

Note: *** p<0.01, ** p<0.05, * p<0.1. Both panels present differences-in-differences estimates. The dependent variables are at the state/year level. In Panel A, rank of effect relative to placebo estimates in parentheses and p-value based on this rank in brackets. In Panel B, robust standard error in parentheses. Estimates in Panel B are the coefficients on year group interacted with Delaware. See paper for details.

Appendix Table A.14: Policy Effect on Sectoral Composition

Differences-in-Differences Estimates Relative to Five Years Pre-Policy

	Share Employed in:						
	FIRE	Trade	Manufacturing	Transportation and Utilities	Services	Construction	Government
Panel A: Delaware Relative to Synthetic Control							
Pre-1976	0.001	-0.004	0	0	0	0.004	-0.001
	(20/50)	(33/50)	(22/50)	(28/50)	(29/50)	(14/50)	(29/50)
	[.4]	[.66]	[.44]	[.56]	[.58]	[.28]	[.58]
1981-1987	0.013	-0.013	0.005	-0.004	0	-0.001	0.001
	(1/50)	(49/50)	(13/50)	(46/50)	(26/50)	(27/50)	(21/50)
	[.02]	[.98]	[.26]	[.92]	[.52]	[.54]	[.42]
1988-1992	0.039	-0.022	-0.003	-0.003	-0.001	-0.003	-0.009
	(1/50)	(49/50)	(33/50)	(41/50)	(28/50)	(36/50)	(39/50)
	[.02]	[.98]	[.66]	[.82]	[.56]	[.72]	[.78]
1993-2000	0.064	-0.018	-0.026	-0.005	-0.009	-0.003	-0.005
	(1/50)	(49/50)	(47/50)	(43/50)	(37/50)	(40/50)	(32/50)
	[.02]	[.98]	[.94]	[.86]	[.74]	[.8]	[.64]
Panel B: Delaware Relative to Bordering States							
Pre-1976	0.001	0.000	-0.015	-0.001	0.003	0.003	0.010
	(0.001)	(0.005)	(0.021)	(0.003)	(0.006)	(0.004)	(0.013)
1981-1987	0.012***	-0.012**	0.011	-0.004*	-0.010	-0.002	0.004
	(0.004)	(0.005)	(0.022)	(0.002)	(0.008)	(0.004)	(0.013)
1988-1992	0.034***	-0.016***	0.009	-0.003	-0.015**	-0.007	-0.004
	(0.002)	(0.004)	(0.021)	(0.003)	(0.007)	(0.005)	(0.013)
1993-2000	0.059***	-0.009**	-0.014	-0.007***	-0.025***	-0.004	-0.002
	(0.003)	(0.004)	(0.020)	(0.002)	(0.007)	(0.004)	(0.012)

Note: Both panels present differences-in-differences estimates. The dependent variables are at the state/year level. In Panel A, rank of effect relative to placebo estimates in parentheses and p-value based on this rank in brackets. In Panel B, robust standard error in parentheses. Estimates in Panel B are the coefficients on year group interacted with Delaware. See paper for details.