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

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Abstract

This paper examines the causal effect of bank credit expansion on fertility by exploiting exogenous increases in bank credit supply generated by U.S. interstate branching deregulation between 1994 and 2005. I employ both traditional and staggered difference-in-difference methods to estimate the causal effect of credit expansion on fertility rates and maternal age. I find that credit expansion reduces fertility rates by 7 percent and increases maternal age by 0.37 percent. I also provide evidence that the housing cost effect is the main mechanism behind the fertility response. My findings highlight the importance of financial market policies and housing affordability for demographic outcomes.

Keywords: Bank Branching Deregulation, Credit Expansion, House Prices, Homeownership, Fertility

JEL Codes: J13, G21, R21, R31

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1 Introduction

Fertility rates in the United States have gradually declined since the 1990s, posting challenges for economic and social policy (Kearney et al., 2022; Doepke et al., 2022).[1] Though researchers have been searching for explanations behind this decline, 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, particularly before the 2008 Great Recession and during the recent COVID-19 pandemic. 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 uncovers the causal effects of bank credit expansion on fertility decline, revealing the unintended consequences of financial market policies in explaining demographic trends.

Theoretically, bank credit supply expansion can affect the demand for children through three offsetting channels. First, credit supply can lead to an increase in house prices which can increase or decrease fertility depending on the household's tenure status. On one hand, the increase in house prices can have a negative "housing cost effect" on the demand for children for both potential first-time homeowners (i.e., current renters who would buy a house with the addition of a child) and current homeowners who want to buy a larger house with the addition of a child (Simon and Tamura, 2009; Dettling and Kearney, 2014). On the other hand, for homeowners with a decent-sized house, an increase in house prices can increase childbirth through a "housing wealth effect" when housing equity is used to pay for childcare costs (Dettling and Kearney, 2014; Lovenheim and Mumford, 2013; Daysal et al., 2021). These effects are referred to as a "housing market channel." Second, credit supply expansion can help households relax financial liquidity constraints which can smooth the costs of childbearing and child care which can increase the demand for children (Cumming and Dettling, 2020). This is referred to as a "financial market or liquidity channel." Third, if the credit supply can stimulate local economic and job growth, it may also reduce fertility by increasing the mother's opportunity costs. This is referred to as a "labor market channel." In summary, the relationship between

[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 (Figure 1).

[2] Fertility rates have declined since the 1960s and many studies have explored potential explanations behind the declines which include the 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), 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), and the changes in social norms, attitude and preferences for having children (Fernández and Fogli, 2006, 2009; Stone, 2018; De Silva and Tenreyro, 2020; Boelmann et al., 2021).

[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.

credit supply expansion and fertility is complex. On one hand, housing costs and labor market effects suggest a negative relationship. On the other hand, housing wealth and liquidity effects suggest a positive relationship. The overall impact of credit supply expansion on fertility is theoretically ambiguous and depends on the relative size of these effects. This paper aims to identify the causal net effects of credit supply expansion on fertility and explore the different channels through which it operates.

Identifying the causal effects of credit supply expansion is challenging due to the issue of endogeneity. First, there may be omitted variables that correlate with both credit supply and household childbirth decisions. For example, positive labor demand shocks may increase credit supply and affect childbirth by either increasing household income or raising the opportunity cost of time for female workers. This can create an upward or downward bias in estimation. Moreover, there may be reverse causality, where childbirth outcomes influence credit supply. For example, areas with higher childbirth rates may have a higher demand for credit, leading to the expansion of the banking sector and easier credit access (Lisack et al., 2017; Gong and Yao, 2022).

To address this identification challenge, I examine the exogenous expansion of bank credit supply caused by the Interstate Banking and Branching Efficiency Act (IBBEA) of 1994. This act deregulated the banking industry and made interstate bank branching legal. Between 1994 and 2005, states had the option to impose barriers to out-of-state bank entry, resulting in staggered deregulation across states and a significant increase in credit supply in deregulated states.[4] The staggered deregulation in interstate bank branching provides a source of quasi-experimental variation for identifying the causal effect of credit supply expansion on fertility.

For the main outcome variable, I construct the fertility rate by aggregating births and female population counts to the county-year cell for the period of 1990-2004. The number of births comes from the Vital Statistics Natality Files 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 CDC SEER database. Moreover, to explore the impact on the timing of fertility decisions, I calculate the average maternal age at the county-year level and use it together with fertility rates as two main outcome variables to establish the connections between credit expansion and fertility outcomes. Considering the large variations of house price changes across counties within a state or metropolitan area, I choose county as the geographic unit to measure fertility rate instead of

[4]Though it is well-documented in the literature that this deregulation increases the supply of credit, most existing studies focus on the impact of deregulation on financial-related outcomes. Previous studies have found that deregulated states experienced an increase in the supply of credit which translated into the reduction of financial cost among small firms(Rice and Strahan, 2010), the rising housing prices in areas where the housing supply is inelastic (Favara and Imbs, 2015), and the increased financial inclusive among low-income household (Célerier and Matray, 2019).

state or metropolitan areas as in Dettling and Kearney (2014) and Kearney et al. (2022).

I estimate the causal effect of bank branching deregulation on fertility adopting a difference-in-difference (DID) design with two-way fixed effects (TWFE) regressions to compare fertility rates in deregulated and regulated states before and after the deregulation. Under a parallel trends assumption, the state-year variation generated by the sharp but staggered deregulation of interstate bank branches allows me to obtain causal estimates of the credit supply expansion on fertility.

Moreover, since states are deregulated in a staggered matter, the DID setup has multiple periods and multiple treatment groups with different years of treatment, 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. This is because early-treated states are used as a control for later-treated states which introduces bias when treatment effects are heterogeneous.[5]

To alleviate this concern, I use an alternative DID estimation newly developed by Callaway and Sant'Anna (2021). 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. By shutting down the comparisons between early-treated and later-treated units, the robust estimators deliver consistent estimates even in the presence of heterogeneous treatment effects across time and/or treated units. This method also produces easy-to-interpret causal parameters that can be used to learn about treatment effect heterogeneity and construct dynamic causal parameters at the calendar year or event-year level.

Both standard TWFE and CSDID estimates show a sharp and persistent decline in fertility rate and an increase in maternal age among females in deregulated states after the bank branching deregulation. This suggests that credit supply leads to reduced and delayed fertility. The effects based on CSDID are greater, suggesting that the TWFE models underestimate the effect of credit supply expansion. Specifically, based on the CSDID estimates, county-level female fertility rates in deregulated states are lower by 0.005 percentage points after the deregulation which is equivalent to a reduction of 7 percent in fertility rate or a reduction of the number

[5]As explained in Goodman-Bacon (2021), Roth et al. (2022), Baker et al. (2022), and de Chaisemartin and D'Haultfoeuille (2022), TWFE DID estimates make both clean comparisons between treated and not-yet-treated units as well as forbidden comparisons between units that are both already treated where early treated units act as control groups. When treatment effects are heterogeneous, these forbidden comparisons potentially can obtain the opposite sign compared to the true ATE or ATT, even when treatment assignment is randomized and the parallel trends assumption holds. Also see Borusyak et al. (2022); de Chaisemartin and D'Haultfoeuille (2020); Callaway and Sant'Anna (2021); Sun and Abraham (2021); Athey and Imbens (2022) for discussion of this issue.

of births by 4 per 1000 women aged 15-44.[6] This negative effect on the fertility rate is substantial in magnitude considering that the annual fertility rate has decreased by 21% between the thirty-year period of 1991 and 2020. Meanwhile, the county-level average of maternal age in deregulated states is higher by 0.101 years after the deregulation which is equivalent to an increase of 0.37 percent in maternal age.[7] Those results are robust when adding time-varying economic, demographic, and policy controls, additional state-specific time-trends, or excluding not-yet-treated states as a control sample. Further analysis shows that the effect of reduced and delayed fertility is stronger among unmarried or Hispanic Millennial females.

Why does the increase in credit supply reduce and delay fertility? The theory suggests two plausible explanations. The first is the housing cost effect which suggests that the local credit supply expansion reduces and delays fertility by increasing local house costs which contribute to a large proportion of childbearing costs (Simon and Tamura, 2009; Dettling and Kearney, 2014). The second is a labor market effect which suggests the local credit supply expansion reduces and delays fertility by stimulating local economic and job growth which increases the mother's opportunity costs.[8]

To test these mechanisms, I conduct four sets of empirical analyses. First, I divide the sample into counties 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 that house price responses caused by the credit supply expansion should be more evident in counties where the housing supply is inelastic and muted in elastic areas, where the stock of housing increased instead. Thus, if the housing market channel is important, we should observe a more evident reduction and delay of fertility in counties with less developable land. This is indeed the case. Additionally, I estimate the effect of bank deregulation on house prices using a similar DID design where housing prices are 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 and is only significant in areas with less land. Those results suggest an important role of the housing cost effect in explaining the connection between credit supply expansion and reduced and delayed fertility.

Second, I show that bank branching deregulation constitutes a legitimate instrument for the independent variable of fertility outcomes on house price growth at the county level. In an instrumental variable sense, branching deregulation can account for the rise of housing prices and can explain a significant share of the resulting decline in the fertility rate and the increase

[6]The average fertility rate during the sample period is about 68 births per 1000 women aged 15-44 (0.068 percentage points).

[7]The average maternal age during the sample period is 27.

[8]Housing wealth and liquidity effects are predicted to increase fertility which can not explain why the credit supply expansion reduces and delays fertility.

in maternal age, confirming the housing cost effect.

Third, I show that bank branching deregulation has no significant effects on labor market outcomes at the aggregate level which excludes the mechanism of credit supply expansion that reduces and delays fertility through mother's opportunity costs.

Finally, one limitation of the Natality birth data is that it lacks information on the housing tenure status (Dettling and Kearney, 2014). So, it is difficult to directly test the role of housing tenure and to decompose different mechanisms. To overcome this challenge, I use the Survey of Income Program and Participation (SIPP) 1990-2004 panels as additional data sources to test the effect of credit supply on fertility among homeowners and renters, separately. I find the negative effect on fertility mainly comes from renters (those who are renters before the deregulation) instead of homeowners, which supports the housing cost channel. Additionally, I find the effects of bank branching deregulation have insignificant effects on female labor supply at the individual level which further exclude the feasibility of the labor market channel.

The rest of the paper proceeds as follows. In section 2, I review the literature. In section 3, I provide a conceptual theory behind the fertility decision and describe the nature of the banking deregulation in the United States. In Section 4, I describe the data and present summary statistics of the sample. In Section 5, I present the traditional and staggered DID estimation models. In section 6, I present the main empirical results and provide a variety of robustness checks and discuss heterogeneous effects. In Section 7, I discuss possible mechanisms behind the main effects. Section 8 concludes the paper.

2 Literature Review

This paper contributes to the literature that connects financial market policies and housing market dynamics with fertility decisions in the context of developed countries.[9] Dettling and Kearney (2014) investigated the effect of house prices on fertility by examining the connection between MSA-level housing prices and births. Cumming and Dettling (2020) studied how monetary policy pass-through to mortgage interest rates and increased household fertility among homeowners using administrative data on mortgages and births in the UK. Additionally, a strand of literature used household-level data in the U.S., Japan, Australia, and Denmark to study the connection between housing wealth and fertility (Lovenheim and Mumford, 2013; Mizutani et al., 2015; Atalay et al., 2021; Daysal et al., 2021). Those studies focus on homeowners and thus only identify the housing wealth channel instead of the housing costs channel. Results in this paper are largely consistent with the literature and complement it in three ways. First,

[9] In the context of developing countries, particularly in Asia, studies have found housing booms are negatively associated with fertility rates (Yi and Zhang, 2010; Lin et al., 2016; Liu et al., 2020; Pan and Yang, 2022; Tang et al., 2022; Liu et al., 2023) unless the focus is on the housing wealth effect among homeowners (Tan et al., 2023).

this paper provides new evidence of the impact of the expansion of credit supply on fertility outcomes which are rarely explored in the literature. Second, it uses bank branching expansion as exogenous shocks for variations in credit supply and a newly-developed DID method to better identify the causal effect. Third, this paper provides extensive discussions of different channels besides the housing market in determining the overall trend of fertility.

Besides housing market dynamics and policies, many other explanations have been proposed to explain why the birth rate in the U.S. is declining since the 1960s and is at or near historic lows. First, the widespread usage of the contraceptive pill has been found to significantly reduce fertility in the U.S. as well as in other countries (Bailey, 2010; Rau et al., 2021). Second, childcare costs and the availability of childcare facilities outside of the home have been shown to play an important role when females make fertility and 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). Fourth, changes in social norms, attitudes, and preferences for having children have also been proposed as explanations (Fernández and Fogli, 2006, 2009; Stone, 2018; De Silva and Tenreyro, 2020; Boelmann et al., 2021). This paper contributes to this strand of literature by providing a new explanation behind declining birth rates.

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. and provides new evidence on the nature of financial market policy transmission to the real economy. 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),[10] 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) found U.S. bank branching expansion between 1994 and 2005 led to exogenous expansion in mortgage

[10]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.[11] 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 deregulated 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 rising 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.[12] .

This paper is the first to explore the connection between bank branching deregulation and demographic outcomes, significantly broadening the scope of current literature. The results have important implications for financial market policies and demonstrate how this policy can affect the aggregate economy through changes in fertility that feed into the population and economic growth. The adoption of the newly-developed DID method also improves the estimation results on house prices in previous studies.

One thing worth noticing is that most existing studies on banking deregulation consider the first wave of deregulation that happened in the 1970s and 1980s, while this paper focuses on the second wave of banking deregulation that happened in the 1990s. Other papers that explored the second wave of bank deregulation include Rice and Strahan (2010), Favara and Imbs (2015), and Célerier and Matray (2019). These studies documented significant differences in the consequences of the two waves of banking deregulation. For example, the first wave had a large impact on income, unemployment, and economic growth, but not on house prices. In contrast, the second wave had a large effect on house prices but not on the economic outcomes mentioned above. A comprehensive comparison of the two waves of banking deregulation would

[11] 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).

[12] 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.

be an interesting topic for future research.

3 Background

3.1 Theoretical Background: Fertility Decision

In a simple static model of fertility, 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 they face. This approach assumes children are normal goods (Becker, 1960) because they bring utility in the form of life satisfaction, happiness, or pleasurable experiences. This implies that fertility should respond positively to an increase in household income or wealth.[13] Children also come with associated costs, broadly defined, including both time and money. Thus, an increase in the cost of raising a child negatively affects the demand for children.

Based on this static model of fertility, I present three channels through which banking deregulation can affect household fertility decisions. The first one is the housing market channel. The increase in credit supply can push up local house prices. On one hand, an increase in house prices can lead to rising rents and costs for future house purchasing which can have a negative "housing cost effect" on the demand for children for both potential first-time homeowners (i.e., current renters who would buy a house with the addition of a child) and current homeowners who might buy a larger house with the addition of a child (Simon and Tamura, 2009; Dettling and Kearney, 2014). This rising cost effect might be particularly evident among young females who have relatively fewer financial resources and are more likely to be down-payment-constrained. The association between homeownership and the demand for children is strong in the context of the U.S. and other developed countries not only because homeownership is generally associated with stable home and school environment that are essential for child development (Green and White, 1997; Glaeser and Sacerdote, 2000; Dietz and Haurin, 2003; Coulson and Li, 2013) but also because parenting has become increasing resource and time intensive and homeownership provides a commitment for investment in child investment(Doepke and Zilibotti, 2019; Doepke et al., 2022; Lundberg and Pollak, 2014; Lafortune and Low, 2023). On the other hand, for

[13]The early or first generation of empirical evidence based on pre-2000 data, however, this has been largely inconsistent with this prediction showing a negative correlation between income and the number of children. There are two major explanations for this observed negative correlation that maintains the assumption of children as normal goods. The first one is the quantity and quality trade-off Becker (1960) which suggests when income rises, parents can substitute away from the number of children, toward quality per child if the income elasticity of demand for quality exceeds the elasticity for quantity. The second is the cost of time hypothesis which attributes the observed negative relationship between income and fertility to the higher cost of parental time experienced by higher-income families, either because of increased market wage rates or because higher household income raises the value of parental time in non-market activities (Mincer, 1963; Becker, 1965). However, as pointed out by Doepke et al. (2022), much has changed over the last few decades regarding fertility trends, and fertility is no longer negatively related to income across high-income countries which suggests the research of fertility has entered a new era that calls for new explanations.

homeowners with a decent-sized house, an increase in house prices can increase childbirth rates through a "housing wealth effect" when housing equity is used to pay for childcare costs (Dettling and Kearney, 2014; Lovenheim and Mumford, 2013; Daysal et al., 2021).

Notable, the housing cost and wealth effects both depend on local housing supply elasticity. In areas with elastic housing supply, bank credit expansion increases the availability of mortgage credit, and relaxed lending standards can increase the likelihood of becoming a homeowner without pushing up housing prices, resulting in moderate housing cost and wealth effects. However, in areas with inelastic housing supply, bank credit expansion can push up house prices from a general equilibrium perspective which increases the costs for down payment-constraint households and makes them less likely to become a homeowner or update to a larger house. It also increases the wealth among homeowners with a decent-sized house. These divergent effects across geographic areas and across different housing tenure groups suggest that we should explore beyond overall trends.

A second is a financial market channel which suggests that bank branching 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. The third is a labor market channel that suggests that if the credit supply can stimulate local economic and job growth, it may also reduce fertility by increasing the mother's opportunity costs. In sum, the housing cost and labor market effects suggest a negative relationship between credit supply and fertility, while the housing wealth and liquidity effects suggest a positive relationship. The net effect of credit supply expansion on aggregate birth rates is ambiguous and depends on the relative size of these effects. One goal of this paper is to discuss and test the relative importance of these different mechanisms.

3.2 Institutional Background: Banking Deregulation

Most U.S. states historically restricted interstate banking and branching,[14] 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

[14]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 were 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.

Strahan, 2006). In that year, the Interstate Banking and Branching Efficiency Act (IBBEA) was adopted, permitting bank holding companies to enter other states and operate branches. The IBBEA also granted individual states some latitude in deciding the timing of deregulation independently (Rice and Strahan, 2010; Favara and Imbs, 2015). Most of the policy changes took place between 1996 and 2002. By 2005, nine mid-western states still were not deregulated and we did observe additional deregulation afterward.[15] The staggered deregulation of the banking industry across states provides a natural setting for identifying the effect of banking deregulation on fertility outcomes.

Figure 2 Panel (a) shows the interstate bank branching deregulation was concentrated in 1996, 1997, and 1998.[16] Figure 2 Panel (b) maps the timing of deregulation across states, with darker shades representing later deregulation. This deregulation is more common and happened earlier in eastern and western states and relatively uncommon or happened later among states in the middle. These figures suggest that the timing of banking deregulation changes over time and geographically which helps us identify the causal effect based on a staggered DID design. To capture this policy variation, I create a deregulation dummy indicating whether the deregulation has taken place in a state.

Interstate bank branching deregulation differs across states not only in timing but also in degrees. States can relax restrictions in four dimensions: (1) requiring a minimum age of the targeted bank to be less than three years, (2) allowing de novo branching without an explicit agreement by state authorities, (3) allowing the acquisition of individual branches without acquiring the entire bank, and (4) allowing 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, the overall deregulation dummy can be decomposed into four dummies to capture the policy variation across states and years. Appendix Figures A1 and A2 show the timing of the four types of deregulation across states and we see that the reduction of the statewide deposit cap on bank branch acquisitions happened earlier and in more states compared with the other three types of deregulation. When evaluating the effect of the four types of deregulation separately, I find the effect of bank deregulation is mainly evident when adopting this policy dummy but not the other three. The results are consistent when I use this policy dummy or the overall deregulation dummy. Thus, for the rest of the paper, I adopt the overall deregulation dummy as the main measure of interstate bank branching deregulation.[17]

[15] Nine states never deregulated which include Arkansas, Colorado, Idaho, Iowa, Kansas, Mississippi, Missouri, Montana, and Nebraska.

[16] Appendix Table A1 reports the years when each state interstate bank branching deregulation took place.

[17] Rice and Strahan (2010) compute a time-varying regulation index that ranges from 0 to 4 to capture the state-level branching restrictions. 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).

4 Data and Summary Statistics

4.1 Vital Statistics Natality Birth Data

Data on births come from the Vital Statistics Natality Files (1990-2004).[18] Vital statistics data contain birth certificate information for virtually every live birth that takes place in the United States. Following Kearney et al. (2022), I construct county-year fertility rates for the years between 1990 to 2004 where fertility rate is defined as the total number of births to women in the county-year cell divided by the county-year female population counts which are obtained from the CDC SEER database.[19] Considering the large variations of house price changes across counties within a state or metropolitan area, I choose county as the geographic unit to measure fertility rate instead of state or metropolitan areas as in Kearney et al. (2022) and Dettling and Kearney (2014). To explore the impact on the timing of fertility decisions, I construct the average maternal age at the county-year level. For the rest of the paper, I use fertility rates as well as maternal age at the county-year level as two main outcome variables to establish the connections between banking deregulation and fertility outcomes.

Vital statistics data also identify a broad range of characteristics of each birth and mother. To explore heterogeneous effects, I also construct the fertility rate and maternal age by demographic groups such as marital status, race and ethnicity (Non-Hispanic White, Non-Hispanic Black, and Hispanic), birth order, and different cohort and age groups at the county level.

4.2 Other Data Sources

Following Kearney et al. (2022), I also consider other state-level time-varying economic factors that might affect fertility trends which include the state unemployment rate, unemployment rate, generosity of welfare benefits, the state minimum wage, and expenditures on child support enforcement. The generosity of welfare benefits measures the monthly maximum TANF benefit for a family of three and is measured in thousands of dollars. The child support enforcement expenditure measures the total annual expenditure of the state on programs that help noncustodial parents to pay for the financial support of their children and is measured in millions of dollars.[20] I also consider two reproductive health policies which include abortion restrictions in

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

[19] These data are available at `https://seer.cancer.gov/popdata/download.html`

[20] The economic and policy variable data come from Kearney et al. (2022). The unemployment rates, state minimum wage, and generosity of welfare benefits come from the University of Kentucky's Center for Poverty Research National Welfare Database (UKCPR 2021) which is publicly accessible at `https://ukcpr.org/resources/national-welfare-data`. The expenditures on child support enforcement are measured annually and come from Kearney and Levine (2015).

the form of parental notification laws or waiting periods.[21] For demographic controls, I include county-level population shares of women ages 15-29, women ages 30-44, non-Hispanic white women ages 15-44, non-Hispanic black women ages 15-44, and Hispanic women ages 15-44. Those variables are calculated based on the CDC SEER population count database.[22]

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 developable 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. Third, 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 as having less land if the percentage of developable land is less than 70% of the total area.[24]

To measure the local labor market outcomes, 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.

[21] Kearney et al. (2022) have considered six reproductive health policies with the potential to affect a woman's ability to achieve desired fertility: abortion restrictions in the form of parental notification laws, abortion restrictions in the form of waiting periods, health insurance coverage through Medicaid, mandatory coverage of contraception in private insurance plans, mandatory sex education, and mandatory contraception instruction laws. I consider two abortion restrictions in the form of parental notification laws or waiting periods as control variables among the six policy variables since the other four either lack information or lack variation between 1990 and 2004.

[22] One limitation of this data is it lacks information such as education, so demographic characteristics such as the share of females by education groups can not be calculated directly. Instead, following Kearney et al. (2022), I apply population shares estimated from the Current Population Survey Annual Social and Economic Supplement (CPS ASEC). Those shares are available at the state level, but not the county-level. Nevertheless, the main results are robust including stat-level share of subgroups by education.

[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.

4.3 Summary Statistics

I start by examining national fertility and housing time-series data in order to describe the aggregate correlation between bank credit supply, home price, and fertility between 1980 and 2020. I construct an annual birthrate measure, which is the number of births per thousand women ages 15 to 44 from the Vital Statistics Natality Files. The aggregate bank credit supply of all commercial banks is collected from Federal Reserve Economic Data.[25] The housing price measure is the national-level Housing Price Index (HPI) constructed by Federal Housing Finance Agency. The time-serious correlation between birth rates (solid line), the annual percent change of bank credit(red dashed line), and the annual percent change of housing prices (blue dashed line) over time is shown in Figure 1. Although on different scales, these data are consistent with a positive relationship between bank credit and housing price, and a negative relationship between the two serious with fertility rate. In times of high bank credit growth, we observe high housing price growth and low fertility rate, and vice versa. Thus, at least in the aggregate, this figure is suggestive that bank credit supply is positively associated with housing prices but negatively associated with fertility.

The upper panel of Table 1 reports summary statistics of the mother's characteristics in the natality birth data. The average maternal age is around 27. Among these mothers, 61 percent of them are non-Hispanic White, 15 percent are non-Hispanic Black, and 18 percent are Hispanic. 22 percent of them have no high school degree, 33 percent of them have a high school degree, 35 percent have a college degree, and 8 percent have a graduate degree. Moreover, 32 percent of them are married when the child is born. Those characteristics are quite consistent across states that went through interstate bank branching deregulation and those that did not.

The lower panel of Table 1 presents the regression sample which includes outcomes and control variables at the county or state-year level. The average county-level fertility rate is 6 percent, meaning 60 births among 1000 females who are aged 15-44. The average county-level maternal age is around 27 which is consistent with the summary statistics in the upper panel. Treated states have slightly higher average unemployment rates (5.52 vs. 4.77), minimum wages ($4.67 vs. $4.39), maximum monthly welfare benefits ($7000 vs. $640), and much larger annual child support enforcement expenditures (201 million vs. 60 million). Regarding reproduction policy, treated states are less likely to impose abortion restrictions compared with states that have never implemented this deregulation.

[25]See the website of this data source at https://fred.stlouisfed.org/series/TOTBKCR.

5 Empirical Strategy

5.1 Two-way Fixed Effects Model

To estimate the aggregate effect of banking deregulation on county-level fertility outcomes, 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 or average maternal age in county c of state s in year t. The dummy variable D_{st} indicates whether the banking deregulation took place in state s in year t. X_{ct} include time-varying economic control variables such as the state unemployment rate, the state minimum wage, the generosity of welfare benefits, child support enforcement expenditure, demographic control variables such as county-level population shares of women ages 15-29, women ages 30-44, non-Hispanic white women ages 15-44, non-Hispanic black women ages 15-44, and Hispanic women ages 15-44, and the two reproductive health policies which include abortion restrictions in the form of parental notification laws or waiting. δ_c and δ_t are county and year fixed effects, respectively.[26] These fixed effects remove 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.

In order to test for parallel trends and study the dynamics of treatment effects of banking deregulation, I also 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.

Since states are deregulated in a staggered way, the DID setup has multiple periods and multiple treatment groups with different years of treatment, deviating from the canonical two time periods and two groups DID setup. In this setup, several studies have noted that the

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

coefficients from standard TWFE models may not represent a straightforward weighted average of unit-level treatment effects both clean comparisons between treated and not-yet-treated units as well as forbidden comparisons between units that are both already treated where early treated units act as control groups. When there is treatment effect heterogeneity across time and/or units, changes in the outcomes of early-treated units may reflect changes in their treatment effects over time. Thus, the forbidden comparisons could reflect differences in treatment effects over time between different treatment cohorts and can obtain the opposite sign compared to the true ATE or ATT, even when the researcher is able to randomize treatment assignment (where the parallel-treads assumption holds).[27]

5.2 CSDID

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

[27] See Borusyak et al. (2022); de Chaisemartin and D'Haultfoeuille (2020); Goodman-Bacon (2021); Callaway and Sant'Anna (2021); Sun and Abraham (2021); de Chaisemartin and D'Haultfoeuille (2022); Baker et al. (2022); Roth et al. (2022); Athey and Imbens (2022) for more discussion of this issue.

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.

The 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 are parallel across treatment and control units.

In addition to CSDID developed by Callaway and Sant'Anna (2021), I also present the event study results generated by a set of other methods recently introduced by de Chaisemartin and D'Haultfoeuille (2020); Borusyak et al. (2022); Sun and Abraham (2021).

6 Empirical Results

6.1 Bank Deregulation and Fertility Rate

Table 1 presents estimates of the overall effect of banking deregulation on the county-level fertility rate and shows consistently negative effects across different specifications. The first column shows results using a two-way fixed effect model. In column (2), I adopt the CSDID model using both not-yet-treated as well as never-treated observations as controls. In column (3), I adopt the CSDID model using only never-treated observations as controls to avoid additional bias caused by not-yet-treated observations. In column (4), I add economic control including the state unemployment rate, the state minimum wage, the generosity of welfare benefits, and child support enforcement expenditure. Column (4) adds demographic control variables including share of county-level population shares of women ages 15-29, women ages 30-44, non-Hispanic white women ages 15-44, non-Hispanic black women ages 15-44, and Hispanic women ages 15-44. In column (6), I exclude states that have experienced changes in abortion restrictions between 1990 and 2004. In (4)-(6) columns, I adopt the CSDID model using both not-yet-treated as well as never-treated observations as controls. Results are quite stable across specifications.

The point estimate on the effect of banking deregulation in the preferred specification (the second column) is -0.005 and significant at a 1 percent level. Considering that the average county-level fertility rate is around 0.068 in the sample period, this estimation suggests a 7 percent decrease in fertility rate.[28] This negative effect on the fertility rate is substantial in magnitude considering that the annual fertility rate has decreased by 21% between the thirty-year period of 1991 and 2020.

To test for parallel trends and study the dynamics of treatment effects of banking deregulation, I present the event-study results in the lower panel of Table 2 and plot those results in Figure 3.[29] Figure 3 shows that the estimates are consistent with the parallel trends assumption: the coefficients on the years prior to the introduction of banking deregulation are all close to zero and exhibit no discernible pretrends across all specifications. More importantly, Figure 3 shows that there is a sharp and persistent decrease in fertility in deregulated states that persists 5 years after the deregulation. The estimated size of these negative effects increases from -0.001 to -0.007 according to the CSDID model in column (2).

6.2 Bank Deregulation and Maternal Age

Table 3 presents estimates of the overall effect of banking deregulation on the county-level average of maternal age and shows consistently positive effects across different specifications. The first column in the table shows results using a two-way fixed effect model. In column (2), I adopt the CSDID model using both not-yet-treated as well as never-treated observations as controls. In column (3), I adopt the CSDID model using only never-treated observations as controls to avoid additional bias caused by not-yet-treated observations. In column (4), I add

[28] A regression model using the log of county-level fertility rate as outcome variable generates similar results. The main results are also robust when the state-specific time trends are included as control variables.

[29] 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.

economic control variables. Column (4) adds demographic control variables. In column (6), I exclude states that have experienced changes in abortion restrictions between 1990 and 2004. Results are quite stable across specifications.

The point estimate on the effect of banking deregulation in the preferred specification (the second column) is 0.101 and significant at a 5 percent level. Considering that maternal age is around 27 in the sample period, this estimation suggests a 0.37 percent increase in the average age or about a month's delay in birth.

To test for parallel trends and study the dynamics of treatment effects of banking deregulation, I present the event-study results in the lower panel of Table 3 and plot those results in Figure 4. As Figure 3, Figure 4 shows that the coefficients in the years prior to the introduction of banking deregulation are all close to zero and exhibit no discernible pretrends across all specifications. Figure 4 also shows that there is a sharp and persistent increase in maternal age in deregulated states that persists 5 years after the deregulation. The estimated size of these positive effects increases from 0.012 to 0.146 according to the CSDID model according to the CSDID model in column (2). Those results suggest that banking deregulation not only reduces the overall number of birth but also delay the timing of births. This result is also consistent with subsample results by age groups as presented in Figure A5 where I find the reduction of fertility is more evident among younger females (the 20-24 age group) which is also the group of females who are more likely to be down-payment constrained for purchasing houses and having children.

6.3 Diagnose of TWFE and Alternative DID Estimators

Several recent papers introduce diagnostic approaches for understanding the extent of the bias caused by TWFE DID specification issues under staggered treatment timing and treatment effect heterogeneity, with a focus on the static specification as in equation 1. First, de Chaisemartin and D'Haultfoeuille (2020) proposes calculating and reporting the number/fraction of group-time ATTs that receive negative weights. In this paper's setting, the estimate of α_{1e} in the equation 1 is a weighted sum of 3220 ATT estimates, among which, 2430 ATTs receive a positive weight, and 790 receive a negative weight. The sum of the positive weights is equal to 1.127, and the sum of the negative weights is equal to -0.127, suggesting the impact of negative weights exists but might not be substantial in this application.[30]

Moreover, Goodman-Bacon (2021) proposes a decomposition theorem and suggests reporting the weights that α_1 places on the different two-group and two-period difference-in-differences (2X2 DID), which allows one to evaluate how much weight is being placed on "forbidden" comparisons of already-treated units and how removing the comparisons would change the

[30]Those results are calculated and reported using the STATA command `twowayfeweights`.

estimate. As suggested in Goodman-Bacon (2021), I illustrate the source of variation that contributes to the TWFE estimates of α_1 (-0.002, as in column (1) of Table 2) in Figure 5.[31] Particularly, I find the weight and estimate are 0.326 and -0.007 for the 2x2 DID components where never treated counties are control groups. The weight and estimate are 0.295 and -0.001 for the 2x2 DID components where early-treated counties are treatment groups and later-treated counties are control groups. The weight and estimate are 0.304 and 0.003 for the 2x2 DID component where later-treated counties are treatment groups and early-treated counties are control groups. The weight and estimate are 0.075 and 0.000 for the 2x2 DID components where already-treated counties are control groups. Those results suggest that the two types of problematic 2x2 DID (later-treated counties are treatment groups and early-treated counties and already-treated counties are control groups) bias down the negative effects of bank deregulation on fertility rates. Those results are consistent with the findings that the negative effects I find based on the CSDID estimates are about 1.5 times larger compared with the TWFE estimates (-0.005 vs. -0.002) and suggest the adoption of CSDID as the main estimation method for more reliable results. The decomposition when the outcome variable is maternal age is similar and available upon request.

In addition to the CSDID model developed by Callaway and Sant'Anna (2021), several other recent papers have proposed various other approaches to deal with this issue. While the literature has not settled on a standard, the proposed solutions all deal with the biases arising from the bad comparison problem inherent in TWFE DID regressions by modifying the set of effective comparison units in the treatment effect estimation process. They differ in which observations are used as effective comparison units. For example, Borusyak et al. (2022) proposed an imputation estimator that imputes counterfactual outcomes for each treated unit based on the never-treated or not-yet-treated groups.[32] Sun and Abraham (2021) proposed a fully parametric regression-based estimator that estimates the full set of cohort-specific relative-time treatment effects jointly using an interacted specification that is saturated in relative time indicator and cohort indicator. This method 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 and when treatment is continuous.[33] In addition to CSDID, I also present the event study results generated by de Chaisemartin and D'Haultfoeuille (2020); Borusyak et al. (2022); Sun and Abraham (2021) as in Figure 6. The main results are largely consistent across different methods.

[31] Those results are generated using the STATA command `bacondecomp`

[32] Gardner (2022); Liu et al. (2022); Wooldridge (2003) also proposed similar methods.

[33] See Roth et al. (2022); de Chaisemartin and D'Haultfoeuille (2022) for additional discussion of the tradeoffs between the strength of these different methods in staggered treatment settings.

6.4 Heterogeneity Effects

In this section, I test whether bank branching deregulation has heterogeneous effects based on demographic characteristics. We consider two types of within-demographic group fertility rates. One is calculated based on the mother's birth cohort the other is based on the mother's race. Table 4 presents the results when the outcome is the fertility rate. I find the negative fertility effect is larger among Millennials (birth cohort 1981-1996) with an estimated coefficient of -0.008 compared to Generation X (birth cohort 1965-1980) with an estimated coefficient of -0.004. This negative effect is evident among all racial groups, though the coefficient is larger among Hispanic females (-0.009) compared to non-Hispanic white and black females (-0.005 and -0.004). Additionally, I explore the fertility rate by mother's marital status as well as the birth order of the child using the total number of females ages 15-44 as the denominator. I find the decline of fertility is larger among females who are not married compared to those who are married (-0.004 vs.-0.001). At the same time, fertility rates have equally decreased for the first, second, and higher order of birth which suggests the negative effect is driven by a combination of a decline in initial childbearing (first births) as well as by women not having more number of children (third and higher order births).[34] The event study plots of the dynamic effects are presented in Figure A6.

Table 5 presents heterogeneous results when the outcome is maternal age. I find the positive effect on maternal age is larger among Generation X (birth cohort 1965-1980) compared to Millennials (birth cohort 1981-1996) but is not significant among the two groups. These results do not necessarily imply the delay of fertility is not evident among Millennials since they are around their 20s in our analysis periods and the delay of fertility may not be captured yet. This positive effect is larger among Hispanic females (0.129) and non-Hispanic white (0.119) compared to non-Hispanic black females (0.082). I also find the increase in maternal age is larger among females who are not married compared with those who are married (0.161 vs.-0.029). Finally, I calculate the maternal age by birth order and find that maternal age has increased more among second (0.104) and higher-order birth (0.19) compared with the first birth (0.065). The event study plots of the dynamic effects are presented in Figure A7.

7 Discussion of Mechanisms

Why does the increase in credit supply reduce and delay fertility? The theory points to two plausible explanations for why an increase in credit supply reduces and delays fertility. The first is the housing cost effect, which suggests that local credit supply expansion reduces and delays fertility by increasing local house costs, which contribute to a large proportion of child-

[34] There is no clear evidence of the increase of childlessness caused by credit supply expansion.

bearing costs (Simon and Tamura, 2009; Dettling and Kearney, 2011). The second is a labor market effect, which suggests that local credit supply expansion reduces and delays fertility by stimulating the local economy and job growth, which increases the motherâs opportunity costs. To test these two different mechanisms, I conduct the following empirical analyses.

7.1 Housing Cost Effect: Results by Land Availability

First, 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).[35] The idea is house price responses caused by the increase in credit supply should be most pronounced in counties where the housing supply is inelastic and muted in elastic areas, where the stock of housing increased instead. Thus, if the housing market channel is important, we should observe a more evident reduction and delay of fertility in areas with less developable land.

Columns (1) and (2) in Table 6 show that bank branching deregulation reduces the fertility rate by 0.009 percentage points in counties with less developable land and only by 0.003 percentage points in counties with more land. Similarly, columns (3) and (4) show that the effect of bank branching deregulation on maternal age is also much larger in counties with less developable land than in counties with more land (0.164 vs.0.084). Figure 8 panels a) and b) plot the dynamic effects of bank branching deregulation on the fertility rate and maternal age in counties with less and more land and confirms the stronger effects in counties with less land. Importantly, across all three sets of results, there are no pre-trends in the years leading up to bank branching deregulation.

Additionally, I estimate the effect of bank deregulation on county-level house prices using a similar DID design where housing prices are measured at the county level using the Federal Housing Finance Agency (FHFA) house price index. Columns (5) and (6) in Table 6 show that bank branching deregulation increases house prices at the county level and this effect is much stronger and significant in areas with less land (0.042 and 0.05).[36] Those results suggest a strong connection between interstate bank branching expansion, rising house prices, and the decline

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

[36] 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. 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, the results in this paper complement the existing results by refining the connection between bank deregulation and local house price. Following Favara and Imbs (2015), I also test the effect of the banking deregulation on the number of mortgage loans originating at the county level with the Home Mortgage Disclosure Act (HMDA) database and confirm the positive connection between deregulation and the increase of the number of loans. Those results are presented in Appendix Figure A8.

and delay of fertility.

7.2 Housing Cost Effect: Results by Local Homeownership Rate

Theoretically, the housing cost effect is expected to be stronger among renters. However, the Natality birth data lack information on the housing tenure status of mothers, so it is not possible to estimate the effects of bank deregulation on renters and homeowners separately. Instead, as in Dettling and Kearney (2014), I test whether the negative effects of bank deregulation on fertility are stronger in counties with lower rates of homeownership. A county is defined as having a lower homeownership rate if its rate in 1990 is lower than the median rate as reported in the census. Table 7 shows that the negative effects of bank deregulation on fertility are greater in areas with a lower rate of homeownership rate in 1990, but the increase in maternal age is slightly greater in areas with a higher rate of homeownership rate. I interpret those results with caution since bank deregulation can have a direct impact on homeownership decisions.[37]

7.3 Housing Cost Effect: Instrumental Variable Results

To further confirm the housing cost channel, I show that bank branching deregulation constitutes a legitimate instrument for the independent variable of fertility outcomes on house price growth at the county level. In an instrumental variable sense, branching deregulation can account for the rise of housing prices and can explain a significant share of the resulting decline in the fertility rate and the increase in maternal age. The instrumental variable results are presented in Table 8 and show that a one percent increase in log change of house price decreases the fertility rate by about 2 percentage points after the deregulation which is equivalent to a reduction of 3 percent in fertility rate or a reduction of the number of births by 2 per 1000 women aged 15-44. Meanwhile, a one percent increase in log change of house price increases maternal age by 0.047 which is equivalent to an increase of 1.5 percent in maternal age. The F-statistics range from 43.43 to 9.35, indicating that the instrument is not weak. Those results confirm rising housing prices as an important channel for explaining the decrease and delay in fertility.

7.4 Labor Market Effect

To test the labor market channel, I show how bank branching deregulation affects county-level growth in employment wages measured in the Quarterly Census of Employment and Wages (QCEW). I find that bank deregulation has no significant effects on labor market outcomes at the aggregate level, which excludes the labor market channel (Appendix Figure 1). I also

[37] I have another ongoing working paper that studies the impact of bank deregulation on homeownership and housing wealth accumulation.

explored the growth in employment and wages by different industries and found that none of those outcomes are significant.

7.5 Additional Results using the SIPP

As mentioned earlier, one limitation of the Natality birth data is that it lacks information on the housing tenure status (Dettling and Kearney, 2014). So, it is difficult to directly test the role of housing tenure and to decompose different mechanisms. In this section, to complement the main results, I use the Survey of Income Program and Participation (SIPP) 1990-2004 panels to further explore the effect of bank branching deregulation on fertility and the underline mechanisms at the individual level. The SIPP is a national-level household survey that contains detailed information on the timing of homeownership and basic demographic information which makes it possible to explore the role of homeownership on the connection of bank branching deregulation on fertility outcomes.

Particularly, to measure fertility, I select females of age 15-44 in the sample and construct a fertility dummy indicating whether this female has given birth to a child in a given year. Considering the important role of housing tenure, I construct a dummy variable of homeowner that takes the value 1 if the individual owned a house and has purchased this house before 1994 (before the bank branching deregulation took place) and 0 otherwise.[38] The construction of this variable is feasible in the SIPP since it provides not only current housing tenure information in the core module but also information on house purchase years in the topical module. The SIPP also provides other key demographic and household characteristics such as age, race and ethnicity, educational attainment, and marital status. Observations with missing information on fertility or homeownership variables are dropped. The final sample contains 161795 observations for the periods of 1990-2007.

Table 9 column (1) reports the effect of bank branching deregulation on fertility and shows a negative effect consisting of the main results based on the Natality birth data. In columns (2) and (3), I divide the sample into homeowners and renters where homeownership status is measured in 1994. I find the negative effect on fertility is concentrated among renters. On the contrary, the fertility effect among homeowners is negligible. Those results suggest negative fertility effects are largely driven by the "housing cost 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

[38] I use the housing tenure status before the bank branching deregulation instead of the current housing tenure considering that housing tenure is an endogenous choice that could be affected by credit supply. Indeed, in another working paper, I explore the effect of credit supply on homeownership using the bank branching deregulation as exogenous variations and find that credit supply reduces homeownership and increases housing wealth among homeowners who have purchased a house before 1994.

negative effect to some extent. Overall, the housing cost effect dominates and leads to a net negative effect. Those results are consistent with our main results based on the Natality birth data and also consistent with empirical evidence provided in Dettling and Kearney (2014) which finds MSA-level house prices reduced the fertility rate among renters but not among homeowners. Columns (4) and (5) further explore the effect of bank branching deregulation on female labor supply and find no significant impact which is consistent with the county-level results and suggests that the labor market channel can not be the major driving force behind the connection between bank branching deregulation and fertility.

8 Conclusion

This paper proposes a new explanation for declining fertility rates. I find that bank credit expansion decreases annual county-level fertility rates by 7% and increases the average maternal age by 0.37%, after addressing the endogeneity issue of credit expansion using the U.S. interstate branching deregulation that occurred between 1994 and 2005. I also show that the decrease and delay in fertility are mainly caused by a housing cost effect. From a normative perspective, the results suggest that the current fertility rate is suboptimal and that the decline in fertility rates may be a negative development, as women may prefer to have more children but are discouraged by increasing housing costs or have to wait longer until they are financially prepared

These results reveal a meaningful connection between housing market dynamics and demographic trends which are rarely discussed in the literature. Recently, many developed countries have experienced declined and historically unprecedented low fertility rates. In Germany, Italy, Japan, and Spain fertility has been well below 1.5 for more than two decades which is lower than the average of just over two children per woman needed to maintain a stable population size. Low and declining fertility implies dramatic population aging and rapid population decline, which are linked to numerous economic and social policy concerns such as slower economic growth and larger financial pressure on social insurance programs. Results in this paper provide important policy implications and suggest a critical role of housing affordability in addressing the issue of declining fertility.

Several avenues for future research are worth considering. First, the credit expansion that is studied in this paper covers the time period of 1990 to 2005 when the overall fertility rate is relatively stable which hides away large geographic variations in the trends of fertility rates. The results suggest that fertility rates could have been higher without the rising housing prices caused by bank credit expansion. This also implies that rising housing costs may have played a role in the decline of fertility after the 2008 recession. Future research could explore the long-term effect of the credit supply in the 1990s or explore more recent credit supply policy variations to directly address the puzzle of falling birth rates since 2008 as proposed in Kearney et al. (2022). Moreover, since the overall negative effect might be mitigated by the housing wealth and relaxed liquidity-constraint effects, the net negative housing cost effect on fertility could be larger than what has been estimated in this paper. It would be interesting to further investigate and better estimate the magnitude of those effects separately.

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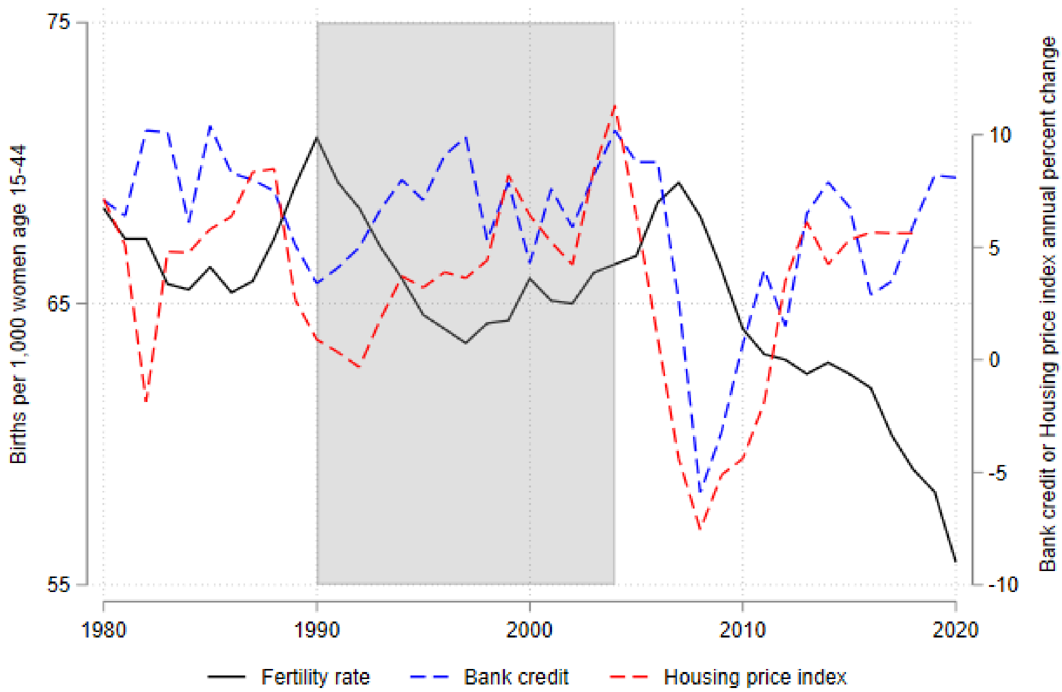
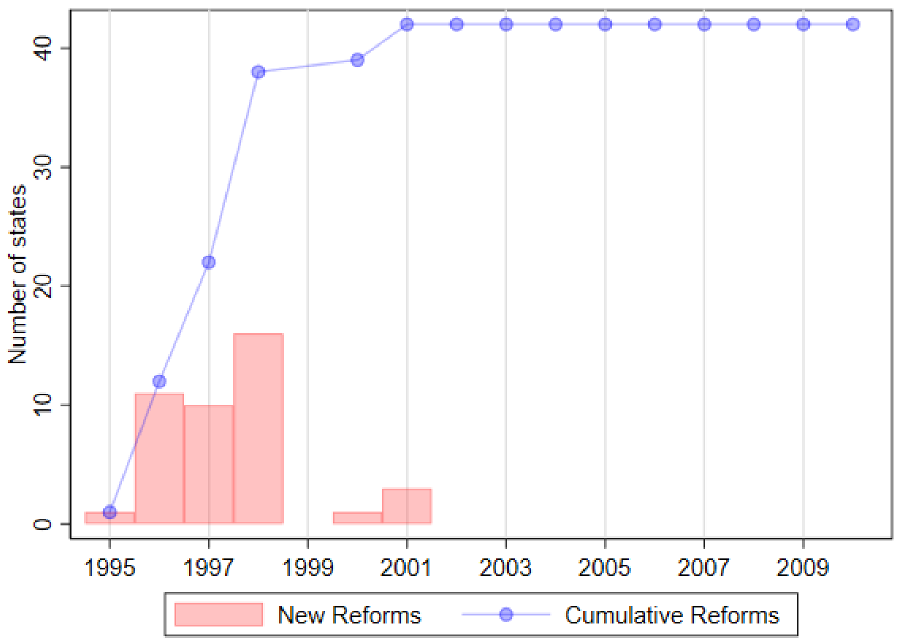
Figures and Tables

Figure 1: National Birth Rate, Bank Credit, and Housing Price, 1980-2020

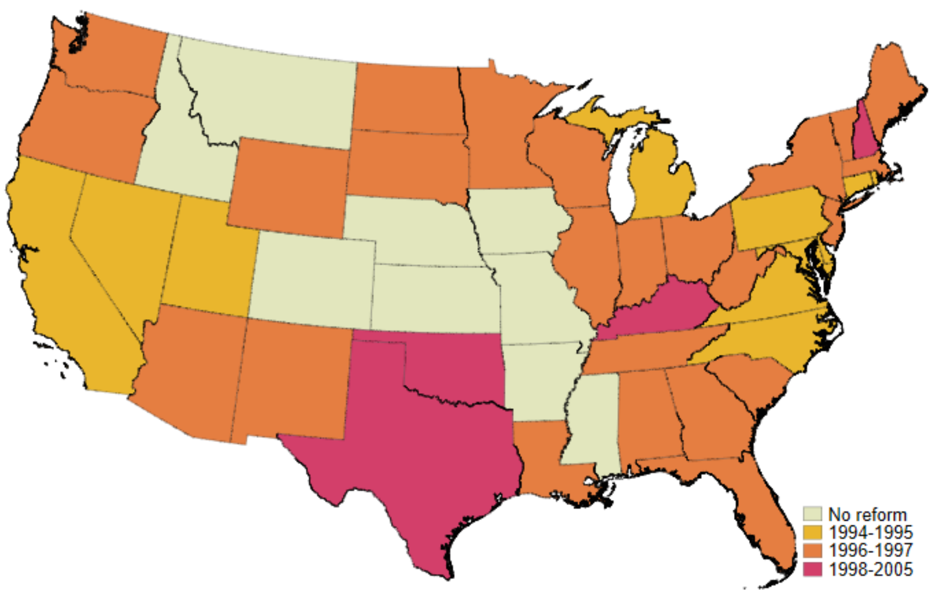


Notes: The birth rates (black solid line) are calculated as the number of births per thousand women ages 15-44 (CDC National Vital Statistics Reports) divided by the yearly population of women ages 15-44 (National Center for Health Statistics). The national-level annual percent change of bank credit supply comes from Federal Reserve Economic Data and covers all commercial banks in the U.S.. The national-level annual percent change of home prices (red dashed line) comes from the Federal Housing Finance Agency (FHFA). The shadow area covers the sample analysis period of the paper which is between 1990 and 2004.

Figure 2: Timing of Interstate Bank Branching Deregulation



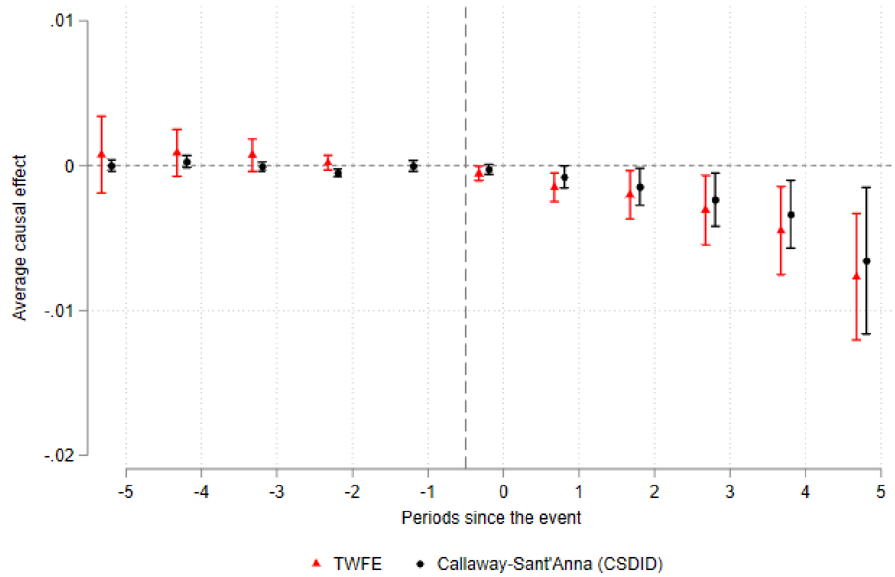
(a) Accumulated number of deregulated states



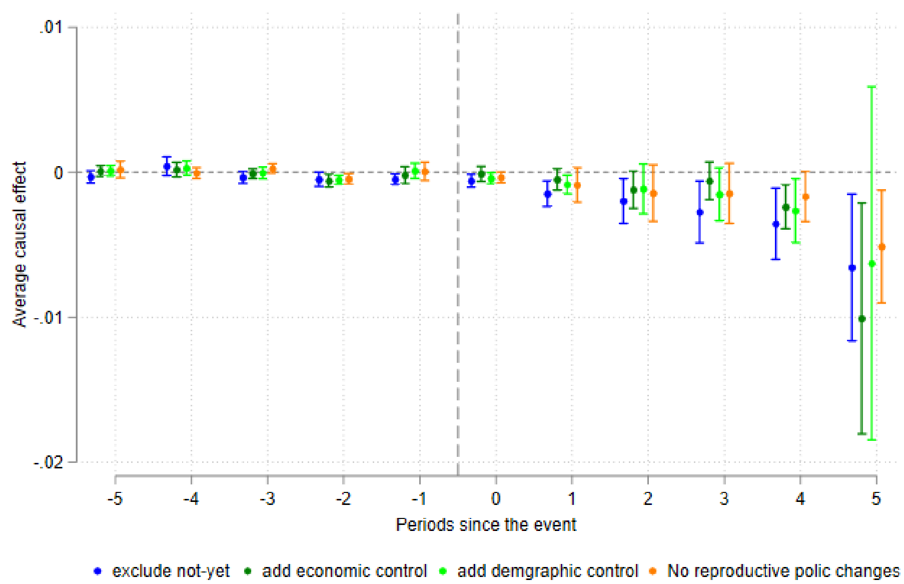
(b) Timing of deregulation by state

Notes: Data on interstate branching deregulation by state and by year come from Rice and Strahan (2010). Nine states never deregulated which include Arkansas, Colorado, Idaho, Iowa, Kansas, Mississippi, Missouri, Montana, and Nebraska.

Figure 3: Bank Branching Deregulation and Fertility Rate



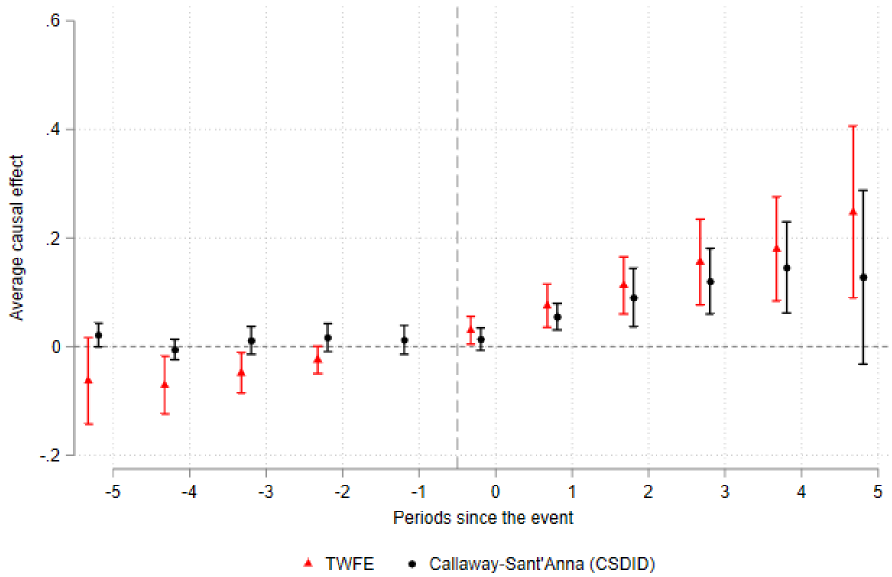
(a) TWFE and CSDID



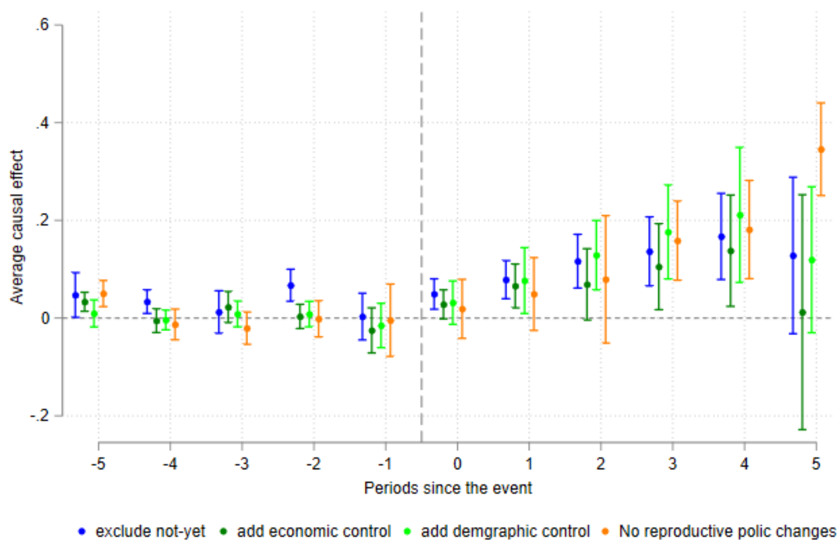
(b) CSDID: Robustness Checks

Notes: This figure plot the effects of the interstate bank branching deregulation on the county-level fertility rate calculated based on the Vital Statistics Natality Files (449 counties between 1990 and 2004). The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. In panel (b), economic controls include the state unemployment rate, the state minimum wage, the generosity of welfare benefits, and child support enforcement expenditure. demographic controls include county-level population shares of women ages 15-29, women ages 30-44, non-Hispanic white women ages 15-44, non-Hispanic black women ages 15-44, and Hispanic women ages 15-44. Reproduction policies include abortion restrictions through parental notification laws or waiting periods. Standard errors are clustered at the state level. All Figures show 95 percent confidence intervals.

Figure 4: Bank Branching Deregulation and Maternal Age



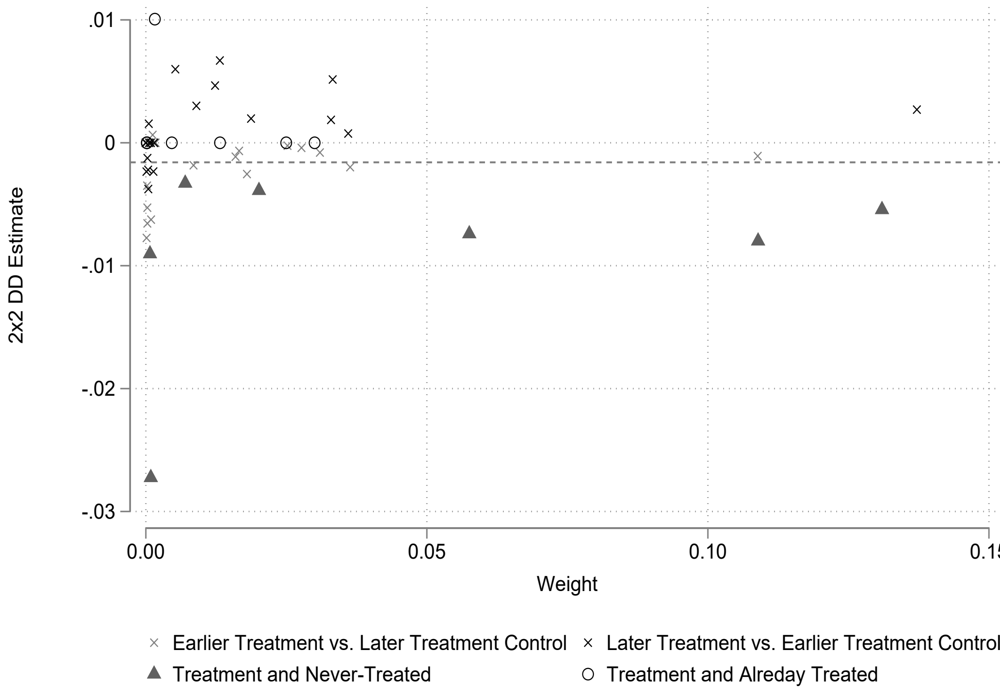
(a) TWFE and CSDID



(b) CSDID: Robustness Checks

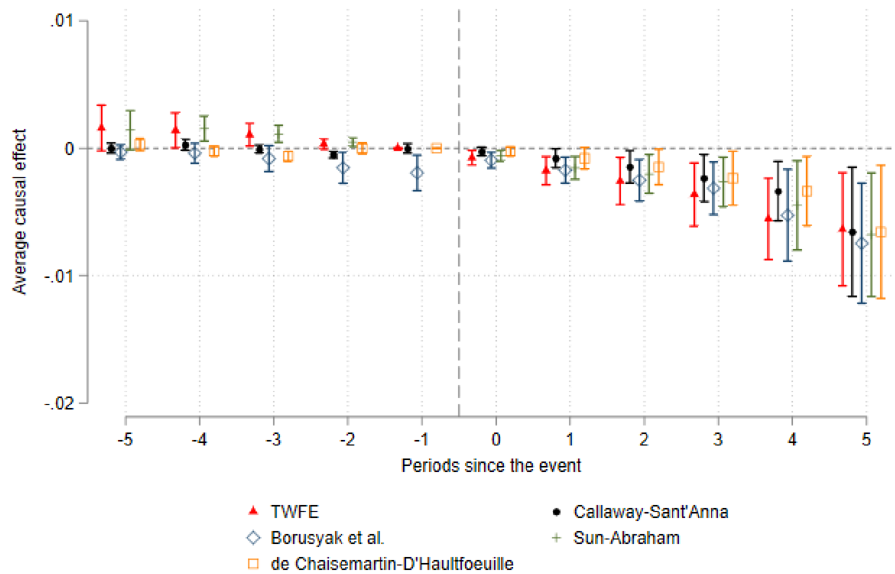
Notes: This figure plot the effect of the interstate bank branching deregulation on the county-level average of maternal age calculated based on the Vital Statistics Natality Files (449 counties between 1990 and 2004). The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. In panel (b), economic controls include the state unemployment rate, the state minimum wage, the generosity of welfare benefits, and child support enforcement expenditure. demographic controls include county-level population shares of women ages 15-29, women ages 30-44, non-Hispanic white women ages 15-44, non-Hispanic black women ages 15-44, and Hispanic women ages 15-44. Reproduction policies include abortion restrictions through parental notification laws or waiting periods. Standard errors are clustered at the state level. All Figures show 95 percent confidence intervals.

Figure 5: Bacon Decomposition

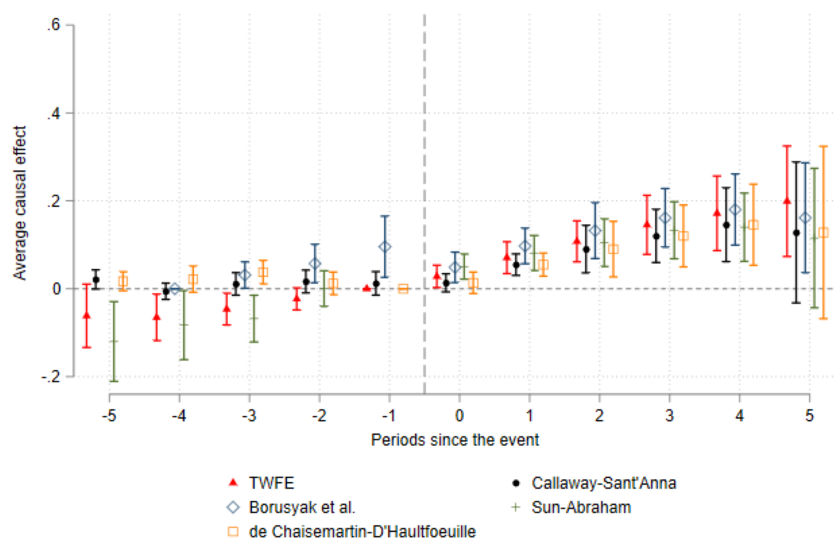


Notes: The figure plots each 2x2 DID component from the decomposition theorem against their weight for the deregulation dummy indicating whether the state has implemented interstate bank branching deregulation. The weight and estimates are 0.326 and -0.007 for the 2x2 DID component where never treated counties are control groups; The weight and estimates are 0.295 and -0.001 for the 2x2 DID component where early-treated counties are treatment groups and later-treated counties are control groups; The weight and estimates are 0.304 and 0.003 for the 2x2 DID component where later-treated counties are treatment groups and early-treated counties are control groups; The weight and estimates are 0.075 and 0.000 for the 2x2 DID component where already treated counties are control groups. The figure notes the average DID estimate and the total weight on each type of comparison. The two-way fixed effects estimate, -0.002, equals the average of the y-axis values weighted by their x-axis value. The outcome variable in this decomposition is the fertility rate. The decomposition when the outcome variable is material age is similar and available upon request.

Figure 6: Bank Branching Deregulation, Fertility Rate, and Maternal Age: Different Estimators



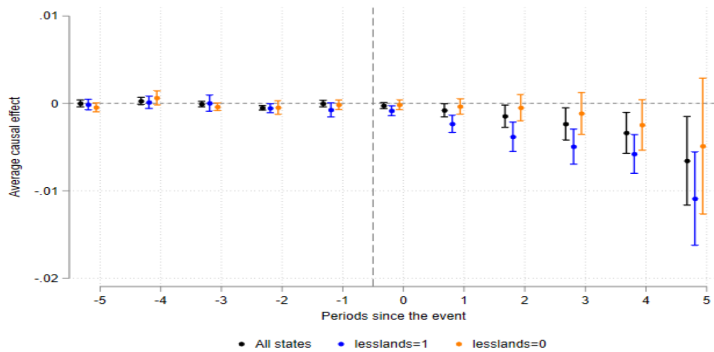
(a) Fertility Rate



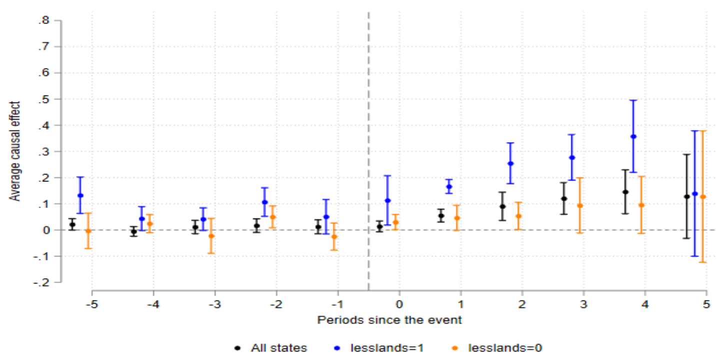
(b) Maternal Age

Notes: This figure overlays the event-study plots constructed using five different estimators: a dynamic version of the TWFE model was estimated using OLS (in red with triangle markers); Callaway and SantâAnna (2021) (in black with dot markers); Borusyak, Jaravel, and Spiess (2021) (in navy with diamond markers); Sun and Abraham (2021) (in green with triangle markers); and De Chaisemartin and dâHaultfoeuille (2020) (in orange with square markers). The outcome in panel (a) is the county-level fertility rate and the outcome in panel (b) is the county-level average of maternal age. Both outcome variables are calculated based on the Vital Statistics Natality Files (449 counties between 1990 and 2004). The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. Standard errors are clustered at the state level. All Figures show 95 percent confidence intervals.

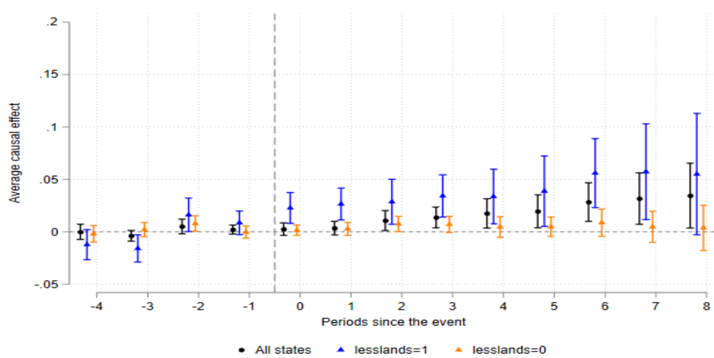
Figure 7: Mechanism: Effect of Bank Branching Deregulation on Fertility Rate, Maternal Age, and House Price by Land Availability



(a) Fertility Rate



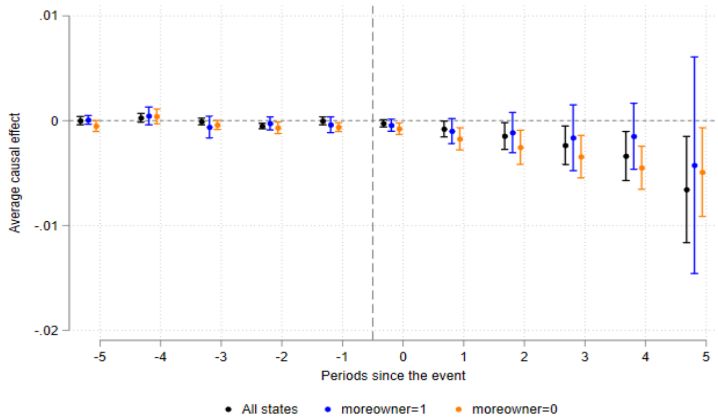
(b) Maternal Age



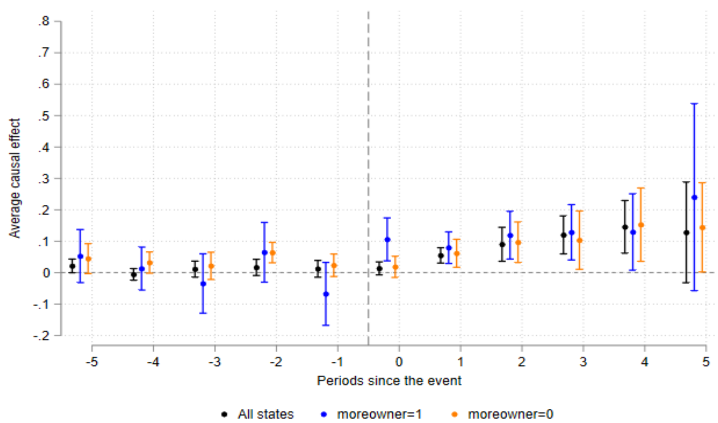
(c) House Price

Notes: Counties of $lessland = 1$ (blue dots) and $lessland = 0$ (orange dots) are defined as counties with developable land that are less or more than 70% of the total areas based on satellite data collected by Lutz and Sand (2019). House price is measured as the log change in the FHFA house price index at the county level. All Figures are event studies based on the CSDID estimates and show 95 percent confidence intervals. Standard errors are clustered at the state level.

Figure 8: Mechanism: Effect of Bank Branching Deregulation on Fertility Rate, Maternal Age, and House Price by Homeownership



(a) Fertility Rate



(b) Maternal Age

Notes: Counties of *moreowner* = 1 (blue dots) and *moreowner* = 0 (orange dots) are defined as counties with homeownership rates in 1990 below and above the median rate based on the census data. All Figures are event studies based on the CSDID estimates and show 95 percent confidence intervals. Standard errors are clustered at the state level.

Table 1: Summary Statistics

	Treated and Control				Different Treatment Groups		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Total	Never Treated	Treated	Diff.	Treated 1996	Treated 1997	Treated 1998
Mothers in Natality Data							
Age	26.92	26.29	26.98	-0.69***	27.25	27.50	26.64
No-Hispanic White	0.61	0.75	0.59	0.16***	0.55	0.65	0.61
No-Hispanic Black	0.15	0.14	0.15	-0.02***	0.13	0.12	0.18
Hispanic	0.18	0.08	0.19	-0.11***	0.24	0.16	0.16
Less than HS	0.22	0.19	0.22	-0.03***	0.23	0.18	0.22
HS	0.33	0.34	0.33	0.01***	0.32	0.32	0.35
College	0.35	0.39	0.34	0.04***	0.34	0.37	0.34
Graduate	0.08	0.07	0.08	-0.00***	0.08	0.08	0.07
Not Married	0.32	0.30	0.32	-0.02***	0.32	0.31	0.32
Observations	51829572						
County-Year Sample							
Fertility Rate	0.06	0.07	0.06	0.00***	0.06	0.06	0.07
Maternal Age	27.08	26.69	27.11	-0.43***	27.32	27.63	26.78
Unemployment Rate	5.47	4.77	5.52	-0.75***	5.59	5.41	5.51
Minimum Wage	4.65	4.39	4.67	-0.27***	4.80	4.96	4.48
Welfare Benefit	0.70	0.64	0.70	-0.06***	0.76	0.74	0.65
CS Enforcement Exp	190.49	59.60	200.96	-141.37***	284.59	109.59	174.51
Abortion Parental	0.55	0.77	0.53	0.24***	0.54	0.48	0.54
Abortion Wait	0.24	0.56	0.22	0.34***	0.25	0.21	0.20
Less Land	0.64	0.50	0.65	-0.15***	0.71	0.83	0.54
Observations	6735	499	6236		2132	1074	3030

Note: Natality sample comes from the Vital Statistics Natality Files which covers 449 counties between 1990 and 2004. County-year fertility rate and maternal age are calculated based on the Natality sample. Unemployment rates, minimum wage, the generosity of welfare benefits, and child support enforcement expenditure are measured at the state level. The generosity of welfare benefits measures the monthly maximum TANF benefit for a family of three and is measured in thousands of dollars. The child support enforcement expenditure measures the total annual expenditure of the state on programs that help noncustodial parents to pay for the financial support of their children and is measured in millions of dollars. Less land measures whether the county-level developable land is less than 70% of the total area based on satellite data collected by Lutz and Sand (2019). Treatment indicates whether the state implemented interstate bank branching deregulation. Treated 1996 includes states where deregulation takes place in 1996 or before; Treated 1997 includes states where deregulation takes place in 1997; Treated 1998 includes states where deregulation takes place in 1998 or after.

Table 2: Bank Branching Deregulation and Fertility Rate:
Main Effect

	TWFE	CSDID				
	(1)	(2)	(3)	(4)	(5)	
Deregulation Dummy	-0.001***	-0.005***	-0.005***	-0.003**	-0.005**	-0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Event Study:						
Lead5event	0.001	0.000	-0.000	0.000	0.000	0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Lead4event	0.001	0.000	0.000	0.000	0.000	-0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Lead3event	0.001	-0.000	-0.000*	-0.000	-0.000	0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Lead2event	0.000	-0.001***	-0.000*	-0.001***	-0.001***	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Lag0event	-0.001**	-0.000	-0.001***	-0.000	-0.000**	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Lag1event	-0.001***	-0.001**	-0.001***	-0.000	-0.001***	-0.001
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Lag2event	-0.002**	-0.001**	-0.002**	-0.001*	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Lag3event	-0.003**	-0.002**	-0.003**	-0.001	-0.002	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Lag4event	-0.005***	-0.003***	-0.004***	-0.002***	-0.003**	-0.002*
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Lag5event	-0.008***	-0.007**	-0.007**	-0.010**	-0.006	-0.005***
	(0.002)	(0.003)	(0.003)	(0.004)	(0.006)	(0.002)
Observations	6735	6735	6735	6735	6735	4,051
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	No	No	No	Yes	Yes	Yes
Policy Controls	No	No	No	No	Yes	Yes

Note: This table studies the effect of the interstate bank branching deregulation on the county-level fertility rate, calculated based on the Vital Statistics Natality Files (449 counties between 1990 and 2004). The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. Column (2) excludes non-yet-treated control groups. Column (3) adds economic control variables including unemployment rates, minimum wage, the generosity of welfare benefits, and child support enforcement expenditure are measured at the state level. Column (4) adds demographic control variables including share of county-level population shares of women ages 15-29, women ages 30-44, non-Hispanic white women ages 15-44, non-Hispanic black women ages 15-44, and Hispanic women ages 15-44. Column (5) excludes states that have experienced changes in abortion restrictions in the form of parental notification laws or waiting between 1990 and 2004. Standard errors are clustered at the state level.* p<0.10, ** p<0.05, *** p<0.01.

Table 3: Bank Branching Deregulation and and Maternal Age:
Main Effect

	TWFE	CSDID				
	(1)	(2)	(3)	(4)	(5)	(6)
Deregulation Dummy	0.087***	0.101**	0.118**	0.060*	0.132***	0.228***
	(0.029)	(0.045)	(0.046)	(0.036)	(0.036)	(0.036)
Event Study:						
Lead5event	-0.063	0.021*	0.047**	0.033***	0.009	0.050***
	(0.041)	(0.011)	(0.023)	(0.010)	(0.014)	(0.014)
Lead4event	-0.071**	-0.006	0.034***	-0.005	-0.004	-0.013
	(0.027)	(0.009)	(0.012)	(0.012)	(0.010)	(0.016)
Lead3event	-0.049**	0.011	0.012	0.023	0.008	-0.021
	(0.019)	(0.013)	(0.022)	(0.016)	(0.013)	(0.017)
Lead2event	-0.025*	0.017	0.067***	0.003	0.008	-0.002
	(0.013)	(0.013)	(0.017)	(0.013)	(0.013)	(0.019)
Lag0event	0.030**	0.013	0.049***	0.028*	0.032	0.019
	(0.013)	(0.011)	(0.016)	(0.015)	(0.023)	(0.031)
Lag1event	0.075***	0.055***	0.079***	0.066***	0.077**	0.049
	(0.020)	(0.012)	(0.020)	(0.023)	(0.034)	(0.038)
Lag2event	0.113***	0.090***	0.116***	0.069*	0.129***	0.079
	(0.027)	(0.028)	(0.028)	(0.037)	(0.036)	(0.067)
Lag3event	0.155***	0.120***	0.136***	0.105**	0.176***	0.158***
	(0.040)	(0.031)	(0.036)	(0.045)	(0.049)	(0.041)
Lag4event	0.180***	0.146***	0.167***	0.138**	0.211***	0.181***
	(0.049)	(0.043)	(0.045)	(0.058)	(0.070)	(0.051)
Lag5event	0.248***	0.128	0.128	0.012	0.119	0.346***
	(0.081)	(0.082)	(0.082)	(0.123)	(0.076)	(0.048)
Observations	6735	6735	6735	6735	6735	4,051
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	No	No	No	Yes	Yes	Yes
Policy Controls	No	No	No	No	Yes	Yes

Note: This table studies the effect of the interstate bank branching deregulation on the county-level average of maternal age calculated based on the Vital Statistics Natality Files (449 counties between 1990 and 2004). The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. Column (2) excludes non-yet-treated control groups. Column (3) adds economic control variables including unemployment rates, minimum wage, the generosity of welfare benefits, and child support enforcement expenditure are measured at the state level. Column (4) adds demographic control variables including share of county-level population shares of women ages 15-29, women ages 30-44, non-Hispanic white women ages 15-44, non-Hispanic black women ages 15-44, and Hispanic women ages 15-44. Column (5) excludes states that have experienced changes in abortion restrictions in the form of parental notification laws or waiting between 1990 and 2004. Standard errors are clustered at the state level.* p<0.10, ** p<0.05, *** p<0.01.

Table 4: Bank Branching Deregulation and Fertility Rate:
Heterogeneous effect

	Mother Birth Cohort		Race and Ethnicity		
	1965-1980	1981-1996	No-His White	No-His Black	Hispanic
	(1)	(2)	(3)	(4)	(5)
Deregulation Dummy	-0.004**	-0.008***	-0.005***	-0.004**	-0.009***
	(0.002)	(0.003)	(0.001)	(0.002)	(0.004)
Observations	6735	6735	6735	6735	6735
	Marital Status		Birth Order		
	Not Married	Married	1st birth	2nd birth	more
	(6)	(7)	(8)	(9)	(10)
Deregulation Dummy	-0.004***	-0.001***	-0.002***	-0.002***	-0.002***
	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Observations	6735	6735	6735	6735	6735
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes
Policy Controls	Yes	Yes	Yes	Yes	Yes

Note: This table studies the heterogeneous effect of the interstate bank branching deregulation on the county-level fertility rate which is calculated based on the Vital Statistics Natality Files (449 counties between 1990 and 2004). The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. Each regression adopts the CSDID model and includes economic and policy controls such as state-level unemployment rates, minimum wage, and abortion restrictions in the form of parental notification laws or waiting periods. Standard errors are clustered at the state level.* p<0.10, ** p<0.05, *** p<0.01.

Table 5: Bank Branching Deregulation and Maternal Age:
Heterogeneous effect

	Mother Birth Cohort		Race and Ethnicity		
	1965-1980	1981-1996	No-His White	No-His Black	Hispanic
	(1)	(2)	(3)	(4)	(5)
Deregulation Dummy	0.104	-0.013	0.119**	0.082	0.129
	(0.068)	(0.018)	(0.054)	(0.075)	(0.156)
Observations	6735	6735	6735	6735	6735
	Marital Status		Birth Order		
	Not Married	Married	1st birth	2nd birth	more
	(6)	(7)	(8)	(9)	(10)
Deregulation Dummy	0.161***	-0.029	0.065	0.104**	0.190***
	(0.053)	(0.074)	(0.057)	(0.045)	(0.044)
Observations	6735	6735	6735	6735	6735
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes
Policy Controls	Yes	Yes	Yes	Yes	Yes

Note: This table studies the heterogeneous effect of the interstate bank branching deregulation on the county-level fertility rate which is calculated based on the Vital Statistics Natality Files (449 counties between 1990 and 2004). The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. Each regression adopts the CSDID model. Standard errors are clustered at the state level.* p<0.10, ** p<0.05, *** p<0.01.

Table 6: Mechanism: Effect of Bank Branching Deregulation on Fertility Rate, Maternal Age, and House Price by Land Availability

	Fertility Rate		Maternal Age		House Price	
	Less Land	More Land	Less Land	More Land	Less Land	More Land
	(1)	(2)	(3)	(4)	(5)	(6)
Deregulation Dummy	-0.009***	-0.003*	0.164***	0.084	0.042***	0.005
	(0.001)	(0.002)	(0.055)	(0.058)	(0.015)	(0.004)
Observations	6735	6735	6735	6735	25755	25755
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Policy Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table studies the effect of the interstate bank branching deregulation on the county-level fertility rate and average maternal age calculated based on the Vital Statistics Natality Files (449 counties between 1990 and 2004) by county-level land availability. The outcome variable in columns (5) and (6) is the log change in the FHFA house price index at the county level (1717 counties between 1990 and 2005). The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. Standard errors are clustered at the state level. Counties of $lessland = 1$ and $lessland = 0$ are defined as counties with developable land that are less or more than 70% of the total areas based on satellite data collected by Lutz and Sand (2019). Each regression adopts the CSDID model. * p<0.10, ** p<0.05, *** p<0.01.

Table 7: Mechanism: Effect of Bank Branching Deregulation on Fertility Rate and Maternal Age by Homeownership Rate

	Fertility Rate		Maternal Age	
	High Rate	Low Rate	High Rate	Low Rate
	(1)	(2)	(3)	(4)
Deregulation Dummy	-0.000	-0.005***	0.192*	0.112**
	(0.002)	(0.001)	(0.107)	(0.044)
Observations	6735	6735	6735	6735
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes
Policy Controls	Yes	Yes	Yes	Yes

Note: This table studies the effect of the interstate bank branching deregulation on the county-level fertility rate and average maternal age calculated based on the Vital Statistics Natality Files (449 counties between 1990 and 2004) by county-level homeownership rate. The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. Standard errors are clustered at the state level. Counties with higher and lower homeownership rates are defined as whether the county's homeownership rate in 1990 is higher or lower than the median rate as reported in the census. Each regression adopts the CSDID model. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 8: Mechanism Test: Effect of House Price on Fertility Rate, Maternal Age
(Bank Branching Deregulation as IV)

	Fertility Rate				Maternal Age			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
House Price	-0.041***	-0.037***	-0.105***	-0.190**	1.262***	0.993***	3.044***	4.711*
	(0.007)	(0.008)	(0.032)	(0.092)	(0.221)	(0.221)	(1.029)	(2.696)
R^2	0.60	0.60	-0.03	-0.19	0.97	0.97	-0.02	-0.11
Efficient F			43.43	9.35			43.43	9.35
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	No	Yes	No	Yes	No	Yes	No	Yes
Policy Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	6137	6137	6137	6137	6137	6137	6137	6137

Note: This table presents the second stage county-level linear regression of an IV specification of the fertility rate and maternal age on the log change in house prices which is instrumented with the interaction of interstate bank branching deregulation dummy and land availability. The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01.

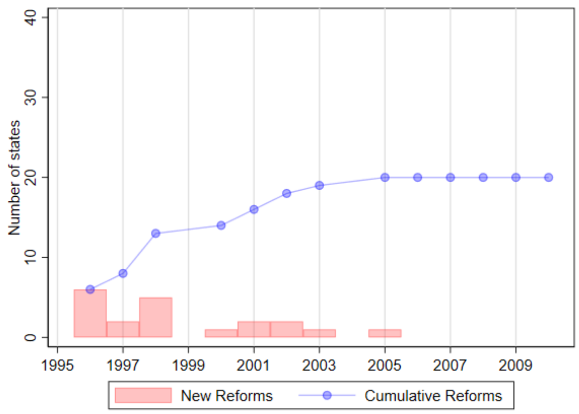
Table 9: Mechanism: additional Results using the SIPP

	Fertility			Labor Supply	
	Total Sample	Renter	Homeowner	Not in LF	Hours Worked
	(1)	(2)	(3)	(4)	(5)
Deregulation Dummy	-0.006*	-0.008**	-0.001	-0.004	-0.237
	(0.003)	(0.003)	(0.004)	(0.009)	(0.622)
Observations	161795	51966	109829	161795	98398
R^2	0.018	0.023	0.017	0.027	0.024
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Ind and HH controls	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes
Policy Controls	Yes	Yes	Yes	Yes	Yes

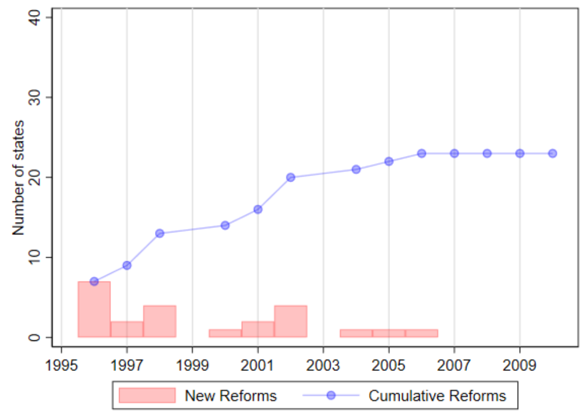
Note: The SIPP sample comes from the SIPP 1990, 1991, 1992, 1993, 1996, 2001, and 2004 panels which consist of females ages 15 and 44. The outcome variable in columns (1)-(3) is a dummy variable indicating whether the female gives birth to a child that year. Column (1) includes the total sample; Column (2) includes females who have not become homeowners before 1994; Column (1) includes females who have become homeowners before 1994. The outcome variable in columns (4)-(5) measures female labor supply and indicates whether she participates in the labor force and what is usually hours worked per month. The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. Each regression adopts the CSDID model and includes individual and household controls such as age, education levels, and marital status as as well as economic and policy controls such as state-level unemployment rates, minimum wage, and abortion restrictions in the form of parental notification laws or waiting periods. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Tables and Figures

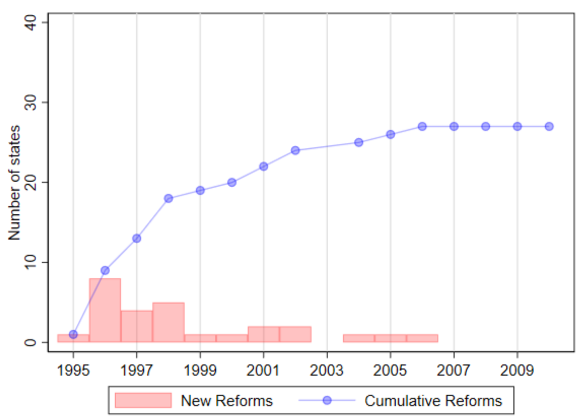
Figure A1: Timing of Bank Interstate Branching Deregulation:
Four Types of Deregulation



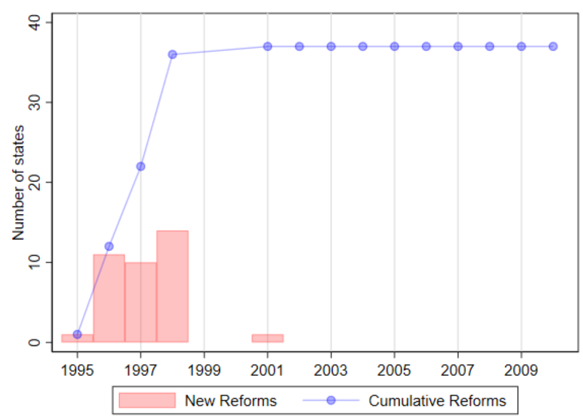
(a) Minimum Age of Targeted Banks to be 3 Years or Fewer



(b) Allow de novo Interstate Branching



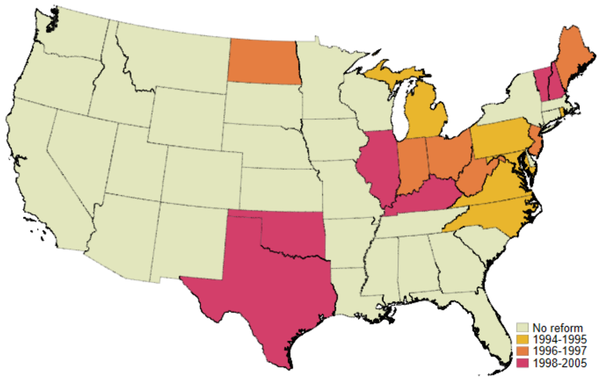
(c) Allow Acquisition of Individual Branch without Acquiring the Entire Bank



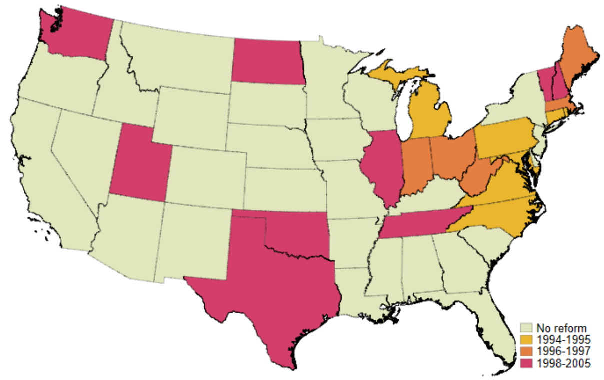
(d) Statewide Deposit Cap to be 30% or Higher

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

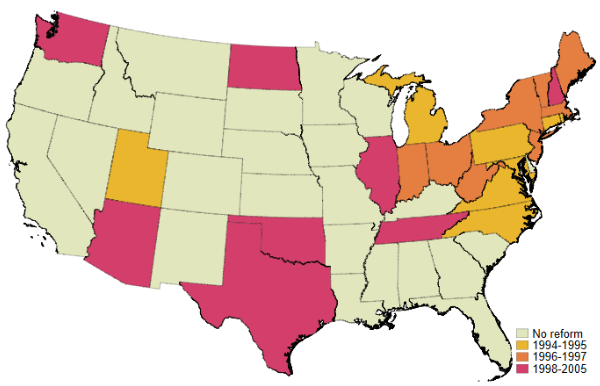
Figure A2: Timing of Bank Interstate Branching Deregulation by State:
Four Types of Deregulation



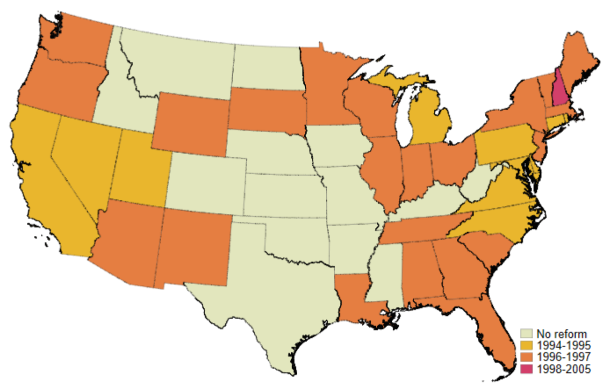
(a) Minimum Age of Targeted Banks to be 3 Years or Fewer



(b) Allow de novo Interstate Branching



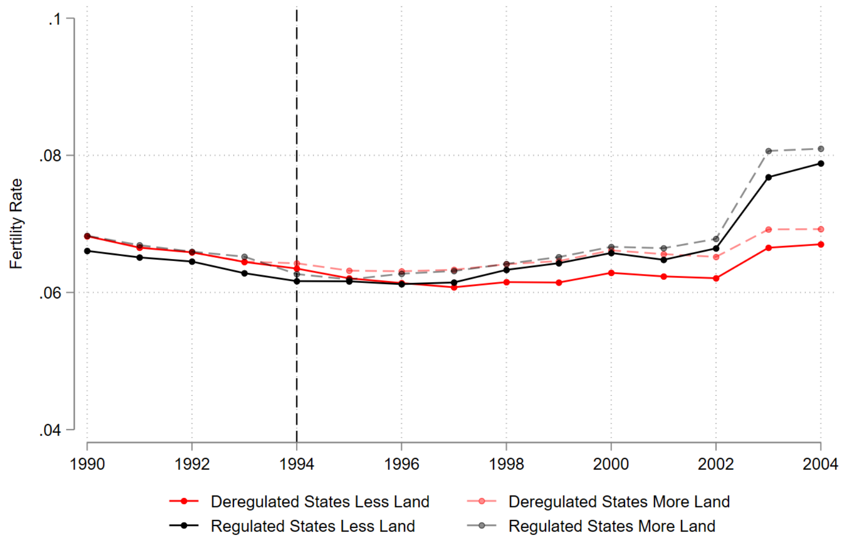
(c) Allow Acquisition of Individual Branch without Acquiring the Entire Bank



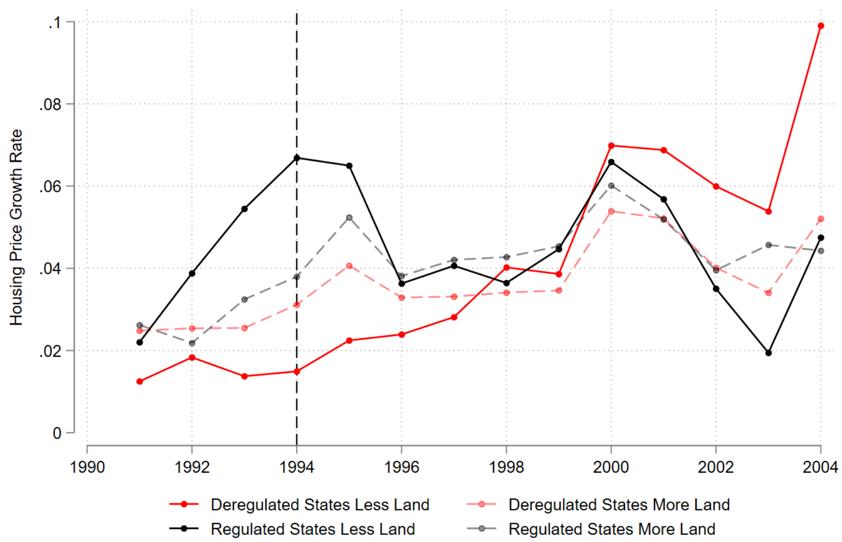
(d) Statewide Deposit Cap to be 30% or Higher

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

Figure A3: Time Trends of County-level Fertility Rate and House Prices Growth Rate



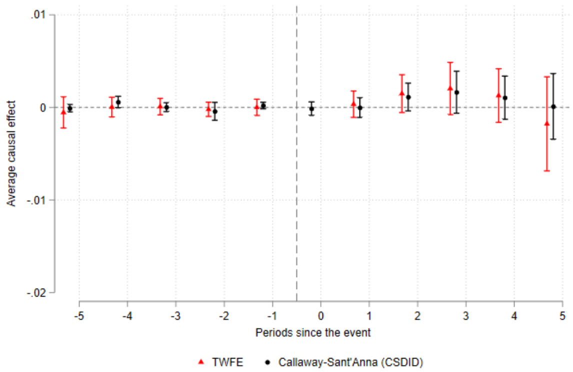
(a) Fertility Rate



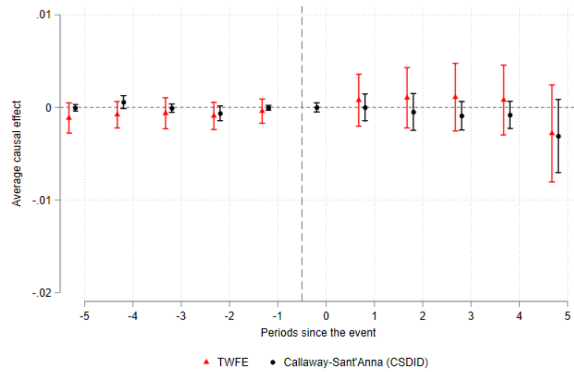
(b) Housing Price Growth Rate

Notes: County-level birth rates are calculated as the number of births per thousand women ages 15-44 divided by the yearly population of women ages 15-44. County-level house prices come from Federal Housing Finance Agency Housing Price Index (HPI).

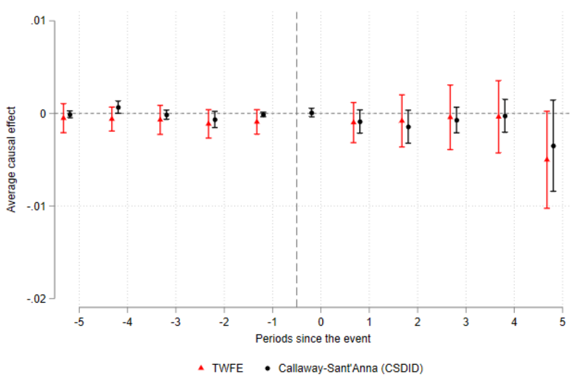
Figure A4: Bank Branching Deregulation and Fertility Rate:
Four Types of Deregulation



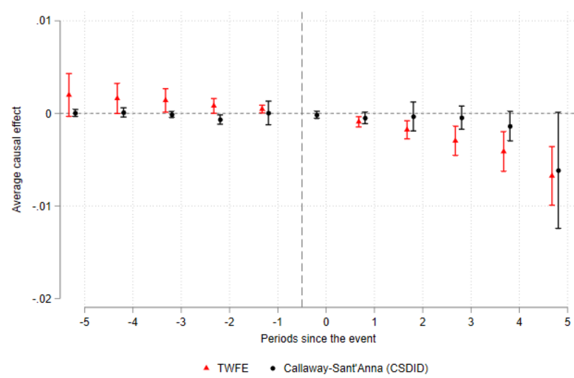
(a) Minimum Age of Targeted Banks to be 3 Years or Fewer



(b) Allow de novo Interstate Branching



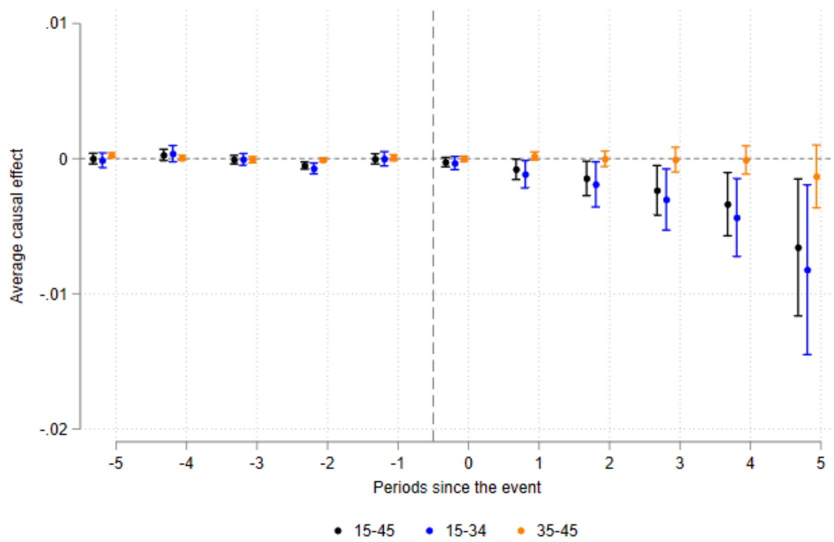
(c) Allow Acquisition of Individual Branch without Acquiring the Entire Bank



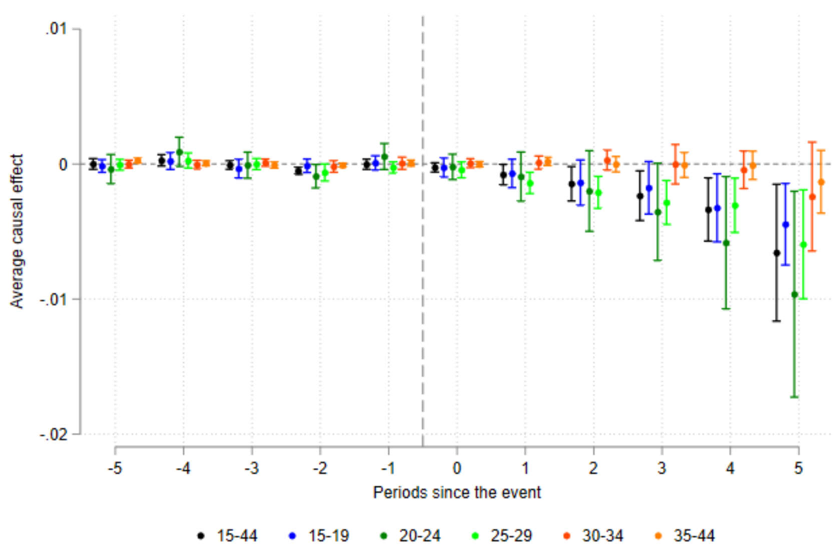
(d) Statewide Deposit Cap to be 30% or Higher

Notes: This figure plot the effects of the four types of interstate bank branching deregulation on the county-level fertility rate calculated based on the Vital Statistics Natality Files (449 counties between 1990 and 2004). Data on interstate branching deregulation by state and by year come from Rice and Strahan (2010). The deregulation dummy indicates whether the state has implemented a certain type of interstate bank branching deregulation. Standard errors are clustered at the state level. All Figures show 95 percent confidence intervals.

Figure A5: Bank Branching Deregulation and Fertility Rate
by Mother's Age



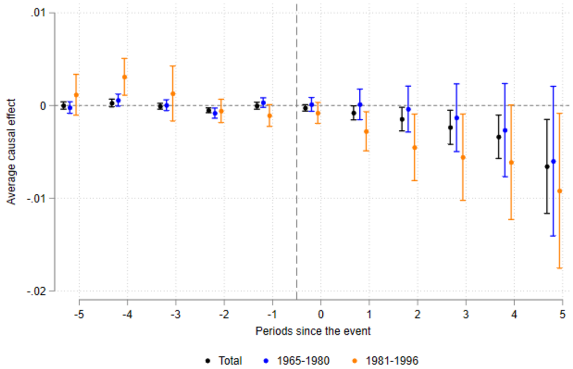
(a) Two Age Groups



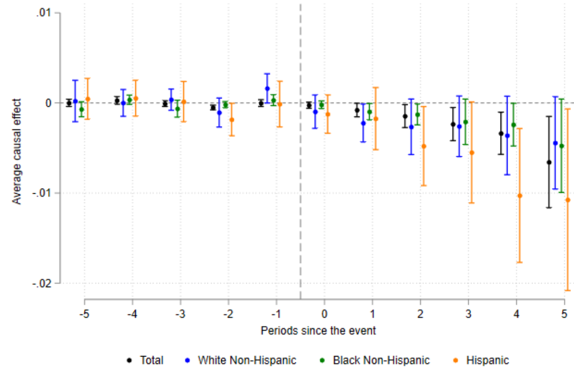
(b) Five Age Groups

Notes: This figure plot the effects of interstate bank branching deregulation on the county-level fertility rate calculated based on the Vital Statistics Natality Files (449 counties between 1990 and 2004) by mother's age. The deregulation dummy indicates whether the state has implemented a certain type of interstate bank branching deregulation. Panel (a) divides the sample into two age groups while panel (b) divides the sample into five age groups. All Figures are event studies based on the CSDID estimates and show 95 percent confidence intervals. Standard errors are clustered at the state level.

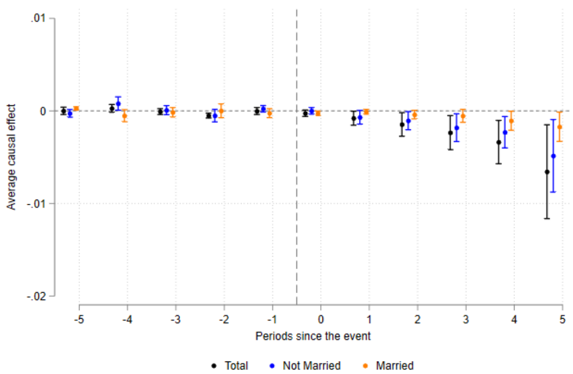
Figure A6: Bank Branching Deregulation and Fertility Rate:
Heterogeneous Effects



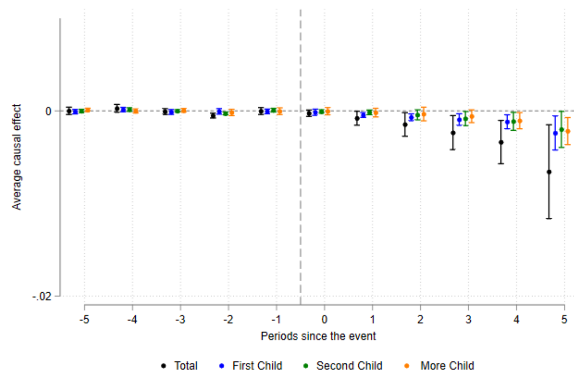
(a) Birth Cohort



(b) Race and Ethnicity



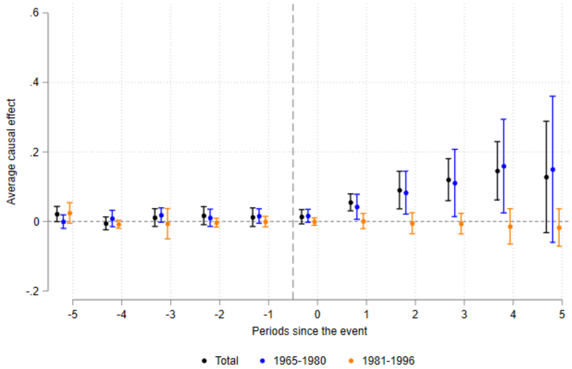
(c) Marital Status



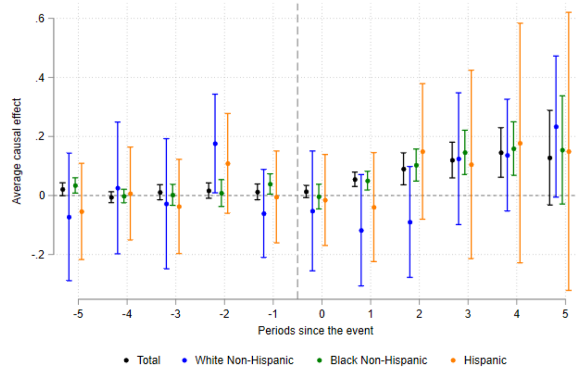
(d) Birth Order

Notes: This figure plots the heterogeneous effect of the interstate bank branching deregulation on the county-level fertility rate which is calculated based on the Vital Statistics Natality Files (449 counties between 1990 and 2004). The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. All Figures are event studies based on the CSDID estimates and show 95 percent confidence intervals. Standard errors are clustered at the state level.

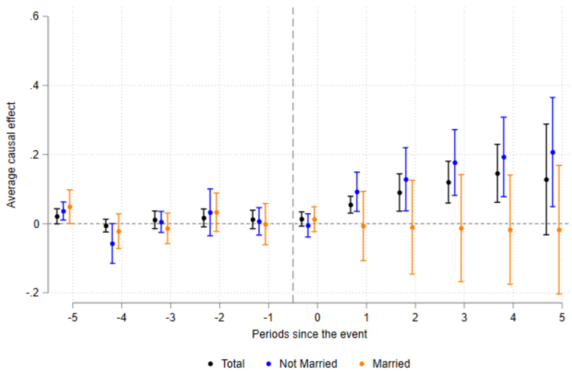
Figure A7: Bank Branching Deregulation and Maternal Age:
Heterogeneous Effects



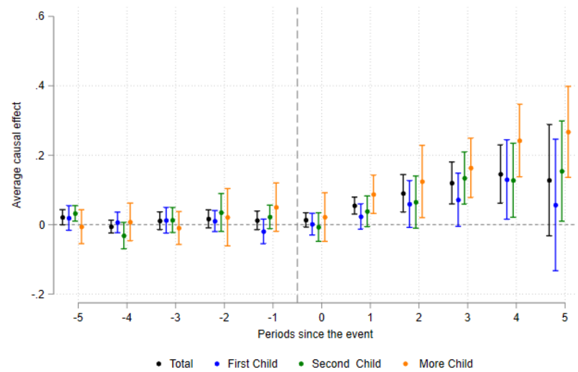
(a) Birth Cohort



(b) Race and Ethnicity



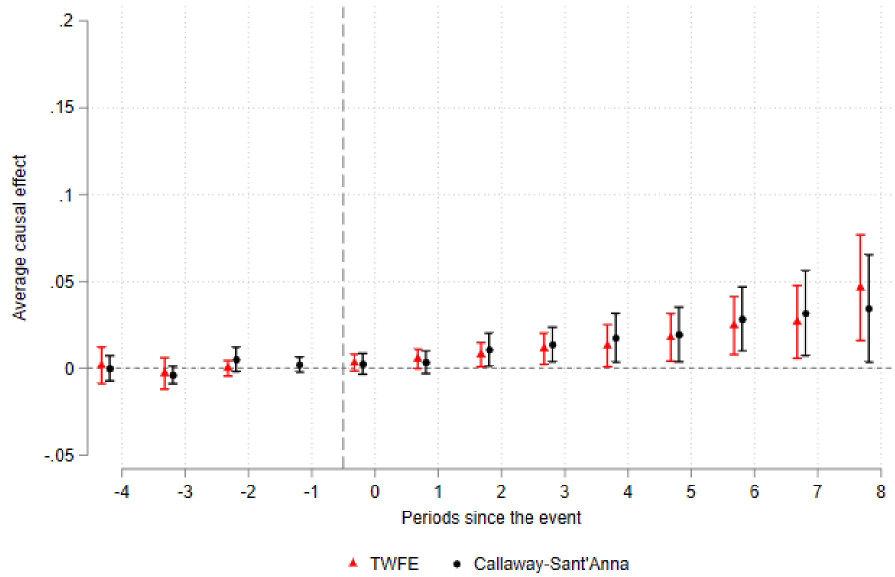
(c) Marital Status



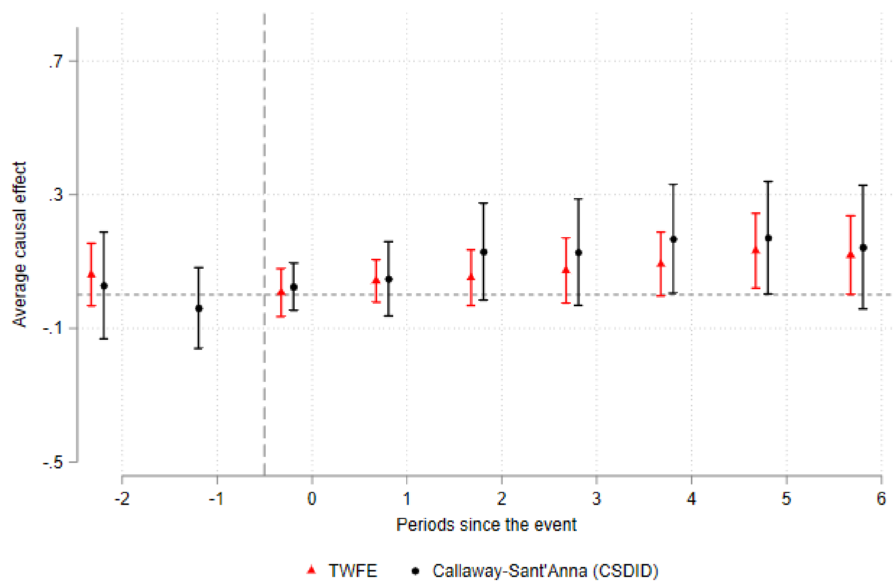
(d) Birth Order

Notes: This figure plots the heterogeneous effect of the interstate bank branching deregulation on the county-level maternal age which is calculated based on the Vital Statistics Natality Files (449 counties between 1990 and 2004). The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. All Figures are event studies based on the CSDID estimates and show 95 percent confidence intervals. Standard errors are clustered at the state level.

Figure A8: Bank Branching Deregulation, House Price, and Mortgage Loans



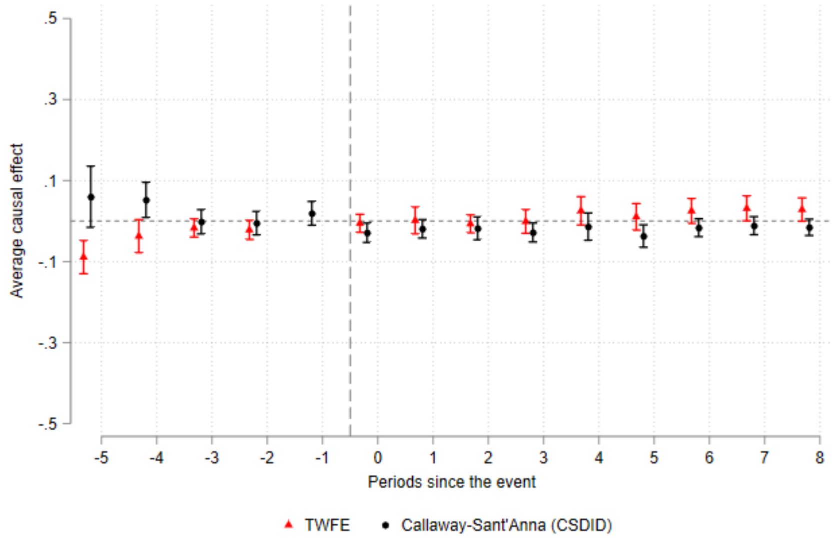
(a) County-level House Price



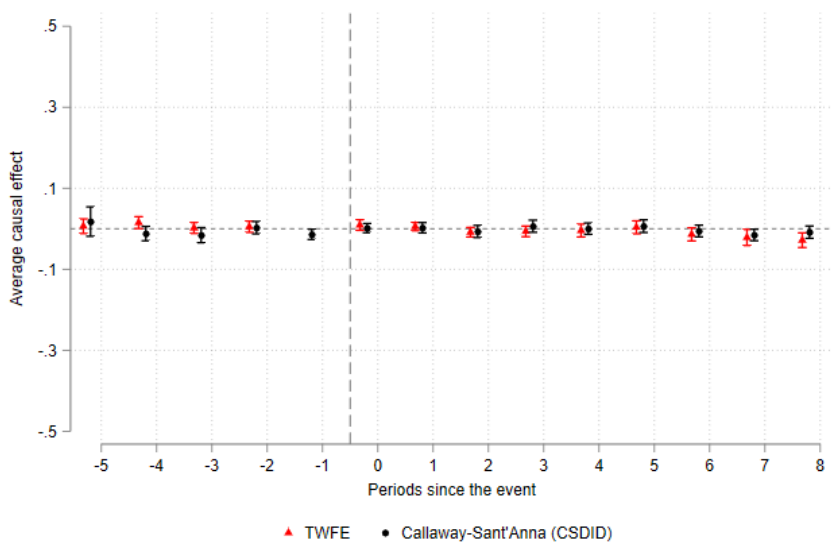
(b) County-level Number of Mortgage Loan

Notes: The outcome in panel (a) is the log change in the FHFA house price index at the county level; The outcome in panel (a) is the log number of mortgage loans at the county level calculated based on the Home Mortgage Disclosure Act (HMDA) data. The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. All Figures show 95 percent confidence intervals. Standard errors are clustered at the state level.

Figure A9: Bank Branching Deregulation and Local Labor Market Outcomes: TWFE and CSDID



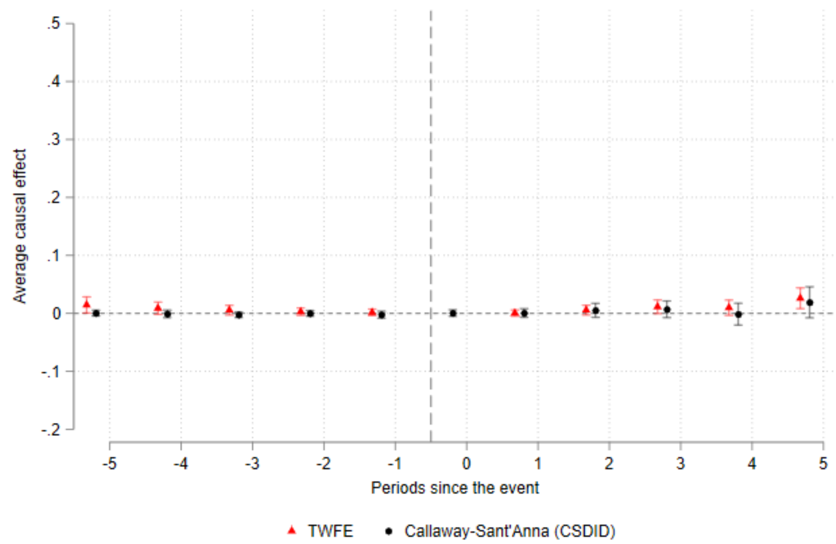
(a) County-level Employment



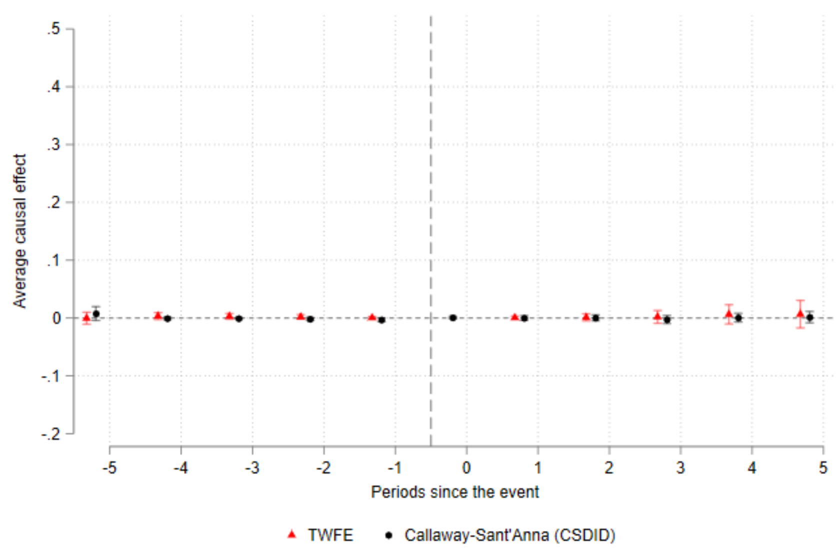
(b) County-level Wage

Notes: The outcome in panels (a) and (b) are logs of county-level employment and wage calculated based on the Quarterly Census of Employment and Wages (QCEW) (1990-2005). The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. All Figures show 95 percent confidence intervals. Standard errors are clustered at the state level.

Figure A10: Bank Branching Deregulation and Birth Health Outcomes:
TWFE and CSDID



(a) Birth Weight



(b) 5-minute Apgar score

Notes: The outcome in panels (a) and (b) are county-level birth weight and five-minute Apgar score (which is a quick test performed on a baby 5 minutes after birth and tells the health care provider how well the baby is doing outside the mother's womb) calculated based on the Vital Statistics Natality Files. The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. All Figures show 95 percent confidence intervals. Standard errors are clustered at the state level.

Table A1: Years of Bank Branching Deregulation

Year	Deregulated States
1995	Alaska
1996	California, Connecticut, Delaware, Maryland, Michigan, Nevada, North Carolina, Pennsylvania, Rhode Island, Utah, Virginia
1997	Arizona, DC, Massachusetts, New Jersey, New Mexico, South Carolina, South Dakota, Vermont, Washington, Wisconsin
1998	Alabama, Florida, Georgia, Hawaii, Indiana, Illinois, Louisiana, Maine, Minnesota, New York, North Dakota, Ohio, Oregon, Tennessee, West Virginia, Wyoming
2000	Texas
2001	Kentucky, New Hampshire, Oklahoma

Notes: Data on interstate branching deregulation by state and by year come from Rice and Strahan (2010). Nine states never deregulated which include Arkansas, Colorado, Idaho, Iowa, Kansas, Mississippi, Missouri, Montana, and Nebraska.

Table A2: Years of Four Types of Bank Branching Deregulation

State	Reform Timing				State	Reform Timing			
	T1	T2	T3	T4		T1	T2	T3	T4
Alabama	x	x	x	1998	Montana	x	x	x	x
Alaska	x	x	1995	1995	Nebraska	x	x	x	x
Arizona	x	x	2002	1997	Nevada	x	x	x	1996
Arkansas	x	x	x	x	New Hampshire	2003	2001	2001	2001
California	x	x	x	1996	New Jersey	1997	x	1997	1997
Colorado	x	x	x	x	New Mexico	x	x	x	1997
Connecticut	x	1996	1996	1996	New York	x	x	1998	1998
Delaware	x	x	x	1996	North Carolina	1996	1996	1996	1996
DC	1997	1997	1997	1997	North Dakota	1998	2004	2004	x
Florida	x	x	x	1998	Ohio	1998	1998	1998	1998
Georgia	x	x	x	1998	Oklahoma	2001	2001	2001	x
Hawaii	2002	2002	2002	1998	Oregon	x	x	x	1998
Idaho	x	x	x	x	Pennsylvania	1996	1996	1996	1996
Illinois	2005	2005	2005	1998	Rhode Island	1996	1996	1996	1996
Indiana	1998	1998	1998	1998	South Carolina	x	x	x	1997
Iowa	x	x	x	x	South Dakota	x	x	x	1997
Kansas	x	x	x	x	Tennessee	x	2002	1999	1998
Kentucky	2001	x	x	x	Texas	2000	2000	2000	x
Louisiana	x	x	x	1998	Utah	x	2002	1996	1996
Maine	1998	1998	1998	1998	Vermont	2002	2002	1997	1997
Maryland	1996	1996	1996	1996	Virginia	1996	1996	1996	1996
Massachusetts	x	1997	1997	1997	Washington	x	2006	2006	1997
Michigan	1996	1996	1996	1996	West Virginia	1998	1998	1998	x
Minnesota	x	x	x	1998	Wisconsin	x	x	x	1997
Mississippi	x	x	x	x	Wyoming	x	x	x	1998
Missouri	x	x	x	x					

Notes: Data on interstate branching deregulation by state and by year come from Rice and Strahan (2010). The reform deregulates banking restrictions from four dimensions (T1-T4): (1). requires a minimum age of the targeted bank to be less than three years; (2). allows de novo branching without an explicit agreement by state authorities; (3). allows the acquisition of individual branches without acquiring the entire bank, and (4). allows the total amount of state-wide deposits controlled by a single bank or bank holding company to be larger than 30%.