More Credit, Fewer Babies? Bank Credit Expansion, House Price, and Fertility*

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(Preliminary draft; please do not cite or circulate)

February 2023

Abstract

This paper examines the causal effect of bank credit expansion on fertility by exploiting exogenous increases in bank credit supply coming from U.S. interstate branching deregulation between 1994 and 2005. Adopting the traditional dynamic difference-in-difference (DID) method and a newly developed staggered DID method, I find strong and persistent negative effects of state-level banking deregulation on fertility rates at both aggregate and individual levels. Decomposing this negative effect into a housing cost effect among renters, a housing wealth effect among homeowners, and a liquidity effect caused by easier access to credit among both renters and homeowners, I demonstrate that the housing cost effect is the dominant force. These results reveal the critical role of financial market policies and housing affordability in explaining demographic trends.

Keywords: Banking Deregulation, Credit Expansion, House Prices, Homeownership, Fertility

JEL Codes: J13, R21, R31, G21

*We are grateful for helpful comments and suggestions from George Hayward, Yonghong An as well as other seminar participants at the Texas A&M University, University of North Texas, the 2023 Virtual SIPP Conference, and the 2023 CSWEP CeMENT workshop.

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1 Introduction

Fertility rates in the United States gradually declined since the 1990s creating big challenges for economic and social policy(Kearney et al., 2022; Doepke et al., 2022).[1] Though researchers have been searching for explanations behind these declines, no consensus has been reached so far.[2] During the same time period, the U.S. economy experienced a substantial expansion in bank credit supply. While a growing number of studies have examined the impact of credit expansion on housing market outcomes and household financial decisions,[3] relative little is known about its broader impact on household behavior such as fertility decisions. This paper connects the two strands of literature and reveals the role of financial market policies in explaining demographic trends by uncovering a causal connection between local bank credit expansion and fertility decline. Those results provide new insights for policies that aim to increase the fertility rate.

Theoretically, if we assume that children are normal goods,[4] rising credit supply can affect the demand for children through three offsetting channels. First, a direct impact of credit supply can expansion is that it helps households relax financial liquidity constraints and smooth the costs of childbearing and child care which increases the demand for children and fertility(Cumming and Dettling, 2020). This is referred to as a "liquidity channel" (Célerier and Matray, 2019). Second, from a general equilibrium perspective, increased credit supply can push up local house prices (Favara and Imbs, 2015) which affect homeowners and renters differently. For homeowners with a decent-sized house, an increase in house prices can increase childbirth rates through a "housing wealth channel" when housing equity is used to pay for childcare costs (Dettling and Kearney, 2014; Lovenheim and Mumford, 2013; Daysal et al., 2021). For renters, an increase in house prices is associated with rising rents and costs for future house purchasing and will have a negative substitution effect on the demand for children among renters (Simon and Tamura, 2009; Dettling and Kearney, 2014). This is referred to as a "housing cost or substitution channel." In sum, the substitution channel implies a negative effect, and the other two channels predict a positive effect on birth rates. The net effect of credit supply expansion on aggregate birth rates

[1] According to CDC Vital Statistics Births Reports, the US birth rate peaked in 1991 with 71 births per 1,000 women between ages 15 and 44 and gradually declined to 55.8 per 1,000 women in 2020.

[2] These factors include more widespread usage of contraceptive pill (Bailey, 2010; Rau et al., 2021), the rising costs associated with raising children such as housing and childcare costs (Blau and Robins, 1989; Hirazawa and Yakita, 2009; Rindfuss et al., 2010; Dettling and Kearney, 2014; Bick, 2016; Bar et al., 2018), and the increase in women's opportunities of accessing higher education and the labor market(Basu, 2002; Monstad et al., 2008; Cygan-Rehm and Maeder, 2013). Also, see Stone (2018) and Kearney et al. (2022) for descriptive discussions and a summary of factors behind recent falling fertility rates.

[3] See Favara and Imbs (2015); Maggio and Kermani (2017); Landier et al. (2017); Gete and Reher (2018); Justiniano et al. (2019); Saadi (2020); Hoffmann and Stewen (2020); Mian and Sufi (2021) for examples of recent studies.

[4] Becker (1960) introduced children into economic models as durable goods in the utility function of the parents. Because there are few substitutes for children, they generally are assumed to be normal goods, implying that fertility should respond positively to an increase in household income or wealth.

is ambiguous depending on the relative size of the three effects. Motivated by the theoretical ambiguity, the goals of this paper are to identify the causal effects of credit supply expansion on aggregate fertility rates and individual fertility decisions and to discuss and distinguish different mechanisms.

Identifying the causal effects of credit supply expansion is challenging due to the endogeneity issue. First, there may be individual and local-level omitted variables that correlate with both credit supply and childbirth decisions, which could bias our estimates in either direction. If couples with a higher preference for children are more likely to sort into areas with larger credit expansion, this would bias the estimates upward. Another example is unobserved positive shocks to labor demand in local markets. Those shocks may increase childbirth by increasing household income or decrease childbirth by increasing the opportunity cost of time among female workers, creating an upward or downward bias in estimation. Moreover, there is likely a reverse causality issue. That is, childbirth outcomes can be a determinator instead of being determined by changes in credit supply. For example, areas with higher childbirth rates may induce higher demand for credit which may lead to the expansion of the banking sector and easier credit access in the area (Lisack et al., 2017; Gong and Yao, 2022).

To address this identification challenge, I explore the exogenous expansion in bank credit supply induced by bank branching expansion (Rice and Strahan, 2010; Favara and Imbs, 2015; Célerier and Matray, 2019). Adopted in 1994, the Interstate Banking and Branching Efficiency Act (IBBEA) deregulated the banking industry and made interstate bank branching legal. Between 1994 and 2005, deregulations happened in a staggered way across states since states were given the right to impose barriers to out-of-state bank entry. Deregulated states experienced an increase in the supply of credit such as mortgage loans, which translated into rising housing demand and large price effects in areas where the housing supply is inelastic.[5] The cross-state and cross-year variation in banking deregulations provides a natural setting for identifying the connection between credit supply, house price, and fertility.

As the main outcome variable, fertility is measured at both aggregate and individual levels. At the aggregate level, I construct county-year fertility rates by aggregating births and female population counts to the county-year cell. The number of births comes from the Vital statistics data provided by National Center for Health Statistics (NCHS) which contains birth certificate information for virtually every live birth that takes place in the United States. The annual female population counts also come from the NCHS. The county-level fertility rates cover the period 1990-2004 and allow for the investigation of the connection between bank branching expansion and the declining fertility trend at the aggregate level.

[5] A growing number of papers have documented the expansion of credit supply on house prices. See Glaeser et al. (2008); Favara and Imbs (2015); Maggio and Kermani (2017); Gete and Reher (2018); Justiniano et al. (2019); Saadi (2020); Hoffmann and Stewen (2020); Mian and Sufi (2021) for examples.

One limitation of the NCHS data is that it lacks information on the housing tenure status of the parents (Dettling and Kearney, 2014)[6]. So it is difficult to directly test the role of housing tenure and to decompose different mechanisms behind the effect of bank branching expansion and fertility. To overcome this challenge and explore different channels behind the effect of credit supply, I construct a sample of individual-level fertility events using the Survey of Income Program and Participation (SIPP) 1990-2004 panels. The SIPP is a national-level household survey that contains detailed information on the fertility history of sampled females. Most importantly, the SIPP contains information on household housing tenure, childcare costs, rental costs, as well as on financial assets which makes it possible to explore different mechanisms.

I estimate the causal effect of bank branching expansion on fertility adopting a DID design with two-way fixed effects (TWFE) regressions to compare fertility rates in deregulated and regulated states before and after the deregulation. Since states are deregulated in a staggered way, the DID setup has multiple periods and multiple treatment groups with different treatment time, deviating from the canonical two time periods and two groups DID setup. In this setup, several studies have noted that the coefficients from standard TWFE models may not represent a straightforward weighted average of unit-level treatment effects because they use early-treated states as a control for later-treated states which introduces bias when treatment effects are heterogeneous.[7]

To alleviate this concern, I adopt an alternative DID estimation newly developed by Callaway and Sant'Anna (2021).[8] I refer to this method CSDID for the rest of the paper. This method separates the DID analysis into three separate steps: (i) identification of policy-relevant disaggregated causal parameters, the so-called cohort-specific average treatment effects on the treated; (ii) aggregation of these parameters to form summary measures of the causal effects; and (iii) estimation and inference about these different target parameters. This method allows

[6]The birth certificate data has detailed information on other demographic characteristics of the parents such as age, race, and education level.

[7]As explained in Roth et al. (2022), "TWFE regressions make both clean comparisons between treated and not-yet-treated units as well as forbidden comparisons between units who are both already-treated. When treatment effects are heterogeneous, these forbidden comparisons potentially lead to severe drawbacks such as TWFE coefficients having the opposite sign of all individual-level treatment effects due to negative weighting problems. Even if all of the weights are positive, the weights chosen by TWFE regressions may not correspond with the most policy-relevant parameter. See Borusyak and Jaravel (2017); de Chaisemartin and D'Haultfoeuille (2020); Goodman-Bacon (2021); Callaway and Sant'Anna (2021); Sun and Abraham (2021); Athey and Imbens (2022); Baker et al. (2022) for more discussion of this issue."

[8]See Stata package CSDID `https://friosavila.github.io/playingwithstata/main_csdid.html`. Several other recent papers propose similar approaches to deal with this issue. For example, Sun and Abraham (2021) proposed an estimator that uses the last-to-be-treated units as controls, rather than the not-yet-treated; de Chaisemartin and D'Haultfoeuille (2020) proposes an estimator that can be applied when treatment turns on and off. Alternatively, Borusyak and Jaravel (2017) proposed an imputation estimator that imputes counterfactual outcomes for each treated unit based on the never-treated or not-yet-treated groups. Cengiz et al. (2019) adopted a stacked regression approach where each treated unit is matched to not-yet-treated controls and there are separate fixed effects for each set of treated units and its control. See Roth et al. (2022) for additional discussion of the tradeoffs between the strength of these different methods in staggered treatment settings.

for arbitrary treatment effect heterogeneity and dynamic effects, thereby completely avoiding the issues of interpreting the results of TWFE regressions. More importantly, this method provides easy-to-interpret causal parameters that can be directly used for learning about treatment effect heterogeneity, and constructing calendar-year or event-year level dynamic causal parameters.

Both standard TWFE and CSDID estimates reveal a sharp and persistent decline in fertility among females in deregulated states after the bank branching expansion, suggesting that credit supply leads to a reduction in fertility at both aggregate and individual levels. Particularly, county-level female fertility rates in deregulated states are lower by about 0.1 to 4.6 percent after the deregulation. Meanwhile, at the individual level, the probability of having the first child decreased by about 0.1 to 0.7 percentage points after the deregulation (about 0.1 to 1 percent decrease in fertility rate). Those results are robust when adding time-varying controls, excluding large-population states, and excluding not-yet-treated states as a control sample.

Next, the overall negative fertility effects suggest that the "housing cost or substitution channel" is playing a dominant role. To further confirm this conjecture, I conduct four sets of additional empirical analyses. First, I divide the SIPP sample into homeowners and renters and explore the heterogeneity effects in the two samples. These sub-sample results show that the negative fertility effects mainly come from renters instead of homeowners, consistent with the idea of a dominant "housing cost or substitution channel." Second, I directly estimate the effect of bank deregulation on rent costs and house prices using a similar DID design where rental costs are reported in the SIPP and housing price is measured at the county level using the Federal Housing Finance Agency (FHFA) house price index. The estimation results show that banking deregulation increases house prices at the county level but has no significant effect on rents. Those results suggest that the rising costs are mostly driven by future home purchase costs instead of current rents. Third, to confirm bank deregulation works as a demand shock in the local housing market and that it affects fertility through this housing market channel, I divide the sample into areas where the housing supply is inelastic or elastic, where elasticity is approximated by whether the percentage of developable land is less than 70% based on newly developed topological data provided in Lutz and Sand (2019). The idea is house price responses should be most pronounced in counties where the housing supply is inelastic and muted in elastic areas, where the stock of housing increased instead. The results show that the increase in house prices, as well as the decline in fertility, is more evident in areas where the housing supply is inelastic. Those results suggest a strong connection between bank branching expansion, rising house prices, and declining fertility rates.[9] Finally, I find the effects of bank branching

[9]Making use of a restricted version of SIPP which links households to their residential location at the county level, I adopted bank branching expansion as an instrument to estimate the effects of county-level housing prices on fertility, which reveal similar negative effects of housing prices on fertility. Those results are available after the approval of the Census Research Data Center disclosure process.

expansion have insignificant effects on household debts but positive effects on housing equity which suggests that negative fertility effects can be mitigated at the aggregate level through the "wealth channel" instead of the "liquidity channel." [10]

The rest of the paper proceeds as follows. In section 2, I review the literature, provide a conceptual theory behind the fertility decision and describe the nature of the banking deregulation in the United States. In Section 3, I describe the data and present summary statistics of the sample. In Section 4, I present the traditional and staggered DID estimation models. In section 5, I present the main empirical results and provide a variety of robustness checks. In Section 6, I discuss possible mechanisms behind our main effects. Section 7 provides some concluding remarks and discussions.

2 Literature Review

This paper directly speaks to the literature that connects housing market dynamics with fertility decisions in the context of developed countries.[11] Dettling and Kearney (2014) has investigated the effect of house prices on fertility which focuses on the connection between MSA-level housing prices and total births. Results in this paper are largely consistent with those in Dettling and Kearney (2014) and complement it from three perspectives. First, this paper provides county-level and individual-level evidence on the connection between housing prices and fertility outcomes. Second, this paper adopts bank branching expansion as exogenous shocks for variations in housing prices to better identify the causal effect. Third, this paper provides extensive discussions on the potential mechanisms. Related, a strand of literature (Lovenheim and Mumford, 2013; Mizutani et al., 2015; Atalay et al., 2021; Daysal et al., 2021) used household-level data in the U.S., Japan, Australia, and Denmark to study the connection between housing wealth and fertility. They focus on homeowners and thus only identify the housing wealth channel instead of the liquidity or housing costs channel. Using administrative data on mortgages and births in the UK, (Cumming and Dettling, 2020) studies how monetary policy pass-through to mortgage interest rates and increased household fertility decisions among homeowners. This paper looks at both homeowners and renters and tests the relative importance of different channels in determining the overall trend of fertility. Our results confirm the existence of the wealth effect but highlight that this effect is only evident among homeowners in areas with an inelastic

[10] Additionally, I find bank branching expansion has no significant effects on childcare costs at the household level and no significant effects on labor market outcomes at the aggregate level which exclude the mechanisms on fertility through childcare and mother's opportunity costs.

[11] In the context of developing countries, particularly in Asia, studies find the housing booms are negatively associated with fertility rates (Yi and Zhang, 2010; Lin et al., 2016; Liu et al., 2020; Pan and Yang, 2022; Liu et al., 2023).

housing supply.[12]

Many other explanations have been proposed to explain why the birth rate in the U.S. is declining and is at or near historic lows. First, the widespread usage of the contraceptive pill has been found largely reduced fertility in the U.S. as well as in other countries (Bailey, 2010; Rau et al., 2021). Second, besides housing costs, couples of studies find the childcare cost and the availability of childcare facilities outside of the home play an important role when females make fertility as well as labor market decisions (Blau and Robins, 1989; Hirazawa and Yakita, 2009; Rindfuss et al., 2010; Bick, 2016; Bar et al., 2018). Third, gender equality and the increase in women's opportunities of accessing higher education and the labor market is another potential factor (Basu, 2002; Skirbekk et al., 2004; Monstad et al., 2008; McCrary and Royer, 2011; Cygan-Rehm and Maeder, 2013). Overall, establishing a causal link between these factors and changes in birth rates would require identifying exogenous variations in factors that are usually hard to obtain. Thus, some of the previous studies on this topic are mostly descriptive.

This paper also contributes to the large and growing empirical literature that investigates the consequences of the successive waves of banking deregulation in the U.S. This literature can be broadly categorized by different outcomes such as local economic growth (Jayaratne and Strahan, 1996; Huang, 2008), state business cycles (Morgan et al., 2004; Demyanyk et al., 2007; Hoffmann and Shcherbakova-Stewen, 2011), income inequality (Black and Strahan, 2001; Beck et al., 2010; Levine et al., 2014),[13] innovation and entrepreneurship (Black and Strahan, 2002; Kerr and Nanda, 2009; Rice and Strahan, 2010; Hombert et al., 2017), financial integration and inclusion (Landier et al., 2017; Célerier and Matray, 2019), as well as housing market outcomes such as house price, mortgage supply, and homeownership (Favara and Imbs, 2015; Hoffmann and Stewen, 2020; Tewari, 2014; Chu, 2017; Lin et al., 2021).

Results in this paper build on and confirm studies that find banking deregulation increases local house prices through expanding credit supply. Particularly, Favara and Imbs (2015) finds U.S. bank branching expansion between 1994 and 2005 led to exogenous expansion in mortgage

[12] A large body of studies have found that house wealth has significant effects on household decisions including consumption (Campbell and Cocco, 2007; Attanasio et al., 2009; Mian and Sufi, 2009; Bostic et al., 2009; Disney et al., 2010; Aladangady, 2017), educational choices (Lovenheim, 2011; Lovenheim and Reynolds, 2013; Laeven and Popov, 2016), divorce (Klein, 2017), innovation and entrepreneurship (Adelino et al., 2015; Corradin and Popov, 2015; Bernstein et al., 2017; Schmalz et al., 2017), portfolio choices (Cocco, 2005; Chetty et al., 2017), and demand for long-term care insurance (Davidoff, 2010). These findings suggest the possibility that during the recent housing boom, individuals and couples can use some of their increased housing equity to fund child-related expenses.

[13] Several papers find that banking deregulation leads to less discrimination or less inequality in the labor market. For example, Black and Strahan (2001) find that women's share of employment and wage relative to men's both increase following deregulation, suggesting that banking deregulation leads to less discrimination in the labor market. Beck et al. (2010) find that banking deregulation decreases income inequality. Levine et al. (2014) further finds that banking deregulation decreases racial income inequality.

credit and have significant positive effects on house prices.[14] They argue the underlying theory behind this effect is that deregulation allowed commercial banks to expand credit by improving the geographic diversification of their portfolio. Indeed, the balance sheets of banks operating in deregulating states suggest that they experienced significantly higher deposit growth, and lower deposit costs. They also charged significantly lower rates, presumably because some of the cost savings were passed through to borrowers. That is, credit terms improved, more borrowing happened, and the demand for housing increased. They also show that in areas where the housing supply is inelastic, the response of house prices was pronounced, and the price effect was muted in areas where the housing supply is elastic. Another recent paper, Hoffmann and Stewen (2020), focuses on how the interaction between banking deregulation and capital inflows contributed to the rise of the house price and finds house prices were more sensitive to aggregate U.S. capital inflows from 1997 to 2012 in states that opened their banking markets to out-of-state banks in the 1980s.[15]

One thing worth noticing is that most existing studies on banking deregulation consider the first wave of deregulation that happened in the 70s and 80s, while this paper focuses on the second wave of banking deregulation that happened in 1994-2000. The other papers that explored the second wave of bank deregulation include Rice and Strahan (2010); Favara and Imbs (2015) and Célerier and Matray (2019), which documented meaningful different consequences of the two waves of banking deregulation. For example, the first wave tends to have a large impact on income, unemployment, and economic growth, but not on house prices. In contrast, the second wave of deregulation tends to have a large effect on house prices but not on the economic outcomes mentioned above. It would be interesting to conduct a more comprehensive comparison of the two waves of banking deregulation in the future.

This paper is the first that explores the connection between bank deregulation and demographic outcomes which largely broadens the scope of current literature. Moreover, instead of exploring the banking deregulation index, I study four banking deregulation dummies separately and provide new evidence and confirm the house price effects of banking deregulation. The adoption of the newly-developed DID method also improved the estimation results on

[14] A growing number of recent studies have confirmed the positive relationship between credit supply and house price (Favara and Imbs, 2015; Maggio and Kermani, 2017; Gete and Reher, 2018; Justiniano et al., 2019; Saadi, 2020; Hoffmann and Stewen, 2020; Mian and Sufi, 2021).

[15] Several recent papers studied other housing market consequences of bank deregulation besides housing prices. For example, using CPS, Tewari (2014) studies how banking deregulation affects homeownership and finds an increase in homeownership following the removal of geographic restrictions on banks in the 1980s and early 1990s. Furthermore, this paper shows that this positive effect is stronger for marginal borrowers, such as lower-middle-income, black, and young households, and confirms that the increase in mortgage access is the underlining mechanism. Similarly, using PSID, Lin et al. (2021) find renters who experienced both inter-state and intra-state banking deregulations in the 1980s are more likely to become a homeowner. Chu (2017) examines how banking deregulation affects credit supply in the real estate market, with a focus on distinguishing between the bank competition and balance sheet channels. Using a regression discontinuity design, the paper finds the impact of interstate banking deregulation on credit supply is mostly driven by the bank competition channel.

housing prices in previous studies.

3 Background

3.1 Theoretical Background: Fertility Decision

Following Becker (1960), our conceptual model is built on a large literature in neoclassical economics investigating the nature and determinants of fertility in developed countries. In the most simple static approach to this question, parents are viewed as consumers who choose the number of children that maximizes their lifetime utility subject to the price of children and the budget constraint that they face. This approach recognizes that children bring people utility, perhaps in the form of life satisfaction, general happiness, or pleasurable experiences. Thus, children are conventionally thought to be normal goods, implying that fertility should respond positively to an increase in household income or wealth. Children also come with associated costs, broadly defined, including both time and money.[16] Thus, the demand for children is negatively affected by an increase in the cost of raising a child.

Based on this model, we present three channels of how banking deregulation can affect household fertility decisions. First, the state banking deregulation makes it easier for households to get access to credit and borrow, thus, relaxing their liquidity constraints and increasing the consumption of normal goods such as children. That is, the increase in credit supply can have a positive effect on fertility through this " liquidity channel." Second, from a general equilibrium perspective, increased credit supply can push up local house prices (Favara and Imbs, 2015) which affect homeowners and renters differently. For homeowners with a decent-sized house, an increase in house prices can increase childbirth rates through a "housing wealth channel" when housing equity is used to pay for childcare costs (Dettling and Kearney, 2014; Lovenheim and Mumford, 2013; Daysal et al., 2021). For renters, an increase in house prices is associated with rising rents and costs for future house purchasing which makes families substitute away from children (Simon and Tamura, 2009; Dettling and Kearney, 2014). The so-called "housing cost or substitution effect." One goal of this paper is to discuss and test the relative importance of the different mechanisms.

[16] Becker (1960) also introduced the concept of the child's "quality," a term he uses to refer to expenditures per child.

3.2 Institutional Background: Banking Deregulation

Most U.S. states historically restricted interstate banking and branching,[17] dating back to colonial times. Since the 1970s, the banking sector has gone through decades of deregulatory changes regarding banks' geographic expansion. However, interstate branching was still not allowed until 1994 (Kroszner and Strahan, 1999; Johnson, 2008; Kroszner and Strahan, 2014; Cetorelli and Strahan, 2006). In 1994, the Interstate Banking and Branching Efficiency Act (IBBEA) was adopted which effectively permitted bank holding companies to enter other states and operate branches. Meanwhile, the IBBEA also granted individual states some latitude in deciding the rules governing entry by out-of-state branches. Particularly, states can relax restrictions from four dimensions: (1) require a minimum age of the targeted bank to be less than three years, (2) allow de novo branching without an explicit agreement by state authorities, (3) allow the acquisition of individual branches without acquiring the entire bank, and (4) allow a state-wide deposit cap, that is, the total amount of state-wide deposits controlled by a single bank or bank holding company to be larger than 30%. Thus, states had the opportunity to change the above four provisions independently, which generated large geographic variations of the banking deregulation (Rice and Strahan, 2010; Favara and Imbs, 2015). By 1996, nine states[18] had already moved to full deregulation. But most changes took place between 1996 and 2002. By 2005, eight mid-western states still were not deregulated and we did observe additional deregulation afterward.

The staggered deregulation of the banking industry across states provides a natural setting for identifying the effect of banking deregulation on fertility decisions at aggregate and individual levels. Based on the four types of deregulation rules, I constructed four deregulation dummies that equal one when the state relaxed one of the restrictions. Those dummies change across states and years to evaluate the different impacts of the four types of regulations. Rice and Strahan (2010) compute a time-varying regulation index that ranges from 0 to 4 to capture the

[17] Interstate banking refers to the control by bank-holding companies of banks across state lines, whereas interstate branching means that a single bank may operate branches in more than one state without requiring separate capital and corporate structures for each state. Since U.S. states were collecting revenues from local banks through taxes and fees, they indeed had incentives to restrict competition from out-of-state banks. Until the late 1950s, several laws are adopted to make and keep interstate banking and branching prohibited. For example, in 1927, the adoption of the McFadden Act implicitly prohibited interstate branching by commercial banks. Then, in 1956, the Bank Holding Company Act ended the development of bank holding companies that were circumventing the existing law and acquiring branches across states. The Bank Holding Company Act prevented banks from acquiring banks or branches outside their state unless the state of the targeted bank permitted such acquisitions.

[18] These nine states include Alaska, Massachusetts, New York, Oregan, Rhode Island, Nevada, North Carolina, and Utah.

state-level branching restrictions.[19] In our preferred specification, I decompose this index into four components to evaluate the relative importance of the four types of deregulations.

Figure 1 shows the timing of the four types of banking deregulations. The reduction of the statewide deposit cap on bank branch acquisitions is concentrated in 1996, 1997, and 1998, while the bulk of the other three deregulations happened between 1996 and 2000. Figure 2 maps the timing of those deregulations, with darker shades representing later deregulations. Again, we see the spatial patterns of the first three deregulations are similar and somewhat concentrated in eastern states. Regarding the reduction of the statewide deposit cap on bank branch acquisitions, we see this deregulation is more common in both eastern and western states and relatively uncommon among states in the middle. This figure suggests that the timing of banking deregulation changes over time and geographically which helps us identify the causal effect based on a DID design.

As shown in the later section, I find the effects of bank deregulation are mainly caused by the reduction of the statewide deposit cap on bank branch acquisitions instead of the other three deregulations. We focus our empirical analysis on this deregulation. Table 1 reports the year when states relaxed this regulation.

4 Data and Summary Statistics

4.1 Data: Vital Statistics Natality Files

Data on births come from the Vital Statistics Natality Files (1990-2004).[20] Vital statistics data contain birth certificate information for virtually every live birth that takes place in the United States. Vital statistics data identify the race and ethnicity, marital status, age, and education of the mother and the father.

To measure the aggregate-level fertility rate, I construct county-year fertility rates defined as the total number of births to women in the county-year cell divided by the county-year population, where the annual county-year female population counts are obtained from the National Center for Health Statistics. I also construct the fertility rate by demographic groups such as race/ethnicity (Non-Hispanic White, Non-Hispanic Black, and Hispanic) and age groups (20-29 and 30-44) at the county level. County-level fertility rates are used to establish the connections between banking deregulation and fertility trends. This data, however, lacks information on

[19] Previous studies have adopted this index to evaluate the impact of banking deregulation on the price of housing Favara and Imbs (2015) and financial inclusion Célerier and Matray (2019). Our results on the effects of banking deregulation on house prices are robust using this index and are consistent with findings in Favara and Imbs (2015).

[20] Since 2005, national vital statistics micro-data files that include state, county, or larger city geography are no longer available without approval. These data are available at `http://www.cdc.gov/nchs/nvss.htm`.

housing tenure status, we complement it with the SIPP to further explore the fertility events at the individual level and to explore different channels.

4.2 Data: SIPP

I use SIPP 1990-2004 panels to construct a sample of individual fertility events. Each of these panels contains a nationally representative sample of between 20,000 and 46,000 households over 8 to 12 waves during 2 to 3 years. Each SIPP wave includes a "core" survey that collects detailed information on household sociodemographics and income and one or several topical modules that collect information on various topics. Particularly, the SIPP Fertility History topical module provides information for each female when the first child is born. I construct a fertility event dummy for each female with available information in the sample. This dummy takes the value 1 in years after the first child is born, and 0 otherwise. I focus on the sample period between 1990 and 2005 to ensure enough pre and post-deregulation periods. For example, if state-level bank deregulation happened in 1994, then there are four pre-periods and 11 years of post-periods for each individual in the sample. Similarly, I construct a marriage event variable based on the information of the year when the female first got married and a homeownership event variable for homeowners based on the information of the year when the house is purchased.[21]

Since housing tenure status plays an important role in determining the effects of banking deregulation, I construct a dummy variable *own1990* that takes the value 1 if the individual owned a house in 1990 (the beginning of the sample period) and 0 otherwise. I define an individual who owned a house in 1990 if the current house is purchased before 1990. The SIPP also provides other key demographic and household characteristics such as age, race and ethnicity, educational attainment, marital status, and whether one lives in metro areas.

I focus on a sample of women aged 16 to 50 in 1990 because the birth rates among women over 50 or below 16 are extremely low. Observations with missing information on fertility or homeownership variables are dropped. The final sample contains 57,202 females with 30,207 females having given birth and 26,995 having no children during 1990-2005.[22]

[21] The year of first marriage provides in the Marriage History topical module and the year when the house is purchase is provided in the Asset and Liability topical module.

[22] This sample provides fertility information spanning from 1990 to 2005 which enables dynamic DID methods. But one drawback is that it lacks other individual and household-level time-varying variables. One may worry about time-variant variables such as female workers' labor supply and household income can largely affect fertility decisions. To deal with this issue and complement our main analysis, I created a SIPP sample that only covers female workers between 1996 and 2005. For this sample, we can observe individual and household information between 2 and 4 years. Estimation results using this sample are similar.

4.3 Data: Housing Market and Other Variables

To measure house prices, one of the major channels through which banking deregulation affected fertility trends, I use Federal Housing Finance Agency (FHFA) housing prices indexes. The FHFA indexes series are available from 1975 through 2015, which is constructed from all repeat-sale, single-family homes whose mortgages have been securitized by Fannie Mae or Freddie Mac each year. It measures average changes in house prices at state, MSA and county levels. [23] I use the county-level housing price index to better capture housing price variation at a more refined geographic unit.

To measure the geographic determinants of housing supply elasticity, I use the newly developed measures of the percentage of undevelopable land in Lutz and Sand (2019). This measure expands on the popular topological land unavailability proxy from Saiz (2010) in three dimensions. First, it uses higher resolution satellite imagery from the United States Geological Survey; Second, it provides more precise geographic boundaries; Finally, this measure is available at multiple levels of disaggregation. Land unavailability is used to proxy the housing supply elasticities and test whether house prices increase more in less-elastic areas during the deregulation periods. Particularly, a state is defined with less land meaning the percentage of developable land is less than 70%.[24]

To control for the impact of local labor market shocks, I use the Quarterly Census of Employment and Wages (QCEW) which provides information on employment and wages reported by employers covering more than 95 percent of U.S. jobs. These variables are available at the county, MSA, state, and national levels by industry.

4.4 Summary Statistics

I start by examining national fertility and housing time-series data in order to describe the aggregate correlation between home price variation and fertility. I construct an annual birthrate measure, which is the number of births per thousand women ages 15 to 44 from the CDC National Vital Statistics Reports, from 1980 to 2020. The housing price measure is the state-level Federal Housing Finance Agency Housing Price Index (HPI). The correlation between housing price changes and birth rates over time is shown in Figure 3. This figure plots the births per 1,000 women ages 15 to 44 in the country as the solid line; the dashed line is the percentage change in real housing prices. Although on different scales, these data are consistent with a negative cross-sectional relationship between housing price variation and fertility. In

[23] These data are available at `http://www.fhfa.gov.` This index, previously known as the Office of Federal Housing Enterprise Oversight (OFHEO) index, is widely used in the housing literature.

[24] Other measures of geographic variations in housing supply conditions include Wharton Residential Land Use Regulatory Index (WRLURI) from Gyourko et al. (2008) and the proportion of undevelopable land from Saiz (2010), I plan to use those measures for robustness checks to mitigate the measurement issue.

times of high housing price growth, fertility appears to fall, and vice versa. Thus, at least in the aggregate, this figure is suggestive that housing prices negatively affect fertility.

Table 2 reports summary statistics of the natality sample (column 1) and the SIPP sample of females with children (column 2). The main characteristics of the two samples are quite comparable. The average age of first childbirth is 27 in the natality sample and 23 in the SIPP sample. This four-year gap might be explained by the fact that the natality sample includes all births while the SIPP sample includes just the first birth. In the natality sample, 79 percent of females are non-Hispanic White and 15 percent are black. 21 percent of females in the sample have no high school degrees, 33 percent of them have high school degrees, and 46 percent have a college or above education. Moreover, 68 percent of them are married when the child is born. In the SIPP sample, 82 percent of females are non-Hispanic White and 14 percent are black. 10 percent of females have no high school degrees, 39 percent of them have high school degrees, and 52 percent have college or above education. Moreover, 74 percent of them are married when the child is born. One of the benefits of using SIPP is that it has information on housing tenure status and shows that 11 percent of females are homeowners in the SIPP sample in 1990. Table 2 also reports summary statistics of the SIPP sample of females without children (column 3) and the total sample of females (column 4).

5 Empirical Strategy

5.1 TWFE: County-level Fertility Rate

To estimate the aggregate effect of banking deregulation on county-level fertility, I use a DID framework with the following two-way fixed effects estimation equation:

$$F_{ct} = \alpha_1 D_{st} + \alpha_2 X_{ct} + \delta_c + \delta_t + \epsilon_{c,t} \tag{1}$$

where F_{ct} is fertility rate in county c of state s in year t. The dummy variable D_{st} indicates whether the banking deregulation took place in state s at year t. X_{ct} include time-varying control variables such as county-level changes in employment and wages as calculated in the QCEW. δ_c and δ_t are county and year fixed effects, respectively.[25] These fixed effects remove every unobserved time-varying heterogeneity at the local market level, such as differences in business cycles and trends and aggregate shocks that could stem from changes in federal regulations of the banking sector. The parameter of interest is α_1. Typically, researchers interpret α_1 as the weighted sum of the average treatment effect on treated (ATT). Since banking deregulation was implemented by states, the error term $\epsilon_{c,t}$ are clustered at the state level.

[25]The results are similar using state instead of county fixed effects.

To estimate the dynamic treatment effect of banking deregulation on county-level fertility, I estimate equation 1 adding leads and lags of treatment with the following estimation equation

$$F_{ct} = \sum_{e} \alpha_{1e} D_{st+e} + \alpha_2 X_{ct} + \delta_c + \delta_t + \epsilon_{c,t} \tag{2}$$

where e stands for the time period relative to a treatment year. For example, $e = 2$ indicates two years after the treatment and $e = -2$ indicates two years before the treatment. Thus, D_{st+e} is the dummy indicating whether year t is e years apart from state s is initially treated, that is, when banking deregulation took place. Notice that $D_{st} = \sum_e D_{st+e}$, so we can think of the estimated coefficients α_{1e} as the causal effect of the treatment on the outcome of interest e periods from the shock. Thus, α_{1e} is dynamic decomposition of ATT as estimated by α_1.

5.2 CSDID: County-level Fertility Rate

Since states deregulated in a staggered way, we have multiple treatment groups with different treatment times. Many studies have noted that the coefficients from standard TWFE models may not represent a straightforward weighted average of unit-level treatment effects in this setting because they use early-treated states as a control for later-treated states which introduce bias when treatment effects are heterogeneous. To alleviate this concern, I adopt CSDID estimation newly developed by Callaway and Sant'Anna (2021).

This method separates the DID analysis into three separate steps. First, it identifies group-specific average treatment effects on the treated, denoted $ATT(g,t)$, reflecting average treatment effects on the treated in time period t for the group treated at time g. For example, states that adopted deregulation in 1996 are defined as group 1996; states that adopted deregulation in 1997 are defined as group 1997. Following Callaway and Sant'Anna (2021), I formalize the idea of group-specific average treatment effects using standard potential outcome notation:

$$ATT(g,t) = E[Y_t(1) - Y_t(0)|G_g = 1] \tag{3}$$

where G_g is a dummy variable equal to one if the unit is in treatment time group g, $Y_t(1)$ is the outcome variable at time t for treated units, and $Y_t(0)$ is the potential outcome for those units that had they have not been treated. As in standard difference-in-difference settings, $Y_t(0)$ is not observed for periods after g. That is, we do not observe the counterfactual non-treated outcome for treated units. This the fundamental problem of causal inference motivates the use of a control group of never-treated units (C) as a proxy for what would have occurred if a unit had not been treated. Callaway and Sant'Anna (2021) show that under the assumption that the control and treatment groups follow counterfactual parallel trends, we can express the

treatment effect in equation (3) as:

$$ATT(g,t) = E[Y_t - Y_{g-1}|G_g = 1] - E[Y_t - Y_{g-1}|C = 1] \tag{4}$$

where the first term is the evolution of the outcome for the treatment group and the second term is the equivalent evolution of the outcome for the control group. Both quantities are simple averages and are easily calculated from the data. Notice that equation (4) makes no comparisons across groups treated at different times, avoiding the issues that occur when researchers use early-treated units as controls for later-treated units.

In step 1, $ATT(g,t)$ are calculated from equation (4) for every treatment group g and time period t. In step 2, $ATT(g,t)$ are aggregated to calculate the dynamic treatment effect for each time period e relative to a treatment date since the causal effects of bank deregulation are expected to differ as a function of years relative to the treatment period g. For example, for states treated in 1996; $e = 2$ corresponds to 1998, and for states treated in 1997, $e = 2$ corresponds to 1999. I then take an average of those average treatment effects across groups g, weighting by the group size. This procedure results in a single average treatment effect on treated (ATTs) for every relative period e, including periods before the treatment occurs ($e < 0$). I plot these averages, which are analogous to the relative time coefficients generated from the standard TWFE regression in Equation 2. To create a single, overall point estimate, I take the average of these aggregated relative time estimates when $t > g$. Finally, in step 3, I use a bootstrapping procedure and report simultaneous confidence bands that are robust to multiple hypothesis testing and cluster errors by county.

Th CSDID estimator allows for arbitrary treatment effect heterogeneity and dynamic effects, thereby completely avoiding the issues of interpreting the results of TWFE regressions. More importantly, this method provides easy-to-interpret causal parameters that can be directly used for learning about treatment effect heterogeneity, and constructing calendar-year or event-year level dynamic causal parameters. It is also worth noticing that this method also relies on standard difference-in-difference parallel trend assumption for identification: treatment and control units would have followed parallel outcome trends after g if not for the existence of the treatment. In our setting, this implies that outcomes in states with bank deregulation would have followed parallel paths to outcomes in states where bank deregulation did not take place. This assumption is untestable but can be checked by checking whether outcome trends in the years leading up to the treatment year g is parallel across treatment and control units.

5.3 TWFE and CSDID: Individual Fertility Decision

Using the SIPP, I apply the same TWFE and CSDID methods using individual fertility variable f_{it} as the outcome variable, where f_{it} is a dummy variable measuring whether the first child is

born for an individual i in state s at year t. Particularly, the aggregate effect is estimated by β_1 as the following equation,

$$f_{it} = \beta_1 D_{st} + \beta_2 X_{it} + \delta_i + \delta_t + \epsilon_{i,t}. \tag{5}$$

The dynamic effect is estimated by β_{1e} as in the following equation

$$f_{it} = \sum_e \beta_{1e} D_{st+e} + \beta_2 X_{it} + \delta_i + \delta_t + \epsilon_{i,t}. \tag{6}$$

In both models, I include individual fixed effects instead of county-fixed effects. Since banking deregulation implemented by states, the error term $\epsilon_{i,t}$ is also clustered at the state level.

6 Empirical Results

6.1 Bank Deregulation and County-level Fertility Rate

In Figure 4, I plot the effects of four types of banking deregulation on county-level fertility rate. Unlike in a standard event study framework, there is no omitted category here. Instead, each coefficient measures the average treatment effect on the treated (ATT) e years after the deregulation averaging over the event-time coefficients for groups deregulated in different years. e is the running variable on the x-axis. Red dots are TWFE estimates and black dots are CSDID estimates. The two methods print out quite similar patterns. First, across most of the deregulations, there are observe no pre-trend in fertility in the years leading up to the banking deregulation, validating the difference-in-difference design. One exception is the TWFE estimate in panel d) which shows some positive pre-trend. Second, in the year after bank deregulation, there is a clear sharp, and persistent decrease in fertility in deregulated states that persists 5 years after the deregulation in panel d). The estimated size of these negative effects increases from -1.2 to -5.2 percent according to TWFE estimates an increase from -1.3 to -4.6 according to CSDID estimates. The estimated coefficients are also presented in Table 4. Meanwhile, the plots for the other three types of bank deregulation show the changes in fertility are quite moderate compared with the relaxation of the deposit cap. Considering that the negative effect is only evident for panel d) with CSDID showing no pre-trend, I use this deregulation and empirical design as a baseline for the rest of the paper.

To test the robustness of our main results in Figure 4, I conduct the four robustness analyses in Figure 5. First, a potential concern is that our results are biased by time-varying changes in the local market that correlate with the timing of bank deregulation and also affect household fertility decisions. I find no evidence of pre-trends in our event studies, implying that fertility is not changing differently in the years leading up to bank deregulation. However, if local economic

conditions change at the same time as bank deregulation, the analysis could be biased. I address this concern by adding state-level employment and wage variables as additional controls in Panel a) of Figure 5. It shows that the main results are similar to the main results with a clear sharp and persistent decrease in fertility in deregulated states. The estimated size of this negative effect increased from -0.006 to -0.049. Thus, the biased caused by local labor market conditions are not severe. I also provide direct tests of the effects of bank deregulation on those variables in Appendix Table A6 showing that banking deregulation is not driving a pattern of local economic conditions.

Next, the CSDID so far has used not-yet-treated observations as well as never-treated observations as controls to have a larger observation in the control group. To avoid additional bias caused by not-yet-treated observations, in Panel b) of Figure 5, I exclude not-yet-treated observations from the control sample. Again, a similarly sharp and persistent decrease in fertility is observed in deregulated states. The estimated size of this negative effect increases from -0.005 to -0.049. The overall estimated coefficients are statistically indistinguishable from our preferred specification.

Additionally, to make sure the results are not driven by large population states such as California and Texas, I exclude those two states from our sample in Panel c) of Figure 5. I find the decrease in fertility rates is not concentrated in California and Texas, as the sharp and persistent decrease in fertility in deregulated states remains. The estimated size of this negative effect increases from -0.004 to -0.045. Similar to the main results, across all three sets of results, there are no pre-trends in the years leading up to the banking deregulation.

Finally, to make sure that our results are driven by the relaxation of the deposit cap and not the other three types of banking deregulation, I select the sample of states where no other three types of regulations are relaxed. These results are plotted in Panel d) of Figure 5. The negative effect on fertility is more evident when we use this smaller sample of states, suggesting that the relaxation of the deposit cap plays a determinant role in the negative effects on fertility.

Figure A2 decomposes the CSDID aggregated dynamic effect in panel d) of Figure 4 by different treatment groups defined by different years when the states deregulated. As described in Table 1, the years of deregulation are concentrated in 1996, 1997, and 1998.[26] Thus, I have three different treatment groups defined by different years when the states deregulated. We use those three groups as treated samples separately, while using never-treated as the control sample. Figure A2 shows the treatment effect plot for the three groups. The average treatment effects are driven by 1996 treatment groups, with the group-level average treatment coefficient being significant and as large as -0.046, while the group-level average treatment effects for the

[26]In 1995 and 2000, deregulation happened in one state. Decompose analysis indicate those states have little impact on the main results, and thus, are dropped in the sample. The main results are similar adding those states to the main sample.

years 1997 and 1998 were smaller and not significant.

6.2 Bank Deregulation and Individual Fertility Decision in SIPP

In Figure 6, I show the ATTs for individual fertility event variables using the SIPP sample. As in Figure 4, there is no omitted category. Instead, each coefficient measures the average treatment effect on the treated (ATT) e years after the deregulation averaging over the event-time coefficients for groups deregulated in different years. e is the running variable on the x-axis.

Largely consistent with panel d) of Figure 4, panel d) of 6 shows no pre-trend in individual fertility in the years leading up to the bank deregulation. In the year after bank deregulation, there is a clear sharp, and persistent decrease in fertility in deregulated states which persist 6 years after the deregulation. The estimated size of this decrease increased from -0.003 to -0.007. The estimated coefficients are also presented in Table 5. Meanwhile, for the other three types of banking deregulation (panels a to c), the change in fertility after the other types of banking deregulation is quite moderate compared with the relaxation of the deposit cap.

To test the robustness of the main results in panel d) of Figure 6, I conduct similar robustness checks as in Figure 5 by adding time-varying control variables, by excluding not-yet treated states as control samples, by excluding California and Texas, and by excluding states that have experienced other three types of deregulation. Those results are presented in Figure 7 which are statistically indistinguishable from the main results. Across all three sets of results, there are no pre-trends in the years leading up to the banking deregulation, and a sharp and consistent decline in fertility following the deregulation.

Figure A3 decomposes the CSDID aggregated dynamic effect in panel d) of Figure 6 by different treatment groups defined by different years when the states deregulated. Similar to A2, we see the negative effect of banking deregulation on fertility is concentrated among the 1996 treatment group, while the group-level average treatment effect for the years 1997 and 1998 is smaller and not significant.

7 Discussion of Mechanisms

As mentioned in section 3.1, I hypothesize that banking deregulation can affect fertility through three channels. First, banking deregulation can directly affect individual fertility decisions through the "liquidity effect," easier access to credit, and therefore relaxed borrowing constraints. Moreover, as credit expanded, the demand for housing increased, pushing local house prices up which lead to the "housing cost or substitution effect" where house prices affect the cost or the expected costs of childbearing or the "housing wealth effects" where house prices

affect the ability to consume in the current period. In this section, I provide three sets of empirical results to help decompose these three different mechanisms.

First, I divide the sample into homeowners and renters and explore the heterogeneity fertility effect between the two samples. Second, I directly estimate the effect of bank deregulation on housing costs by connecting it with individual rents and county-level house prices. Third, to verify bank deregulation works as a demand shock in the local housing market, I explore the house price effects in areas where the housing supply is inelastic or elastic. The idea is price responses should be more pronounced in counties where the housing supply is inelastic and muted in elastic areas, where the stock of housing increased instead. These results help reveal the relative importance of the potential channels behind the connection between financial market changes on demographic trends, that is bank deregulation and fertility. I also provide empirical tests to exclude alternative mechanisms.

7.1 Homeowners vs. Renters

I divide the sample into homeowners and renters in Figure 8. The homeownership status is measured in 1990 based on the home purchase information. The black circle dot line shows our main results that bank deregulation is associated with a lower individual fertility rate. Meanwhile, Figure 8 also shows that the negative effects on fertility are concentrated among renters (orange dot line). On the contrary, the fertility effect among homeowners is negligible (blue dot line). Those results suggest negative fertility effects are largely driven by the "substitution effect" among renters. That is, bank deregulation decreases the willingness of having children by increasing housing costs among renters who are paying rent currently and expecting to purchase houses in the future. Homeowners, instead, may have experienced increases in housing wealth which mitigated the negative effect to some extent. This housing wealth effect is confirmed in the later section. Overall, the substitution effect dominates and leads to a net negative effect. Those results are consistent with aggregate empirical evidence provided in Dettling and Kearney (2014) which finds MSA-level house prices reduced the fertility rate among renters but not among homeowners.

7.2 Housing costs: Rent and County-Level House Prices

In this section, I directly explore the effect of banking deregulation on housing prices measured as the county-level house prices from FHFA.[27] The equation to estimate the effects of bank deregulation on house prices is similar to the one on fertility.

In Figure 10, I show the ATTs for county-level FHFA house prices using both TWFE (red dot line) and CSDID (black dot line). I observe no pre-trend in house prices in the years leading up to

[27]Unless housing price, official historical measures on rents across geographic locations are limited.

the banking deregulation. In the year after bank deregulation, I see a clear sharp, and persistent increase in house prices in deregulated states which persist 8 years after the deregulation. The estimated size of this increase ranged from 0.004 to 0.038 according to TWFE and 0.004 to 0.030 according to CSDID. The estimated coefficients are also presented in Table 6. As illustrated in Figure A4, this house price effect is largest among the 1996 treatment group and gets smaller in the 1997 and 1998 treatment groups, which is consistent with the results on fertility where the most evident effect is found among the 1996 treatment group. Those results suggest that the rising costs of future home purchases instead of rent are potentially the major driving force behind the negative fertility effects.

7.3 The Role of Housing Supply Elasticity

To measure the effect of elastic housing supply elasticity, I divide the sample into states with less and more elastic housing supplies. Particularly, I define states with a percentage of developable land greater than 0.7 as more elastic and states with a percentage smaller than 0.7 as less elastic.[28] As expected, Figure 11 panels a) and b) show that house price effects are much stronger or sharper in states with less developable land than in other states. Particularly, the estimated sizes of this increase range from 0.005 to 0.061 for states with less land. For states with more land, these price effects are not significant. Importantly, across all three sets of results, there are no pre-trends in the years leading up to banking deregulation. To summarize, Figure 11 panels a) and b) show that bank deregulation has sharp, positive, and persistent effects on house prices, particularly in counties with less developable land.

Those results are consistent with literature that find that bank deregulation imposes a positive demand shock on the local housing market and leads to a higher equilibrium house price (Favara and Imbs, 2015; Chu, 2017; Tewari, 2014; Hoffmann and Stewen, 2020). For example, (Favara and Imbs, 2015) also finds U.S. branching deregulation between 1994 and 2005 has significant positive effects on house prices and these effects are stronger among areas with inelastic housing supply. The method in this paper is slightly different from that in (Favara and Imbs, 2015). First, I focus on one particular bank deregulation, that is whether the state has imposed a higher deposit cap that a single bank or bank holding company can control instead of the regulation index constructed based on four types of deregulation. My choice of variable here is supported by results that the main house price effect is driven by one particular deregulation, while house price effects of the other three types of deregulation are moderate and not significant (See Appendix Figure A5). Second, results in (Favara and Imbs, 2015) are derived from a traditional DID method which does not consider the potential bias caused by using early-treated states as controls for later-treated units as treatments. Thus, our results

[28] Other cutoff values lead to similar results and are available upon request.

complement the existing results by refining the connection between bank deregulation and local house price.

Additionally, I divide the sample into homeowners and renters in panels c) and d) of Figure 11. The homeownership status is measured in 1990 based on the home purchase information. In panel c), for states with less developable land, I find a stronger "substitution effect" as well as stronger "housing wealth effects." This is consistent with a stronger house price effect that we observed in panel a). That is, bank deregulation imposes a positive demand shock in house markets in those areas. With limited land for development, the housing price supply in those areas is inelastic, leading to a larger house price increase in those areas. This large house price effect transforms into a larger increase in the cost of childbearing and reduces renters' fertility (orange dot line). Meanwhile, this same large house price effect transforms into a larger housing wealth effect among homeowners which cancels out the increase in cost and eventually increase fertility among them (blue dot line).

In panel d), for states with more developable land, I find smaller "substitution effect" as well as smaller "housing wealth effects" compared with those in panel c). This is consistent with a weaker house price effect that we observed in panel b). That is, bank deregulation imposes a positive demand shock in house markets in those areas. With more land for development, the housing price supply in those areas is more elastic, leading to a smaller house price increase in those areas. This smaller house price effect transforms into a smaller increase in the cost of childbearing and reduces renters' fertility (orange dot line). Meanwhile, this same house price effect transforms into a relatively smaller housing wealth effect among homeowners which mitigates some of the increase in cost but cannot cancel it out completely (blue dot line). Across all sets of results, there are no pre-trends in the years leading up to bank deregulations. Overall, results in Figure 11 suggest that the house price effect and the induced "substitution effect" among renters, and the "housing wealth effects" among homeowners are potentially the main explanation behind the impact of bank deregulation on fertility.

7.4 Rent, Housing Wealth, and Liquidity

Rising housing prices push up housing costs faced by renters which can be decomposed into current rents measured as self-reported rent as in the SIPP and future home-purchasing costs. Panel (a) of Figure 12 shows that bank branching expansion has no significant effects on rents, suggesting most of the cost effects come from the financial pressure of future home purchasing.

Additionally, the overall negative fertility effects and the above tests suggest that the "housing cost or substitution channel" is playing a dominant role. Nevertheless, these negative effects can be mitigated to some extent if bank credit expands households' borrowing constraints or increases homeowners housing wealth. This section explores those possibilities by estimating

the effects of banking deregulation on household amounts of debts and housing equality among homeowners using the SIPP. Figure 12 shows the effects on household debts are insignificant but the effects on housing equity are positive and significant which suggests that negative fertility effects can be mitigated at the aggregate level through the "wealth channel" instead of the "liquidity channel."

7.5 Alternative Mechanisms

Bank credit expansion can affect fertility decisions through other channels. First, banking deregulation can directly affect homeownership decisions. Considering that parents usually would like to purchase a house before the birth of their child. A delay in homeownership can also delay fertility. To test this mechanism, panel a) of Figure 13 shows the ATTs for homeownership events based on the SIPP. The estimated coefficients range from -0.005 to -0.027. Those results suggest that decreasing homeownership is another channel through which house prices can decrease fertility by making homeownership less affordable. The negative effect of bank deregulation may be counter-intuitive considering that bank deregulation expanded mortgage credits which should make it easier for renters to become homeowners. One possible explanation is that bank deregulation increases mortgage credit among existing homeowners but not potential homeowners.[29]

Another channel through which banking deregulation can potentially affect fertility is marriage. If the rise in housing costs can delay marriage, then fertility rates are likely to be delayed as well. Panel b) of Figure 13, however, shows that the probability of getting married is increased due to banking deregulations. Those results suggest that marriage is not playing a major role in explaining the reduced fertility here.

Moreover, if banking deregulation affects local labor market conditions, then it can also affect fertility by affecting the mother's opportunity costs. In Appendix Figure A6, I use state-level log employment and log wage as outcome variables, and we find those local labor market outcomes are not responding to banking deregulation suggesting that bank deregulation is not affecting fertility through the labor market channel. Those results are consistent with Célerier and Matray (2019) which also finds a small labor market effect of the 1990s banking deregulation.

7.6 Heterogeneity Effects on Fertility

In this section, I test whether bank deregulation has heterogeneous effects on individual characteristics, such as age, education, race, and whether the female lives in a metro area using the SIPP data. First, I investigate how effects differ by age by dividing females by whether they are older than 30 in 1990, the beginning of the sample period. Figure A8 panel a) shows that

[29] Those hypotheses and findings are discussed in detail in another ongoing project.

the main findings are driven by the negative effects among females who are younger than 30 in 1990 (blue dot line). The negative effects on the fertility of females who are older than 30 are small and insignificant. In Panel b) of Figure A8, I explore how fertility effects differ by race which shows results from the baseline fertility event study were estimated separately for black and non-black females. It shows the sharp and persistent decrease in fertility in deregulated states for both groups of females, though the effect is slightly larger among white females. Next, I estimate heterogeneous effects by education status dividing females as college graduates and non-college graduates in panel c). I observe a sharp and persistent decrease in fertility in deregulated states for both groups of females, though the effect is slightly larger among non-college graduates. Last, I explore heterogeneous effects among females who live in metro or non-metro areas. Figure A8 panel d) shows that the negative fertility effects are slightly stronger among females who live in non-metro areas. Those results are consistent with studies that find that bank deregulation has a larger impact on rural branches which might impose a larger effect among females living in non-metro areas (Célerier and Matray, 2019).

8 Conclusion

This paper studies how credit expansion affects aggregate and fertility rates and individual fertility decisions. To address the endogeneity issue of credit supply, I use the U.S. interstate branching deregulation that happened between 1994 and 2005. Both TWFE and CSDID results point out that an increase in credit supply leads to a sharp and persistent decline in fertility. Those negative results are particularly evident among renters. Further discussions suggest no significant effects on rental and childcare costs. Finally, these negative effects are mitigated at the aggregate level as homeowners have accumulated more housing wealth due to credit expansion, and their fertility rates are increased.

This paper reveals the meaningful connection between housing market dynamics and demographic trends which are rarely discussed in the literature. Recently, many developed countries have experienced historically unprecedented low fertility rates. Low and declining fertility implies dramatic population aging and rapid population decline, which, in turn, are linked to numerous economic and social policy concerns. Results in this paper, thus, provide important policy implications and suggest a critical role of housing affordability in addressing the issue of declining fertility.

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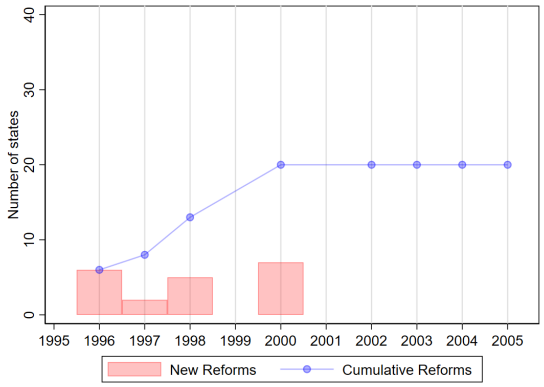
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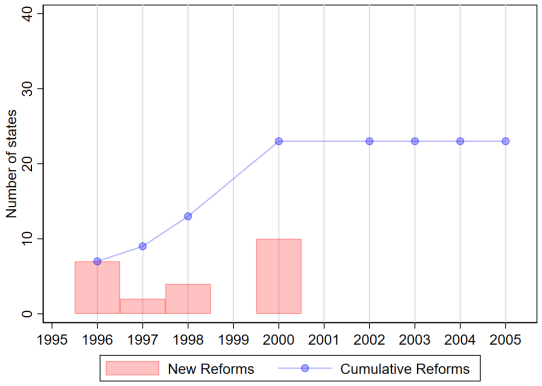
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Figures and Tables

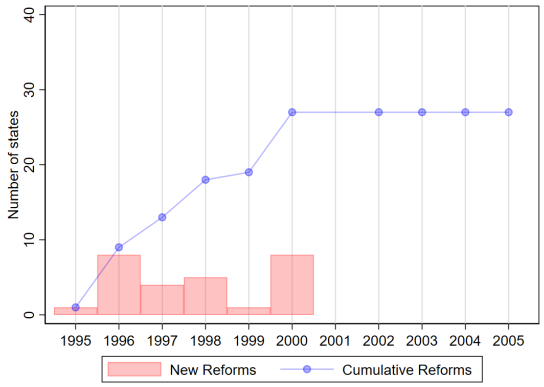
Figure 1: Timing of Interstate Branching Deregulation



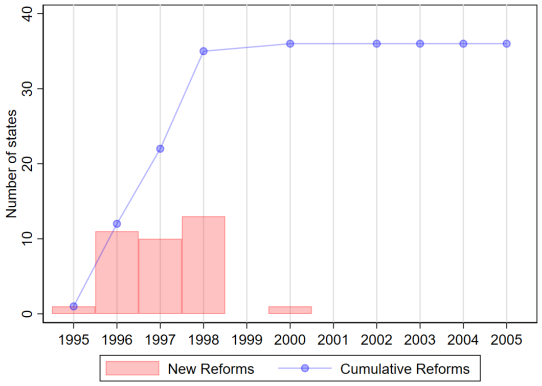
(a) Minimum Age of Targeted Banks to be 3 Years or Fewer



(b) Allow de novo Interstate Branching



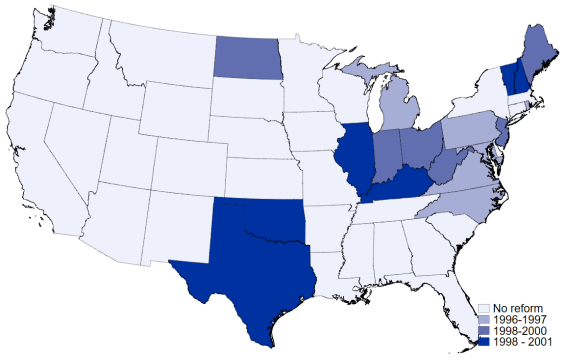
(c) Allow Acquisition of Individual Branch without Acquiring the Entire Bank



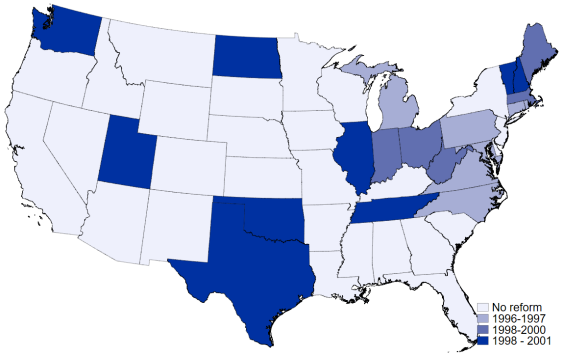
(d) Statewide Deposit Cap to be 30% or Higher

Notes: Data on interstate branching deregulation by state and by year come from Rice and Strahan (2010).

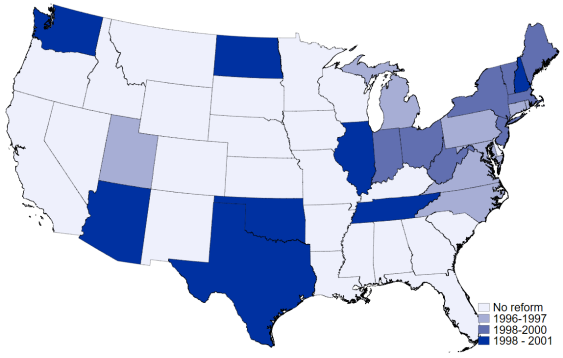
Figure 2: Timing of Interstate Branching Deregulation by State



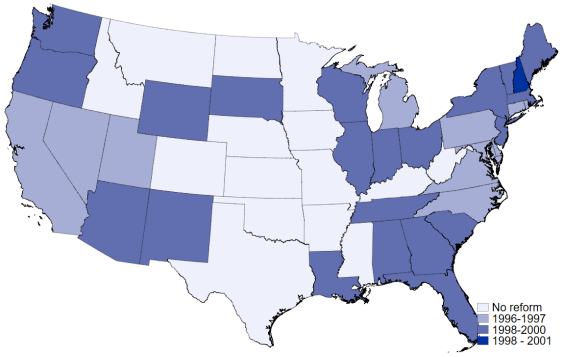
(a) Minimum Age of Targeted Banks to be 3 Years or Fewer



(b) Allow de novo Interstate Branching



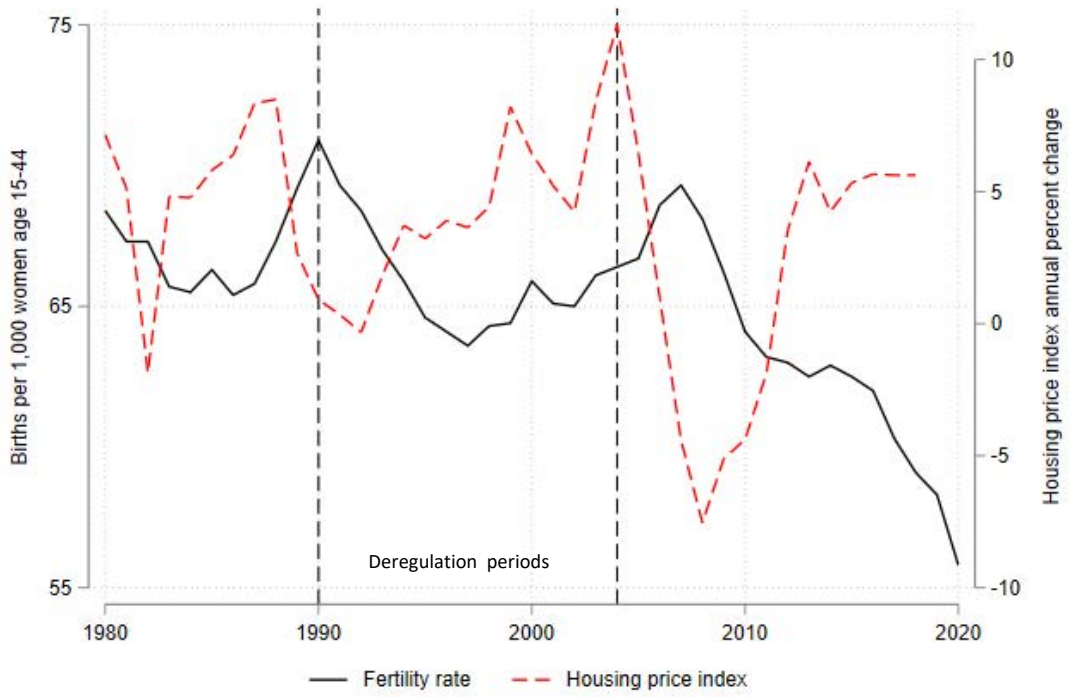
(c) Allow Acquisition of Individual Branch without Acquiring the Entire Bank



(d) Statewide Deposit Cap to be 30% or Higher

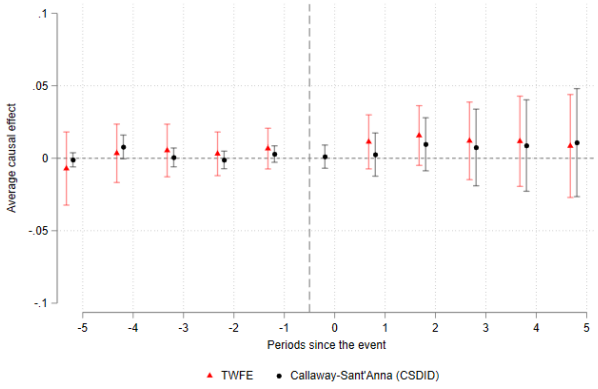
Notes: Data on interstate branching deregulation by state and by year come from Rice and Strahan (2010).

Figure 3: National Birth Rate and Real Housing Price Percentage Change, 1980-2020

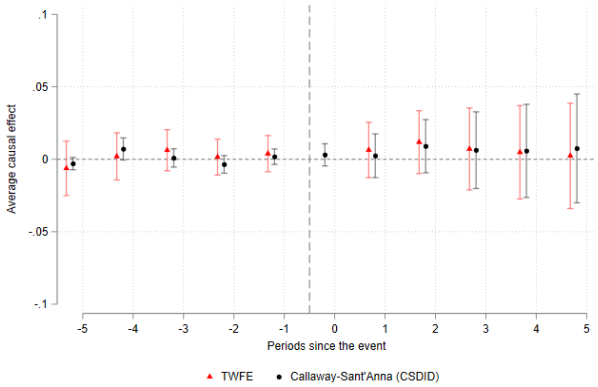


Notes: Birth rates are calculated as the number of births per thousand women ages 15-44 (reported by the CDC National Vital Statistics Reports from 1980 to 2020) divided by the yearly population of women ages 15-44 (reported by National Center for Health Statistics). State-level home prices come from the annual state-level Federal Housing Finance Agency Housing Price Index (HPI). The dashed vertical line indicates the starting and ending years of interstate branching deregulation.

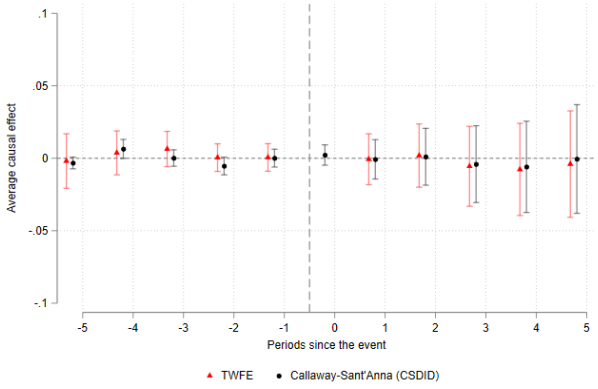
Figure 4: Banking Deregulation and County-level Fertility Rate: TWFE and CSDID



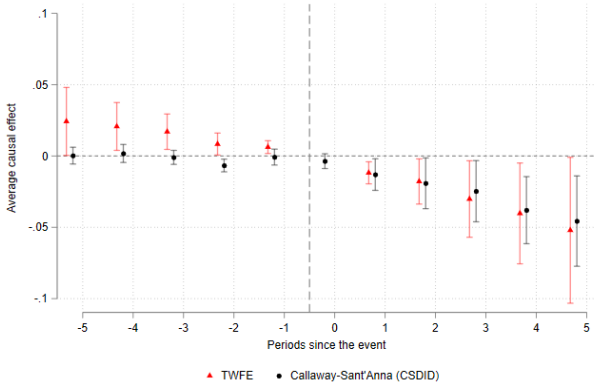
(a) Minimum Age of Targeted Banks to be 3 Years or Fewer



(b) Allow de novo Interstate Branching



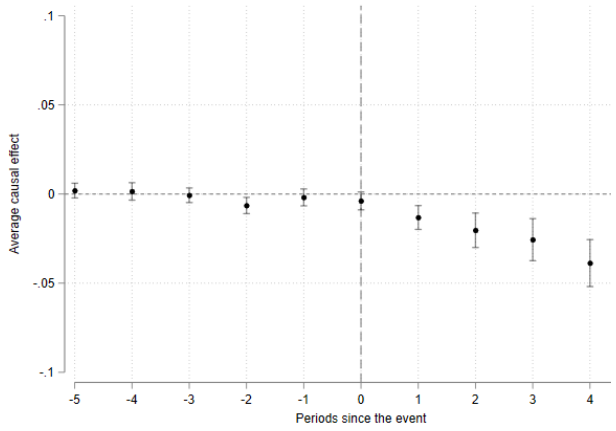
(c) Allow Acquisition of Individual Branches without Acquiring the Entire Bank



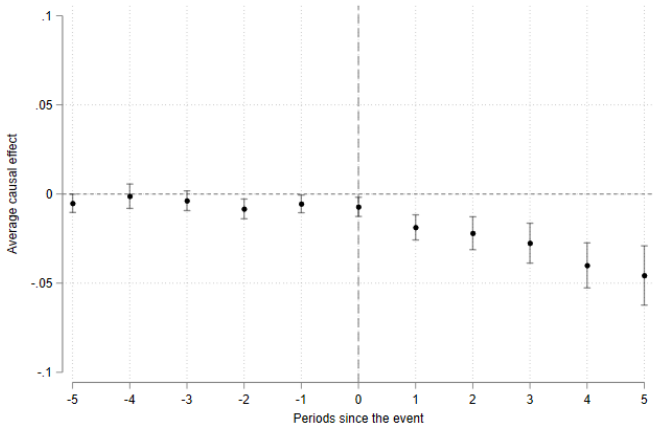
(d) Statewide Deposit Cap to be 30% or Higher

Notes: Outcome variable is the log county-level fertility rate calculated based on the CDC National Vital Statistics Reports (318 counties between 1990 and 2004). County code became confidential information beginning in 2005. The banking deregulation variable in panel (a) is a dummy variable indicating hether the minimum age of the targeted bank for acquisition is less than 3 years; in panel (b) is a dummy variable indicating whether de novo interstate branching acquisition is allowed; in panel (c) is a dummy variable indicating whether the acquisition of individual branches without acquiring the entire bank is allowed; in panel (d) is a dummy variable indicating whether the statewide deposit cap is higher than 30%. Red dots are TWFE estimates; Black dots are CSDID estimates. All Figures show 95 percent confidence intervals. Standard errors are clustered at the state level.

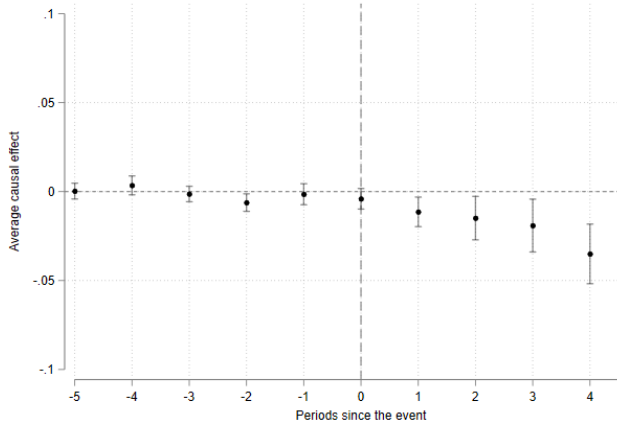
Figure 5: Bank Deregulation and County-level Fertility Rate
CSDID Robustness Checks



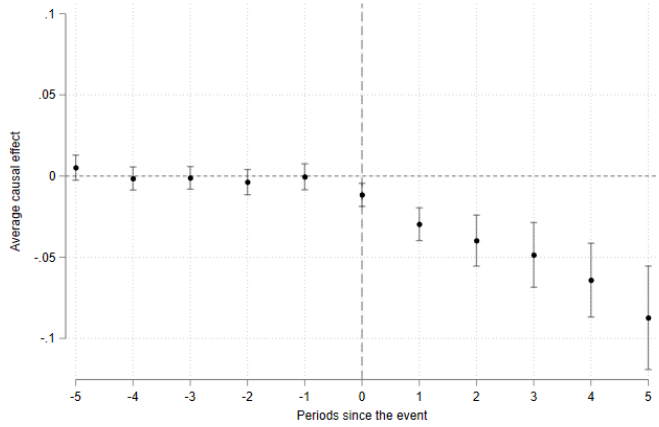
(a) Add time-varying control variables



(b) No not-yet treated as control samples



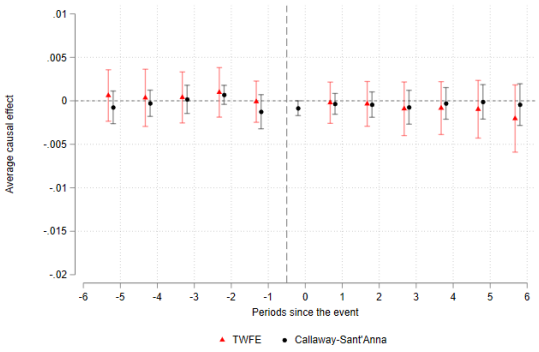
(c) No California or Texas



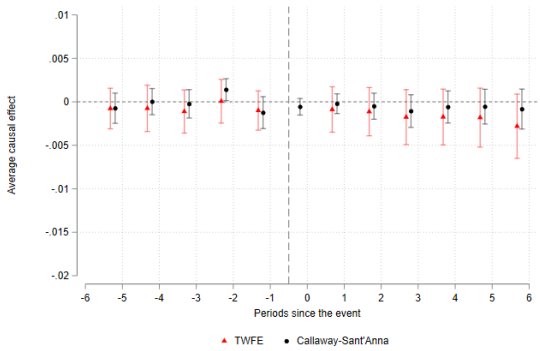
(d) Exclude other types deregulation

Notes: Outcome variable is the log county-level fertility rate calculated based on the CDC National Vital Statistics Reports. The banking deregulation variable is a dummy variable indicating whether the statewide deposit cap is higher than 30%. All Figures show 95 percent confidence intervals. Standard errors are clustered at the state level.

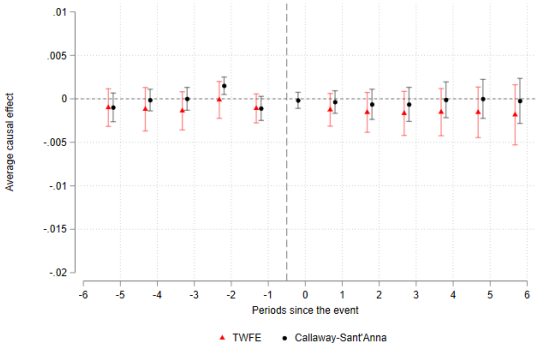
Figure 6: Banking Deregulation and Individual Fertility in the SIPP: TWFE and CSDID



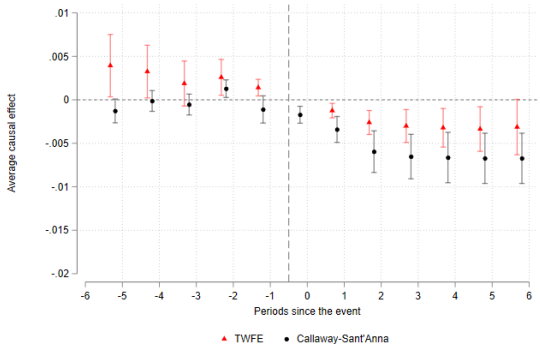
(a) Minimum Age of Targeted Banks to be 3 Years or Fewer



(b) Allow de novo Interstate Branching



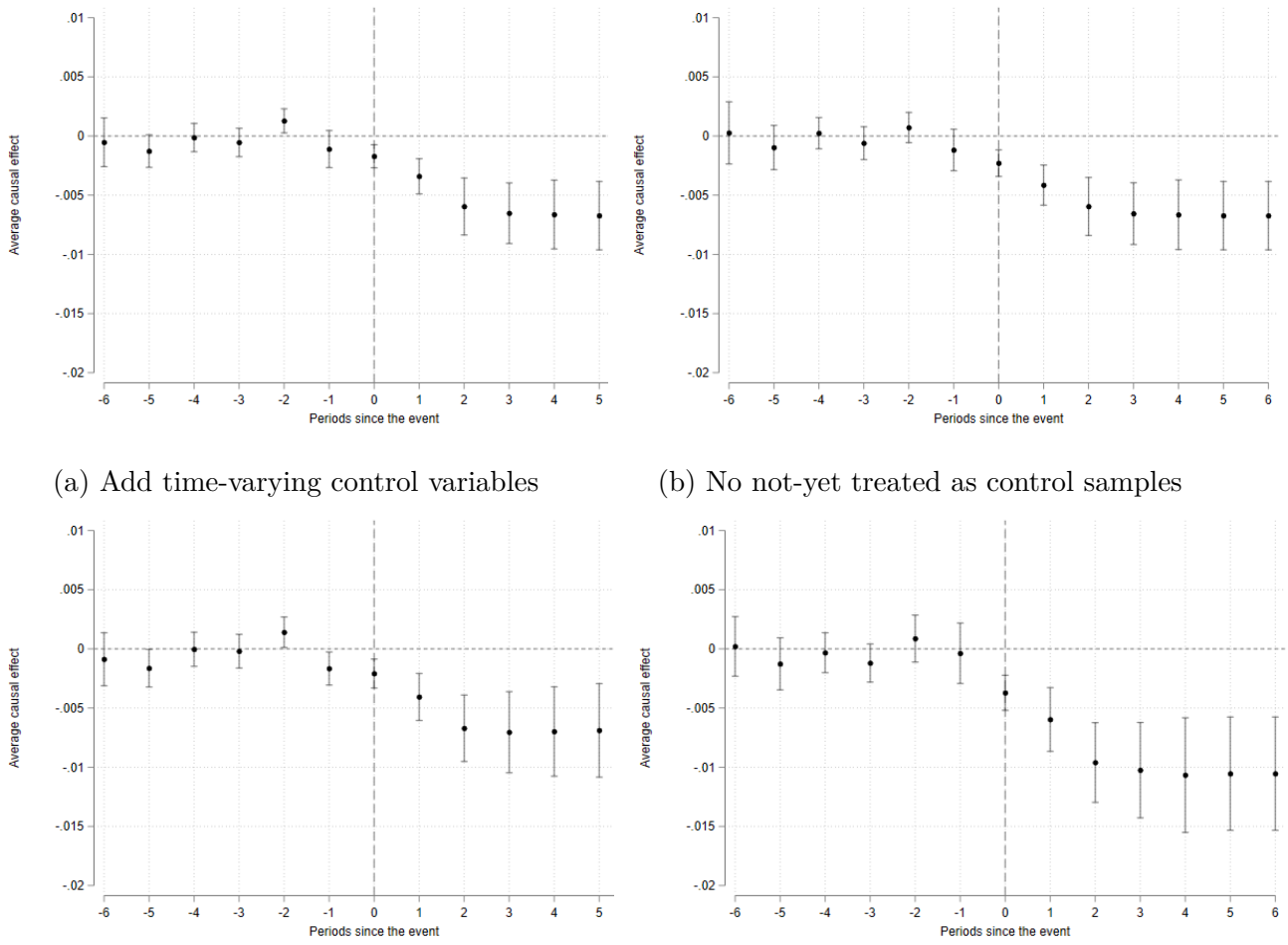
(c) Allow Acquisition of Individual Branches without Acquiring the Entire Bank



(d) Statewide Deposit Cap to be 30% or Higher

Notes: Outcome variable is the fertility dummy indicating whether the first child is born based on the SIPP 1990-2004 panels (62,026 individuals between 1990 and 2005). The banking deregulation variable in panel (a) is a dummy variable indicating whether the minimum age of the targeted bank for acquisition is less than 3 years; in panel (b) is a dummy variable indicating whether de novo interstate branching acquisition is allowed; in panel (c) is a dummy variable indicating whether the acquisition of individual branches without acquiring the entire bank is allowed; in panel (d) is a dummy variable indicating whether the statewide deposit cap is higher than 30%. Red dots are TWFE estimates; Black dots are CSDID estimates. All Figures show 95 percent confidence intervals. Standard errors are clustered at the state level.

Figure 7: Banking Deregulation and Individual Fertility in the SIPP:
CSDID Robustness Checks



(a) Add time-varying control variables

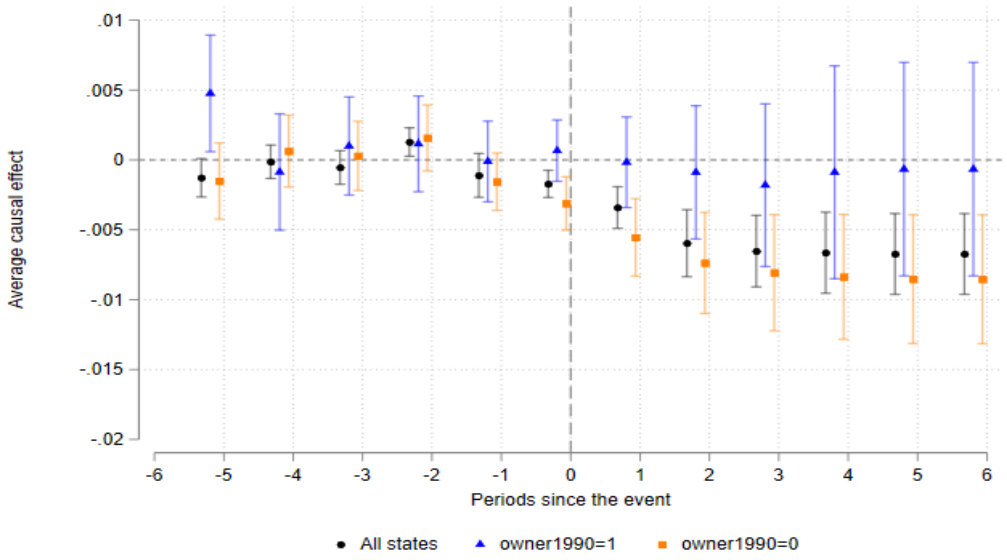
(b) No not-yet treated as control samples

(c) No California or Texas

(d) Exclude other types deregulation

Notes: Outcome variable is the fertility dummy indicating whether the first child is born based on the SIPP 1990-2004 panels (62,026 individuals between 1990 and 2005). The banking deregulation variable is a dummy variable indicating whether the statewide deposit cap is higher than 30%. Black dots are CSDID estimates. All Figures show 95 percent confidence intervals. Standard errors are clustered at the state level.

Figure 8: Mechanisms I: Banking Deregulation and Fertility by Housing Tenure



Notes: Outcome variable is the fertility dummy indicating whether the first child is born based on the SIPP 1990-2004 panels (62,026 individuals between 1990 and 2005). The banking deregulation variable is a dummy variable indicating whether the statewide deposit cap is higher than 30%. Black dots are CSDID estimates. "own1990" is a dummy variable that indicates that females own a house in 1990. All Figures show 95 percent confidence intervals. Standard errors are clustered at the state level.

Figure 9: Mechanisms II: Banking Deregulation and House Prices

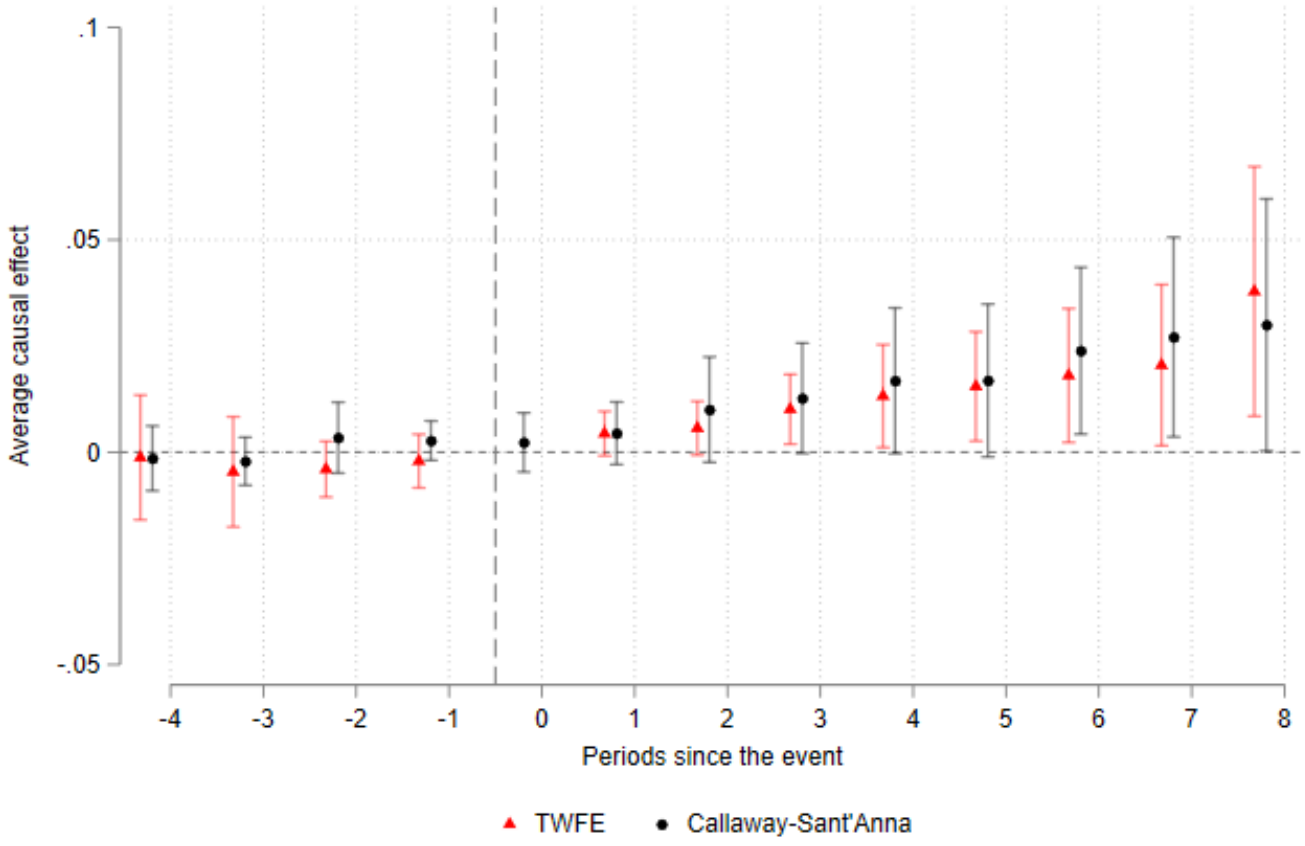
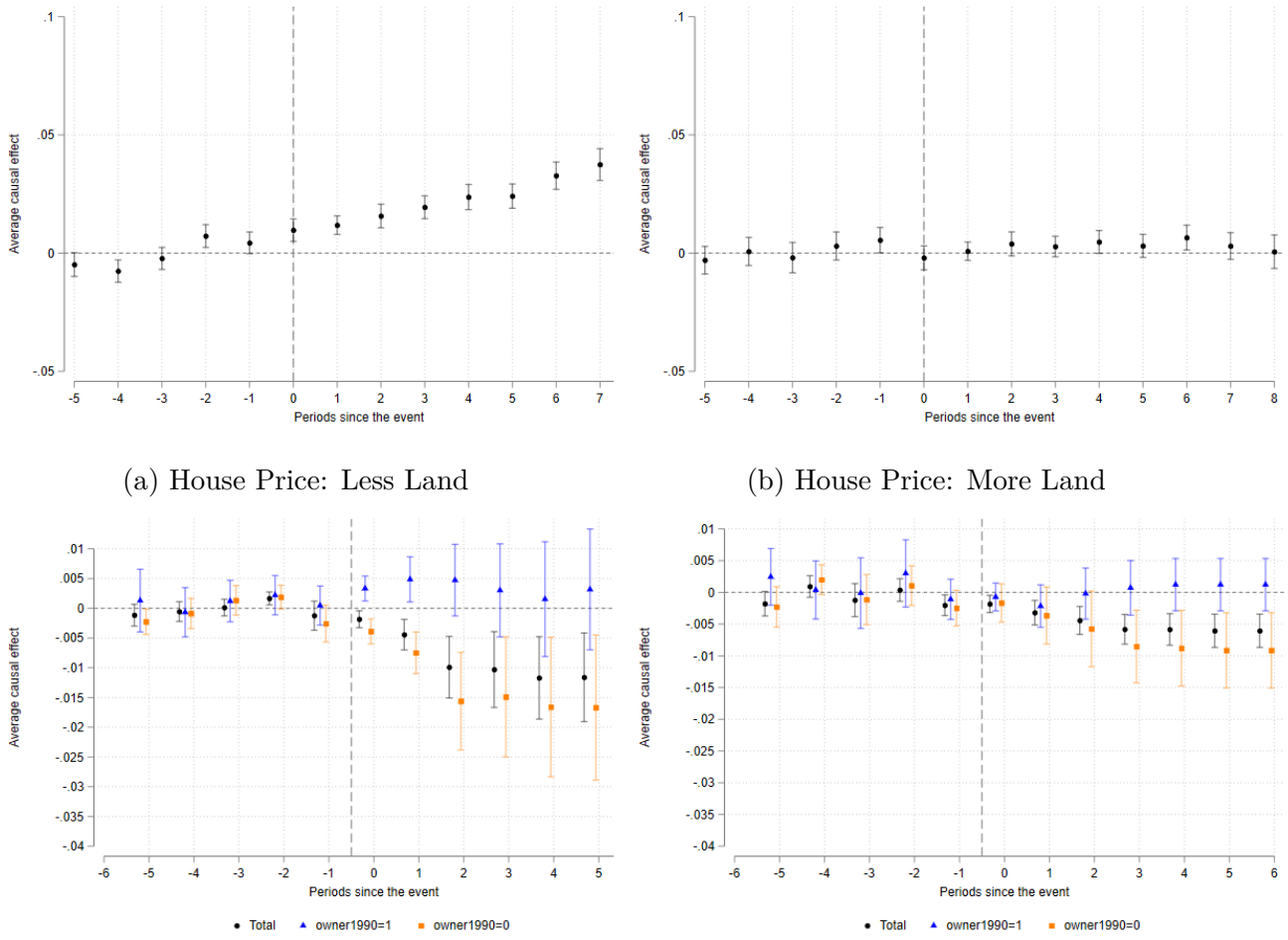


Figure 10: Housing Price FHFA Index

Notes: The outcome variable is the log of county-level House Price index come from FHFA (1717 counties between 1990 and 2005). The banking deregulation variable is a dummy variable indicating whether the statewide deposit cap is higher than 30%. Red dots are TWFE estimates; Black dots are CSDID estimates. All Figures show 95 percent confidence intervals. Standard errors are clustered at the state level.

Figure 11: Mechanisms III: Bank Deregulation and Fertility by Housing Tenure and Land Availability in the SIPP



(a) House Price: Less Land

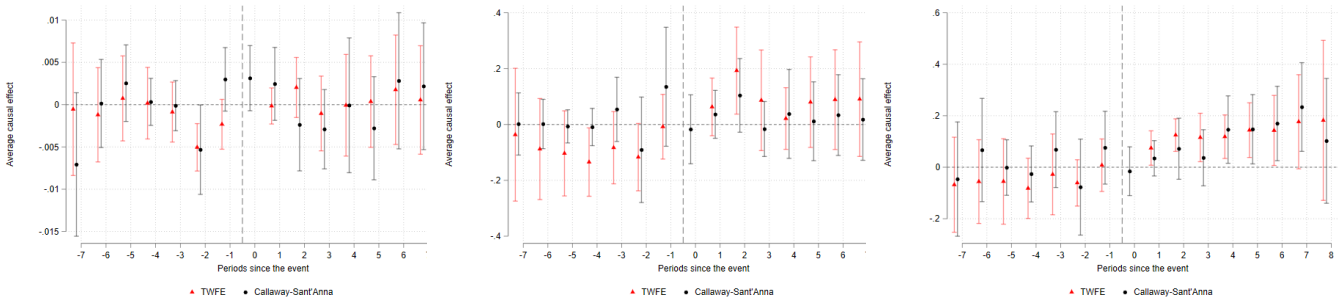
(b) House Price: More Land

(c) Fertility: Less Land

(d) Fertility: More Land

Notes: State-level land availability data comes from Lutz and Sand (2019) and a state is defined with less land meaning the percentage of developable land is less than 70%. The outcome variable in panels (a) and (b) is a log of the county-level House Price index that comes from the FHFA (1717 counties between 1990 and 2005). The outcome variable in panels (c) and (d) is the fertility dummy indicating whether the first child is born based on the SIPP 1990-2004 panels (62,026 individuals between 1990 and 2005). "own1990" is a dummy variable that indicates the female own a house in 1990. In all panels, the banking deregulation variable is a dummy variable indicating whether the statewide deposit cap is higher than 30%. All results are CSDID estimates. All Figures show 95 percent confidence intervals. Standard errors are clustered at the state level.

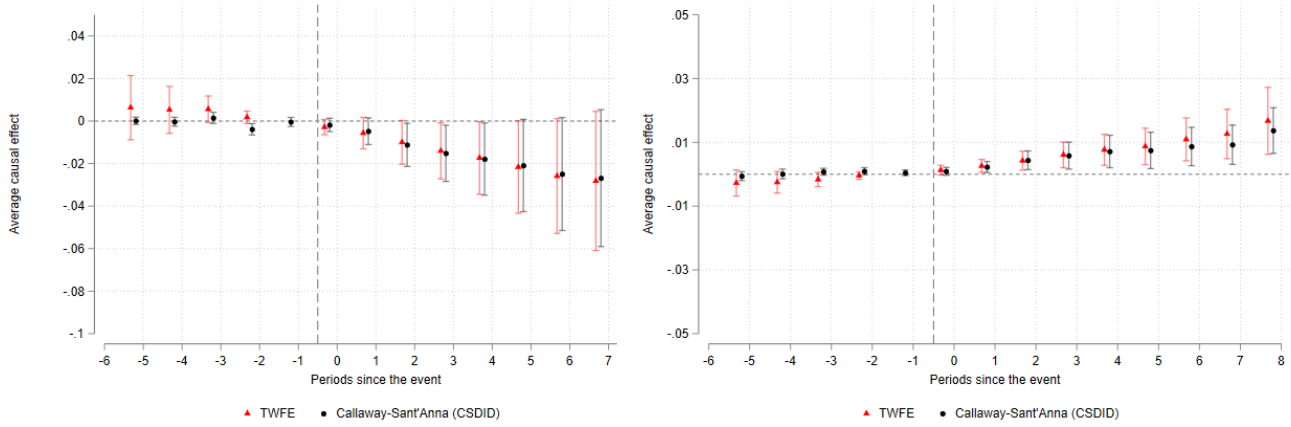
Figure 12: Mechanisms IV: Rent, Housing Wealth, and debts in the SIPP



(a) log of Rents (b) log of Household Debt (c) log of Household Home Equity

Notes: The Outcome variable in panel (a) is the log of self-reported monthly rent in the SIPP. The outcome variable in panel (b) is the log of household debt in the SIPP. The outcome variable in panel (c) is the log of household home equity among homeowners. The banking deregulation variable is a dummy variable indicating whether the statewide deposit cap is higher than 30%. Red dots are TWFE estimates; Black dots are CSDID estimates. All Figures show 95 percent confidence intervals. Standard errors are clustered at the state level.

Figure 13: Other Mechanism: Home-ownership and Marriage in the SIPP



(a) Home-ownership (b) Marriage

Notes: Outcome variable is the homeownership dummy indicating whether the female owns a house in panel (a) and the marriage dummy indicating whether the female is married in panel (b). The data is from the SIPP 1990-2004 panels (62,026 individuals between 1990 and 2005). The banking deregulation variable is a dummy variable indicating whether the statewide deposit cap is higher than 30%. Red dots are TWFE estimates; Black dots are CSDID estimates. All Figures show 95 percent confidence intervals. Standard errors are clustered at the state level.

Table 1: Years of Bank Deregulation

Year	Deregulated States
1995	Alaska
1996	Michigan, North Carolina, Pennsylvania, Rhode Island, Connecticut
	Delaware, Virginia, Maryland, California, Utah, Nevada
1997	South Dakota, Washington, Arizona, Massachusetts, Wisconsin
	South Carolina, Vermont, New Jersey, DC, New Mexico
1998	New York, Wyoming, Tennessee, Indiana, Illinois, Georgia
	Florida, Oregon, Alabama, Hawaii, Louisiana, Ohio, Maine
2000	New Hampshire

Data source: Rice and Strahan (2010).

Notes: The banking deregulation variable is a dummy variable indicating whether the statewide deposit cap is higher than 30%. 14 states never deregulated in terms of Deposit Cap. Those states include Kentucky, Kansas, Nebraska, North Dakota, Montana, Idaho, Colorado, Missouri, Mississippi, Oklahoma, Iowa, Minnesota, West Virginia, Arkansas, and Texas.

Table 2: Summary Statistics of Natality Sample and the SIPP Sample

	Natality Sample	SIPP Sample			
	With Child	With Child	No Child	Total	Diff.
	(1)	(2)	(3)	(4)	(5)
Age of Birth	26.92	23.22			
NH White	0.79	0.82	0.85	0.83	0.03***
NH Black	0.15	0.14	0.11	0.13	-0.03***
Less than HS	0.21	0.10	0.04	0.08	-0.06***
High School	0.33	0.39	0.30	0.36	-0.08***
College and above	0.42	0.52	0.66	0.56	0.14***
Married	0.68	0.74	0.47	0.65	-0.27***
Metro Area	0.63	0.77	0.82	0.79	0.05***
Own a house in 1990		0.11	0.13	0.12	0.02***
# of Birth/Individual	51,829,572	42,060	19,966	62,026	

Data source: Natality sample come from the Vital Statistics Natality Files which covers 318 counties between 1990 and 2004; the SIPP sample comes from the SIPP 1990, 1991, 1992, 1993, 1996, 2001, and 2004 panels. The SIPP sample consists of female workers who are between 10 and 44 in 1990.

Table 3: Summary Statistics of Females in the SIPP: by Banking Deregulation Status

	Total	Deregualated States	Regulated States	Diff.
	(1)	(2)	(3)	(4)
Age of Birth	23.22	23.39	22.57	-0.82***
Have Children	0.68	0.67	0.70	0.02***
White	0.83	0.82	0.86	0.04***
Black	0.13	0.13	0.11	-0.02***
Less than HS	0.08	0.07	0.09	0.02***
High School	0.36	0.36	0.36	0.00
College	0.56	0.56	0.55	-0.02***
Married	0.65	0.64	0.68	0.03***
Metro Area	0.79	0.83	0.61	-0.22***
Own a House in 1990	0.23	0.23	0.24	0.01**
Observations	62,026	49,804	12,222	

Data source: SIPP sample comes from the SIPP 1990, 1991, 1992, 1993, 1996, 2001, and 2004 panels. The sample consists of female workers who are between 10 and 44 in 1990. "Deregulated states" are states that ever experienced banking deregulation in the period of 1990-2005. The banking deregulation variable is a dummy variable indicating whether the statewide deposit cap is higher than 30%.

Table 4: Banking Deregulation and County-level Fertility Rate: TWFE and CSDID

	TWFE		CSDID	
	(1)	(2)	(3)	(4)
Deregulation Dummy	-0.024***		-0.025***	
	(0.006)		(0.009)	
Lead5event		0.024*		0.000
		(0.012)		(0.003)
Lead4event		0.021**		0.002
		(0.009)		(0.003)
Lead3event		0.017***		-0.001
		(0.006)		(0.003)
Lead2event		0.008**		-0.007***
		(0.004)		(0.002)
Lead1event		0.006***		-0.001
		(0.002)		(0.003)
Lag1event		-0.012***		-0.013**
		(0.004)		(0.006)
Lag2event		-0.018**		-0.019**
		(0.008)		(0.009)
Lag3event		-0.030**		-0.025**
		(0.014)		(0.011)
Lag4event		-0.040**		-0.038***
		(0.018)		(0.012)
Lag5event		-0.052*		-0.046***
		(0.026)		(0.016)
Observations	5700	5700	5700	5700
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Note: The outcome variable is the log of county-level fertility rate calculated based on the Vital Statistics Natality Files (318 counties between 1990 and 2004). The banking deregulation variable is a dummy variable indicating whether the statewide deposit cap is higher than 30%. Standard errors are clustered at the state level.* p<0.10, ** p<0.05, *** p<0.01.

Table 5: Banking Deregulation and Individual Fertility in SIPP: TWFE and CSDID

	TWFE		CSDID	
	(1)	(2)	(3)	(4)
Deregulation Dummy	-0.005***		-0.006***	
	(0.002)		(0.001)	
Lead6event		0.008***		-0.001
		(0.003)		(0.001)
Lead5event		0.004**		-0.001
		(0.002)		(0.001)
Lead4event		0.003**		-0.000
		(0.002)		(0.001)
Lead3event		0.002		-0.001
		(0.001)		(0.001)
Lead2event		0.003**		0.001
		(0.001)		(0.001)
Lead1event		0.001***		-0.001
		(0.000)		(0.001)
Lag1event		-0.001***		-0.003***
		(0.000)		(0.001)
Lag2event		-0.003***		-0.006***
		(0.001)		(0.001)
Lag3event		-0.003***		-0.007***
		(0.001)		(0.001)
Lag4event		-0.003***		-0.007***
		(0.001)		(0.001)
Lag5event		-0.003**		-0.007***
		(0.001)		(0.001)
Lag6event		-0.003*		-0.007***
		(0.002)		(0.001)
Observations	992416	992416	992416	992416
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Note: Outcome variable is the fertility dummy indicating whether the first child is born based on the the SIPP 1990-2004 panels (62,026 individuals between 1990 and 2005). The banking deregulation variable is a dummy variable indicating whether the statewide deposit cap is higher than 30%. Standard errors are clustered at the state level.* p<0.10, ** p<0.05, *** p<0.01.

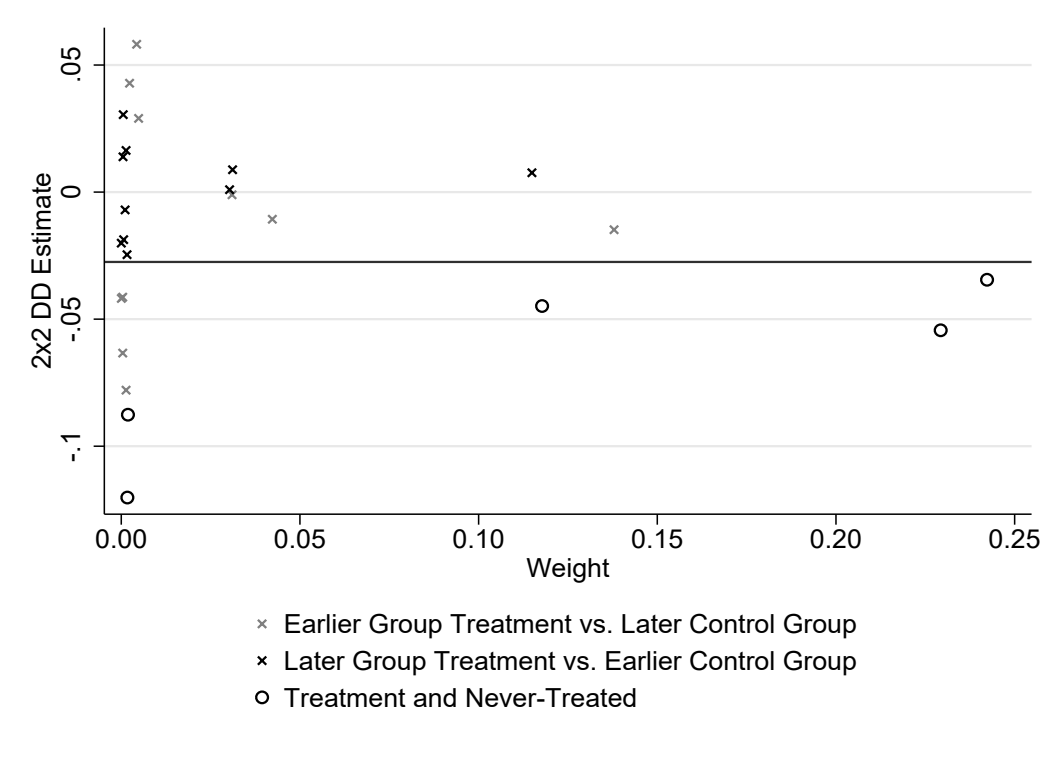
Table 6: Banking Deregulation and County-level House Price:
TWFE and CSDID

	TWFE		CSDID	
	(1)	(2)	(3)	(4)
Deregulation Dummy	0.015**		0.019**	
	(0.006)		(0.009)	
Lead4event		-0.001		-0.001
		(0.007)		(0.004)
Lead3event		-0.005		-0.002
		(0.007)		(0.003)
Lead2event		-0.004		0.003
		(0.003)		(0.004)
Lead1event		-0.002		0.003
		(0.003)		(0.002)
Lag1event		0.004		0.004
		(0.003)		(0.004)
Lag2event		0.006*		0.010
		(0.003)		(0.006)
Lag3event		0.010**		0.013*
		(0.004)		(0.007)
Lag4event		0.013**		0.017*
		(0.006)		(0.009)
Lag5event		0.016**		0.017*
		(0.007)		(0.009)
Lag6event		0.018**		0.024**
		(0.008)		(0.010)
Lag7event		0.021**		0.027**
		(0.010)		(0.012)
Lag8event		0.038**		0.030**
		(0.015)		(0.015)
Observations	27472	27472	27472	27472
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Note: Outcome variable is the log of county-level House Price index come from FHFA (1717 counties between 1990 and 2005). The banking deregulation variable is a dummy variable indicating whether the statewide deposit cap is higher than 30%. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01.

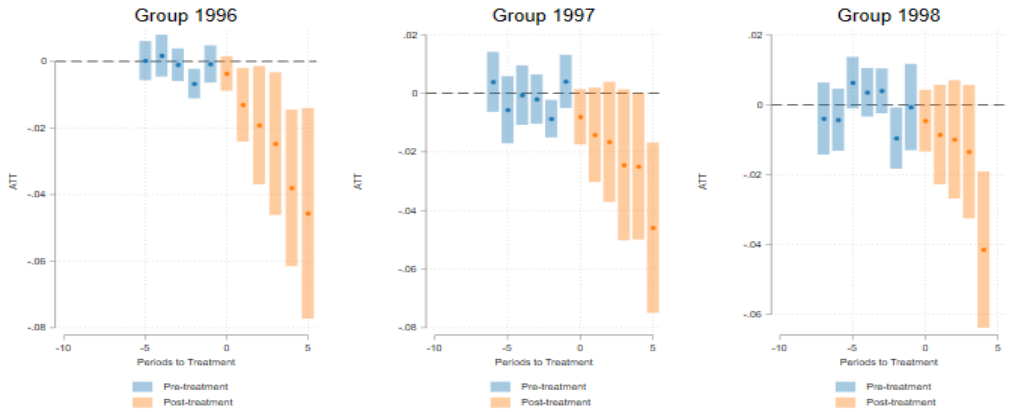
Appendix Tables and Figures

Figure A1: Bacon Decomposition



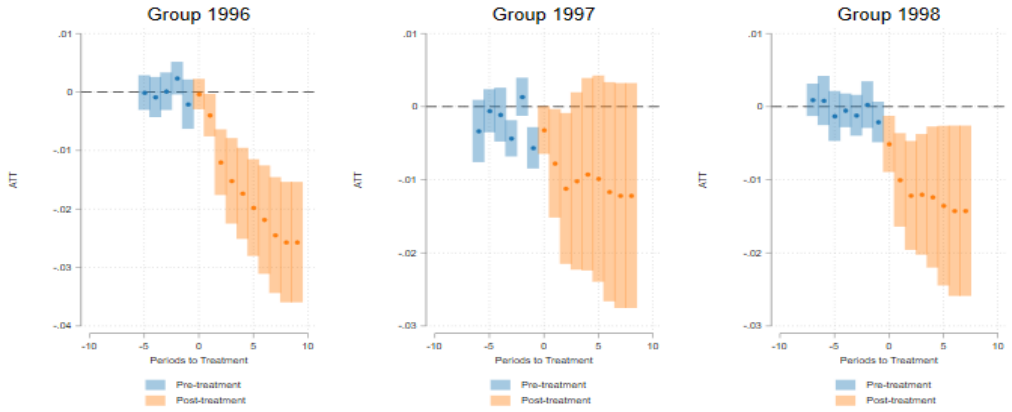
Notes: The figure plots each 2x2 DD components from the decomposition theorem against their weight for the banking deregulation which is measured as a dummy variable indicating whether the statewide deposit cap is higher than 30%.

Figure A2: CSDID: Banking Deregulation and County-level Fertility by Treatment Groups



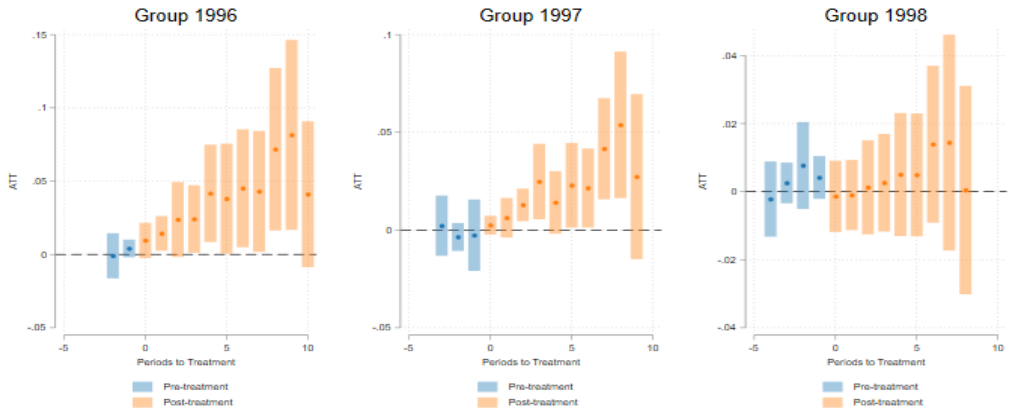
Notes: Outcome variable is the log of county-level fertility rate calculated based on the Vital Statistics Natality Files (318 counties between 1990 and 2004). The banking deregulation variable is a dummy variable indicating whether the statewide deposit cap is higher than 30%. All Figures show 95 percent confidence intervals. Standard errors are clustered at the state level.

Figure A3: CSDID: Banking Deregulation and Individual Fertility in SIPP by Treatment Groups



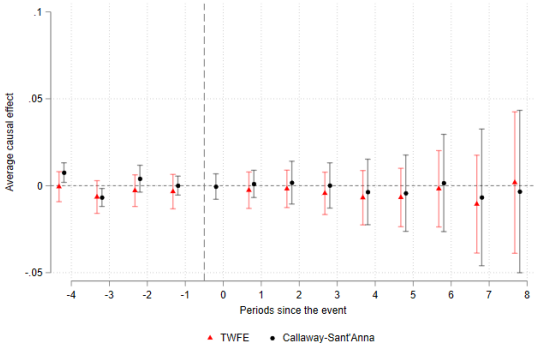
Notes: Outcome variable is the fertility dummy indicating whether the first the child is born based on the SIPP 1990-2004 panels (62,026 individuals between 1990 and 2005). The banking deregulation variable is a dummy variable indicating whether the statewide deposit cap is higher than 30%. All Figures show 95 percent confidence intervals. Standard errors are clustered at the state level.

Figure A4: CSDID: Banking Deregulation and House Price by Treatment Groups

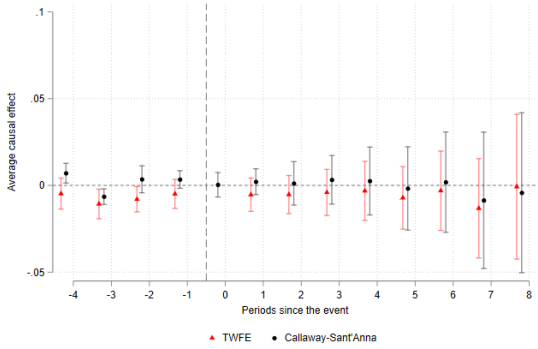


Notes: Outcome variable is the log of county-level House Price index come from FHFA (1717 counties between 1990 and 2005). The banking deregulation variable is a dummy variable indicating whether the statewide deposit cap is higher than 30%. All Figures show 95 percent confidence intervals. Standard errors are clustered at the state level.

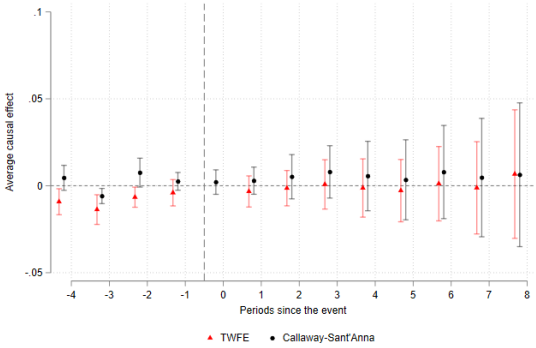
Figure A5: Other Types of Banking Deregulation and House Price (FHFA)



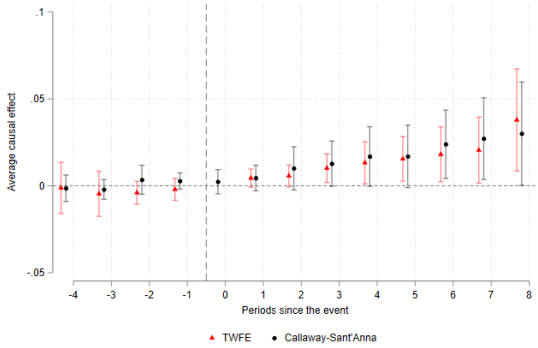
(a) Minimum Age of Institution for Acquisition Less than 3 Years



(b) Allow de novo Interstate Branching



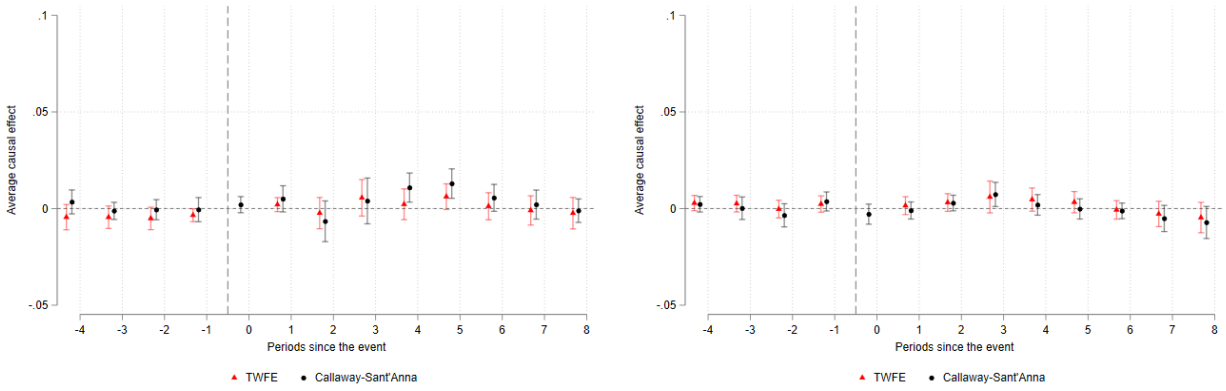
(c) Allow Acquisition of Individual Branches without Acquiring the Entire Bank



(d) Statewide Deposit Cap on Branch Acquisitions> 30%

Notes: Outcome variable is the log of county-level House Price index come from FHFA (1717 counties between 1990 and 2005). The banking deregulation variable in panel (a) is a dummy variable indicating whether the minimum age of institution for acquisition is less than 3 years; in panel (b) is a dummy variable indicating whether de novo interstate branching acquisition is allowed; in panel (c) is a dummy variable indicating whether the acquisition of individual branches without acquiring the entire bank is allowed; in panel (d) indicating whether the statewide deposit cap is higher than 30%. Red dots are TWFE estimates; Black dots are CSDID estimates. All Figures show 95 percent confidence intervals. Standard errors are clustered at the state level.

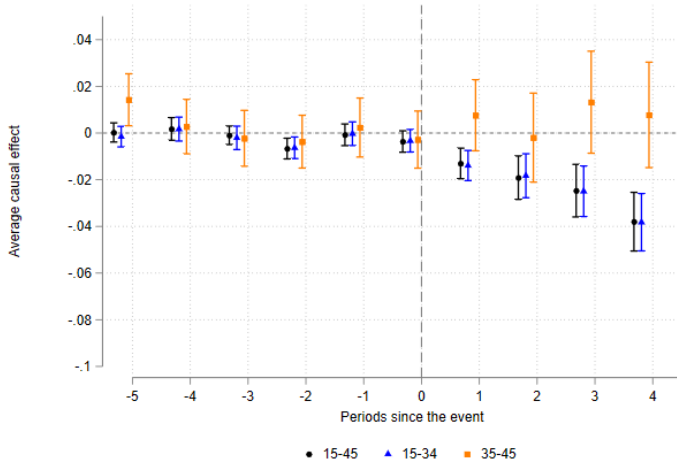
Figure A6: Banking Deregulation and Local Labor Market Outcomes: TWFE and CSDID



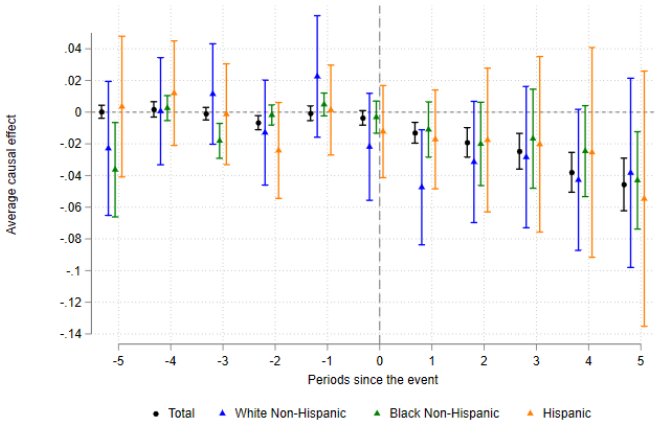
(a) County-level Employment (b) County-level Wage

Notes: Outcome variables are logs of county-level employment and wage calculated based on the Quarterly Census of Employment and Wages (QCEW) (1990-2005). The banking deregulation variable is a dummy variable indicating whether the statewide deposit cap is higher than 30%. Red dots are TWFE estimates; Black dots are CSDID estimates. All Figures show 95 percent confidence intervals. Standard errors are clustered at the state level.

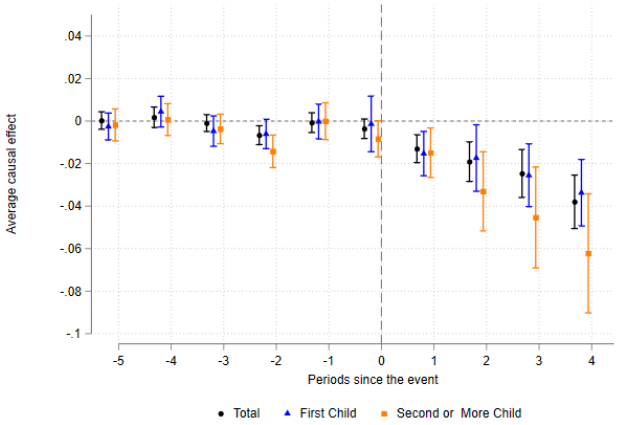
Figure A7: Banking Deregulation and County-level Fertility
Heterogeneity Effects



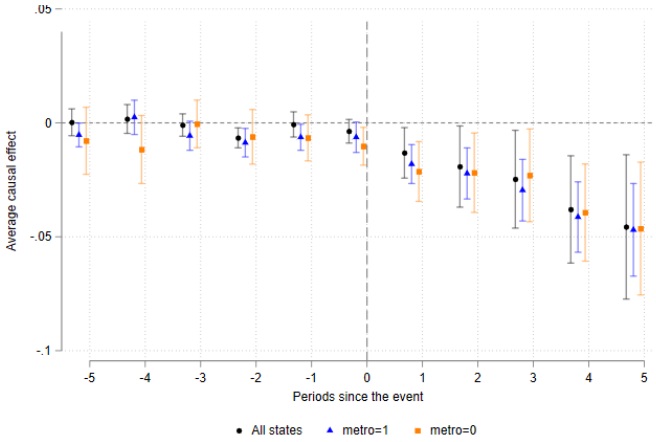
(a) Age Group



(b) Race



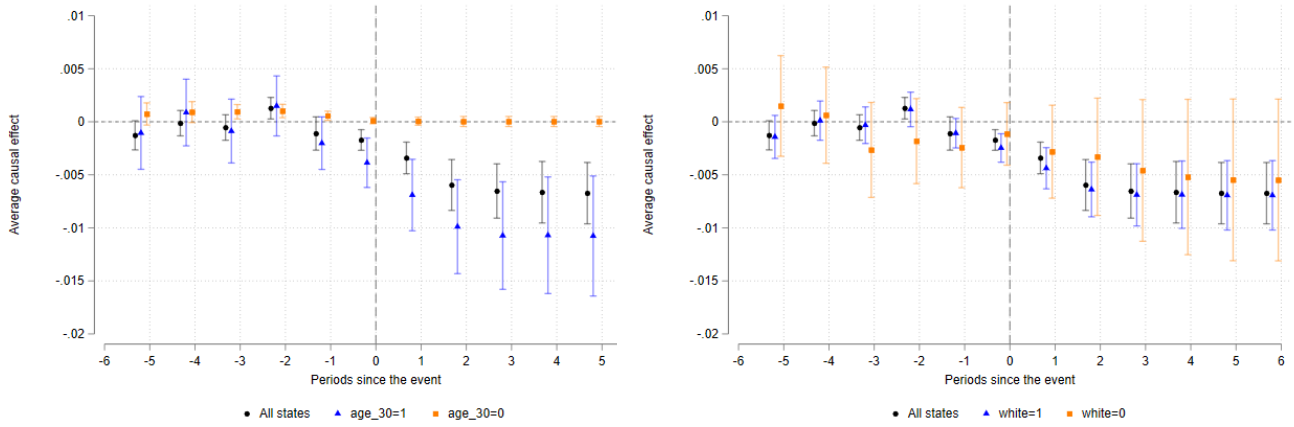
(c) Birth Order



(d) Metro vs Non-Metro

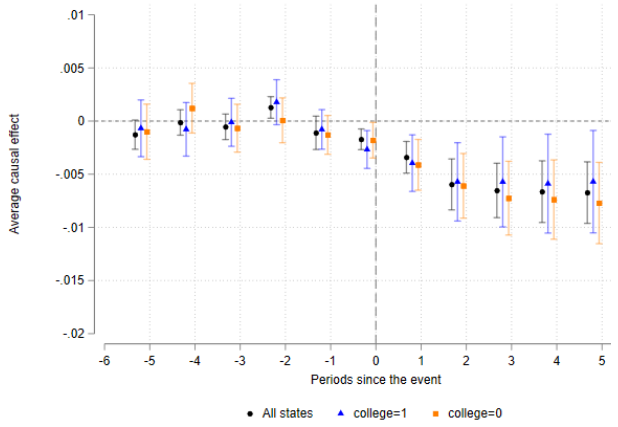
Notes: Outcome variable is the log of county-level fertility rate calculated based on the Vital Statistics Natality Files (318 counties between 1990 and 2004). The banking deregulation variable is a dummy variable indicating whether the statewide deposit cap is higher than 30%. All results are CSDID estimates. All Figures show 95 percent confidence intervals. Standard errors are clustered at the state level.

Figure A8: Banking Deregulation and Fertility in the SIPP
Heterogeneity Effects

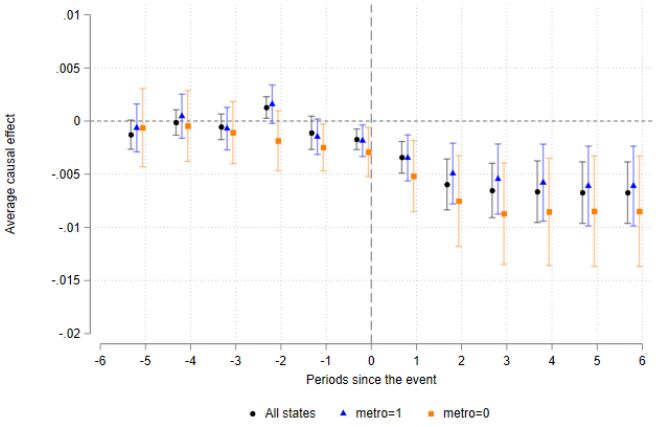


(a) below 30 or Above 30 in 1990

(b) Black vs non-Black



(c) College vs Non-college



(d) Metro vs Non-Metro

Notes: Outcome variable is the fertility dummy indicating whether the first child is born based on the SIPP 1990-2004 panels (62,026 individuals between 1990 and 2005). The banking deregulation variable is a dummy variable indicating whether the statewide deposit cap is higher than 30%. All results are CSDID estimates. All Figures show 95 percent confidence intervals. Standard errors are clustered at the state level.