Women-Centered Finance and the Health of the Next Generation: Evidence from Feminist Credit Unions[*]

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Abstract

This paper examines whether women-centered financial institutions can affect infant health and women's economic and reproductive decisions. We study 16 federally chartered feminist credit unions that opened across U.S. metropolitan areas during the 1970s and exploit variation in the timing of their openings using a two-stage difference-in-differences design. We find that feminist credit union openings increased average birth weight by 5.7 grams and reduced low birth weight by 0.11 percentage points, or 1.3 percent relative to the mean. The openings also reduced fertility by 2.9 percent, with declines concentrated among women ages 20–34 and a smaller increase among women ages 35–39, a pattern consistent with delayed childbearing. First-trimester prenatal-care initiation increased by 4.8 percentage points. Women's employment increased by 1.1 percentage points and shifted from part-time toward full-time work, while the number of women-owned businesses and paid employment in these businesses increased by approximately 10 and 17 percent, respectively. The findings show that institutions designed to expand women's financial independence can affect women's economic opportunities and reproductive decisions, with consequences extending to the health of the next generation.

Keywords: Feminist Credit Unions; Women's Economic Empowerment; Infant Health; Fertility; Financial Access; Female Employment; Entrepreneurship

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1. Introduction

Women's economic agency can shape the next generation through two closely related margins: whether and when women have children, and the health of the children they bear. A large literature shows that changes in women's reproductive autonomy, employment opportunities, and control over economic resources can alter fertility timing and family formation (Bailey, 2006, 2010, 2013; Goldin & Katz, 2002; Jensen, 2012; Schaller, 2016). At the same time, maternal economic resources and conditions during pregnancy can affect fetal growth and other measures of health at birth through changes in nutrition, healthcare utilization, stress, and employment conditions (Almond et al., 2011; Almond & Currie, 2011; Amarante et al., 2016; Hoynes et al., 2015; Lindo, 2011; Mocan et al., 2015). These two margins are closely connected because changes in fertility timing can alter the composition of women giving birth, while changes in women's economic circumstances can affect the prenatal environment conditional on a birth. Understanding whether institutions that expand women's financial independence influence both fertility decisions and infant health is therefore important for evaluating the broader consequences of women's economic empowerment.

During the early 1970s, women in the United States faced substantial barriers in conventional credit markets. Lenders often required unmarried women to obtain male cosigners, discounted or ignored married women's earnings when evaluating loan applications, and frequently failed to maintain independent credit histories for divorced or widowed women (Dumaine, 2022; Hyman, 2011; Michals, 2018). Although the Equal Credit Opportunity Act of 1974 prohibited discrimination based on sex or marital status, discriminatory practices did not immediately disappear. In response to these constraints, feminist activists established a series of feminist credit unions across U.S. metropolitan areas during the 1970s. These member-owned institutions sought to mobilize women's savings, provide credit to women and women-owned businesses, offer financial education, and promote women's economic independence (Dumaine, 2022; Mailliard & Anderson, 1987; Michals, 2018). Their openings therefore provide a distinct setting in which to examine whether expanding women's access to and control over financial resources affected not only their economic choices, but also fertility behavior and the health of the children they subsequently bore.

The effects of women-centered financial institutions on fertility and infant health are theoretically ambiguous. Greater access to credit and savings can relax liquidity constraints and increase the resources available for housing, consumption, and childbearing, channels through which broader expansions in credit and wealth have often increased fertility (Black et al., 2013; Dettling & Kearney, 2014; Lovenheim & Mumford, 2013). At the same time, financial independence can increase women's control over reproductive decisions, strengthen their outside options, and raise the returns to employment and entrepreneurship, potentially delaying marriage and childbearing or shifting births toward older ages (Ashraf et al., 2014; Jensen, 2012; Page et al., 2017; Schaller, 2016; Schaller et al., 2020). These fertility responses are also important for infant health because changes in the timing and composition of births can alter the characteristics of women observed giving birth. Conditional on a birth, greater financial resources and control may improve nutrition, reduce financial stress, and facilitate healthcare utilization, while greater employment or business activity may also increase time constraints, work intensity, or stress during pregnancy (Corman et al., 2019; Currie & Rossin-Slater, 2013; Lindo, 2011; Mocan et al., 2015; Vu, 2024). The effects of feminist credit unions on both fertility decisions and health at birth are therefore empirical questions, and the two outcomes should be considered jointly when evaluating the broader consequences of women's financial independence.

We find that feminist credit union openings improved infant health and altered fertility behavior. Average birth weight increased by approximately 5.7 grams, while the incidence of low birth weight declined by 0.11 percentage points, or about 1.3 percent relative to the mean. The event-study estimates show little evidence of differential pre-trends and indicate that the improvements in birth outcomes emerged and strengthened following the openings.

We conduct an extensive set of robustness checks to assess whether the infant-health estimates are sensitive to concurrent policies, local conditions, sample construction, statistical inference, or the choice of difference-in-differences estimator. We first account separately for major policy changes occurring during the same period, including fair-employment legislation, state Equal Rights Amendments, abortion legalization, access to the contraceptive pill, and Medicaid. We then control for state income, county temperature, county land cover, and local pollution measures. We further examine a broad set of social-insurance and public-transfer programs, including per capita workers' compensation, medical payments, military medical payments, income maintenance, Supplemental Security Income, family assistance, Food Stamp

payments, unemployment insurance, and education and training payments. The estimates also remain similar when we use all U.S. counties as the comparison group. We additionally allow for spatial correlation using Conley standard errors with several threshold distance cutoffs. Finally, while our baseline analysis uses the two-stage difference-in-differences estimator of Gardner et al. (2022), we re-estimate the effects using conventional two-way fixed effects, the interaction-weighted estimator of Sun & Abraham (2021), the estimator of de Chaisemartin & D'Haultfœuille (2020), and Wooldridge (2025)'s extended two-way fixed-effects estimator. Across these exercises, the estimated improvements in birth weight and low birth weight remain broadly comparable to the baseline findings.

We also find significant changes in fertility. The overall fertility rate declined by about 1.8 births per 1,000 women, equivalent to a 2.9 percent reduction relative to the pre-period mean. The declines are concentrated among women ages 20–34, while fertility increases modestly among women ages 35–39 and is essentially unchanged among women ages 40–49. This age pattern is consistent with a postponement of childbearing toward older ages rather than a uniform decline in fertility across the reproductive life cycle.

We next explore several potential pathways through which feminist credit unions may have affected infant health. The openings increased women's employment by 1.1 percentage points, or about 2.8 percent relative to the mean, with a 2.0 percentage point increase in full-time employment and a 1.0 percentage point decline in part-time employment. We also find increases in women-owned businesses and employment generated by these businesses of approximately 10 and 17 percent, respectively. Housing tenure changed substantially: renting increased, while homeownership both with and without a mortgage declined. Turning to pregnancy-related outcomes, the probability of initiating prenatal care during the first trimester increased by 4.8 percentage points, or about 6.2 percent relative to the mean. We also observe changes in the composition of mothers giving birth, including a decline in younger mothers and an increase in mothers ages 36–54, patterns that are consistent with the age-specific fertility results. Taken together, these findings suggest that feminist credit union openings changed women's economic opportunities, household choices, fertility timing, and prenatal-care utilization. Although these outcomes do not identify a single mechanism, they provide evidence on several margins through which greater financial independence may have altered the prenatal environment and the composition of births.

The current paper contributes to three strands of literature. First, our paper contributes to the literature linking financial access and women's economic agency to infant health by providing, to our knowledge, the first causal evidence that the entry of women-centered financial institutions affected health at birth. Earlier work on microfinance shows that access to credit, particularly when resources are directed toward women, can affect child health and households' ability to smooth consumption following health shocks (DeLoach & Lamanna, 2011; Gertler et al., 2009; M. Pitt et al., 2003). Other studies link women's financial inclusion and economic empowerment to maternal healthcare utilization, reproductive health, and investments in children (Dehingia et al., 2019; Sarkar et al., 2025; Singh & Yu, 2019). Related evidence shows that greater control over household resources can alter the allocation of resources toward children and other household members (Duflo, 2000, 2003; Lindo et al., 2018; Lundberg et al., 1997; Thomas et al., 1990), while broader expansions in women's agency can improve child health through changes in household and public resource allocation (Doepke et al., 2012; Miller, 2008). Most closely related to our institutional setting, Cramer (2025) shows that bank expansion can affect health through employment, account access, household health investments, and credit to healthcare providers. Using historical county-level bank deposits, Noghanibehambari et al. (2024) further show that local financial conditions through bank deposits are associated with infant mortality and longer-run health. We connect these literatures by studying financial institutions explicitly created to relax women's financial constraints and promote economic independence. Unlike interventions that expand general credit or introduce a single financial product, feminist credit unions combined access to credit and savings with financial information, member ownership, social networks, and an explicit focus on women's economic independence. Our findings show that the consequences of these women-centered financial institutions extended beyond women's own economic behavior to the health of the next generation.

Second, our paper contributes to the literature on women's economic agency, financial access, and fertility. Several strands of research show that women's fertility decisions respond to changes in economic opportunities and control over resources, although the direction of the effect depends on the underlying constraint. Studies of women's empowerment, bargaining power, and reproductive autonomy show that greater control over household and reproductive decisions can reduce or postpone childbearing (Ashraf et al., 2014; Bhalotra et al., 2023; Doepke & Tertilt, 2019; Godefroy, 2019). A related literature shows that improvements in women-specific employment

opportunities and the returns to work can delay marriage and fertility, while employment losses and economic insecurity can also reduce subsequent births (Del Bono et al., 2012; Heath & Mobarak, 2015; Huttunen & Kellokumpu, 2016; Jensen, 2012; Schaller, 2016). The finance literature provides a less uniform prediction. Studies of women-focused microcredit find mixed or modest fertility effects, in part because borrowing in a woman's name does not necessarily imply control over the resources (Desai & Tarozzi, 2011; Duvendack & Palmer-Jones, 2017; Kuchler, 2012; Pitt et al., 1999), whereas broader expansions in credit, housing wealth, and household liquidity often increase fertility by relaxing financial and housing constraints (Cumming & Dettling, 2024; Kim et al., 2022; Lovenheim & Mumford, 2013). Other studies emphasize the role of information, peer networks, and changing social norms in reducing early childbearing (Bandiera et al., 2020; Jensen & Oster, 2009; Kearney & Levine, 2015; La Ferrara et al., 2012). For instance, Ashraf et al. (2014) show experimentally that providing women with greater private control over contraceptive access reduced subsequent fertility, while Schaller (2016) finds that stronger female labor demand reduced or postponed births, in contrast to the positive fertility response to stronger male labor demand. We contribute to these literatures by studying the entry of women-centered financial institutions that simultaneously expanded access to financial services, economic opportunities, information, and networks. To our knowledge, no prior causal study has examined the effects of such institutions on age-specific fertility in the United States. Our findings show reductions in fertility among women ages 20–34 together with a smaller increase among women ages 35–39, a pattern consistent with delayed childbearing rather than a uniform reduction in fertility across the reproductive life cycle.

Third, our paper contributes to the literature on financial access, women's economic activity, and economic empowerment. A large body of research shows that changes in local financial access can affect business formation, investment, employment, and wealth accumulation (Black & Strahan, 2002; Bruhn & Love, 2014; Burgess & Pande, 2005; Cai & Szeidl, 2024; Célerier & Matray, 2019; Kaboski & Townsend, 2012; Kerr & Nanda, 2009; Nguyen, 2019; Van Doornik et al., 2024). For instance, Black & Strahan (2002) and Kerr & Nanda (2009) show that greater banking competition and banking deregulation increased entrepreneurial entry, while Van Doornik et al. (2024) document causal effects of credit access on employment and labor-market outcomes. An important related literature emphasizes that the consequences of financial access can differ for women because access to credit does not necessarily imply control over financial

resources. Studies document persistent gender differences in credit access and lending terms (Alesina et al., 2013; Beck et al., 2013; Bellucci et al., 2010; Blanchflower et al., 2003; Cavalluzzo et al., 2002; Coleman, 2000; Muravyev et al., 2009; Ongena & Popov, 2016), while another strand shows that product design, privacy, and control over resources can substantially affect women's labor supply, entrepreneurship, and household decision-making (Almås et al., 2018; Ashraf et al., 2010; Bernhardt et al., 2019; Goetz & Sen Gupta, 1996; Kabeer, 2001; Schaner, 2015). For instance, Field et al. (2021) show that directing wages into individually controlled accounts increased women's labor supply and shifted gender norms, while Ashraf et al. (2010) find that a commitment savings product increased women's decision-making power with relatively low baseline authority. Bartscher (2023) shows that relaxing restrictions on the treatment of wives' earnings in mortgage underwriting increased borrowing, homeownership, and married women's labor-force participation.

We connect these literatures by studying institutions created specifically to address women's financial constraints and promote their economic independence. Unlike changes in general credit supply, banking regulation, or a single financial product, feminist credit unions combined credit and savings services with financial information, member ownership, and women-centered institutional support. Our results show that their entry affected women's employment, the creation and expansion of women-owned businesses, and household economic choices, providing evidence that the design and institutional context through which financial access is provided can shape women's economic responses.

The findings also have implications for policies aimed at expanding women's financial inclusion, particularly in developing countries. A large literature shows that simply expanding credit does not necessarily translate into greater economic agency for women, and that the effects of financial interventions depend importantly on women's control over resources, product design, and complementary institutional support (Ashraf et al., 2010; Banerjee, Karlan, et al., 2015; Dupas & Robinson, 2013; Duvendack et al., 2026; Field et al., 2021). Our results suggest that institutions designed specifically around women's financial needs may generate broader effects by combining access to credit and savings with financial information, individual control, and supportive networks. These effects may extend beyond women's employment and entrepreneurship to fertility decisions, the timing of childbearing, and infant health, generating consequences that reach the next generation. This distinction may be especially important in settings where formal financial

inclusion has expanded rapidly but social and household constraints continue to limit women's effective use of financial services and their ability to translate financial access into greater economic and reproductive agency.

The rest of the paper is organized as follows. Section 2 provides historical background on feminist credit unions and the institutional environment in which they emerged. Section 3 describes the data sources, sample construction, and outcome measures. Section 4 discusses the empirical strategy. Section 5 presents the main results on infant health and fertility, examines the robustness of the findings, and explores potential mechanism channels through women's economic activity, household choices, maternal composition, and prenatal care. Section 6 concludes.

2. Historical Background of Feminist Credit Unions

By the early 1970s, there were no laws to protect women's right to obtain loans from financial institutions without a male cosigner and lenders routinely required unmarried women to obtain a male cosigner before loan approval. Mortgage lenders commonly discounted or entirely disregarded a married woman's income when calculating household eligibility, particularly if she was of childbearing age, on the assumption that she would leave the workforce to raise children. Divorced or widowed women faced an additional obstacle, as credit histories built jointly during marriage were often recorded solely under the husband's name, leaving women with no independent credit record even after years of active participation in household finances. Overall, financial institutions made it difficult for single and divorced women or women seeking small business loans to borrow independently (Dumaine, 2022; Hyman, 2011; Michals, 2018).

Feminist credit unions emerged from the women's movement of the 1970s in response to persistent barriers women faced in conventional credit markets. In 1973, activists Valerie Angers and Joanne Parrent established the Detroit Feminist Federal Credit Union, the first institution of its kind. The model spread quickly: By 1976, there were 18 feminist federal credit unions across the U.S. and Canada, with combined assets exceeding $3 million. By 1977, nine more feminist credit unions opened across the U.S. (Dumaine, 2022). These institutions were member-owned, generally staffed by women, and organized around feminist associations. The number of members varied by city. For example, by the end of the second year, Detroit feminist credit union served 300 members and held $750,000 in assets (Michals, 2018). In 1976, the Massachusetts feminist credit union had 850 members and had issued $258,000 in loans (Topping, 2019). The Connecticut

feminist credit union had 1100 members and $200,000 in assets (Westerman, 2012). Members pooled their savings, which the credit unions used to make loans to women and to support women-owned and feminist businesses that conventional lenders often viewed as undesirable or risky. They also offered financial education and sought to give women greater knowledge of and control over financial decisions (Michals, 2018). Although federal antidiscrimination protections expanded during the same period, feminist credit unions pursued a distinct strategy: creating women-centered financial institutions rather than relying solely on changes in the rules governing existing lenders themselves.

Feminist credit unions were distinct from the women's banks that emerged during the same period. Whereas feminist credit unions were member-owned, not-for-profit cooperatives organized around feminist associations and explicitly linked financial activity to movement goals, women's banks operated as conventional, shareholder-owned commercial banks that generally followed standard lending practices and served both women and men. Although some women's banks were founded by feminists, they were generally more commercially oriented and sought to expand women's access within the existing banking system rather than create an alternative to it (Mailliard & Anderson, 1987; Michals, 2018).

3. Data Sources, Sample, and Summary Statistics

Our primary source of data is the restricted-access Natality Detail Files from the National Center for Health Statistics (NCHS, 2024), covering the universe of U.S. births from 1968 to 1990. These records provide information on infants' health at birth, maternal sociodemographic characteristics, and prenatal-care utilization, together with the geographic identifiers required to assign exposure to feminist credit union openings. For both the infant-health and fertility analyses, we restrict the sample to urban counties located within metropolitan statistical areas. Counties outside metropolitan areas, as well as nonurban counties, may follow different demographic, economic, and health trends and may therefore provide less appropriate comparison groups for the metropolitan areas in which feminist credit unions opened. This restriction makes the comparison sample more similar to the treated locations and reduces concerns that the estimates reflect differential trends between metropolitan and more rural areas. Later, as a robustness check, we expand the sample to include all U.S. counties and show that the main infant-health results remain similar.

Our primary infant-health outcomes are birth weight and an indicator for low birth weight, defined as birth weight below 2,500 grams. We do not examine gestational age or gestational-age-related outcomes because gestational age was not consistently reported in the Natality files during our sample period. In particular, a substantial number of states did not report gestational age in many years, especially during the late 1960s and early 1970s. Using this outcome would therefore require dropping a large and systematically changing set of state-year observations, reducing both the geographic coverage and comparability of the analysis over time. We consequently focus on birth weight and low birth weight, which are available much more consistently throughout the period. The data also report maternal race, age, education, and birth order, which we use as covariates in the infant-health analysis and as outcomes to examine whether FFCU openings changed the composition of women giving birth. We further examine prenatal-care utilization, including whether mothers received no prenatal care and whether care began during the first trimester. Finally, we use the natality records to construct county-level birth counts and combine them with single-age female population estimates from the Surveillance, Epidemiology, and End Results (SEER) Program to calculate fertility rates (SEER, 2019). Fertility is measured as births per 1,000 women ages 20–49, and we construct rates for all women as well as separately by race and maternal age. Because the SEER population estimates begin in 1969, the fertility analysis covers the years 1969–1990.

To examine potential economic and household channels, we use the Current Population Survey Annual Social and Economic Supplement (CPS-ASEC) for the years 1970–1989 (Flood et al., 2025). We restrict the sample to women ages 18 and older and use the metropolitan-area identifiers to link respondents to the timing of feminist credit union openings. The CPS-ASEC provides information on women's labor-market activity, including employment and whether employment is full-time or part-time, allowing us to examine whether the openings changed women's attachment to the labor market and the intensity of work. We also use information on separation or divorce and housing tenure, distinguishing between renting, homeownership with a mortgage, and homeownership without a mortgage. These outcomes provide evidence on broader changes in household formation and residential choices that may accompany greater financial independence, although we do not assign a direct welfare interpretation to changes in housing tenure or marital status. The data additionally provide information on race and educational attainment, which we include as individual-level controls in the corresponding regressions.

We measure female entrepreneurship using the Census Bureau's Censuses of Women-Owned Businesses for 1972, 1977, 1982, and 1987. These data are available at the metropolitan-area level. We construct two measures: the number of women-owned businesses per 1,000 adult women and the number of paid employees working in women-owned businesses per 1,000 adult women. The business sample contains 868 metropolitan-area-by-year observations. These data allow us to examine whether the effects of feminist credit unions extended beyond women's participation in wage employment to business formation and employment within women-owned firms.

Table 1 reports summary statistics for the main analysis samples, separately for observations in years through 1975 and those observed after 1975. We begin with the natality sample, which is the primary dataset in our analysis. Average birth weight is approximately 3,250 grams in the earlier period and 3,284 grams in the later period, while the incidence of low birth weight declines from 9.1 to 8.4 percent. The table also documents changes in maternal characteristics and prenatal-care utilization over time. For example, the share of mothers initiating prenatal care during the first trimester increases from 68.9 to 80.6 percent. The CPS-ASEC sample shows corresponding changes in women's economic outcomes, including increases in employment and full-time work, while the Census of Women-Owned Businesses indicates substantial growth in both the number of women-owned businesses and paid employment within these firms. These differences are descriptive and should not be interpreted as the effects of feminist credit union openings, as they combine secular changes over time with differences in the composition of observations before and after 1975.

Figure 1 summarizes the institutional histories of the sixteen federally chartered feminist credit unions in our sample. The figure reports the timing of each opening, the documented period of operation, closures and mergers, and, when available, information on membership, assets, lending activities, and sponsoring organizations. The openings were concentrated between 1974 and 1977, while the subsequent histories varied considerably across institutions, with some closing or merging within several years and others remaining active substantially longer. Figure 2 shows the geographic distribution of these openings together with the comparison counties used in the analysis. The treated metropolitan areas are dispersed across different regions of the United States rather than concentrated in a single geographic area. The comparison sample consists of urban counties located in metropolitan statistical areas that did not experience an FFCU opening,

consistent with our objective of comparing treated locations with areas that were more similar in their underlying urban and metropolitan characteristics.

4. Econometric Method

Our empirical strategy exploits variation in the timing and location of feminist credit union openings. Because the credit unions opened in different metropolitan areas at different times, we estimate treatment effects using the two-stage difference-in-differences estimator developed by Gardner et al. (2022). This approach is designed for settings with staggered treatment adoption and avoids the weighting problems that can arise in conventional two-way fixed-effects models when treatment effects vary across locations or over time. Gardner et al. (2022)'s estimator identifies the fixed effects using untreated observations in the first stage and then estimates the treatment effect from outcomes net of these fixed effects in the second stage.

Let D_{jt} be an indicator equal to one if geographic unit j has experienced a feminist credit union opening by year t, and zero otherwise. In the first stage, we estimate the relationship between the outcome and the relevant geographic and time fixed effects using only untreated observations:

$$Y_{ijt} = \alpha_j + \lambda_t + X'_{ijt}\gamma + \varepsilon_{ijt}, D_{jt} = 0$$

Where Y_{ijt} denotes the outcome for individual i in location j and year t, α_j denotes location fixed effects, λ_t denotes time fixed effects, and X contains the controls included in the corresponding specification. The common state-level control across specifications is the interaction of state fixed effects with indicators for age-of-majority laws, which affected access to the contraceptive pill and abortion, both of which the literature has shown to significantly influence women's outcomes during this period (Bailey, 2006, 2013; Bailey et al., 2012; Goldin & Katz, 2002; Myers, 2017). We then remove the estimated fixed effects and covariate component from the outcome. In the second stage, the residualized outcome is regressed on the post-opening indicator:

$$\widetilde{Y_{ijt}} = \beta D_{jt} + u_{ijt}, D_{jt} = 1$$

The coefficient β captures the average change in the outcome following the opening of a feminist credit union relative to the counterfactual change estimated from untreated observations. This two-stage procedure is robust to treatment-effect heterogeneity under staggered adoption and does not use already-treated observations to identify the untreated location and time effects.

The exact fixed effects, controls, weights, and level of inference vary with the outcome and the underlying data. For the CPS-ASEC analysis, the regressions include metropolitan-area and year fixed effects and controls for women's race and educational attainment. Observations are weighted using the CPS-ASEC survey weights, and standard errors are two-way clustered at the metropolitan-area and year level. For the Census of Women-Owned Businesses, we include metropolitan-area and year fixed effects, weight observations by the metropolitan-area population of women ages 18 and older, and cluster standard errors at the metropolitan-area level.

For the natality analysis, the geographic unit is the county. The specifications include county fixed effects and year fixed effects, allowing common shocks to vary flexibly across regions over time. The baseline regressions additionally control for maternal race, age, education, and birth order. Observations are cells constructed from the natality records and are weighted by the number of births represented by each cell. Standard errors are two-way clustered at the county and year level.

In addition to the average post-opening effects, we estimate event-study specifications using the two-stage Gardner estimator. We replace the post-opening indicator in the second stage with indicators for time relative to the year in which the feminist credit union opened:

$$\widetilde{Y_{ijt}} = \sum_{k \neq -1} \beta_k 1(t - G_j = k) + u_{ijt}, D_{jt} = 1$$

Where G_j denotes the opening year in treated location j, k indexes event time, and $k = -1$ is the omitted pre-opening reference period. The resulting coefficients trace the evolution of outcomes before and after an opening. The pre-opening coefficients provide a direct assessment of whether treated and comparison locations exhibited differential trends before the establishment of the credit union, while the post-opening coefficients show the timing and persistence of the estimated effects.

5. Results

5.1. Infant Health

We begin by examining the effects of feminist credit union openings on infant health. Figure 3 presents the event-study estimates for birth weight and low birth weight. The coefficients in the periods preceding an FFCU opening are generally small and close to zero, providing little

evidence that birth outcomes in treated and comparison areas were evolving differently before the openings. Following an opening, the birth-weight coefficients become positive, while the corresponding estimates for low birth weight become negative. The effects also tend to increase in magnitude over the post-opening period, suggesting that the improvements in infant health developed gradually rather than occurring immediately at the time of entry.

Table 2 reports the corresponding average post-opening effects. FFCU openings are associated with a 5.7-gram increase in birth weight, statistically significant at the 1 percent level. Although this represents a relatively small increase of about 0.17 percent compared with the mean birth weight of 3,278 grams, the effect is accompanied by a more pronounced improvement in the lower tail of the birth-weight distribution. The probability of low birth weight declines by 0.11 percentage points, equivalent to approximately 1.3 percent relative to the mean of 8.6 percent. Taken together, the results indicate that the improvements were not limited to a small shift in average birth weight but also reduced the incidence of an adverse outcome among infants at the lower end of the birth-weight distribution.

We next examine heterogeneity in these effects across maternal characteristics. The positive birth-weight estimates are observed among both White and non-White mothers, across maternal age groups, and among both first-time and higher-parity mothers, although statistical precision varies across groups. The effects are particularly pronounced among mothers ages 36–54. For this group, FFCU openings increase birth weight by approximately 7.5 grams and reduce the incidence of low birth weight by 0.46 percentage points, or about 5.4 percent relative to the overall mean. The estimates are also positive and statistically significant for mothers ages 20–25 and 26–35 and for both first-time and higher-parity mothers. These patterns suggest that the improvement in infant health is not concentrated in a single maternal subgroup, although the magnitude of the lower-tail effect is larger among older mothers.

5.2. Robustness Checks

We next examine the robustness of the infant-health results to a wide range of alternative controls, sample definitions, inference procedures, and difference-in-differences estimators. Figure 4 summarizes these exercises for birth weight and low birth weight.

One concern is that feminist credit unions emerged during a period of substantial changes in women's legal rights, labor-market opportunities, reproductive autonomy, and access to

healthcare. These changes could be correlated with the location or timing of FFCU openings and independently affect infant health. We therefore separately control for the Fair Employment Practices Act and state Equal Rights Amendments, both of which altered the legal and economic environment facing women and had the potential to affect women's labor-market outcomes (Collins, 2003; Medoff, 1985; Neumark & Stock, 2006). We also control for abortion legalization and access to the contraceptive pill because these policies substantially changed women's reproductive control, fertility timing, and selection into motherhood (Bailey, 2006, 2013; Bailey et al., 2012; Farin et al., 2024; Goldin & Katz, 2002; Myers, 2017). Finally, we account for Medicaid because expansions in public health insurance can alter prenatal healthcare utilization and birth outcomes (Currie & Gruber, 1996a, 1996b; Goodman-Bacon, 2018). The estimated effects on birth weight and low birth weight remain close to the baseline estimates after accounting for each of these policy changes.

A second concern is that FFCU openings may have coincided with changes in local economic or environmental conditions that themselves affect fetal development. We first control for state income because household and local economic conditions can influence birth outcomes through income, employment, stress, and access to health-related resources (Lindo, 2011; Mocan et al., 2015). We then separately account for county temperature, county land cover, and local pollution measured using SO2, NOx, CO, and OC. These controls are important because environmental conditions during pregnancy, including ambient temperature and air pollution, have been linked to fetal growth, low birth weight, and other adverse birth outcomes (Currie et al., 2009; Currie & Walker, 2011). Controlling for land cover further allows counties with different changes in their physical and developed environments to evolve differently over the sample period. The infant-health estimates remain similar across these specifications.

We next examine whether the results are confounded by contemporaneous changes in social-insurance and public-transfer programs. This is particularly important because such programs can change household disposable resources, consumption, healthcare utilization, and financial stress, all of which may affect health during pregnancy. Prior research, for example, documents improvements in birth outcomes following the introduction of the Food Stamp Program, expansions in the Earned Income Tax Credit, and greater Unemployment Insurance payments (Almond et al., 2011; Bitler & Currie, 2005; Hoynes et al., 2015). We therefore separately control for workers' compensation, total medical payments, military medical payments,

income-maintenance payments, Supplemental Security Income, family-assistance payments, Food Stamp payments, Unemployment Insurance, and education and training payments. The estimated effects remain broadly stable after accounting for these alternative sources of household resources and public support.

We also examine several aspects of sample construction and statistical inference. Our baseline sample excludes nonmetropolitan and nonurban counties because FFCUs opened in metropolitan areas and these counties may provide a more comparable counterfactual than rural areas. To examine whether this restriction drives the results, we expand the comparison group to include all U.S. counties; the estimates remain similar.

We next address the treatment definition. The baseline specification treats an FFCU opening as a persistent treatment, as is conventional in staggered-adoption difference-in-differences designs (Gardner et al., 2022). Because several FFCUs subsequently closed, we alternatively drop observations beginning more than one year after a documented closure. The results remain comparable, suggesting that they are not driven by observations long after an institution ceased operating. Finally, nearby counties may experience correlated shocks or spillovers that make conventional geographic clustering insufficient. We therefore recalculate inference using Conley spatial standard errors with 100- and 500-kilometer distance cutoffs (Conley, 1999). Statistical inference remains similar under both spatial-correlation allowances.

Finally, we examine sensitivity to the choice of difference-in-differences estimator. Our baseline estimates use the two-stage estimator of Gardner et al. (2022). This issue is important because recent work shows that conventional two-way fixed-effects estimators can produce misleading estimates in staggered-adoption settings when treatment effects differ across groups or over time (de Chaisemartin et al., 2022; Goodman-Bacon, 2021). We therefore replicate the analysis using conventional two-way fixed effects, the interaction-weighted estimator of Sun & Abraham (2021), the estimator of de Chaisemartin & D'Haultfœuille (2020), and Wooldridge (2025)'s extended two-way fixed-effects estimator. Across these approaches, the estimates continue to indicate higher birth weight and lower incidence of low birth weight following FFCU openings, with differences primarily in magnitude and statistical precision.

We conduct two additional exercises to examine whether the baseline results are driven by particular locations or treatment assignments. First, Appendix Figure A-5 reports leave-one-

county-out estimates in which we sequentially exclude each treated county from the analysis. The estimated effects on birth weight and low birth weight remain close to the full-sample estimates across these exclusions, suggesting that the results are not driven by a single treated location. Second, Appendix Figure A-6 and Appendix Figure A-7 report placebo exercises in which FFCU openings are randomly reassigned across locations and years. The placebo estimates for both birth weight and low birth weight are generally centered around zero and are smaller in magnitude than the baseline estimates. These exercises provide further evidence that the main findings are not driven by a particular treated county or by randomly assigned treatment patterns.

5.3. Fertility

We next examine whether feminist credit union openings affected fertility decisions. This analysis is important for two reasons. First, changes in women's financial independence, employment opportunities, and control over resources may alter the timing of childbearing. Second, fertility responses can change the composition of women observed giving birth and are therefore relevant for interpreting the infant-health results.

Table 4 reports the estimated effects on county fertility rates, measured as births per 1,000 women ages 20–49. FFCU openings reduce the overall fertility rate by 1.83 births per 1,000 women, equivalent to a 2.9 percent decline relative to the pre-period mean of 63.1 births per 1,000 women. The corresponding estimate for White women is a reduction of 2.33 births per 1,000, or approximately 3.8 percent relative to the mean. The point estimate for Black women is also negative and somewhat larger in proportional terms, but it is less precisely estimated and statistically insignificant.

The age-specific estimates reveal that the overall decline masks substantial differences across the reproductive life cycle. Fertility falls by 3.96 births per 1,000 women ages 20–24 and by 5.01 births per 1,000 women ages 25–29, corresponding to declines of approximately 3.4 and 4.3 percent relative to their respective means. We also observe a smaller reduction of 1.40 births per 1,000 among women ages 30–34. In contrast, fertility among women ages 35–39 increases by 0.64 births per 1,000, or about 2.7 percent relative to the mean, while the estimate for women ages 40–49 is small and statistically insignificant. Thus, the fertility response is concentrated in reduced childbearing at younger ages together with a smaller increase at older reproductive ages.

Appendix Figure A-4 presents the corresponding event-study estimates. The pre-opening coefficients are generally small and centered around zero, with no systematic pattern of differential trends before an FFCU opening. Following an opening, fertility declines progressively for all women and particularly for White women. The age-specific estimates show a similar pattern: the reductions become increasingly pronounced among women ages 20–24 and 25–29, while the estimates for ages 35–39 become positive after the opening. The estimates for Black women are generally negative after an opening but considerably less precise.

Taken together, the age profile is more consistent with a postponement of childbearing than with a uniform reduction in fertility across ages. We interpret this pattern cautiously. Because our fertility panel ends in 1990, we do not observe the complete reproductive histories of younger women exposed to FFCU openings and therefore cannot determine whether the decline in period fertility translated into lower completed family size. The results instead indicate that FFCU openings changed when births occurred, shifting childbearing away from younger ages and, to some extent, toward older ages.

5.4. Maternal Composition and Prenatal Care

We next examine whether FFCU openings were accompanied by changes in the composition of women giving birth and in prenatal-care utilization. Table 3 shows several compositional changes. The share of births to White mothers declines by 0.92 percentage points, while the share to mothers of other races increases by 0.34 percentage points; the estimate for Black mothers is small and statistically insignificant. The share of births to women ages 20–25 declines by 0.60 percentage points, while the share to women ages 36–54 increases by 0.54 percentage points. These age patterns are consistent with the fertility results in Table 4. The share of first-time mothers also declines by 0.47 percentage points, while most changes in maternal educational composition are statistically insignificant.

We also find evidence of earlier prenatal-care initiation. FFCU openings increase the probability that prenatal care begins during the first trimester by 4.8 percentage points, or about 6.2 percent relative to the mean, while the probability of receiving no prenatal care does not change significantly. Prior evidence suggests that earlier prenatal-care initiation can affect infant health (Corman et al., 2019; Cygan-Rehm & Karbownik, 2022; Guldi & Hamersma, 2023). We therefore interpret this pattern as a plausible pathway rather than a separately identified mechanism. Overall,

the results indicate that FFCU openings changed both the composition of births and pregnancy-related healthcare utilization, two margins relevant for interpreting the infant-health effects.

5.5. Economic and Household Mechanisms

We next examine several economic and household responses that may help characterize the channels through which FFCU openings affected women and, potentially, infant health. We first examine employment and work intensity. Financial access can facilitate employment by relaxing liquidity constraints, while greater individual control over financial resources can increase women's incentives and ability to work. Bartscher (2023), for example, finds that recognizing wives' earnings in mortgage underwriting increased married women's labor-force participation, while Field et al. (2021) show that directing wages into individually controlled accounts increased women's labor supply. More generally, access to credit can affect employment and local labor-market activity (Van Doornik et al., 2024). Employment is also potentially relevant for infant health, although its effect is theoretically ambiguous: work can increase income, insurance coverage, and bargaining power, but greater work intensity can also increase physical demands, time constraints, and stress during pregnancy ((Lindo, 2011; Page et al., 2017).

Motivated by this literature, Table 5 shows that FFCU openings increase women's employment by 1.1 percentage points, or 2.8 percent relative to the mean. Full-time employment increases by 2.0 percentage points, or 5.3 percent, while part-time employment declines by 1.0 percentage point, or 7.2 percent relative to its mean. Thus, the labor-market response reflects not only greater employment but also a shift toward full-time work.[1]

We next examine women-owned businesses because supporting female entrepreneurship was a central purpose of feminist credit unions. A broad literature shows that greater local financial access can increase business formation and entrepreneurial entry (Black & Strahan, 2002; Kerr & Nanda, 2009), while women-owned firms have historically faced greater credit constraints, loan denials, and less favorable lending terms (Alesina et al., 2013; Coleman, 2000; Muravyev et al., 2009). Consistent with this channel, FFCU openings increase the number of women-owned businesses by 2.83 per 1,000 women, or 10.2 percent relative to the mean, and increase paid employment in women-owned businesses by 3.10 workers per 1,000 women, or approximately 17

[1] Appendix Figure A-1 through Appendix Figure A-3 report the corresponding event-study estimates for the CPS-based mechanism outcomes, including employment and work intensity, separation or divorce, and housing tenure, respectively.

percent. Entrepreneurship may increase income, autonomy, and work flexibility, but it may also involve income volatility, long hours, and financial stress. We therefore interpret these results as evidence that FFCUs altered women's local economic opportunities rather than as evidence that entrepreneurship itself caused the improvement in infant health.

Finally, we examine separation or divorce and housing tenure because greater financial independence can alter women's outside options, household bargaining, and the feasibility of forming an independent household. Prior work shows that women's relative economic opportunities and the resources available upon marital dissolution can affect bargaining and household behavior (Aizer, 2010; Gray, 1998; Heath, 2014; Voena, 2015). We find essentially no change in separation or divorce following an FFCU opening. Housing tenure, however, changes substantially. Credit access can affect both asset accumulation and housing choices; For instance, Célerier & Matray (2019) show that greater bank access can promote wealth accumulation. At the same time, stronger financial independence may facilitate residential mobility or independent household formation, so a movement toward renting need not have an unambiguous welfare interpretation. In our estimates, renting increases by 25.4 percentage points, while homeownership with a mortgage and without a mortgage decline by 9.1 and 16.3 percentage points, respectively. These changes indicate a substantial reallocation of housing tenure following FFCU openings, although the estimates alone cannot distinguish changes in financial capacity from household reorganization or residential independence.

6. Magnitude and Economic Significance of the Infant-Health Effects

To place the magnitude of the infant-health effects in context, FFCU openings increase average birth weight by 5.7 grams, equivalent to approximately 0.17 percent of the sample mean of 3,278 grams. The reduction in low birth weight is economically more pronounced: the estimated decline of 0.11 percentage points corresponds to 1.28 percent of the mean low-birth-weight rate of 8.6 percent. Expressed in population terms, this is an 11-basis-point decline, or approximately 110 fewer low-birth-weight births per 100,000 births. Thus, while the shift in average birth weight is modest, the results indicate a relatively larger improvement in the lower tail of the birth-weight distribution.

One way to assess these magnitudes is to compare them with the effects of other policies that altered maternal resources, nutrition, or prenatal environments. Hoynes et al. (2011) find that

the introduction of WIC increased average birth weight by approximately 2 grams overall and by 7 grams among infants born to mothers with low education, placing our 5.7-gram estimate between these two intent-to-treat effects. Almond et al. (2011) similarly find that third-trimester exposure to the Food Stamp Program increased birth weight by approximately 2.0–2.6 grams among White infants and about 3–6 grams among Black infants. Our estimated 5.7-gram increase is therefore toward the upper end of the Food Stamp intent-to-treat estimates. For low birth weight, the 1.28-percent decline associated with FFCU openings is also comparable in magnitude to the approximately 1-percent intent-to-treat reduction associated with Food Stamp availability among White infants. These comparisons are particularly useful because WIC and the Food Stamp Program, like FFCUs, could affect infant health through changes in household resources and women's ability to make health-related investments, although the programs and treatment margins are substantively different.

Income-based interventions provide another useful benchmark. Chung et al. (2016) find that $2,331 of additional Alaska Permanent Fund income in 2011 dollars, approximately $3,336 in 2025 dollars, increased birth weight by 17.7 grams, while Hoynes et al. (2015) estimate an approximately 19.1-gram increase per $1,000 of EITC income in 2009 dollars, approximately $1,501 in 2025 dollars. Applying these estimates only as effect-size benchmarks, our 5.7-gram birth-weight estimate is comparable in magnitude to the birth-weight improvement associated with approximately $450 to $1,075 of additional household income in 2025 dollars.

Labor-market and earnings shocks provide another benchmark for the magnitude of our estimates. Mocan et al. (2015) use skill-biased technology shocks as an instrument for maternal earnings and find that higher earnings improve infant health among lower-skilled mothers. In their preferred subsamples, a 10 percent increase in mothers' weekly earnings increases birth weight by approximately 9 to 12.3 grams; doubling maternal earnings is associated with an increase of roughly 100 grams. Our estimated 5.7-gram increase is therefore approximately 46 to 63 percent of the birth-weight gain associated with a 10 percent increase in maternal earnings in their study. At the other end of the labor-market spectrum, Lindo (2011) finds that a husband's job displacement reduces the birth weight of subsequently born children by approximately 4.5 percent, or about five ounces, roughly 140–150 grams. Our estimate is therefore about 4 percent of the birth-weight loss associated with this much larger adverse employment shock.

The reduction in low birth weight also has potentially important implications for medical spending. Beam et al. (2020) report that, among commercially insured infants, low-birth-weight infants incurred $107,694 more in healthcare spending during the first six months of life than normal-birth-weight infants, an incremental difference that corresponds to approximately $144,460 in 2025 dollars. Applied mechanically to the 110 fewer low-birth-weight births per 100,000 implied by our estimate, this benchmark corresponds to approximately $15.9 million in lower healthcare spending during the first six months per 100,000 births. Using an average annual birth cohort of approximately 3.6 million births for years 1968-1990, the estimated reduction corresponds to about 3,960 fewer low-birth-weight births and approximately $572 million in lower healthcare spending during the first six months of life per birth cohort.

The low-birth-weight estimate provides a separate benchmark for medium-run educational outcomes. Fletcher (2011) finds that low-birth-weight children are approximately 3.9 percentage points more likely to have a reported learning disability and to receive special-education services, and about 4 percentage points more likely to repeat a grade. Applying these estimates mechanically to the 0.11-percentage-point reduction in low birth weight implies approximately 4 fewer learning-disability cases, 4 fewer special-education cases, and 4 fewer instances of grade repetition per 100,000 births.

These comparisons place the FFCU estimates in a useful range. The average birth-weight effect is modest, but it is comparable in magnitude to several well-studied social and health policies, while the reduction in low birth weight represents a meaningful change in an outcome associated with substantial medical expenditures and subsequent educational disadvantage. The medical-cost and later-life calculations are mechanical projections from the existing literature and should not be interpreted as outcomes directly caused or measured in our analysis.

7. Conclusion

This paper examined whether the opening of women-centered financial institutions affected health at birth and related maternal decisions by expanding women's economic opportunities and control over financial resources. Using variation in the timing of feminist credit union openings across U.S. metropolitan areas during the 1970s, we found improvements in infant health. Average birth weight increased by about 5.7 grams, while the incidence of low birth weight declined by 0.11 percentage points, or approximately 1.3 percent relative to its mean. Although

the shift in average birth weight was modest, the reduction in low birth weight indicated an improvement in the lower tail of the birth-weight distribution, where differences in health at birth have been linked to medical expenditures, educational attainment, and subsequent economic outcomes (Almond et al., 2005; S. Black et al., 2007; Oreopoulos et al., 2008).

The openings also affected fertility and the composition of women giving birth. The overall fertility rate declined by 1.8 births per 1,000 women ages 20–49, or about 2.9 percent relative to the mean. The decline was concentrated among women ages 20–34, whereas fertility increased modestly among women ages 35–39 and did not change significantly among women ages 40–49. This age pattern was more consistent with a postponement of childbearing than with a uniform reduction in fertility, although the end of our sample in 1990 prevented us from determining whether completed family size ultimately changed. This interpretation was consistent with evidence that greater reproductive autonomy and stronger economic opportunities for women can alter the timing of marriage and childbearing (Ashraf et al., 2014; Jensen, 2012; Schaller, 2016). The composition of births shifted in a corresponding direction, with fewer births to younger and first-time mothers and more births to older mothers. We also found that first-trimester prenatal-care initiation increased by 4.8 percentage points, or approximately 6.2 percent relative to the mean. Earlier prenatal-care initiation therefore provided one plausible pathway through which the prenatal environment may have changed, although we did not interpret it as a separately identified mediator of the infant-health effects (Corman et al., 2019; Cygan-Rehm & Karbownik, 2022; Guldi & Hamersma, 2023). More generally, because FFCU openings affected fertility timing and maternal composition, the birth-outcome estimates should be interpreted as total reduced-form effects that may incorporate both changes in conditions during pregnancy and changes in who gave birth.

The economic and household results provided additional evidence on the behavioral responses underlying these effects. Women's employment increased by 1.1 percentage points, or 2.8 percent relative to the mean, with full-time employment increasing by 2.0 percentage points and part-time employment declining by 1.0 percentage point. The number of women-owned businesses increased by about 10 percent, while paid employment in women-owned businesses increased by approximately 17 percent. These patterns were consistent with evidence that greater financial access and greater individual control over financial resources can affect women's labor supply and entrepreneurial activity (Bartscher, 2023; S. Black & Strahan, 2002; Kerr & Nanda,

2009). We found essentially no change in separation or divorce, but housing tenure shifted substantially toward renting and away from both mortgaged and outright homeownership. We did not assign a welfare interpretation to these housing responses because changes in tenure may reflect residential independence, household reorganization, asset accumulation, or changes in women's outside options (Gray, 1998; Voena, 2015). These economic and household outcomes therefore characterized potential channels and broader responses to FFCU entry rather than establishing any single mechanism through which infant health improved.

Taken together, the results suggested that the consequences of financial access depended in part on the institutions through which it was provided. Feminist credit unions did not simply expand the availability of loans; they combined credit and savings services with financial information, member ownership, women-centered service, social networks, and an explicit emphasis on women's economic independence. This distinction was important in light of evidence that expanding credit alone often generated modest and heterogeneous effects on women's economic agency, whereas individual control over financial resources, privacy, product design, and complementary institutional support could substantially alter women's economic behavior and household decision-making (Ashraf et al., 2010; Banerjee, Duflo, et al., 2015; Duvendack et al., 2026). Our findings extended this literature by showing that the entry of institutions organized specifically around women's financial independence was followed not only by changes in work, entrepreneurship, and household choices, but also by changes in fertility timing and improvements in the health of the next generation.

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Tables

Table 1 - Summary Statistics

	Year ≤ 1975		*Year > 1975*	
	Mean	SD	Mean	SD
Natality Data				
Birth weight (grams)	3,249.744	399.039	3,284.432	416.895
Low birth weight	0.091	0.186	0.084	0.183
Mother white	0.576	0.494	0.513	0.500
Mother black	0.270	0.444	0.252	0.434
Mother other races	0.152	0.359	0.139	0.346
Mother age 20 - 25	0.283	0.450	0.266	0.442
Mother age 26 - 35	0.472	0.499	0.493	0.500
Mother age 36 - 54	0.245	0.430	0.240	0.427
Mother education 0-8 years	0.123	0.329	0.111	0.314
Mother education 9-12 years	0.354	0.478	0.376	0.484
Mother education college	0.320	0.467	0.397	0.489
Mother education missing	0.203	0.402	0.117	0.321
First-time mother	0.367	0.482	0.390	0.488
Child female	0.489	0.306	0.488	0.311
Number of prenatal doctor visits	10.192	2.892	10.630	2.951
No prenatal doctor visits	0.005	0.049	0.016	0.086
Prenatal care started in first trimester	0.689	0.374	0.806	0.288
Prenatal care started in second trimester	0.107	0.209	0.111	0.213
Observations	178,744	680,105		
CPS-ASEC, women aged 18 and over				
White	0.888	0.316	0.875	0.331
Black	0.102	0.303	0.097	0.296
Other races	0.010	0.098	0.028	0.166
High school or less	0.756	0.430	0.662	0.473
High school, diploma reported	0.028	0.164	0.035	0.183
Some college	0.126	0.332	0.170	0.376
College graduate	0.063	0.244	0.087	0.281
Graduate degree	0.027	0.162	0.046	0.210
Age	44.384	17.996	44.032	18.406
FFCU opening year	1975.763	0.425	1976.006	2.094
Employed	0.399	0.490	0.474	0.499
Full-time	0.371	0.483	0.406	0.491
Part-time	0.148	0.355	0.181	0.385
Separated or divorced	0.077	0.267	0.107	0.309
Renter	0.312	0.463	0.286	0.452
Own with mortgage	0.392	0.488	0.458	0.498
Own without mortgage	0.295	0.456	0.257	0.437
Observations	179,259		815,148	
Women-owned businesses				
Number of WOBs per 1,000 women	4.997	1.939	26.386	17.499
Number of paid employees in WOBs per 1,000 women	3.163	1.726	18.578	12.012
Observations	217		651	

Notes. CPS-ASEC data covers the years 1970 to 1989. Women's business ownership data covers the years 1972, 1977, 1982, and 1987. Natality data covers the years 1968 to 1990.

Table 2 - Effects of FFCU openings on Birth Weight-Related Outcomes

	Samples:							
	Full Sample	Mother white	Mother nonwhite	Mother age 20 – 25	Mother age 26 – 35	Mother age 36 – 54	First-time mother	Non-first-time mother
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Birth Weight								
Post FFCU Opening	5.6958***	5.5731***	6.4353	5.9824***	4.3111**	7.4793*	4.0378**	6.6789***
	(1.9158)	(1.9337)	(3.9320)	(2.0490)	(2.1790)	(3.9016)	(1.7285)	(2.3592)
Observations	824901	436839	388062	222887	403870	198144	318213	506688
Mean DV	3277.512	3277.512	3277.512	3277.512	3277.512	3277.512	3277.512	3277.512
% Change	0.17	0.17	0.20	0.18	0.13	0.23	0.12	0.20
Panel B. Low Birth Weight								
Post FFCU Opening	-.0011**	-.0014***	-.0002	-.0006	-.0008	-.0046***	-.0011	-.0012**
	(.0005)	(.0005)	(.0017)	(.0007)	(.0006)	(.0014)	(.0007)	(.0006)
Observations	826297	437179	389118	223242	404516	198539	318598	507699
Mean DV	0.086	0.086	0.086	0.086	0.086	0.086	0.086	0.086
% Change	-1.28	-1.63	-0.23	-0.70	-0.93	-5.35	-1.28	-1.40

Notes. Standard errors, tw-way clustered at the county and year level, are reported in parentheses. Regressions include county fixed effects and year fixed effects. All specifications control for maternal race, age, education, and child birth order. All specifications additionally control for state fixed effects interacted with indicators for age-of-majority laws. Regressions are weighted by the number of births in each cell.

*** p<0.01, ** p<0.05, * p<0.1

Table 3 - Effects of FFCU openings on Maternal Composition and Prenatal Care Utilization

	Outcomes:						
	Mother white	Mother black	Mother other races	Mother age 20 – 25	Mother age 26 – 35	Mother age 36 – 54	Mother education 0-8 years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post FFCU Opening	-.0092***	-.0010	.0034**	-.006***	.0009	.0054***	.0040
	(.0019)	(.0011)	(.0017)	(.0017)	(.0017)	(.0007)	(.0068)
Observations	826297	826297	826297	826297	826297	826297	826297
Mean DV	0.531	0.257	0.139	0.271	0.489	0.241	0.114
% Change	-1.73	-0.39	2.45	-2.21	0.18	2.24	3.51
	Mother education 9-12 years	Mother education college	Mother education missing	First-time mother	Child female	No prenatal doctor visits	Prenatal care started in the first trimester
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Post FFCU Opening	.0154	.0017	-.021	-.0047*	-0	.0010	.0480**
	(.016)	(.0131)	(.0343)	(.0028)	(.0007)	(.0009)	(.0199)
Observations	826297	826297	826297	826297	826297	826297	826297
Mean DV	0.373	0.381	0.132	0.384	0.488	0.014	0.780
% Change	4.13	0.45	-15.91	-1.22	-0.00	7.14	6.15

Notes. Standard errors, tw-way clustered at the county and year level, are reported in parentheses. Regressions include county fixed effects and year fixed effects. All specifications additionally control for state fixed effects interacted with indicators for age-of-majority laws. Regressions are weighted by the number of births in each cell.
*** p<0.01, ** p<0.05, * p<0.1

Table 4 - Effects of FFCU Openings on County Fertility Rates, Overall and by Race and Maternal Age

	Outcome: Fertility Rate, Subpopulation:							
	All Women	White Women	Black Women	Age 20 - 24	Age 25 - 29	Age 30 - 34	Age 35 - 39	Age 40 - 49
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post FFCU Opening	-1.8251**	-2.3259***	-3.0526	-3.9553**	-5.0122***	-1.4039*	0.6427*	0.0782
	(0.8416)	(0.7734)	(1.9054)	(1.6413)	(1.5628)	(0.8217)	(0.3317)	(0.0616)
Observations	11750	11750	11668	11750	11750	11750	11750	11750
Mean DV	63.079	61.691	70.694	115.272	116.140	66.318	23.646	2.512
% Change	-2.89	-3.77	-4.32	-3.43	-4.32	-2.12	2.72	3.11

Notes. Each column reports estimates from a two-stage difference-in-differences model of the effect of feminist credit union openings on county fertility rates for the indicated population. Fertility rates are measured as births per 1,000 women. Birth counts are constructed from the natality records, and population denominators are from SEER single-age county population estimates; age-specific fertility rates use the corresponding female population at risk. The sample covers women ages 20–49. Regressions include county and year fixed effects, county-year covariates, and state fixed effects interacted with indicators for age-of-majority laws. Regressions are weighted by the female population at risk, and standard errors are two-way clustered at the county and year levels. Mean DV reports the population-weighted pre-period mean of the dependent variable. Percent change is calculated relative to the corresponding Mean DV.
*** p<0.01, ** p<0.05, * p<0.1

Table 5 - Mechanism Channels: Effect of FFCU openings on women's Outcomes

	Data and Outcome:								
	CPS-ASEC, 1970-1989							**Economic Census, 1972-1987**	
	Employed	Full-time	Part-time	Separated or divorced	Renter	Own with Mortgage	Own without Mortgage	Number of WOBs per 1,000 women	Number of paid employees in WOBs per 1,000 women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
FFCU Opening	0.011**	0.020***	-0.010***	-0.000	0.254***	-0.091***	-0.163***	2.833*	3.102***
	(0.005)	(0.005)	(0.003)	(0.002)	(0.019)	(0.012)	(0.013)	(1.641)	(1.145)
Observations	966044	966044	966044	966044	963902	963902	963902	868	868
Mean DV	0.405	0.379	0.135	0.100	0.235	0.411	0.354	27.817	18.284
% Change	2.78	5.25	-7.23	-0.04	108.01	-22.14	-45.99	10.18	16.97

Notes of columns 1-7: Each column reports regression results from a two-stage difference-in-differences model, weighted using CPS-ASEC survey weights. Race (White, Black, other) and education (high school, some college, college degree, graduate degree) were included as individual-level explanatory variables. All specifications additionally control for state fixed effects interacted with indicators for age-of-majority laws. Standard errors are two-way clustered at the metro-area and year level. Metro area and year dummies are also included.
Notes of columns 8-9: Each column reports regression results from a two-stage difference-in-differences model weighted using MSA-level population of women 18+. The data are from the MSA-level 1972, 1977, 1982, and 1987 censuses of women-owned businesses from the Census Bureau. All specifications additionally control for state fixed effects interacted with indicators for age-of-majority laws. Standard errors are tow-way clustered at the MSA and year level. MSA and year fixed effects are also included.
*** p<0.01, ** p<0.05, * p<0.1

Figures

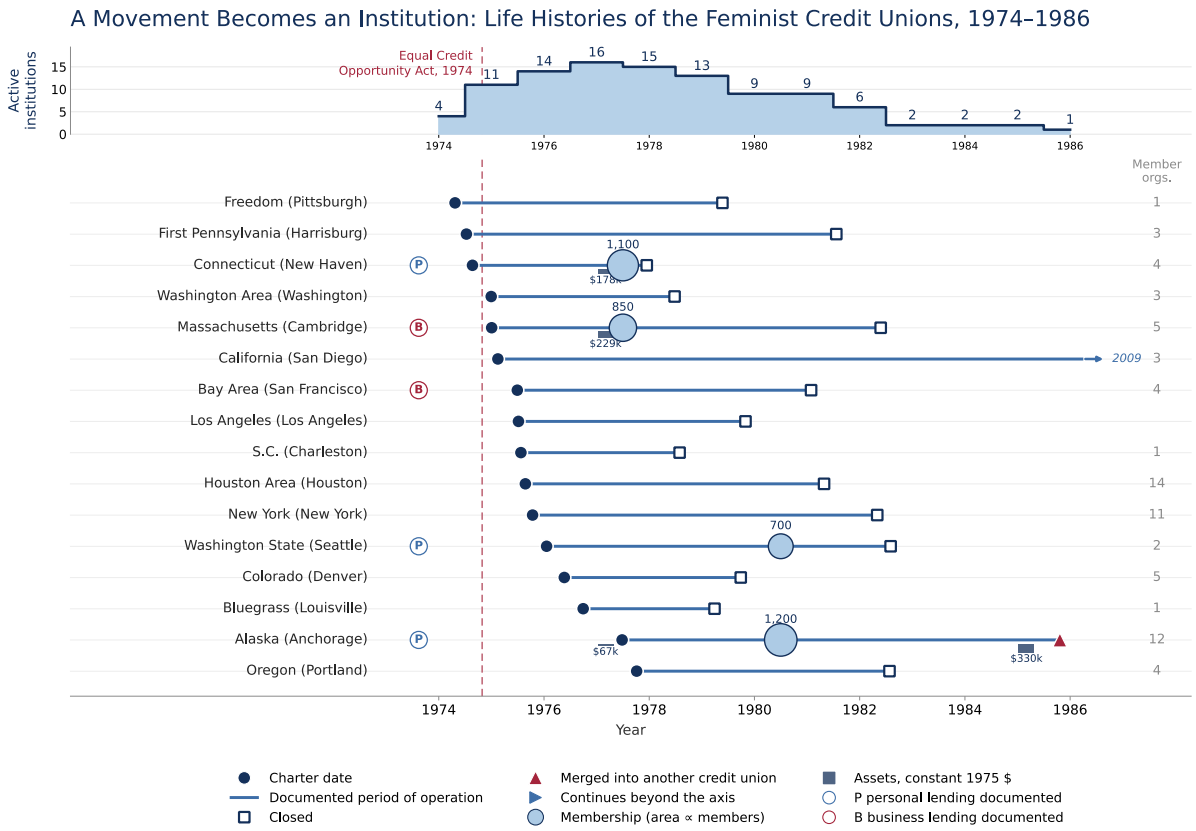


Figure 1 - Life Histories of the Feminist Credit Unions, 1974–1986

Notes: The figure traces the institutional histories of the sixteen federally chartered feminist credit unions in the sample. Filled circles indicate charter dates, horizontal lines indicate documented periods of operation, open squares indicate closures, and triangles indicate mergers into other credit unions. Arrows denote institutions that continued operating beyond the displayed period. The shaded area in the upper panel shows the number of active feminist credit unions in each year. Bubble size is proportional to documented membership, while asset values are reported in constant 1975 dollars. “P” and “B” indicate institutions for which personal and business lending, respectively, are documented. The right-hand column reports the number of sponsoring member organizations associated with each institution. The vertical dashed line marks passage of the Equal Credit Opportunity Act in 1974. Charter and closing dates are from the National Credit Union Administration FOIA response 2026-FOI-068. Membership, asset, and service information is displayed only when documented by the available sources; blank entries therefore indicate unavailable information rather than zero values. Assets are deflated to 1975 dollars using the CPI-U. The Detroit Feminist Federal Credit Union, established in 1973 and generally regarded as the first feminist credit union, is not included because it does not appear in the NCUA federal charter records used to construct the sample.

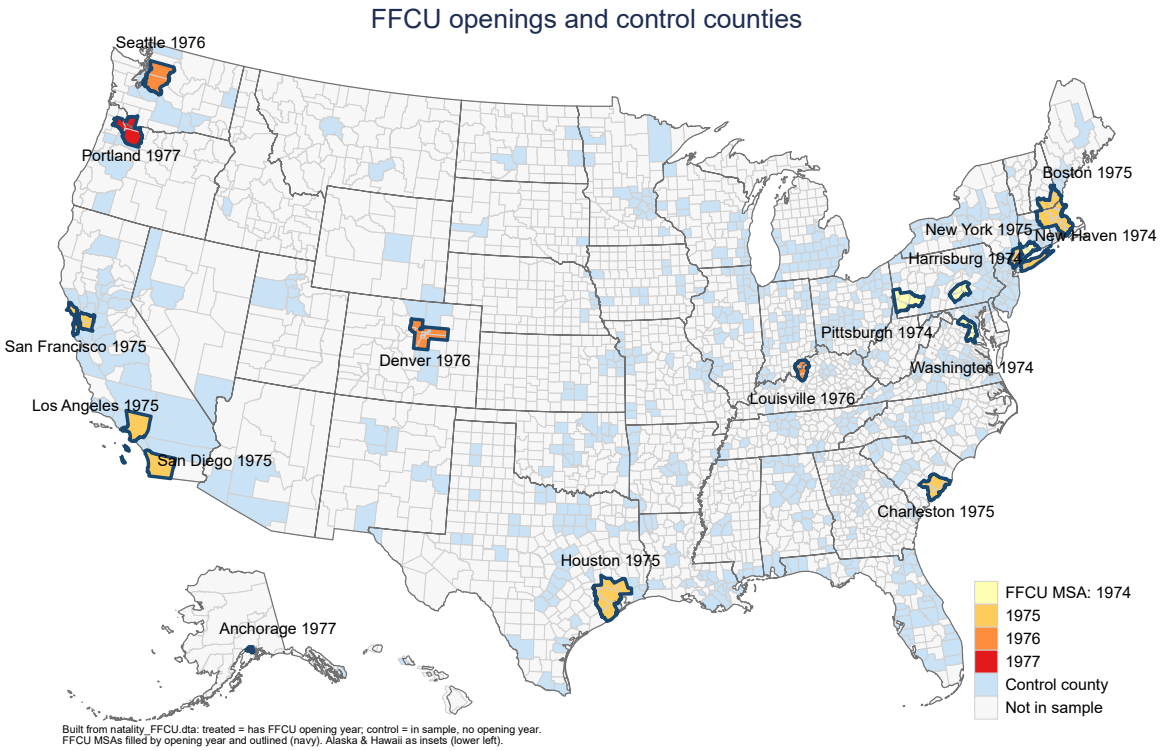


Figure 2 - Geographic Distribution of FFCU Openings and Control Counties

Notes: The figure shows the geographic distribution of feminist credit union (FFCU) openings and the comparison counties used in the analysis. Treated metropolitan statistical areas are outlined in navy and shaded according to the year of the FFCU opening, ranging from 1974 to 1977. Labels identify the metropolitan area and opening year. Light-blue counties denote comparison counties included in the estimation sample that did not experience an FFCU opening, while unshaded counties are not included in the sample. Alaska and Hawaii are displayed as insets.

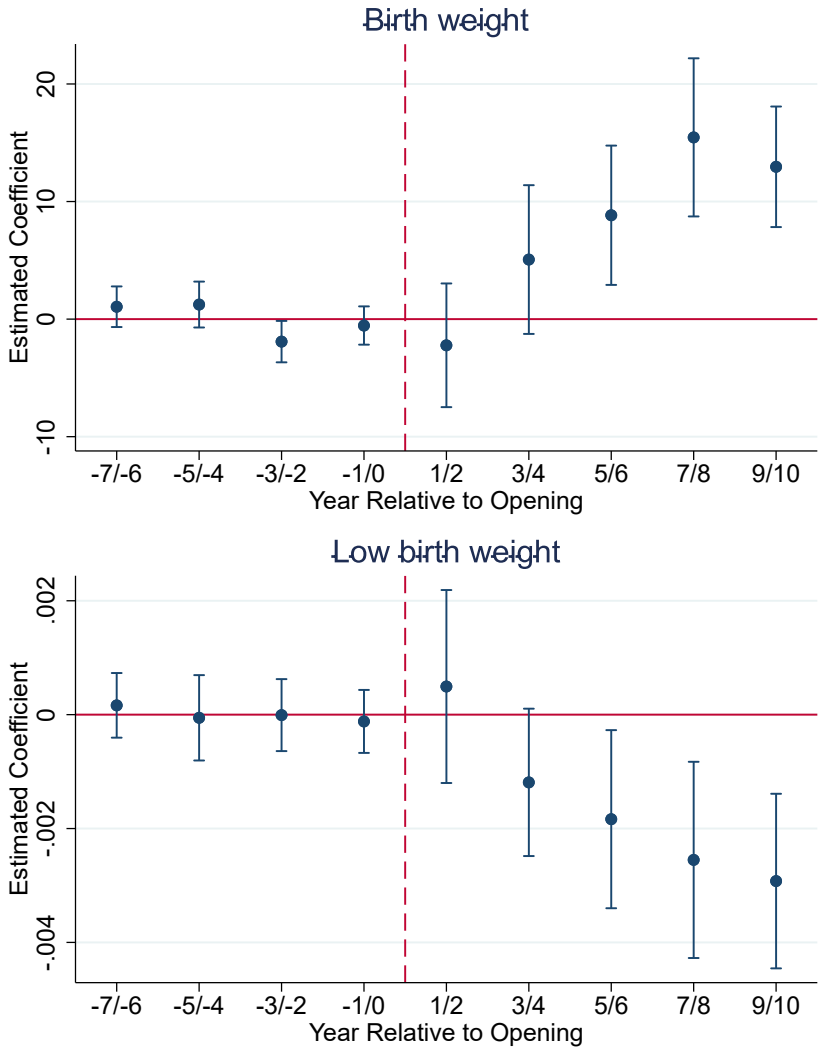


Figure 3 - Event-Study Estimates for Birth Weight and Low Birth Weight

Notes: Each panel reports event-study estimates using the two-stage difference-in-differences estimator of Gardner et al. (2022) for the effect of feminist credit union openings on the indicated birth outcome. Regressions include county and year fixed effects, controls for maternal race, age, education, and birth order, and state fixed effects interacted with indicators for age-of-majority laws. Regressions are weighted by the number of births in each cell, and standard errors are two-way clustered at the county and year levels. Vertical bars denote confidence intervals. The horizontal line marks zero, and the vertical dashed line indicates the timing of the credit union opening.

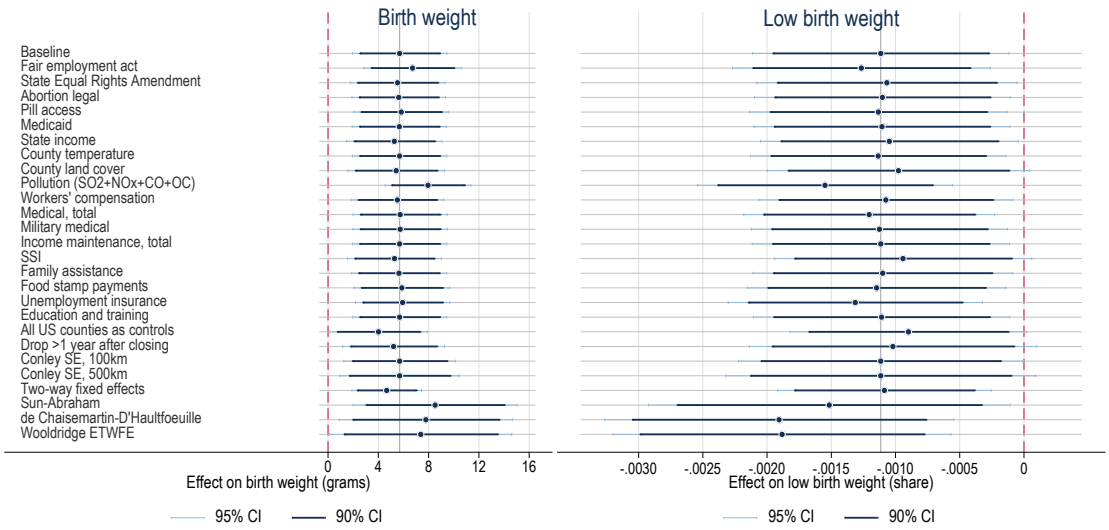
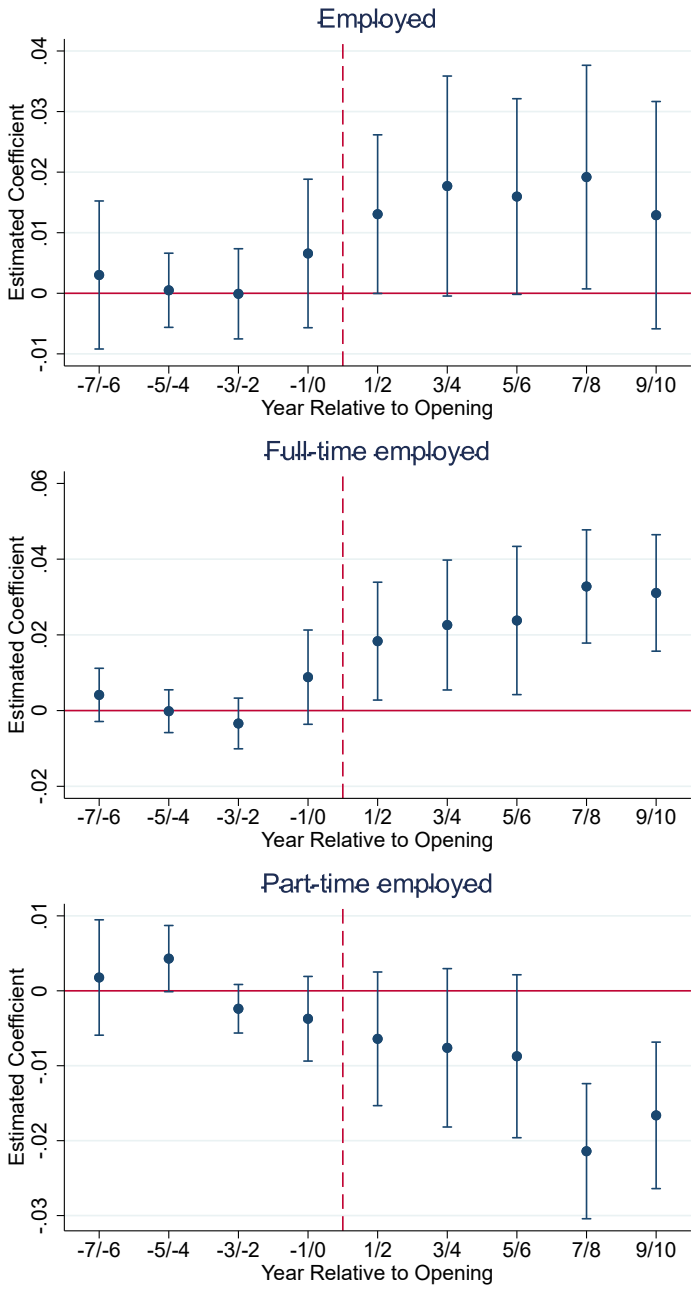


Figure 4 - Robustness of Infant-Health Effects to Alternative Controls and Inference

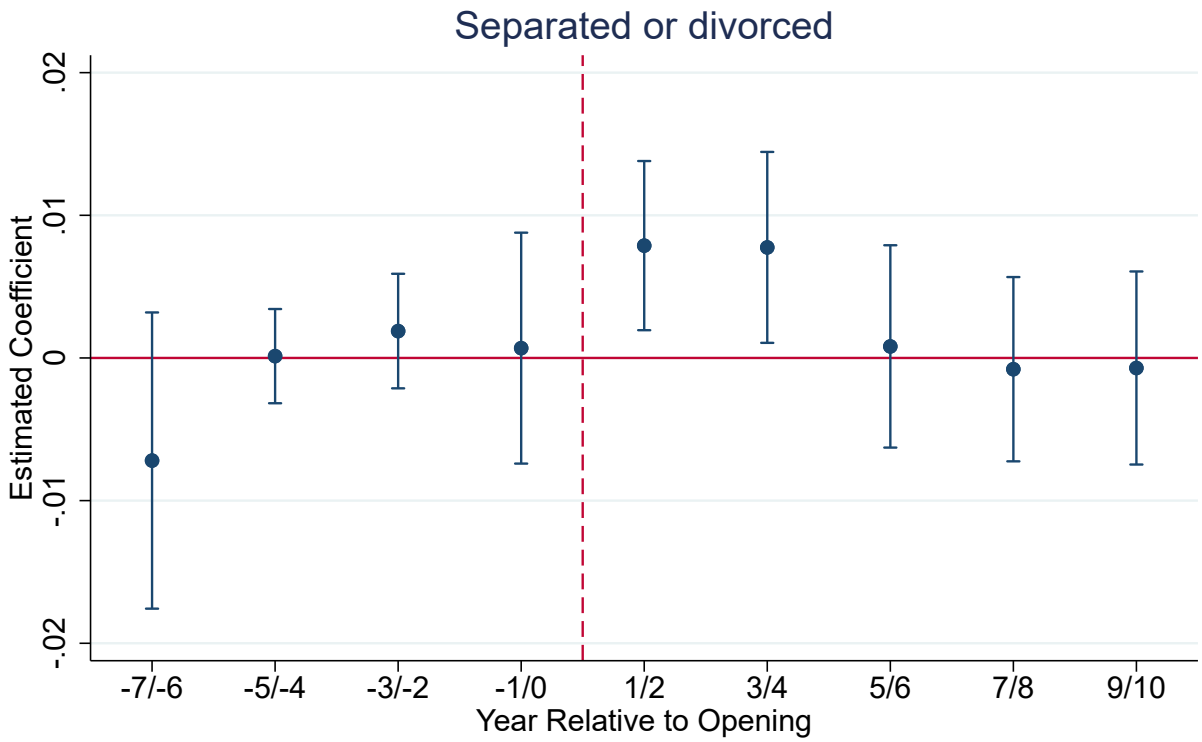
Notes: The figure reports robustness estimates for the effects of feminist credit union openings on birth weight and low birth weight. Each row presents the estimated coefficient from the baseline specification or from an alternative specification incorporating the indicated policy, economic, environmental, or social control. The left panel reports effects on birth weight in grams, and the right panel reports effects on the probability of low birth weight. The final two specifications report inference using Conley spatial standard errors with 100-km and 500-km distance cutoffs. Dark and light horizontal bars denote 90 and 95 percent confidence intervals, respectively. The vertical dashed line marks zero.

Appendix A



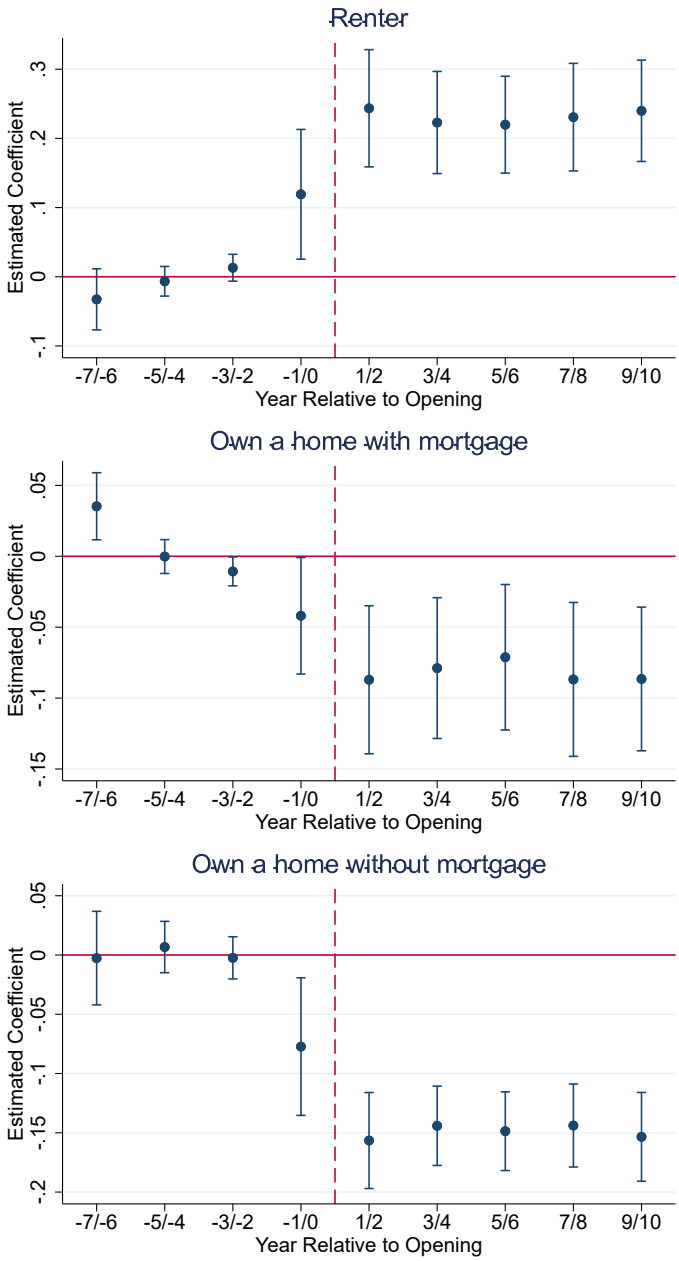
Appendix Figure A-1 - Event study of FFCU openings on women's employment, 1970-1989 CPS-ASEC

Notes: Each panel reports event-study estimates using the two-stage difference-in-differences estimator of Gardner et al. (2022) for the effect of feminist credit union openings on the indicated employment outcome. Regressions are weighted using CPS-ASEC survey weights and include metropolitan-area and year fixed effects, controls for race and education, and state fixed effects interacted with indicators for age-of-majority laws. Standard errors are two-way clustered at the metropolitan-area and year levels. Vertical bars denote confidence intervals. The horizontal line marks zero, and the vertical dashed line indicates the timing of the credit union opening.



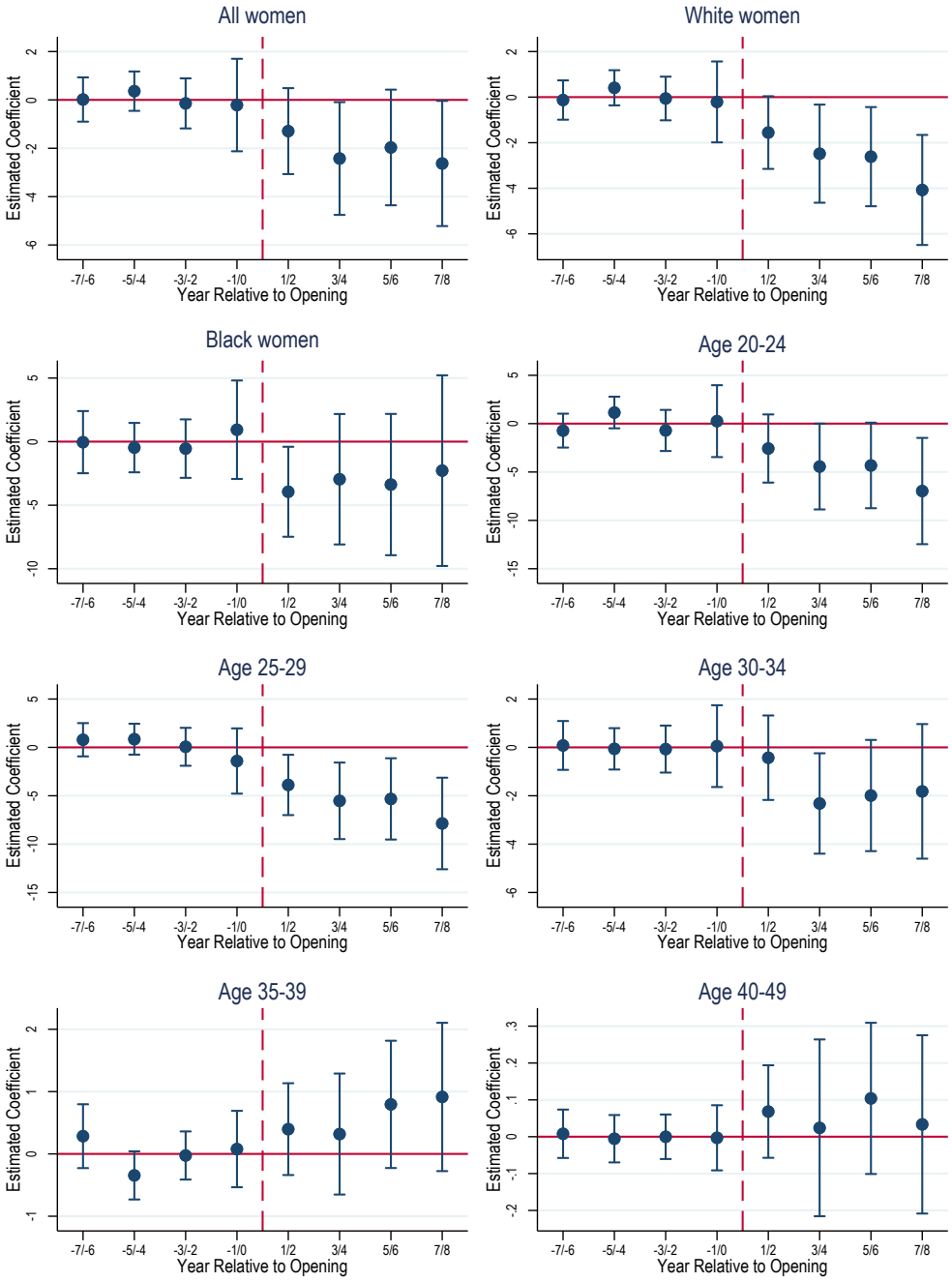
Appendix Figure A-2 - Event study of FFCU openings on divorce, 1970-1989 CPS-ASEC

Notes: The figure reports event-study estimates using the two-stage difference-in-differences estimator of Gardner et al. (2022) for the effect of feminist credit union openings on separation or divorce. The regression is weighted using CPS-ASEC survey weights and includes metropolitan-area and year fixed effects, controls for race and education, and state fixed effects interacted with indicators for age-of-majority laws. Standard errors are two-way clustered at the metropolitan-area and year levels. Vertical bars denote confidence intervals. The horizontal line marks zero, and the vertical dashed line indicates the timing of the credit union opening.



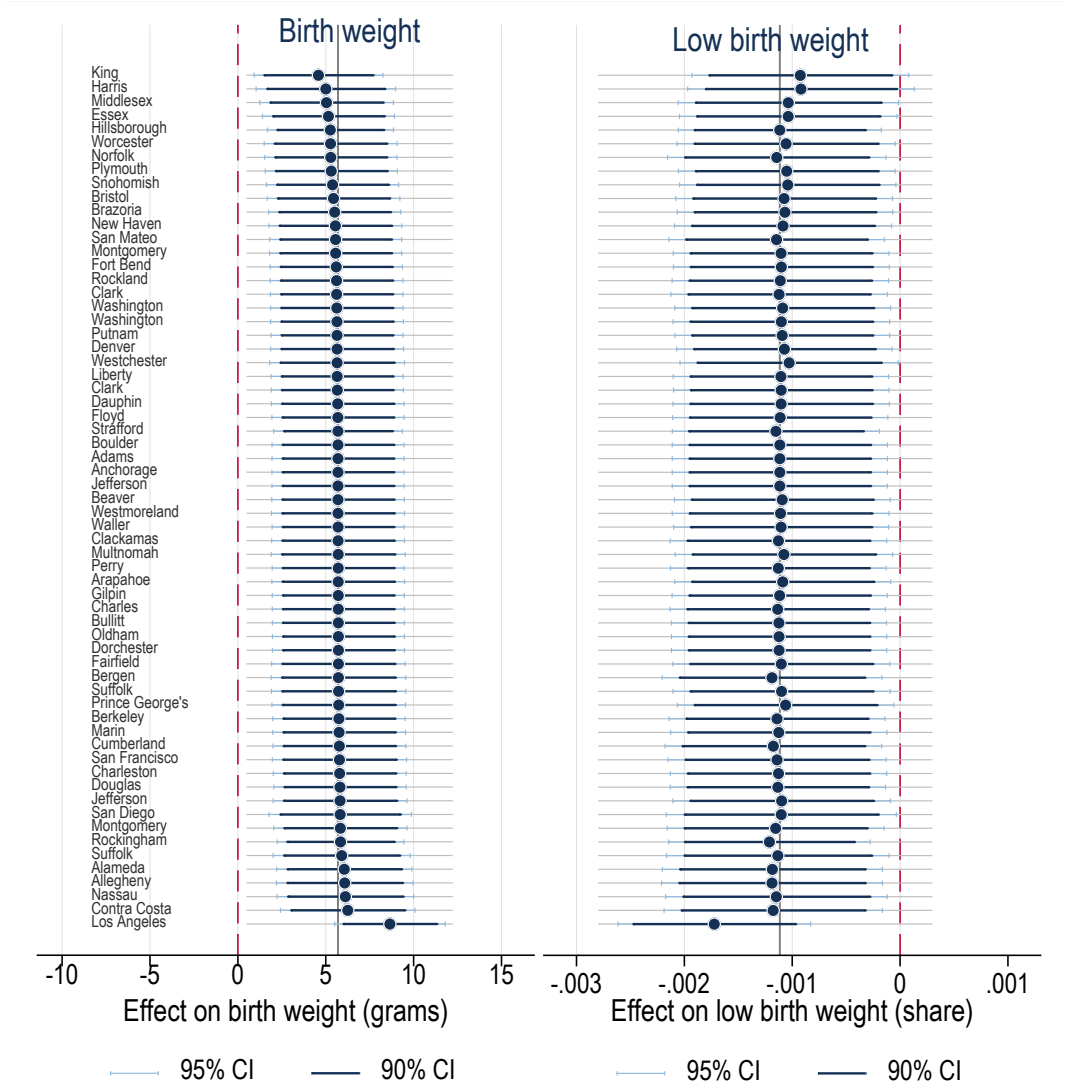
Appendix Figure A-3 - Event study of FFCU openings on women's home ownership, 1970-1989 CPS-ASEC

Notes: Each panel reports event-study estimates using the two-stage difference-in-differences estimator of Gardner et al. (2022) for the effect of feminist credit union openings on the indicated housing-tenure outcome. Regressions are weighted using CPS-ASEC survey weights and include metropolitan-area and year fixed effects, controls for race and education, and state fixed effects interacted with indicators for age-of-majority laws. Standard errors are two-way clustered at the metropolitan-area and year levels. Vertical bars denote confidence intervals. The horizontal line marks zero, and the vertical dashed line indicates the timing of the credit union opening.



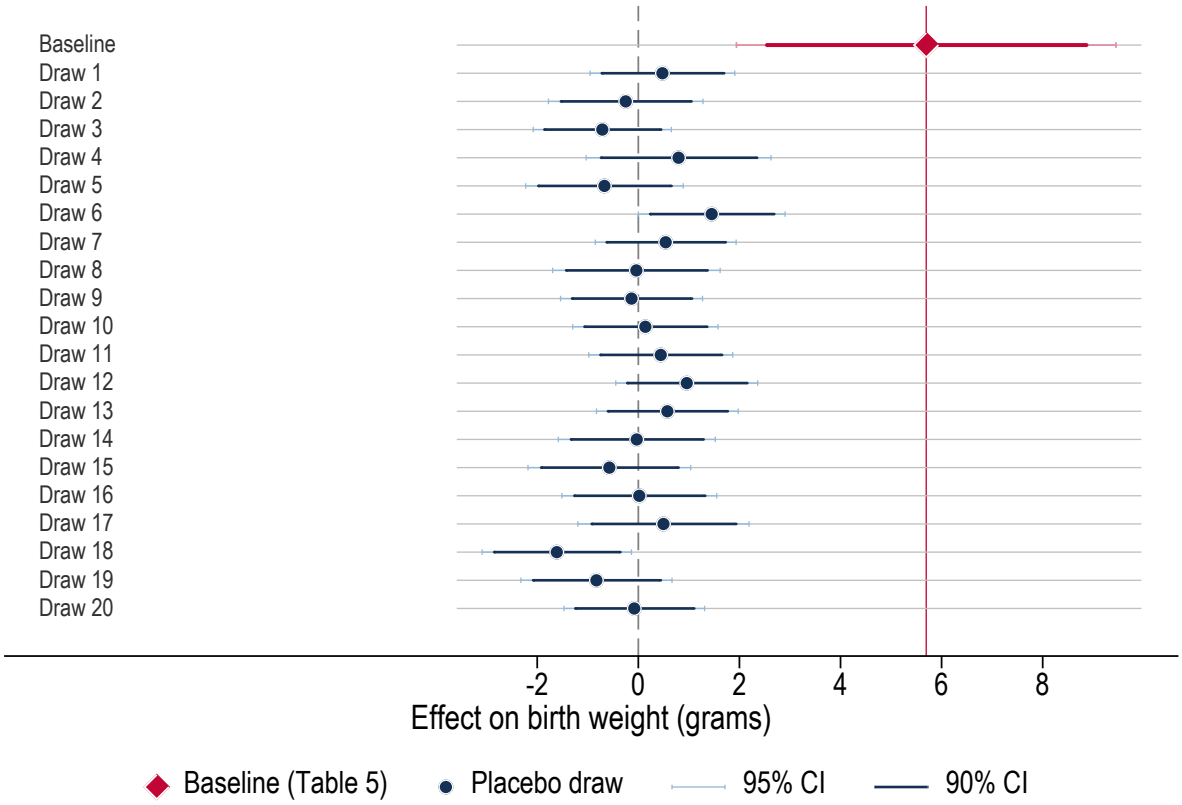
Appendix Figure A-4 - Event-Study Estimates of FFCU Openings on Fertility Rates, by Race and Age

Notes: Each panel reports event-study estimates using the two-stage difference-in-differences estimator of Gardner et al. (2022) for the effect of feminist credit union openings on fertility rates for the indicated group. Fertility rates are measured as the number of births per 1,000 women. Regressions include county and year fixed effects and state fixed effects interacted with indicators for age-of-majority laws. Standard errors are two-way clustered at the county and year levels. Event time is grouped into two-year intervals relative to the opening of a feminist credit union. Vertical bars denote confidence intervals. The horizontal line marks zero, and the vertical dashed line indicates the timing of the credit union opening.



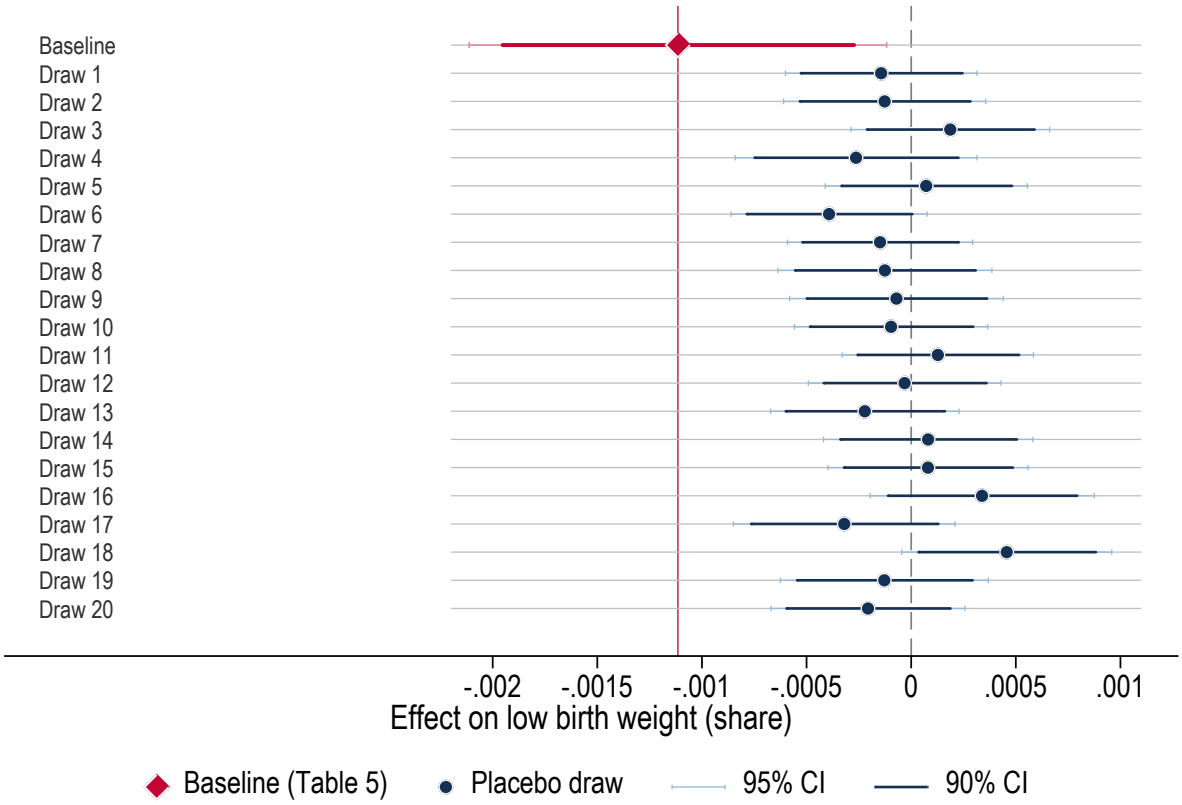
Appendix Figure A-5 - Leave-One-County-Out Robustness of Infant-Health Effects

Notes: Each row reports the estimated effect of a feminist credit union opening after excluding the indicated county from the estimation sample. The left panel reports effects on birth weight in grams, and the right panel reports effects on the probability of low birth weight. Estimates are obtained using the two-stage difference-in-differences estimator of Gardner et al. (2022) and the baseline natality specification. Regressions include county and year fixed effects, controls for maternal race, age, education, and birth order, and state fixed effects interacted with indicators for age-of-majority laws. Regressions are weighted by the number of births in each cell, and standard errors are two-way clustered at the county and year levels. Dots denote coefficient estimates, dark horizontal bars denote 90 percent confidence intervals, and light horizontal bars denote 95 percent confidence intervals. The solid gray vertical line indicates the corresponding full-sample baseline estimate, and the red dashed vertical line marks zero.



Appendix Figure A-6 - Placebo Tests for the Effect of FFCU Openings on Birth Weight

Notes: The figure compares the baseline estimate of the effect of feminist credit union openings on birth weight with estimates from 20 placebo draws. The red diamond reports the baseline estimate from the main specification, while the blue circles report the estimates obtained from the placebo assignments. Dark horizontal bars denote 90 percent confidence intervals, and light horizontal bars denote 95 percent confidence intervals. The gray dashed vertical line marks zero, and the red vertical line indicates the baseline point estimate. The placebo estimates are centered near zero and are substantially smaller than the baseline estimate, providing evidence that the estimated improvement in birth weight is unlikely to arise from randomly assigned treatment patterns.



Appendix Figure A-7 - Placebo Tests for the Effect of FFCU Openings on Low Birth Weight

Notes: The figure compares the baseline estimate of the effect of feminist credit union openings on low birth weight with estimates from 20 placebo draws. The red diamond reports the baseline estimate from the main specification, while the blue circles report estimates obtained from the placebo assignments. Dark horizontal bars denote 90 percent confidence intervals, and light horizontal bars denote 95 percent confidence intervals. The gray dashed vertical line marks zero, and the red vertical line indicates the baseline point estimate. The placebo estimates are concentrated near zero and are substantially smaller in magnitude than the baseline estimate.