More Credit, More Babies? Bank Credit Expansion, House Prices, 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 the U.S. interstate branching deregulation between 1994 and 2005. It employs both traditional and staggered difference-in-difference methods to estimate the causal effect of credit expansion on fertility rates and maternal age. The paper finds that credit expansion reduces fertility rates by 10 percent and increases maternal age by 0.75 percent. Further evidence shows that the housing cost effect is the main mechanism behind the fertility response as this negative fertility effect is more evident in areas where the housing supply is inelastic. These findings highlight the importance of financial market policies and housing affordability for demographic outcomes.

Keywords: Interstate Bank Branching Deregulation, Credit Expansion, House Prices, Housing Supply, Fertility

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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 (Bailey and Hershbein, 2018; 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. Meanwhile, 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 impacts of credit expansion on aggregate economic and housing market dynamics,[2] 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 household fertility decisions through financial, housing, and labor market channels. The financial market channel suggests that easier credit access can lower financial barriers to parenthood by helping parents manage child-related expenses, potentially boosting fertility rates. Conversely, the housing market channel recognizes that increased credit availability can drive up house prices (Favara and Imbs, 2015), which can either discourage fertility by raising housing costs or encourage fertility among homeowners through increased housing wealth. The labor market channel posits that expanded credit can stimulate local economies and job creation. This may impact fertility either positively by increasing household income, or negatively by raising the opportunity costs for potential mothers. Given these competing effects, the net impact of credit expansion on fertility is theoretically ambiguous. The ultimate outcome depends on the relative strengths of these channels, which calls for an empirical analysis to determine the net impact.

Identifying the causal effects of credit supply expansion is challenging due to the endogeneity issue. First, there may be omitted variables that correlate with both credit supply and household childbirth decisions. For example, unobserved positive economic shocks may increase credit supply and affect childbirth by 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 fertility outcomes influence credit supply. For example, areas with higher fertility rates may have a higher demand for credit, leading to the expansion of the credit supply from banks (Lisack et al., 2017; Gong and Yao, 2022).

To address this identification challenge, I explore a natural experiment in bank credit expan-

[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 4).

[2] See Kroszner and Strahan (2014); Favara and Imbs (2015); Hoffmann and Stewen (2020) for examples of studies on this topic.

sion to estimate its impact on fertility rates. The experiment arises from the Interstate Banking and Branching Efficiency Act (IBBEA) of 1994, which allowed banks to operate branches across state lines. Interstate branching allowed banks to improve the geographic diversification of their portfolios, resulting in exogenous credit expansion as demonstrated in Favara and Imbs (2015).[3] Meanwhile, between 1994 and 2005, states had the option to impose barriers to out-of-state bank entry, resulting in staggered interstate branching deregulation across states. This staggered deregulation provides across-state and year variation in credit supply expansion for identifying its causal effect on fertility.

I measure fertility outcomes using two indicators: fertility rate and average maternal age, both at the county and year levels. The fertility rate is the ratio of the number of births to the female population in each county-year cell from 1990 to 2004. The number of births comes from the Vital Statistics Natality Files provided by the National Center for Health Statistics (NCHS) which contains birth certificate information for virtually every live birth in the United States. The annual female population counts come from the CDC Surveillance, Epidemiology, and End Results (SEER) database. The average maternal age at the county-year level is calculated based on the Natality Files. To account for the large fluctuations in housing prices driven by credit expansion as shown in Favara and Imbs (2015), and their effects on fertility, it is essential to measure fertility outcomes at the county level here.[4]

To estimate the causal effect of bank deregulation on fertility, this paper first adopts 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. Moreover, since states are deregulated in a staggered matter, we have multiple periods and multiple treatment groups with different years of treatment, deviating from the canonical case with two time periods and two groups. With staggered treatments, several recent studies have noted that the coefficients from standard TWFE models may not represent a straightforward weighted average of unit-level treatment effects (e.g., de Chaisemartin and D'Haultfoeuille (2020)). This is because early-treated states are used as a control for later-treated states which introduces bias when treatment effects are heterogeneous. To alleviate this concern, this paper also adopts an alternative DID estimation newly developed by Callaway and Sant'Anna (2021), which avoids comparisons between early-treated and later-treated units and allows for consistent estimation

[3] Favara and Imbs (2015) demonstrates that from 1994 to 2005, deregulation accounted for 50-67% of the increase in mortgage loans and 33-50% of house price growth. Using two placebo samples, they show interstate branching deregulation led to exogenous credit expansion. First, only commercial banks, subject to deregulation, increased credit supply, unlike other lenders such as Independent Mortgage Companies, Thrifts, and credit unions. Second, this increase was driven by out-of-state banks entering deregulated states, not local banks. These differential responses identify an exogenous credit supply shock, ruling out demand-based explanations.

[4] Prior research, including studies by Dettling and Kearney (2014) and Kearney et al. (2022), has primarily focused on state or metropolitan areas as geographic units of analysis. This approach may not fully capture the variations that exist across counties. Studies that used the county-level fertility rates also include Bailey (2012) which studies the effects of family planning programs on fertility in the 1960s and 1970s.

under heterogeneous treatment effects. This estimator is denoted as CSDID in the rest of the paper.

The key findings in this paper show that bank credit expansion reduced and delayed fertility, with both TWFE and CSDID estimates showing an evident and persistent decline in fertility rate and an increase in maternal age among females in deregulated states after the interstate bank branching deregulation. Based on the CSDID estimates, county-level female fertility rates in deregulated states are lower by 0.007 percentage points after the deregulation which is equivalent to a 10 percent reduction. Meanwhile, the county-level average maternal age in deregulated states is higher by 0.101 years after the deregulation which is equivalent to a 0.75 percent increase.[5] Those results are robust when adding time-varying economic and demographic controls, excluding not-yet-treated states as control samples or states that experienced other state-level changes in family policies. Further analysis shows that the decline in fertility rates is mainly driven by women who were born in the period from 1970 to 1985, and by women who are not married. These fertility effects are statistically significant for both white and non-white women and different birth cohorts.

The observed reduced and delayed fertility highlights the significance of housing costs and labor market opportunities as potential explanations.[6] To assess their relative importance, I conducted three empirical analyses. First, using a similar DID approach as in the main estimation, I find that bank deregulation significantly increased housing prices but had no significant effects on labor market outcome variables such as employment and wage at the county level. Second, I divide the sample into counties with low and high levels of housing supply elasticity approximated by the proportion of developable land using topological data compiled by Lutz and Sand (2019). The argument is that the impact of housing costs on fertility varies depending on how fast housing prices increase and therefore, how elastic the local housing supply is. However, the labor market effects are independent of these factors. Consisting of the housing cost effects, I find the negative fertility effects are concentrated in areas with low housing supply elasticity. Third, I show that the interaction of bank branching deregulation and local land availability constitutes a legitimate instrument for local house price growth and find that the rise of housing prices can explain a significant share of the resulting decline in the fertility rate and the increase in maternal age. These three analyses consistently support the housing cost effect as the primary mechanism driving the observed negative fertility effects,

[5]The average fertility rate during the sample period is 0.068 percentage points (or 68 births per 1000 women aged 15-44) and the average maternal age during the sample period is 27. This negative effect on the fertility rate is substantial in magnitude considering that the annual fertility rate has decreased from 0.071 to 0.066 between 1990 and 2004.

[6]This analysis aims to explore possible mechanisms explaining the overall fertility effect, not to exclude other potential factors such as housing wealth effects. Some homeowners may have benefited from housing wealth appreciation caused by deregulation, potentially increasing their fertility. However, these positive effects may be outweighed by the negative impacts of high housing costs, resulting in a net negative effect on fertility.

underscoring the significant influence of financial market policies and housing affordability on demographic outcomes.

The Natality birth data enables the estimation of bank deregulation's causal effects on fertility and the examination of potential mechanisms at the county level. However, it lacks information on mothers' housing tenure and labor market status, limiting direct testing of labor and housing market channels in explaining fertility effects. To complement the main findings, I use the Survey of Income and Program Participation (SIPP) 1990-2004 panels to further investigate the impact of bank branching deregulation on fertility and its underlying mechanisms at the household level. The results confirm the negative fertility effects of bank credit expansion, with these effects concentrated among younger birth cohorts of mothers (1970-1985). Importantly, the analysis reveals that bank branching deregulation had more significant effects in areas with less land availability and among homeowners who purchased houses after deregulation, as well as renters. In contrast, homeowners who purchased houses before deregulation were not significantly affected. Moreover, the deregulation had negligible effects on females' labor market outcomes, such as labor supply and wages. These findings lend further support to the housing cost channel, rather than the labor market channel, as the primary driver behind the observed fertility effects.

Overall, this paper makes three significant contributions to the existing literature. First, by highlighting the role of financial market policies and rising housing costs, I offer an alternative explanation to traditional demographic theories and reveal a new dimension of bank deregulation consequences. This approach bridges the gap between economic policy and demographic trends, providing a more comprehensive understanding of factors influencing fertility decisions. Second, I employ methodological innovation through the use of the newly developed Callaway and Sant'Anna Difference-in-Differences (CSDID) method, coupled with a well-designed causal identification strategy. This approach leverages the across-state and year variation in bank branch deregulation to establish a robust causal relationship between bank deregulation and fertility outcomes. Third, I provide a detailed analysis of the underlying mechanisms, emphasizing the critical role of housing costs and local housing supply elasticity in contributing to the negative fertility effects of financial deregulation.

These results reveal how financial policies and housing market dynamics jointly influence demographic outcomes, emphasizing the need for an integrated policy approach and offering valuable insights for policymakers. These findings have significant implications for policymakers, urban planners, and researchers, underscoring the potential unintended consequences of financial and housing market policies on population dynamics.

The rest of the paper is structured as follows. In section 2, I review the related literature. In section 3, I describe the nature of bank branching deregulation in the United States, provide a conceptual theory behind the causal connection between credit expansion and household fertility decisions, and some motivational empirical evidence. 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, robustness checks, and discussions on heterogeneous effects. In Section 7, I discuss possible mechanisms behind the main effects. Section 8 provides individual-level results on the main fertility results and additional tests on the mechanism using the SIPP. Section 9 concludes the paper.

2 Literature Review

2.1 Fertility Literature

This paper is closely related to the growing literature that connects financial market policies or housing market dynamics with fertility decisions in developed countries. Many studies in this field have suggested the positive effects of financial market liberalization and easier credit access on household financial conditions and fertility. For example, research in Hacamo (2021) using data from the American Community Survey and the Panel Study of Income Dynamics found that increased access to mortgage credit, resulting from the bypass of antipredatory laws among national banks in the 2000s, was associated with higher fertility rates. Kim et al. (2022) found that bank deregulation in the 1980s increased households' access to credit improving their subjective expectations for personal prospects, resulting in higher fertility rates. Cumming and Dettling (2023) studied how a decrease in mortgage interest rates increased household fertility among homeowners using administrative data on mortgages and births in the UK. Meanwhile, several studies have identified a positive link between homeowners' housing wealth and fertility, using household-level data in the U.S., Japan, Canada, Australia, and Denmark (Lovenheim and Mumford, 2013; Mizutani et al., 2015; Clark and Ferrer, 2019; Atalay et al., 2021; Daysal et al., 2021).

This paper offers a new perspective in two important ways. First, it explores the possibility that financial liberalization can reduce fertility through the housing cost channel, a housing cost effect of credit expansion. Only a few papers have explored the potential negative fertility effects of rising housing costs in the context of developed countries.[7] For example, Simon and Tamura (2009) found a negative cross-sectional correlation between housing costs and fertility using U.S. Census data over the period 1940-2000 across cities. Dettling and Kearney (2014) found house prices had negative effects on fertility in Metropolitan Statistical Areas (MSA) with low homeownership rates and positive effects on fertility in MSAs with high homeownership rates.

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

To address bias caused by other local factors, it adopts an IV strategy that exploits exogenous variation in house price movements induced by variation across MSAs in their housing supply elasticity, as measured by Saiz (2010). This paper uses interstate bank branch deregulation in the 1990s as a natural experiment and the recently developed staggered DID method to identify the causal impacts of the liberalized financial market on fertility and highlight the possible mechanisms through the housing cost channel.

Second, this paper used the Natality files to examine how fertility outcomes changed over time across different counties. These cross-county variations are crucial for identifying the causal effects of banking deregulation on fertility outcomes considering the large variations of housing market dynamics across counties. Other studies have used the same data but at higher levels of aggregation, which might miss the cross-county differences (Dettling and Kearney, 2014; Kearney et al., 2022), or have used household surveys that were too small to capture the geographic variation in fertility trends (Hacamo, 2021; Kim et al., 2022).[8]

More generally, this paper contributes to the extensive literature that seeks to understand the reasons behind the falling birth rate in the U.S. and other developed countries in history and in recent years. Existing studies have proposed several possible explanations as summarized in Bailey and Hershbein (2018), Kearney et al. (2022) and Doepke et al. (2022). First, the widespread use of the contraceptive pill and family planning programs has significantly reduced fertility in the U.S. (Goldin and Katz, 2002; Goldin, 2006; Bailey, 2010, 2012; Rau et al., 2021). Second, studies find childcare costs and the availability of childcare facilities outside of the home play an important role in females' fertility and labor market decisions (Blau and Robins, 1989; Hirazawa and Yakita, 2009; Rindfuss et al., 2010; Bick, 2016; Bar et al., 2018). Other explanations include gender equality and the increase in women's opportunities to access higher education and the labor market (Basu, 2002; Skirbekk et al., 2004; Monstad et al., 2008; McCrary and Royer, 2011; Cygan-Rehm and Maeder, 2013) and changes in social norms, attitudes, and preferences for having children (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 highlighting financial market policies and rising housing costs as an alternative explanation behind declining birth rates in the modern world.

2.2 Bank Branching Deregulation Literature

This project also contributes to the extensive literature examining the consequences of bank branching deregulations in the United States. These regulatory changes, which allowed banks

[8]For example, the average fertility rate is about 60 births per 1000 females aged 15-44 between 1990 and 2004 based on the Natality Files. This implies that for household surveys like PSID, which covers about 5,000 households each year, only about 300 births can be identified, which may not be enough to study the spatial patterns of fertility.

to expand geographically, occurred in two distinct waves. The first wave, characterized by intrastate branching deregulation, took place in the 1970s-1980s. The second wave, marked by interstate branching deregulation, followed in the 1990s-2000s.

Most research in this field has concentrated on the earlier wave of deregulation, revealing a range of positive economic outcomes. Studies have shown that this deregulation improved financial integration (Landier et al., 2017), stimulated local economic growth (Jayaratne and Strahan, 1996; Huang, 2008), positively impacted state business cycles and small business (Morgan et al., 2004; Demyanyk et al., 2007; Hoffmann and Shcherbakova-Stewen, 2011), reduced income inequality across different groups (Black and Strahan, 2001; Beck et al., 2010; Levine et al., 2014), and enhanced innovation and entrepreneurship (Black and Strahan, 2002; Kerr and Nanda, 2009; Rice and Strahan, 2010; Hombert et al., 2017).[9] The early wave of deregulation has also been found to have significant effects on household-level outcomes. By relaxing household credit constraints, it influenced individual unemployment and labor supply This early wave of deregulation has also been found to relax household credit constraints, and thus affected individual unemployment and labor supply, homeownership entry (Hoffmann and Stewen, 2020; Tewari, 2014; Lin et al., 2021), and education decisions (Stein and Yannelis, 2020). More recently, studies have begun to explore its impact on fertility (Kim et al., 2022; Diebold and Soriano-Harris, 2023).[10]

The wave of financial deregulation in the 1990s and 2000s, though less studied than earlier deregulation, has attracted scholarly attention primarily focused on its effects on mortgage and housing market outcomes. Research has shown that interstate branching led to an exogenous expansion in mortgage credit, significantly impacting house prices (Favara and Imbs, 2015; Chu, 2017; Choi and Hansz, 2021)[11] and increased housing wealth inequality (Yang, 2024b,a). Particularly, Favara and Imbs (2015) uses county-level mortgage origination data to analyze the impact of banking deregulation on mortgage approvals by commercial banks and non-bank lenders. The findings revealed that deregulation exclusively affected the credit supply of commercial banks subject to legal changes, while non-bank lenders remained unaffected. According to the authors, deregulation enabled banks to geographically diversify their portfolios and reduce funding costs, resulting in lower interest rates and increased lending. This consequently led to higher housing demand and prices. Interestingly, the house price response was more

[9]See Kroszner and Strahan (2014) for a detailed survey on studies of the consequences of the first wave of bank deregulation.

[10]Different from this paper, Kim et al. (2022) focused on bank deregulation in the 1980s and found positive effects of bank deregulation on fertility. Diebold and Soriano-Harris (2023) attempts to reconcile results in Kim et al. (2022) and this paper by looking at how both waves of bank deregulations affect fertility outcomes.

[11]A growing body of recent studies has also identified a positive relationship between credit supply and house prices by exploring financial market policies beyond interstate bank branching deregulation (Maggio and Kermani, 2017; Gete and Reher, 2018; Justiniano et al., 2019; Saadi, 2020; Hoffmann and Stewen, 2020; Mian and Sufi, 2021).

pronounced in areas with less elastic housing supply and less significant in regions with more elastic supply. In a related paper, Chu (2017) employed a regression discontinuity design and commercial real estate data to demonstrate that interstate banking deregulation also influences credit supply through the bank competition channel. Choi and Hansz (2021) discovered that interstate banking deregulation not only contributed to housing booms in the 2000s but also led to increased comovement of house prices across U.S. metropolitan areas. Yang (2024b) and Yang (2024a) show that bank deregulation increases housing wealth inequality. It leads to greater housing wealth accumulation for homeowners in states after deregulation and in areas with inelastic housing supply, but not for other groups of households.[12]

Despite its impacts on the housing and financial market, the deregulations in the 1990s and 2000s did not seem to have large impacts on income, unemployment, and economic growth as the first wave of deregulation (Favara and Imbs, 2015; Célerier and Matray, 2019; Yang, 2024b,a). This is likely because the financial market was more integrated in the later periods. A more comprehensive comparison of the two waves of banking deregulation is beyond the scope of this paper but would be an interesting topic for future research.

Adopting the recently developed staggered DID method, this paper confirms previous findings that the second wave of banking deregulation raises local house prices, especially in areas where the housing supply is inelastic (Favara and Imbs, 2015). More importantly, by exploring the connection between banking deregulation and demographic outcomes, this paper contributes to the evidence on the consequences of the second wave of deregulations and broadens the scope of current literature on the nature of financial market policy transmission to the real economy.

2.3 Housing Supply Literature

The housing cost channel emphasized in this paper aligns closely with the literature highlighting the crucial role of housing supply in determining housing price dynamics. The U.S. housing market exhibits significant geographic heterogeneity in prices (Ferreira and Gyourko, 2023), largely attributable to differences in housing supply elasticity. These differences stem from various factors, including local costs, land use regulations, and geographical constraints (Quigley and Raphael, 2005; Gyourko et al., 2008; Glaeser et al., 2008; Saiz, 2010; Gyourko and Molloy, 2015; Song, 2021; Gyourko et al., 2021). These supply-side factors not only affect housing affordability and urban development patterns, and also have broader effects far beyond real estate markets, influencing economic mobility, job market creation and mismatch, and urban population growth (Glaeser et al., 2006; Glaeser and Gyourko, 2018; Saks, 2008; Hsieh and

[12]Other papers that studied the deregulation in the 1990s include Célerier and Matray (2019) which find deregulation improves financial market access for low-income households and Yang and Zou (2023) which exploits interstate branching deregulation as state tax revenue shocks and using school finance data, revealed that deregulation led to an increase in per-pupil total revenue and expenditure.

Moretti, 2019; Diamond, 2016; Ganong and Shoag, 2017; Shoag and Russell, 2018; Gabriel and Painter, 2020; Liu and Yang, 2021).

This paper specifically highlights how housing supply elasticity mediates the effects of credit expansion on fertility broadening the scope of the literature. While we use land availability as a proxy for local housing supply, findings in this paper suggest that land use regulations could have similar effects on fertility outcomes through their impact on housing costs. Thus, these results have important implications for understanding the broader economic outcomes of housing market dynamics and regulatory policies.

3 Background

3.1 Institutional Background: Interstate Bank Branching Deregulation in the U.S.

Most U.S. states historically restricted interstate banking and branching, dating back to colonial times.[13] In the 1970s and 1980s, the banking sector went through decades of deregulatory changes regarding banks' geographic expansion. However, interstate bank branching was still not allowed until 1994 (Calomiris, 2000; Kroszner and Strahan, 1999, 2014). 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).

Figure 2 maps the timing of deregulation across states, with darker shades representing earlier deregulation. Most of the policy changes took place between 1996 and 2002. By 2005, eight mid-western states still were not deregulated, and we did not observe additional deregulation afterward.[14] Deregulation is more common and happened earlier in eastern and western states and relatively uncommon or happened later among states in the middle. The cross-state and -year policy variation enables a staggered DID design to identify the causal effect of credit expansion. To capture this policy variation, a deregulation indicator is created to indicate whether the deregulation has taken place in a state. As in Rice and Strahan (2010), every state is assumed to be fully restricted in 1994, and deregulation is defined if the state has relaxed at least

[13]Interstate banking is when bank-holding companies own banks in different states, while interstate branching is when a single bank has branches in multiple states without separate structures.

[14]More details on the timing of deregulation across each state are presented in Appendix Table A1. The eight states are Arkansas, Colorado, Iowa, Kansas, Mississippi, Missouri, Montana, and Nebraska.

one restriction on interstate branching.[15]

More importantly, Favara and Imbs (2015) shows that from 1994 to 2005, banking deregulation drove 50-67% of mortgage loan growth and 33-50% of house price appreciation. They establish the exogenous nature of this credit expansion through two placebo tests: 1) only commercial banks, subject to deregulation, increased lending, unlike other financial institutions; 2) credit growth came from new out-of-state banks, not local lenders. These findings confirm an exogenous credit supply shock, distinct from demand-side factors. This paper exploits this exogenous shift in credit supply to identify its effects on fertility outcomes.

3.2 Theoretical Background: Interstate Bank Branching Deregulation and Fertility

Why interstate bank branching deregulation can affect household fertility decisions? The static model of fertility assumes that parents choose how many children to have based on their lifetime utility, which depends on the price of children and the budget constraint they face. Children are normal goods (Becker, 1960) that provide utility in terms of life satisfaction, happiness, or pleasurable experiences. This assumption implies that demand for children will increase with household income or wealth and decrease with the associated costs.[16] Based on this static model of fertility, I present housing, labor, and financial market channels through which banking deregulation can affect household fertility decisions (Figure 1).

Housing Market Channel: The connection between interstate banking deregulation and the housing boom in the 1990s and 2000s is well documented in the literature (Favara and Imbs, 2015; Chu, 2017; Choi and Hansz, 2021). The impact of housing prices on fertility can be negative or positive, depending on the relative size of the "housing cost effects" and the "housing wealth effects." On the one hand, higher housing prices can increase the cost of living for renters and future or recent home-buyers, who have to pay more for rent or mortgage. This can

[15] The IBBEA permitted states to establish restrictions on out-of-state bank entry in four key areas: (i) setting a minimum age requirement for the targeted bank, (ii) prohibiting de-novo branching without explicit approval from state authorities, (iii) allowing the acquisition of individual branches without the need to acquire the entire bank, and (iv) implementing a state-wide deposit cap. 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). The results of this paper are robust when adopting the index instead of the dummy. More discussion regarding the history of the interstate branching deregulation and the index is presented in Appendix I.

[16] The early empirical evidence based on pre-2000 data, however, often finds a negative correlation between income and the number of children which is inconsistent with this prediction. Two main theories tried to explain this negative correlation maintaining the assumption of children as normal goods: one focused on the trade-off between child quantity and quality (Becker, 1960); the other focused on the higher opportunity cost of time for high-income parents (Mincer, 1963; Becker, 1965). However, as documented in Doepke et al. (2022), recent data shows that fertility is no longer negatively related to income across high-income countries, which challenges the previous theories and requires new ones.

reduce their demand for children since housing is a major component of child-rearing expenses (Simon and Tamura, 2009; Dettling and Kearney, 2014). On the other hand, higher house prices can increase fertility among homeowners who have purchased a decent-sized house before the deregulation, and thus, benefit from the housing price and wealth appreciation (Lovenheim and Mumford, 2013; Dettling and Kearney, 2014; Mizutani et al., 2015; Clark and Ferrer, 2019; Atalay et al., 2021; Daysal et al., 2021).

Labor Market Channel: Banking deregulation can impact fertility decisions by influencing local economic conditions and labor markets. This influence operates through two competing mechanisms: it may lower fertility by increasing the opportunity cost of motherhood through expanded employment opportunities, particularly for women. Conversely, it may raise fertility by enhancing household income and financial security.(Black and Strahan, 2001; Jayaratne and Strahan, 1996; Huang, 2008; Unel, 2020; Dao Bui and Ume, 2020).

Financial Market Channel: Another way that bank deregulation affects fertility is through the financial market channel, which suggests that banking deregulation can lower the barriers to credit access and borrowing for households (Tewari, 2014; Célerier and Matray, 2019; Hacamo, 2021; Kim et al., 2022; Cumming and Dettling, 2023). As a result, households face less liquidity constraints and can afford to consume more normal goods, such as having more children.

Overall, bank deregulation has complex, potentially contradictory effects on fertility. Increased credit supply can reduce fertility by raising housing costs and enhancing job opportunities, which increases the cost of having children. However, it can also promote fertility by increasing housing wealth, easing financial constraints, and boosting household income. The overall impact on birth rates depends on which of these opposing effects is stronger. This paper aims to estimate the overall impact of credit expansion on fertility and discuss the relative importance of the different mechanisms behind this impact.

3.3 Motivation Evidence

To begin the analysis, I examine national time-series data to describe the aggregate correlation between bank credit supply, home prices, and fertility from 1980 to 2020. The annual birthrate, calculated as the number of births per thousand women ages 15 to 44, is derived from the Vital Statistics Natality Files. The aggregate bank credit supply of all commercial banks is collected from Federal Reserve Economic Data.[17] For housing prices, I use the national-level Federal Housing Finance Agency (FHFA) Housing Price Index (HPI).

Figure 4 illustrates the relationships among these variables. The fertility rates (solid line) demonstrate a negative relationship with both the annual percent change in bank credit (red

[17]These data can be accessed at `https://fred.stlouisfed.org/series/TOTBKCR`.

dashed line) and the annual percent change in housing prices (blue dashed line) at the national level. Additionally, the figure indicates a positive relationship between bank credit and housing prices over time. These observed connections between bank credit, housing prices, and fertility rates motivate the primary objectives of this paper: to identify the causal effects of bank credit expansion on fertility and to explore the role of housing market dynamics as the potential underlying mechanism.

Figure 5 presents graphical evidence of the impact of bank branching deregulation on fertility rates, zoom into the period from 1990 to 2005, which captures the key years of bank branch deregulation and subsequent effects across the United States. Panel (a) of Figure 5 plots the time trends of fertility rates in two groups of states, those that underwent deregulation (represented by the red line) and those that remained regulated (represented by the black line). In the years preceding deregulation, specifically before 1996, we observe relatively steady decreases in fertility rates in both groups of states. This parallel movement lends credibility to the key identifying assumption of the difference-in-differences strategy, suggesting that treatment and control states were following similar trends prior to the policy change. The divergence in trends becomes apparent post-1996, coinciding with the deregulation. We see fertility rates begin to increase in both groups but with a notably slower rate of increase in the deregulated states. This divergence persists and grows larger over time, suggesting a sustained impact of the policy change on fertility decisions.

To investigate the role of housing supply in these effects, Panel (b) of Figure 5 introduces an additional dimension to the analysis. Here, we disaggregate the deregulated states into two groups based on land availability for development. This decomposition unveils a compelling pattern: the deceleration in fertility rate growth is predominantly observed in deregulated states with limited land availability. The observed pattern could be attributed to housing affordability dynamics. In states constrained by less available land, the deregulation of bank branching likely facilitated increased credit accessibility. This expansion of credit, coupled with limited land supply, may have exerted upward pressure on housing prices more intensely than in states with abundant land resources. The resultant elevation in housing costs could, in turn, influence fertility decisions, potentially leading couples to postpone childbearing due to affordability concerns. These results presented in Panel (b) offer persuasive preliminary evidence for the significant role of housing prices and affordability in shaping fertility trends within the context of bank deregulation. The findings underscore the complex interplay between financial policy, local geographic conditions, and demographic outcomes.

Figure 6 further investigates the housing price channel by illustrating the impact of bank branching deregulation on housing price growth. The findings are consistent with the housing price hypothesis and align with existing literature, such as Favara and Imbs (2015). Panel (a) of Figure 6 depicts the trends in housing prices for deregulated and regulated states. In the years preceding deregulation, specifically before 1996, we observe a relatively steady and similar increase in housing prices in both groups of states. However, a noticeable divergence in trends emerges post-1996, coinciding with the implementation of deregulation. While housing prices continue to rise in both groups, the rate of increase is markedly greater in the deregulated states. Panel (b) further disaggregates this effect based on land availability. It reveals that the acceleration in housing price growth is concentrated in states with less available land. This pattern aligns with the slower growth of fertility rates observed in these areas, lending additional support to the housing affordability channel as a key mechanism influencing fertility decisions.

The analysis then turns to labor market conditions, utilizing data from the QCEW. Panels (c) and (d) focus on employment trends. Panel (c) shows a general decrease in employment across both regulated and deregulated states, and this decrease is less pronounced in deregulated states. While these employment trends don't rule out labor market mechanisms as a factor in fertility decisions, they fail to explain the stronger fertility effect observed in areas with less available land since panel (d) shows that the decrease in employment is slightly slower in areas with less available land. Panels (e) and (f) examine wage trends and reveal an increase in wages across both regulated and deregulated states with no significant differences in wage growth between the two. Moreover, land availability does not appear to play a substantial role in generating wage differentials as shown in panel (f). These wage trends, similar to the employment trends, do not provide strong support for labor market conditions as the primary driver of the observed fertility rate patterns, particularly the more pronounced effects in land-constrained areas.

Overall, the above analysis provides preliminary evidence for the housing price channel as a key mechanism through which bank branching deregulation affects fertility rates. The more dramatic increase in housing prices in deregulated states, especially those with limited land availability, aligns closely with the observed patterns in fertility rates. While labor market effects are present, they do not seem to explain the spatial variation in fertility responses, particularly the stronger effects in areas with less available land. These findings underscore the importance of considering housing market dynamics when analyzing the demographic impacts of financial deregulation policies.

4 Data and Summary Statistics

4.1 Vital Statistics Natality Birth Data

The primary fertility outcome data are derived from the Vital Statistics Natality Files (1990-2004), which I used to calculate two main outcome variables at the county-year level: fertility rate and average maternal age.[18] These natality files contain information from birth certificates for nearly every live birth in the United States. The fertility rate is computed as the ratio of total births in a county-year group to the female population counts in the same group, with population data obtained from the CDC SEER database.[19]

Given the significant variations in house price changes across counties within states or metropolitan areas, I chose county as the geographic unit for measuring fertility rates, in contrast to the state or metropolitan area level used in previous studies (Kearney et al., 2022; Dettling and Kearney, 2014). The sample comprises approximately 449 counties, primarily located in high population density areas (Figure 3). The period from 1990 to 2004 encompasses both pre- and post-deregulation years.

The natality files also provide a wide range of characteristics for each birth and mother. To explore heterogeneous effects, I constructed fertility rate and maternal age variables for various demographic groups at the county level, including marital status, race and ethnicity (Non-Hispanic White, Non-Hispanic Black, and Hispanic), birth order, and different birth cohorts and age groups.

4.2 Other Data Sources

To control for other factors that might affect fertility outcomes and to explore potential mechanisms, this paper also relies on various other data sources. First, I consider state-level time-varying economic factors that might influence fertility trends. These include the state unemployment rate, generosity of welfare benefits, the state minimum wage, and expenditures on child support enforcement. The generosity of welfare benefits is measured by the monthly maximum TANF benefit for a family of three, expressed in thousands of dollars.[20]

In addition to economic factors, I consider two reproductive health policies: abortion restrictions in the form of parental notification laws and waiting periods.[21] For demographic controls, I include county-year level population shares of women aged 15-29 and 30-44, as well as non-Hispanic white, non-Hispanic black, and Hispanic women aged 15-44. These variables

[18]These data can be accessed at http://www.cdc.gov/nchs/nvss.htm. National vital statistics micro-data files that include geographic information at the state, county, or city level have not been publicly available since 2005 without approval.

[19]These data can be accessed at https://seer.cancer.gov/popdata/download.html.

[20]These variables 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.

[21]Kearney and Riley (2022) have considered four other reproductive health policies: health insurance coverage through Medicaid, mandatory coverage of contraception in private insurance plans, mandatory sex education, and mandatory contraception instruction laws. These policy variables are not included in this study either due to lack of information or lack of variation between 1990 and 2004.

are calculated based on the CDC SEER population count database.[22]

Moreover, to test housing cost and labor market channels, I explore county-level variables that capture housing and labor market conditions. For housing market conditions, I use the Federal Housing Finance Agency (FHFA) housing price indexes.[23] The FHFA indexes series are available from 1975 through 2015, constructed from all repeat-sale, single-family homes whose mortgages have been securitized by Fannie Mae or Freddie Mac each year. These data are available at state and MSA levels and are widely used in the housing literature. I use the county-level housing price index, which became available more recently, 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 elasticity 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 percent of the total area.

For local labor market conditions, 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 level and by industry.

4.3 Summary Statistics of the Analysis Sample

Table 1 reports summary statistics of the natality birth data and the regression sample utilized in this study. The table is divided into two distinct panels: the upper panel, which summarizes mothers' characteristics from the natality birth data, and the lower panel, which describes the regression sample, including outcomes and control variables at the county or state-year levels.

The upper Panel shows that the average maternal age is approximately 27 years. In terms of race and ethnicity, 61% of the mothers are Non-Hispanic White, 33% are Non-Hispanic Black or Hispanic, and 6% belong to other racial or ethnic groups. The birth cohorts show that 7% of the mothers were born in the 1950s, 40% in the 1960s, and 53% in the 1970s or later. Regarding

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

[23]These data can be accessed at http://www.fhfa.gov.

marital status, 69% of the mothers were married at the time of childbirth, while 31% were unmarried. Educational attainment varies, with 22% having less than a high school diploma, 33% holding a high school diploma, 35% possessing a college degree, and 8% having an advanced degree. hose characteristics are quite consistent across states that underwent interstate bank branching deregulation in different years 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 levels. The county-level fertility rate is 6 percent on average, which means 60 births per 1000 females aged 15-44. The average maternal age at the county level is about 27, which matches the summary statistics in the upper panel. The county-level population compositions are quite similar between states that experienced interstate bank branching deregulation and those that did not. Deregulated states have slightly higher average values for unemployment rates (5.52 vs. 4.77), minimum wages ($4.67 vs. $4.39), and maximum monthly welfare benefits ($700 vs. $640).

5 Empirical Strategy

5.1 Two-way Fixed Effects Model

To estimate the effect of interstate branching deregulation on fertility, I use a DID framework with the following two-way fixed effects (TWFE) estimation equation:

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

where F_{cst} is fertility rate or average maternal age in county c of state s in year t. The dummy variable D_{st} indicates whether the 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, and 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. 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 deregulation was implemented by states, the error term $\epsilon_{c,t}$ are clustered at the state level. All regressions are weighted by the county-level share of females ages 15-44 among the sample population.[24]

To test for parallel trends and study the dynamics of treatment effects, leads and lags of treatment are added as in the following estimation equation

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

where e stands for the 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 deregulation took place. Notice that $D_{st} = \sum_e D_{st+e}$, so α_{1e} are usually interpreted as dynamic decomposition of ATT as estimated by α_1.

However, since states are deregulated staggered, the DID framework here has multiple periods and multiple treatment groups with different years of treatment, deviating from the canonical case with two time periods and one treatment group. Several studies have noted that with staggered treatments, the coefficients from standard TWFE DID models may not represent a straightforward weighted average of unit-level treatment effects. This is because 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 ATT, even when the researcher can randomize treatment assignment (where the parallel-treads assumption holds).[25]

5.2 CSDID

To alleviate this concern, I adopt a staggered DID estimation newly developed by Callaway and Sant'Anna (2021). I refer to this method CSDID for the rest of the paper. This method decomposes the DID analysis into three steps. In step one, group-specific average treatment effects on the treated are identified, denoted $ATT(g, t)$, reflecting average treatment effects on the treated in period t for the group treated at time g. Callaway and Sant'Anna (2021) show that under the assumption that the control and treatment groups follow counterfactual parallel trends, $ATT(g, t)$ can be expressed as

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

[24] Results using alternatively weighting by county-level share of females ages 15-44 among the national population nation are similar and are available upon request.

[25] 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.

where the first term is the evolution of the outcome for the treatment group and the second term is the equivalent evolution of the outcome for the control group. Both quantities are simple averages and are easily calculated from the data. Notice that equation (3) makes no comparisons across groups treated at different times, avoiding the issues of forbidden comparisons where early-treated units are used as controls for later-treated units.

In step two, $ATT(g, t)$ are aggregated to calculate the dynamic treatment effect for each period e relative to a treatment date since the causal effects of treatment time 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 ($e < 0$). These averages are plotted in graphs that are analogous to the relative time coefficients generated from the standard TWFE regression on dynamic effects as in Equation (2). To create a single, overall point estimate, one can take the average of these aggregated relative time estimates when $t > g$. In step three, a bootstrapping procedure is adopted to calculate the simultaneous confidence bands that are robust to multiple hypothesis testing and cluster errors by county.

6 Empirical Results

6.1 Bank Branching Deregulation and Fertility Rate

Table 2 presents estimates of the overall effect of bank branching deregulation on the county-level fertility rate and shows consistently negative effects across different specifications. Column (1) shows results using a TWFE model. Column (2) employs the CSDID model using both not-yet-treated as well as never-treated observations as controls. Column (3) employs the CSDID model by using only never-treated observations as controls to avoid potential bias from not-yet-treated observations. Column (4) incorporates economic control variables, such as state unemployment rate, minimum wage, and welfare benefit generosity, into the baseline model in column (2). Column (5) 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 to the model in column (2). Column (6) excludes states with changes in abortion restrictions between 1990 and 2004. Results are quite stable across specifications.[26]

The estimated negative effects of banking deregulation on fertility rates range from -0.002 to -0.010 across different specifications. In the preferred specification (column 2), the estimated coefficient is -0.007, significant at the 1 percent level. Given the average fertility rate of 0.068

[26] Additional robustness tests, such as excluding California, Texas, and Utah to ensure effects are not driven by large states or religious factors, adding state-specific trends to account for different state-level economic dynamics, and adopting weights based on national population, further confirm the main results, as presented in Appendix Table A2.

during the sample period, this estimate suggests that banking deregulation is associated with a 10 percent reduction in fertility rates in deregulated states. This effect is substantial, considering that the aggregate annual fertility rate decreased by 7 percent from 1990 to 2004.

To examine parallel trends and study the dynamic treatment effects of banking deregulation, Figure 2 (lower panel of Table 2) plots the dynamic effects results. The graph shows no discernible pre-trends across all specifications, with coefficients for years preceding banking deregulation close to zero. However, it reveals a clear and persistent decrease in fertility in deregulated states, which continues for at least five years post-deregulation. Unlike a standard event study framework, there is no omitted category in this analysis. Instead, each coefficient measures the average treatment effect on the treated (ATT) in every year after deregulation, averaging over the event-time coefficients for groups deregulated in different years, where the number of the years before after the deregulation is the running variable on the x-axis.

6.2 Bank Deregulation and Maternal Age

Table 3 presents estimates of banking deregulation's overall effect on county-level average maternal age, consistently showing positive impacts across various specifications. Columns (1) to (6) adopt similar specifications as in Table 2, with results remaining stable across specifications. The estimated coefficients range from 0.098 to 0.325. In the preferred specification (column 2), the estimated coefficient is 0.204, significant at the 1 percent level. Given the average maternal age of approximately 27 years during the sample period, this implies that banking deregulation leads to a 0.75 percent increase in average age, or a delay of about two months in giving birth.

To examine parallel trends and study the dynamic treatment effects of banking deregulation, Figure 8 (lower panel of Table 3) plots the dynamic results. Similar to Figure 7, it shows no discernible pre-trends across all specifications. However, it reveals a sharp and persistent increase in maternal age in deregulated states, which continues for at least five years post-deregulation. These results suggest that banking deregulation not only reduces the overall number of births but also delays the timing of births. [27]

6.3 Diagnose of TWFE and Alternative DID Estimators

Recent literature has introduced diagnostic approaches to assess the bias caused by TWFE DID specifications, particularly in contexts with staggered treatment timing and heterogeneous treatment effects, focusing on the static specification as in equation (1). First, de Chaisemartin and D'Haultfoeuille (2020) proposes calculating and reporting the number/fraction of group-

[27] Additional analyses of other fertility outcomes, such as infant health measures including birth weight and Apgar score, are presented in Appendix Figure A10; however, the estimated coefficients for these outcomes are not statistically significant.

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 potential bias in the TWFE DID estimates.[28]

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) components. This approach allows for the evaluation of how much weight is being placed on "forbidden" comparisons of already-treated units and how removing these comparisons would affect the estimate. Following this suggestion, 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 9.[29] The decomposition reveals three main components: First, for 2x2 DID components where never-treated counties serve as control groups, the weight is 0.328 and the estimate is -0.006. Second, for 2x2 DID components where early-treated counties are treatment groups and later-treated counties are control groups, the weight is 0.330 and the estimate is -0.001. Third, for the 2x2 DID component where later-treated counties are treatment groups and early-treated counties are control groups, the weight is 0.342 and the estimate is 0.003. These results suggest that the problematic 2x2 DID (where later-treated counties are treatment groups and early-treated counties are control groups) biases down the negative effects of bank deregulation on fertility rates. This finding is consistent with the observation that the negative effects based on the CSDID estimates are approximately three times larger compared to the TWFE estimates (-0.007 vs. -0.002). Consequently, these results support the adoption of CSDID as the main estimation method for more reliable results. A similar decomposition analysis for the maternal age outcome variable is available upon request.

In addition to the CSDID model, several recent papers have proposed various approaches to address the biases arising from the bad comparison problem inherent in Two-Way Fixed Effects (TWFE) DID regressions. While the literature has not yet settled on a standard method, these proposed solutions all modify the set of effective comparison units in the treatment effect estimation process, differing primarily 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.[30] 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

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

[29]These results are generated using the STATA command `bacondecomp`, without including any covariates in the Bacon decomposition analysis, as suggested in Goodman-Bacon (2021).

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

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.[31] To ensure robustness, in addition to CSDID, I present the event study results generated using the methods proposed by de Chaisemartin and D'Haultfoeuille (2020); Borusyak et al. (2022); Sun and Abraham (2021) in Figure 10. Notably, the main results remain largely consistent across these different estimation methods.

6.4 Heterogeneity Effects

This section explores the heterogeneous effects of deregulation based on demographic characteristics such as the mother's race, birth cohort, marital status, and birth order. Table 4 presents the estimation results for fertility rates. Columns (1) and (2) show the negative effect is evident among both white and non-white females, with a slightly stronger coefficient for non-white females (-0.009) compared to white females (-0.007). Columns (3)-(5) show the effect is primarily driven by mothers born between 1970 and 1985, with an estimated coefficient of -0.009 (significant at 1% level), while females born in the 1950s or 1960s show a negligible and insignificant coefficient of -0.001. This difference is likely because mothers born between 1970-1985 were in their prime reproductive age during the 1990s and had more flexibility to adjust their fertility plans in response to deregulation. This hypothesis is supported by evidence that the negative effect on fertility is mostly concentrated among mothers in their 20s (see Appendix Figure A5). Columns (6)-(7) shows that fertility reduction is more pronounced for unmarried women (-0.005) than for married women (-0.003). Columns (8)-(10) show that fertility rates have declined similarly for first, second, and higher-order births, indicating that the negative effect stems from both a decrease in initial childbearing and a lower propensity to have more children. Figure A3 displays the event study graphs of these heterogeneous effects on fertility rates.[32]

Regarding maternal age, the results in Table 5 are largely consistent with the fertility rate findings. Deregulation had a stronger positive impact on White females (0.252) than on non-White females (0.129). The effect is more pronounced for females born between 1970-1985, who experienced a significant increase in maternal age of 0.123 at the 1% level. In contrast, females born in the 1950s or 1960s saw slight, non-significant decreases in maternal ages (-0.027 and -0.016, respectively). Deregulation increased maternal age more for unmarried females (0.272) than for married females (-0.043). The effect on maternal age by birth order shows larger

[31] 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.

[32] For analyses involving the mother's race and birth cohort, fertility rates were calculated as the number of births among females in specific demographic groups. For marital status and birth order analyses, fertility rates were calculated as the number of certain types of births among females in the total sample.

increases for the second (0.239) and higher-order births (0.294) compared to first births (0.199). Figure A4 displays the event study plots of these heterogeneous effects on maternal age.

7 Discussion of Mechanisms

The theory proposes two potential explanations for the observed reduction and delay in fertility resulting from increased credit supply. The first is the housing cost effect: expanded credit supply drives up house prices, potentially discouraging individuals from having more children due to increased housing expenses. The second is the labor market effect: local credit supply expansion stimulates economic growth and job creation, encouraging women to prioritize career pursuits over motherhood, thereby reducing and delaying fertility.

To assess the relative importance of these two effects, I have designed and conducted three empirical tests. These tests aim to disentangle the housing cost effect from the labor market effect and determine which plays a more significant role in shaping fertility patterns in response to credit supply changes. It's worth noting that while the negative impact of banking deregulation on fertility rates is evident, this doesn't entirely rule out other potential effects of credit expansion, such as enhancing housing wealth, easing liquidity constraints, and raising household income. However, the observed fertility trends suggest that these potential benefits are not the primary drivers of the negative relationship between banking deregulation and fertility rates. The following sections will detail the methodology and results of these empirical tests, providing insights into the dominant mechanism through which credit supply affects fertility decisions.

7.1 Test I: Local Housing and Labor Market Variables as Outcomes

First, to assess the relative importance of housing cost and labor market channels in explaining the observed negative fertility effects, I employed the CSDID approach to examine how deregulation affects housing and labor market outcomes at the county level. The results, presented in Table 6, offer compelling insights into these mechanisms. Column (1) of Table 6 reports the impact on county-level FHFA house price index, showing that deregulation leads to significantly higher house prices, with the coefficient significant at the 1 percent level.[33]

Columns (2) through (5) examine the impact of deregulation on local labor market outcomes, using county-level data from the QCEW to measure growth in employment and wages. Columns (2) and (3) consider all industries, while columns (4) and (5) focus on female-dominated indus-

[33]This effect is more pronounced and significant in areas with less land availability (0.042 and 0.05) as reported in Table 7 columns (5) and (6). These findings are in line with those of Favara and Imbs (2015). Following Favara and Imbs (2015), I also use the Home Mortgage Disclosure Act (HMDA) database to test the impact of the banking deregulation on the number of mortgage loans originating at the county level and confirm the positive relationship between deregulation and loan volume. These results are shown in Appendix Figure A8.

tries such as education and health services, leisure and hospitality, and financial industries. Notably, the results show no significant relationship between deregulation and labor market outcomes, regardless of industry.[34] These findings align with Célerier and Matray (2019), who also noted that bank deregulation in the 1990s had a relatively limited impact on income and economic growth. While earlier studies documented the effects of bank deregulation on income and economic growth in the 1970s and 1980s, this paper and Célerier and Matray (2019) focus on 1990s deregulation when the financial market was more integrated, potentially explaining the differing results. Appendix Figure A6 provides event study plots of these estimates using county-level housing and labor market variables as outcomes. The results in Table 6 suggest that the housing market channel, rather than the labor market channel, is more likely to be a major driver of the negative fertility effects.

To further test the relative importance of these channels, I included log changes in house prices, employment, and wages as additional control variables in the baseline regression model sequentially (Appendix Table A3). The fertility effects are substantially reduced and become insignificant when log changes in house prices are included (column 2). In contrast, the fertility effects remain largely unchanged and become slightly larger when log changes in employment and wages are included (columns 3 and 4). These additional tests further confirm that the housing cost channel, rather than the labor market channel, is the main driving force behind the fertility effects found in this study.

7.2 Test II: Local Housing Supply Elasticity (Land Availability)

The impact of credit expansion on housing and labor markets varies across geographic areas, providing a unique opportunity to assess the relative importance of these channels in affecting fertility. A key insight here is that the housing price effects largely depend on local housing supply elasticity. In regions with elastic housing supply, an increase in credit availability and relaxed lending standards primarily stimulate housing demand. This demand increase is met with a corresponding supply response, resulting in minimal increases in housing prices. In contrast, areas with inelastic housing supply experience a markedly different outcome. Here, the same expansion in bank credit leads to a significant surge in housing prices, as the limited supply cannot easily adjust to meet increased demand. Crucially, while housing effects vary with supply elasticity, labor market effects are expected to remain relatively uniform across different areas. This divergence in effects across geographic regions provides a valuable tool for distinguishing between housing and labor market channels in their influence on fertility.

To leverage this geographic variation, I divided the sample into counties with elastic or inelastic housing supply. The elasticity of housing supply was proxied using the percentage of

[34] I also explored the growth in employment and wages by major industries categorized in the QCEW and found that those outcomes are insignificant across major industries.

developable land in each county, drawing on recently compiled topological data by Lutz and Sand (2019).[35] This approach is based on the logic that if housing costs are a significant channel, fertility effects should vary across counties with different levels of housing supply elasticity. Conversely, if the labor market channel is more relevant, fertility effects should remain relatively consistent across these countries. This test is based on the following idea, if housing costs significantly influence fertility, we should observe varying fertility effects across counties with different levels of housing supply elasticity. Conversely, if labor market factors are the primary driver, fertility effects should remain relatively consistent across these counties, regardless of their housing supply elasticity.

Table 7 shows fertility rate changes are more pronounced in counties with less developable land (inelastic supply), showing a decrease of 0.009 percentage points, compared to a decrease of 0.003 percentage points in counties with more developable land (elastic supply). Similarly, maternal age changes are more significant in counties with less developable land, with an increase of 0.164 years, compared to an increase of 0.084 years in counties with more developable land. These results demonstrate a clear pattern: the effects of deregulation on both fertility rates and maternal age are substantially more pronounced in counties with less developable land, where housing supply is more inelastic. The dynamic nature of these effects is further illustrated in Figure A7, panels (a) and (b). These graphs visually reinforce the finding that counties with less developable land experience more significant changes in both fertility rates and maternal age following deregulation. The marked difference in outcomes between areas with elastic and inelastic housing supply provides strong support for the housing cost channel as the primary mechanism through which deregulation affects fertility.

These findings underscore the importance of considering local housing market conditions when assessing the impact of financial deregulation on demographic outcomes. The results suggest that the relationship between credit availability and fertility is mediated significantly by the housing market, with the strongest effects observed in areas where increased credit is most likely to translate into higher housing costs.

7.3 Test III: Instrumental Variable Approach

To further confirm the housing cost channel, I show that the interaction of bank branching deregulation and local land availability serves as a valid instrument for house price growth to fertility outcomes at the county level. This instrumental variable approach reveals that branching deregulation can account for the rise in housing prices in areas with less available land, and consequently, explain a significant portion of the observed decline in fertility rates and increase in maternal age.

[35] The results are robust to different cutoff values and are available upon request.

The results of this instrumental variable analysis, presented in Table 8, provide compelling evidence for the housing cost channel. It shows that a one percent increase in house price is associated with a decrease in the fertility rate of about 2 percentage points. This translates to a 3 percent reduction in the fertility rate or a decrease of 2 births per 1,000 women aged 15-44. Meanwhile, the same one percent increase in house prices corresponds to an increase in maternal age of 0.047 years, equivalent to a 1.5 percent rise in maternal age. These magnitudes are consistent with the main results, where banking deregulation increased house prices by 4 percent, implying an 8 percentage point decline in the fertility rate (compared to the 7 percentage point decrease reported in Table 2).

The strength of this instrumental variable approach is underscored by the F-statistics, which range from 9.35 to 43.43. These values indicate that the instrument is sufficiently strong, lending credibility to the results. The robustness of these findings provides strong support for the hypothesis that rising housing prices serve as a crucial mechanism in explaining both the decrease and delay in fertility observed following banking deregulation.

8 Estimation Results Using the SIPP

The Natality birth data provides valuable information on fertility outcomes, allowing us to estimate the causal effects of bank deregulation on fertility and examine potential mechanisms at the county level. However, this data lacks information on mothers' housing tenure and labor market status, limiting our ability to directly test the relative importance of labor and housing market channels in explaining the observed fertility effects.

To complement the main results and further explore the effects and mechanisms of bank branching deregulation on fertility at the individual level, I use data from the Survey of Income and Program Participation (SIPP) 1990-2004 panels. The SIPP is a nationally representative survey that follows samples of 20,000 to 46,000 households and their members for 2 to 4 years. The SIPP data provides rich information on demographics, employment, income, and housing tenure at both household and individual levels. This allows me to confirm the main fertility effects and test different mechanisms. The fertility outcome is measured by a dummy variable indicating whether a female gives birth in a given year (defined as having a child less than one-year-old). While the SIPP data offers additional insights, it's important to note that it wasn't specifically designed to estimate fertility, which may affect its reliability for this purpose. Consistent with the main analysis, I restrict the sample to females aged 15-44 who were born between 1950 and 1985. The final sample covers 1990-2006 and includes 180,215 females. The summary statistics of this sample are presented in Table A4 which are largely comparable to mothers in the Natality data.[36]

8.1 Main Fertility Effects

Table 9 presents the main fertility effects of bank branching deregulation using the SIPP sample, employing a CSDID estimation model similar to the main regression in section 5. All regressions include individual and household controls such as age, race, education levels, and marital status and economic and policy controls such as state-level unemployment rates. Column (1), using the total sample, shows a negative and significant fertility effect with an estimated coefficient of -0.009, significant at the 5% level. Columns (2) and (4) divide the sample by mother's birth cohort. Consistent with the main results, the negative effect on fertility is driven by mothers born in 1970-1985, with an estimated coefficient of -0.010, significant at the 5% level. In contrast, the fertility effect among mothers born in the 1950s or 1960s is negligible and insignificant, with coefficients ranging from 0.001 to 0.00.

8.2 Discussion of Mechanisms

To further examine the housing cost and labor market channels, I use the SIPP Sample to replicate Test II from the main analysis, dividing the sample into areas with less or more available land. Due to the lack of county information in the public SIPP version, I define land availability at the state level using new topological data from Lutz and Sand (2019) as a proxy for local housing supply elasticity. Table 10, columns (1) and (2), show that the negative fertility effect is significant in states with low land availability (-0.012) but insignificant in states with high land availability (-0.004). These results align with the main findings, suggesting that the housing cost channel is more relevant than the labor market channel in explaining the fertility effects.

The SIPP dataset also allows for direct measurement of labor market variables as outcomes, enabling a test of the labor market channel. In Table 10, columns (6)-(10), I explore this channel using female labor force participation, monthly hours worked, unemployment, monthly wage, and household income as dependent variables. I find no significant impacts on these outcomes.[37] These results are consistent with the county-level findings and suggest that the labor market channel is not the primary driver of the relationship between bank branching deregulation and fertility.

[36] Another potential data source is the Census and American Community Survey (ACS). However, this dataset lacks annual data between 1990 and 2000, which is the critical period when bank deregulation occurred. This gap makes it challenging to accurately capture the immediate effects of deregulation.

[37] The effects on household borrowing variables are not statistically significant (results not reported but available upon request), indicating that the financial market channel may be less relevant in this context.), suggesting that the financial market channel plays a less prominent role in this context.

8.3 The Role of Housing Tenure and the Purchase Timing

Previous studies have emphasized the role of housing tenure in explaining the relationship between housing price dynamics and household fertility decisions (Dettling and Kearney, 2014).[38] Typically, rising housing prices are expected to have more pronounced negative effects on renters, who face greater housing cost uncertainty and affordability constraints, compared to homeowners, who may benefit from increased housing wealth. However, categorizing households solely as homeowners or renters may overlook the crucial role of home purchase timing, particularly in the context of this paper. For instance, households that purchased homes before deregulation are more likely to experience substantial housing wealth accumulation. Conversely, households planning to purchase or those who have recently bought a home face higher housing costs and potentially smaller wealth effects.[39]

Given the significant roles of housing tenure and purchase timing, I construct two key variables. First, a dummy variable for housing tenure indicates whether the female is a homeowner or renter. Second, I create a dummy variable for homeowners that takes the value 1 if the individual purchased their house before 1994 (prior to bank branching deregulation) and 0 otherwise. The SIPP dataset enables the construction of these variables, as it provides current housing tenure information in the core module and house purchase years in the topical module. Using these variables, I examine how bank deregulation influences fertility outcomes across different housing tenure groups. Table 10, columns (3) and (4), reveals that both homeowners and renters experienced lower fertility rates following bank deregulation. The effect is stronger for homeowners (-0.015) compared to renters (-0.009). Among homeowners, we observe a more pronounced negative effect for those who purchased houses after deregulation (-0.023), while those who bought before deregulation show an insignificant effect (0.006). These findings suggest that the increase in housing costs outweighs the increase in housing wealth, leading to lower fertility even among renters and homeowners. The results underscore the importance of considering both housing tenure and the timing of house purchases when analyzing the impact of financial deregulation on fertility decisions.

These results align with heterogeneity effects across female birth cohorts. The data reveal distinct patterns in homeownership rates and average purchase years. The 1950-1959 cohorts had an average homeownership rate of 72% with an average purchase year of 1986, the 1960-1969 cohorts had an average homeownership rate of 63% with an average purchase year of

[38] Dettling and Kearney (2014) which finds MSA-level house prices reduced the fertility rate, particularly in areas with lower homeownership rate. Using the data from the Nationality files, I examined whether fertility effects vary by county-level homeownership rates, dividing counties into two groups based on the median rate. Appendix Figure A9 shows similar magnitude and significance of fertility effects for both groups.

[39] A more comprehensive analysis of bank deregulation's effects on homeownership and housing wealth accumulation across various demographic groups is presented in Yang (2024b) and Yang (2024a). These studies find that increased credit supply leads to greater housing wealth accumulation when the households live in states longer years after the bank branch deregulation, particularly in areas with inelastic housing supply.

1993, and the 1970-1985 cohorts had a 48% homeownership rate with an average purchase year of 1998. This trend suggests that younger cohorts were more likely to face higher mortgages due to increased housing prices, rather than benefiting from housing wealth appreciation. The negative cost effect of higher housing prices appears to outweigh any potential positive wealth effects. Conversely, older cohorts were more likely to have owned homes before deregulation, potentially benefiting from increased housing wealth. While older cohorts may have experienced housing wealth increases, many of them might have already completed their fertility plans before the deregulation took effect. Consequently, we observe the overall negative effects of bank deregulation on fertility.

These findings both complement and extend previous research on the relationship between housing costs and fertility decisions. These analysis, particularly the breakdown by the timing of housing purchase, provides strong support for the importance of housing cost effects on fertility. The results indicate that bank deregulation's negative impact on fertility is largely driven by the housing cost effect among renters and recent homeowners. This suggests that deregulation decreases the willingness to have children by increasing housing costs for these groups. However, we must interpret these results with caution, recognizing that housing tenure and purchase timing may be endogenous to fertility decisions and that housing choices and fertility decisions are often made jointly.[40] Given these considerations, future research should focus on disentangling the joint choice of housing tenure and fertility decisions to provide a more comprehensive understanding of these dynamics.

9 Conclusion

This paper proposes a new explanation for declining fertility rates. I find that bank credit expansion decreases annual county-level fertility rates by 10 percent and increases the average maternal age by 0.75 percent, after addressing the endogeneity issue of credit expansion using the U.S. interstate branching deregulation that occurred in the 1990s. I also show that the decrease and delay in fertility are mainly caused by the housing cost effects. 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.

This study reveals a significant yet often overlooked relationship between housing market dynamics and demographic trends, particularly in the context of historically low fertility rates in many developed countries. Over recent decades, nations such as the United States, Germany, Italy, Japan, and Spain have consistently recorded fertility rates below 1.5 children per woman,

[40]The relationship between housing and children can be both substitutive and complementary.

far lower than the 2.1 replacement level necessary for population stability. This persistent low fertility has profound implications, including accelerated population aging and potential decline, which in turn pose challenges for economic growth and social insurance systems.

This research demonstrates that housing affordability plays a crucial role in shaping fertility rates. Specifically, I find that credit expansion in areas with inelastic housing supply significantly contributes to the housing affordability crisis, which in turn affects fertility decisions. This finding illuminates an important mechanism through which financial deregulation and housing market policies can inadvertently influence population patterns.

Moreover, the findings in this paper highlight the complex interplay between financial deregulation, housing markets, and demographic outcomes. Policies aimed at stimulating economic growth through financial deregulation may have far-reaching and nuanced effects on family formation decisions and population patterns, mediated through their impact on housing markets. Overall, this paper advocates for an integrated policy approach that acknowledges the complex interplay between financial markets, housing affordability, and fertility. By illuminating these connections, we enhance our understanding of demographic trends and provide a basis for more effective policymaking in response to current demographic challenges.

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, given that fertility and housing decisions are often made concurrently, further investigation into their interplay and evolution over time would be valuable for future research.

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Figures and Tables

Figure 1: How Interstate Bank Branching Deregulation Affect Fertility Decision?

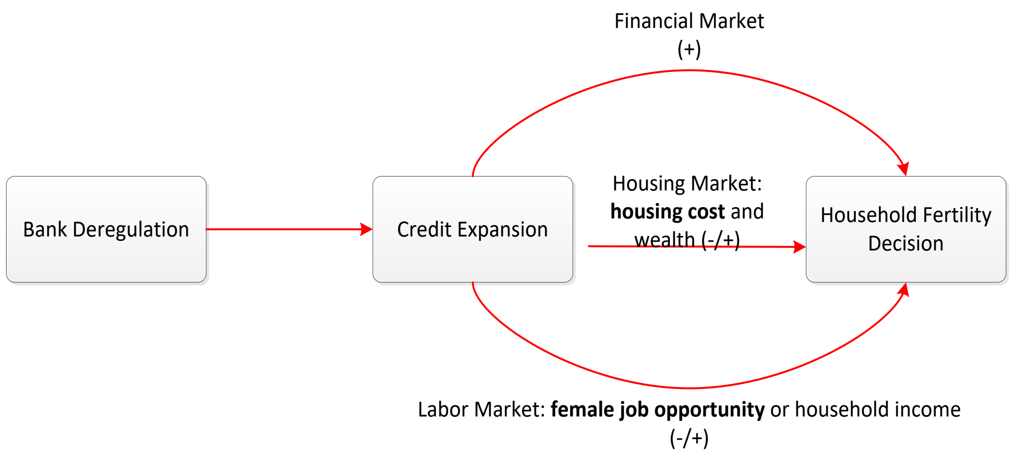
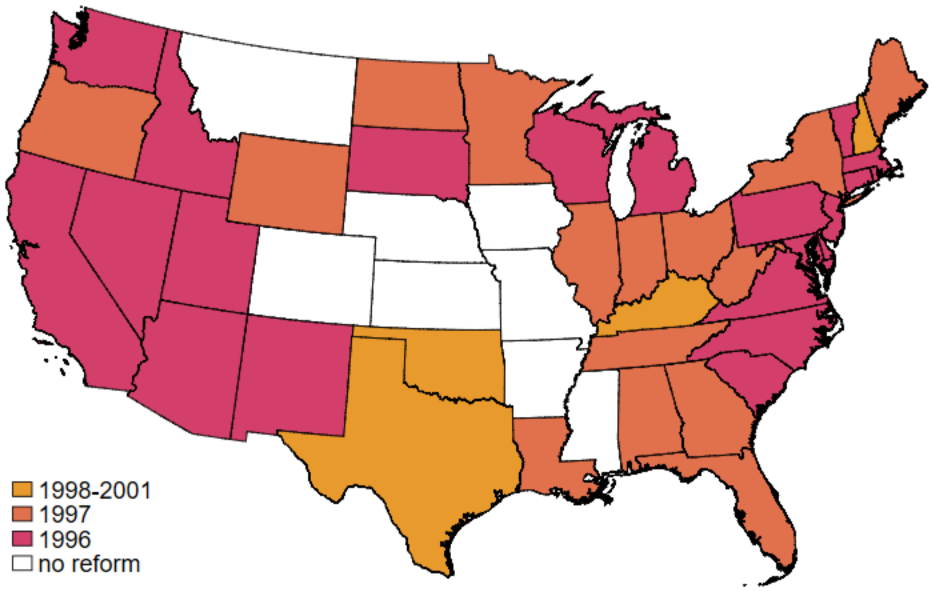
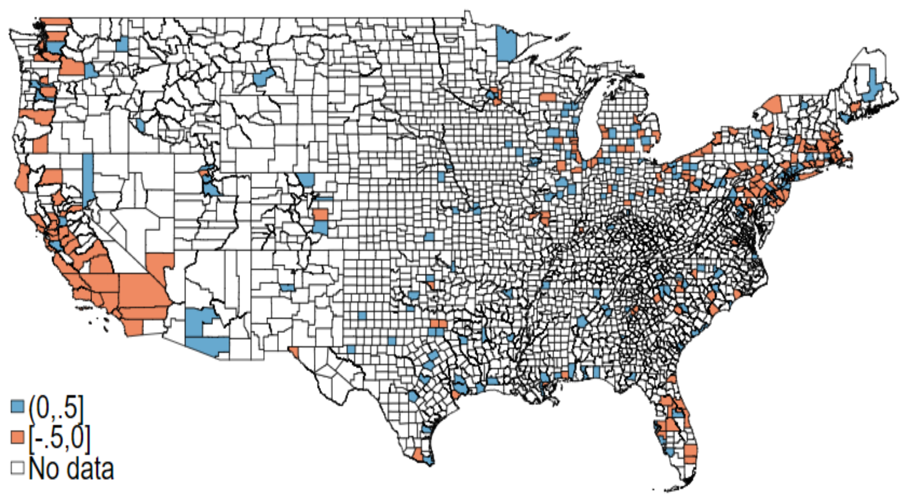


Figure 2: Timing of Interstate Bank Branching Deregulation



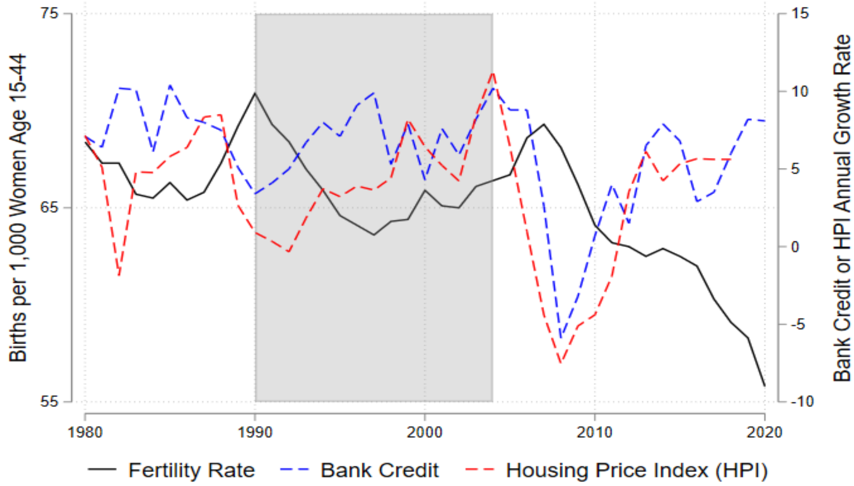
Notes: Data on interstate bank branching deregulation by state and by year come from Rice and Strahan (2010). Eight states never deregulated which include Arkansas, Colorado, Iowa, Kansas, Mississippi, Missouri, Montana, and Nebraska.

Figure 3: Fertility Rate Changes across Counties, 1990-2004



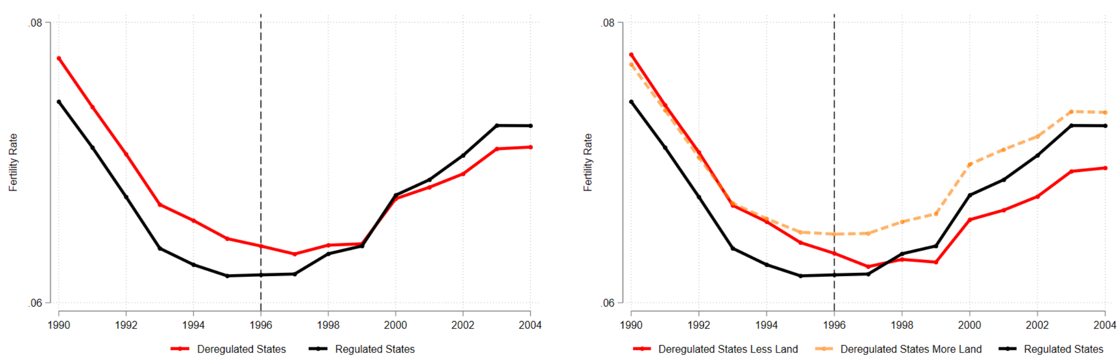
Notes: The county-level fertility rates are calculated as the number of births per thousand women ages 15-44 (CDC National Vital Natality Files) divided by the yearly population of women ages 15-44 (National Center for Health Statistics) in the county. This figure shows that 449 counties have the number of births available from 1990 to 2004.

Figure 4: National Fertility Rate, Bank Credit, and Housing Price, 1980-2020



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

Figure 5: Fertility Rate Trends in Deregulated vs. Regulated States

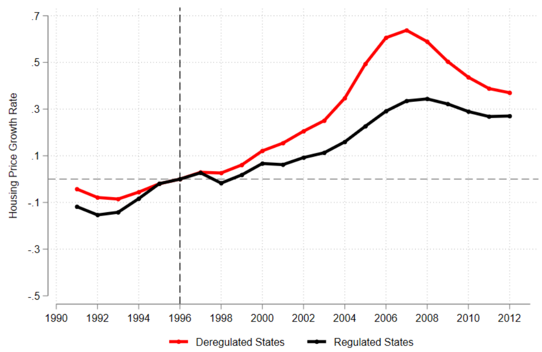


(a) Fertility Rate

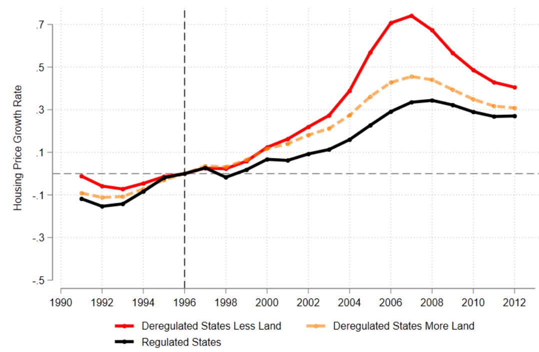
(b) Fertility Rate

Notes: The fertility rates are calculated as the number of births per thousand women ages 15-44 (CDC National Vital Natality Files) divided by the yearly population of women ages 15-44 (National Center for Health Statistics) in the state.

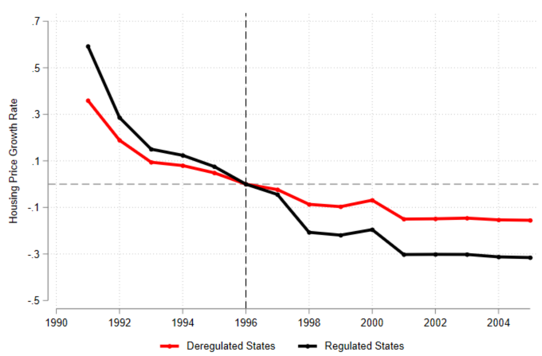
Figure 6: Housing Price, Employment, and Wage Trends in Deregulated vs. Regulated States



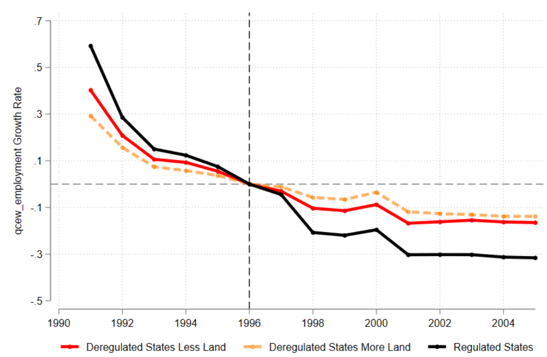
(a) Housing Price Growth Rate



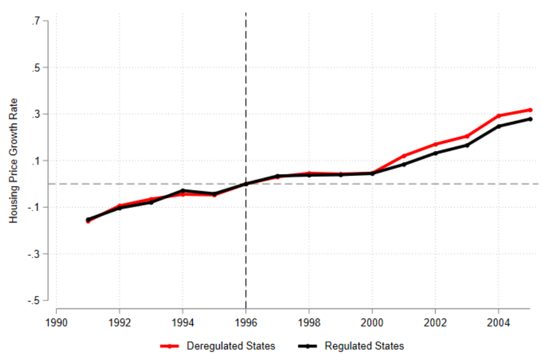
(b) Housing Price Growth Rate



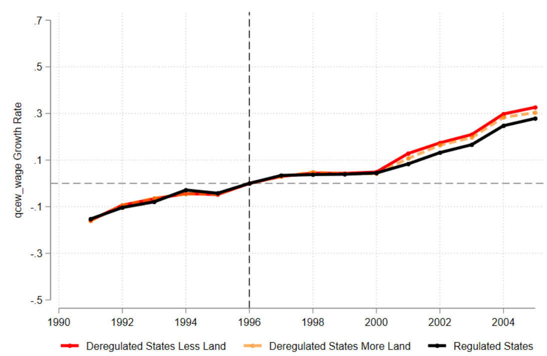
(c) QCEW Employment Growth Rate



(d) QCEW Employment Growth Rate



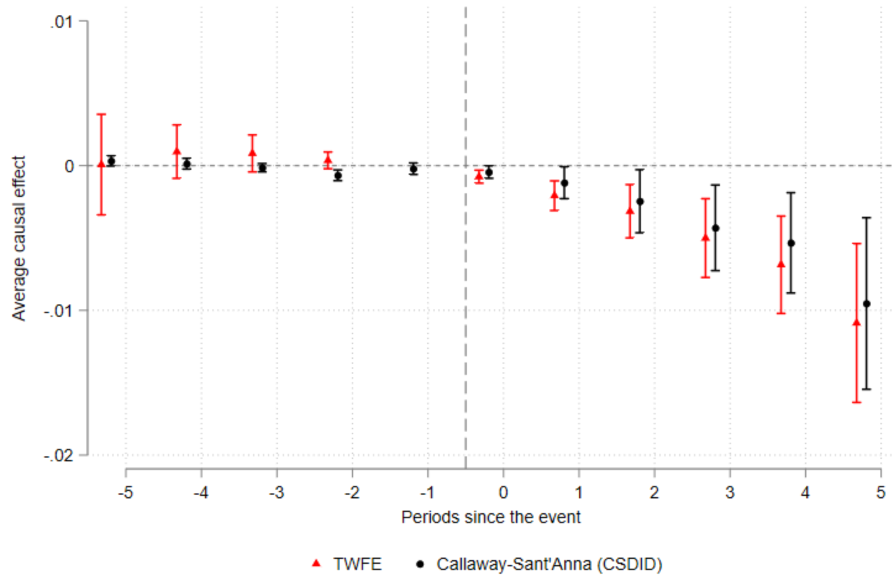
(e) QCEW Wage Growth Rate



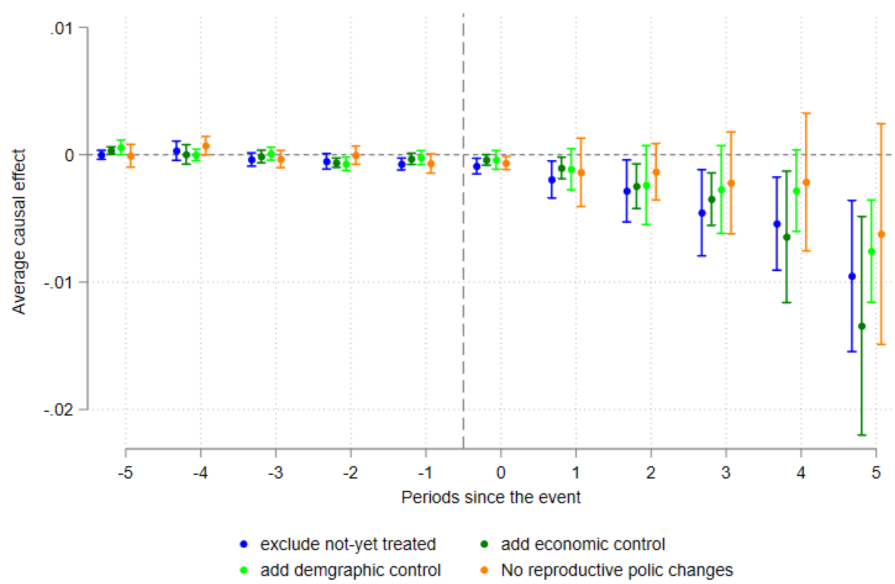
(f) QCEW Wage Growth Rate

*Notes:*House price growth comes from the Federal Housing Finance Agency Housing Price Index (HPI). Employment and wage growths come from the Bureau of Labor Statistics 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.

Figure 7: Bank Branching Deregulation and Fertility Rate: DID Results



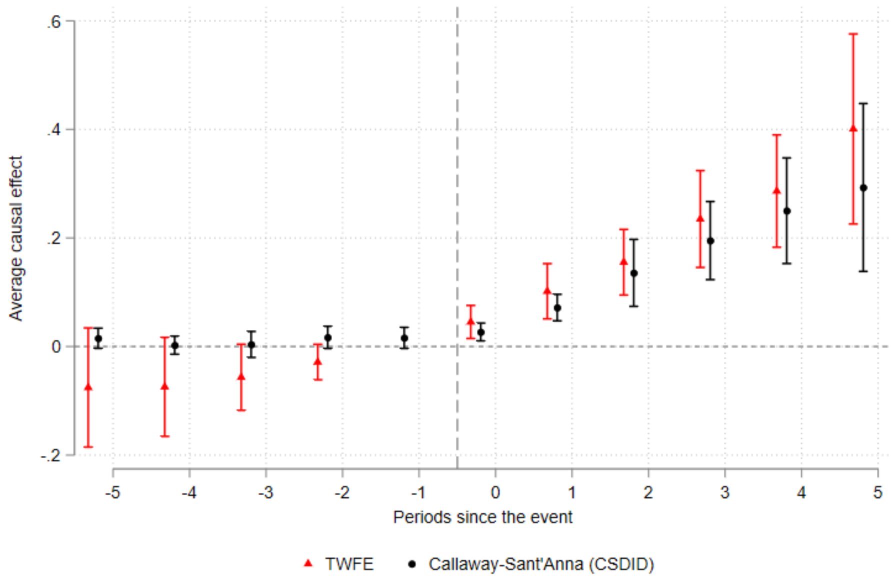
(a) TWFE and CSDID



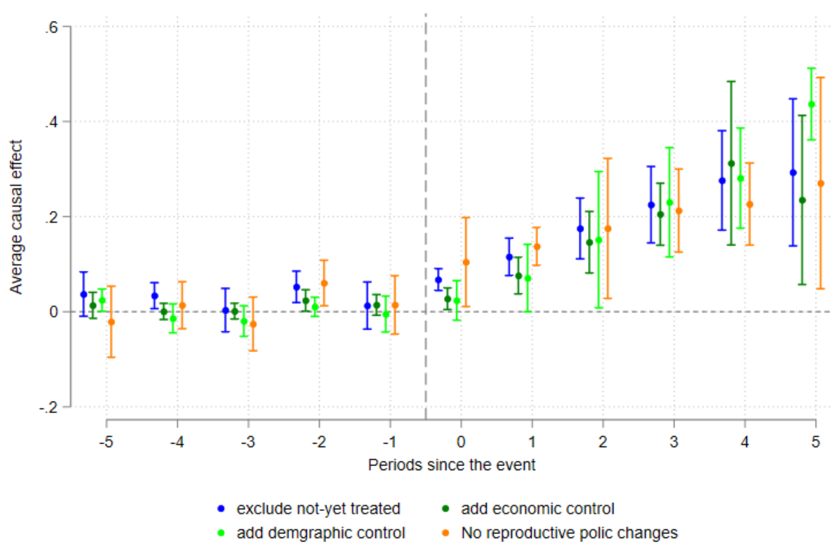
(b) CSDID: Robustness Checks

Notes: This figure plots 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. In panel (b), the economic controls include the state unemployment rate, minimum wage, and the generosity of welfare benefits. The demographic controls include county-level population shares of women aged 15-29, 30-44, non-Hispanic white women aged 15-44, non-Hispanic black women aged 15-44, and Hispanic women aged 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 8: Bank Branching Deregulation and Maternal Age



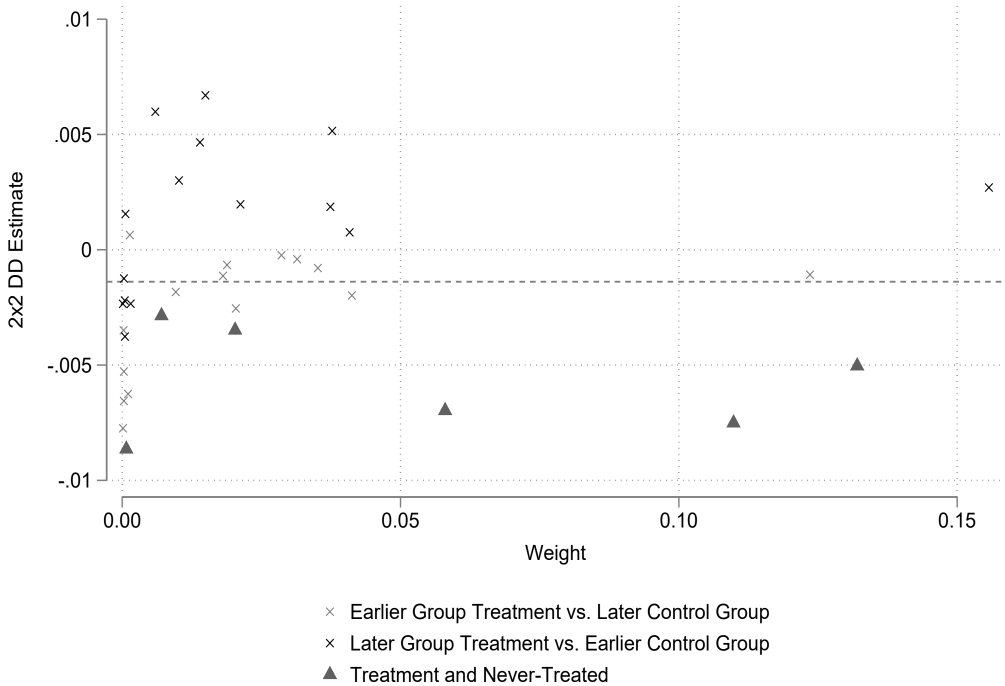
(a) TWFE and CSDID



(b) CSDID: Robustness Checks

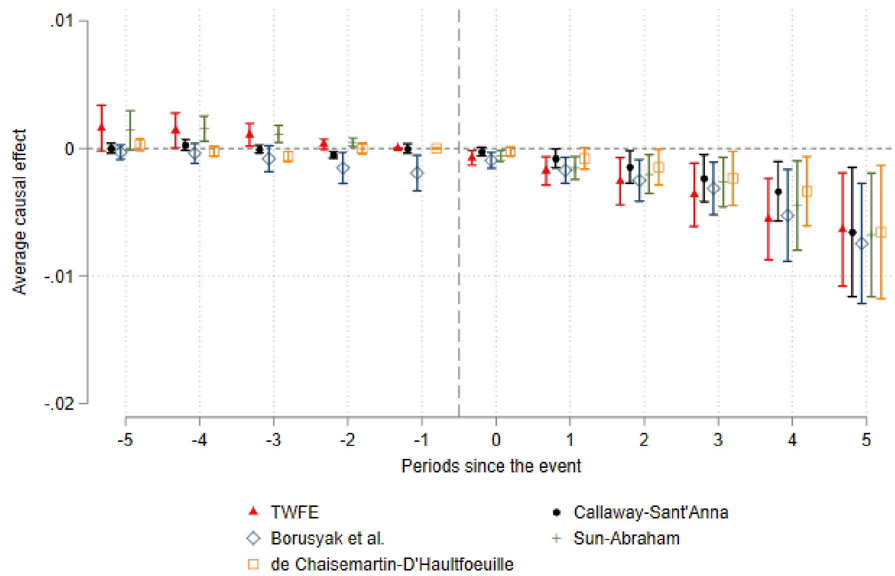
Notes: This figure plots the effect of the interstate bank branching deregulation on the county-level average of maternal age calculated based on the Vital Statistics Natality Files (449 counties between 1990 and 2004). The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. In panel (b), the economic controls include the state unemployment rate, minimum wage, and the generosity of welfare benefits. The demographic controls include county-level population shares of women aged 15-29, 30-44, non-Hispanic white women aged 15-44, non-Hispanic black women aged 15-44, and Hispanic women aged 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 9: Bacon Decomposition

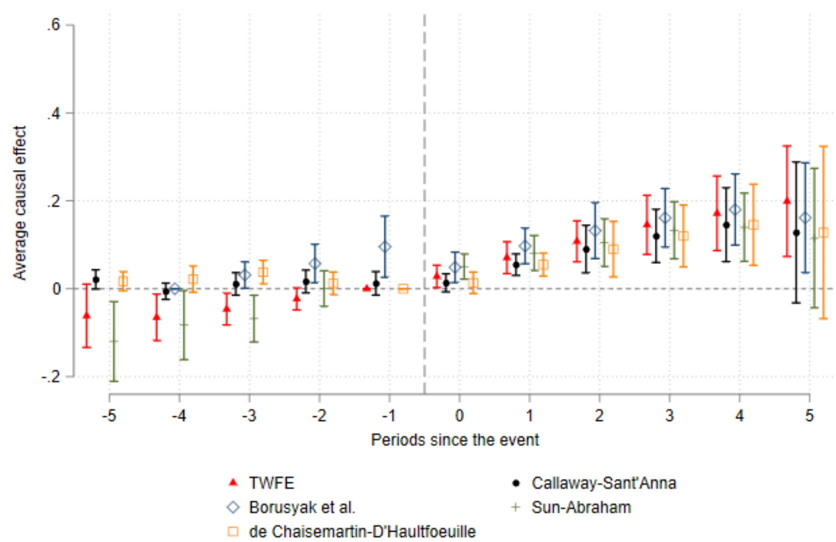


Notes: The figure plots each 2x2 DID component from the decomposition theorem against their weight for the deregulation dummy indicating whether the state has implemented interstate bank branching deregulation. For the 2x2 DID components where never-treated counties are control groups, the weight and estimate are 0.328 and -0.006; For the 2x2 DID components where early-treated counties are treatment groups and later-treated counties are control groups, the weight and estimate are 0.330 and -0.001; For the 2x2 DID component where later-treated counties are treatment groups and early-treated counties are control groups, the weight and estimate are 0.342 and 0.003. The figure notes the average DID estimate and the total weight on each type of comparison. 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 10: Bank Branching Deregulation, Fertility Rate, and Maternal Age: Alternative DID 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.

Table 1: Summary Statistics

	Treated and Control States				Stats Treated in Different Years		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Total	Never Treated	Treated	Diff.	Treated 1995 or before	Treated 1996	Treated 1997 or later
Mothers in Natality Data							
Age	26.96	26.33	27.01	-0.68***	27.28	27.53	26.68
White	0.61	0.75	0.59	0.16***	0.55	0.65	0.61
Black and Hispanic	0.33	0.21	0.34	-0.13***	0.37	0.28	0.34
Birth Cohort 1950s	0.07	0.06	0.07	-0.01***	0.07	0.08	0.06
Birth Cohort 1960s	0.40	0.36	0.40	-0.04***	0.42	0.43	0.38
Birth Cohort 1970s	0.53	0.58	0.53	0.05***	0.51	0.49	0.55
Not Married	0.31	0.30	0.32	-0.02***	0.31	0.31	0.32
Less than HS	0.22	0.19	0.22	-0.03***	0.23	0.18	0.22
High School	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
Observations	51568902						
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
Share of Women (15-29)	0.11	0.11	0.11	0.01***	0.10	0.10	0.11
Share of Women (30-44)	0.12	0.12	0.12	0.00*	0.12	0.12	0.12
Share of White Women	0.17	0.18	0.17	0.02***	0.16	0.17	0.17
Share of Black Women	0.03	0.03	0.03	-0.00	0.03	0.03	0.03
Share of Hispanic Women	0.02	0.02	0.02	-0.01***	0.02	0.02	0.02
Less Land	0.64	0.50	0.65	-0.15***	0.71	0.83	0.54
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
Observations	6735	499	6236		2132	1074	3030

Notes: The 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. The population decomposition variables are measured at the county level. Less land measures whether the county-level developable land is less than 70 percent of the total area based on satellite data collected by Lutz and Sand (2019). Treatment indicates whether the state implemented interstate bank branching deregulation. Unemployment rates, minimum wage, and the generosity of welfare benefits 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.

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

	TWFE	CSDID				
	(1)	(2)	(3)	(4)	(5)	(6)
Deregulation Dummy	-0.002***	-0.007***	-0.008***	-0.004***	-0.010***	-0.006***
	(0.000)	(0.002)	(0.002)	(0.001)	(0.004)	(0.002)
Event Study:						
Lead5event	0.000	0.000*	-0.000	0.000	0.000**	0.001**
	(0.002)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Lead4event	0.001	0.000	0.000	0.001	0.000	-0.000
	(0.001)	(0.000)	(0.000)	(0.001)	(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***	-0.001*	-0.001	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.001)	(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	-0.002***	-0.001**	-0.002***	-0.000	-0.001**	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
Lag2event	-0.003***	-0.002**	-0.003**	-0.003***	-0.002***	-0.002
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.002)
Lag3event	-0.005***	-0.004***	-0.005***	-0.002***	-0.003***	-0.003
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)
Lag4event	-0.007***	-0.005***	-0.005***	-0.003***	-0.006**	-0.003*
	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)	(0.002)
Lag5event	-0.011***	-0.010***	-0.010***	-0.015***	-0.013***	-0.008***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(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
Demographic Controls	No	No	No	No	Yes	Yes

Notes: 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 (3) excludes non-yet-treated control groups. Column (4) adds economic control variables including unemployment rates, minimum wage, and the generosity of welfare benefits measured at the state level. Column (5) adds demographic control variables. Column (6) 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	0.098**	0.204***	0.228**	0.112***	0.163***	0.325***
	(0.038)	(0.057)	(0.038)	(0.038)	(0.029)	(0.054)
Event Study:						
Lead5event	-0.076	0.015	0.037	0.013	0.013	0.024**
	(0.056)	(0.009)	(0.024)	(0.009)	(0.014)	(0.012)
Lead4event	-0.074	0.002	0.034**	0.011	0.001	-0.014
	(0.046)	(0.008)	(0.014)	(0.013)	(0.009)	(0.016)
Lead3event	-0.057*	0.004	0.003	0.018	0.001	-0.020
	(0.031)	(0.012)	(0.023)	(0.014)	(0.008)	(0.016)
Lead2event	-0.029*	0.017	0.052***	0.021	0.024**	0.010
	(0.017)	(0.010)	(0.017)	(0.017)	(0.011)	(0.010)
Lag0event	0.045***	0.027***	0.068***	0.041	0.027**	0.024
	(0.016)	(0.008)	(0.012)	(0.026)	(0.012)	(0.021)
Lag1event	0.102***	0.072***	0.115***	0.064	0.076***	0.071*
	(0.026)	(0.012)	(0.020)	(0.052)	(0.020)	(0.036)
Lag2event	0.155***	0.136***	0.175***	0.099***	0.146***	0.151**
	(0.031)	(0.031)	(0.033)	(0.038)	(0.033)	(0.073)
Lag3event	0.235***	0.195***	0.225***	0.144***	0.205***	0.230***
	(0.046)	(0.037)	(0.041)	(0.052)	(0.033)	(0.059)
Lag4event	0.286***	0.250***	0.276***	0.172***	0.312***	0.281***
	(0.053)	(0.050)	(0.053)	(0.049)	(0.088)	(0.054)
Lag5event	0.401***	0.293***	0.293***	0.192*	0.235***	0.437***
	(0.089)	(0.079)	(0.079)	(0.112)	(0.091)	(0.038)
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
Demographic Controls	No	No	No	No	Yes	Yes

Notes: 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 (3) excludes non-yet-treated control groups. Column (4) adds economic control variables including unemployment rates, minimum wage, and the generosity of welfare benefits measured at the state level. Column (5) adds demographic control variables. Column (6) 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

	Race and Ethnicity		Mother Birth Cohort		
	White	Black and Hispanic	1950-1959	1960-1969	1970-1985
	(1)	(2)	(3)	(4)	(5)
Deregulation Dummy	-0.007***	-0.009**	-0.001*	-0.001	-0.009***
	(0.001)	(0.004)	(0.001)	(0.002)	(0.003)
Test		[0.627]		[0.999]	[0.011]
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	-0.005***	-0.003***	-0.002***	-0.002***	-0.003***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Test		[0.161]		[0.998]	[0.999]
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
Demographic Controls	Yes	Yes	Yes	Yes	Yes

Notes: 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 demographic controls such as state-level unemployment rates, minimum wage, and population decomposition. Standard errors are clustered at the state level. *Test* reports p-values associated with the null hypothesis that the coefficients in this column are equal to those in the first column of this category. * p<0.10, ** p<0.05, *** p<0.01.

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

	Race and Ethnicity		Mother Birth Cohort		
	White	Black and Hispanic	1950-1959	1960-1969	1970-1985
	(1)	(2)	(3)	(4)	(5)
Deregulation Dummy	0.252***	0.129***	-0.027	-0.016	0.123***
	(0.080)	(0.043)	(0.027)	(0.024)	(0.040)
Test		[0.175]		[0.761]	[0.002]
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	0.272***	-0.043	0.199***	0.239***	0.294***
	(0.084)	(0.099)	(0.073)	(0.061)	(0.056)
Test		[0.015]		[0.629]	[0.302]
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
Demographic Controls	Yes	Yes	Yes	Yes	Yes

Notes: 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 demographic controls such as state-level unemployment rates, minimum wage, and population decomposition. Standard errors are clustered at the state level. *Test* reports p-values associated with the null hypothesis that the coefficients in this column are equal to those in the first column of this category. * p<0.10, ** p<0.05, *** p<0.01.

Table 6: Mechanism Test I: Effects of Bank Branching Deregulation on County-Level Housing and Labor Market Outcomes

	FHFA	All Industries		Female-Dominated Industries	
	House Price	Employment	Wage	Employment	Wage
	(1)	(2)	(3)	(4)	(5)
Deregulation Dummy	0.041**	0.008	-0.004	0.012	-0.004
	(0.020)	(0.005)	(0.005)	(0.017)	(0.005)
Observations	5671	6556	6556	5552	5552
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes

Notes: The outcome variable in column (1) is the log change in the FHFA house price index at the county level. The outcome in panels (2)-(5) are log changes of county-level employment and wage calculated based on the Quarterly Census of Employment and Wages (QCEW). Female-dominated industries include education and health services, leisure and hospitality, and financial industries according to the Bureau of Labor Statistics data in 2006. 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 7: Mechanism Test II: Effect of Bank Branching Deregulation on Fertility: by County-Level 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)
Test		[0.004]		[0.317]		[0.019]
Observations	4287	2448	4287	2448	3613	2058
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: 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 deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. The outcome variable in columns (5) and (6) is the log change in the FHFA house price index at the county level. Counties of $lessland = 1$ and $lessland = 0$ are defined as counties with developable land that is 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. Standard errors are clustered at the state level. *Test* reports p-values associated with the null hypothesis that the coefficients in this column are equal to those in the first column of this category. * p<0.10, ** p<0.05, *** p<0.01.

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

	Fertility Rate				Maternal Age			
	OLS (1)	OLS (2)	IV (3)	IV (4)	OLS (5)	OLS (6)	IV (7)	IV (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
Demographic Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	6137	6137	6137	6137	6137	6137	6137	6137

Notes: 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 price index which is instrumented with the interaction of interstate bank branching deregulation dummy and county-level land availability based on satellite data collected by Lutz and Sand (2019). 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: SIPP: Main Results on Fertility Results

		Mother Birth Cohort		
	Total Sample	1950-1959	1960-1969	1970-1985
	(1)	(2)	(3)	(4)
<u>Outcome: New Born</u>				
Deregulation Dummy	-0.009**	0.001	-0.004	-0.010**
	(0.004)	(0.002)	(0.016)	(0.005)
Test			[0.757]	[0.041]
Observations	180,215	48,840	81,430	49,945
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Ind and HH controls	Yes	Yes	Yes	Yes

Notes:: The sample comes from the SIPP 1990-2004 panels which consists of females ages 15 and 44 and covers the period of 1990-2006. The fertility outcome variable is a dummy variable indicating whether the female gives birth to a child that year. Column (1) includes the total sample; Column (2)-(4) divides the sample by mother's birth cohort. The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. Each regression adopts the DID model and includes individual and household controls such as age, race, education levels, and marital status as well as economic and demographic controls such as state-level unemployment rates, minimum wage, and population decomposition. Standard errors are clustered at the state level. *Test* reports p-values associated with the null hypothesis that the coefficients in this column are equal to those in the first column of this category. * p<0.10, ** p<0.05, *** p<0.01.

Table 10: SIPP: More Tests of Mechanisms

	by Land		by Housing Tenure			
	Less Land	More Land	Homeowner	Renter	Buy House after 1995	Buy House before 1995
	(1)	(2)	(3)	(4)	(5)	(6)
<u>Newborn</u>						
Deregulation Dummy	-0.011***	-0.004	-0.015***	-0.009*	-0.023***	0.006
	(0.003)	(0.008)	(0.005)	(0.005)	(0.009)	(0.006)
Test		[0.412]		[0.396]		[0.007]
Observations	101,835	78,380	104,363	58,908	52,772	51,591
	LFP	Hours Worked	Unemployed	Wage	HH Income	
	(7)	(8)	(9)	(10)	(11)	
<u>Labor Market Outcomes</u>						
Deregulation Dummy	-0.008	0.452	-0.006	-0.053	-0.573	
	(0.012)	(1.245)	(0.011)	(0.050)	(1.920)	
Observations	180,215	137,195	119,754	132,762	180,215	
State FE	Yes	Yes	Yes	Yes	Yes	
Year FE	Yes	Yes	Yes	Yes	Yes	
Ind and HH controls	Yes	Yes	Yes	Yes	Yes	

Notes: The sample comes from the SIPP 1990-2004 panels which consists of females ages 15 and 44 and covers the period of 1990-2006. The fertility outcome variable is a dummy variable indicating whether the female gives birth to a child that year. The outcome variable in columns (1)-(5) is a dummy variable indicating whether the female gives birth to a child that year. The outcome variable in columns (6)-(10) measures female labor market outcomes. The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. State of less land and more land are defined as counties with developable land that is less or more than 70% of the total areas based on satellite data collected by Lutz and Sand (2019). Each regression adopts the DID model and includes individual and household controls such as age, race, education levels, and marital status as well as economic and demographic controls such as state-level unemployment rates, minimum wage, and population decomposition. Standard errors are clustered at the state level. *Test* reports p-values associated with the null hypothesis that the coefficients in this column are equal to those in the first column of this category. * p<0.10, ** p<0.05, *** p<0.01.

Online Appendix to:

"More Credit, More Babies? Bank Credit Expansion, House Prices, and Fertility"

Xi Yang

Appendix I: More on Bank Branch Deregulation

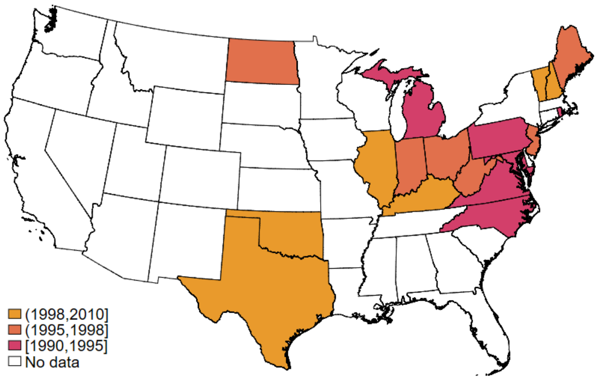
AI.1 History of Bank Branch Deregulation

Throughout the history of the United States, banks have been subject to extensive regulatory provisions, including restrictions on bank branching. The first law to limit bank branching was the National Banking Act of 1864, which required federal charters for banks and confined them to a single location. The McFadden Act of 1927 gave states the power to regulate national banks' branching within and across state borders. The Bank Holding Company Act of 1956 imposed further restrictions on multi-bank holding companies (MBHCs) and their ability to acquire banks in other states. These laws were influenced by the lobbying of small and local banks that wanted to avoid competition and the creation of local monopolies by state governments. However, this banking system was not efficient for borrowers, hampered financial development, and reduced competition, as Peltzman (1976) argued. In the 1970s, states started to relax their branching rules and allowed MBHCs to operate multiple branches. The deregulation process typically involved three stages: (1) allowing the formation of MBHCs under the unit banking system, (2) enabling MBHCs to consolidate separate banks and transform them into branches of a single bank, and (3) permitting full intrastate branching expansion. Finally, the state-level bank liberalization led to the enactment of the Riegle-Neal Interstate Banking and Branching Efficiency Act of 1994, which gave banks the freedom to branch across state lines.

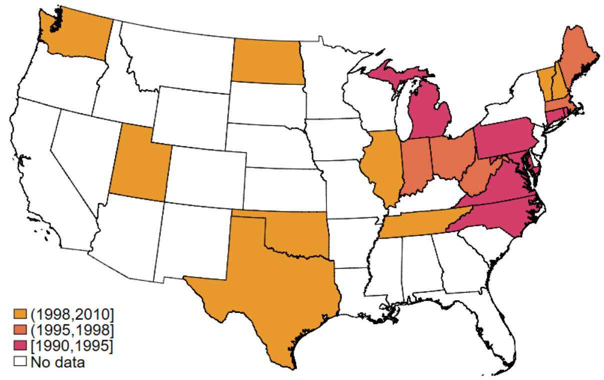
AI.2 Interstate Bank Branching Deregulation Index

The IBBEA allowed banks to operate across state borders, but it also allowed each state to impose restrictions on interstate branching. Compared with the most restricted states(non-deregulated states), 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. Rice and Strahan (2010) computes a time-varying regulation index that ranges from 0 to 4 to capture the state-level branching restrictions. Appendix Figures A1 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 deregulation dummy but not the other three, probably because this relaxation usually is implemented earlier than the other three (Appendix Figure A2). Using main results in this paper are similar using the deregulation index or the deregulation dummy, I choose the dummy as the primary measure of interstate bank branching deregulation considering it is earlier to interpret in the DID setting.

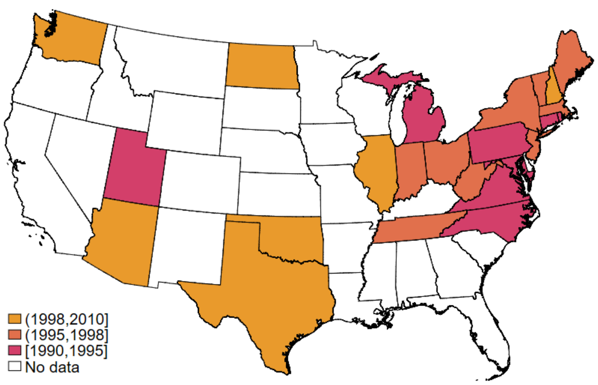
Figure A1: Maps of Bank Interstate Branching Deregulation:
Relaxation of Four Restrictions



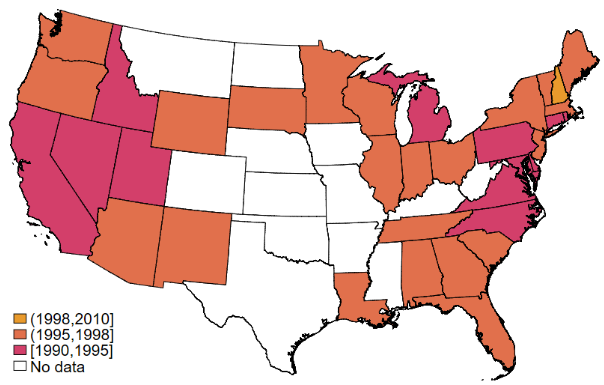
(a) Minimum Age of Targeted Banks to be 3 Years or Fewer



(b) Allow de novo Interstate Branching



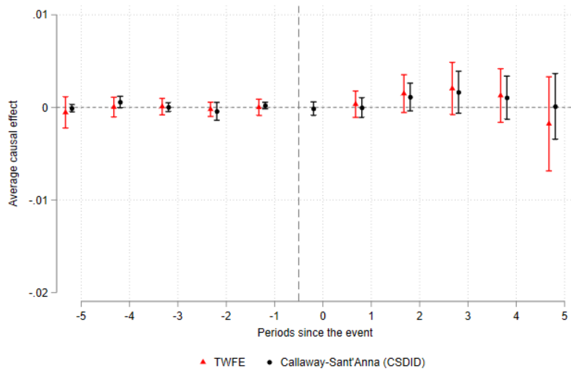
(c) Allow Acquisition of Individual Branch without Acquiring the Entire Bank



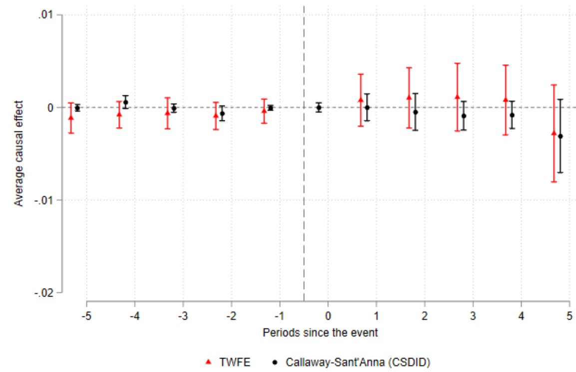
(d) Statewide Deposit Cap to be 30% or Higher

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

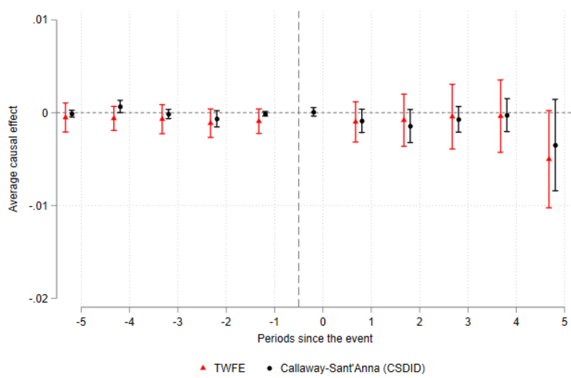
Figure A2: Bank Branching Deregulation and Fertility Rate:
Relaxation of Four Restrictions



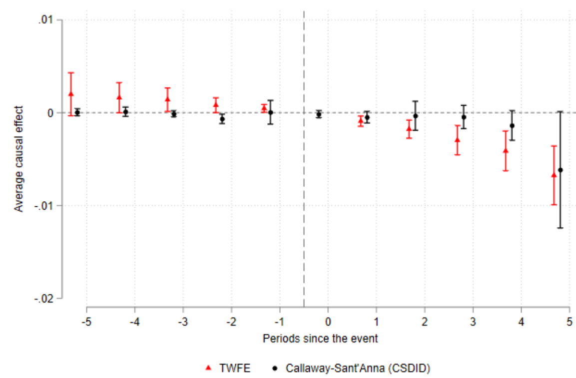
(a) Minimum Age of Targeted Banks to be 3 Years or Fewer



(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 plots the effects of the relaxation of four different restrictions regarding interstate bank branching 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 in each figure indicates whether the state has relaxed a certain type of restriction regarding interstate bank branching. Standard errors are clustered at the state level. All Figures show 95 percent confidence intervals.

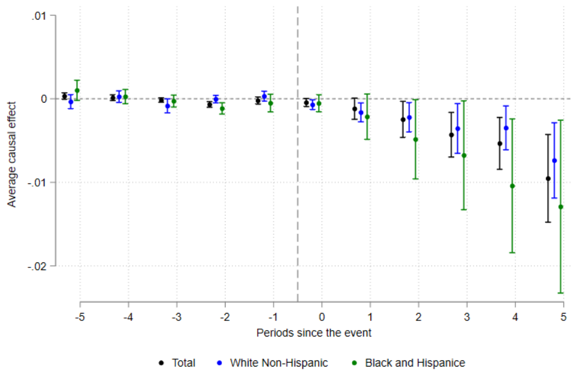
Table A1: Years of Four Types of Bank Branching Deregulation

State	Reform Timing				State	Reform Timing			
	T1	T2	T3	T4		T1	T2	T3	T4
Alabama	x	x	x	1997	Montana	x	x	x	x
Alaska	x	x	1994	1994	Nebraska	x	x	x	x
Arizona	x	x	2001	1996	Nevada	x	x	x	1995
Arkansas	x	x	x	x	New Hampshire	2002	2000	2000	2000
California	x	x	x	1995	New Jersey	1997	x	1996	1996
Colorado	x	x	x	x	New Mexico	x	x	x	1996
Connecticut	x	1995	1995	1995	New York	x	x	1997	1997
Delaware	x	x	x	1995	North Carolina	1995	1995	1995	1995
DC	1996	1996	1996	1997	North Dakota	1997	2003	2003	x
Florida	x	x	x	1997	Ohio	1997	1997	1997	1997
Georgia	x	x	x	1997	Oklahoma	2000	2000	2000	x
Hawaii	2001	2001	2001	1998	Oregon	x	x	x	1997
Idaho	x	x	x	1995	Pennsylvania	1995	1995	1995	1995
Illinois	2004	2004	2004	1997	Rhode Island	1995	1995	1995	1995
Indiana	1997	1997	1997	1997	South Carolina	x	x	x	1996
Iowa	x	x	x	x	South Dakota	x	x	x	1996
Kansas	x	x	x	x	Tennessee	x	2001	1998	1997
Kentucky	2000	x	x	x	Texas	1999	1999	1999	x
Louisiana	x	x	x	1997	Utah	x	2001	1995	1995
Maine	1997	1997	1997	1997	Vermont	2001	2001	1996	1996
Maryland	1995	1995	1995	1995	Virginia	1995	1995	1995	1995
Massachusetts	x	1996	1996	1996	Washington	x	2005	2005	1996
Michigan	1995	1995	1995	1995	West Virginia	1997	1997	1997	x
Minnesota	x	x	x	1997	Wisconsin	x	x	x	1996
Mississippi	x	x	x	x	Wyoming	x	x	x	1997
Missouri	x	x	x	x					

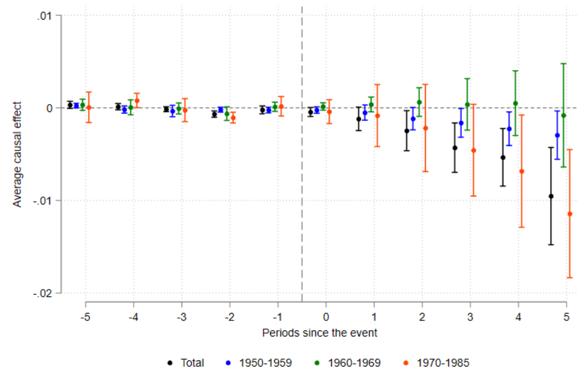
Notes: Data on interstate branching deregulation by state and by year come from Rice and Strahan (2010). The four relaxed restrictions include (T1-T4) (1) requires a minimum age of the targeted bank to be less than three years; (2) allows de novo branching without an explicit agreement by state authorities; (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%.

Appendix II: Other Figures and Tables

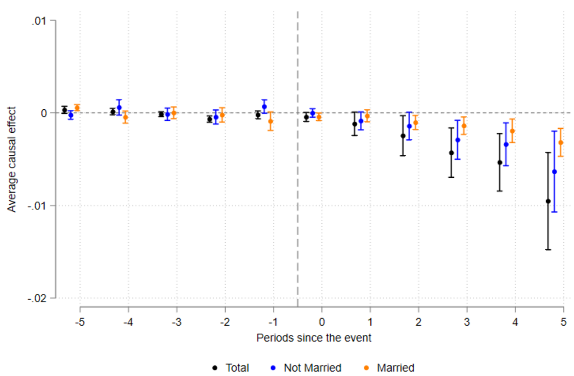
Figure A3: Bank Branching Deregulation and Fertility Rate:
Heterogeneous Effects



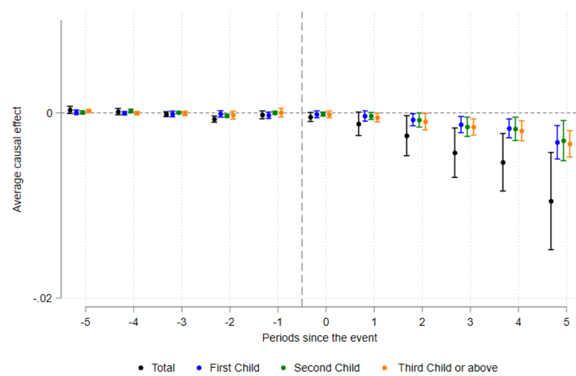
(a) Race and Ethnicity



(b) Birth Cohort



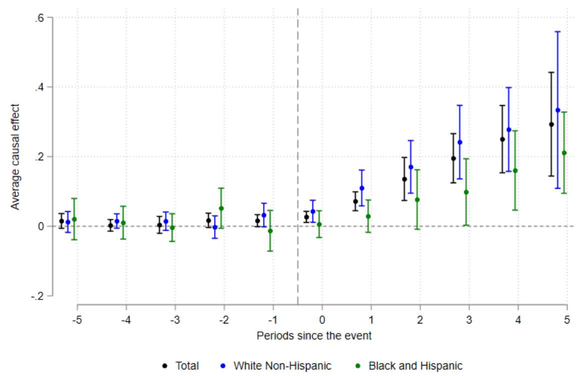
(c) Marital Status



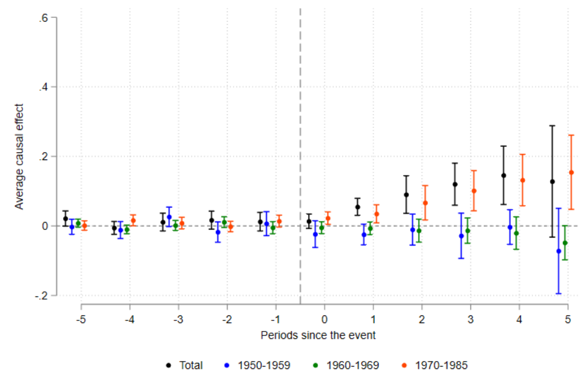
(d) Birth Order

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.

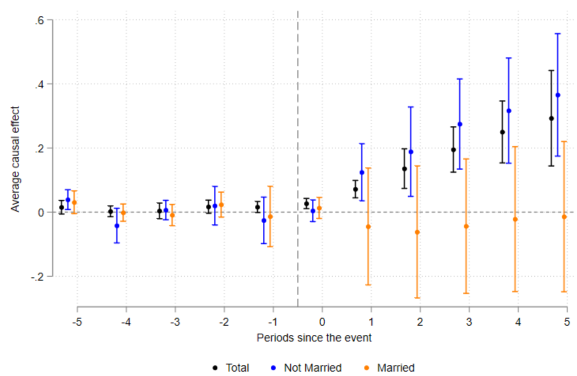
Figure A4: Bank Branching Deregulation and Maternal Age:
Heterogeneous Effects



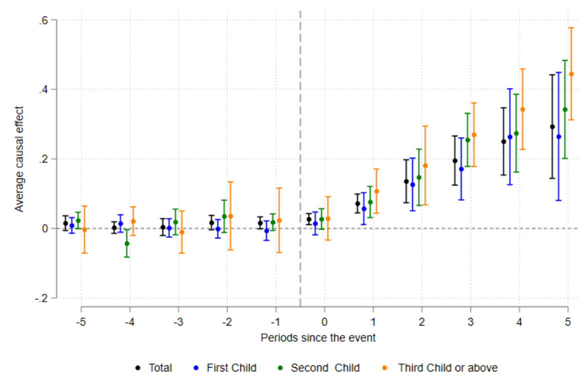
(a) Race and Ethnicity



(b) Birth Cohort



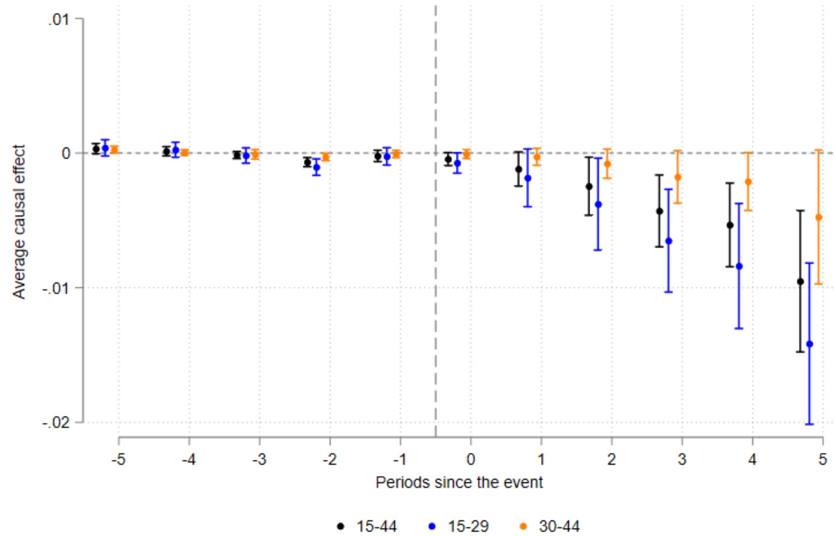
(c) Marital Status



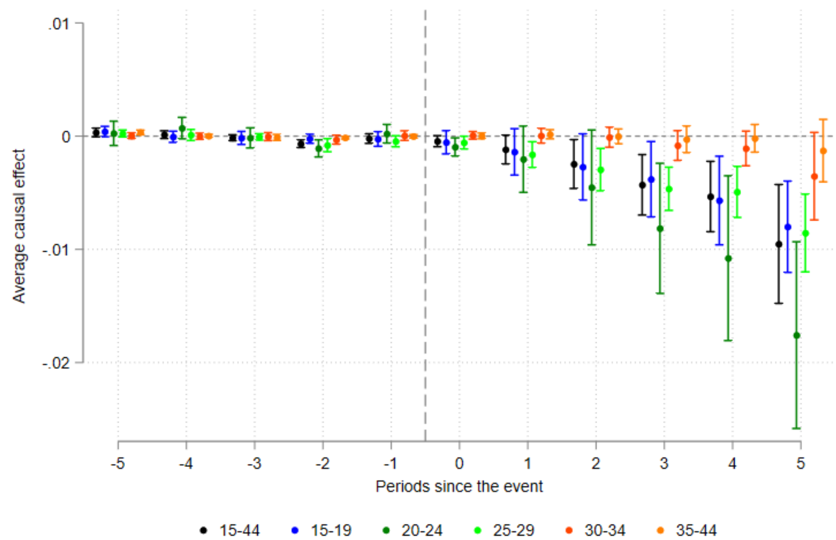
(d) Birth Order

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



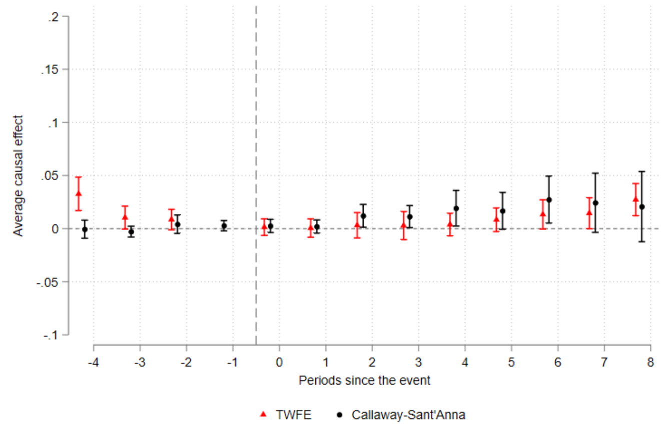
(a) Two Age Groups



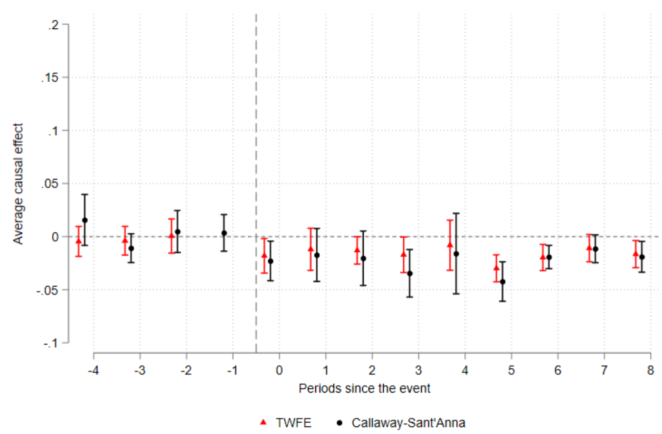
(b) Five Age Groups

Notes: This figure plots 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.

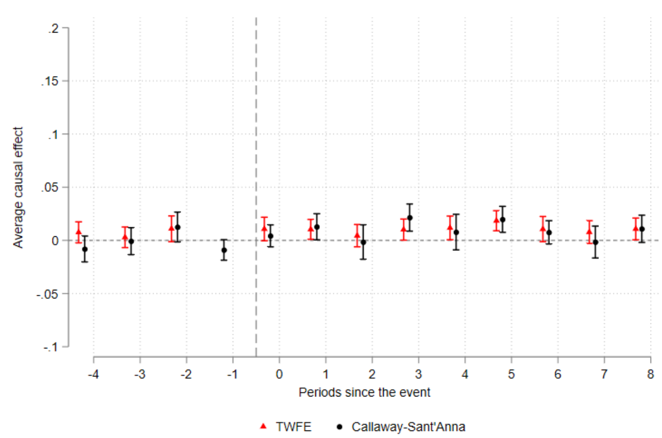
Figure A6: Mechanism Test I: Effects of Bank Branching Deregulation on County-Level Housing and Labor Market Outcomes



(a) Housing Price



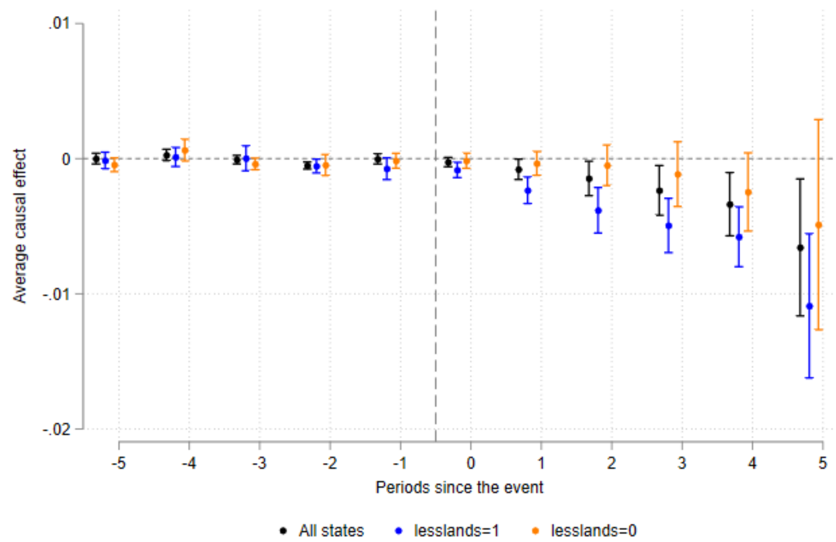
(b) Employment



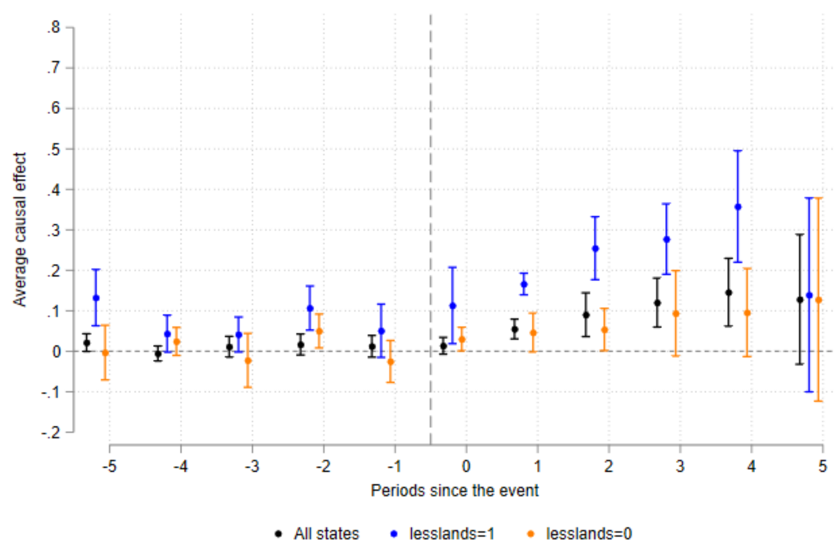
(c) Wage

Notes: The dependent variable in panel (a) is the log change in the FHFA house price index at the county level. Dependent variables in panels (b) and (c) are the log changes of county-level employment and wage calculated based on the Quarterly Census of Employment and Wages (QCEW). The deregulation dummy indicates whether the state has implemented a certain type of interstate bank branching deregulation. All Figures are event studies based on the CSDID estimates and show 95 percent confidence intervals. Standard

Figure A7: Mechanism Test II: Effects of Bank Branching Deregulation on Fertility: by County-Level Land Availability



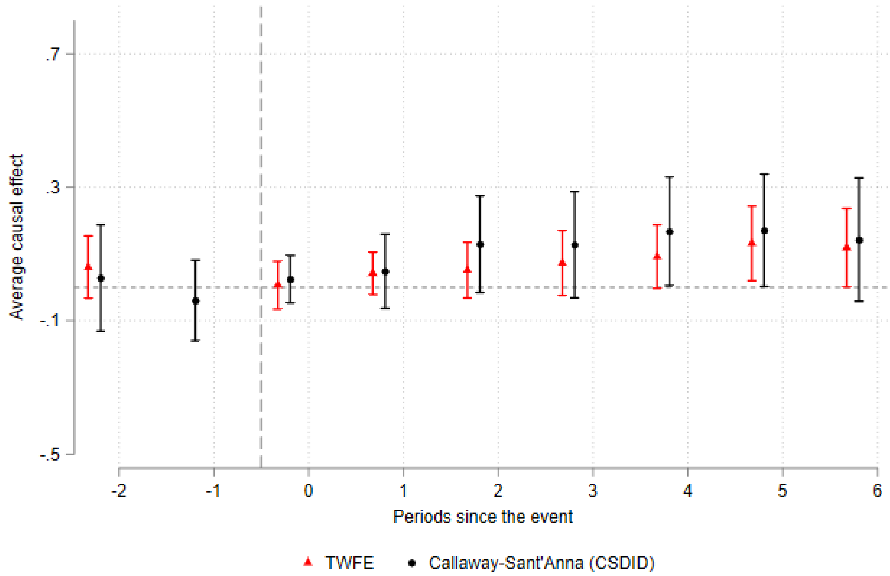
(a) Fertility Rate



(b) Maternal Age

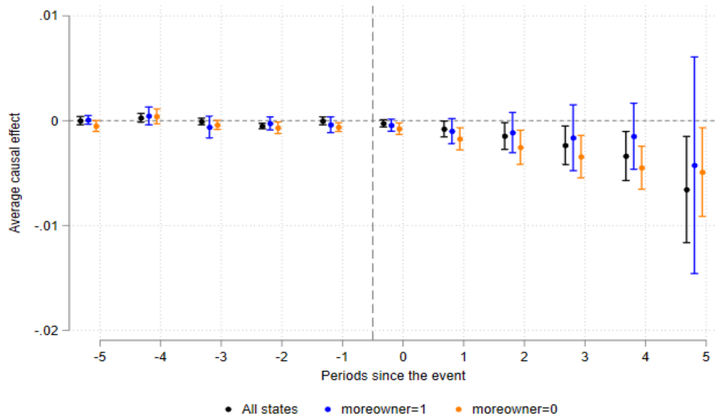
Notes: The deregulation dummy indicates whether the state has implemented a certain type of interstate bank branching deregulation. Counties of *lessland* = 1 (blue dots) and *lessland* = 0 (orange dots) are defined as counties with developable land that are less or more than 70% of the total areas based on satellite data collected by Lutz and Sand (2019). 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 and Mortgage Loans

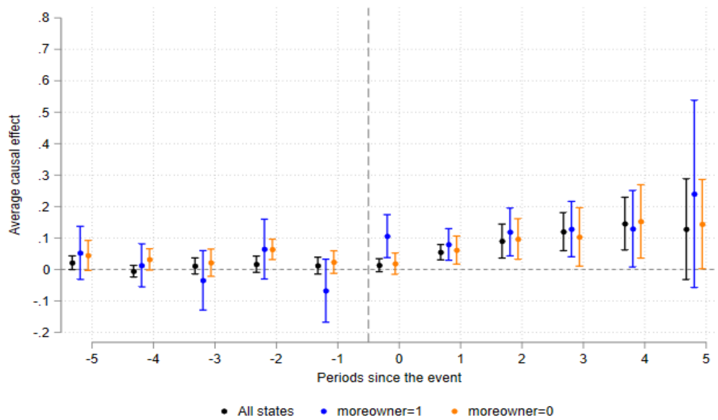


Notes: The outcome is the log number of mortgage loans at the county level calculated based on the Home Mortgage Disclosure Act (HMDA) data. The deregulation dummy indicates whether the state has implemented the interstate bank branching deregulation. All Figures show 95 percent confidence intervals.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

Figure A9: Mechanism: Effect of Bank Branching Deregulation on Fertility by County-level Homeownership rate



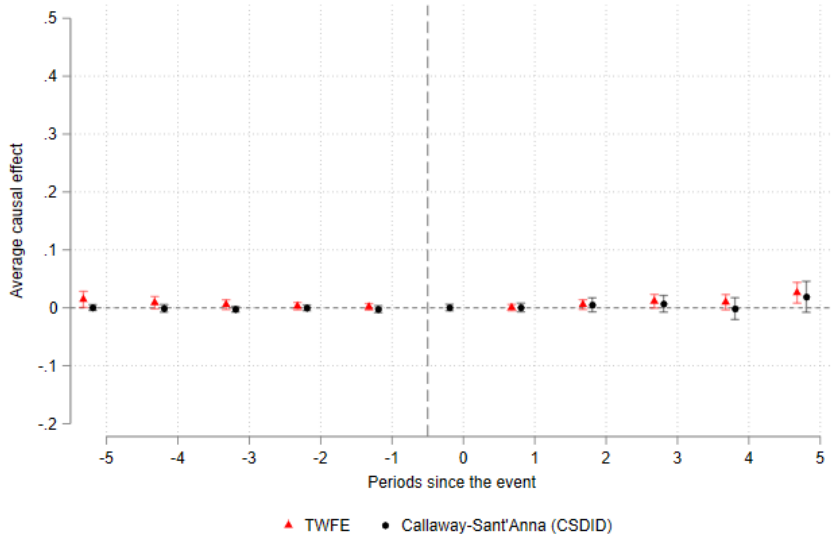
(a) Fertility Rate



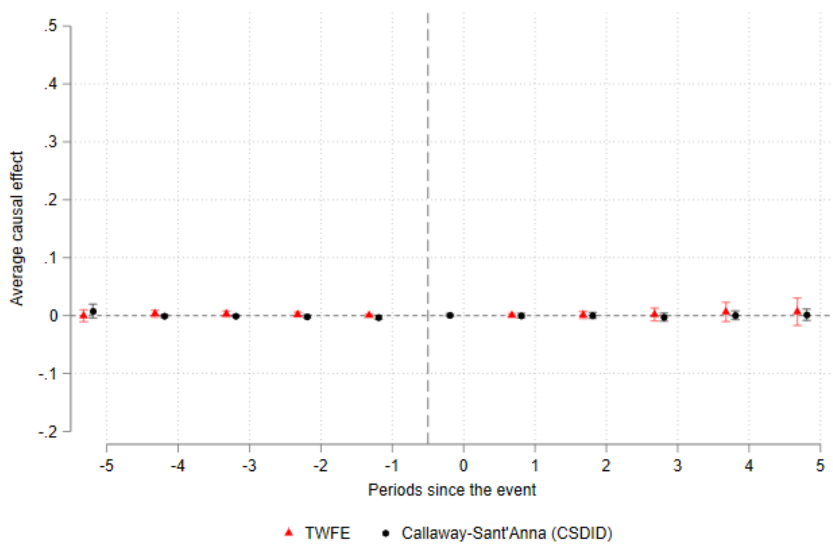
(b) Maternal Age

Notes: The deregulation dummy indicates whether the state has implemented a certain type of interstate bank branching deregulation. 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.

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



(a) Birth Weight



(b) 5-minute Apgar score

Notes: The dependent variables 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 A2: More Robustness Checks

	Baseline (1)	Excludes States (2)	State Trends (3)	Different Weights (4)
Fertility Rate				
Deregulation Dummy	-0.007***	-0.004**	-0.003**	-0.004***
	(0.002)	(0.002)	(0.002)	(0.002)
Maternal Age				
Deregulation Dummy	0.204***	0.149***	0.147***	0.119***
	(0.057)	(0.051)	(0.051)	(0.046)
Observations	6137	5671	5552	6556
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: This table presents additional robustness tests for main estimation results. Column (1) excludes California and Texas to make sure the effects are not driven by certain large states and excluding Utah to make sure the effect is not driven by effects of religious factors; Column (2) adds state-specific trends to account for different economic dynamics at the state level; Column (3) adopts weights calculated based on the national population. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01.

Table A3: More Mechanism Test: Include House Price and Labor Market Outcomes as Controls

	Baseline (1)	Add HP (2)	Add Female LM (3)	Add All LM (4)
Fertility Rate				
Deregulation Dummy	-0.007***	-0.002	-0.009***	-0.008***
	(0.002)	(0.005)	(0.001)	(0.002)
Maternal Age				
Deregulation Dummy	0.204***	0.126	0.184***	0.209***
	(0.057)	(0.111)	(0.057)	(0.066)
Observations	6137	5671	5552	6556
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: This table presents CSDID results for the fertility rate and maternal age including house price and labor market outcomes as additional controls gradually. House Price variable is the log change in the FHFA house price index at the county level. Labor market variables are log changes in county-level employment and wages calculated based on the Quarterly Census of Employment and Wages (QCEW). Female-dominated industries include education and health services, leisure and hospitality, and financial activities according to the Bureau of Labor Statistics data in 2006. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01.

Table A4: Summary Statistics of the SIPP Sample

	(1) Total	(2) Never-treated States	(3) Trated States	(4) Diff.
New Born	0.10	0.10	0.10	-0.00
Age	33.81	33.41	33.85	-0.43***
Birth Cohort 1950s	0.27	0.24	0.27	-0.04***
Birth Cohort 1960s	0.45	0.43	0.45	-0.02***
Birth Cohort 1970s	0.28	0.33	0.27	0.06***
White	0.82	0.85	0.82	0.03***
Black and Hispanic	0.21	0.14	0.22	-0.08***
Not Married	0.30	0.28	0.30	-0.01***
Less than HS	0.08	0.07	0.08	-0.02***
High School	0.45	0.47	0.45	0.02***
College	0.38	0.39	0.38	0.01*
Graduate	0.09	0.08	0.09	-0.01***
Homeowner	0.61	0.66	0.61	0.05***
Purchase Year	1993	1993	1992	-0.03***
Hours Worked	141.30	142.16	141.21	0.95**
Unemployed	0.12	0.13	0.12	0.01**
Monthly Wage	2590.42	2305.55	2619.84	-314.29***
HH Total Income	65496.34	60551.62	65986.18	-5434.56***
Less Land	0.57	0.50	0.60	-0.40***
Unemployment Rate	5.70	4.97	5.78	-0.81***
Observations	180215	16246	163969	

Notes: The sample comes from the SIPP 1990-2004 panels which consists of females ages 15 and 44 and covers the period of 1990-2006. “New Born” is a dummy variable indicating whether the female gives birth to a child that year.