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

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Abstract

This paper analyzes an important shock to local labor demand in financial services: firm relocation to Delaware following a Supreme Court ruling and state legislation in the 1980s. Using synthetic controls and bordering states, I find large effects on employment, unemployment, and participation in the first decade. Wage effects, and in many cases employment spillovers to the nontradable sector, appear larger than estimates from shocks to the tradable sector. Effects persist for ten to twenty 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.

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

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

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 impact of competing for new firms depends on whether the policy successfully attracts firms, directly affects productivity, or yields local spillovers to other industries. The policy effects also depend on whether and how quickly individuals respond through migration (Bartik 1991, Blanchard and Katz 1992). Importantly, the local, long-run impact depends on whether companies remain in their new jurisdiction, or eventually leave for another jurisdiction offering a more attractive package.[2]

The impact of attracting new firms, both the magnitude and who is affected, may vary with the industry that is targeted. Industries pay different wages, and employ people with different characteristics, including different mobility frictions. Spillover effects may also depend on the targeted industry.

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 the impact of local labor demand shocks affecting the manufacturing and energy sectors, there is a particular lack of evidence on policies attracting white-collar jobs (studies of the motion picture production industry are an exception (Button 2016, 2017)). There are no papers to my knowledge studying policies targeting financial services. These are an important target for local jurisdictions.[3] As described above, attracting these jobs may yield significant differences relative to attracting manufacturing or energy jobs.

This paper contributes to discussions of how finance benefits society (Zingales 2015). I complement the literature by focusing on the societal impact of finance jobs rather than financial products.[4] I identify whether new finance jobs translate into

[2]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 US or Regional Selective Assistance in Great Britain). 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]Reflecting this importance, of 48 companies identified to have received more than $100 million dollars in state grants from 2007 to 2012, Prudential was ranked 11th and Royal Bank of Scotland 39th (Story, Fehr, and Watkins 2012).

[4]In an effort to increase high-quality, middle-class jobs, New York City Mayor De Blasio recently announced a plan to create 100,000 jobs by investing in several non-finance sectors (The City of New York 2017).

total job growth or simply replace existing jobs. I also identify whether attracting finance jobs reduces the number unemployed and out-of-the labor force in the local market, or whether it simply attracts individuals from other markets. This evidence helps fill a gap, highlighted by Neumark and Simpson (2015), in understanding who benefits from place-based policies.[5]

Additionally, I provide the first estimates of which I am aware of local multiplier effects from a nontradable sector (finance) to other nontradable sectors. I compare these magnitudes to recent estimates of local multiplier effects from tradable to nontradable sectors (Moretti 2010, Moretti and Wilson 2013). For policymakers engaged in attracting new firms, comparing multiplier effects by industry may help identify optimal target industries. I also apply the relatively new, though increasingly-used, synthetic control method to the local labor market literature.

The second contribution of the paper is that 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 the policy created strong agglomerative effects and thus a self-sustaining cluster. As discussed in Duranton and Puga (2004), the theoretical microfoundations for agglomerative effects among the relocating firms may include sharing (e.g. sharing gains from specialization), matching (e.g. larger labor pool), and learning mechanisms.

An existing literature studies whether temporary shocks have long-run effects on industry location and population, consistent with the theoretical prediction of multiple equilibria when agglomeration is strong (Carrington 1996, Davis and Weinstein 2002, 2008, Hanlon 2015, Kline and Moretti 2014a, b, Miguel and Roland 2011, Redding, Sturm, and Wolf 2011). Many studies focus on shocks creating stark differences between locations, such as war-time bombing or divisions. In contrast, after a jurisdiction gains a regulatory advantage there are often viable and attractive alternative locations. This implies that for firms to remain after a regulatory advantage disappears, agglomeration there would need to be especially high, fundamentals elsewhere especially low, or relocation very costly. As I discuss, Kline and Moretti (2014a) also study persistence after a local economic development policy, though in a very

[5]Notowidigdo (2013) finds low-skill workers are less likely to relocate following an adverse labor demand shock because the shock's incidence is lower for these workers.

different industry.

I study the dynamic effects of an exogenous increase in local labor demand affecting the finance sector, resulting from a landmark United States Supreme Court decision. In 1978, the US Supreme Court ruled in *Marquette National Bank of Minneapolis v. First Omaha Service Corp.* that a bank could export the highest interest rate allowed by the state in which it is headquartered. Previously, state usury laws determined the maximum interest rate that banks could charge customers residing in that state (regardless of where the bank was headquartered).

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 bank tax. Likely because of its proximity to New York and its regressive tax, many more banks and credit card companies opened subsidiaries in Delaware than in South Dakota.

Within just a few years other states, including Delaware's neighbors, responded with similar policies eliminating or increasing interest rate limits. As more states with low taxes passed these policies, Delaware's tax advantage weakened too. By ten years after Delaware's 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]

Several earlier papers study the effect of the FCDA. Butkiewicz and Latham (1991) use Delaware data to estimate an econometric model of Delaware's economy. Comparing simulations with an FCDA indicator in the wage and hours equation set to one and zero, they find important effects on regional growth, employment (total, and in several sectors), and state tax revenues in the first seven post-policy years. Using Delaware data, Abrams and Butkiewicz (2007) find Delaware's unemployment rate is on average 1.92 percentage points lower in the post-legislation decade than in

[6]In an additional paper, I study the shock's impact on college major choice (Weinstein 2017).

the pre-legislation decade, controlling for the national unemployment rate.

I extend these earlier studies in several important ways. Methodologically, I focus more on identifying a credible control group for Delaware. I also provide greater details into the economic adjustment mechanism, identifying who benefits from the increase in jobs, and estimating local employment multipliers from FIRE to other sectors in the short- and long-run. I further extend these studies by testing for productivity effects, direct or indirect through agglomeration.

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 Delaware's economy had the policy not been implemented (Abadie, Diamond, and Hainmueller 2010, 2014). The relative value of these methods depends on whether pre-policy predictor variables, or geographic proximity, are a better predictor of post-policy outcomes.

I construct a dataset from 1960 to 2013 using the Current Employment Statistics (CES), Local Area Unemployment Statistics (LAUS), Federal Housing Finance Agency (FHFA) Index, and the US Census.

The policy had large effects within the first decade. FIRE (Finance, Insurance, and Real Estate) employment grew an additional 60 to 70%. These jobs were not all filled by substitution across sector, as total employment grew an additional 10 to 14%. Some of these jobs appear to benefit new residents, as there is a nontrivial effect on population growth. However, there is also evidence the policy benefited unemployed and out-of-the labor force individuals. The unemployment rate falls 1 to 1.8 percentage points below the control group, and the participation rate increases 3 to 5 percentage points above the control. This is not only driven by changing workforce composition, there are also relative decreases in the number of unemployed and out-of-the labor force individuals.

The new FIRE jobs have spillover effects to other industries. For every FIRE job created from 1980-1989, magnitudes suggest 1 to 2 jobs are created in the nontradable sector (excluding FIRE). There appear to be nontrivial effects on wages over the first decade, with increases exceeding 7%, although these effects are not significant.

Evaluating the policy's long-run impact requires the stronger assumption that any long-run difference-in-difference derives from the initial shock. This is consistent with

popular media and scholarly work on Delaware's history and politics, which attribute long-run effects to the policy twenty to thirty years after enactment. Based on extensive research, I attribute later FIRE growth to two large firms, and rule out additional legislation as an explanation for long-run effects. With this caveat, thirty years after the policy employment, population, and wages are still higher, and unemployment lower, even after other states competed away the original advantage. This persistence differs dramatically from Blanchard and Katz (1992), who find unemployment and participation converge to the pre-shock equilibrium after five to seven years, and only temporary effects on consumption wages.

Local multipliers suggest for every FIRE job created from 1980-1989, by 2000 there were an additional 1.7 jobs in FIRE (from 1989-2000), and 3.3 jobs in other nontradable sectors.

I test whether the persistently lower unemployment rate is explained by the shift to a sector with lower unemployment rates nationally. Specifically, I compare the unemployment rate in Delaware to the predicted unemployment rate based on Delaware's sectoral composition and national sectoral unemployment rates from the Bureau of Labor Statistics (BLS). This is also an informal test of whether agglomerative effects in Delaware are stronger than in finance nationally. By approximately 15 years after the shock, I find 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 local multipliers from finance are at least as large or larger than multipliers from several other industries. Short-run local multipliers from finance to nontradable employment are slightly larger and long-run multipliers significantly larger than recent estimates from tradable to nontradable employment (Moretti 2010). Spillover effects from finance to nontradable sectors in percentage terms are larger than spillovers from oil and gas employment to manufacturing (Allcott and Keniston forthcoming).[7] The multipliers also contrast with the absence of employment loss after military base closures, beyond direct transfers (Hooker and Knetter 2001). However, the very short-run local multiplier from biotech to nontradable employment is substantially larger than the short-run multiplier from finance (Moretti and Wilson 2013).

[7]I am comparing to the oil and gas spillover to manufacturing in counties with one standard deviation larger endowment (Allcott and Keniston forthcoming). Allcott and Keniston discuss the literature on local effects of resource booms.

The large, though statistically insignificant, short-run wage impacts of stimulating finance labor demand are larger than those from several other important local labor demand shocks, including oil and gas booms (Allcott and Keniston forthcoming), stimulating biotech (Moretti and Wilson 2013), the Tennessee Valley Authority targeting manufacturing and agriculture (Kline and Moretti 2014), and wages of nonresident workers in federal urban Empowerment Zones (EZs) (Busso, Gregory, and Kline 2013). The wage effects are smaller than those experienced by resident workers in federal urban Empowerment Zones (EZs), about 10% of EZ workers.

Similar to Delaware's finance shock, evidence from other local demand shocks shows individuals who were previously nonresidents may take newly created jobs either by moving or commuting (Allcott and Keniston forthcoming, Kline and Moretti 2014, Busso, Gregory and Kline 2013). These papers do not analyze changes in the number of unemployed or out-of-the labor force individuals in the affected areas. As a result, it is not possible to determine whether a shock to finance labor demand has stronger effects on these measures than shocks to other sectors. While the population response in Delaware is not precisely estimated in the first decade, it is dramatically larger in the second decade than the short-run effects cited above.

Finally, both Delaware's finance shock and the Tennessee Valley Authority (TVA) provide settings in which an original policy-induced advantage weakens over time. The effects of Delaware's finance shock appear more robust to this weakening advantage than those of the TVA, 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 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.[9]

The results have important policy implications: local policies can successfully incentivize firms to relocate, with significant local labor market effects, and these effects can be sustained in the longer-run. However, this is conditional on the policy shifting the economy's composition towards sectors with low unemployment and high

[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).

[9]Allcott and Keniston (forthcoming) find no long-term effects of oil and gas booms and busts in counties with greater oil and gas endowments. This is not directly comparable to the Delaware setting since finance never experiences a bust returning the industry to pre-boom levels.

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.[10] The US Supreme Court's ruling in *Marquette* allowed a bank to export the highest interest rate allowed by the state in which it is headquartered. At the time, large banks claimed losses in their credit card divisions due to 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 industry.[11] After *Marquette*, the state recognized the opportunity to attract the finance industry. In 1981, Delaware eliminated its usury laws, with the passage of the Financial Center Development Act (FCDA). In addition to eliminating ceilings on interest rates for most kinds of loans, the FCDA reduced other industry regulation and introduced a regressive tax structure for banks.[12]

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). 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

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

[11]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). In addition, Delaware exempts from corporate taxation companies whose only business within the state is managing intangible assets. This has led companies to form holding companies in Delaware for this purpose, and Delaware only requires that the holding companies are physically located in the state (Boyer 2000). This exemption started in 1958, and was expanded in 1984 (Delaware General Laws).

[12]There were capitalization and employment requirements for FCDA banks. Other FCDA provisions include allowing borrowers and lenders to negotiate terms without interference from regulators, and banks to charge certain fees for credit accounts.

nonbanks (financial institutions without deposit accounts) had been opened or been acquired through the FCDA (Appendix Figure A7, 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 1 shows that around the time of the policy there were clear increases in FIRE employment in Delaware.

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 comparing 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 1982 most states with large banking sectors had relaxed or repealed their interest rate ceilings (Ausubel 1981). By 1985, fifteen 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 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, Appendix Figure A7 Panel B shows that the number of new banks remained relatively flat in Delaware during the early 1990s, ten years after the policy. 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

[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.

[15]Some of these states may not have formally invited out-of-state bank holding companies as Delaware had, implying 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 this was no longer a necessary requirement (Medley 1994).

left the state.

This suggests that after 1991, banks and credit card companies were relocating or adding jobs in Delaware even though Delaware no longer offered a policy advantage. Thus, policy effects thirty years after the policy 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 had disappeared.

3 Data

I obtain annual data from 1960 through 2000 on non-farm employment by state and SIC industry from the Bureau of Labor Statistics (BLS) Current Employment Statistics (CES). To obtain a longer time series for total employment I use total employment from the CES, NAICS basis. From the BLS Local Area Unemployment Statistics (LAUS), I obtain annual data from 1976 through 2013 on the labor force and unemployment rate by state. I obtain state unemployment rates from 1970 through 1976 constructed from labor market areas.[16] As a measure of participation, I use labor force divided by population. If Delaware experiences differential population growth of nonemployment age individuals, the effects on labor force participation will be underestimates.

I obtain population by state and year from the intercensal estimates of the US Census, available annually through 2010. From the 1960, 1970, and 1980 US Censuses I obtain for each state: percent with at least a high school diploma, percent of the population age 15 to 64, and percent living in metropolitan areas.

I obtain annual data on housing prices from the Federal Housing Finance Agency All-Transactions Index, which begins in 1975. I adjust the index using the Consumer Price Index for All Urban Consumers (CPI-U). When analyzing wages, I use the 1970 1% Form 1 state sample, the 1970 1% Form 2 state sample, the 1980 5% state sample, the 1990 5% state sample, the 2000 5% state sample, and the 2014 ACS (Ruggles 2015) (since wages are not publicly available in the 2010 10% sample). I use the 2014 ACS rather than the 2010 ACS to avoid complicating effects of the Great Recession.

[16]These data were used in Blanchard and Katz (1992).

4 Empirical Methods

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 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 post-policy 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 such that the outcome predictor variables match those in Delaware before the policy. The assumption is that if the pre-policy trends appear similar in these states, their post-policy 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.

To use the synthetic control framework with individual-level wage data, I first construct regression-adjusted average state wages (in 1999 dollars) in each sample year to adjust for differences in worker characteristics across states. I regress log wages on state-year fixed effects and individual characteristics, including years of education, a quadratic in potential years of experience, 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.[17] To focus on individuals with significant attachment to the labor force, I include only individuals between the ages of 25 and 59, who usually worked at least 35 hours per week.

I estimate regression-adjusted average state wages in each sample year using the

[17] In 1970, the hours worked variable denotes hours worked last week rather than usual hours worked per week last year. See appendix for details.

full sample (including industry and occupation fixed effects), and separately for individuals whose industry was "Banking and credit agencies," and separately for a group of occupations relevant to the banking and credit industry (accountants and auditors, and clerks and managers relevant to the banking and credit industries).[18] Wages for these occupations, regardless of industry, may have increased because of demand from financial firms.

Synthetic Control

I construct a synthetic control with similar sectoral, economic, and demographic characteristics as Delaware in the pre-policy period. Specifically, I include as predictors five-year averages of the following variables in the pre-policy period: sectoral employment shares, unemployment rate; labor force participation rate; and housing prices, employment, and population (indexed to one in 1981).[19] I also include as predictors the 1960, 1970, and 1980 Census values for the percent living in metropolitan areas; percent of the population 15 to 64; and percent with at least a high school diploma. By matching Delaware to a control with similar five-year averages of these variables, often starting in 1960 and through 1980, I capture not only pre-policy levels but pre-policy trends.

By assigning equal weight to each of these predictors, I hold constant the composition of the synthetic control across outcomes. I will use the same synthetic control when studying the impact on employment as I do when studying the impact on wages. For robustness, I allow the weight on the predictors to vary with each outcome, and estimate a separate synthetic control for each outcome variable. I include each state and Washington, DC as potential components of the synthetic control.[20] The robustness section addresses the concern that the policy negatively affected certain control states.

Following Bohn, Lofstrom, and Raphael (2014), I obtain differences-in-differences

[18] See Appendix Table A8 for the Census Bureau codes pertaining to these groups. I include occupation fixed effects when including only those in the banking and credit industry. I include occupation and industry fixed effects when the sample is limited to relevant occupations.

[19] See appendix for details. Including five-year averages reduces the number of predictor variables, which would be 241 if the predictors included the value of each variable in each year. Annual variations may obscure general trends, implying a potentially lower-quality synthetic control when matching annually. Appendix Tables A14 through A17 show that including yearly values of the predictors instead of five-year averages yields very similar results.

[20] I exclude Oregon since it is missing data in some pre-policy years.

estimates by comparing changes in Delaware to the synthetic control in the period before the policy and in each period after the policy. Specifically, I estimate:

$$\triangle Y_{DE,t} - \triangle Y_{synth,t} \quad = \quad \alpha_0 + \beta_t + u_t \tag{1}$$

The variable $\triangle Y_{st}$ is the annualized change in Y from t' to t in state s ($\frac{Y_t - Y_{t'}}{t - t'}$), where s is either Delaware or the synthetic control. Thus, the dependent variable is the differential annualized change in Y in Delaware relative to the synthetic control from t' to t. I analyze changes over the following periods (t', t): (1960, 1970), (1970, 1980), (1980, 1989), (1989, 2000), (2000, 2007), (2007, 2010), (2010, 2013). I analyze changes from 1980 to 1989, rather than 1990, to avoid confounding effects of the 1990/1991 recession. Similarly, I analyze changes from 2000 to 2007, rather than 2010, because of the recession beginning in December 2007.[21]

In an additional specification, I analyze changes over the entire post-policy period. For all variables except employment by sector, I analyze changes over the following (t', t) : (1960, 1970), (1970, 1980), (1980, 2010). Given the data limitations on sectoral employment, I analyze changes over the pre-policy decades and (1980, 2000).

The omitted period in regression (1) is the decade preceding Delaware's policy (1970 to 1980). Thus, the coefficients β_t identify the differential annualized change in Y in Delaware from t' to t, relative to the synthetic control, and relative to this difference in the period preceding the policy. To obtain the total additional growth in Delaware over (t', t), I multiply the additional annualized growth β_t by the number of years in the interval $(t - t')$.

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.[22] As in Bohn, Lofstrom, and Raphael (2014), I obtain the differences-in-differences estimates for each of these placebo states. If the differences

[21] Many variables only have time series until 2010, and so to compare effects across variables I analyze changes from 2007 to 2010, and 2010 to 2013. Several variables do not have observations for (t', t) = (1960, 1970). For housing prices, participation, out-of-the labor force, and unemployment, data are not available in 1970. Instead, the pre-period consists of (t', t) = (1976, 1980).

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

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 β_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 $\beta_t > 0$ is $1 - F(\beta_t)$.

Bordering States

I estimate the following specification, including Delaware and bordering states (Maryland, New Jersey, and Pennsylvania):

$$\triangle Y_{st} = \alpha_0 + \beta_t Decade\_t_t + \gamma_t Decade\_t_t * DE_s + \delta_s + u_{st} \quad (2)$$

The variable $\triangle Y_{st}$ is the annualized change in Y from t' to t in state s ($\frac{Y_t - Y_{t'}}{t - t'}$). I analyze changes over the same periods (t', t) as in (1), and the omitted period is the decade preceding Delaware's policy (1970 to 1980).

The regression identifies annualized within-state growth over decade t, relative to what it would have been had the preexisting trend from 1970-1980 continued. The coefficients γ_t measure the differential annualized growth over decade t relative to trend in Delaware.[23] To obtain the total additional growth in Delaware over (t', t), I multiply the additional annualized growth γ_t by the number of years in the interval $(t - t')$.

With four states, and up to seven year groups, the maximum number of observations in these regressions is 28. Including indicators for each state, and each year

[23]This is equivalent to estimating the trend from 1970-1980 separately for each state, obtaining the residuals, and then identifying additional growth in the residuals in Delaware from t' to t relative to bordering states. Given that the dependent variable is a measure of within-state annualized decadal growth (consistent with the synthetic control specification), I allow for an overall state-specific trend by including state fixed effects in equation (2). These state fixed effects allow annualized decadal growth to be different for each state, in the same way for each decade. However, allowing for overall state-specific trends does not affect the coefficients $\hat{\beta}_t$ and $\hat{\gamma}_t$, since these identify differences in within-state decadal growth over decade t relative to 1970 to 1980, removing the state-specific trends that are constant across decades. Including the state fixed effects does affect the interpretation of the coefficient on DE_s, and generally results in smaller standard errors of $\hat{\beta}_t$ and $\hat{\gamma}_t$, consistent with these state fixed effects reducing the error term u_{st}. The lower standard errors rarely affect whether the coefficients of interest $\hat{\gamma}_t$ are statistically significant at conventional levels. Appendix Tables A19-A21 show results without state fixed effects.

group, and interacting with Delaware additionally decreases the likelihood of identifying precise effects.

Spillover Employment Effects

The difference-in-difference coefficients from regressions (1) and (2) identify the exogenous change in Y in Delaware, by comparing Delaware to the control, and subtracting out the trend in the immediately preceding period. I calculate employment multipliers using these exogenous changes. In particular, I obtain the percentage change in FIRE employment resulting from the policy, and the resulting percentage change in employment in other sectors. This allows me to obtain the percent change in other sectors' employment for a given percent change in FIRE employment. Specifically, if $Y = ln(Employment)$, then an exogenous 10% increase in FIRE employment from 1980-1989 yields a $10 * (\beta_{Y,1989}/\beta_{ln(FIRE),1989})$ percent increase in total employment, using the β coefficients estimated in regression (1) (separately when the dependent variable is $ln(Employment)$ and $ln(FIRE\ Employment)$. Writing the multiplier in terms of jobs, every FIRE job created from 1980 through 1989, creates the following number of jobs in sector Y:

$$\frac{\beta_{Y,1989}}{\beta_{ln(FIRE),1989}} * \frac{Jobs_{Y,1980}}{Jobs_{FIRE,1980}}$$

5 Pre-Policy Differences: Delaware, Bordering States, and the Synthetic Control

The potential advantage of the synthetic control is identifying good counterfactuals for Delaware based on economic and demographic characteristics, but not necessarily those in close geographic proximity. Table 2 shows several important differences between Delaware and bordering states. In the pre-policy period, relative to bordering states Delaware had a larger manufacturing and high-technology manufacturing sector, smaller services sector, experienced greater population growth, greater declines in housing prices, and had a smaller share living in metropolitan areas. These differences may imply a potential improvement upon the traditional control group.

Role of Manufacturing

American manufacturing experienced significant declines in the 1980s, precisely the period following Delaware's financial services legislation. As a result, states where manufacturing was similarly important may best approximate Delaware in the absence of the policy.

In every period the share employed in manufacturing is larger in Delaware than in the bordering states. In 1980, 27% of Delaware's economy was employed in manufacturing, while in bordering states this sector represented only 22% of employment. Bordering states also experience a more significant pre-policy decline in manufacturing employment share. Finally, high-technology manufacturing was historically a much larger share of manufacturing employment in Delaware than in the bordering states.[24] This is especially important given high-technology manufacturing industries experienced smaller employment declines over the 1980s (Plunkert 1990). As a result, I include as an additional predictor the percent employed in high-technology manufacturing.

Table 2 shows that compared to bordering states, the synthetic control more closely matches Delaware's pre-policy manufacturing employment share, and similar to Delaware was historically more specialized in high-technology manufacturing. The synthetic control more closely matches the relatively smaller pre-policy services sector in Delaware. Finally, relative to bordering states, the synthetic control more closely matches various demographic characteristics of Delaware in the pre-policy period, including percent living in metropolitan areas and population growth.

Composition of the Synthetic Control

Consistent with the synthetic control yielding more similar pre-policy characteristics, it does not heavily weight bordering states. Given differences from the more conventional control group, and concerns about transparency of synthetic controls, I provide further discussion on why specific states receive a high weight. In the robustness section, I also test sensitivity of the results to composition of the synthetic

[24]From 1960-1964 this was nearly 46% in Delaware, but 14% in bordering states. This reflects the importance of chemical and automotive manufacturing in Delaware. The average increase in high-technology employment share over time in bordering states is due to increases in Maryland and Pennsylvania (which had a share of zero in in the 1960s), while the share decreased in New Jersey (which had nonzero share in the 1960s). See appendix for high-technology manufacturing definition.

control.

Ohio has the greatest weight in the synthetic control. Both the level and trend of the proportion employed in manufacturing and high-technology manufacturing are very similar to Delaware. The same is true for Indiana, which comprises over 10% of the control. Despite being nontraditional controls for Delaware, the similarity in manufacturing and the overall manufacturing decline during the post-policy period may suggest these are good counterfactuals for Delaware.

Virginia is 23% of the synthetic control. Despite large differences in the proportion employed in manufacturing, the trends are similar to Delaware.[25] The inclusion of Virginia in the synthetic control helps match the proportion of individuals age 15 to 64.[26] Without these as predictors, Virginia's share falls to 11% while Ohio's share rises to 32% and Indiana's share to 18%.

Maryland is the only bordering state in the synthetic control, and it comprises only 4.5%. Removing the proportion employed in manufacturing and high-technology manufacturing as predictor variables increases Maryland's share to 32%, and decreases Indiana's share to 0%. Because levels and trends in other predictors are similar in Delaware and Ohio, its share remains large at 34% when removing the manufacturing variables as predictors.

New Jersey and Pennsylvania do not enter the synthetic control. For many of the predictor variables, the levels and trends in New Jersey and Pennsylvania are similar to Delaware, but there are other states for which these are more similar.

Figure 1 shows that for the main outcomes of interest, pre-trends generally look similar in the synthetic control and bordering states, and these fairly closely approximate the pre-policy outcome in Delaware.

While the synthetic control contains vastly different states than those bordering Delaware, this may yield only small differences in the results. These groups of states may only experience small differences in outcomes over this period, implying the effects of Delaware's legislation are robust to using different control groups.

[25] Virginia's manufacturing employment share, averaged over period in parentheses: .27 (1960-1964), .26 (1965-1969), .23 (1970-1974), .20 (1975-1979), .19 (1980).

[26] In 1980, 68% of Delaware's population was 15 to 64 years old. In Ohio and Indiana this was 66%, while in Virginia this was 68%.

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, from 1980-1989. Even by the end of this period, Delaware's policy-induced advantage had not completely disappeared relative to other states. Also, because additional shocks are always possible in the long-run, attributing short-run effects to the policy is more straightforward.

From 1980 to 1989, employment in Delaware's FIRE sector grows by an additional 70% as a result of the policy, when using the synthetic control as a counterfactual Delaware (Figure 1; Table 3, column 1). This FIRE growth relative to the synthetic control is larger than all of the placebo estimates. The magnitude is also large (58%) and significant when comparing Delaware to bordering states (Table 3). These results confirm Delaware's legislation successfully attracted thousands of new finance jobs, and helped transform the state into a center for financial services employment. More generally, the results show relocation costs for finance jobs are small relative to potential state-level differences in taxation and regulatory environments. In this environment, local economic development policies may have large impacts.

Total Employment Growth and the Recipients of New Jobs

New FIRE jobs were not simply filled by substitution across sector. Total employment grows by an additional 13.5% or 10.5% in Delaware in the period immediately after the policy, using the synthetic control and bordering states respectively. The effect relative to the synthetic control is larger than all but 4 of the placebo estimates, while the effect relative to bordering states is statistically significant at the 5% level.

Delaware employment in 1980 was 259,200, implying an additional 34,992 jobs using the synthetic control estimate. I next discuss results from a simple accounting of whether this job growth exclusively benefited new residents and individuals who were already employed in Delaware, or whether it also reduced the number of unemployed and out-of-the labor force individuals in Delaware.

The synthetic control suggests nontrivial additional population growth in Delaware of 4% immediately after the policy, though the effect is not statistically significant. Figure 1 also shows differential population growth in Delaware relative to border-

ing states, but the difference-in-difference is small given larger pre-policy growth in Delaware. In 1980, Delaware's population was 594,338. Assuming 4% differential population growth (based on the synthetic control), this implies 23,774 additional residents. In 1990, the proportion of Delaware residents 15 to 64 was approximately 67%, implying an additional 15,929 working-age individuals. Assuming prime-working ages of 18 to 64, the number of new working-age individuals would be even lower. Population growth appears to explain less than half of the additional jobs created by the policy.

There are large, immediate effects on the unemployment and participation rate. Delaware's unemployment rate drops by an additional 1.8 or 1.1 percentage points from 1980 to 1989 as a result of the policy, using the synthetic control and bordering states respectively as a counterfactual Delaware. Only the effect relative to bordering states is statistically significant.

Similarly, from 1980-1989 the participation rate in Delaware increased an additional 2.7 or 5.1 percentage points as a result of the policy, using the synthetic control and bordering states respectively as a counterfactual Delaware. Again, the effect relative to bordering states is statistically significant.

These changes may not reflect reductions in the number of unemployed or out-of-the labor force individuals. Instead, they may reflect that new residents in Delaware move directly into jobs, mechanically decreasing the unemployment rate and increasing the participation rate. I estimate additional specifications looking at the number of unemployed and working-age out-of-the labor force individuals (age 15 to 64). From 1980 to 1989, the number of unemployed individuals in Delaware falls by an additional 24% or 12%, using bordering states and the synthetic control respectively as a counterfactual. Only the effect relative to bordering states is statistically significant. In 1980, there were 20,966 unemployed individuals in Delaware. Using the effect relative to the synthetic control, this suggests an additional 2,516 individuals exited (or did not enter) unemployment resulting from the policy.

Delaware experienced an additional decrease of 19.6% or 35.1% in the number of people 15-64 not in the labor force, using the synthetic control and bordering states respectively.[27] Only the effect relative to bordering states is statistically significant

[27]I subtract labor force from population age 15 to 64, available in the decennial censuses through 2000. I impute the population age 15 to 64 in non-census years by assuming the proportion age 15 to 64 increases linearly between censuses. See Appendix Table A1 for details.

(Appendix Figure A1, Appendix Table A1). In 1980, there were 119,404 Delawareans age 15-64 not in the labor force. Using the effect relative to the synthetic control, this suggests the number of individuals out-of-the labor force (or leaving the labor force) decreased by an additional 23,403 individuals. This will also be an overestimate if the proportion of the population age 15-64 does not increase linearly in Delaware between 1980 and 1990 (which I assume given this proportion is only known in census years). An overestimate may be particularly likely given that migration in response to the shock appears to have operated with some lag.

This simple accounting exercise relies on several assumptions. Nonetheless, it provides a general decomposition of who benefited from the increased number of jobs in Delaware. The additional 35,000 jobs in Delaware nearly matches the increase of 15,900 new, working-age residents, 2,500 exiting (or not entering) unemployment, and 23,400 reentering (or not leaving) the labor force. The sum of new residents, unemployment exits, and individuals reentering the labor force would likely match the number of new jobs even more closely, if I assumed prime-working age of 18, rather than 15, to 64 and if I did not impose that proportion 15 to 64 increases linearly.

Employment Spillovers to Other Sectors Table 4 shows spillover effects to other industries. I first show spillovers to all other nontradable sectors, excluding FIRE. As in Moretti (2010), I use nontradable to refer to industries other than manufacturing, agriculture, mining, government, and the military.[28]

The magnitudes are often large, though they are not statistically significant, and so these results are certainly more suggestive. The existence of effects is consistent with Figure 2. The coefficients suggest that from 1980-1989 Delaware experienced an additional 12.3% increase in non-FIRE nontradable employment. This is larger than all but eight of the placebo effects, and so has a p-value of .18.

The magnitude suggests the exogenous 70% growth in FIRE yielded a 12.3% increase in non-FIRE nontradable employment. Equivalently, a 10% increase in FIRE employment (1230 jobs) results in non-FIRE nontradable employment growth of 1.76% (2302 jobs). Rewritten in terms of a jobs multiplier, 1 FIRE job yields 1.87 non-FIRE jobs in nontradable sectors. Using the bordering states as a control, the magnitude is also large, suggesting 1 FIRE job yields 1.07 non-FIRE jobs in

[28]Because the CES measures nonagricultural employment, nontradable employment is constructed by subtracting manufacturing, mining, and government from total employment.

nontradable sectors.

These multipliers are similar to the current RIMS II finance to non-finance employment multipliers estimated by the Bureau of Economic Analysis using input-output models, which are not associated with a length of time until the effects are realized.[29] Despite important shortcomings of the RIMS II multipliers for studying the impact of this large labor demand shock, I think the comparison is informative given these multipliers are widely used.[30]

The services sector is the only sector where the effect relative to the synthetic control is statistically significant. The magnitude suggests 1 FIRE job yields .75 services jobs (larger than all but four of the placebo effects). Using the bordering states as a control, the magnitude is much smaller, though the standard error is large so we cannot rule out large effects. Relative to bordering states, we see significant effects on trade employment as well as manufacturing. The significance of the manufacturing results may be driven by differences in the type of manufacturing between Delaware and bordering states discussed above.

Delaware's legislation required that banks opening as a result of their policy must employ at least 100 people by the end of the first year of operation. I compare the number of new FIRE jobs with the number of jobs required given the number of new firms. This may be a measure of within-finance spillovers or alternatively the value of locating employees at centralized offices rather than across many locations. Thirty new banks had been opened in Delaware by 1989 (Epstein 2001a).[31] Thus, between 1980 and 1989 there should have been at least 3,000 new FIRE jobs in Delaware. In fact, the coefficients suggest there were more than 8,600 new FIRE jobs in this period.[32]

Delaware housing prices increase substantially, and statistically significantly, rel-

[29] Averaging the RIMS II multipliers across all states they are 1.6, they are 1.7 in the synthetic control, and 2 in Delaware's bordering states (and in Delaware). See discussion of long-run multipliers and appendix for details.

[30] The main drawbacks are that the archived multipliers are not available, and even had they been, RIMS multipliers assume fixed prices and purchasing patterns. The multipliers also do not capture additional impacts from the new jobs serving as inputs, and thus may not capture important agglomerative effects. See appendix for greater discussion.

[31] While not all of these were likely opened through the FCDA, Appendix Figure A7 Panel A shows this is a good approximation.

[32] 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. This estimate is not a difference-in-difference, and so includes jobs that may have been created even in the absence of the policy.

ative to both the synthetic control (60%) and bordering states (34.5%) (Appendix Figure A1, Appendix Table A1). This is consistent with increased demand for new housing, and relatively fixed supply in the short run. However, post-period housing price growth looks almost identical in Delaware and bordering states. The difference-in-difference is only large because of different pre-policy trends. Given a very short pre-policy period with available housing price data this may suggest we place less weight on these results. Differences in pre-policy trends also lead to much higher differences-in-differences relative to the synthetic control.

Wages

The differences-in-differences estimates of the wage impact are imprecisely estimated (Figure 4, Table 5). While we cannot rule out zero effect, we also cannot rule out large effects. The magnitudes suggest Delaware experienced moderate additional wage growth of 7.4% in the decade after the policy when comparing to the synthetic control, and 4.1% when comparing to bordering states (Table 5, columns 1 and 2). The effects are similar when focusing on relevant clerks, accountants and managers (columns 5 and 6), and smaller and less significant when focusing on workers in the banking and credit industry (columns 3 and 4).[33] [34]

To identify the policy's effect on real wages, wage growth should be adjusted by the policy's impact on local prices. Moretti (2013) estimates that an increase in local housing costs of 1% yields an increase in overall local prices of .588%.[35] This implies the estimated additional increase of 60% in Delaware housing costs from 1980-1989, relative to the synthetic control, implies overall prices increase 35.3%. Putting the wage and price results together, the synthetic control estimates suggest wages increase

[33]For robustness, I estimate specifications using yearly data from the Current Population Survey (CPS) (King et al. 2010), as well as the Census, allowing me to look for short-run positive effects on wages consistent with Blanchard and Katz (1992). I find significant decreases in wages in the early part of the decade (from 1982-1983 and 1984-1985), consistent with an initial increase in labor supply. However, I find statistically significant additional wage growth in Delaware relative to the pre-policy trend from 1983-1984, and in the later part of the decade (1986-1987 and 1987-1988). These exercises cannot be done using banking and credit wages due to very small sample sizes. See Appendix Table A13.

[34]Appendix Table A18 shows results comparing Delaware to all states, controlling for pre-policy characteristics (and allowing these to have different effects over time). The estimates are more precise, and some are statistically significant. The magnitudes are generally similar; however, these specifications show much larger effects on wages in the banking and credit industry.

[35]Using the local CPI made available by the BLS for 23 MSAs, Moretti (2013) regresses change in the local price index on change in local housing costs.

by 7.4% in Delaware, but prices increase by 35%, implying no positive effect on wages. The bordering states specification also imply no positive effect on real wages.

These real wage effects are more tentative, given the housing price effects appear largely affected by differences in pre-trends which are estimated over a short time period, as discussed above.

In sum, fiscal or regulatory competition can effectively incentivize employers to relocate. Within the first decade, new finance employment yields growth in total employment, which benefits new residents, unemployed, and out-of-the labor force individuals. There is suggestive evidence that these new finance jobs have large spillovers to nontradable sectors. Importantly, these employment effects and the suggestive effects on wages exist despite attempts from other states to compete away the advantage within this first decade.

6.2 Long-Run Adjustment

By 1991 Delaware's tax and usury law advantage had disappeared due to 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 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 initial sectoral composition.

Attributing Long-Run Effects to the 1981 Policy and Examining Additional Delaware Legislation

Twenty years after Delaware's policy was enacted, popular media and scholarly work were attributing long-run effects to the legislation. Newspaper articles were published with headlines such as "1981 Banking Act: How One Law Transformed Delaware" (Epstein 2001a). In 2009, Boyer and Ratledge write in their book Delaware Politics and Government, "Changes wrought by FCDA were to prove as important historically for Delaware's economy as Delaware's Chancery Court, the general corporation laws,

and even the influence of the DuPont Company itself."

I provide some further details on the long-run FIRE growth, and discuss why several other policies do not explain the long-run effects.[36] From 1989 to 2000, Delaware continues to experience additional FIRE employment growth (49% additional growth using the synthetic control as a counterfactual and 45% using bordering states). Figure 1 shows that while FIRE growth in Delaware had leveled off in the late 1980s and early 1990s, this was followed by a resurgence. Figure 5 shows this appears mainly due to the growth of MBNA, a credit card company that spun off 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.

After the FCDA, Delaware passed two other pieces of legislation aimed at helping smaller banks and international banks to take advantage of the FCDA provisions. These had smaller initial impacts than the FCDA (Erdevig 1988). Delaware also attempted to create a second regulatory advantage by allowing banks to enter the insurance industry, though this policy was not successful in creating significant employment growth (explained in detail in the appendix).

Long-Run Impacts of Stimulating Finance Labor Demand

Relative to the counterfactual, from 1980-2010 Delaware experiences an additional 102% or 119% growth in finance employment, 16 or 31% growth in total employment, and an approximately 9 or 20% increase in population, using the bordering states and synthetic control respectively (Table 3). These effects are statistically significant at least at the 10% level using the synthetic control, and the FIRE employment effect is significant relative to bordering states. These long-run effects are larger in magnitude than the initial effects of the policy, which is consistent with the continued, significant growth in later decades (especially 1989-2000) (Table 3).

[36] My research included 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).

[37] The insurance firm AIG also grew from 150 Delaware employees in the mid-1980s to 2700 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.

The unemployment rate is substantially lower in Delaware thirty years later, relative to the counterfactual, from .3 percentage points (bordering states) to 1.8 percentage points (synthetic control). Neither effect is statistically significant. There is some convergence in the unemployment rates during the 1990s, evident in relative increases in the unemployment rate from 1989-2000 (Table 3). Convergence may be due to much higher population growth in Delaware, especially in these later decades. While this mechanism is similar to Blanchard and Katz (1992), the dramatic difference is that convergence is not complete.

There is no long-run positive effect on the participation rate.[38] Housing prices are an additional 151% higher relative to the synthetic control, and 113% higher relative to bordering states (only significant relative to the synthetic control) (Appendix Table A1). Unlike previous papers (Blanchard and Katz 1992, Bartik 1991), these results do not suggest a flat long-run housing supply curve, a curve implying constant housing prices even with increases in demand (reflecting that short-run price increases result in long-run supply increases and price decreases). However, as discussed above, the short pre-policy period for which housing prices are available may be problematic.

Thirty years after the policy total nontradable employment excluding FIRE is higher by nearly 22% using the synthetic control and 13.5% using bordering states (statistically significant at the 10% level using the synthetic control). This implies that one additional FIRE job from 1980-1989 yielded an additional 3.33 nontradable jobs excluding FIRE from 1980-2010, or 2.48 jobs relative to bordering states. Decomposing this long-run effect by sector, the largest and most statistically significant multipliers among the nontradable sectors are in trade. The multiplier for services relative to the synthetic control is also large, though not statistically significant.

Given that Delaware's regulatory advantage had disappeared by 1991, additional FIRE jobs during the 1990s are arguably the result of within FIRE spillovers. Delaware's policy no longer sets it apart from other states, and so this cannot explain why new firms continue to arrive. The results suggest that for every 1 additional FIRE job from 1980 to 1989, there were an additional 1.72 or 1.89 jobs in FIRE from 1989-2000, using the synthetic control and bordering states respectively. Taking the multipliers

[38] From 1980-2010, participation increases less in Delaware than in the synthetic control. Appendix Figure A4 shows, based on CPS data, that in these years (2006 until 2010) there was also an increase in the percent of new residents in Delaware who were 55 and older. In 2014, Kiplinger ranked Delaware as the 7th most tax-friendly state for retirees, and the tax-friendliest in the Northeast (*10 Most* 2014).

to FIRE (1989-2000) and non-FIRE nontradable (1980-2000) together implies an additional 4.4 to 5 jobs from 1980-2000 per additional FIRE job from 1980-1989.

These multipliers are significantly larger than the current RIMS II finance to non-finance employment multipliers estimated by the Bureau of Economic Analysis using input-output models, which are closer to two.

Thus, the short-run multipliers I construct are similar to the RIMS II multipliers, but the long-run multipliers I construct are larger. This is consistent with the RIMS II multipliers not capturing forward linkages, or new jobs created in which the new finance jobs are an input. If the increased finance employment in Delaware yields further increases in finance employment, for example through agglomeration, this will not be captured by the RIMS II multipliers. Much of the difference between the RIMS II multipliers and the long-run multipliers I construct is explained by the multipliers to FIRE from 1989-2000. The difference in the multipliers may also be explained by the Delaware finance jobs being different than the overall industry, for example because these were largely jobs in company headquarters.[39]

The results suggest moderate to large long-run wage growth, consistent with initially positive effects, followed by the absence of large, negative effects in later decades. However, these effects are imprecisely estimated. Comparing to the synthetic control, wages in Delaware grew an additional 12.2% from 1979 to 2013, and this effect is 7.5% when comparing to bordering states. Magnitudes are similar when focusing on banking and credit wages, and wages for clerks, accountants, and managers.

In sum, even after Delaware had lost its policy-induced advantage relative to other states, the policy effects strongly persist for the next decade. Thirty years after the policy, Delaware still enjoyed higher total employment, lower unemployment rate, higher population, and higher wages.

The results differ dramatically from Blanchard and Katz (1992), who find that unemployment and participation converge to the pre-policy equilibrium five to seven years after the shock, and only small temporary effects on consumption wages. 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.

[39]See appendix for other potential reasons the RIMS II multipliers are lower than the long-run multipliers I estimate.

Robustness

Predictor variables The principal results hold constant the composition of the synthetic control across outcomes. 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 for population. For robustness, I allow for the synthetic control to differ across outcome variables. Specifically, as described in Abadie, Diamond, and Hainmueller (2010), for each outcome I find the predictor weights minimizing the mean squared prediction error of the outcome variable in the pre-policy period.

This exercise yields nontrivial differences in the composition of the synthetic control (Appendix Table A5), and generally larger and more statistically significant results (Appendix Tables A6 and A7).[40] For example, the results suggest that from 1980 to 1989 Delaware experienced additional 21% employment growth and 8% population growth. The employment multipliers for non-FIRE nontradable jobs, services, trade, and transportation and utilities are all much larger and also statistically significant for the 1980-1989 period and the 1980-2000 period. From 1980-1989, the results suggest an additional 3 non-FIRE nontradable jobs for each FIRE job created.[41] It is reassuring that even with dramatic changes in the composition of the synthetic control, the overall results still suggest large impacts (and this robustness exercise suggests the results may be even larger than the principal results).

I also construct a robustness synthetic control using predictor values in each year, rather than multi-year averages. This increases the number of predictors substantially. The same states remain in the synthetic control, though their weights change (Appendix Table A14). Most notably, Ohio's weight decreases while Indiana's increases. Again, despite this change in the composition of the synthetic control, the results are relatively unchanged (Appendix Tables A15-A17).

Adjusting for Negative Policy Effects in Control States The migration of firms and individuals to Delaware will yield negative effects in other states. If these

[40] Connecticut, Indiana, and Michigan comprise a clear majority of the control for most outcomes, and there is significantly more weight placed on these states compared to the principal synthetic control (Appendix Table A5). However, there are also some differences across outcomes. For example, while Michigan comprises over 20% of the control for nearly all of the outcomes, it is only 2.7% of the control for labor force/population. Instead, it appears that relative to these other variables, labor force over population in Delaware looks more similar to Ohio.

[41] The robustness specification yields a smaller effect on the unemployment rate, suggesting a decrease of 1.1 percentage points instead of 1.8 using the principal specification.

negative effects are large in states that are influential in the control, the policy's effect will be overestimated. The objective of the empirical strategy is to compare outcomes in Delaware to counterfactual outcomes in Delaware had the policy not been implemented. If the states in the control experience negative policy effects, outcomes in the control will not be reflective of Delaware outcomes absent the policy. 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 US Census data, 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 policy's effect on mobility. Of the top five states losing population to Delaware from 1985 to 1990, relative to 1975 to 1980, Maryland is the only state in the synthetic control (and only 4.5% of the synthetic control).

Further, even for Maryland, which lost the most population to Delaware, the population loss is very small. In the years before the policy (1975 to 1980), Maryland lost .28% of its population to Delaware (approximately 11,600 people). After the policy (1985 to 1990), Maryland lost .32% of its population to Delaware (approximately 14,200 people). The pre-post policy difference in migration to Delaware is not large.[43] If I compared population in Delaware to population in the US excluding Delaware, there would certainly be double counting since positive effects in Delaware must be outweighed by negative effects elsewhere. This exercise simply shows states in the synthetic control were not those losing the most population to Delaware as a result of the policy, suggesting it serves as a good counterfactual and will not lead to double counting.

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 Current Population Survey (CPS)). If Delaware's unemployment is lower than

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

[43] See appendix for the other states losing the most population to Delaware.

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 a potential agglomerative effect of the policy.[44]

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 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.[45] 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 used 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 6). The difference reaches up to two 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 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. This could be evidence of the policy's agglomerative effects, which drive unemployment rates below national sectoral rates in finance or in other sectors.

During the mid-1990s, the unemployment rate is one to two percentage points lower in Delaware than bordering states and the synthetic control. However, the difference between Delaware's unemployment rate, and the predicted rate based on

[44] 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.

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

sectoral composition is significantly smaller in this period, approximately .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. If agglomerative effects were stronger in Delaware, Delaware's unemployment rate should be lower than the predicted rate based on national unemployment rates by sector.

This exercise suggests the newly created finance cluster in Delaware does not appear to be outperforming the industry nationally, implying the policy was inefficient at the aggregate level. Firms relocated creating local benefits without aggregate growth.[46] These results are based on national unemployment rates by sector, but Delaware largely attracted firms from New York, an important financial center with agglomerative effects likely equal to at least the national average in the industry.

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. 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 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

[46]Similarly, Givord et al. (2013) and Mayer et al. (2015) find that the positive local effects of the French enterprize zones are equal in magnitude to the negative effects in neighboring areas.

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 fiscal or regulatory competition can effectively incentivize employers to relocate, and this has positive effects on the local economy. By the end of the first decade, total employment, wages, population, and participation were higher, while the unemployment rate was lower. Further, for every FIRE job created from 1980-1989, there were up to 1.9 additional non-FIRE nontradable jobs created. Thirty years after the policy, and 20 years after Delaware lost its original policy-induced advantage, Delaware still had higher employment, population, and wages and lower unemployment. In addition, by 2000 the nontradable multiplier was up to 5, including FIRE jobs from 1989-2000.

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 and only small temporary effects on consumption wages. 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 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 generally larger spillover effects to nontradable employment from finance than from tradable sectors, larger wage effects, and greater robustness to weakening policy advantages over time.

The implication for policymakers is that short-run effects from attracting firms can be sustained if the policy shifts the economic composition towards a low unemployment and high-wage sector in the long run. This is challenging for two reasons. First, it requires that the relocating firms remain in the jurisdiction in the long run. Second, it requires that the attracted firms or industries are high performing in the long run.

Delaware's experience may not be easily replicated in other settings today. First, evidence suggests jurisdictional competition is dramatically higher in 2015 relative to 1990 (Bartik 2017). Local governments may face quicker responses from other jurisdictions than Delaware faced in the early 1980s.

Second, predicting which firms or industries will perform well in the long run is not necessarily easy. There are certainly examples of jurisdictions which successfully

attracted new companies only to have them fail as businesses (Story 2012). Fiscal incentives may be attractive to businesses, but they are perhaps unlikely to transform an industry and pave the way for years of growth. In Delaware, fiscal incentives were combined with a dramatic shift in the regulatory environment that changed the nature of business in the credit card industry. This led to dramatic industry growth, benefiting Delaware as the industry headquarters.

With these considerations in mind, Delaware's experience suggests a framework for how local jurisdictions might create positive long-run change through local economic development policies. However, at least in this setting, the successful local policy appears inefficient at the aggregate level. Agglomerative effects in Delaware do not appear stronger than in the industry nationally.

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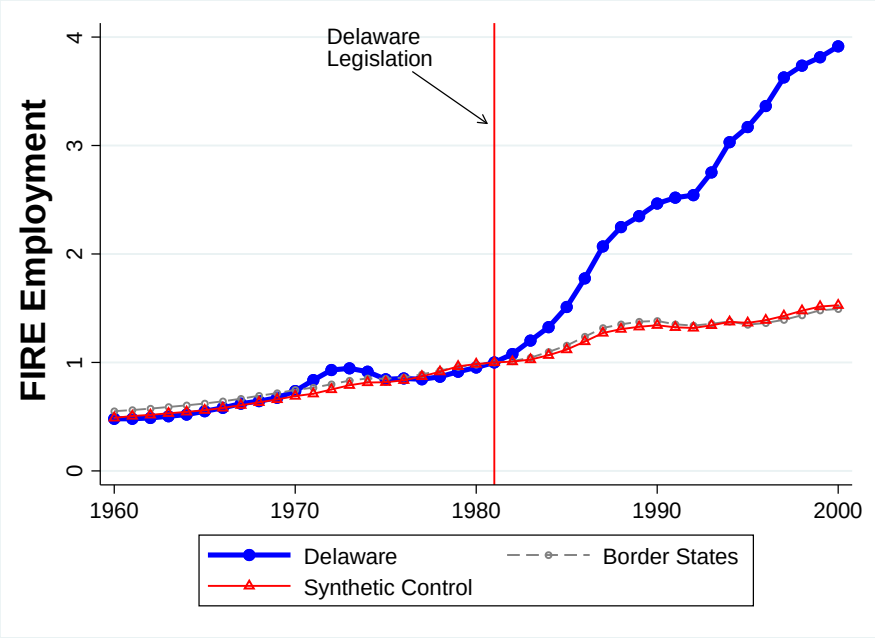
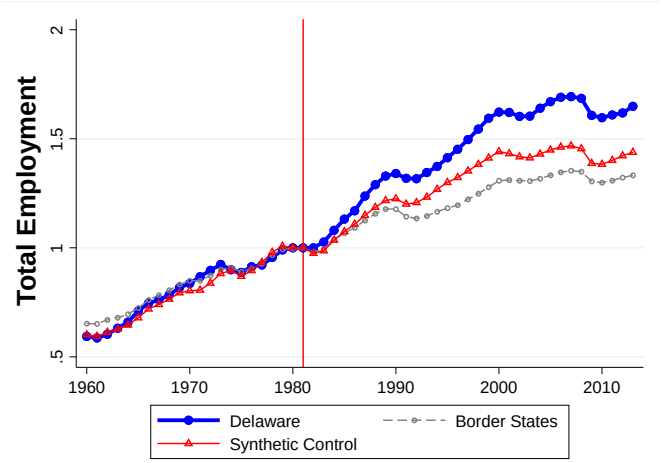
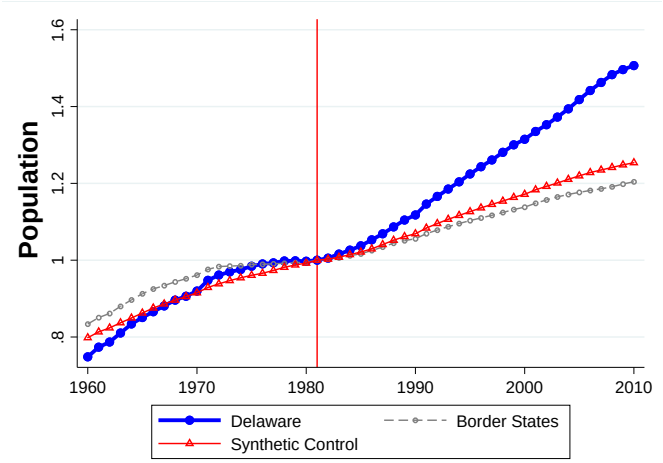
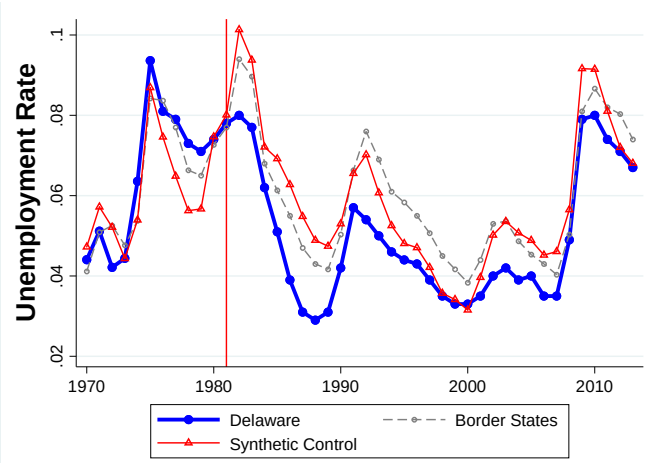
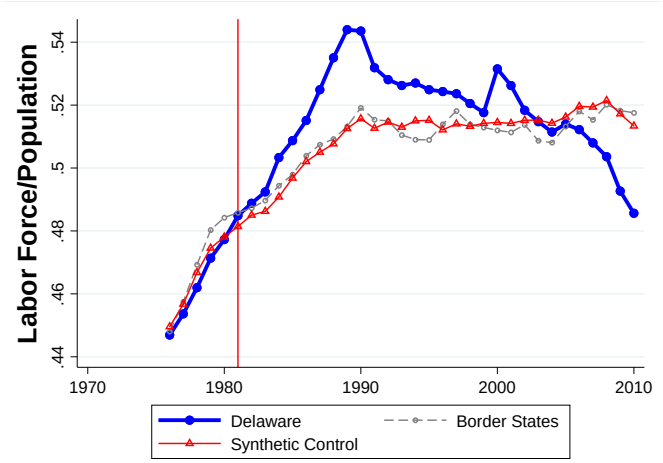
Figure 1: Policy Effects on the Labor Market, Delaware Relative to the Synthetic Control and Bordering States





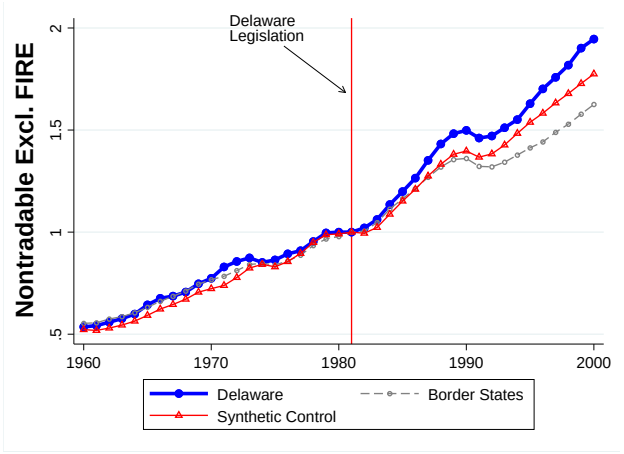


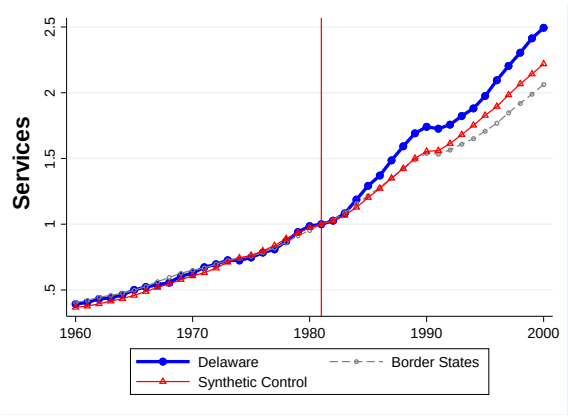


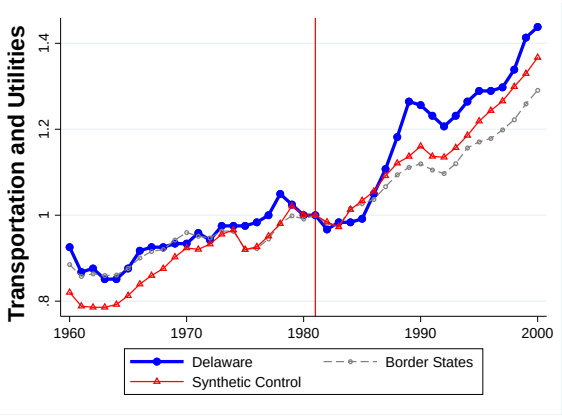


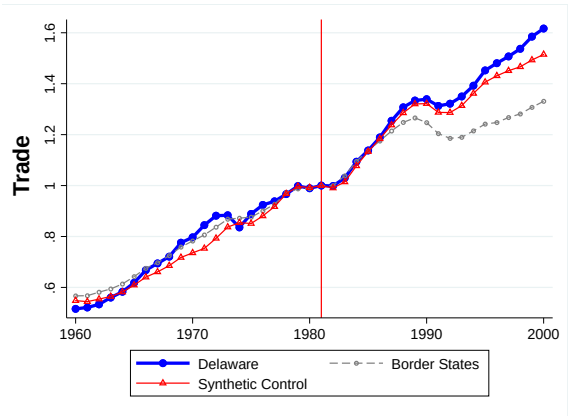
Note: Figures show variables indexed to one in 1981 for Delaware, the bordering states' average, and synthetic control, except for the unemployment rate and labor force/population. See paper for details on construction of the synthetic control.

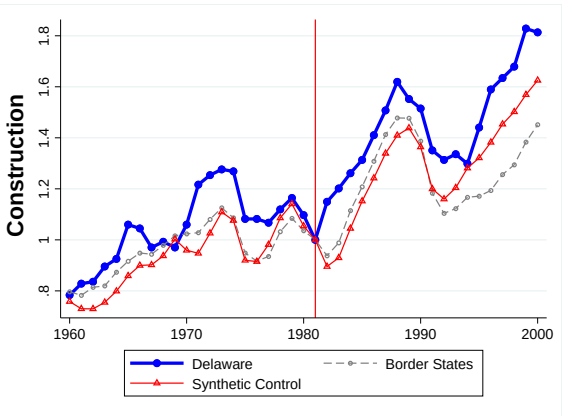
Figure 2: Cross-Industry Spillover Effects, Delaware Relative to the Synthetic Control and Bordering States

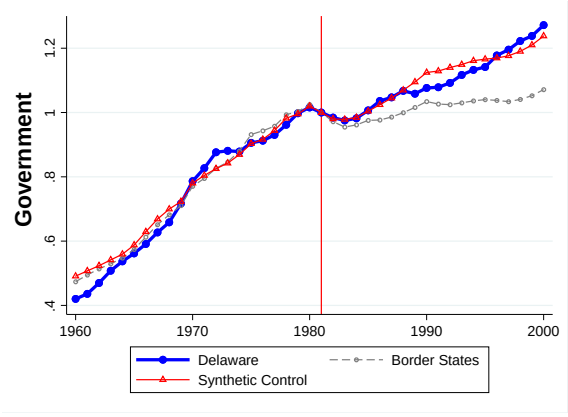


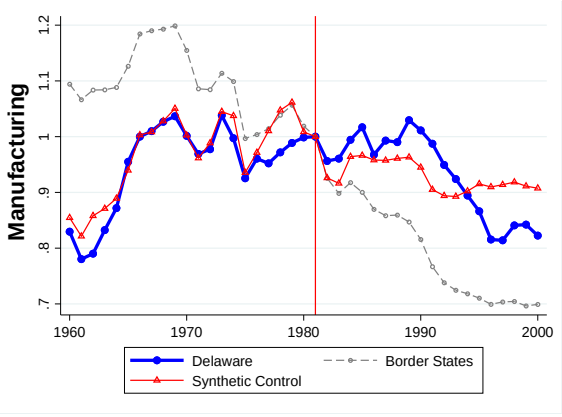






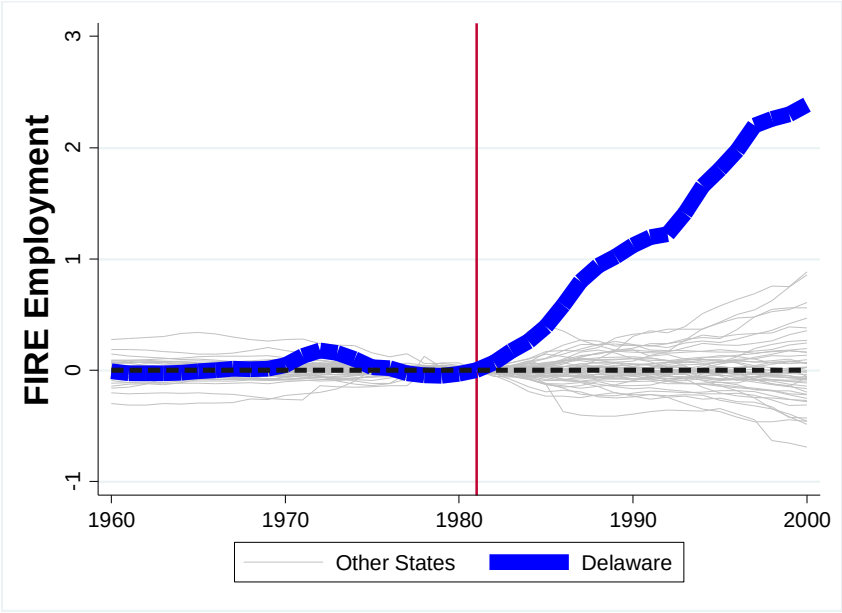


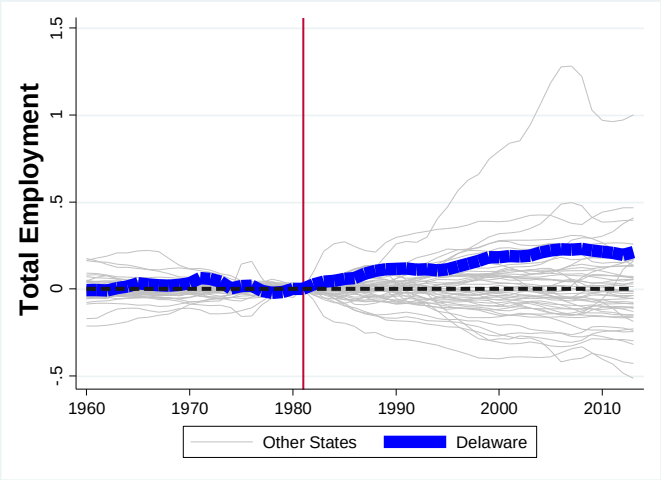


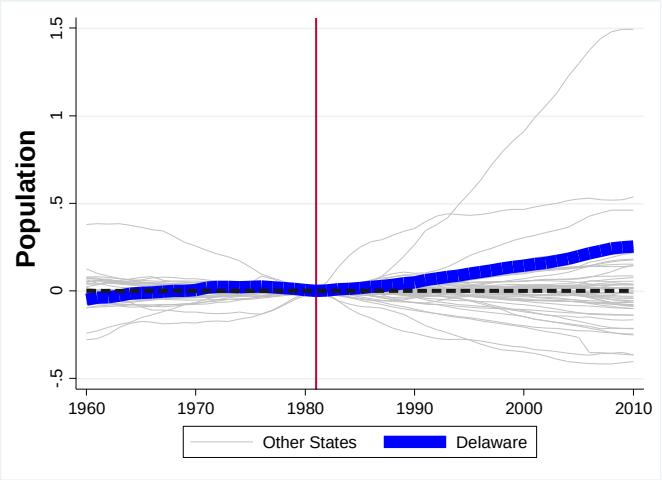


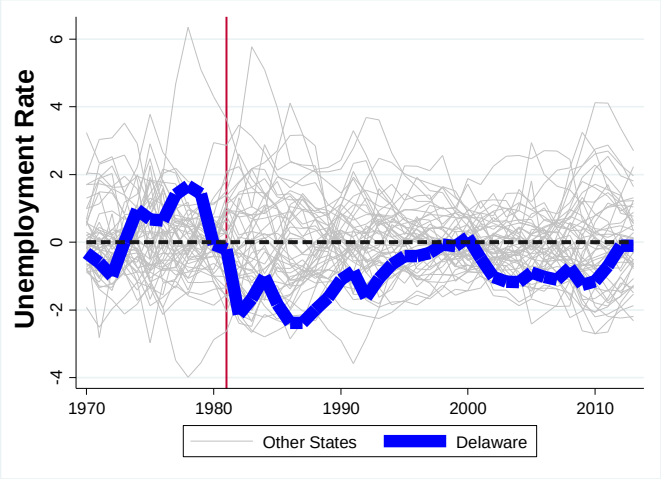
Note: Figures show variables indexed to one in 1981 for Delaware, the bordering states' average, and synthetic control. See paper for details on construction of the synthetic control.

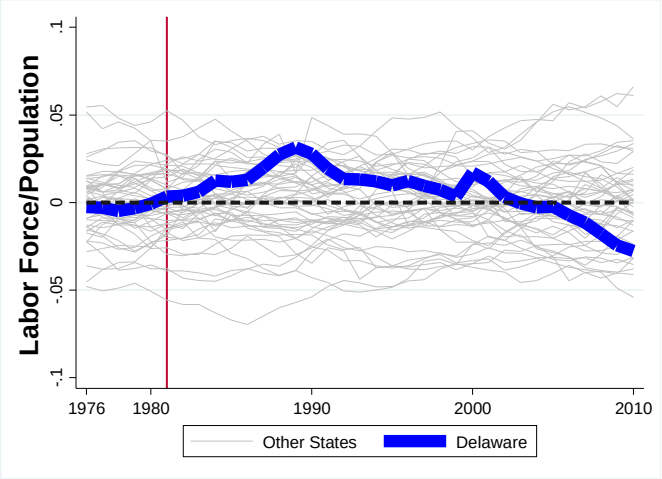
Figure 3: Inference - Estimated Effects in Delaware Relative to Placebos







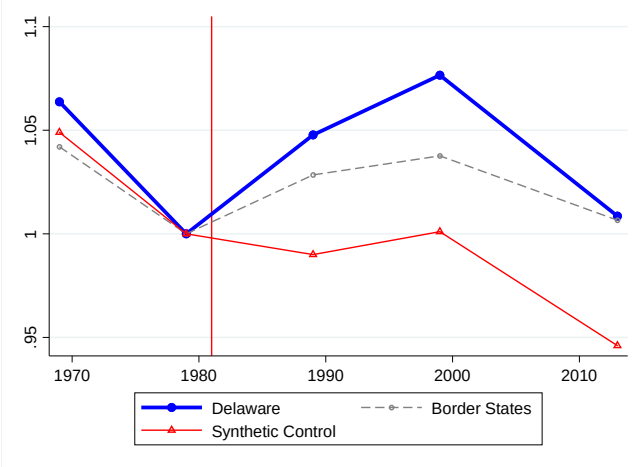




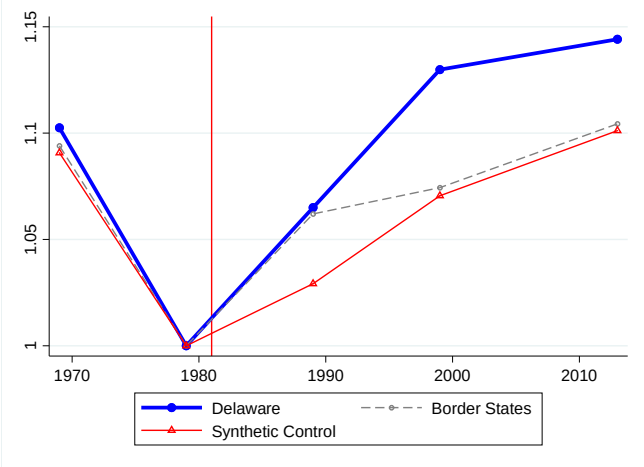
Note: Figures show the difference in outcomes between Delaware and the synthetic control, and between other states and their synthetic control. Outcomes indexed to one in 1981, except for the unemployment rate and labor force/population. See paper for details.

Figure 4: Policy Effect on Average Wages: Delaware Relative to Synthetic Control and Bordering States

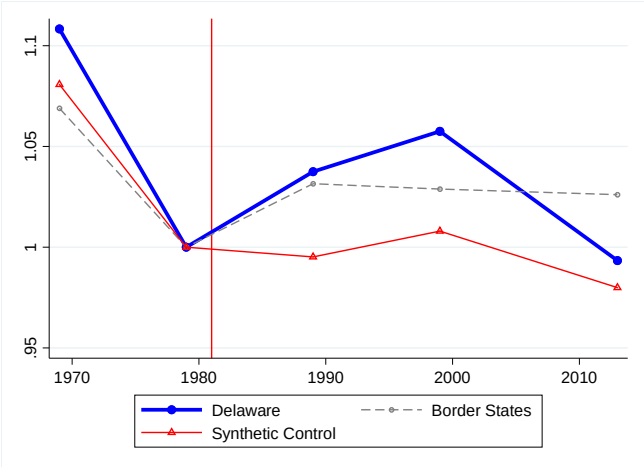
(a) All Occupations and Industries



(b) Banking and Credit Industry



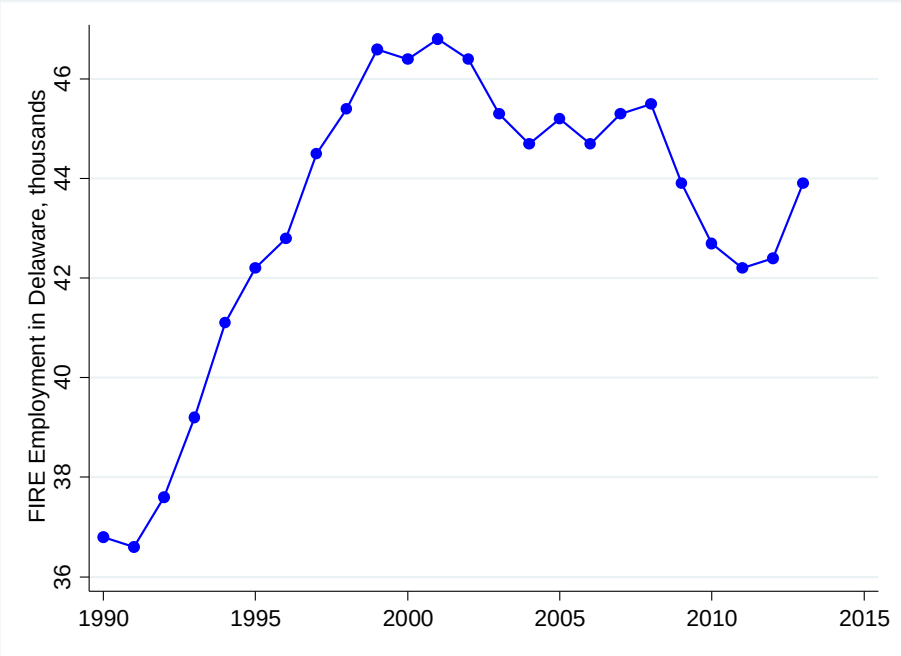
(c) Relevant Clerks, Accountants, and Managers



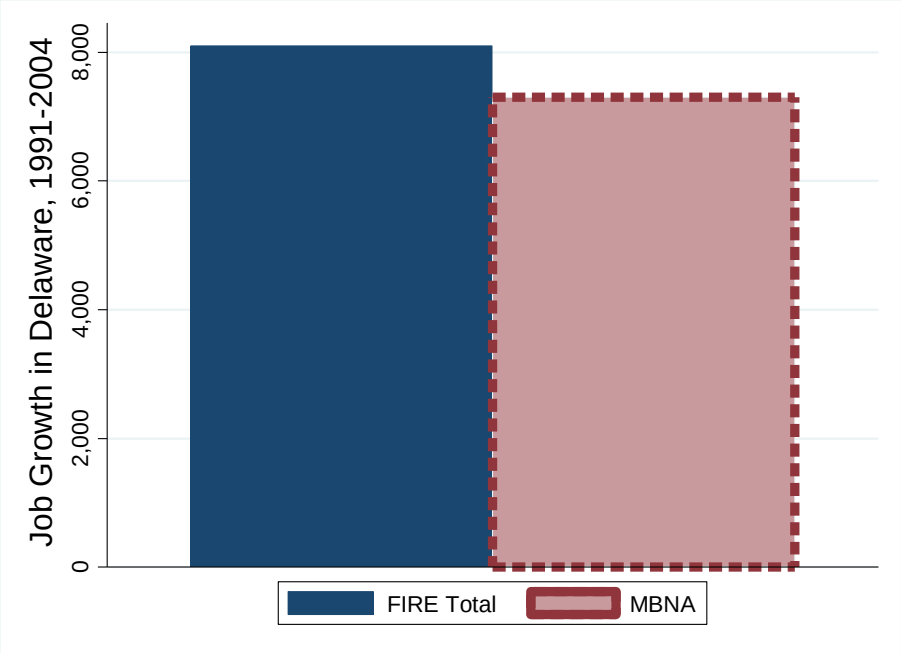
Note: Dependent variable is regression-adjusted average state wage, indexed to one in 1979 for Delaware, the bordering states' average, and the synthetic control. Wage data are from the 1970, 1980, 1990, and 2000 Censuses and the 2014 ACS. See text for details.

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

Panel A: FIRE Employment in Delaware, 1990-2015

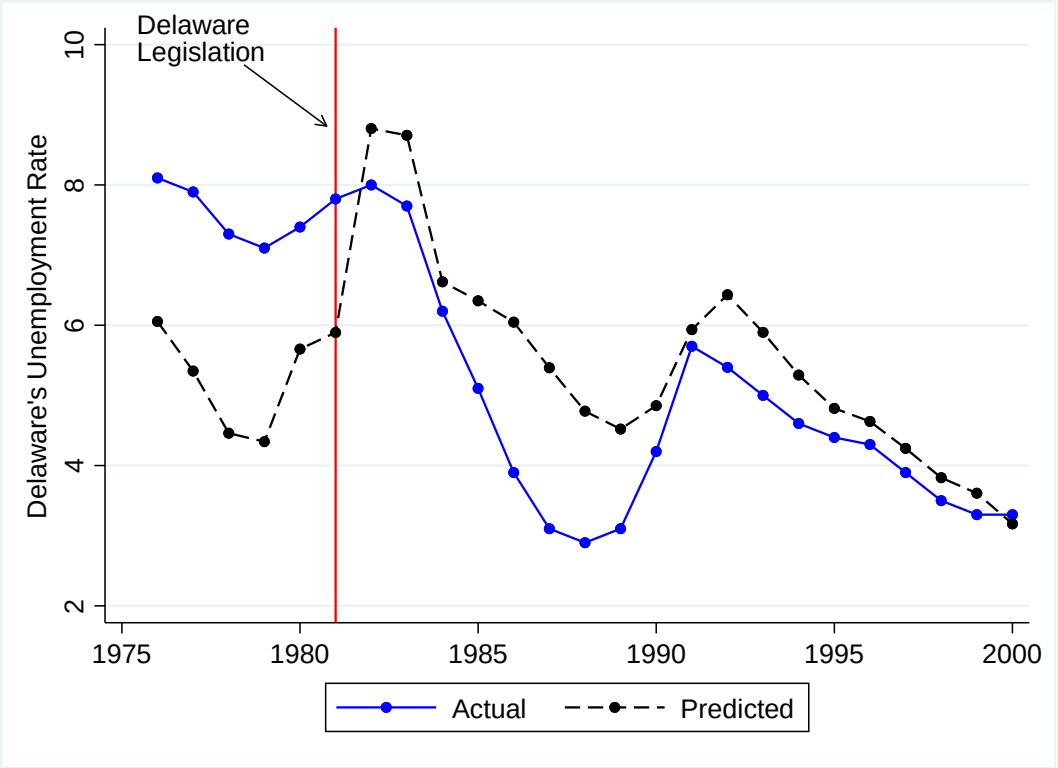


Panel B: FIRE Employment Growth Explained by MBNA Employment Growth



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: 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: Synthetic Control Composition

Ohio	0.282
Virginia	0.230
Indiana	0.111
Connecticut	0.091
Vermont	0.091
Michigan	0.079
Florida	0.050
Maryland	0.045
South Carolina	0.021

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

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

Predictors		Delaware	Bordering States	Synthetic Control
Share Employed in				
FIRE				
	1960-1964	0.040	0.046	0.043
	1965-1969	0.040	0.045	0.042
	1970-1974	0.049	0.048	0.046
	1975-1979	0.046	0.050	0.048
	1980	0.047	0.052	0.051
Manufacturing				
	1960-1964	0.37	0.35	0.35
	1965-1969	0.36	0.32	0.34
	1970-1974	0.31	0.28	0.30
	1975-1979	0.28	0.24	0.27
	1980	0.27	0.22	0.25
High-Tech Manufacturing				
	1960-1964	0.17	0.050	0.14
	1965-1969	0.17	0.047	0.14
	1970-1974	0.15	0.088	0.13
	1975-1979	0.14	0.097	0.13
	1980	0.13	0.095	0.12
Services				
	1960-1964	0.13	0.14	0.13
	1965-1969	0.13	0.15	0.14
	1970-1974	0.15	0.17	0.16
	1975-1979	0.17	0.19	0.18
	1980	0.18	0.20	0.19
% Metropolitan,	1980	67.0	87.4	72.7
% with ≥ a HS Diploma,	1980	68.6	66.5	66.3
% age 15-64				
	1960	59.9	61.2	59.4
	1970	61.8	62.5	61.8
	1980	67.8	67.3	66.7
Unemployment Rate				
	1970-1974	0.049	0.049	0.051
	1975-1979	0.080	0.075	0.068
	1980	0.074	0.073	0.075
Population (1981 = 1)				
	1960-1964	0.79	0.86	0.82
	1965-1969	0.88	0.93	0.89
	1970-1974	0.95	0.98	0.94
	1975-1979	0.99	0.99	0.97
	1980	1.00	1.00	0.99
Housing Prices (1981 = 1)				
	1975-1979	1.15	1.06	1.09
	1980	1.07	1.05	1.07

Note: This table compares the balance of a subset of predictor variables in the synthetic control, Delaware, and states bordering Delaware. Text and appendix table contain a complete list and balance.

Table 3: Labor Market Effects: Differences-in-Differences Relative to Pre-Policy Growth

	(1)	(2)	(3)	(4)	(5)
	Ln(FIRE Empl.)	Ln(Empl.)	Ln(Pop.)	Unempl. Rate	LF/Pop.
Panel A: Delaware Relative to Synthetic Control					
I. Decadal Changes					
1980-1989	0.7**	0.135*	0.040	-0.018	0.027
	(1/50)	(5/50)	(14/50)	(7/50)	(9/50)
	[.02]	[.1]	[.28]	[.14]	[.18]
1989-2000	0.488**	0.088*	0.085**	0.015	-0.023
	(1/50)	(5/50)	(2/50)	(39/50)	(40/50)
	[.02]	[.1]	[.04]	[.78]	[.8]
2000-2007		0.066	0.062**	-0.014	-0.034
		(6/50)	(2/50)	(7/50)	(48/50)
		[.12]	[.04]	[.14]	[.96]
2007-2010		0.017	0.017*	-0.001	-0.018
		(16/50)	(5/50)	(26/50)	(49/50)
		[.32]	[.1]	[.52]	[.98]
2010-2013		0.008		0.010	
		(17/50)		(43/50)	
		[.34]		[.86]	
II. Long-Run Changes					
1980-2010	1.188**	0.306*	0.204*	-0.018	-0.048
	(1/50)	(3/50)	(3/50)	(12/50)	(42/50)
	[.02]	[.06]	[.06]	[.24]	[.84]
Panel B: Delaware Relative to Bordering States					
I. Decadal Changes					
1980-1989	0.579***	0.105**	0.006	-0.011*	0.051**
	[.046]	[.037]	[.015]	[.005]	[.015]
1989-2000	0.445***	0.077*	0.046**	0.007	0.005
	[.058]	[.037]	[.015]	[.006]	[.019]
2000-2007		-0.002	0.035**	0.001	-0.017
		[.017]	[.012]	[.004]	[.013]
2007-2010		-0.022	0.001	-0.001	-0.02***
		[.015]	[.007]	[.004]	[.005]
2010-2013		0.003		0.0001	
		[.009]		[.002]	
II. Long-Run Changes					
1980-2010	1.024***	0.158	0.087	-0.003	0.019
	[.081]	[.107]	[.075]	[.014]	[.058]

Note: *** p<0.01, ** p<0.05, * p<0.1. In each column, the coefficients in Section I are from a single regression, and coefficients in Section II are from a separate regression. The dependent variables in Panel A are ((Y_t,DE - Y_t',DE)/(t-t')) - ((Y_t,synth - Y_t',synth)/(t-t')), for (t',t): (1960,1970), (1970,1980), (1980-1989), (1989-2000), (2000-2007), (2007-2010), (2010-2013). These regressions include fixed effects for t, and have as many observations as (t',t) pairs. The omitted group is (1970, 1980). The appendix shows coefficients for (t',t)= (1960, 1970). The effect's rank relative to placebo estimates is in parentheses and the p-value based on this rank in brackets. Dependent variables in Panel B are (Y_t,s - Y_t',s)/(t-t') and the explanatory variables include state fixed effects, fixed effects for t, and the time fixed effects interacted with Delaware. These regressions have as many observations as (t',t) pairs multiplied by four (Delaware plus three bordering states). Panel B shows coefficients on year group interacted with Delaware, with robust standard errors in brackets. In both Panels A and B, the estimates show the additional growth over the given period, and so are scaled up by (t-t'). In column 1, the long-run change is from 1980-2000 not 1980-2010. In the case of tied ranks, I assign the worse rank to Delaware. See paper for details.

Table 4: Cross-Industry Spillovers: Differences-in-Differences Relative to Pre-Policy Growth

	FIRE	Nontrad. excl. FIRE	Services	Trade	Transp. / Util.	Constr.	Manufact.	Govt.
Panel A: Delaware Relative to Synthetic Control								
I. Decadal Changes								
1980-1989	0.7**	0.123	0.135*	0.095	0.128	0.106	0.098	-0.014
	(1/50)	(9/50)	(5/50)	(12/50)	(12/50)	(18/50)	(9/50)	(32/50)
	[.02]	[.18]	[.1]	[.24]	[.24]	[.36]	[.18]	[.64]
New Jobs per New FIRE Job		1.87	0.75	0.62	0.18	0.18	0.81	-0.07
1989-2000	0.488**	0.096	0.037	0.155*	-0.031	0.109	-0.136	0.081
	(1/50)	(7/50)	(22/50)	(3/50)	(36/50)	(15/50)	(42/50)	(12/50)
	[.02]	[.14]	[.44]	[.06]	[.72]	[.3]	[.84]	[.24]
New Jobs per New 1980-89 FIRE Job	1.72	2.16	0.35	1.36	-0.06	0.26	-1.15	0.44
II. Long-Run Changes								
1980-2000	1.188**	0.219*	0.172	0.25*	0.096	0.216	-0.038	0.068
	(1/50)	(3/50)	(9/50)	(3/50)	(18/50)	(17/50)	(29/50)	(18/50)
	[.02]	[.06]	[.18]	[.06]	[.36]	[.34]	[.58]	[.36]
New Jobs per New 1980-89 FIRE Job		3.33	0.96	1.63	0.13	0.37	-0.31	0.36
Panel B: Delaware Relative to Bordering States								
I. Decadal Changes								
1980-1989	0.579***	0.058**	0.033	0.069***	0.092	-0.02	0.107*	0.081
	[.046]	[.022]	[.024]	[.014]	[.048]	[.082]	[.049]	[.08]
New Jobs per New FIRE Job		1.07	0.22	0.54	0.16	-0.04	1.07	0.51
1989-2000	0.445***	0.077	-0.01	0.162*	-0.06	0.149	-0.162*	0.168**
	[.058]	[.06]	[.054]	[.068]	[.073]	[.098]	[.079]	[.066]
New Jobs per New 1980-89 FIRE Job	1.89	2.10	-0.12	1.72	-0.13	0.44	-1.66	1.11
II. Long-Run Changes								
1980-2000	1.024***	0.135	0.022	0.23*	0.032	0.13	-0.056	0.249
	[.081]	[.072]	[.087]	[.095]	[.058]	[.1]	[.103]	[.18]
New Jobs per New 1980-89 FIRE Job		2.48	0.15	1.81	0.05	0.27	-0.56	1.58
Jobs in Delaware, 1980	12,300	130,800	47,900	56,000	12,100	14,700	70,900	45,200
Jobs in Delaware, 1989	30,300	193,900	82,200	75,500	15,300	20,800	73,100	47,100

Note: *** p<0.01, ** p<0.05, * p<0.1. See Table 3 notes for description of regressions. New jobs from 1980-1989 in sector Y per New FIRE job is calculated as (Beta_{Y,1980-1989}/Beta_{ln(FIRE),1980-1989})*(Jobs_{Y,1980}/Jobs_{FIRE,1980}). New jobs from 1980-2000 in sector Y per New FIRE job from 1980-1989 is calculated as (Beta_{Y,1980-2000}/Beta_{ln(FIRE),1980-1989})*(Jobs_{Y,1980}/Jobs_{FIRE,1980}). New jobs from 1989-2000 in sector Y per New FIRE job from 1980-1989 is calculated as (Beta_{Y,1989-2000}/Beta_{ln(FIRE),1980-1989})*(Jobs_{Y,1989}/Jobs_{FIRE,1980}). In case of tied ranks I assign the worse rank to Delaware. See paper for details.

Table 5: Effects on Wage Growth - Differences-in-Differences in Delaware Relative to Pre-Policy Growth

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Banking and Credit		Clerks, Accountants, Managers	
I. Decadal Changes						
(1) 1979-1989	0.074	0.041	0.049	0.013	0.070	0.043
	[11/50]	[.057]	[20/50]	[.053]	[11/50]	[.046]
	(.22)		(.4)		(.22)	
(2) 1989-1999	0.030	0.039	0.031	0.056	0.032	0.058**
	[10/50]	[.032]	[19/50]	[.033]	[12/50]	[.024]
	(.2)		(.38)		(.24)	
(3) 1999-2013	0.017	-0.005	0.009	-0.002	0.005	-0.009
	[19/50]	[.046]	[20/50]	[.044]	[24/50]	[.042]
	(.38)		(.4)		(.48)	
II. Long-Run Changes						
(4) 1979-2013	0.122	0.075	0.090	0.066	0.107	0.093
	[9/50]	[.111]	[17/50]	[.118]	[14/50]	[.085]
	(.18)		(.34)		(.28)	
Control	Synthetic	Bordering	Synthetic	Bordering	Synthetic	Bordering

Note: *** p<0.01, ** p<0.05, * p<0.1. See Table 3 notes. The dependent variables in columns 1, 3, and 5 are ((Ln(AvgWage_t,DE) - Ln(AvgWage_t',DE))/(t-t')) - ((Ln(AvgWage_t,synth) - Ln(AvgWage_t',synth))/(t-t')), for (t',t) in Section I: (1969, 1979), (1979, 1989), (1989, 1999), (1999, 2013), and in Section II (1969, 1979), (1979, 2013). The omitted group is (1969, 1979). Explanatory variables include indicators for t. The dependent variables in columns 2, 4, and 6 are (Ln(AvgWage_t,s) - Ln(AvgWage_t',s))/(t-t'). Explanatory variables include state fixed effects, indicators for t, and those indicators interacted with an indicator for Delaware. Average wages by state are based on the sample of workers aged 25-59 who usually worked at least 35 hours per week. These average wages by state are also regression-adjusted, controlling for years of education, a quadratic in potential experience, indicators for grouping of usual hours worked per week last year and weeks worked last year, and indicators for white, black, Asian, male, and married. In case of tied ranks I assign the worse rank to Delaware. See paper for details.

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

May 31, 2018

Fiscal and Regulatory Competition from Other States

As part of the Financial Center Development Act (FCDA), Delaware introduced a regressive tax on banks of 8.7% on the first $20 million of net income, 6.7% on net income from $20 to $25 million, 4.7% on net income from $25 to $30 million, and 2.7% on net income over $30 million (Moulton 1983).

Other states responded to Delaware's FCDA, and specifically to the elimination of the interest rate ceiling in Delaware. 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% surcharge on tax liability attributable to business in the Metropolitan Commuter Transportation District, later reduced to 17% in 1982. This surcharge remains in place today. The tax rate in 1981 for banks was 12%, reduced to 9% in 1985, to 7.5% in 1999 (over three years), and 7.1% 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%, 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% at the time of introduction in 1968, and at repeal in 2000 (Maryland Session Laws 1968, Michie's Annotated Code 2004).

While Pennsylvania responded as well, it did not match Delaware's policy. In 1982, Pennsylvania raised the interest rate ceiling from 15% to 18%, and also allowed banks to charge a fee of up to $15 per year (Erdevig 1988). Pennsylvania taxes banks and financial institutions based on their equity capital, rather than on their net income (as in Delaware and in most other states) (Pennsylvania Department of Revenue 2015). From 1971 through 1983, this tax rate was 1.5%. In 1984, it was reduced to 1.075%, in 1990 it increased to 1.25%, and in 2014 reduced to .89% (Pennsylvania Department of Revenue 2008, 2015). The effective rate on net income was estimated to be 9.84% based on the 2014 rate, making it higher than the top rate in Delaware (Pennsylvania Department of Revenue 2015).

From 1979 until 1991, South Dakota imposed a tax of 6% on the net income of financial institutions (South Dakota Session Laws 1979). In 1991, South Dakota introduced a regressive tax on the net income of financial institutions, with a lower top rate, and a lower bottom rate than Delaware (South Dakota Session Laws 1991).

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, conveyed 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 as $Age - Education - 6$, and set this to zero if it is less than zero. I code education as 0 if the *educ* variable from

the CPS denoted the respondent received no education, preschool, or kindergarten. I code education as 4 if the individual attained nursery school to grade 4 (Census code 1); 8 if grades 5, 6, 7, or 8 (Census code 2); for grades 9 through 12 I code the education variable as the grade attained. I code education as 13 for 1 year of college; 14 for two years of college; 15 for 3 years of college; 16 for 4 years of college; and 17 for 5+ years of college (Census code 11).

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

I code groupings of hours and weeks worked in the CPS to be consistent with the census variable. I include indicators for the following groups of usual hours worked per week last year: 1 through 14 hours, 15 through 29 hours, 30 through 34 hours, 35 through 39 hours, 40 hours, 41 through 49 hours, 49 through 59 hours, and 60 hours. In 1970, usual hours worked per week last year is not available. Instead I use hours worked last week, in the same groupings as defined above. 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.

I define high-technology manufacturing using the SIC major industry groups corresponding to the Level I and Level II high-technology industries identified in Hecker (2005). These are: Industrial and Commercial Machinery and Computer Equipment (35); Electronic and Other Electrical Equipment and Components, Except Computer Equipment (36); Transportation Equipment (37); Measuring, Analyzing, and Controlling Instruments, Photographic, Medical and Optical Goods, Watches and Clocks (38); Chemicals and Allied Products (28); Rubber and Miscellaneous Plastics Products (30). Hecker's definitions identify 4-digit NAICS codes, and I use the three-digit SIC codes containing those industries.

To obtain a longer time series for total employment I use total employment from the CES, NAICS basis. Unlike the NAICS-basis data for total employment, NAICS-basis data by industry are only available starting in 1990. When constructing shares of total employment by industry, the denominator is total employment, SIC basis. As a result, I measure employment by industry as a share of total non-farm employment.

State unemployment rates constructed from labor market areas, provided by Larry Katz and used in Blanchard and Katz (1992) were normalized to equal the LAUS unemployment rate in 1976.

Synthetic Control

In the principal synthetic control, I include as predictors five year averages from 1960 through 1979, as well as the value in 1980 of the following variables: population and employment indexed to one in 1981, share of employment in construction; FIRE; manufacturing; high-technology manufacturing; trade; services; transportation and utilities; government. I include five year averages from 1970 through 1979, and the value in 1980 of the unemployment rate. I include the average from 1976 through 1980 of labor force over population, and the average from 1975 through 1979, as well as the value in 1980, of housing prices indexed to one in 1981.

States Losing Population to Delaware

The top five states losing population to Delaware from 1985 to 1990, relative to 1975 to 1980 are mostly those in close geographic proximity: (with difference in the fraction of population lost to Delaware in parentheses): Maryland (.00043), Pennsylvania (.00039), New Jersey (.00035), West Virginia (.00025), and Rhode Island (.00023).

Comparison to BEA RIMS II Multipliers

The Bureau of Economic Analysis (BEA) calculates multipliers using input-output models (RIMS II Multipliers), and they are used by researchers, government officials, and planners (Bureau of Economic Analysis 2013). Other organizations provide input-output multipliers as well.

There are several reasons why RIMS II Multipliers are not well-suited for studying the impact of the FCDA. First, the historical multipliers are not available, and so they are based on 2007 national input-output tables and 2015 regional data, rather than the structure of Delaware's economy at the time it received the shock.

Even with multipliers constructed in the appropriate year, there are several important underlying assumptions of I-O models, such as fixed prices and purchase patterns, which may be problematic for studying the impact of a large economic shock. Further, the RIMS II multipliers will not capture any increase in production resulting from the new finance jobs serving as inputs (which may occur through agglomeration). RIMS II multipliers will only capture the additional demand for inputs

from the new finance jobs.

Despite these shortcomings, it is useful to compare the multipliers I estimate to multipliers that are used by practitioners considering local economic development policies. Averaging the RIMS II finance employment multiplier across all states, an additional job in finance yields an average 1.6 jobs outside of finance (1.7 in the synthetic control, and 2 averaging the multipliers in Delaware's bordering states (and in Delaware)).[1] As there is no time dimension in I-O models, it is unclear how long it should be before these impacts are realized. These multipliers are quite similar to short-run multipliers I estimate from finance to non-FIRE nontradable employment (1.87 using the synthetic control and 1.07 using bordering states, though neither is statistically significant).

However, the RIMS II multipliers are significantly lower than the long-run multiplier I estimate from finance to non-FIRE nontradable employment and FIRE employment after the initial policy effect (5 using the synthetic control and 4.4 using the bordering states), which may reflect the drawbacks of the RIMS II methodology discussed in the paper (not capturing forward linkages, and Delaware finance jobs not reflecting the overal industry).

There are several other reasons that might explain why the RIMS II multipliers are smaller than the long-run multipliers I estimate. RIMS II multipliers assume no regional feedback. Suppose increasing finance jobs creates demand for inputs from another industry that is not located in Delaware, and this industry uses inputs from a Delaware industry. These inputs from the Delaware industry will not be included in the multiplier since the demand came from a non-Delaware source. Given Delaware is a small state, and businesses in the Wilmington-area may have connections with businesses in nearby Pennsylvania, this type of feedback may exist, and would result in the RIMS II multipliers underestimating the true multipliers.

Similarly, the RIMS II multipliers use national input-output tables, but adjust these to account for local supply conditions. In particular, they account for the possibility that Delaware industries may not supply all of the needed inputs resulting from the increased demand for finance. RIMS II multipliers adjust for leakages from the Delaware economy by considering the concentration of each industry in Delaware

[1]Specifically, I use the Type II direct effect employment multipliers for Federal Reserve banks, credit intermediation, and related activities. This includes both depository and nondepository credit intermediation, with related 2007 NAICS codes of 521, 5221, 5222-3). Type II multipliers include the impact of the additional finance jobs on household spending.

relative to nationally. Because historical multipliers are not available, the RIMS II multipliers use the structure of Delaware's economy in 2015. Manufacturing in Delaware has fallen substantially since the original finance shock in 1981. If relative to 1981, manufacturing in Delaware is less concentrated relative to the US, RIMS II multipliers will make stronger downward adjustments. The assumption will be that goods are more likely to be manufactured outside of Delaware, relative to using the economic structure in 1981.

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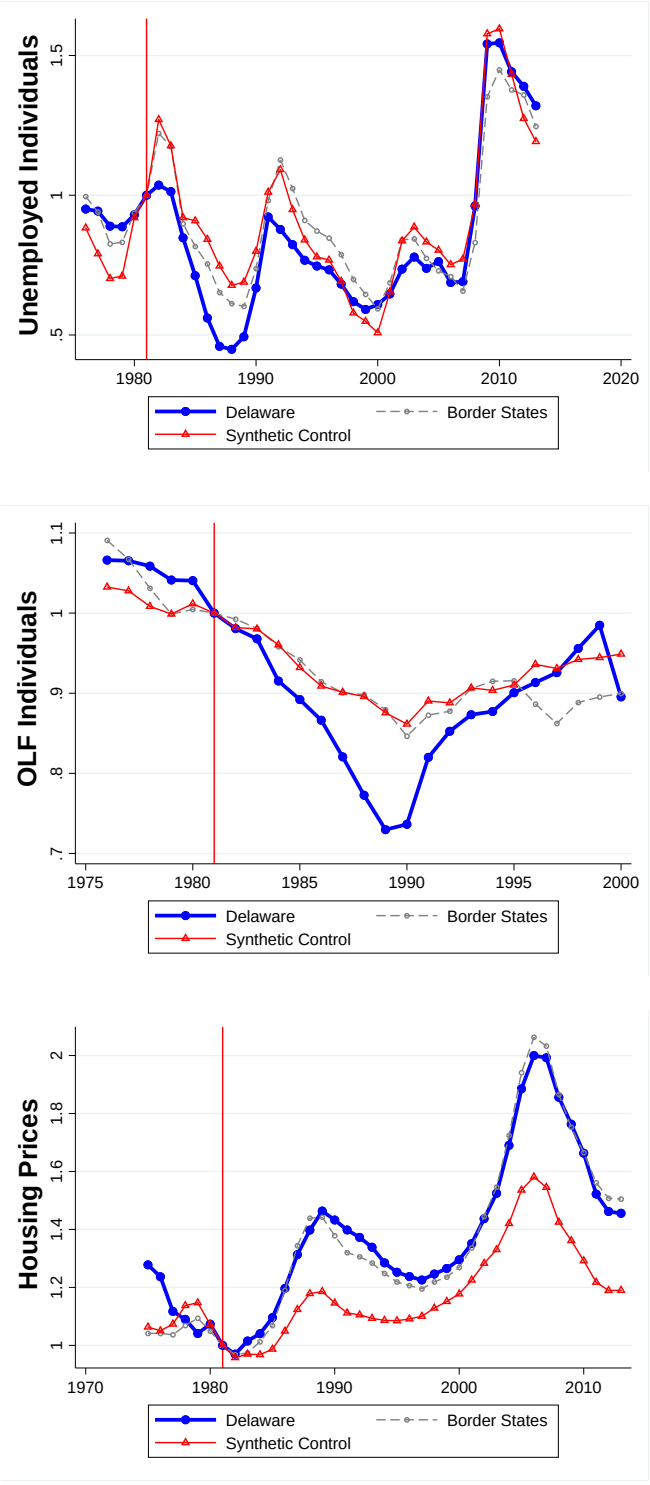
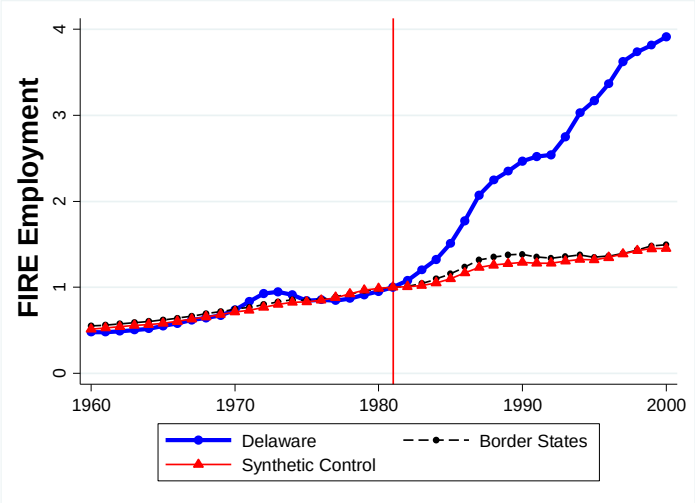
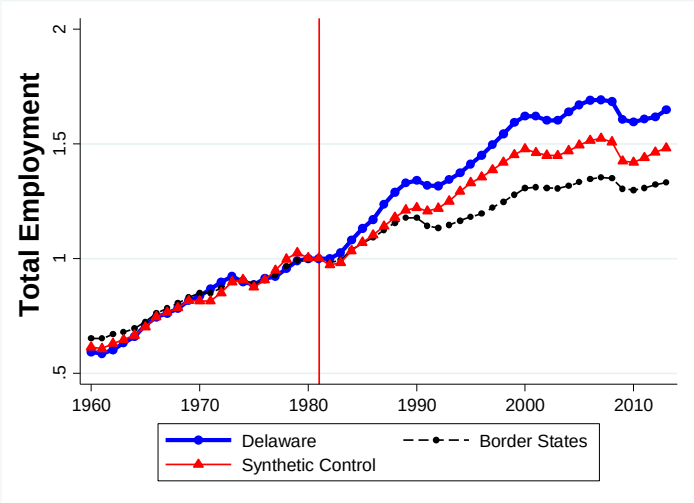
Appendix Figure A1: Additional Policy Effects

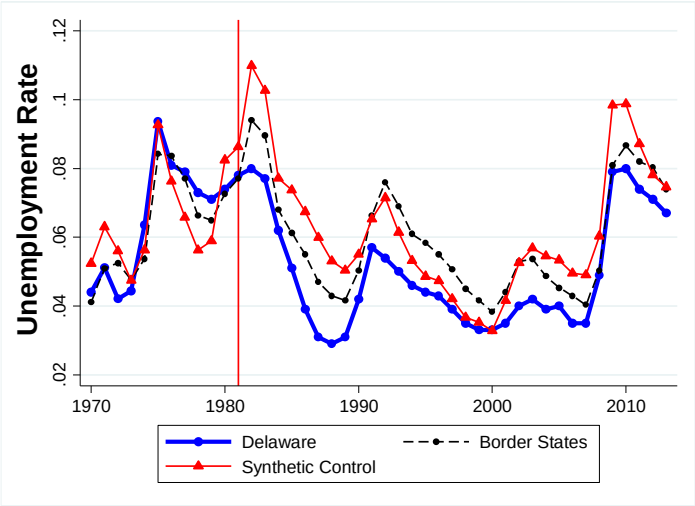


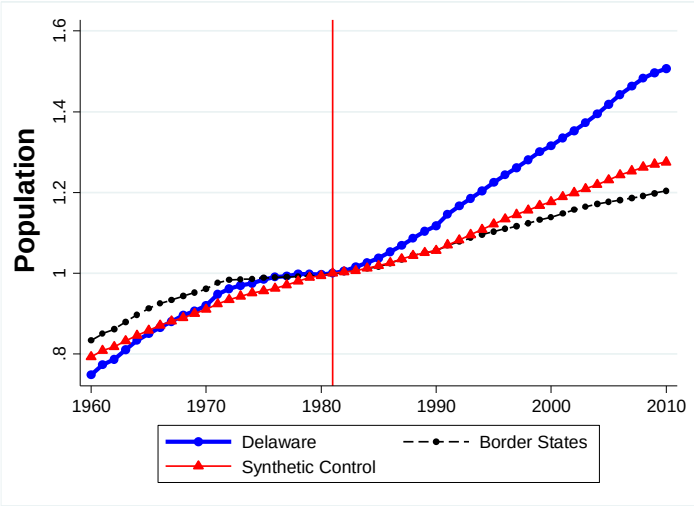
Note: Outcomes are indexed to one in 1981 for Delaware, the bordering states' average, and the synthetic control. I construct OLF individuals by subtracting the labor force from the population age 15 to 64. See paper and Appendix Table A1 for details.

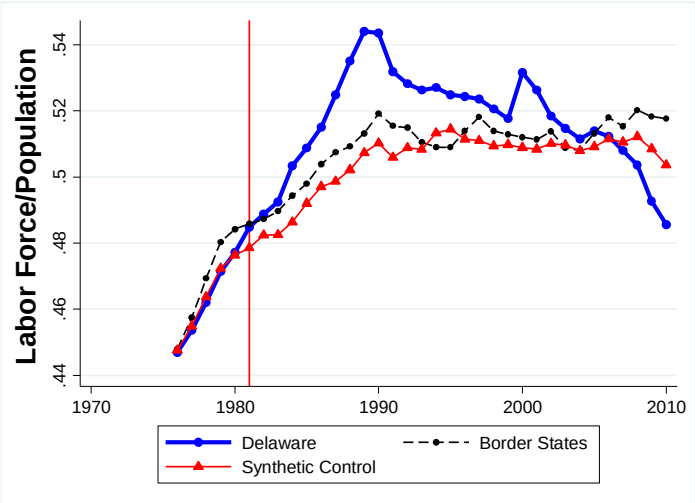
Appendix Figure A2: Policy Effect on the Labor Market, Allowing the Synthetic Control to Vary by Outcome

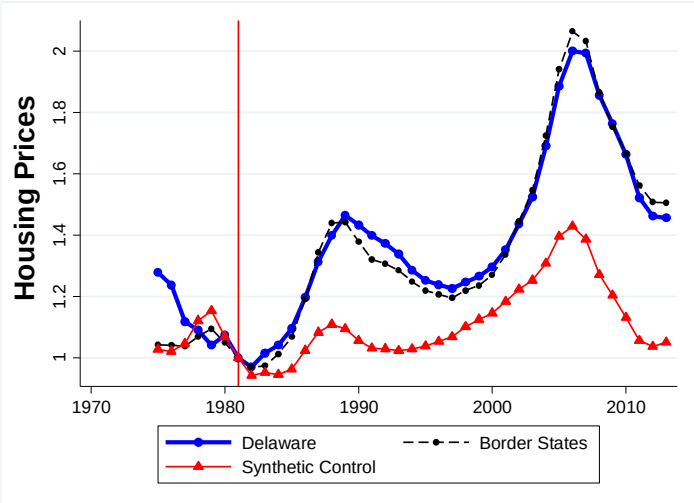






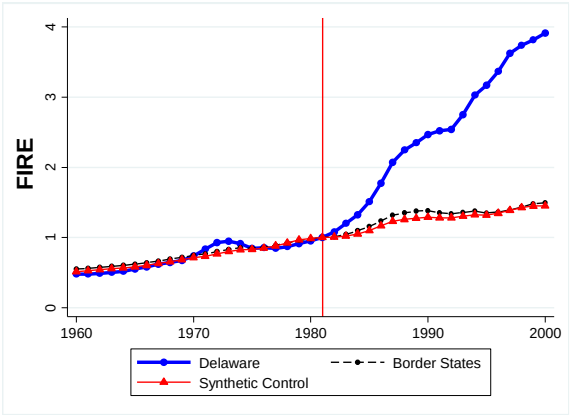


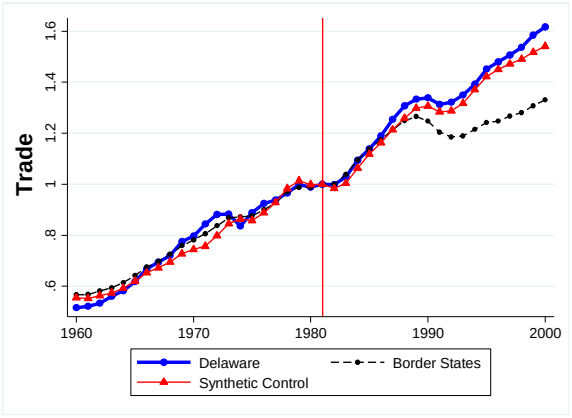


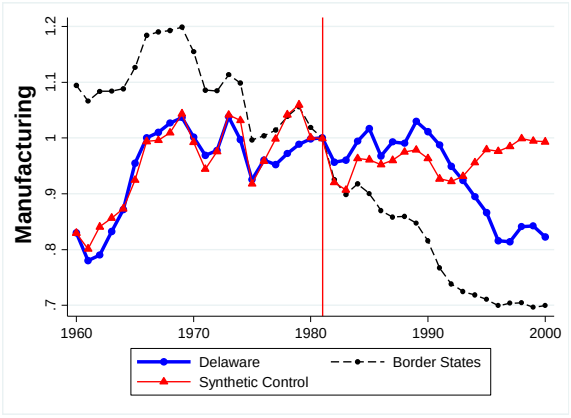


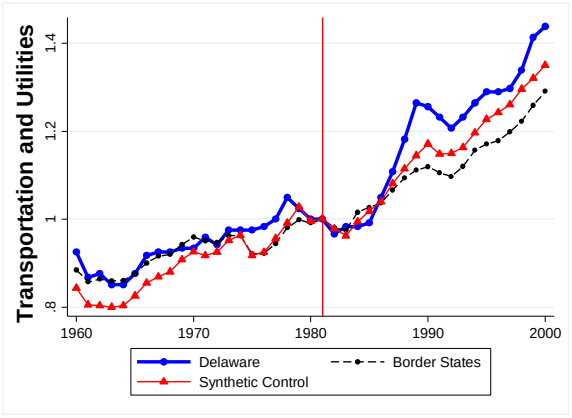
Note: Figures show variables indexed to one in 1981 for Delaware, the bordering states' average, and synthetic control, except for the unemployment rate and labor force/population. See paper for details on construction of the synthetic control.

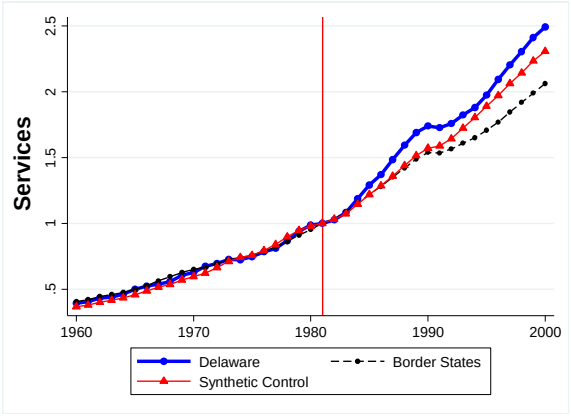
Appendix Figure A3: Cross-Industry Spillovers: Allowing the Synthetic Control to Vary Across Outcome

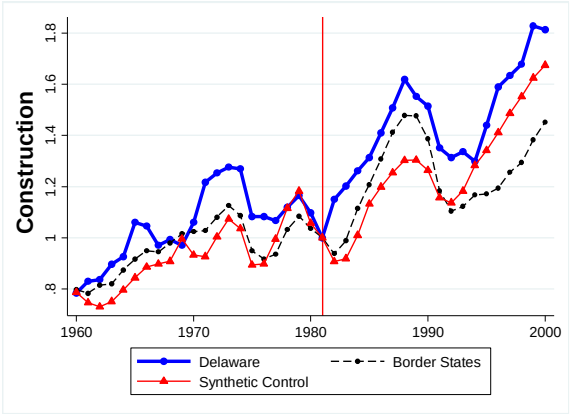


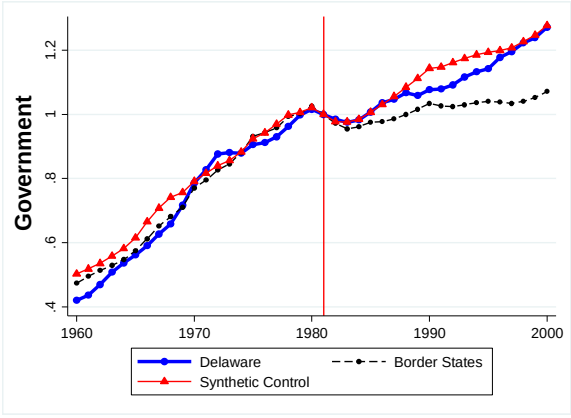






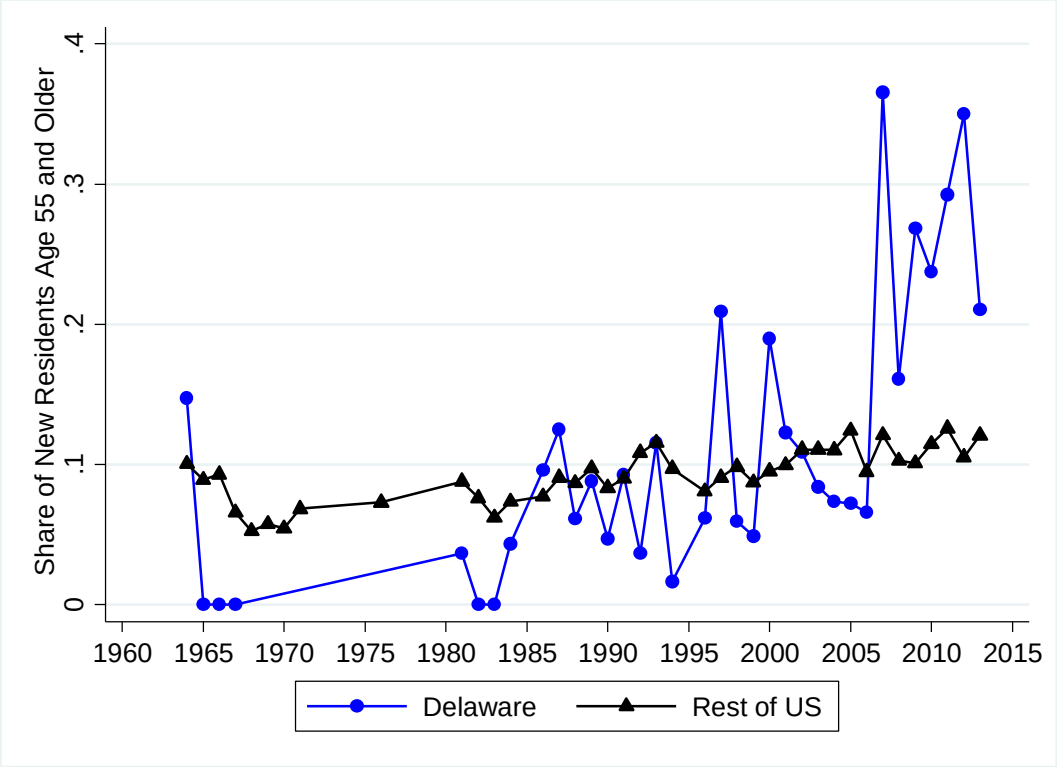






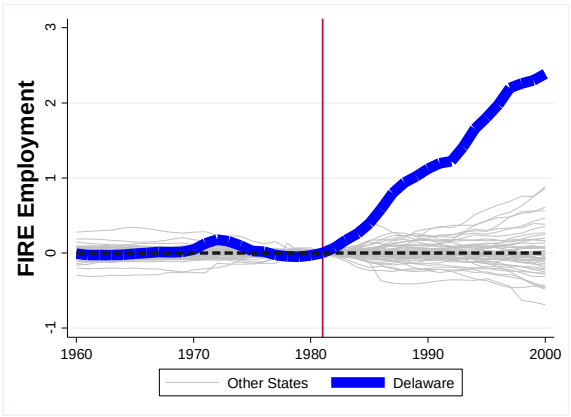
Note: Figures show variables indexed to one in 1981 for Delaware, the bordering states' average, and synthetic control. See text for details on construction of the synthetic control.

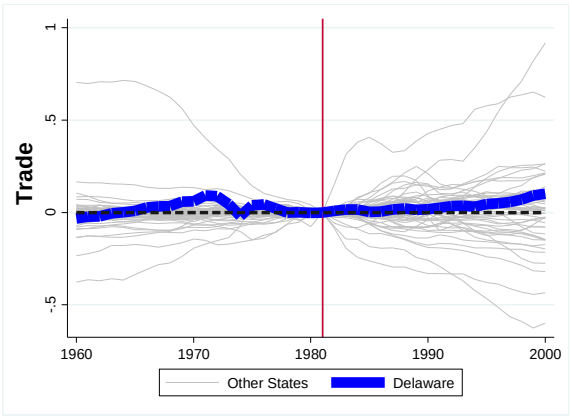
Appendix Figure A4: Share of New Residents 55 and Older

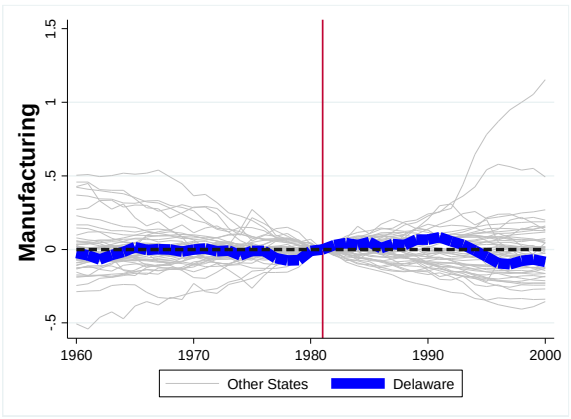


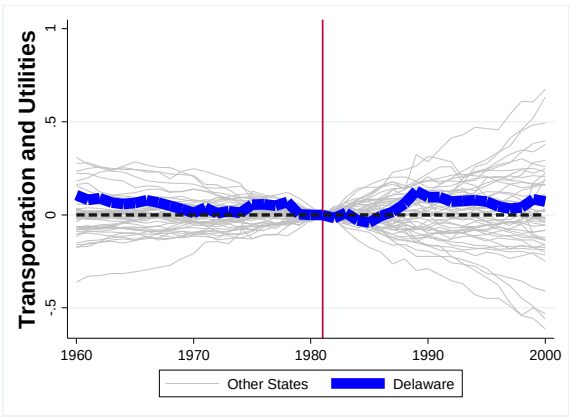
Note: This plot is based on CPS Microdata and compares the weighted share of residents who migrated across states last year who are 55 and older. The data are missing for Delaware from 1968 through 1976, and there were no new Delaware residents in the CPS from 1977 through 1980, or in 1985 and 1995.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

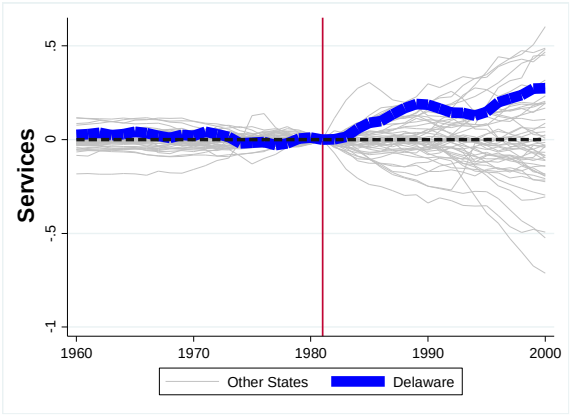
Appendix Figure A5: Cross-Industry Spillovers Inference, Estimated Effects in Delaware Relative to Placebos

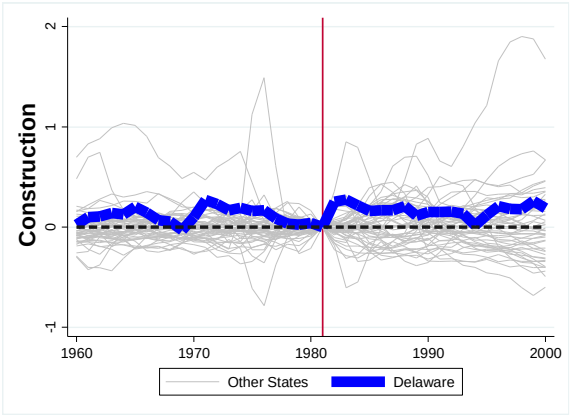


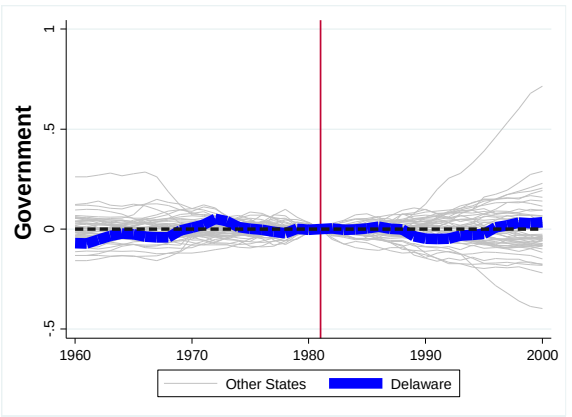








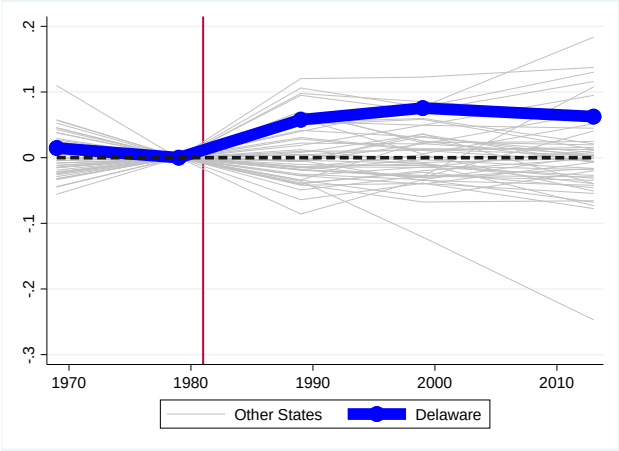




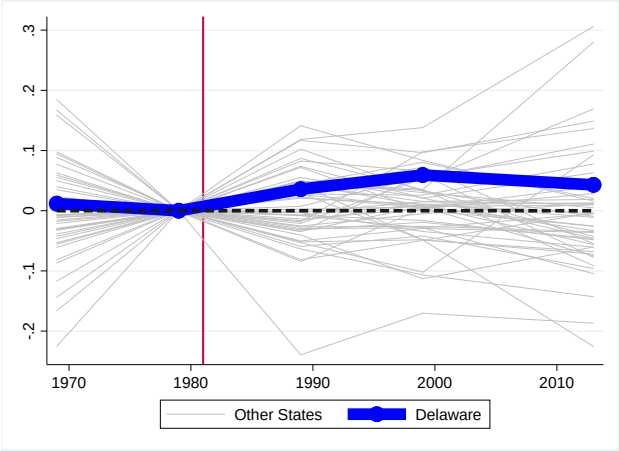
Note: Figures show the difference in outcomes between Delaware and the synthetic control, and between other states and their synthetic control. Outcomes indexed to one in 1981. See paper for details.

Appendix Figure A6: Inference: Estimated Effects on Average Wages in Delaware Relative to Placebos

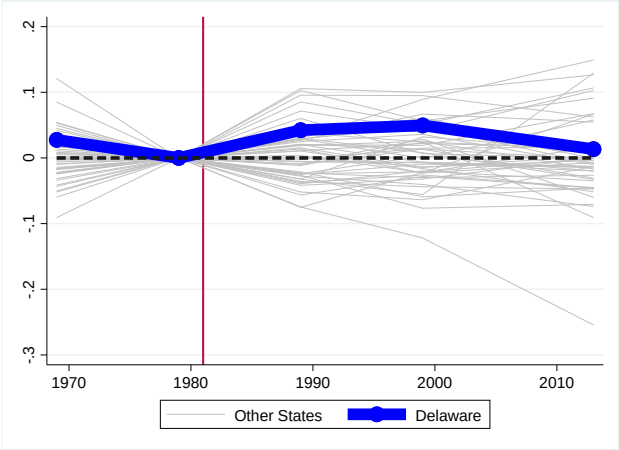
(a) All Occupations and Industries



(b) Banking and Credit Industry



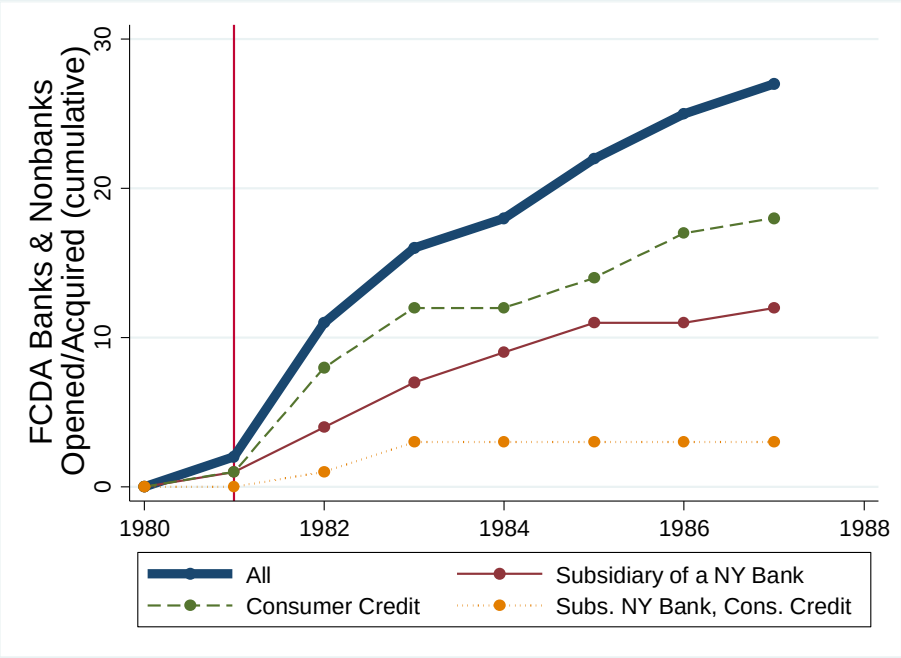
(c) Relevant Clerks, Accountants, and Managers



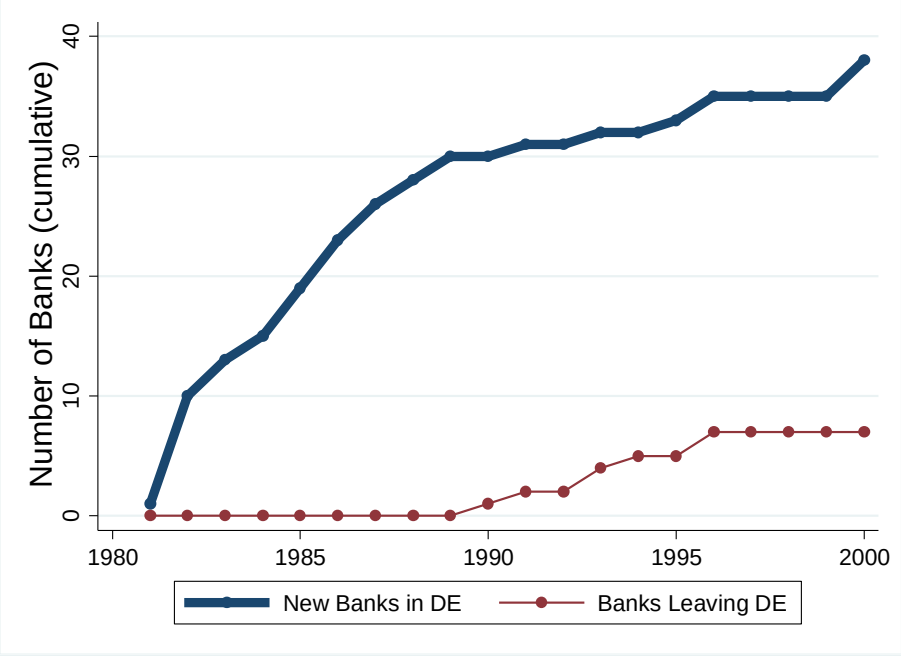
Note: Figures show the difference in outcomes between Delaware and the synthetic control, and between other states and their synthetic control. Outcomes are regression-adjusted average state wages for full-time workers, indexed to one in 1979 in both the treatment and synthetic control. See text and Table 5 notes for details.

Appendix Figure A7: 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.

Appendix Table A1: Additional Policy Effects - Differences-in-Differences Relative to Pre-Policy Growth

	Ln(Unemployed)	Ln(OLF)	Ln(Housing Price Index)
Panel A: Delaware Relative to Synthetic Control			
I. Decadal Changes			
1980-1989	-0.124	-0.196	0.6**
	(17/50)	(6/50)	(2/50)
	[.34]	[.12]	[.04]
1989-2000	0.780	0.146	0.317*
	(47/50)	(38/50)	(4/50)
	[.94]	[.76]	[.08]
2000-2007	-0.145		0.477*
	(18/50)		(3/50)
	[.36]		[.06]
2007-2010	0.173		0.118
	(40/50)		(7/50)
	[.8]		[.14]
2010-2013	0.207		0.072*
	(42/50)		(4/50)
	[.84]		[.08]
II. Long-Run Changes			
1980-2010	0.68	-0.051	1.511*
	(28/50)	(24/50)	(2/50)
	[.56]	[.48]	[.04]
Panel B: Delaware Relative to Bordering States			
I. Decadal Changes			
1980-1989	-0.237	-0.351**	0.345*
	[.307]	[.091]	[.161]
1989-2000	0.173	0.024	0.425**
	[.373]	[.085]	[.18]
2000-2007	-0.009		0.233
	[.259]		[.145]
2007-2010	0.008		0.13
	[.121]		[.08]
2010-2013	-0.024		0.082
	[.105]		[.152]
II. Long-Run Changes			
1980-2010	-0.065	-0.327	1.132
	[1.101]	[.179]	[.415]

Note: *** p<0.01, ** p<0.05, * p<0.1. See notes to Table 3 for regression details. The variable OLF is the difference between the population aged 15-64 and the labor force. Population age 15 to 64 is available in the census years, and I use this to obtain the proportion of the population age 15 to 64 in the census years. I then impute the population age 15 to 64 in non-census years by assuming the proportion age 15 to 64 increases linearly between the censuses, and then I multiply this by the population in each year. In column 2, the long-run change is from 1980-2000 not 1980-2010. In the case of tied ranks, I assign the worse rank to Delaware.

Appendix Table A2: Labor Market Effects - Differences-in-Differences Relative to Pre-Policy Growth
Delaware Relative to Bordering States

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(FIRE Employment)	Ln(Employment)	Ln(Population)	Unemployment Rate	Labor Force/ Population	Ln(Housing Price Index)
I. Decadal Changes						
1960-1970	0.004	0.011*	0.011**			
	(0.006)	(0.006)	(0.004)			
1980-1989	0.010*	0.002	0.003	-0.007***	-0.006***	0.032
	(0.005)	(0.004)	(0.002)	(0.001)	(0.002)	(0.018)
1989-2000	-0.020***	-0.007*	0.004**	-0.003***	-0.009***	-0.014
	(0.005)	(0.003)	(0.001)	(0.001)	(0.002)	(0.016)
2000-2007		-0.012***	0.002	-0.003***	-0.009***	0.064**
		(0.002)	(0.002)	(0.001)	(0.002)	(0.021)
2007-2010		-0.030***	0.002	0.012***	-0.008***	-0.068**
		(0.005)	(0.002)	(0.001)	(0.002)	(0.027)
2010-2013		-0.008**		-0.007***		-0.036*
		(0.003)		(0.001)		(0.018)
DE	-0.005	-0.004	-0.00005	-0.00008	-0.002	-0.037**
	(0.004)	(0.003)	(0.002)	(0.001)	(0.002)	(0.015)
1960-1970*DE	0.013*	0.005	0.001			
	(0.006)	(0.006)	(0.004)			
1980-1989*DE	0.064***	0.012**	0.001	-0.001*	0.006**	0.038*
	(0.005)	(0.004)	(0.002)	(0.001)	(0.002)	(0.018)
1989-2000*DE	0.040***	0.007*	0.004**	0.001	0.0004	0.039**
	(0.005)	(0.003)	(0.001)	(0.001)	(0.002)	(0.016)
2000-2007*DE		-0.0002	0.005**	0.0002	-0.002	0.033
		(0.002)	(0.002)	(0.001)	(0.002)	(0.021)
2007-2010*DE		-0.007	0.0002	-0.0003	-0.007***	0.043
		(0.005)	(0.002)	(0.001)	(0.002)	(0.027)
2010-2013*DE		0.001		0.00005		0.027
		(0.003)		(0.001)		(0.018)
II. Long-Run Changes						
1960-1970	0.004	0.011**	0.011**			
	(0.006)	(0.004)	(0.004)			
1980-2010	-0.006	-0.008	0.003	-0.003**	-0.008*	0.012
	(0.004)	(0.004)	(0.002)	(0.0005)	(0.002)	(0.014)
DE	-0.006	-0.007***	-0.001	-0.0002	-0.003	-0.040**
	(0.005)	(0.003)	(0.003)	(0.0003)	(0.002)	(0.008)
1960-1970*DE	0.013*	0.005	0.001			
	(0.006)	(0.004)	(0.004)			
1980-2010*DE	0.051***	0.005	0.003	-0.0001	0.001	0.038
	(0.004)	(0.004)	(0.002)	(0.0005)	(0.002)	(0.014)

Note: *** p<0.01, ** p<0.05, * p<0.1. See notes to Table 3 for description of regressions. In column 1, the long-run change is from 1980-2000 not 1980-2010. These estimates are not scaled up by (t-t') as in Table 3.

Appendix Table A3: Cross-Industry Spillover Effects - Differences-in-Differences Relative to Pre-Policy Growth
Delaware Relative to Bordering States

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	FIRE	Nontrad. excl. FIRE	Services	Trade	Transp./ Util.	Constr.	Manufact.	Govt.
I. Decadal Changes								
1960-1970	0.004	0.009	0.010	0.010	0.006	0.024***	0.018***	0.022
	(0.006)	(0.005)	(0.007)	(0.007)	(0.004)	(0.004)	(0.004)	(0.011)
1980-1989	0.010*	0.012***	0.011***	0.004**	0.009	0.037***	-0.008	-0.030**
	(0.005)	(0.002)	(0.003)	(0.002)	(0.005)	(0.009)	(0.005)	(0.009)
1989-2000	-0.020***	-0.008	-0.009	-0.019**	0.010	-0.003	-0.005	-0.024***
	(0.005)	(0.005)	(0.005)	(0.006)	(0.007)	(0.009)	(0.007)	(0.006)
DE	-0.005	-0.005	-0.001	-0.007*	0.001	-0.006	0.010*	-0.011
	(0.004)	(0.003)	(0.004)	(0.004)	(0.003)	(0.006)	(0.004)	(0.007)
1960-1970*DE	0.013*	0.002	-0.009	0.012	-0.012**	0.002	0.001	0.015
	(0.006)	(0.005)	(0.007)	(0.007)	(0.004)	(0.004)	(0.004)	(0.011)
1980-1989*DE	0.064***	0.006**	0.004	0.008***	0.010	-0.002	0.012*	0.009
	(0.005)	(0.002)	(0.003)	(0.002)	(0.005)	(0.009)	(0.005)	(0.009)
1989-2000*DE	0.040***	0.007	-0.001	0.015*	-0.005	0.014	-0.015*	0.015**
	(0.005)	(0.005)	(0.005)	(0.006)	(0.007)	(0.009)	(0.007)	(0.006)
II. Long-Run Changes								
1960-1970	0.004	0.009	0.010	0.010	0.006	0.024***	0.018***	0.022*
	(0.006)	(0.005)	(0.007)	(0.007)	(0.005)	(0.003)	(0.003)	(0.010)
1980-2000	-0.006	0.001	0.000	-0.009	0.010**	0.015**	-0.007	-0.027**
	(0.004)	(0.004)	(0.004)	(0.005)	(0.003)	(0.005)	(0.005)	(0.009)
DE	-0.006	-0.006	-0.002	-0.008	0.001	-0.007	0.011**	-0.013
	(0.005)	(0.004)	(0.005)	(0.004)	(0.002)	(0.004)	(0.004)	(0.008)
1960-1970*DE	0.013*	0.002	-0.009	0.012	-0.012*	0.002	0.001	0.015
	(0.006)	(0.005)	(0.007)	(0.007)	(0.005)	(0.003)	(0.003)	(0.010)
1980-2000*DE	0.051***	0.007	0.001	0.012*	0.002	0.006	-0.003	0.012
	(0.004)	(0.004)	(0.004)	(0.005)	(0.003)	(0.005)	(0.005)	(0.009)

Note: *** p<0.01, ** p<0.05, * p<0.1. See notes to Table 3 for description of regressions. These coefficients are not scaled up by (t-t') as in Table 4.

Appendix Table A4: Differences-in-Differences Relative to Pre-Policy Growth (1970-1980)
Delaware Relative to Synthetic Control

Panel A: Labor Market Effects, Decadal Changes

	(1)	(2)	(3)
	FIRE Empl.	Empl.	Pop.
1960-1970	0.18*	0.089*	0.069
	(5/50)	(5/50)	(6/50)
	[.1]	[.1]	[.12]

Panel B: Cross-Industry Spillover Effects, Decadal Changes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	FIRE	Nontrad. excl. FIRE	Services	Trade	Transp./ Util.	Constr.	Manufact.	Govt.
1960-1970	0.18*	0.098	-0.013	0.222*	-0.104	0.114	0.033	0.165*
	(5/50)	(8/50)	(27/50)	(3/50)	(41/50)	(17/50)	(26/50)	(5/50)
	[.1]	[.16]	[.54]	[.06]	[.82]	[.34]	[.52]	[.1]

Note: *** p<0.01, ** p<0.05, * p<0.1. All outcomes in logs. See Table 3 notes for description of regressions. These coefficients are from the same regressions as those in Section I (decadal changes) of Panel A of Tables 3 and 4, but were not included in those tables. In case of tied ranks I assign the worse rank to Delaware.

Appendix Table A5: Composition of Synthetic Control, Predictor Weights Determined by Regressions

	FIRE Empl.	Empl.	Pop.	Unemp. Rate	LF/Pop.	Housing Prices	Nontrad. excl. FIRE	Services	Trade	Transp./ Util.	Constr.	Manufact.	Govt.
Alaska	0.041	0.042	0.034	0	0	0	0.043	0.044	0.042	0.043	0.037	0.043	0.041
Arizona	0.066	0.058	0.056	0.075	0.132	0.129	0.061	0.06	0.06	0.065	0.056	0.073	0.05
Connecticut	0.219	0.221	0.171	0.161	0.166	0.163	0.22	0.218	0.22	0.222	0.215	0.174	0.216
Florida	0	0	0	0	0.005	0	0	0	0	0	0	0	0
Indiana	0.231	0.201	0.158	0.252	0.275	0.29	0.208	0.198	0.221	0.195	0.188	0.235	0.225
Maryland	0	0.015	0.055	0.042	0	0.026	0.005	0	0	0.006	0.026	0.049	0.029
Michigan	0.203	0.212	0.241	0.133	0.027	0.049	0.207	0.202	0.205	0.217	0.215	0.187	0.205
New Hampshire	0	0	0	0.019	0	0	0	0	0	0	0	0	0
North Dakota	0	0	0	0	0.002	0	0	0	0	0	0	0	0
Ohio	0.014	0.03	0.075	0.24	0.303	0.209	0.03	0.049	0.018	0.029	0.049	0.036	0.015
South Carolina	0.102	0.109	0.117	0	0	0.015	0.106	0.105	0.101	0.115	0.115	0.133	0.108
Vermont	0	0	0	0	0	0.051	0	0	0	0	0	0	0
Virginia	0.038	0.021	0	0.044	0.089	0.059	0.032	0.037	0.047	0.019	0.003	0	0.018
Wyoming	0.085	0.091	0.091	0.033	0	0.01	0.088	0.087	0.087	0.089	0.096	0.069	0.091

Note: Each column shows the composition of the synthetic control for the given outcome. All outcomes are in logs, except the unemployment rate and labor force/population. The synthetic control is constructed using predictor weights that minimize the mean squared prediction error of the outcome variable in the pre-policy period (as in Abadie, Diamond, and Hainmueller 2010, 2014), allowing the composition of the control to vary by outcome. See text and appendix for details.

Appendix Table A6: Labor Market Effects - Differences-in-Differences Relative to Pre-Policy Growth Allowing for Differences in the Synthetic Control Across Outcome

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(FIRE Empl.)	Ln(Empl.)	Ln(Pop.)	Unemp. Rate	LF/ Pop.	Ln(Housing Price Index)
Panel A: Delaware Relative to Synthetic Control						
I. Decadal Changes						
1980-1989	0.791**	0.211**	0.08*	-0.011	0.032*	0.717**
	(1/50)	(2/50)	(5/50)	(14/50)	(5/50)	(2/50)
	[.02]	[.04]	[.1]	[.28]	[.1]	[.04]
1989-2000	0.565**	0.132*	0.115**	0.020	-0.018	0.391*
	(2/50)	(3/50)	(2/50)	(46/50)	(39/50)	(3/50)
	[.04]	[.06]	[.04]	[.92]	[.78]	[.06]
2000-2007		0.088**	0.078**	-0.014	-0.028	0.576**
		(2/50)	(2/50)	(7/50)	(46/50)	(1/50)
		[.04]	[.04]	[.14]	[.92]	[.02]
2007-2010		0.034	0.023*	-0.005	-0.017	0.154
		(9/50)	(4/50)	(17/50)	(49/50)	(6/50)
		[.18]	[.08]	[.34]	[.98]	[.12]
2010-2013		0.016		0.011		0.089*
		(10/50)		(45/50)		(5/50)
		[.2]		[.9]		[.1]
II. Long-Run Changes						
1980-2010	1.356**	0.464**	0.296*	-0.010	-0.031	1.838**
	(1/50)	(1/50)	(3/50)	(27/50)	(34/50)	(2/50)
	[.02]	[.02]	[.06]	[.54]	[.68]	[.04]

Note: *** p<0.01, ** p<0.05, * p<0.1. See notes to Table 3 for description of regressions. The synthetic control is constructed using predictor weights that minimize the mean squared prediction error of the outcome variable in the pre-policy period (as in Abadie, Diamond, and Hainmueller 2010, 2014), allowing the composition of the synthetic control to vary by outcome. See text and appendix for details. In column 1, the long-run change is from 1980-2000 not 1980-2010. In case of tied ranks, I assign the worse rank to Delaware.

Appendix Table A7: Cross-Industry Spillovers - Differences-in-Differences Relative to Pre-Policy Growth Allowing for Differences in the Synthetic Control Across Outcome

	FIRE	Nontrad. excl. FIRE	Services	Trade	Transp./ Util.	Constr.	Manufact.	Govt.
Panel A: Delaware Relative to Synthetic Control								
I. Decadal Changes								
1980-1989	0.791**	0.224**	0.207**	0.179*	0.203*	0.336	0.118	-0.036
	(1/50)	(2/50)	(2/50)	(4/50)	(4/50)	(11/50)	(6/50)	(34/50)
	[.02]	[.04]	[.04]	[.08]	[.08]	[.22]	[.12]	[.68]
New Jobs/New FIRE Job		3.011	1.019	1.030	0.252	0.508	0.860	-0.167
1989-2000	0.565**	0.143**	0.087	0.185**	0.076	0.089	-0.127	0.068
	(2/50)	(2/50)	(9/50)	(2/50)	(18/50)	(17/50)	(44/50)	(12/50)
	[.04]	[.04]	[.18]	[.04]	[.36]	[.34]	[.88]	[.24]
New Jobs/New 1980-89 FIRE Job	1.76	2.85	0.74	1.44	0.12	0.19	-0.95	0.33
II. Long-Run Changes								
1980-2000	1.356**	0.367**	0.294**	0.364**	0.279*	0.425	-0.009	0.032
	(1/50)	(1/50)	(2/50)	(2/50)	(5/50)	(6/50)	(24/50)	(20/50)
	[.02]	[.02]	[.04]	[.04]	[.1]	[.12]	[.48]	[.4]
New Jobs/New 1980-89 FIRE Job		4.93	1.45	2.10	0.35	0.64	-0.07	0.15
Jobs in Delaware, 1980	12,300	130,800	47,900	56,000	12,100	14,700	70,900	45,200
Jobs in Delaware, 1989	30,300	193,900	82,200	75,500	15,300	20,800	73,100	47,100

Note: *** p<0.01, ** p<0.05, * p<0.1. See Table 3 and 4 notes for description of regressions. The synthetic control is constructed using predictor weights that minimize the mean squared prediction error of the outcome variable in the pre-policy period (as in Abadie, Diamond, and Hainmueller 2010, 2014), allowing the composition of the control to vary by outcome. In case of tied ranks I assign the worse rank to Delaware. See text and appendix for details.

Appendix Table A8: 1950 Census Bureau Codes for Relevant Industries and Occupations

Relevant Occupations: Accountants, Relevant Clerks, and Relevant Managers

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.)"

Occupational Codes Included as Relevant Clerks

310 "Bookkeepers"

321 "Collectors, bill and account"

341 "Office machine operators"

390 "Clerical and kindred workers (n.e.c.)"

Occupational Code for Accountants: 0

Relevant Industry: Banking and credit agencies, 1950 industry code 716

Appendix Table A9: Effects on Wage Growth - Differences-in-Differences Relative to Pre-Policy Growth
Delaware Relative to Bordering States

	(1)	(2)	(3)
	All	Banking and Credit	Clerks, Accountants, and Managers
I. Decadal Changes			
1979-1989	0.007	0.015**	0.010*
	(0.006)	(0.005)	(0.005)
1989-1999	0.005	0.010**	0.006**
	(0.003)	(0.003)	(0.002)
1999-2013	0.002	0.011**	0.006*
	(0.003)	(0.003)	(0.003)
DE	-0.003	-0.001	-0.004
	(0.003)	(0.003)	(0.002)
1979-1989*DE	0.004	0.001	0.004
	(0.006)	(0.005)	(0.005)
1989-1999*DE	0.004	0.006	0.006**
	(0.003)	(0.003)	(0.002)
1999-2013*DE	-0.0004	-0.0002	-0.001
	(0.003)	(0.003)	(0.003)
N	16	16	16
II. Long-Run Changes			
1979-2013	0.004	0.012*	0.007*
	(0.003)	(0.003)	(0.003)
DE	-0.003	-0.002	-0.003
	(0.002)	(0.002)	(0.002)
1979-2013*DE	0.002	0.002	0.003
	(0.003)	(0.003)	(0.003)
N	8	8	8

Note: *** p<0.01, ** p<0.05, * p<0.1. See notes to Table 5 for description of regressions. These coefficients are not scaled up by (t-t') as in Table 5.

Appendix Table A10: Synthetic Control Composition, Predictor Weights Determined by Regressions

	All Wages	Banking and Credit Wages	Clerks', Accountants', and Managers' Wages
Alaska	0.025	0	0
Arizona	0.105	0.037	0.073
Connecticut	0.098	0.154	0.157
Florida	0	0	0.009
Indiana	0.509	0.42	0.435
Maryland	0.082	0	0.034
Michigan	0.088	0.129	0.079
Nevada	0	0.005	0
Ohio	0	0.009	0.053
South Carolina	0.003	0	0
Vermont	0.062	0.078	0.091
Virginia	0.029	0.143	0.049
Wyoming	0	0.026	0.02

Note: Each column shows the composition of the synthetic control for the given outcome. All outcomes are log regression-adjusted state average wages. The synthetic control is constructed using predictor weights that minimize the mean squared prediction error of the outcome variable in the pre-policy period (as in Abadie, Diamond, and Hainmueller 2010, 2014), allowing the composition of the control to vary by outcome. See text and appendix for details.

Appendix Table A11: Effects on Wage Growth - Differences-in-Differences in Delaware Relative to Pre-Policy Growth, Allowing for Differences in the Synthetic Control Across Outcome

	(1)	(3)	(5)
	All	Banking and Credit	Clerks, Accountants, Managers
I. Decadal Changes			
(1) 1979-1989	0.11	0.06	0.09
	[9/50]	[15/50]	[8/50]
	(.18)	(.3)	(.16)
(2) 1989-1999	0.03	0.02	0.03
	[12/50]	[24/50]	[11/50]
	(.24)	(.48)	(.22)
(3) 1999-2013	0.06	0.03	0.03
	[11/50]	[16/50]	[16/50]
	(.22)	(.32)	(.32)
II. Long-Run Changes			
(4) 1979-2013	0.19	0.105	0.152
	[7/50]	[16/50]	[8/50]
	(.14)	(.32)	(.16)

Note: *** p<0.01, ** p<0.05, * p<0.1. The synthetic control is constructed using predictor weights that minimize the mean squared prediction error of the outcome variable in the pre-policy period (as in Abadie, Diamond, and Hainmueller 2010, 2014), allowing the composition of the control to vary by outcome. See notes to Table 5 for details of regressions. In case of tied ranks, I assign the worse rank to Delaware.

Appendix Table A12: Pre-Policy Characteristics: Delaware, Bordering States, and the Synthetic Control

	(1)	(2)	(3)
Predictors	**Delaware**	**Bordering States**	**Synthetic Control**
Share Employed in			
FIRE			
1960-1964	0.040	0.046	0.043
1965-1969	0.040	0.045	0.042
1970-1974	0.049	0.048	0.046
1975-1979	0.046	0.050	0.048
1980	0.047	0.052	0.051
Manufacturing			
1960-1964	0.37	0.35	0.35
1965-1969	0.36	0.32	0.34
1970-1974	0.31	0.28	0.30
1975-1979	0.28	0.24	0.27
1980	0.27	0.22	0.25
High-Tech Manufacturing			
1960-1964	0.17	0.050	0.14
1965-1969	0.17	0.047	0.14
1970-1974	0.15	0.088	0.13
1975-1979	0.14	0.097	0.13
1980	0.13	0.095	0.12
Transportation and Utilities			
1960-1964	0.067	0.073	0.065
1965-1969	0.056	0.066	0.058
1970-1974	0.051	0.062	0.056
1975-1979	0.050	0.057	0.051
1980	0.047	0.056	0.050
Trade			
1960-1964	0.19	0.20	0.20
1965-1969	0.2	0.20	0.20
1970-1974	0.21	0.21	0.21
1975-1979	0.22	0.22	0.22
1980	0.22	0.22	0.22
Services			
1960-1964	0.13	0.14	0.13
1965-1969	0.13	0.15	0.14
1970-1974	0.15	0.17	0.16
1975-1979	0.17	0.19	0.18
1980	0.18	0.20	0.19
Construction			
1960-1964	0.072	0.054	0.055
1965-1969	0.068	0.054	0.055
1970-1974	0.071	0.053	0.055
1975-1979	0.061	0.046	0.048
1980	0.057	0.045	0.047

	(1)	(2)	(3)
Predictors	**Delaware**	**Bordering States**	**Synthetic Control**
Share Employed in			
Government			
1960-1964	0.13	0.14	0.15
1965-1969	0.14	0.15	0.16
1970-1974	0.16	0.18	0.18
1975-1979	0.17	0.19	0.18
1980	0.17	0.19	0.18
% Metropolitan			
1960	68.9	81.5	57.0
1970	70.4	83.8	64.5
1980	67.0	87.4	72.7
% with ≥ a HS Diploma			
1960	43.3	39.6	40.9
1970	54.6	51.7	52.1
1980	68.6	66.5	66.3
% age 15-64			
1960	59.9	61.2	59.4
1970	61.8	62.5	61.8
1980	67.8	67.3	66.7
Unemployment Rate			
1970-1974	0.049	0.049	0.051
1975-1979	0.080	0.075	0.068
1980	0.074	0.073	0.075
Population (1981=1)			
1960-1964	0.79	0.86	0.82
1965-1969	0.88	0.93	0.89
1970-1974	0.95	0.98	0.94
1975-1979	0.99	0.99	0.97
1980	1.00	1.00	0.99
Labor Force Participation			
1976-1980	0.46	0.47	0.47
Housing Prices (1981=1)			
1975-1979	1.15	1.06	1.09
1980	1.07	1.05	1.07
Total Employment (1981=1)			
1960-1964	0.61	0.67	0.62
1965-1969	0.76	0.78	0.74
1970-1974	0.88	0.88	0.85
1975-1979	0.93	0.94	0.94
1980	1.00	1.00	1.00

Note: This table compares the balance of predictor variables in the synthetic control, Delaware, and the states bordering Delaware.

Appendix Table A13: Annual Delaware Wage Growth Differential, Relative to Pre-Policy Trend

	Control Group	
	Synthetic Control	Bordering States
1980	-0.012	-0.026**
	[37/50]	(0.011)
1981	0.028	0.028***
	[10/50]	(0.008)
1982	-0.003	0.022
	[30/50]	(0.017)
1983	-0.036	-0.053***
	[44/50]	(0.008)
1984	0.061**	0.063***
	[2/50]	(0.021)
1985	-0.074	-0.070***
	[45/50]	(0.015)
1986	-0.021	-0.011
	[31/50]	(0.017)
1987	0.079*	0.059***
	[3/50]	(0.018)
1988	0.064**	0.068***
	[2/50]	(0.019)
1989	-0.007	-0.033
	[24/50]	(0.023)
1990	-0.04	-0.028
	[46/50]	(0.024)
1991	0.011	-0.001
	[15/50]	(0.005)
1992	0.075*	0.081***
	[4/50]	(0.023)
1993	-0.055	-0.052***
	[47/50]	(0.019)
1994	-0.02	-0.016***
	[36/50]	(0.006)
1995	0.021	0.020***
	[14/50]	(0.004)
1996	0.051*	0.058*
	[3/50]	(0.029)
1997	-0.051	-0.052**
	[46/50]	(0.022)
1998	-0.041	-0.009**
	[47/50]	(0.004)
1999	0.083**	0.043
	[2/50]	(0.030)

	Control Group	
	Synthetic Control	Bordering States
2000	-0.087	-0.050
	[50/50]	(0.043)
2001	0.028*	0.012**
	[5/50]	(0.006)
2002	0.04*	0.020
	[3/50]	(0.013)
2003	-0.02	-0.018***
	[42/50]	(0.006)
2004	0.023	0.033***
	[11/50]	(0.010)
2005	0	0.013
	[25/50]	(0.009)
2006	0.004	-0.010
	[23/50]	(0.023)
2007	-0.033	-0.028
	[45/50]	(0.022)
2008	0.027	0.001
	[14/50]	(0.008)
2009	-0.001	0.014
	[29/50]	(0.012)
2010	0.002	0.005
	[22/50]	(0.018)
2011	-0.034	-0.026**
	[43/50]	(0.010)
2012	-0.008	-0.017
	[29/50]	(0.014)
2013	0.073**	0.046*
	[1/50]	(0.027)

Note: *** p<0.01, ** p<0.05, * p<0.1. In column 1, the dependent variable is ((ln(avg. wage_t, DE)-ln(avg. wage_t', DE))/(t-t'))-((ln(avg. wage_t, synth)-ln(avg. wage_t', synth))/(t-t')), for (t', t): (1969, 1979), (1979, 1980), ..., (2012, 2013). The omitted group is (1969, 1979). These regressions include fixed effects for t, and have as many observations as (t', t) pairs. The effect's rank relative to placebo estimates is in brackets. In column 2, the dependent variable is ((ln(avg. wage_t, s)-ln(avg. wage_t', s))/(t-t')), and the explanatory variables include state fixed effects, fixed effects for t, and the time fixed effects interacted with Delaware. These regressions have as many observations as (t', t) pairs multiplied by four (Delaware plus three bordering states). Column 2 shows coefficients on year interacted with Delaware, with robust standard errors in parentheses. In case of tied ranks I assign the worse rank to Delaware. See text for details.

Appendix Table 14: Synthetic Control Composition, Including Yearly Values of Predictors

Ohio	0.208
Virginia	0.217
Indiana	0.191
Connecticut	0.076
Vermont	0.055
Michigan	0.082
Florida	0.084
Maryland	0.025
South Carolina	0.063

Note: This table shows the composition of the synthetic control for Delaware, when I use yearly values of the predictors. See paper for details.

Appendix Table A15: Labor Market Effects: Differences-in-Differences Relative to Pre-Policy Growth, Yearly Values of Predictors

	(1)	(2)	(3)	(4)	(5)
	Ln(FIRE Empl.)	Ln(Empl.)	Ln(Pop.)	Unempl. Rate	LF/Pop.
Panel A: Delaware Relative to Synthetic Control					
I. Decadal Changes					
1980-1989	0.723**	0.138*	0.049	-0.016	0.023
	(1/50)	(4/50)	(10/50)	(8/50)	(7/50)
	[.02]	[.08]	[.2]	[.16]	[.14]
1989-2000	0.496**	0.088*	0.09**	0.017	-0.023
	(2/50)	(5/50)	(3/50)	(46/50)	(40/50)
	[.04]	[.1]	[.06]	[.92]	[.8]
2000-2007		0.069*	0.064**	-0.014	-0.032
		(5/50)	(2/50)	(7/50)	(48/50)
		[.1]	[.04]	[.14]	[.96]
2007-2010		0.024	0.02*	-0.004	-0.017
		(12/50)	(4/50)	(20/50)	(49/50)
		[.24]	[.08]	[.4]	[.98]
2010-2013		0.009		0.012	
		(16/50)		(45/50)	
		[.32]		[.9]	
II. Long-Run Changes					
1980-2010	1.219*	0.319*	0.223*	-0.017	-0.049
	(1/50)	(3/50)	(3/50)	(11/50)	(41/50)
	[.02]	[.06]	[.06]	[.22]	[.82]

Note: *** p<0.01, ** p<0.05, * p<0.1. See Table 3 notes for details of regression. These results are based on a synthetic control constructed using yearly values of predictors, rather than multi-year averages as in the principal specification. See paper for details.

Appendix Table A16: Cross-Industry Spillovers: Differences-in-Differences Relative to Pre-Policy Growth, Yearly Values of Predictors

	FIRE	Nontrad. excl. FIRE	Services	Trade	Transp. / Util.	Constr.	Manufact.	Govt.
Panel A: Delaware Relative to Synthetic Control								
I. Decadal Changes								
1980-1989	0.723**	0.131	0.135*	0.103	0.13	0.155	0.089	-0.017
	(1/50)	(7/50)	(5/50)	(12/50)	(12/50)	(19/50)	(10/50)	(32/50)
	[.02]	[.14]	[.1]	[.24]	[.24]	[.38]	[.2]	[.64]
1989-2000	0.496**	0.096*	0.032	0.156*	-0.021	0.119	-0.131	0.086
	(2/50)	(5/50)	(21/50)	(3/50)	(34/50)	(15/50)	(42/50)	(8/50)
	[.04]	[.1]	[.42]	[.06]	[.68]	[.3]	[.84]	[.16]
II. Long-Run Changes								
1980-2000	1.219*	0.228*	0.167	0.26*	0.109	0.274	-0.041	0.068
	(1/50)	(4/50)	(11/50)	(3/50)	(19/50)	(12/50)	(27/50)	(17/50)
	[.02]	[.08]	[.22]	[.06]	[.38]	[.24]	[.54]	[.34]
Jobs in Delaware, 1980	12,300	130,800	47,900	56,000	12,100	14,700	70,900	45,200
Jobs in Delaware, 1989	30,300	193,900	82,200	75,500	15,300	20,800	73,100	47,100

Note: *** p<0.01, ** p<0.05, * p<0.1. See Table 3 and 4 notes for details of regression. These results are based on a synthetic control constructed using yearly values of predictors, rather than multi-year averages as in the principal specification. See paper for details.

Appendix Table A17: Effects on Wage Growth - Differences-in-Differences in Delaware Relative to Pre-Policy Growth, Yearly Values of Predictors

		(1)	(2)	(3)
		All	Banking and Credit	Clerks, Accountants, Managers
I. Decadal Changes				
(1)	1979-1989	0.088	0.044	0.081
		[8/50]	[11/50]	[13/50]
		(.16)	(.22)	(.26)
(2)	1989-1999	0.032	0.021	0.033
		[22/50]	[23/50]	[25/50]
		(.44)	(.46)	(.5)
(3)	1999-2013	0.035	0.003	0.020
		[7/50]	[13/50]	[18/50]
		(.14)	(.26)	(.36)
II. Long-Run Changes				
(4)	1979-2013	0.155	0.069	0.134
		[9/50]	[21/50]	[9/50]
		(.18)	(.42)	(.18)
	Control	Synthetic	Synthetic	Synthetic

Note: *** p<0.01, ** p<0.05, * p<0.1. See Table 5 notes for description of regressions. These results are based on a synthetic control constructed using yearly values of predictors, rather than multi-year averages as in the principal specification. See paper for details.

Appendix Table A18: Effects on Wage Growth - Differences-in-Differences in Delaware Relative to Pre-Policy Growth, Using All States and Controlling for Pre-Policy Characteristics

	(1)	(2)	(3)
	All	Banking and Credit	Clerks, Accountants, Managers
I. Decadal Changes			
(1) 1979-1989	0.041	0.162***	0.071**
	[.032]	[.049]	[.034]
(2) 1989-1999	0.055**	0.114***	0.067**
	[.022]	[.038]	[.026]
(3) 1999-2013	-0.024	0.102	-0.027
	[.03]	[.061]	[.038]
II. Long-Run Changes			
(4) 1979-2013	0.071	0.378***	0.111
	[.069]	[.135]	[.086]
Control		All states	

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in brackets. See Table 5 for description of regression. Rather than including only bordering states, I include all states in this regression, along with state fixed effects, and control for pre-policy characteristics (average from 1975-1979 of percent employed in each sector; unemployment rate; and housing prices, employment, and population (each indexed to 1 in 1981); average from 1976-1980 of labor force/population, and in 1980 percent metropolitan, percent 15 to 64, and percent with high school degree or higher. These variables are each interacted with year 1989, 1999, and 2013. I also include fixed effects for year 1989, 1999, and 2013, and these interacted with Delaware. The year 1979 (capturing growth between 1969 and 1979) is the omitted category.

Appendix Table A19: Labor Market Effects - Differences-in-Differences Relative to Pre-Policy Growth
Delaware Relative to Bordering States, without Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(FIRE Employment)	Ln(Employment)	Ln(Population)	Unemployment Rate	Labor Force/ Population	Ln(Housing Price Index)
I. Decadal Changes						
1960-1970	0.004	0.011	0.011			
	(0.008)	(0.010)	(0.007)			
1980-1989	0.010	0.002	0.003	-0.007***	-0.006***	0.032
	(0.008)	(0.008)	(0.005)	(0.0003)	(0.002)	(0.020)
1989-2000	-0.020***	-0.007	0.004	-0.003***	-0.009***	-0.014
	(0.003)	(0.005)	(0.003)	(0.0003)	(0.002)	(0.017)
2000-2007		-0.012*	0.002	-0.003***	-0.009***	0.064**
		(0.005)	(0.003)	(0.0005)	(0.002)	(0.021)
2007-2010		-0.030***	0.002	0.012***	-0.008***	-0.068**
		(0.006)	(0.003)	(0.001)	(0.002)	(0.025)
2010-2013		-0.008		-0.007***		-0.036*
		(0.005)		(0.0004)		(0.017)
DE	-0.001	0.002	0.005*	-0.0002	-0.001	-0.038**
	(0.003)	(0.005)	(0.002)	(0.0003)	(0.002)	(0.016)
1960-1970*DE	0.013	0.005	0.001			
	(0.008)	(0.010)	(0.007)			
1980-1989*DE	0.064***	0.012	0.001	-0.001***	0.006***	0.038*
	(0.008)	(0.008)	(0.005)	(0.0003)	(0.002)	(0.020)
1989-2000*DE	0.040***	0.007	0.004	0.001*	0.0004	0.039**
	(0.003)	(0.005)	(0.003)	(0.0003)	(0.002)	(0.017)
2000-2007*DE		-0.0002	0.005	0.0002	-0.002	0.033
		(0.005)	(0.003)	(0.0005)	(0.002)	(0.021)
2007-2010*DE		-0.007	0.0002	-0.0003	-0.007***	0.043
		(0.006)	(0.003)	(0.001)	(0.002)	(0.025)
2010-2013*DE		0.001		0.00005		0.027
		(0.005)		(0.0004)		(0.017)
II. Long-Run Changes						
1960-1970	0.004	0.011	0.011			
	(0.008)	(0.010)	(0.007)			
1980-2010	-0.006	-0.008	0.003	-0.003**	-0.008***	0.012
	(0.004)	(0.006)	(0.004)	(0.001)	(0.002)	(0.017)
DE	-0.001	0.002	0.005*	-0.0002	-0.001	-0.038*
	(0.003)	(0.005)	(0.002)	(0.0003)	(0.002)	(0.016)
1960-1970*DE	0.013	0.005	0.001			
	(0.008)	(0.010)	(0.007)			
1980-2010*DE	0.051***	0.005	0.003	-0.0001	0.001	0.038*
	(0.004)	(0.006)	(0.004)	(0.001)	(0.002)	(0.017)

Note: *** p<0.01, ** p<0.05, * p<0.1. See notes to Table 3 for description of regressions. In column 1, the long-run change is from 1980-2000 not 1980-2010. These coefficients are not scaled up by (t-t') as in Table 3.

Appendix Table A20: Cross-Industry Spillover Effects - Differences-in-Differences Relative to Pre-Policy Growth Delaware Relative to Bordering States, Without State Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	FIRE	Nontrad. excl. FIRE	Services	Trade	Transp./ Util.	Constr.	Manufact.	Govt.
I. Decadal Changes								
1960-1970	0.004	0.009	0.010	0.010	0.006	0.024**	0.018***	0.022
	(0.008)	(0.009)	(0.011)	(0.010)	(0.009)	(0.009)	(0.002)	(0.016)
1980-1989	0.010	0.012	0.011	0.004	0.009	0.037**	-0.008	-0.030**
	(0.008)	(0.006)	(0.007)	(0.005)	(0.009)	(0.012)	(0.005)	(0.009)
1989-2000	-0.020***	-0.008**	-0.009**	-0.019***	0.010***	-0.003	-0.005	-0.024**
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.008)	(0.007)	(0.008)
DE	-0.001	0.001	0.007*	-0.002	0.003	0.002	0.012***	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.007)	(0.002)	(0.008)
1960-1970*DE	0.013	0.002	-0.009	0.012	-0.012	0.002	0.001	0.015
	(0.008)	(0.009)	(0.011)	(0.010)	(0.009)	(0.009)	(0.002)	(0.016)
1980-1989*DE	0.064***	0.006	0.004	0.008	0.010	-0.002	0.012**	0.009
	(0.008)	(0.006)	(0.007)	(0.005)	(0.009)	(0.012)	(0.005)	(0.009)
1989-2000*DE	0.040***	0.007*	-0.001	0.015***	-0.005	0.014	-0.015*	0.015*
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.008)	(0.007)	(0.008)
II. Long-Run Changes								
1960-1970	0.004	0.009	0.010	0.010	0.006	0.024**	0.018***	0.022
	(0.008)	(0.009)	(0.011)	(0.010)	(0.009)	(0.009)	(0.002)	(0.016)
1980-2000	-0.006	0.001	0.000	-0.009**	0.010*	0.015*	-0.007	-0.027**
	(0.004)	(0.004)	(0.005)	(0.003)	(0.005)	(0.008)	(0.005)	(0.008)
DE	-0.001	0.001	0.007*	-0.002	0.003	0.002	0.012***	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.007)	(0.002)	(0.008)
1960-1970*DE	0.013	0.002	-0.009	0.012	-0.012	0.002	0.001	0.015
	(0.008)	(0.009)	(0.011)	(0.010)	(0.009)	(0.009)	(0.002)	(0.016)
1980-2000*DE	0.051***	0.007	0.001	0.012**	0.002	0.006	-0.003	0.012
	(0.004)	(0.004)	(0.005)	(0.003)	(0.005)	(0.008)	(0.005)	(0.008)

Note: *** p<0.01, ** p<0.05, * p<0.1. See notes to Table 3 for description of regressions. These coefficients are not scaled up by (t-t') as in Table 4.

Appendix Table A21: Effects on Wage Growth - Differences-in-Differences Relative to Pre-Policy Growth
Delaware Relative to Bordering States, Without Fixed Effects

	(1)	(2)	(3)
	All	Banking and Credit	Clerks, Accountants, and Managers
I. Decadal Changes			
1979-1989	0.007	0.015**	0.010*
	(0.005)	(0.005)	(0.005)
1989-1999	0.005**	0.010***	0.006***
	(0.002)	(0.002)	(0.001)
1999-2013	0.002	0.011***	0.006***
	(0.002)	(0.002)	(0.002)
DE	-0.002	-0.001	-0.004**
	(0.002)	(0.002)	(0.001)
1979-1989*DE	0.004	0.001	0.004
	(0.005)	(0.005)	(0.005)
1989-1999*DE	0.004*	0.006***	0.006***
	(0.002)	(0.002)	(0.001)
1999-2013*DE	-0.0004	-0.0002	-0.001
	(0.002)	(0.002)	(0.002)
N	16	16	16
II. Long-Run Changes			
1979-2013	0.004	0.012**	0.007**
	(0.002)	(0.003)	(0.002)
DE	-0.002	-0.001	-0.004**
	(0.002)	(0.002)	(0.001)
1979-2013*DE	0.002	0.002	0.003
	(0.002)	(0.003)	(0.002)
N	8	8	8

Note: *** p<0.01, ** p<0.05, * p<0.1. See notes to Table 5 for description of regressions. These coefficients are not scaled up by (t-t') as in Table 5.