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Stefan Bauernschuster
University of Passau, CESifo and IZA

Michael Grimm
University of Passau, IZA and RWI

Cathy M. Hajo
Ramapo College

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

The Impact of Margaret Sanger's Birth Control Clinics on Early 20th Century U.S. Fertility and Mortality*

Margaret Sanger established the first birth control clinic in New York in 1916. From the mid-1920s, "Sanger clinics" spread over the entire U.S. Combining newly digitized data on the roll-out of these clinics, full-count Census data, and administrative vital statistics, we find that birth control clinics accounted for 5.0–7.8% of the overall fertility decline until 1940. Moreover, birth control clinics had a significant and meaningful negative effect on the incidence of stillbirths and infant mortality. The effect of birth control clinics on puerperal deaths is consistently negative, yet insignificant. Further suggestive evidence points towards positive effects on female employment.

JEL Classification: D10, J13, J23, N32, O12

Keywords: birth control, fertility, mortality, Margaret Sanger, demographic transition

Corresponding author:
Michael Grimm
School of Business Administration, Economics, and Information Systems
University of Passau
Innstrasse 27
94032 Passau
Germany

E-mail: michael.grimm@uni-passau.de

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"Margaret Sanger (1879-1966) led a successful campaign from 1914 to 1937 to remove the stigma of obscenity from contraception and to establish a nationwide system of clinics where women could obtain reliable birth control services. She organized research, recruited manufacturers for birth control devices, and won court battles that modified the Comstock laws and laid the groundwork for the formal acceptance of birth control by organized medicine in 1937. After World War II she played key roles in the rise of an international planned parenthood movement and in the development of the birth control pill. Through these achievements she had a greater impact on the world than any other American woman."

– Reed (1978, p. 67)

1. Introduction

In 1873, the U.S. Congress passed the Comstock Act, making it illegal to mail, ship, or import "obscene" articles, drugs, medicines, and printed materials, including any article or information related to the "prevention of conception". In response to these restrictive measures, a counter-movement advocating for birth control and sexual freedom emerged at the beginning of the 20th century. During the 1930s, the birth control movement gained momentum through a series of legal victories. Simultaneously, public support increased, albeit with some hesitation from the medical community.[1] In 1939, the Birth Control Federation was founded, which changed its name to Planned Parenthood Federation three years later. Despite the birth control movement's success, many U.S. states continued to enforce advertising and sales bans on contraceptives until the late 1960s (Bailey, 2010).

Margaret Sanger was one of the leading figures of the birth control movement. On 16 October 1916, she established the United States' first birth control clinic in New York, Brownsville, Brooklyn—despite facing significant opposition (Engelman, 2011, p. 77). At this clinic, women received advice on effective contraception, sexual health and hygiene, with the aim of improving women's and children's health and alleviating poverty among working-class women. Although this clinic was shut down by the police ten days after its opening, similar 'Sanger clinics' began to appear throughout the U.S. from 1923 onwards, with over 600 established by 1940 (Hajo, 2010, p. 21). Despite the historical relevance of Margaret Sanger's birth control movement, the causal impact of her birth control clinics has not yet been assessed. Filling this gap should help to enhance our understanding of the historical fertility transition and its broader socio-economic effects in the U.S.

In this paper, we evaluate the impact of the early birth control clinics on fertility, stillbirths, infant mortality, and puerperal deaths. To this end, we have digitized detailed data on the roll-out of birth control clinics across U.S. counties from 1916 to 1940. We

[1] Engelman (2011) provides an excellent overview of the birth control movement.

combine this unique data with full-count Census data of 1920, 1930 and 1940, yearly administrative vital statistics at the county level, and yearly administrative causes of death data at the city level. The Census data allow us to investigate the effects of birth control clinics on fertility. Using the administrative vital statistics, we re-estimate the effect of birth control clinics on fertility and assess the impact on stillbirths and infant mortality. Causes of death data at the city level allow us to inspect the impact of the clinics on puerperal deaths. We expect that Sanger's birth control clinics satisfied a demand for effective contraception and hence increased birth spacing and reduced fertility rates. Increasing the interval between births can reduce the incidence of fetal and infant mortality as short interpregnancy intervals might prevent maternal folate resources from replenishing adequately and increase the risk of vertical transmission of infections to the fetus as well as horizontal transmission of infections among siblings (Conde-Agudelo *et al.*, 2012). Lower fertility can also alleviate budget constraints for households and thus increase health investments per child. Additionally, an increase in birth spacing and a decrease in fertility rates might lower maternal deaths, especially puerperal deaths.

In the first part of the empirical analysis, we use full-count Census data and exploit differences in the length of exposure to birth control clinics conditional on birth cohort by state fixed effects and a rich set of socio-economic individual and county level controls. Our findings reveal statistically significant and economically meaningful effects of birth control clinics on fertility. Specifically, exposure to a birth control clinic throughout the entire fertile period from age 15 to 39 reduced fertility by 12-15%. From 1920 to 1940, birth control clinics thus account for 5% of the overall fertility decline. Event-study analyses provide evidence for the validity of the key identifying assumption. The findings are robust to alternative specifications in which we control for county and year fixed effects, restrict the sample to big city counties, restrict the sample to ever-treated counties, and use Pseudo Poisson Maximum Likelihood estimators. Moreover, we demonstrate that our results are not sensitive to conventional assumptions about selection on unobservables following Altonji *et al.* (2005) and Oster (2019). Finally, we find suggestive evidence for positive knock-on effects on female labor supply.

In the second part of the empirical analysis, we use yearly county-level administrative vital statistics and exploit the staggered roll-out of birth control clinics across counties over time. To avoid pitfalls of standard two-way fixed effects models in case of heterogeneous treatment effects, we use the interaction weighted estimator proposed by Sun and Abraham (2021). The findings confirm the negative impact of birth control clinics on fertility. Overall, birth control clinics explain 7.8% of the total fertility decline in this specification. Moreover, we find that birth control clinics led to a reduction of stillbirths and infant mortality. Insignificant and small pre-treatment coefficients corroborate the validity of the common trends assumption. The results suggest that birth control clinics particularly reduced births that posed significant health risks. Complimentary city-level data yield negative but insignificant effects of birth control clinics on puerperal deaths. We provide evidence that the roll-out of birth control clinics is not confounded by more general improvements in counties' health infrastructure. Moreover, the results are robust to extending the period of observation, using the last treated counties instead of never-treated counties as a control group, and employing the estimator proposed by Callaway and Sant'Anna (2021) instead of Sun and Abraham's (2021) interaction weighted estimator.

Our contribution to the literature is threefold. First, we add to the general debate about the role of family planning services (see, e.g. Udry *et al.*, 1976; Cutright and Jaffe, 1977; Molyneaux and Gertler, 2000; Mellor, 1998; Bailey, 2012, 2013; Canning and Schultz, 2012). In particular, we look at the first family planning initiatives in the history of the U.S. that have so far only been discussed qualitatively (see, e.g., Reed, 1978; Chesler, 1992; McCann, 1994; Gordon, 2002; Hajo, 2010; Engelman, 2011). Second, we add to the general understanding of the U.S. demographic transition (see, e.g. Bourne Wahl, 1992; Steckel, 1992; Haines, 2000; Greenwood and Seshadri, 2002; Hacker, 2003; Haines and Hacker, 2006; Jones and Tertilt, 2006; Curtis White, 2008; Bleakley and Lange, 2009; Bailey and Collins, 2011; Guinnane, 2011; Wanamaker, 2012; Aaronson *et al.*, 2014; Bailey *et al.*, 2014; Hansen *et al.*, 2018; Bailey and Hershbein, 2018; Beach and Hanlon, forthcoming; Ager *et al.*, 2020; Grimm, 2021). Third, we complement the literature that focuses specifically on the role of anti-abortion legislation (Lahey, 2014a,b, 2022) and modern contraception (Goldin and Katz, 2002; Bailey, 2006, 2010) in the U.S. The situation in the early 20th century differs from today's low income country context. At that time, people were poorly informed about contraceptives; even the medical profession was rather uninformed and not trained in these issues. Moreover, contraceptives were hardly accessible through the market and largely illegal; modern highly effective contraceptives did not yet exist.

The remainder of the paper is structured as follows. In Section 2, we provide information on the historical context, the birth control movement, the roll-out of Sanger's birth control clinics and the services these clinics delivered. In Section 3, we introduce the data sets we use. Section 4 explains the empirical strategy and presents the results with respect to fertility and labor supply using the Census data. Section 5 explains the empirical strategy we employ using the administrative data, cross-validates the effects on fertility and complements these results with an analysis of the effects on stillbirths, infant mortality and puerperal deaths. Section 6 concludes.

2. Historical Background

2.1. The U.S. fertility transition, the Comstock Act, and anti-abortion laws

The fertility transition in the U.S. started to take speed in the mid 19th century (Jones and Tertilt, 2006; Bailey and Hershbein, 2018). At the time, most couples used withdrawal and abstinence as well as extended breastfeeding to space births (Engelman, 2011, p. 4; Reed, 1978, p. 3; David *et al.*, 2007), but also abortion and—as a method of last resort especially among the very poor—infanticide. The second half of the 19th century also witnessed the emergence of the first rudimentary contraceptive devices such as condoms, vaginal sponges, pessaries, and rubber syringes. Initially, these devices were widely advertised in newspapers and flyers and marketed by pharmacies, doctors, midwifes, druggists, and other entrepreneurs (Engelman, 2011, p. 10). At the same time, marriage manuals diffused that explicitly considered that couples could and should enjoy sexual pleasure without procreation (see, e.g., Knowlton, 1832; Owen, 1876).

The religious community as well as the American Medical Association (AMA), and later a larger morality movement, disapproved this development. Anthony Comstock, a leading political figure of the Christian morality movement, associated contraception with illicit sex and pornography. In 1873, he succeeded in making the U.S. Congress codify the Comstock Act, which made it illegal to mail, ship, or import articles, drugs, medicines, and printed materials deemed "obscene", including any article or information related to the prevention of conception (Engelman, 2011, p. 15). While the implementation of the Comstock Act varied across states, it was mostly strictly enforced (Bailey, 2010). People not complying with the law were subject to fines or even arrest, with the respective goods confiscated. Additionally, between 1860 and 1890, many states passed strict anti-abortion laws, which further slowed down the fertility decline (Engelman, 2011, p. 13; Lahey, 2014a; Lahey, 2014b).

2.2. The birth control movement

In opposition to these restrictive interventions, a birth control movement and activism for sexual freedom emerged at the beginning of the 20th century.[2] Margaret Sanger, a nurse whose mother had been through 18 pregnancies in 22 years and died at age 50 of tuberculosis and cervical cancer, became a leading figure of this movement. Sanger was concerned about the hardship brought about by repeated pregnancies, childbirths and self-induced abortions among poor women. Therefore, she devoted her life to make "birth control", a term she coined to refer to contraception, legal and widely accessible (McCann, 1994, p. 210). During a visit to the Netherlands, Sanger was struck by the low rates of infant and maternal mortality, which were not least attributed to the availability of birth

[2] McCann (1994) provides a detailed chronology of events in the U.S. birth control movement.

control clinics providing contraceptive counseling to women (McCann, 1994, p. 59, p. 211; Hajo, 2010, p. 11).

On 16 October 1916, Margaret Sanger established the United States' first birth control clinic (or center for contraceptive instruction) in New York, Brownsville, Brooklyn. The clinic provided women with advice on effective contraception, sexual health and hygiene, and was sponsored by wealthy supporters. It was promoted through flyers in Yiddish, Italian, and English (see Appendix Figure A.1) as well as press releases (Reed, 1978, p. 106). Sanger was convinced that pessaries ("occlusive diaphragm"), at a cost of one to two dollars, were the most effective contraceptive method available to women at the time and that proper instruction and follow-up visits were crucial to ensure their efficacy. She argued that only birth control clinics could provide such services (McCann, 1994, p. 59; Hajo, 2010, p. 67).[3] Sanger was generally against abortions because of the associated health risks. Moreover, supporting abortions would have made the acceptance by the morality movement even more difficult (Engelman, 2011, p. 76; Hajo, 2010, p. 13). Therefore, birth control clinics should focus on contraceptives, in particular pessaries, and refer women to doctors for therapeutic abortions only in exceptional cases. Despite the Brownsville clinic's success, with over 480 women attending in the first few days, it was shut down by the police ten days after it opened (Engelman, 2011, p. 78ff; Hajo, 2010, p. 23). Margaret Sanger was subsequently sentenced to 30 days in jail.

Sanger founded the Birth Control League of New York, which organized mailings, conferences, lectures as well as exhibits and advocated for legislative change. Similar leagues emerged in other cities, leading to the formation of over 30 birth control organizations across the country by 1917. In 1921, Sanger established the American Birth Control League, which later became the Planned Parenthood Federation of America (Engelman, 2011, p. 129). By 1924, the league had more than 27,000 members and 10 branches. Sanger also founded and edited the Birth Control Review, a journal devoted to the birth control movement and mainly sold on the streets (Engelman, 2011, p. 99). Sanger traveled extensively to give speeches, form alliances and raise funds for her cause. However, her speeches were often interrupted or halted by the police, and she faced multiple criminal accusations, resulting in short stints in jail. She also visited European countries to learn from their more advanced family planning policies. Over time, Sanger slightly adapted her strategy in her fight for the legalization of contraception. Instead of emphasizing women's right to self-determination, she began to highlight the health benefits of child spacing for both mothers and children. With this strategy, she hoped to gain support from doctors and hospitals while avoiding legal conflicts (McCann, 1994, p. 60; Engelman, 2011, p. 101). Her ultimate goal was to establish birth control clinics across the nation (McCann, 1994, p. 60).

[3] McCann (1994, p. 60) explains that a "setting was needed where trained practitioners could screen women for existing health problems, fit devices, and teach women to insert the device themselves."

Figure 1 — The Sanger Clinic, 46 Amboy Street, Brooklyn

Notes: Photo published by the Social Press Association, New York, on October 27, 1916, provided by the Library of Congress Prints and Photographs Division Washington, D.C. The photo shows mothers waiting in front of the United States' first birth control clinic in 46 Amboy Street, Brooklyn, New York.

The nationwide roll-out of Sanger's birth control clinics commenced in the mid-1920s (Engelman, 2011, p. 153). The widespread prevalence of sexually transmitted diseases among U.S. servicemen during World War I had turned sexuality and contraception into a public health issue and thus a legitimate scientific research topic (Engelman, 2011, p. 108). In the course of the 1930s, legal victories further strengthened the birth control movement and weakened the anti-contraception laws. Simultaneously, birth control became more and more accepted by the public. Although contraception was finally approved by the American Medical Association in 1937 (McCann, 1994, p. 217), it was only tentatively endorsed by the medical profession. Moreover, advertising and sales bans on contraceptives persisted in many U.S. states until the end of the 1960s (Bailey, 2010). Therefore, women still often relied on ill-informed sources providing ineffective contraceptives (Tone, 2002, p. 155), not comparable with the effective pessaries provided by birth control clinics. There were two types of birth control clinics: independent clinics, such as the Brownsville clinic, typically operated by female activists and a part-time male medical doctor, often the an activist's spouse (Hajo, 2010, p. 73ff), and clinics within hospitals. Over time, the clinics gradually professionalized and were increasingly staffed with skilled practitioners who were either trained in the clinics themselves or in Sanger's

Clinical Research Bureau in New York.[4] Birth control clinics provided their services to women regardless of their ability to pay.[5] The clinics typically admitted only married women who already had several children since they primarily aimed to enable women to space their births, rather than to prevent them altogether. Admission requirements were checked at the entrance of a clinic before women were seen by a nurse or doctor (Hajo, 2010, p. 49ff). Initially, the clinics did not target African American women due to strict segregation laws. However, African Americans began to establish their own Sanger clinics following the same model. As segregation laws started to change, clinics served both White and African American women (Hajo, 2010, p. 83ff). Over the entire period, African Americans made up about 11% of all patients, which roughly corresponded to their population share (Hajo, 2010, p. 113).

Between 1916 and 1940 more than 650 "Sanger clinics" were established in the U.S (Hajo, 2010, p. 21). As shown in Figure 2, this process was prominently covered by the press. The figure plots the clinic openings (right scale) and newspaper articles mentioning the term "birth control" (left scale) over time. The first spike is clearly visible when the Brownsville clinic opened in 1916. In the following years, the spikes in the clinic expansion correlate with the spikes in press coverage. As opposition dwindled and birth control clinics became increasingly accepted in U.S. society in the 1930s, newspaper coverage declined and the correlation between newspaper coverage and the expansion of birth control clinics disappears in the data.

In 1939, the American Birth Control League merged with Sanger's Clinical Research Bureau in New York to form the Birth Control Federation of America. At tha point, Sanger became honorary president and relinquished active leadership. The organization changed its name to Planned Parenthood Federation of America in 1942. Margaret Sanger raised funds, including from Katharine Dexter McCormick and other sources, to support the development of the pill. Her activism for women's rights, birth control and planned parenthood continues to receive praise today. However, she is also criticized for her associations with eugenics and support for forced sterilization (Weisbord, 1973; Engelman, 2011, p. 130ff). Sanger died in Tucson, Texas in 1966.

3. Data

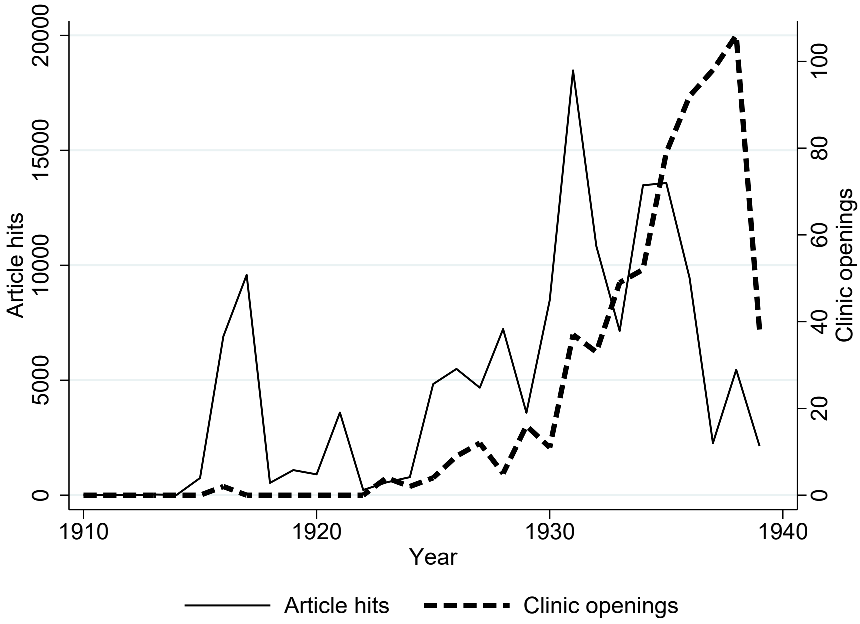
3.1. Birth control clinics

To measure exposure to a birth control clinic, we use a complete inventory of birth control clinics established in the U.S. before 1940. We compiled the data set by digitizing infor-

[4] The Clinical Research Bureau was the first legal birth control clinic in the United States, and soon grew into the world's leading contraceptive research center (Reed, 1978, p. 116; Hajo, 2010, p. 13).

[5] While the costs for an average patient was around USD 6.50, the highest fee charged was typically USD 5 (Reed, 1978, p. 116).

Figure 2 — Birth control clinic openings and press coverage



Notes: Data source: Birth control clinics statistics by Hajo (2010) and Newspapers.com. The left y-axis shows the number of press articles using the term "birth control" by year. The right y-axis shows the number birth control clinic openings by year.

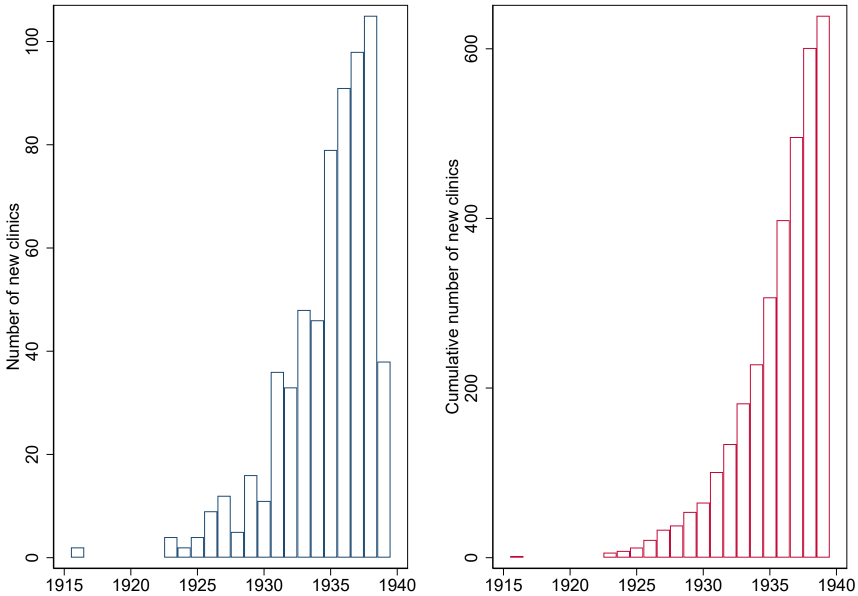
mation gathered by Hajo (2010) from various sources, including historical issues of the Birth Control Review and press archives. For each clinic, we obtained data on the county it was located, the year of establishment as well as, if applicable, the year of closure. The data set encompasses a total of 639 birth control clinics, which are geographically dispersed across 44 states.[6]

Figure 3 gives a first impression of the roll-out of birth control clinics over time. The left panel shows the number of newly established birth control clinics by year, while the right panel shows the cumulative number of clinics. The first birth control clinic opened in Brooklyn in 1916; a second one in the same year in St. Paul, Minnesota. Further birth control clinics did not open until the year 1923. In the early to mid-1930s, we observe increased dynamics in the roll-out. By the end of 1939, 639 birth control clinics had been established in the U.S. Note that some clinics closed soon after opening, primarily due to financial instability or a lack of staff (McCann, 1994, p. 61). In most of these cases, the closed clinic was replaced by another clinic in the region shortly afterward, or other clinics in the region were still available. Therefore, we use the year of the opening of a county's first clinic as the treatment variable in the empirical analysis. This also respects the fact that once a clinic had been operating it could have had an enduring effect as pessaries had been distributed, information had spread and norms might have changed.

It was not uncommon for women to travel to a clinic in a neighboring county if there

[6]We exclude the states of Alaska and Hawaii from the analysis.

Figure 3 — Birth control clinics over time

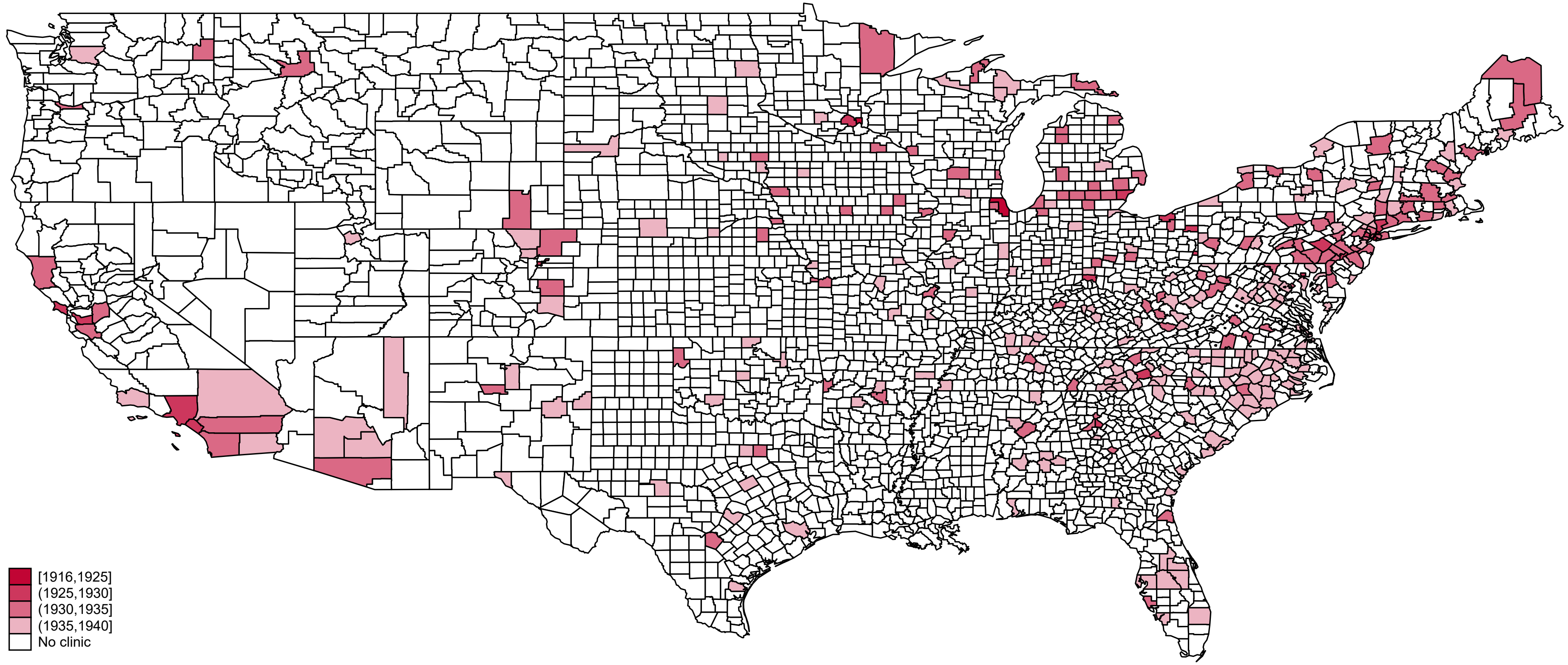


Notes: Data source: Birth control clinics statistics by Hajo (2010). The left panel shows the number of new birth control clinics by year. The right panel shows the cumulative number of new birth control clinics by year.

was no clinic in their county of residence. For example, Engelman (2011, p. 83) reports that women visiting the Brownsville clinic in Brooklyn came from all five boroughs of New York, Long Island, New Jersey and even as far as Philadelphia or New England. Therefore, in the empirical analysis, the variable measuring the presence or the exposure to a birth control clinic takes the value of one if a birth control clinic had opened in the county or any adjacent county before or in the respective year. While married women might have travelled across county borders to access a birth control clinic, historical literature does not provide any evidence of families migrating because of birth control clinics. Once women had learned how to correctly insert a fitted pessary, they could have used it for years, making selective migration highly unlikely.

We illustrate the geographic dimension of the roll-out in Figure 4. Counties that were exposed to a birth control clinic earlier are represented by darker shading. Initially, birth control clinics were established in larger cities such as New York, Chicago, and Los Angeles, before extending into smaller cities and rural areas. The stablishment of clinics was not based on a systematic roll-out plan; they were founded where Sanger could find volunteers to serve in these clinics and wealthy supporters to fund them. Especially in bigger cities, additional clinics often opened soon after the establishment of the first clinic (see Figure A.2 in the Appendix).

Figure 4 — Birth control clinics in U.S. counties 1916–1940



Notes: Data source: Birth control clinics statistics by Hajo (2010). The map shows U.S. counties. The earlier a county was exposed to a birth control clinic, the darker it is colored.

Table A.1 in the Appendix presents a set of regressions that explore the conuty level correlation between the establishment of a clinic and socio-economic characteristics drawn from the Census. In columns 1 and 2, we regress an indicator variable depicting whether a birth control clinic had been established in the county or an adjacent county by 1940 on characteristics observed in 1920 and 1940, respectively. In columns 3 and 4, we use the number of years a clinic had existed in the county or an adjacent county by 1940 as an alternative outcome; given that this variable takes the value of zero for many counties, we employ a tobit model for this analysis. The regressions reveal valuable insights. While characteristics related to urbanity are by far the most important correlates, a county's racial and religious composition as well as its female employment rate and literacy rate are not systematically correlated with the roll-out. In our empirical analysis, we account for the potential non-randomness of the roll-out in various ways. Specifically, we control for a county's observable socio-economic characteristics, including the baseline fertility rate, we check the robustness of the results in a county fixed effects model that accounts for time-constant unobserved heterogeneity and in a sample restricted to big city counties to increase comparability. Moreover, we compute Oster bounds to assess the importance of unobserved confounders. Most importantly, we thoroughly investigate pre-trends in an event study design to provide evidence for the validity of the common trends assumption. Finally, we use not-yet treated counties instead of never-treated counties as an alternative control group.

3.2. Census data

To assess the impact of the exposure to a birth control clinic on the individual level, we use information on women from the Population Censuses of 1920, 1930 and 1940. Harmonized full count data is available from the Integrated Public Use Microdata Series (IPUMS USA) (Ruggles *et al.*, 2020). We exclude the states of Alaska and Hawaii as well as the population living in military camps. Since we have information on the respondents' county of residence, we can merge the birth control clinic data to the Census data at the county level. We use a time-constant set of counties throughout our period of observation employing the county longitudinal template by Horan and Hargis (1995) with 1920 as the base year. Thereby, we avoid that changing county borders over time blur our analyses.[7] We limit the Census sample to White American and African American women as the remaining share of women is small (0.35%) and heterogeneous. Moreover, we focus on marital fertility since birth control clinics did not serve unmarried, childless women.[8]

We follow the historical literature and measure fertility using the child-woman ratio

[7] In particular, county borders might change if an existing county is divided into several new counties, if existing counties are merged to one new county, or if a county simply changes its geographical borders.

[8] Giving birth outside marriage was rare at that time; moreover, out-of-wedlock births were not well-accepted by society, which may result in severe reporting error.

(see, e.g., Beach and Hanlon, forthcoming; Bleakley and Lange, 2009; Hacker, 2003). More precisely, we use the number of children below the age of five living in the household of each 15 to 39-year-old woman (Grimm, 2021). We exclude older women as the likelihood that women above the age of 40 have children below the age of five at home is low in our period of observation. The number of children ever born is not available in the Censuses of the years 1920 and 1930; it is available but vastly incomplete in the 1940 Census. Relying on retrospective fertility in 1940 would come with two additional problems we would like to minimize, namely selective mortality and migration across counties. The latter would result in assigning women to counties they did not live in during their fertile period; as a result, assigned exposure to birth control clinics might suffer from measurement error. To analyze potential effects of birth control clinics on women's labor market outcomes, we use information on women's participation in the labor force (available in the 1920, 1930, and 1940 Census) and their employment status (available in the 1930 and 1940 Census).

The Census data further provide us with a set of observable individual characteristics that we use as control variables. We use information on a woman's age and race, her literacy status[9], an indicator for whether she was born in the U.S., whether she lives in a farm household, and whether the household is located in a rural or urban area, i.e., typically in a town of more than 2,500 inhabitants. Moreover, we use an indicator for whether the household is located in a big city of more than 100,000 inhabitants. At the county level, we use information on the population's religious composition in 1916, i.e., the share of Catholics, Protestants, and followers of another religion, using data from IPUMS NHGIS (Manson *et al.*, 2021). Moreover, we use voting outcomes of presidential elections at the county level taken from Robinson (1934) to study effect heterogeneity. Since birth control clinics started to emerge in bigger cities, we also conduct robustness checks using the sub-sample of 'big city counties' only. We define a county to be a 'big city county' if more than 50% of its female population resided in a city with over 100,000 inhabitants in 1940. By fixing this category in the year 1940, we prevent changes in the group composition over time.

Table 1 shows descriptive statistics for the total sample, and separately for the 1920, 1930, and 1940 Census samples. In total, we observe more than 45 million women. The average woman in our sample is exposed to a birth control clinic for 2.745 years; this number increases from 0.246 in 1920 to 5.773 in 1940. The average number of children below the age of five is 0.629; it is highest in 1920 (0.747) and decreases to 0.541 in 1940. Women are on average 29 years old, 11% are African-American, and also 11% are foreign-born. The literacy rate is as high as 96%. 18% of all women report being in the labor force; the share of employed women is equally high but is only measured in 1930 and 1940. The county-level Protestant share in the total sample is 52%, while the Catholic

[9] In the 1940 Census, literacy status is missing; therefore, we use information on education to proxy literacy status. Any woman who reports to have achieved at least grade 4 is considered to be literate.

Table 1 — Descriptive statistics: Census data

	1920 (I)	1930 (II)	1940 (III)	All years (IV)
Years of exposure to a BCC	0.246	1.550	5.773	2.745
	(0.959)	*(3.369)*	*(5.437)*	*(4.562)*
# childr. <5 in HH	0.747	0.626	0.541	0.629
	(0.876)	*(0.822)*	*(0.767)*	*(0.823)*
Age	29.333	29.518	29.544	29.474
	(5.910)	*(5.984)*	*(5.890)*	*(5.928)*
African American (=1)	0.101	0.122	0.113	0.113
Foreign born (=1)	0.172	0.121	0.064	0.114
Literate (=1)	0.935	0.966	0.969	0.958
Protestant†	0.522	0.517	0.524	0.521
Catholic†	0.307	0.311	0.306	0.308
Other religion†	0.172	0.173	0.171	0.172
In labor force (=1)	0.131	0.176	0.215	0.178
Employed (=1)‡		0.166	0.199	0.183
Farm hh (=1)	0.256	0.210	0.199	0.219
Urban residence (=1)	0.543	0.598	0.577	0.574
Big city residence (=1)	0.278	0.320	0.295	0.299
Obs. (women)	13,001,035	15,336,423	16,783,281	45,120,739

Notes: Data sources: IPUMS US Census, 1920, 1930 and 1940 and NHGIS. The table shows the means of the variables in the total sample, and separately for the Census years 1920, 1930, and 1940. Standard deviations of non-binary individual-level variables are provided in parentheses. † *shares on county level.* ‡ *only available in 1930 and 1940.*

share is 31%, and the share of followers of another religion is 17%. 22% of women live in farm households, 57% reside in urban areas, and 30% in big cities.

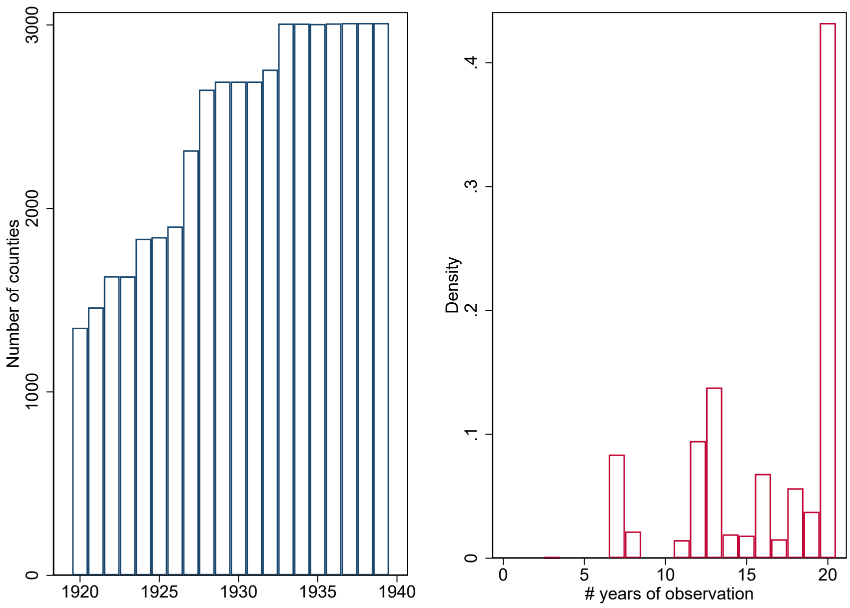
3.3. County level-vital statistics

As a complementary data set, we use administrative county-level natality and mortality data collected by Bailey *et al.* (2016). The team has digitized historical print sources from 1915 onward, double-checked the entries for accuracy, and processed the data to get consistent information at the county level. While the share of reporting counties was still low in 1915, it increased over the years; from 1933 on, the data set includes vital statistics from the universe of counties. We merge these data to the birth control clinic data at the county-year level; again, we employ the county longitudinal template by Horan and Hargis (1995) with 1920 as the base year to create a panel of consistently observed counties or county groups over time.

In our main analysis, we use annual information on the number of live births (exclusive of stillbirths) and infant deaths (i.e., deaths of children below the age of one exclusive of stillbirths). In a validity check, we use the total number of deaths by all ages and subtract infant deaths to obtain a mortality measure unaffected by infant deaths. Births and deaths are assigned to the county of occurrence in this period of observation. To account for changes in counties' population, we compute yearly birth and mortality rates by dividing the number of live births and the number of deaths in a year by the county's total population in the respective year.[10] For infant deaths, we use the number of births instead of the total population as the denominator.

Additionally, we use county level data from IPUMS NHGIS on the yearly number of stillbirths by place of occurrence in the period from 1922 to 1939 (Manson *et al.*, 2022). To obtain stillbirth rates, we divide the number of stillbirths by total population, and, alternatively, by the sum of stillbirths and live births. Thus, we account for changes in fertility and ensure that the stillbirth rate does not just decrease because the number of pregnancies decreases.

FIGURE 5 — Overview of administrative data



Notes: Data source: U.S. Vital Statistics. The left panel of the figure shows the number of counties available in the administrative data by year. The right panel shows the density of the number of observations by county in the period from 1920 to 1939.

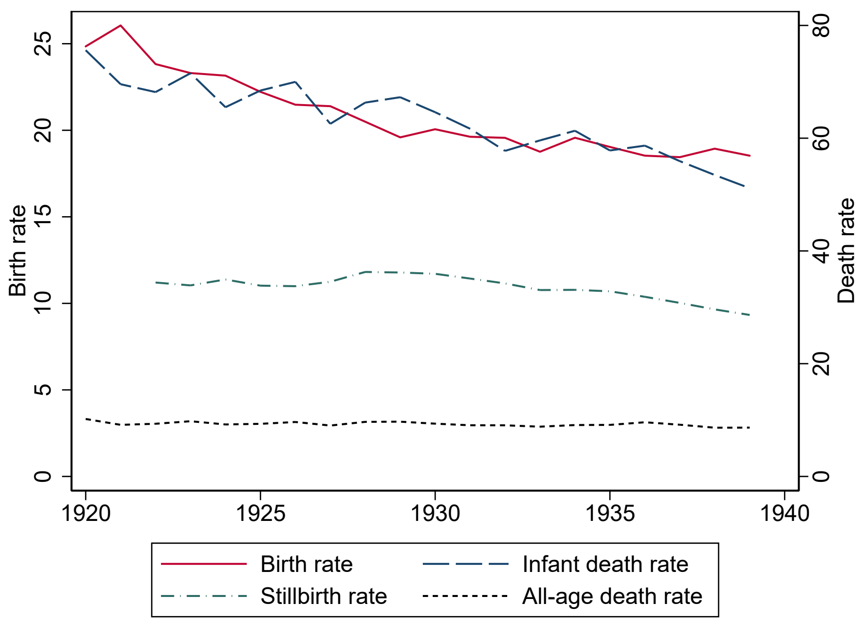
Figure 5 provides an overview of the number of observations available in the administrative data set. The left panel shows the number of counties in the data set by year. In 1920, we have information from 1,350 counties. This number steadily increases over the following years. From 1933 on, the data set includes observations from the universe of more than 3,000 counties. The right panel shows the distribution over the number of observations per county. We observe 43% of all counties every single year in the period

[10]Data on population by gender is not available before 1930. Therefore, we cannot compute fertility rates as the number of births by the number of women living in a county.

from 1920 to 1939. For 63% of all counties, we have observations from 15 years at least.

The development of fertility and mortality rates in our period of observation is depicted in Figure 6. The number of births decreases from 25 per 1,000 population in 1920 to 19 per 1,000 population in 1939. In the same period, the number of infant deaths decreases from 76 per 1,000 births in 1920 to 51 per 1,000 births in 1939. Thus, we observe a 24% decrease in the birth rate and a 33% decrease in the infant death rate within twenty years. The number of stillbirths per 1,000 births decreases from 34 in 1922 to 29 in 1939, which is a decline of 15%. The all-age mortality rate (without infant deaths) only slightly decreases from around 10 to 9 per 1,000 population within the period of observation.

FIGURE 6 — Birth and death rates in the administrative data



Notes: Data source: U.S. Vital Statistics. The figure shows birth rates, stillbirth rates, infant death rates, and all-age death rates (without infant deaths) over the period from 1920 to 1939.

3.4. City-level causes of deaths data

To investigate the impact of birth control clinics on maternal health, we additionally use administrative city-level data on causes of deaths. The U.S. Census Bureau has collected annual city-level mortality data and published them in the Vital Statistics of the United States annual volumes since 1900. The data reports not only the total number of deaths but also deaths by specific causes. This allows us to focus on puerperal deaths, which are most closely associated with potential maternal health effects from birth control clinics. To account for the size of the underlying population, we divide the number of puerperal deaths by the female population aged 15 to 49. The latter data comes from the full-count Population Censuses (Ruggles *et al.*, 2022). Ager *et al.* (forthcoming) have digitized the causes of death data, collapsed the full-count Population Censuses at the city level and provided access to both data sets. We follow Ager *et al.* (forthcoming) and use a log-linear interpolation to estimate the female population in each city in intercensal years.[11]

We restrict the sample to the period of 1920 to 1937. Following Ager *et al.* (forthcoming), we stop in 1937 because the coding of causes of death significantly changes after 1937 (Feigenbaum *et al.*, 2019). In 1931 and 1932, we observe significantly less cities than in the other years. We keep cities in our sample if they consistently report causes of deaths from 1920 to 1930 and 1933 to 1937 even if they are not observed in 1931 and 1932. Proceeding like this, we can use data from 579 cities. If we dropped cities without observations in 1931 and 1932, the sample would shrink to 316 cities.[12] We merge the birth control clinic data to the causes of death data at the county level.

4. Evidence from Census data

4.1. Identification strategy

To identify the impact of a woman's exposure to a birth control clinic on fertility in the Census data, we estimate the following regression equation:

$$y_{ibcst} = \beta yexp^{BCC}_{ibcst} + x'_{ibcst}\gamma + \lambda_b \times \eta_s + u_{ibcst}, \tag{1}$$

where y_{ibcst} is the number of children below the age of 5 in the household of woman i of birth cohort b, in county c, in state s, observed in Census year t. $yexp^{BCC}_{ibcst}$ is the number of years a woman has been exposed to a birth control clinic in her own or an adjacent county since her 15th birthday. Note that this measure does vary by county and year but even within a county-year cell across different ages of women. x'_{ibcst} is a matrix of control variables including age, age squared, literacy status, race, an indicator for being foreign born, an indicator for living in an urban area, an indicator for living in a big city, an indicator for living in a farm household, and the religious composition of the county where a woman resides. We include birth cohort fixed effects λ_b to capture general changes in fertility across birth cohorts. Each birth cohort groups women born within the same five-year period, starting with the period 1880–1885. By interacting the birth cohort fixed effects λ_b with state fixed effects η_s, we allow these cohort-specific changes in fertility to be heterogenous across states. This captures for example differences in the implementation of anti-abortion and Comstock laws across states. In an extended specification, we additionally control for the county-level fertility rate in 1920 interacted with Census year indicators. This allows us to capture county-level heterogeneity in

[11]Our findings are virtually identical if we use a linear interpolation instead.

[12]Our findings do not change if we use a balanced panel of 316 cities that report causes of death in every single year from 1920 to 1937.

fertility prior to the expansion of birth control clinics, and account for differential county-level trends depending on the baseline fertility level. Standard errors u_{ibcst} are clustered at the birth cohort-county level because this is where the variation in the treatment comes from.

The coefficient of interest β measures the impact of one year of exposure to a birth control clinic under the assumption that, conditional on birth cohort by state fixed effects and the set of controls, the years of exposure to a birth control clinic are orthogonal to unobserved determinants of fertility. Holding fixed a birth cohort in a state, the variation we exploit comes from differences in the exposure to a birth control clinic of a fixed birth cohort across counties of the same state.[13] Thus, the assumption can be reformulated in a way that resembles the standard parallel trends assumption for difference-in-differences designs: we assume that conditional on our controls women of the same birth cohort living in the same state follow the same fertility trend across counties in absence of the establishment of a birth control clinic.

To assess the validity of this key identifying assumption, we augment our analysis with an event study approach as laid out in the following equation:

$$y_{ibcst} = \sum_{\tau=-20}^{20} \beta_\tau exp^{BCC}_{ibcst,T+\tau} + x'_{ibcst}\gamma + \lambda_b \times \eta_s + u_{ibcst}. \tag{2}$$

$exp^{BCC}_{ibcst,T+\tau}$ takes the value one if a woman i of birth cohort b observed in county c, state s and year t is τ years prior to or after the first-time exposure. All other variables are defined as in Equation 1. Thus, we compare women of the same birth cohort in the same state that are at different time distances from the event. In our preferred specification, we use intervals of three years instead of single years to increase statistical power. Thus, the first post-treatment interval runs from $\tau = 1$ to $\tau = 3$. Thereby, we also take into account that a prevented pregnancy needs at least nine months to be reflected in the data. In the regressions, we omit the category that captures years 16 or more prior to the treatment. Women from never-treated counties are subsumed under this category as well.[14]

If $\beta_{T+\tau}$ is zero for all $\tau < 0$, this indicates conditional parallel fertility trends across counties in the pre-event period. Thus, this result would corroborate the validity of the key assumption of the empirical approach. Apart from allowing us to investigate pre-treatment trends, the event-study approach also allows us to trace treatment dynamics over the years after the start of the exposure. For example, if the negative impact on fertility of a birth control clinic just slowly unfolds over time, we should observe that

[13] To provide a better sense of the variation in exposure we exploit, Figure A.3 in the Appendix shows for each census year and for each cohort the mean exposure (a) measured by the share of women exposed and (b) measured by the mean years of exposure. As can be seen, exposure increases over time and is higher for younger than for older cohorts.

[14] This is a natural choice since at some point in time after our period of observation, all women got access to birth control. We test the robustness of our results with respect to this choice below.

(negative) $\beta_{T+\tau}$ increases as $\tau \geq 0$ increases.

We check that our results are robust to changes in the empirical specification. In particular, in an alternative model, we use county and time fixed effects instead of birth cohort by state fixed effects. Note that in this specification, we exploit variation within counties over time and across birth cohorts for identification. In additional robustness tests, we restrict the sample to big city counties and to the set of ever-treated counties to further enhance the comparability of treatment and control group counties.

4.2. Main results

We start our empirical analysis with a stripped down version of Equation 1, where we control for state-specific cohort fixed effects as well as age (and its square) and an urban area indicator. Controlling for women's age is indispensable despite the birth cohort fixed effects since the number of children naturally depends on a woman's age and, at the same time, older women are more likely to have lived more years exposed to a birth control clinic. Similarly, we consider the urban area indicator to be essential in our analysis since it accounts for the fact that birth control clinics were initially established in urban areas before spreading to more rural areas, while living conditions in urban and rural areas do differ.

Column 1 of Table 2 shows a highly significant negative effect of exposure to a birth control clinic on the number of children below the age of five in a household. Adding the full set of individual and county level socio-economic controls in column 2 leaves the estimate unaffected. The effect is economically meaningful: being exposed to a birth control clinic throughout the entire fertile period from age 15 to 39 reduces fertility by about 15 percent ($\frac{25\times0.004}{0.629} \approx 15$). In column 3, we additionally control for the county's fertility rate measured in 1920, i.e, prior to the large expansion of birth control clinics. In column 4, we interact the county's fertility rate in 1920 with Census year fixed effects to allow for differential fertility trends depending on the baseline fertility level. While the point coefficient slightly decreases, we still find sizeable and highly significant negative effects of exposure to a birth control clinic. Exposure from age 15 to age 40 translates into a 12 percent ($\frac{25\times0.003}{0.629} \approx 12$) reduction of fertility. Overall, exposure to birth control clinics can explain 5 percent of the total fertility decline in this period of observation.[15]

To rule out the possibility that our findings imply reflect an increase in inter-birth intervals without any effects on completed fertility, we re-estimate the model for the subsamples of women below and above the age of 30. If exposure to a birth control clinic would just postpone births, we would expect to observe negative effects on the fertility of young women and positive effects on the fertility of older women. As Table A.3 in the

[15]Using the point estimate in column 2 of Table 2 (-0.0037) multiplied by the average exposure time in our sample of 2.745 years yields a change in our fertility measure of -0.0102. The total change over the period 1920 to 1940 is -0.2063; hence, birth control clinics explain 4.95 percent of the total change.

Table 2 — The impact of exposure to a birth control clinic on fertility

	(I)	(II)	(III)	(IV)
Years of exposure to BCC	-0.004***	-0.004***	-0.003***	-0.003***
	(0.001)	(0.000)	(0.000)	(0.000)
State-specific cohort FE	yes	yes	yes	yes
Age and urban area controls	yes	yes	yes	yes
Socio-economic controls	no	yes	yes	yes
County level fertility 1920	no	no	yes	no
County level fertility 1920 × year FE	no	no	no	yes
R^2	0.072	0.084	0.088	0.088
Observations	45,104,489	45,104,489	45,104,489	45,104,489

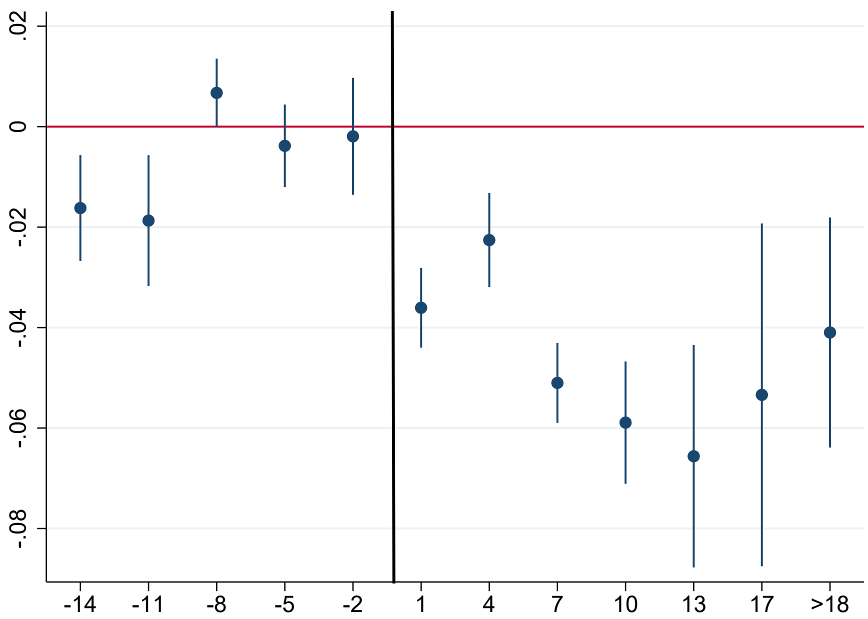
Notes: Data sources: IPUMS US Census, 1920, 1930 and 1940. The table shows OLS regressions. The dependent variable is the woman's number of children below the age of five living in the household. All regressions control for age and age squared as well as a variable indicating whether a houshold is in an urban area. Socio-economic controls are the literacy status, race, an indicator for being foreign born, an indicator for living in a big city, an indicator for living in a farm household, and the share of Catholics in a county. Standard errors are clustered at the birth cohort by county level. The detailed regression results are shown in the Appendix (Table A.2). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix shows, we find significant negative effects for both age groups. The abolute negative effect does decline with age since fertility does decline with age. Yet, the effect stays indeed negative throughout the entire fertile period. Thus, this exercise suggests that exposure to a birth control clinic does not just postpone births but indeed reduces completed fertility.

In a next step, we move to the event-study specification of Equation 2 to inspect pre-treatment trends and post-treatment dynamics. Figure 7 shows the results of this analysis, from which we obtain three insights. First, the analysis provides evidence for the validity of the common trends assumption. The coefficients for the time periods spanning years one to nine prior to the exposure to a birth control clinic are insignificant and close to zero. Although the two pre-treatment coefficients to the very left are significantly different from zero, the overall pattern does not suggest any conspicuous systematic fertility trajectory. In particular, we do not find any evidence for fertility starting to decline already prior to the exposure to a birth control clinic. Secondly, we observe an immediate significant reduction in fertility once women are exposed to a birth control clinic. Thirdly, this negative effect is persistent and even tends to grow larger over the post-treatment years. Because only few women are exposed to a birth control clinic for sixteen years or more, the confidence intervals for these long-term coefficients are rather large. The increasing effect over time may partly be driven by further clinics emerging in the region after the establishment of the first clinic (see Figure A.2 in the Appendix).

As fertility declines with exposure to birth control clinics, we might expect positive knock-on effects on female labor supply. If birth control clinics increase the spacing of births and reduce the total number of births, women might be more likely to re-enter the labor market between births and become more attached to the labor market. Yet, also note that the beginning of the Great Depression in 1929 had a substantial impact on labor demand and might have reduced women's opportunities to enter the labor market. We empirically test the impact of birth control clinics on female labor supply by using an indicator for a woman's labor force participation (available in the Census years 1920, 1930, and 1940) and a woman's employment status (available in the Census years 1930 and 1940) as the dependent variables of Equation 2. Table A.4 and Figure A.4 in the Appendix provide suggestive evidence for a positive effect of exposure to birth control clinics on female labor force participation and employment. Yet, since ambiguous pre-trends partly blur the overall picture, we are cautious and do not over-interpret these effects.

FIGURE 7 — Event-study plot of the impact of exposure to a birth control clinic on fertility



Notes: Data source: IPUMS US Census, 1920, 1930 and 1940. The figure shows event study estimates of the exposure to a birth control clinic on the number of children below the age of five in a household. Treatment effects are estimated along the lines of equation 2. The whiskers mark the 95 percent confidence band.

4.3. Robustness tests

To test the robustness of our findings, we now include county and year fixed effects instead of birth cohort-specific state effects in the model. The county fixed effects capture time-invariant regional heterogeneity below the state level, while the year fixed effects take account of general time trends in fertility. Column 1 of Table 3 shows that our results remain virtually unchanged in this alternative specification.

To further account for county level heterogeneity, we re-run the analysis on the subsample of counties with big cities. Since the roll-out of birth control clinics started in big cities and only later spread to rural areas, this procedure should further enhance the comparability of treatment and control group. Column 2 of Table 3 demonstrates that the findings are confirmed in this subsample. In big city counties, the effect is even larger, which might be due to a higher demand for birth control clinics in big cities than in smaller cities and rural regions. In a similar check, we restrict the sample to women from ever-treated counties and obtain virtually identical results (see Figure A.5 in the Appendix).

Further subsample estimates provide evidence that the effects are not confounded by a black market for contraceptives. Despite the strict implementation of the Comstock Act, a black market for contraceptives seemed to have emerged in the 1930s, especially in some of the bigger cities such as Chicago (Holz, 2012, chapter 2). This market was fueled to a large extent by "charlatans, quacks" (p.46) and entrepreneurs without any medical training. They were primarily motivated by profit rather than a desire to promote charitable causes. The entrepreneurs often closely collaborated with the illegal manufacturers of contraceptives. Their product offerings did typically not include effective pessaries but rather condoms, spermicides and contraceptive gels, most of it not very effective. Still, to rule out that black markets confound the estimates, we drop the 1940 Census and re-estimate the model only using the 1920 and 1930 Censuses, i.e., data from a period where black markets were still absent. Column 3 of Table 3 demonstrates that the estimated effect remains virtually unaffected. Hence, we conclude that the birth control clinic effects are not confounded by a black market for contraceptives.

Moreover, we check that the results are robust to using Pseudo Poisson Maximum Likelihood instead of OLS regressions. Thereby, we take account of the fact that the main outcome variable, i.e., the number of children below the age of five in a household, is a count variable. Column 4 of Table 3 confirms that exposure to a birth control clinic significantly reduced fertility. Indeed, the effects turn out to be even larger in the Pseudo Poisson Maximum Likelihood model than in the OLS model. Every year of exposure reduces fertility by 1.3 percent, as compared to 0.6 percent in the OLS model.

Table 3 — The impact of exposure to a birth control clinic on fertility - robustness tests

	County FE (I)	Big cities (II)	w/o 1940 (III)	Poisson (IV)	Oster bounds (V)
Years of exposure to BCC	-0.003***	-0.005***	-0.004***	-0.013***	-0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
bias-adjusted coeff.					*-0.003*
δ eliminating effect					*4.090*
State-specific cohort FE	no	yes	yes	yes	yes
Age and urban area controls	yes	yes	yes	yes	yes
Socio-economic controls	yes	yes	yes	yes	yes
County FE	yes	no	no	no	no
Year FE	yes	no	no	no	yes
R^2	0.087	0.066	0.085	0.045	0.084
Observations	45,104,489	15,156,837	28,337,458	45,104,489	45,104,489

Notes: Data sources: IPUMS US Census, 1920, 1930 and 1940 and NHGIS. The table shows OLS regressions. The dependent variable is the woman's number of children below the age of five living in the household. All regressions control for age and age squared as well as a variable indicating whether a houshold is in an urban area. Socio-economic controls are the literacy status, race, an indicator for being foreign born, an indicator for living in a big city, an indicator for living in a farm household, and the share of Catholics in a county. Standard errors are clustered at the birth cohort by county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Following the methodology developed by Altonji *et al.* (2005) and elaborated by Oster (2019), we test how robust the coefficient of interest is to selection on unobservables. The underlying idea is that the shift in the R^2 in relation to the shift of the coefficient of interest resulting from the inclusion of observed controls carries information on the shift of the coefficient of interest in case relevant unobservables could be included. This test requires an assumption by how much the R^2 could increase in this latter case. We follow Oster (2019) who suggests, based on illustrative examples, a value of 1.3. We compare the stripped down model (column 1 of Table 3) to the model including the full set of observable socio-economic characteristics (column 2 of Table 3). Column 5 of Table 3 shows that the bias-adjusted coefficient of interest is still negative and comparable in size to coefficients from our main specifications. The effect would be eliminated if the ratio of selection on unobservables to selection on observed socio-economic characteristics was 4.090.

To address the concern that the effects depend on the particular definition of the treatment, we redefine the treatment variable in another robustness test. Instead of relying on the simple rule of thumb that a clinic is relevant if it is in the county of residence or in any adjacent county, we model exposure to a clinic according to the availability of clinics in buffer zones around the centroid of a woman's county of residence. We consider a buffer zone of 20km, a buffer zone of 50km and a ring of 50 to 100km. Table A.5 in the Appendix shows that the effects within the buffer zones of 20km and in particular of 50km are virtually identical to the previous estimates. Indeed, a 50km buffer zone largely corresponds to a county and its adjacent counties as the treatment unit.[16] Moreover, the effects are clearly larger within the buffer zones than in the ring around them. Note that counties are too small to allow for larger buffer zones and rings since the ring of a given county quickly intersects with buffer zones of many other neighboring counties, which blurs the analysis. Taken together, this exercise justifies the choice of restricting exposure to birth control clinics in the county of residence or adjacent counties. This choice is also preferred because the Census data do not provide exact geocodes of households, nor can we identify exact geocodes of the clincis in our inventory.

Finally, we conduct randomization inference tests (Rosenbaum, 2002; Cohen and Dupas, 2010) based on 100 permutations to derive statistical inference for the birth cohort by state as well as the county fixed effects model. Each permutation allocates the entire set of exposure start dates randomly to counties. The distribution of coefficients obtained on the permuted samples includes zero. The point estimate obtained from the actual distribution of the birth control clinics is clearly outside of that distribution (see Figure A.6 in the Appendix). Thus, we conclude that it is unlikely that the negative coefficients are

[16] The average radius of counties is between 21 and 24km in the Northeastern, Midwestern and Southern states, while it is 52km in the Western states. Hence, a county plus the adjacent counties fit well within a radius of 50km.

result of statistical uncertainty.

4.4. Heterogeneity

In additional analyses, we investigate whether the effect of exposure on fertility varies across socio-demographic groups, voting patterns and regions (see Table A.6 in the Appendix). We find that the impact for African Americans is slightly larger than for Whites, although the difference is not substantial. Women born in the U.S. appeared to benefit more from the clinics than first-generation migrants, i.e., women born outside the U.S. We observe somewhat smaller effects in counties with an above-median population share of Catholics (share > 32 percent). Moreover, we find that the fertility effect was greater in counties where the majority voted for the Democrats in the 1920 presidential elections. However, if we consider the election outcome of 1932, the effects in both subsamples are not significantly different from each other. In 1920, support for the Democrats was fully concentrated in the Southern states, whereas in 1932 the Democrats had the majority in all states. Finally, in the eleven Southern states[17], the effect is almost twice as large as in the Northern states. This is consistent with the effect heterogeneity across religious groups as the majority of the population in the Southern states is Protestant, and it is consistent with the heterogeneity in the 1920 voting pattern.

5. Evidence from administrative data

Although the Census data provide a good starting point, there are several limitations that must be taken into account. First, we are unable to directly observe yearly births and instead must rely on respondents' reports of the number of children below the age of five in a household. Secondly, the Census data are only collected every decade, leaving us with information from only three points in time: 1920, 1930, and 1940. Thirdly, we are unable to observe deaths although fetal deaths, infant deaths and maternal deaths are interesting outcome variables in our setting. To overcome these limitations, we complement the empirical analysis with a second approach in which we use yearly administrative data on the universe of live births, stillbirths and infant deaths at the county level as well as causes of death data at the city level.

5.1. Plotting raw data

We start the empirical analysis of the administrative data with simple descriptive plots using the full unbalanced county panel. For the group of *ever-treated* counties, we normalize the year of the opening of the first birth control clinic in a county or any adjacent

[17] These are South Carolina, Mississippi, Georgia, Louisiana, Florida, Alabama, Texas, Virginia, Arkansas, North Carolina and Tennessee.

county to zero. For the group of *never-treated* counties, we randomly assign placebo openings of birth control clinics. To this end, we use the sample of *ever-treated* counties and compute for every single year from 1916 to 1939, which share of counties was affected by the opening of a first birth control clinic. The resulting distribution of probabilities forms the basis for the random assignment of placebo openings to *never-treated* counties. Then, we normalize the year of the placebo opening for *never-treated* counties to zero.

Figure 8 plots the birth rate, the stillbirth rate, and the infant death rate for the treatment and the placebo treatment group against time from the (placebo) opening of a birth control clinic. For the birth rate (upper left graph), we observe a stronger decrease for the treatment group than for the placebo treatment group after the (placebo) opening of a birth control clinic. This result is in line with the results from the Census data and suggests a negative effect of birth control clinics on fertility. Similarly, we observe a stronger decrease in the number of stillbirths per 1,000 population (upper right panel), the number of stillbirths per 1,000 births (lower left figure), and the infant death rate (lower right panel). Thus, these descriptive plots suggest that birth control clinics also had a negative effect on the stillbirth rate and the infant death rate. However, the picture might amongst others be blurred by the fact that the composition of counties changes with event time in the unbalanced county panel.

For the remaining analysis, we create a balanced panel of counties over the period from 1925 to 1939. As explained in Section 3, the number of counties available in the administrative data increases over the years. Therefore, the later we start the balanced panel, the more counties we can draw on. The earlier we start the balanced panel, the more years we can draw on. The restriction to a 15 years balanced panel from 1925 to 1939 maximizes the total number of observations; it leaves us with 27,360 observations from 1,824 counties.[18] We then match untreated counties to treated counties using nearest neighbor matching techniques and drop unmatched counties from the data set.[19] Proceeding like this, we hold the composition of the sample constant over time and ensure that treatment and control counties are comparable in observed baseline characteristcs. Table A.8 in the Appendix shows that the means of the fertility and mortality variables in this sample hardly differ from the respective means in the unbalanced sample of all counties from 1920 to 1939.

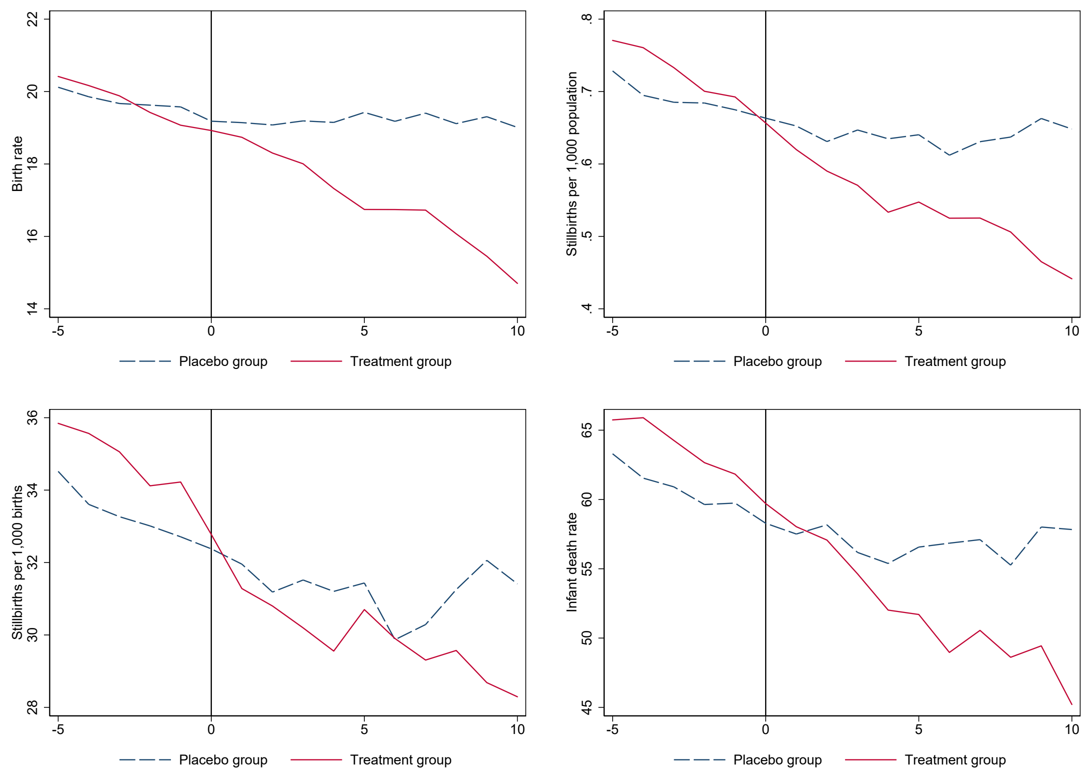
5.2. Identification strategy

With yearly county-level data, our setting boils down to a typical "staggered roll-out design", in which a binary absorbing treatment (i.e., a birth control clinic) is introduced in

[18]In line with Sun and Abraham (2021), we drop counties treated already prior to the first year of observation.

[19]Table A.7 in the Appendix presents the results of the probit model underlying the matching approach and compares treatment and control counties in the total sample as well as treatment and control counties in the matched sample.

Figure 8 — Raw difference-in-differences plots for fertility, stillbirths and infant mortality



Notes: Data source: U.S. Vital Statistics. The figure shows the evolution of fertility rates, stillbirth rates, and infant death rates for the group of ever-treated counties and the group of never-treated counties. For never-treated counties, we randomly assign placebo opening years of birth control clinics. The year of an actual or a placebo opening are normalized to $t = 0$.

different counties at different points in time. The standard two-way fixed effects model yields biased estimates in such a setting if treatment effects are heterogeneous across groups or over time (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020). In particular, in an event-study design, the estimated lead and lag coefficients are combinations of differences in trends from their own relative periods, from relative periods belonging to other periods included in the specification, and from other relative periods excluded from the specification (Sun and Abraham, 2021). Figure A.7 in the Appendix shows that this is a relevant concern in our setting. The relative period coefficients are weighted combinations of own period effects and other period effects, and the weights are sometimes negative. As a result, the event-study coefficients estimated in a dynamic two-way fixed effects model would be hard to interpret.

We avoid these pitfalls by estimating an event-study applying Sun and Abraham's (2021) interaction weighted estimator. The estimator is constructed in three steps. First, we run a linear two-way fixed effects specification, with county (α_i) and year fixed-effects (σ_t), and interact indicators D^l_{it} for relative event period l with cohort indicators e. A cohort is defined as the group of counties i whose treatment starts in the same year. The event, i.e., the start of the absorbing treatment, is determined by the year in which the first birth control clinic in a county or any adjacent county was established; this year constitutes the omitted category. Cohorts that are always treated are excluded from the estimation, while the *never-treated* cohort forms the control group C. The error term ζ_{it} is clustered at the county level.

$$y_{it} = \alpha_i + \sigma_t + \sum_{e \notin C} \sum_{l \neq 0} \phi_{e,l}(\mathbf{1}\{E_i = e\} \cdot D^l_{it}) + \zeta_{it} \tag{3}$$

Second, we estimate the cohort shares in each relative time period l as explained in the following expression, where $T+1$ is the total number of calendar period observations $t \in 0, ..., T$ per unit:

$$Pr\{E_i = e | E_i \in [-l, T-l]\} \tag{4}$$

Finally, we weigh the group-specific estimates $\hat{\phi}_{e,l}$ from Equation 3 with the estimated shares from Equation 4 to obtain the interaction weighted estimator $\hat{v}_g$:

$$\hat{v}_g = \frac{1}{|g|} \sum_{l \in g} \sum_{e} \hat{\phi}_{e,l} \hat{Pr}\{E_i = e | E_i \in [-l, T-l]\} \tag{5}$$

This estimator has a clear interpretation since the weights sum to one for each relative time and are non-negative. Figure A.8 in the Appendix provides an overview of the relative time specific weights. The interaction weighted estimator depicts the causal effect of birth control clinics under the assumptions of parallel trends in absence of the treatment and no anticipation of treatment.

We provide evidence for the validity of the parallel trends assumption by inspecting pre-trends. In particular, if the estimates of the leads in the event-study are zero, trends in the periods prior to the opening of a birth control clinic are parallel. This makes it plausible that trends would also have been parallel in the periods after the opening of a birth control clinic if the birth control clinic had never opened. To investigate the validity of the no anticipation of treatment assumption, we inspect whether there are any conspicuous changes shortly before the opening of the first birth control clinic.

5.3. Effects on fertility

Figure 9 shows the event study estimates for the impact of birth control clinics on fertility. All pre-treatment coefficients are close to zero and insignificant, which suggests that in the years before the opening of a birth control clinic, treatment group counties follow a similar birth rate trend as never-treated counties. This finding corroborates the validity of the key identifying assumptions. Only after the opening of a birth control clinic, we find a conspicuous and highly significant drop of the birth rate in treatment group counties as compared to control group counties. This negative effect is persistent and even tends to grow in the course of ten years after the opening of the birth control clinic.[20] Averaged over the period of ten years, birth control clinics reduce the local annual birth rate by 1.007 births, which amounts to 5.2 percent of the sample mean.

Comparing these estimates to the Census data estimates, we find that the effect sizes are indeed similar. While the Census data suggest that exposure to birth control clinics accounts for 5.0 percent of the total fertility decline, this number is 7.8 percent using the administrative data.[21] In an alternative comparison, we find that 25 years of cohort exposure to a birth control clinic reduces the number of children below five in a household by 11 percent of a standard deviation in the Census data. In comparison, exposure of a cross-section of women of fertile age (roughly 25 years) in a given year reduces the birth rate by 16 percent of a standard deviation in the administrative data. Although the effect sizes seem to be comparable, note that comparability suffers from the fact that both the time period and the set of counties we use differs between the Census data and the administrative data estimations. While we draw on data from all counties in the period from 1920 to 1940 in the Census data, the administrative data restrict us to a panel of counties that consistently report vital statistics from 1925 to 1939, from which we additionally drop unmatched counties. This reduces the number of distinct counties from 3,011 to 1,275.

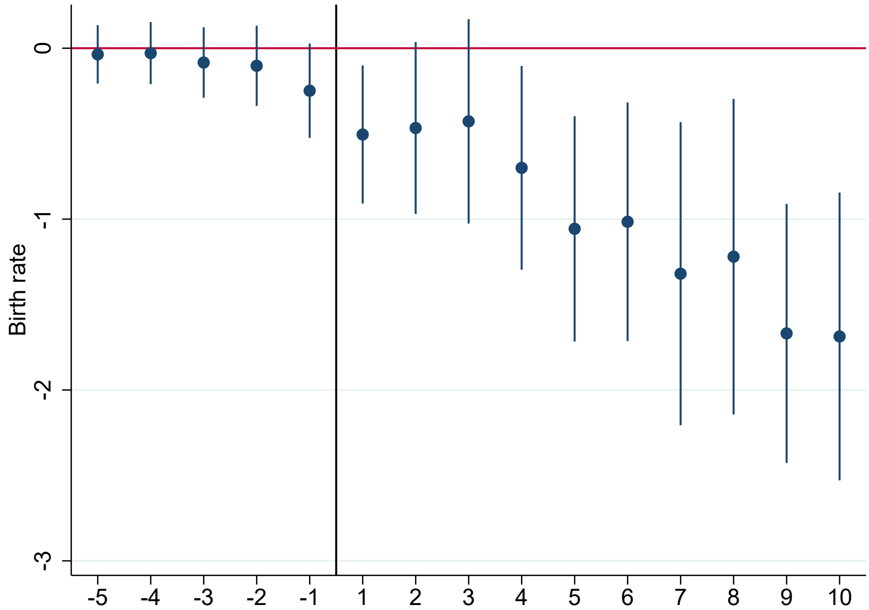
5.4. Effects on stillbirths and infant deaths

If birth control clinics reduce fertility by increasing birth spacing, this might result in a lower incidence of stillbirths and infant deaths. Conde-Agudelo *et al.* (2006) provide a systematic literature review of the role of birth spacing for perinatal health. This review finds that interpregnancy intervals shorter than 18 months or longer than 59 months are associated with a range of adverse perinatal outcomes. More recently, Gupta *et al.* (2019) show in a case control study that shorter intervals between pregnancies increase the risk of stillbirths. Molitoris (2017) exploit within-family variation in the association between preceding birth intervals and infant mortality risks using early 20th century register data from Sweden. He provides evidence for an adverse impact of short interpregnancy intervals on infant mortality. Conde-Agudelo *et al.* (2012) review causal mechanisms underlying these effects. They find that an inadequate time to recover from the insufficient repletion of maternal folate resources, vertical transmission of infections to the fetus, and transmission of infectious diseases among siblings seem to be the most important channels.

[20] This increase of treatment effects with event time can partly be explained by the opening of further clinics in the county (see Figure A.2 in the Appendix).

[21] In the 1925–1939 balanced panel, the total decline in the fertility rate amounts to 4.04 births per 1,000 population. For the 49.7 percent of the ever-treated counties, the average number of years since the establishment of the first birth control clinic is 5.7 years. Over the first five years after the establishment of a birth control clinic, the average fertility effect is -0.631. Thus, birth control clinics can explain (0.497*0.631)/4.04=7.8 percent of the total fertility decline.

FIGURE 9 — The impact of birth control clinics on fertility (Sun and Abraham, 2021)

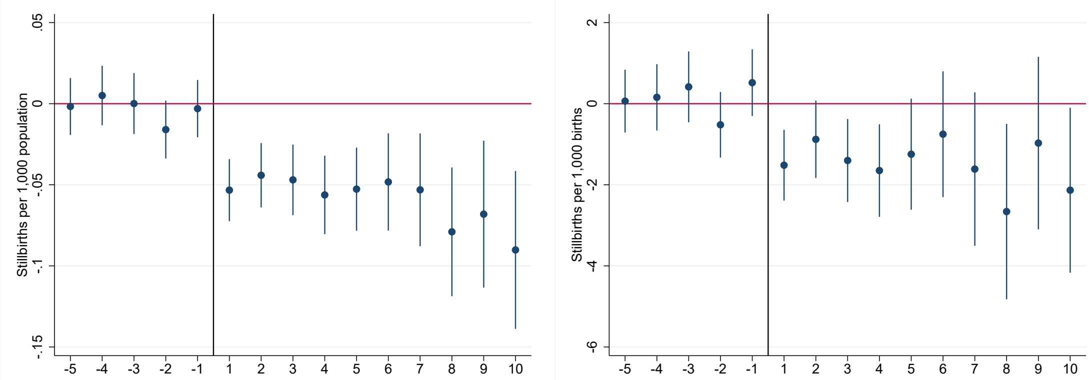


Notes: Data source: U.S. Vital Statistics. The figure shows the dynamic impact of the establishment of the first birth control clinic in a county (or an adjacent county) on the birth rate. Treatment effects are derived using the interaction weighted estimator by Sun and Abraham (2021). We bin ten or more years after the treatment into a single indicator. The control group consists of never-treated counties. Standard errors are clustered at the county level. The whiskers mark the 95 percent confidence band. The sample is restricted to a balanced panel of counties for the years 1925 to 1939.

Figure 10 depicts the impact of birth control clinics on stillbirths. In the left panel, we use the number of stillbirths per 1,000 population, while in the right panel, we use the number of stillbirths per 1,000 births (sum of stillbirths and live births) as the outcome variable of the event-study. For both outcome variables, all pre-treatment coefficients are close to zero and insignificant, which provides evidence for the validity of the key identifying assumptions. After the establishment of a birth control clinic, we observe a significant decrease of stillbirths. The fact that we do not only observe this decrease for the number of stillbirths per 1,000 population but also for the number of stillbirths per 1,000 births suggests that the effect is not just a mechanical effect arising from a decrease in pregnancies. Rather, the results suggest that birth control clinics avert particularly high risk pregnancies. Averaged over ten years, birth control clinics reduce the number of stillbirths per 1,000 births by 1.482, which amounts to 4.5 percent of the sample mean or 9.6 percent of a standard deviation.

In a next step, we investigate the impact of birth control clinics on infant mortality. As we can see in Figure 11, all five pre-treatment coefficients are insignificant and virtually zero, which again corroborates the validity of the common trends and the no-anticipation assumption. After the opening of a birth control clinic, the infant death rate significantly drops; in the following years, the negative effect of birth control clinics on infant mortality

Figure 10 — The impact of birth control clinics on stillbirths (Sun and Abraham, 2021)



Notes: Data source: U.S. Vital Statistics. The figure shows the dynamic impact of the establishment of the first birth control clinic in a county (or an adjacent county) on stillbirths per 1,000 population (upper panel) and stillbirths per 1,000 births (lower panel). Treatment effects are derived using the interaction weighted estimator by Sun and Abraham (2021). We bin ten or more years after the treatment into a single indicator. The control group consists of never-treated counties. Standard errors are clustered at the county level. The whiskers mark the 95 percent confidence band. The sample is restricted to a balanced panel of counties for the years 1925 to 1939.

steadily increases. Since we define the infant death rates over the number of births, these estimates suggest that birth control clinics indeed improve the average health of the babies born. Again, this means that birth control clinics particularly avert the type of births that are characterized by increased health risks. Averaged over the period of ten years, birth control clinics reduce the local infant death rate by 4.206 deaths, which amounts to 7.0 percent of the sample mean or 19.6 percent of a standard deviation.

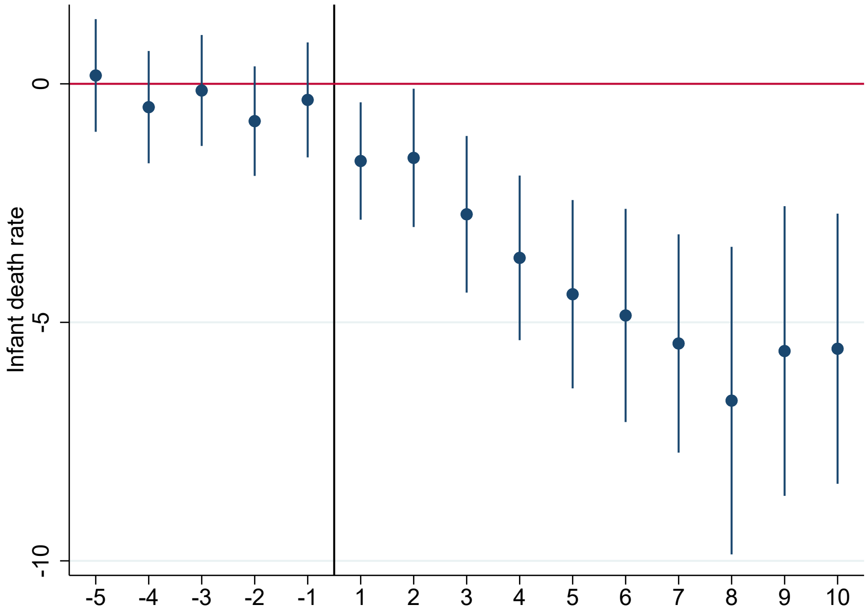
Since birth control clinics reduce infant mortality, the negative effect on fertility identified in the Census data might be a lower bound estimate. This is because the Census fertility measure, i.e., the number of children below five in the household, is driven up in treatment as compared to control households due to reduced infant mortality. Thus, the isolated negative fertility effect in the administrative data should be larger than the one estimated in the Census data, which is in line with our findings.

5.5. Effects on maternal mortality

Since birth control clinics increase birth spacing and reduce the number of births, they might reduce maternal deaths. In their review article, Conde-Agudelo *et al.* (2012) report increasing evidence that an incomplete healing of the uterine scar from a previous cesarean delivery is a major risk factor for maternal health in case of short interpregnancy intervals. Short birth spacing might also result in maternal nutritional depletion, which constitutes another risk factor, although the empirical evidence for this channel is still inconclusive. Furthermore, access to contraceptives via birth control clinics might have reduced unprofessional abortions and thereby reduced health risks.

The left panel of Figure 12 depicts the effect of birth control clinics on the puerperal

Figure 11 — The impact of birth control clinics on infant mortality (Sun and Abraham, 2021)

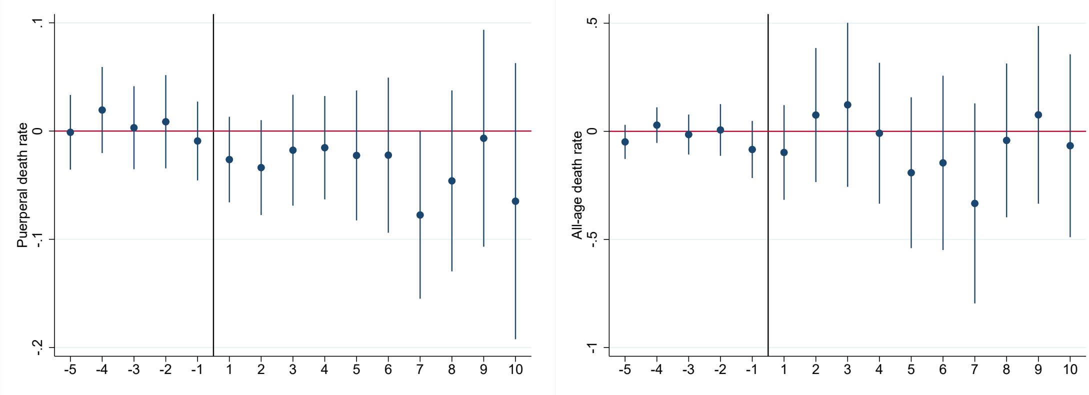


Notes: Data source: U.S. Vital Statistics. The figure shows the dynamic impact of the establishment of the first birth control clinic in a county (or an adjacent county) on the infant death rate. Treatment effects are derived using the interaction weighted estimator by Sun and Abraham (2021). We bin ten or more years after the treatment into a single indicator. The control group consists of never-treated counties. Standard errors are clustered at the county level. The whiskers mark the 95 percent confidence band. The sample is restricted to a balanced panel of counties for the years 1925 to 1939.

death rate at the level of 579 cities using the interaction weighted estimator by Sun and Abraham (2021). The small and insignificant pre-treatment coefficients provide evidence for the validity of the common trends assumption. All ten post-treatment coefficients are consistently negative; yet, they fail to reach conventional significance levels. Running an alternative model, in which we use a single post-treatment indicator instead of ten separate lags, we find that birth control clinics reduce the puerperal death rate by 0.029, or 4.5 percent of the sample mean (0.649). But again, the effect is not significant with a p-value of 0.189. Thus, although the overall pattern is suggestive of a negative effect of birth control clinics on puerperal deaths, we cannot rule out that the effect is zero.

As a validity check, we use the county level all-age mortality statistics provided by Bailey *et al.* (2016) to rule out that any negative mortality effects on infants (and suggestively mothers) are confounded by general improvements in the health infrastructure. These data report the total number of deaths but do not distinguish by cause of death, by sex or by age. Therefore, they are not suitable to identify the deaths likely affected by birth control clinics. However, changes in the all-age mortality rate (without infant deaths) constitute a useful proxy for changes in a county's general health situation. The right panel of Figure 12 shows that all pre-treatment coefficients are insignificant and close to zero. After the opening of the birth control clinic, we do not find any evidence

FIGURE 12 — The impact of birth control clinics on puerperal deaths and all-age total mortality (Sun and Abraham, 2021)



Notes: Data source: City-level causes of death data and U.S. Vital Statistics. The figure shows the dynamic impact of the establishment of the first birth control clinic in a county (or an adjacent county) on the puerperal death rate (left panel) and the all-age death rate (right panel). Treatment effects are derived using the interaction weighted estimator by Sun and Abraham (2021). We bin ten or more years after the treatment into a single indicator. The control group consists of never-treated cities (left panel) or never-treated counties (right panel). Standard errors are clustered at the county level. The whiskers mark the 95 percent confidence band. The sample in the left panel is restricted to cities that consistently report causes of deaths from 1920 to 1930 and 1933 to 1937 no matter whether they report in 1931 and 1932.

for significant effects on all-age mortality (without infant deaths). Indeed, the coefficients hover around zero and are far from statistical significance. Thus, these findings provide evidence that the roll-out of birth control clinics is not confounded by general improvements in counties' health infrastructure.

5.6. Robustness tests

As a first robustness test, we check whether the results are confirmed if we use a 20 years balanced panel instead of the 15 years balanced panel. Overall, the 20 years balanced panel leaves us with 25,900 observations from 1,295 counties. Again, we apply nearest neighbor matching and drop counties that are unmatched, which leaves us with 18,300 observations from 915 counties.[22] Figure A.9 in the Appendix reports the event study estimates on this alternative sample and qualitatively confirms all previous findings. While the effects of birth control clinics on stillbirths and infant mortality are even more pronounced than in the 15 years balanced panel, the effects on fertility are less precisely estimated.

In an alternative specification, we use the last treated cohort instead of matched never-treated counties as the control group. Since the number of counties with a birth control clinic opening in 1939 is small, the resulting estimates might be affected by outliers. Therefore, we extend the last treated cohort to cohorts that opened a birth control clinic either in 1938 or in 1939 and drop these two years from the sample in line with Sun and

[22]Table A.8 in the Appendix shows that this sample hardly differs from the 15 years balanced panel of the main specification.

Abraham (2021).[23] Figure A.10 in the Appendix presents the results of this alternative specification. The effect of birth control clinics on the birth rate is somewhat attenuated; it becomes significant only seven years after the opening of the birth control clinic. In contrast, the negative effect of birth control clinics on stillbirths and the infant mortality rate is clearly confirmed.

Finally, we check whether the results are confirmed using the estimator proposed by Callaway and Sant'Anna (2021) instead of Sun and Abraham's (2021) interaction weighted estimator. As in the main specification, we use a 15 years balanced panel from 1925 to 1939 and restrict the control group to never-treated counties. Moreover, we again use nearest neighbor matching techniques and drop unmatched counties from the data set. Figure A.11 in the Appendix shows that all findings are confirmed using this alternative estimator.

6. Conclusions

This study contributes new insights into the factors that drove the U.S. demographic transition at the beginning of the 20th century and sheds light on the impact of modern contraception, especially pessaries, before the introduction of the birth control pill in 1960. While there is a rich historical and sociological literature on Margaret Sanger's life and her movement, this work is the first quantitative assessment of the first family planning initiative in the U.S. Our research draws on a unique combination of newly digitized data on the roll-out of Sanger's birth control clinics, full-count Census data, as well as county and city-level registry data. To assess the causal impact of birth control clinics on fertility, stillbirths, infant mortality, and maternal deaths, we employ event-study methods that leverage the staggered roll-out of clinics across U.S. counties.

Historical Census data estimates indicate that exposure to a birth control clinic during the entire fertile period from age 15 to 39 reduces fertility by 12 to 15%. Event-study estimates based on administrative data confirm the negative impact of birth control clinics on fertility. Birth control clinics account for 5.0–7.8% of the overall decline in fertility between 1920 and 1940. In addition, we find evidence that birth control clinics had a significant and meaningful negative effect on the incidence of stillbirths and infant mortality. These findings are in line with the notion that birth control clinics increase birth spacing and thus particularly avert births that pose high health risks. The effects of birth control clinics on maternal deaths, particularly puerperal deaths, are consistently negative yet insignificant. We also find some suggestive evidence that birth control clinics have positive effects on female labor supply. Parallel pre-treatment trends provide evidence for the validity of the empirical approach. Various specification checks including placebo

[23] Table A.8 in the Appendix shows that this sample hardly differs from the 15 years balanced panel of the main specification.

outcomes, alternative control groups, and subsample analyses confirm the findings.

Overall, the findings suggest that the relaxation of supply side policies by birth control clinics reduced constraints on the demand side, i.e., in a Beckerian framework (Becker, 1981) birth control clinics reduced the cost of having fewer children. Therefore, these results demonstrate the power of family planning in a context where parents, and especially mothers desire smaller family size and longer birth intervals but are constrained to implement these preferences. The study also indicates that family planning can have substantial indirect effects on health. These findings are likely applicable to much of the developing world, in contexts where women desire smaller families but give birth to many children at a young age in a relatively short period.

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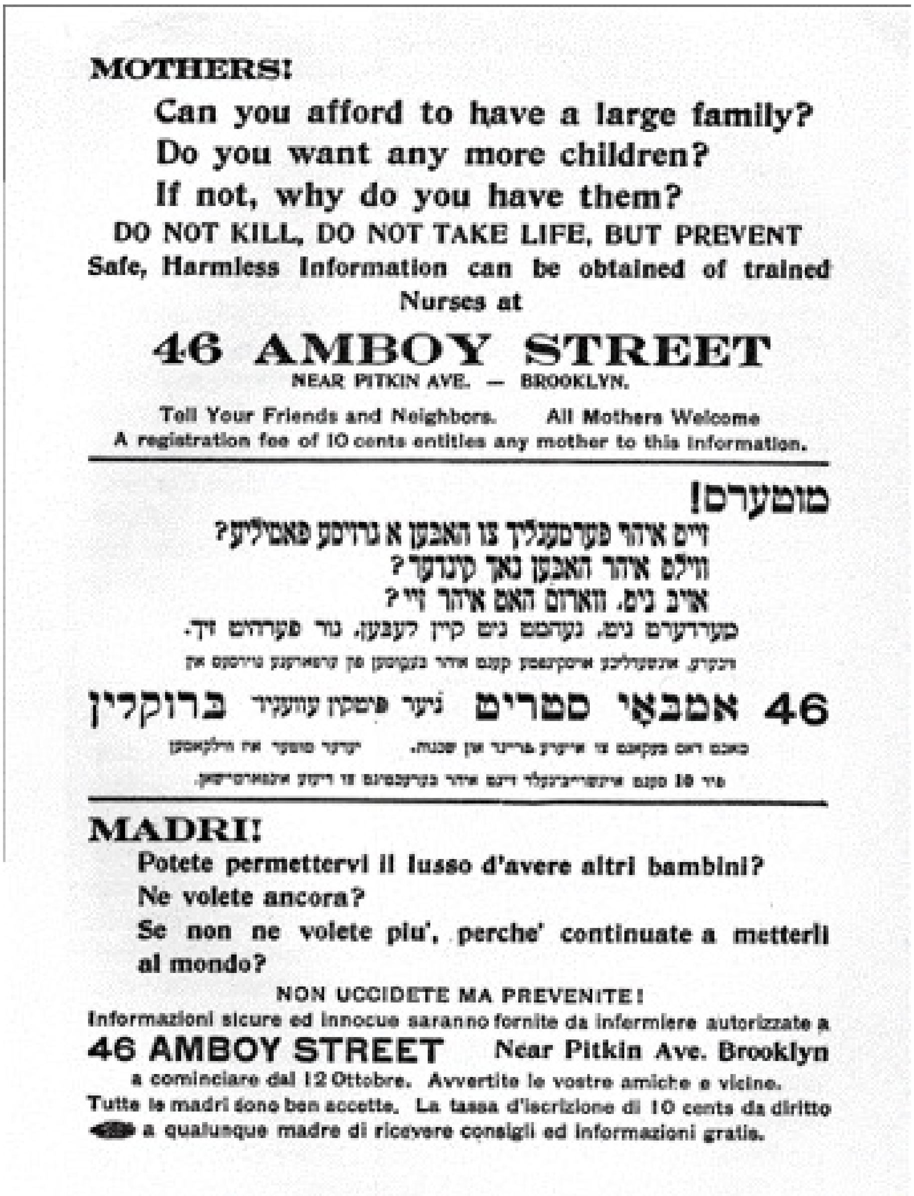
A. Appendix

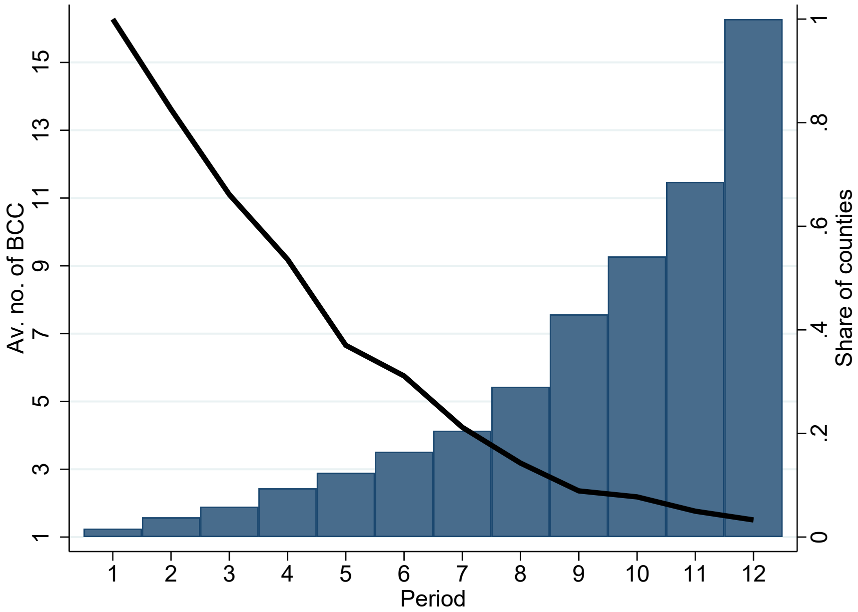
This Appendix (not for publication) provides additional material discussed in the unpublished manuscript "The Impact of Margaret Sanger's Birth Control Clinics on Early 20th Century U.S. Fertility and Mortality" by Stefan Bauernschuster, Michael Grimm, and Cathy M. Hajo.

Figure A.1 — Leaflet advertising the Sanger Clinic in 46 Amboy Street, Brooklyn



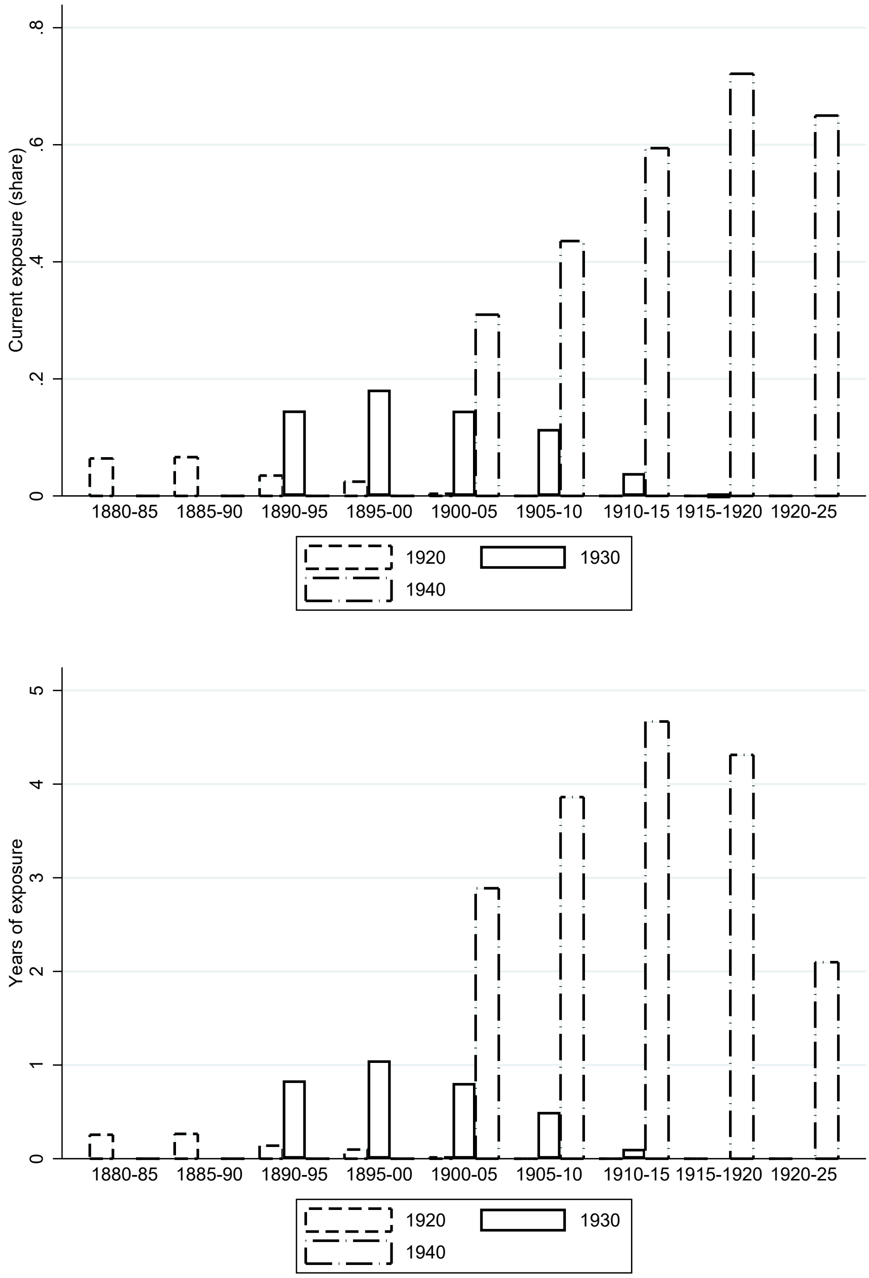
Notes: The photo shows a leaflet advertising the United States' first birth control clinic in English, Yiddish, and Italian.

Figure A.2 — Birth control clinics: dynamics of the roll-out



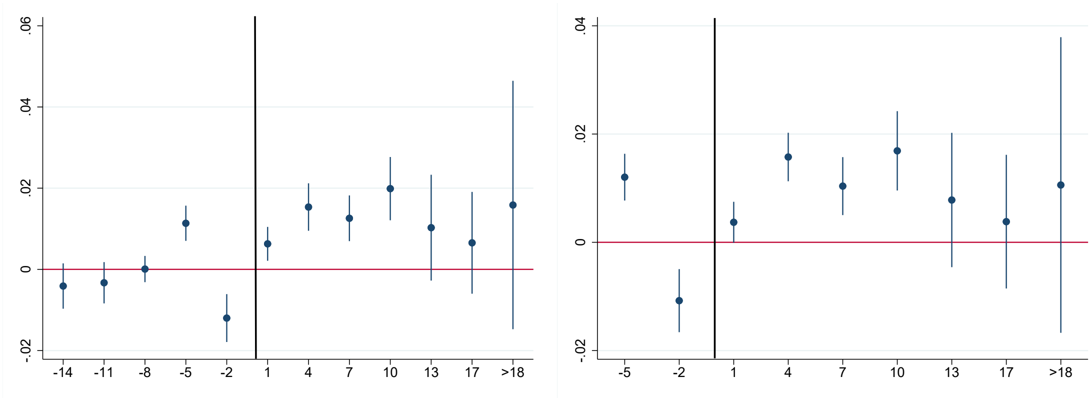
Notes: Data source: Birth control clinics statistics by Hajo (2010). The bars show the average number of birth control clinics in the own or adjacent county over the period since the first clinic was established. The black line shows the share of counties that experience the period depicted on the horizontal axis with at least one birth control clinic in the own or adjacent county. For example, in the fifth period after the establishment of the first birth control clinic, counties have on average nearly three clinics, but less than 30% of the counties experience five periods and more with a birth control clinic in our period of observation until 1940.

Figure A.3 — Exposure to a birth control clinic by cohort and year



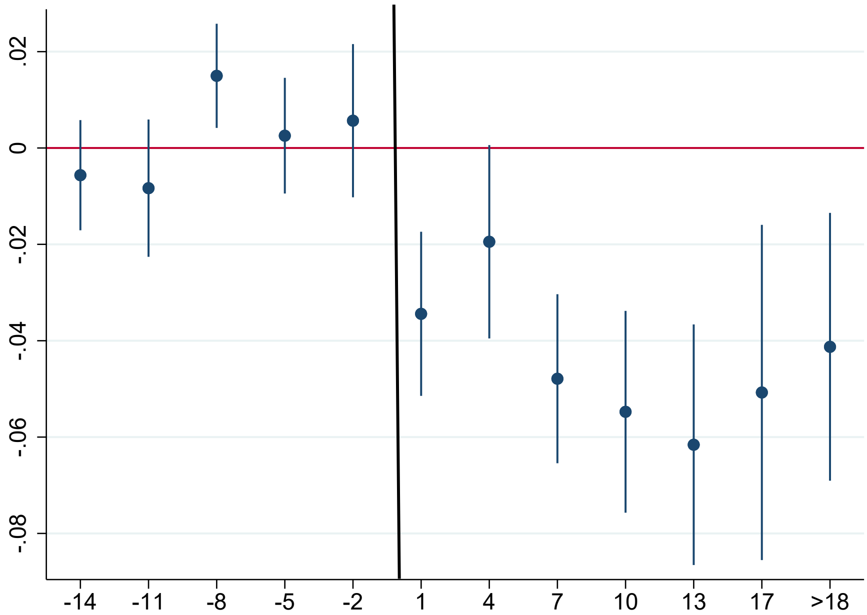
Notes: Data source: IPUMS US Census, 1920, 1930 and 1940.

FIGURE A.4 — The impact of birth control clinics on female labor force participation and employment



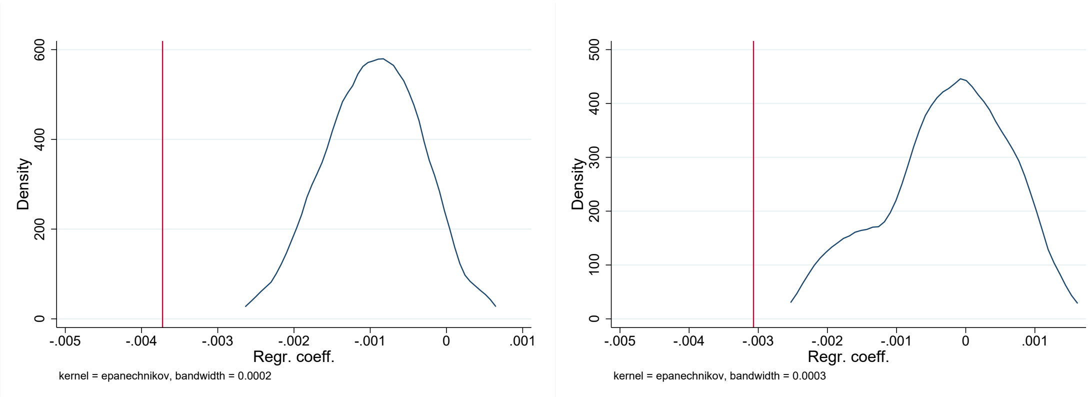
Notes: Data source: IPUMS US Census, 1920, 1930 and 1940. The figure shows event study estimates of the exposure to a birth control clinic on women's labor force participation (left panel) and women's employment status (right panel). Treatment effects are estimated along the lines of equation 2. The whiskers mark the 95 percent confidence band.

Figure A.5 — Event-study plot of the impact of exposure to a birth control clinic on fertility, restricting the sample to women from ever-treated counties



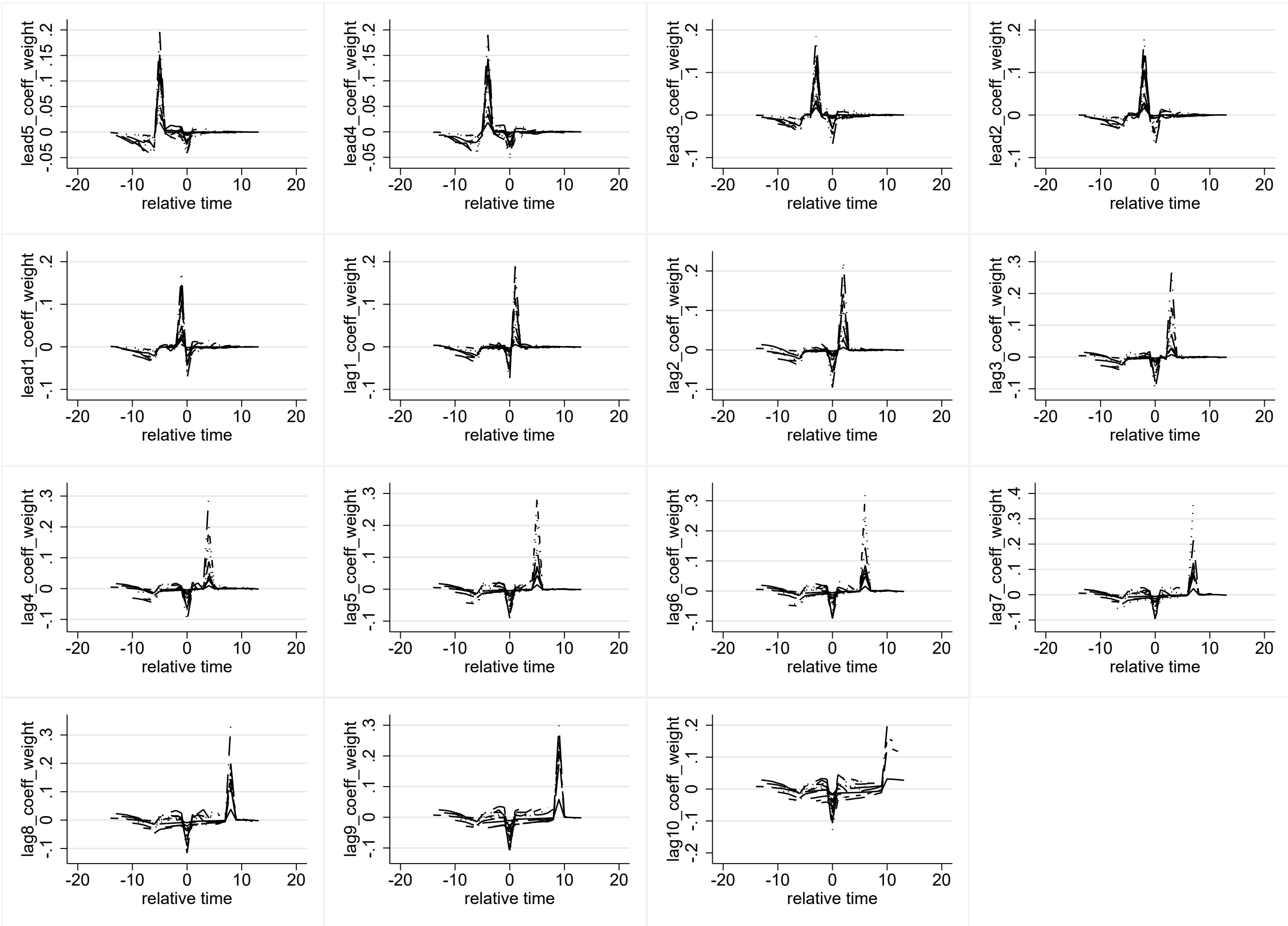
Notes: Data source: IPUMS US Census, 1920, 1930 and 1940. The figure shows event study estimates of the exposure to a birth control clinic on the number of children below the age of five in a household. Treatment effects are estimated along the lines of equation 2, but restricting the sample to women from ever-treated counties. The whiskers mark the 95 percent confidence band.

FIGURE A.6 — Randomization inference test



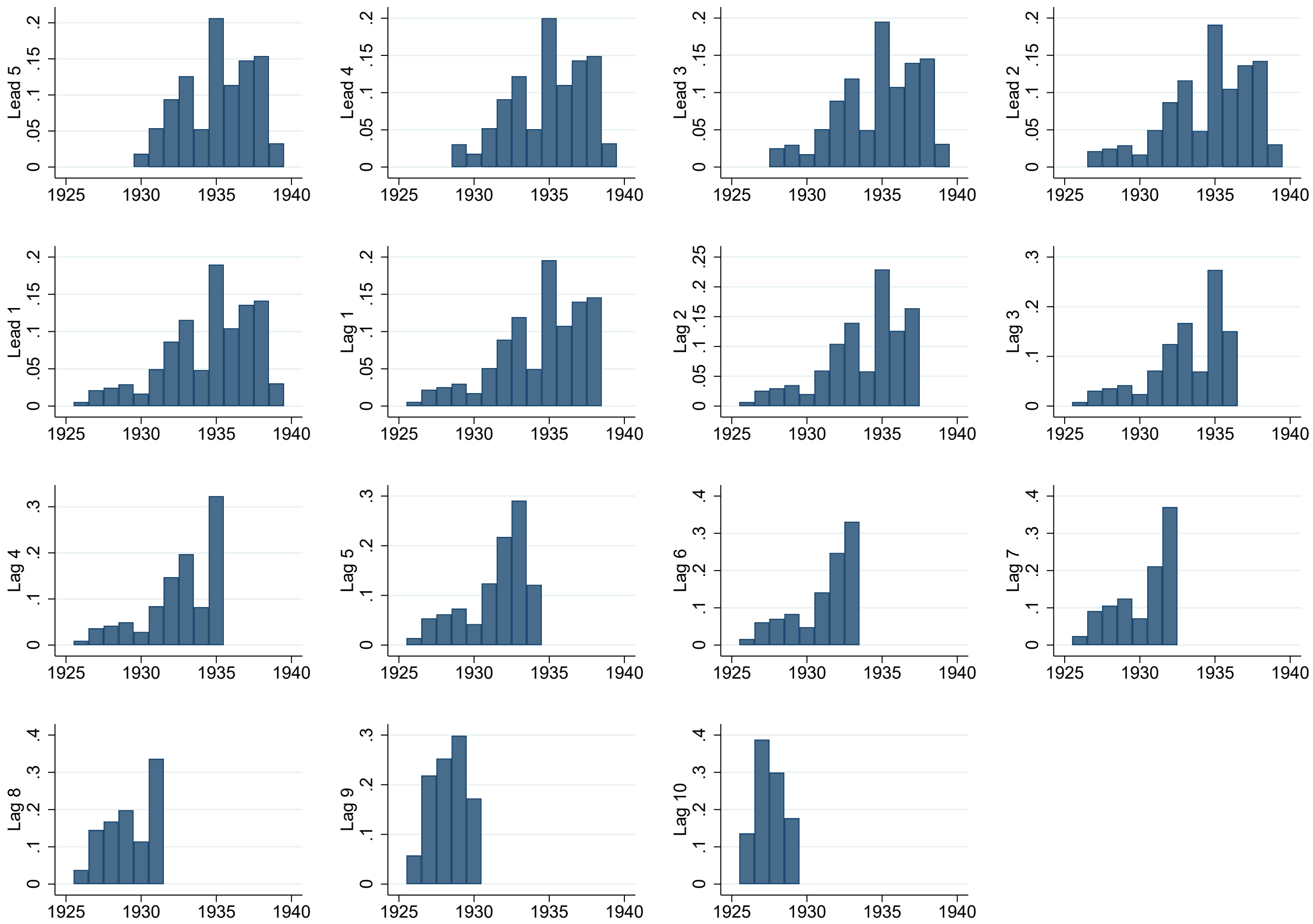
Notes: Data source: IPUMS US Census, 1920, 1930 and 1940. The figure shows randomization inference tests based on 100 permutations following Rosenbaum (2002) and Cohen and Dupas (2010) for the birth cohort by state fixed effects (left panel) and the county fixed effects model (right panel). The fact that the distribution of coefficients obtained on the permuted samples for the birth cohort by state model (left panel) is not centered around zero is due to the fact that states with more counties are more likely to have clinics randomly allocated to them and states with more counties were more urbanized and experienced a faster and earlier fertility decline than those counties that are in states with fewer counties.

Figure A.7 — Lead and lag specific weights in a dynamic TWFE specification



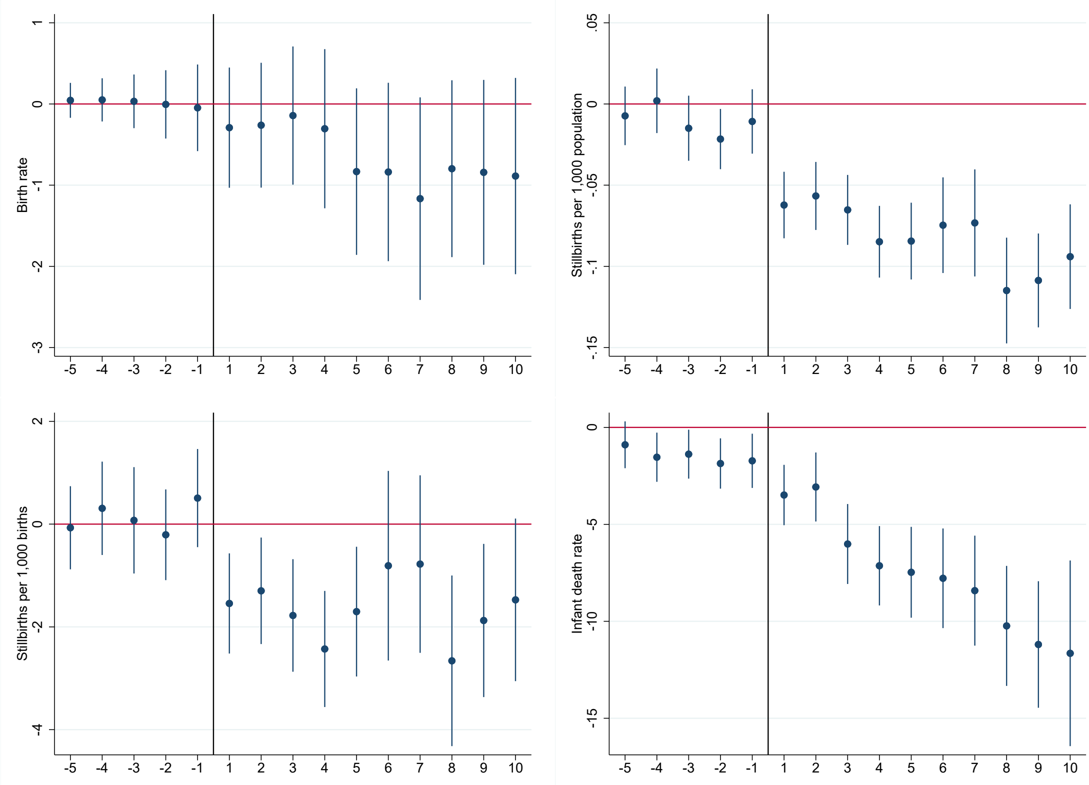
Notes: Data source: U.S. Vital Statistics. The figure shows that in a dynamic two-way fixed effects specification, the estimated event-study coefficients are combinations of differences in trends from their own relative period, from relative periods belonging to other bins included in the specification, and from relative periods excluded from the specification. We employ Sun and Abraham's publicly available Stata package 'eventstudyweights' to estimate the weights.

Figure A.8 — Event-study weights using Sun and Abraham's (2021) interaction weighted estimator



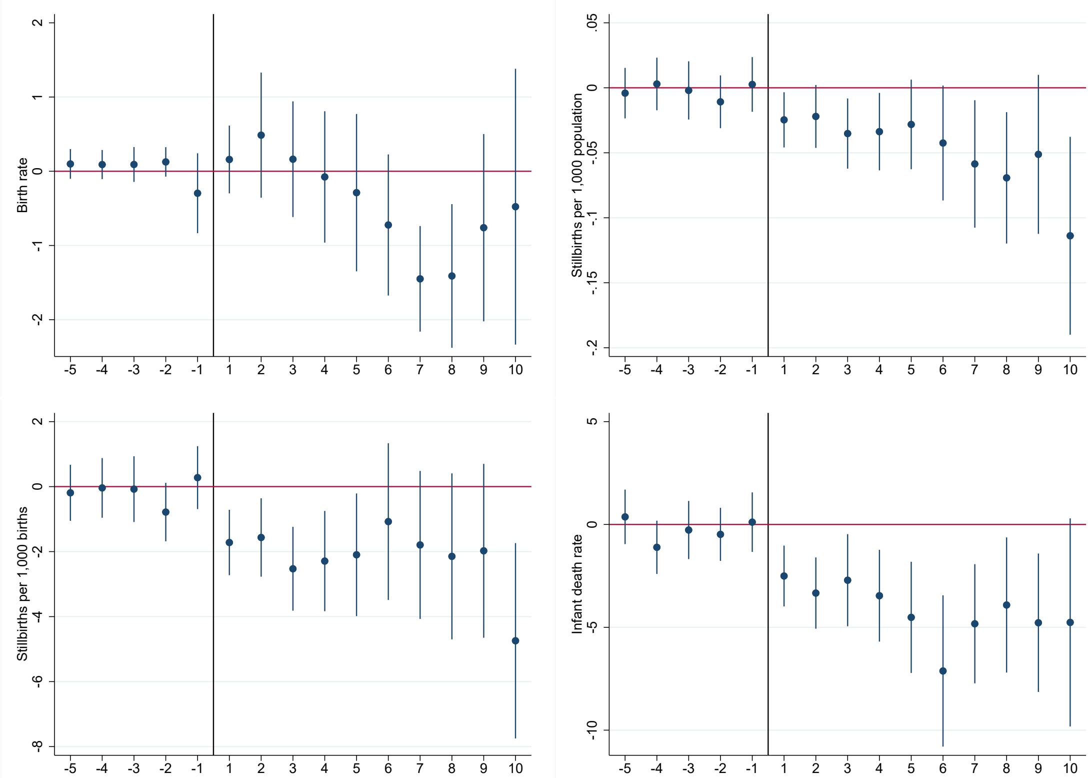
Notes: Data source: U.S. Vital Statistics. The figure shows which cohorts contribute which weight for all lead and lag coefficients estimated in the event-study using Sun and Abraham's (2021) interaction weighted estimator.

FIGURE A.9 — The impact of birth control clinics in a 20 years panel (Sun and Abraham, 2021)



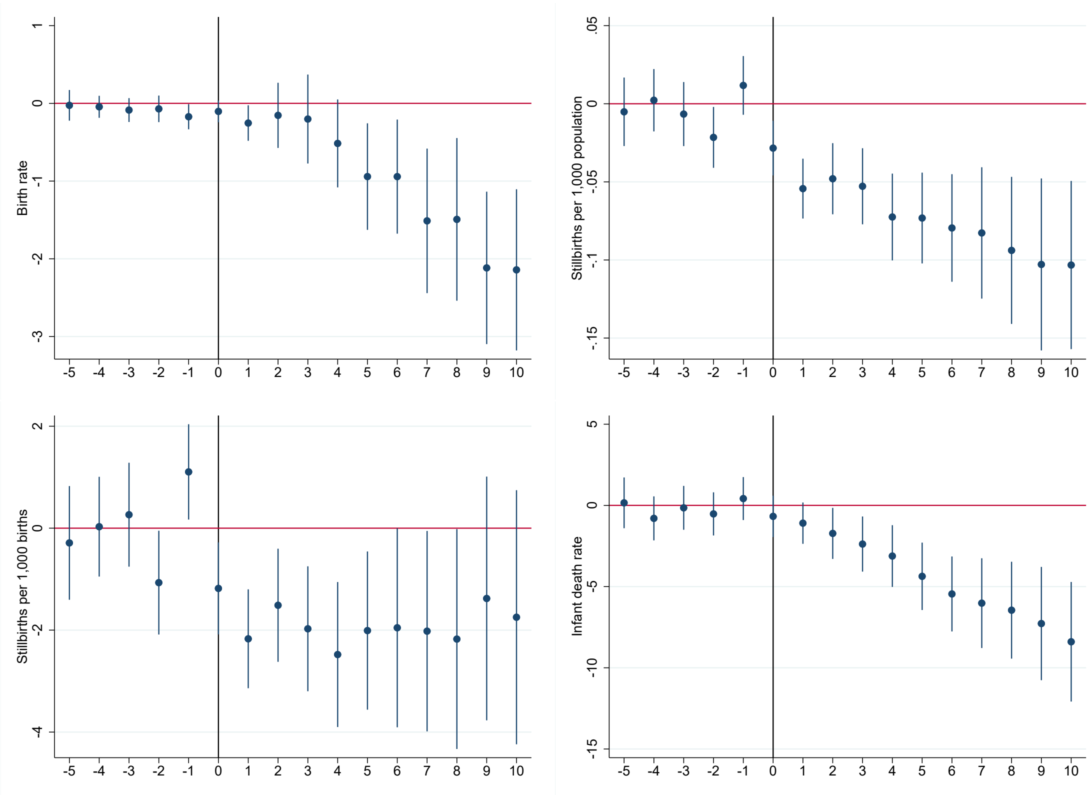
Notes: Data source: U.S. Vital Statistics. The figure shows the dynamic impact of the establishment of the first birth control clinic in a county (or an adjacent county) on the birth rate (upper left panel), the stillbirth rate (per 1,000 population) (upper right panel), the stillbirth rate (per 1,000 births) (lower left panel) and the infant death rate (lower right panel). Treatment effects are derived using the interaction weighted estimator by Sun and Abraham (2021). We bin ten or more years after the treatment into a single indicator. The control group consists of never-treated counties. Standard errors are clustered at the county level. The whiskers mark the 95 percent confidence band. The sample is restricted to a balanced panel of counties for the years 1920 to 1939.

FIGURE A.10 — The impact of birth control clinics using the last treated cohort as control group (Sun and Abraham, 2021)



Notes: Data source: U.S. Vital Statistics. The figure shows the dynamic impact of the establishment of the first birth control clinic in a county (or an adjacent county) on the birth rate (upper left panel), the stillbirth rate (per 1,000 population) (upper right panel), the stillbirth rate (per 1,000 births) (lower left panel) and the infant death rate (lower right panel). Treatment effects are derived using the interaction weighted estimator by Sun and Abraham (2021). We bin ten or more years after the treatment into a single indicator. The control group consists of the counties that are treated in 1938 and 1939. Standard errors are clustered at the county level. The whiskers mark the 95 percent confidence band. The sample is restricted to a balanced panel of counties for the years 1925 to 1937.

Figure A.11 — The impact of birth control clinics (Callaway and Sant'Anna, 2021)



Notes: Data source: U.S. Vital Statistics. The figure shows the dynamic impact of the establishment of the first birth control clinic in a county (or an adjacent county) on the birth rate (upper left panel), the stillbirth rate (per 1,000 population) (upper right panel), the stillbirth rate (per 1,000 births) (lower left panel) and the infant death rate (without infant deaths) (lower right panel). Treatment effects are derived using the estimator by Callaway and Sant'Anna (2021). The control group consists of never-treated counties. The whiskers mark the 95 percent confidence band. The sample is restricted to a balanced panel of counties for the years 1925 to 1939.

Table A.1 — County-level correlates of the roll-out of birth control clinics

	Cty. has BCC by 1940 Charact. of 1920 (I)	Cty. has BCC by 1940 Charact. of 1940 (II)	County expos. time Charact. of 1920 (III)	County expos. time Charact. of 1940 (IV)
Age, mean (women 15 to 39)	0.136***	0.119***	2.421***	2.715***
	(0.019)	(0.020)	(0.312)	(0.336)
Residence in urban area (share)	0.062	-0.014	1.948**	-0.517
	(0.052)	(0.051)	(0.813)	(0.798)
African American (share)	-0.067	0.083	-1.651	0.363
	(0.091)	(0.076)	(1.467)	(1.206)
Literate (share)	-0.364*	-0.010	-5.405	0.047
	(0.208)	(0.083)	(3.700)	(1.445)
Being in labor force (share)	0.079	0.019	0.210	1.699
	(0.164)	(0.164)	(2.783)	(2.481)
Foreign born (share)	0.156	0.465	5.435**	22.366***
	(0.139)	(0.355)	(2.215)	(5.296)
Protestant (share)	Ref.	Ref.	Ref.	Ref.
Catholic (share)	-0.108*	-0.050	-1.150	-0.842
	(0.061)	(0.055)	(1.028)	(0.941)
Other religion (share)	-0.071	-0.089	-1.183	-1.323
	(0.076)	(0.076)	(1.255)	(1.228)
Farm hh (share)	-0.270***	-0.280***	-3.534***	-4.170***
	(0.063)	(0.068)	(1.019)	(1.074)
Residence in big city (share)	0.264***	0.285***	7.084***	5.840***
	(0.073)	(0.070)	(1.022)	(0.975)
State-fixed effects	yes	yes	yes	yes
R2	0.304	0.303		
Observations	3,022	3,022	3,022	3,022

Notes: Data sources: IPUMS US Census, 1920, 1930 and 1940. In cols. (I) and (II) we regress a binary variable 'having a birth control clinic in the own or adjacent county by 1940' on characteristics observed in 1920 and 1940 respectively. In cols. (III) and (IV) we regress the time a clinic existed in the own or adjacent county by 1940, again on characteristics observed in 1920 and 1940 respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.2 — The impact of exposure to a birth conrol clinic on fertility - detailed regression results

	(I)	(II)	(III)	(IV)
Years of exposure to BCC	-0.004***	-0.004***	-0.003***	-0.003***
	(0.001)	(0.000)	(0.000)	(0.000)
Age	0.165***	0.165***	0.166***	0.178***
	(0.002)	(0.002)	(0.002)	(0.002)
Age squared/100	-0.326***	-0.328***	-0.329***	-0.332***
	(0.003)	(0.003)	(0.003)	(0.003)
Residence in urban area (=1)	-0.232***	-0.139***	-0.111***	-0.111***
	(0.002)	(0.002)	(0.001)	(0.001)
County level fertility 1920			0.439***	
			(0.005)	
County level fertility 1920 x year 1920				0.582***
				(0.006)
County level fertility 1920 x year 1930				0.463***
				(0.005)
County level fertility 1920 x year 1940				0.310***
				(0.006)
African American (=1)		-0.148***	-0.128***	-0.130***
		(0.002)	(0.002)	(0.002)
Literate (=1)		-0.103***	-0.098***	-0.097***
		(0.005)	(0.005)	(0.005)
Foreign born (=1)		0.172***	0.168***	0.166***
		(0.005)	(0.004)	(0.004)
Protestant (county share)		Ref.	Ref.	Ref.
Catholic (county share)		0.115***	0.071***	0.072***
		(0.006)	(0.004)	(0.004)
Other religion (county share)		0.015	-0.008	-0.008
		(0.011)	(0.008)	(0.007)
Farm hh (=1)		0.176***	0.154***	0.153***
		(0.001)	(0.001)	(0.001)
Residence in big city (=1)		-0.058***	-0.015***	-0.016***
		(0.002)	(0.002)	(0.002)
State-specific cohort-fixed effects	yes	yes	yes	yes
R2	0.072	0.084	0.088	0.088
Observations	45,104,489	45,104,489	45,104,489	45,104,489

*Notes: Data sources: IPUMS US Census, 1920, 1930 and 1940. The table shows OLS regressions. The dependent variable is the woman's number of children below the age of five living in the household. Standard errors are clustered at the birth cohort by county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

TABLE A.3 — The impact of exposure to a birth control clinic: postponing births vs. reducing completed fertility

	Benchmark (I)	<30 years (II)	>=30 years (III)	Interaction (IV)
Years of exposure to BCC	-0.004***	-0.007***	-0.001***	-0.017***
	(0.000)	(0.001)	(0.000)	(0.003)
Age × Years of exposure to BCC				0.00041***
				(0.000)
One year exposure effect at age 20				*-0.009*
One year exposure effect at age 30				*-0.005*
One year exposure effect at age 40				*-0.001*
State-specific cohort FE	yes	yes	yes	yes
Age and urban area controls	yes	yes	yes	yes
Socio-economic controls	yes	yes	yes	yes
R2	0.084	0.064	0.079	0.084
Observations	45,104,489	22,152,135	22,952,354	45,104,489

Notes: Data sources: IPUMS US Census, 1920, 1930 and 1940. The table shows OLS regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A.4 — The impact of exposure to a birth conrol clinic on female labor force participation and employment

	Labor force (I)	Employ. (II)
Years of exposure to BCC	0.001**	0.001**
	(0.000)	(0.000)
State-specific cohort FE	yes	yes
Age and urban area controls	yes	yes
Socio-economic controls	yes	yes
R^2	0.080	0.062
Observations	41,760,817	29,744,611

Notes: Data sources: IPUMS US Census, 1920, 1930 and 1940. The table shows OLS regressions. All regressions control for age and age squared as well as a variable indicating whether a houshold is in an urban area. Socio-economic controls are the literacy status, race, an indicator for being foreign born, an indicator for living in a big city, an indicator for living in a farm household, and the share of Catholics in a county. Standard errors are clustered at the birth cohort by county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5 — The impact of exposure to a birth control clinic on fertility by buffer zones

	20km zone (I)	50km zone (II)	50-100km ring (III)
Years of exposure to BCC	-0.00313***	-0.00379***	-0.00163***
	(0.000)	(0.000)	(0.001)
State-specific cohort FE	yes	yes	yes
Age and urban area controls	yes	yes	yes
Socio-economic controls	yes	yes	yes
R2	0.090	0.087	0.087
Observations	31,031,911	43,435,184	25,527,073

Notes: Data sources: IPUMS US Census, 1920, 1930 and 1940. The table shows OLS regressions. The dependent variable is the woman's number of children below the age of five living in the household. In cols. (I) and (II) we drop observations that have access to a birth control clinic within the 20 to 50km and 50 to 100km ring respectively. In col. (III) we drop observations that have access to a birth control clinic within the 50km buffer zone. Standard errors are clustered at the birth cohort by county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A.6 — The impact of exposure to a birth control clinic on fertility, heterogeneity

	Yes (I)	No (II)	(I) vs. (II) *p-value* (III)
Afro American (individual level)	-0.005***	-0.004***	
	(0.001)	(0.001)	(0.000)
Foreign born (individual level)	0.000	-0.003***	
	(1.000)	(0.000)	(0.000)
Catholic share at county level above median share	-0.002***	-0.005***	
	(0.001)	(0.001)	(0.000)
County vote in 1920 majority Democrats	-0.006***	-0.003***	
	(0.001)	(0.001)	(0.000)
County vote in 1932 majority Democrats	-0.004***	-0.003***	
	(0.001)	(0.001)	(0.757)
Southern states	-0.006***	-0.003***	
	(0.001)	(0.001)	(0.000)

Notes: Data sources: IPUMS US Census, 1920, 1930 and 1940. The sample, specification and controls used are the same than those used in Table 2 Table 2, col. (II). Each coefficient comes from a different regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7 — Matching treatment and control counties

	Probit	Unmatched sample		Matched sample	
	Treated	Treated	Control	Treated	Control
	(I)	(II)	(III)	(IV)	(V)
Birth rate 1925	0.324***	22.966	21.548	22.559	22.204
	(0.007)	(11.396)	(7.742)	(5.731)	(10.539)
Stillbirth rate 1925	0.005*	35.904	31.910	36.122	34.373
	(0.003)	(14.861)	(16.825)	(14.542)	(18.465)
Infant mortality 1925	0.004***	70.445	66.519	70.833	66.625
	(0.002)	(21.875)	(40.503)	(21.665)	(21.627)
Mortality rate 1925	-0.043***	10.068	8.596	10.099	9.222
	(0.013)	(3.272)	(4.544)	(3.224)	(5.887)
Foreign born (share 1920)	0.727	0.089	0.073	0.089	0.067
	(0.542)	(0.098)	(0.083)	(0.098)	(0.083)
Literate (share 1920)	-8.834***	0.954	0.965	0.954	0.955
	(1.204)	(0.046)	(0.053)	(0.046)	(0.062)
Farm households (share 1920)	-1.737***	0.391	0.518	0.389	0.483
	(0.239)	(0.228)	(0.194)	(0.225)	(0.208)
Blacks (share 1920)	-1.589***	0.074	0.060	0.073	0.089
	(0.476)	(0.128)	(0.147)	(0.127)	(0.178)
Age (average 1920)	0.512***	30.023	29.693	30.035	29.794
	(0.068)	(0.972)	(0.809)	(0.970)	(0.899)
Catholics (share 1920)	-0.844***	0.232	0.218	0.233	0.191
	(0.224)	(0.220)	(0.209)	(0.220)	(0.199)
Female labor (share 1920)	0.521	0.199	0.168	0.200	0.182
	(0.636)	(0.079)	(0.071)	(0.079)	(0.088)
Wage (average 1920)	-0.001***	1,006	1,019	1,005	993
	(0.000)	(256)	(275)	(256)	(263)
Urban county	-0.157	0.277	0.148	0.279	0.184
	(0.104)				
Big city county	1.326***	0.053	0.002	0.053	0.005
	(0.391)				
Observations 1925	1,757	905	916	890	385
Observations total		13,590	13,680	13,350	5,775
Pseudo $R2$	0.146				

Notes: Data sources: U.S. Vital Statistics. The table shows the results of a probit model where the outcome variable is a dummy variable indicating whether a birth control clinic was established in a county or a neigboring county prior to 1940 (column I) as well as a comparison of treated and control counties in the unmatched balanced panel 1925-1939 (columns II and III) as well as a comparison of treated and control counties in the matched balanced panel 1925-1939.

Table A.8 — Descriptive statistics: Administrative data

	Balanced panel 1925-1939, matched (I)	Balanced panel 1920-1939, matched (II)	Balanced panel 1925-1938, no never-treated (III)	Unbalanced panel 1920-1939 (IV)
Birth rate	19.459	20.640	19.752	20.338
	(6.320)	(7.465)	(9.416)	(8.310)
Stillbirth rate	32.827	32.448	33.805	33.325
	(15.399)	(14.408)	(14.895)	(18.382)
Infant death rate	59.663	63.212	61.868	62.054
	(21.484)	(22.374)	(20.854)	(27.173)
All-age death rate	10.026	10.233	10.270	9.244
	(3.898)	(4.251)	(3.526)	(3.682)
Distinct counties	1,275	915	907	3,011
Obs.	19,125	18,300	11,791	48,515

Notes: Data sources: U.S. Vital Statistics. The table shows the means and standard deviations (in parentheses) of the variables in the sample that is balanced in calendar time from 1925-1939 (main sample), balanced in calendar time from 1920 to 1939, balanced in calendar time from 1925 to 1938 without never-treated, and on the full unbalanced panel from 1920 to 1939 available in U.S. Vital Statistics data base.